The detection and accurate estimation of frequency attenuation effects in masked repetition priming: A large scale web browser-based study

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ABSTRACT
This study investigates the controversy surrounding the sensitivity of masked repetition priming to word frequency: while unmasked priming exhibits a frequency attenuation effect, wherein high frequency words yield smaller repetition effects, this phenomenon has been inconsistently reported in masked priming. We conducted two large online experiments with rigorously validated frequency databases to reconcile past discrepancies. The first experiment confirmed the viability of conducting masked priming experiments in web browser-based settings. The pre-registered second study, designed for high statistical power and precision, identified a 10-ms attenuation effect under masked priming. This result suggests that the repetition effect in masked priming is less qualitatively distinct from unmasked priming than previously assumed. This finding has implications for masked priming experimental design and theoretical consequences for models of priming. Crucially, models that predict either the presence or absence of frequency attenuation under masked conditions need to account for a small but reliable effect.

KEYWORDS
masked repetition priming; frequency attenuation effect; online browser-based experiment; power analysis

1. Introduction

The masked priming technique has been an invaluable tool in visual word recognition research. It has allowed researchers to study the conditions under which orthographic, phonological, morphological, and semantic information impact access to visual word forms while mitigating strategic effects and minimizing the influence of controlled processes (Forster 1998). First introduced in its traditional form by Forster and Davis (1984; see also Evett and Humphreys 1981), this technique involves a forward mask (i.e., usually a string of hashes, #####), followed by a prime string presented for very short

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time ($SOA < 60$ ms),[1] and a target string presented immediately after. Because the prime presentation is so brief and masked by preceding and subsequent stimuli, most participants report not being aware that a prime string has been presented, and can at most report a screen flicker just before the target presentation (Forster, Mohan, and Hector 2003).

Among possible manipulations of prime-target relatedness, masked repetition priming (in which the same word is presented as both the prime and target within the same trial: e.g., *love-LOVE*) has been well studied, because its response seems to be qualitatively different from the unmasked counterpart ($SOA > 60ms$): while high-frequency words benefit less from repetition than low-frequency words in the unmasked design (*frequency attenuation effect*, henceforth FAE; Scarborough, Cortese, and Scarborough 1977), this does not seem to be the case when the prime is masked (Forster and Davis 1984; Forster et al. 1987; Segui and Grainger 1990; Sereno 1991; Forster and Davis 1991; Rajaram and Neely 1992; Bodner and Masson 1997; Forster, Mohan, and Hector 2003; Nievas 2010). This asymmetry in sensitivity to lexical frequency between the masked and unmasked repetition priming responses has been important in distinguishing among different models of priming in visual word recognition. More specifically, *interactive activation models* (McClelland and Rumelhart 1981; Grainger and Jacobs 1996; Coltheart et al. 2001) conceive of priming as a “head start” in processing due to the pre-activation of the target word due to the presentation of the prime. Thus, according to *interactive activation* models, priming is ultimately caused by a single mechanism, making the qualitatively different profiles for repetition priming in masked and unmasked conditions a difficult empirical finding to explain.

Similarly, episodic models (e.g., Jacoby and Dallas 1981; Jacoby 1983) posit a different single mechanism for priming effects: the activation/retrieval of the episodic memory trace of the encounter with the prime word. These models therefore encounter the same type of difficulty in accounting for qualitatively different patterns of repetition priming effects in masked and unmasked conditions. A similar type of model, called the *memory recruitment model* makes very similar predictions to the episodic memory models, positing a non-lexical source for priming effects (Bodner and Masson 1997; Masson and Bodner 2003; Bodner and Masson 2014). Repetition priming effects under this view stem from the exploitation, strategically or automatically, of a memory resource created by the encounter with the prime word. The frequency attenuation effect, under episodic and memory recruitment models alike, is predicted on the basis that low frequency primes, being more distinctive stimuli, create a more potent and effective memory resource compared to high frequency primes.

In contrast, other models appear to successfully sidestep the problem posed by the qualitatively different repetition priming profiles observed in masked and unmasked conditions. One such model is the *entry-opening model* (also known as the *bin model*; Forster and Davis 1984). According to this model, when the visual stimulus is presented, lexical entries are assigned to specific bins based on orthographic similarity. In the first stage (fast search stage), a fast, frequency-ordered search goes through the entries within a given bin, and compares each one with the the input stimulus, assigning to each entry a goodness-of-fit score. This comparison is fast and crude, and sorts entries

[1] *SOA: Stimulus Onset Asynchrony*, i.e. the time between the start of one stimulus (in our case, the prime stimulus) and the start of another stimulus (the target stimulus). In the standard repetition priming design, no backward mask occurs between the prime and the target, and therefore SOA equals the duration of prime presentation.

into (a) perfect (i.e., no difference is detected between the input and the entry), (b) close (i.e., small differences are detected), and (c) irrelevant matches (i.e., substantial difference are detected). Any entry of type (a) or (b) is opened, so that the entry can be further analyzed and compared to the input in the subsequent verification stage. Under a masked presentation, the entry of the prime word is opened at the fast search stage, but the short duration of the stimulus prevents it from reaching the evaluation stage. Crucially, the entry is nonetheless left open. Upon the presentation of the target stimulus, the access procedure will follow its two stage course, with a frequency-sensitive fast search and a subsequent entry opening for evaluation/verification. In this view, the fast search for the target word proceeds normally, but the evaluation/verification procedure starts and ends sooner than it otherwise would, because the target entry has already been left open after the brief processing of the prime. Thus, the *entry-opening model* explains the masked repetition priming as the benefit from having the entry of the target word already open by the time the second stage of recognition starts. Crucially, this occurs *after* the target word is initially accessed, which happens in order of frequency. Put differently, according to the *entry-opening model*, masked repetition priming occurs because of the time savings from not having to open the entry of the target, which is a frequency-insensitive process (i.e., every entry takes the same time to be opened), but *after* the frequency-sensitive first access stage. As a consequence, the *entry-opening model* predicts a frequency-insensitive masked repetition priming effect, which is what has been traditionally reported in the literature (see Table 1). In addition, it also predicts that pseudowords should not benefit from masked repetition priming, as they have no entries in the mental lexicon to be left open after the brief processing of the prime.

However, as Table 1 shows, there are nonetheless a few studies that do report significant FAEs in masked repetition priming (Bodner and Masson 2001; Kinoshita 2006; Norris and Kinoshita 2008; Nievas 2010). Bodner and Masson (2001) report that when stimuli are presented in alternating case (e.g., *pHoNe*), this increases the lexical decision difficulty and therefore generates an extra incentive to draw on the memory resource created by the brief processing of the prime. Under such conditions, they were able to observe a statistically significant FAE. In the same vein, Kinoshita (2006) noticed that in earlier studies the low frequency words often had very high error rates, and suggested that perhaps many participants did not know them. If participants treated a substantial number of low frequency words as nonwords, and nonwords do not exhibit repetition priming under masked conditions, it could artificially depress the repetition priming effect for the low frequency condition alone, which could make any existing FAE harder to detect. In two separate experiments, Kinoshita (2006) showed that larger repetition priming effects for low frequency words were only obtained when the low frequency words were vetted to make sure the participants knew them prior to the experiment. Following up on Kinoshita (2006), Norris and Kinoshita (2008) were also able to find an interaction between lexical frequency and repetition in masked repetition priming. Similar FAEs were recently reported in a Spanish study by Nievas (2010) (exp. 1b).

Finally, as Table 1 shows, it is noteworthy that 15 out of 18 previous studies showed numerically larger masked priming effects for low frequency words as opposed to high frequency words, irrespective of statistical significance. Similarly, the average repetition effect for low frequency words in the studies reviewed in Table 1 is 13 ms larger when compared to that of high frequency words. These results are not in line with the predictions dictated by the *entry opening model*, and seem to align better with the predictions made by *interactive activation models* and *memory recruitment* models.

Table 1. Summary of the masked repetition priming effects as a function of word frequency reported in the literature. The power range estimates were calculated by simulating 10,000 datasets with the corresponding sample size (N) and FAE = 15 ms and 30 ms.

				MOP (ms)		FAE (ms)		Power range [min max]	
Study	Language	N	SOA	HF	LF	ES	p<.05?	FAE=15ms	FAE=30ms
Forster, Davis, Schoknecht, & Carter (1987), exp. 1	English	16	60	61	66	5		[0.02 0.24]	[0.04 0.84]
Norris, Kinoshita, Hall, & Henson (2018)	English	16	50	38	51	13		[0.02 0.24]	[0.04 0.84]
Sereno (1991), exp. 1	English	20	60	40	64	24		[0.02 0.33]	[0.04 0.92]
Forster & Davis (1991), exp. 5	English	24	60	54	72	18		[0.02 0.4]	[0.05 0.96]
Bodner & Masson (1997), exp. 1	English	24	60	29	45	16		[0.02 0.4]	[0.05 0.96]
Bodner & Masson (1997), exp. 3	English	24	60	36	50	14		[0.02 0.4]	[0.05 0.96]
Forster, Mohan, & Hector (2003), exp. 1	English	24	60	63	60	-3		[0.02 0.4]	[0.05 0.96]
Kinoshita (2006), exp. 1	English	24	53	32	38	6		[0.02 0.4]	[0.05 0.96]
Kinoshita (2006), exp. 2	English	24	53	29	59	30	*	[0.02 0.4]	[0.05 0.96]
Norris & Kinoshita (2008), exp. 1	English	24	53	35	66	31	*	[0.02 0.4]	[0.05 0.96]
Forster, Davis, Schoknecht, & Carter (1987), exp. 4	English	27	60	34	25	-9		[0.03 0.46]	[0.05 0.98]
Forster & Davis (1984), exp. 1	English	28	60	45	38	-7		[0.03 0.48]	[0.06 0.98]
Nievas (2010), exp. 1b	Spanish	30	50	44	65	21	*	[0.03 0.52]	[0.06 0.99]
Nievas (2010), exp. 2a	Spanish	30	50 or 33[1]	51	58	7		[0.03 0.52]	[0.06 0.99]
Segui & Grainger (1990), exp. 4	French	36	60	42	45	3		[0.03 0.63]	[0.07 1]
Bodner & Masson (2001), exps. 2A, 2B, 3, & 6 (average)[2]	English	40	60	37	69	32	*	[0.03 0.68]	[0.08 1]
Rajaram & Neely (1992), exp. 1	English	48	50	30	37	7		[0.04 0.76]	[0.09 1]
Rajaram & Neely (1992), exp. 2	English	48	50	45	78	33		[0.04 0.76]	[0.09 1]
Mean				41	55	13			
SD				10	14	13			
Correlation						0.46			

[1]SOA for each subject determined by pre-test
[2]Reported in Masson & Bodner (2003)

2. The present study

It is somewhat surprising that the status of the FAE in masked priming remains largely unresolved in the literature, given its non-negligible magnitude and its theoretical significance in elucidating the underlying cognitive processes of masked priming.

One possible interpretation of the conflicting past findings revolves around the fact that only 4 out of 18 studies demonstrate a statistically significant FAE in masked repetition priming. Notably, this number potentially diminishes further when considering that, among these four studies, the FAE is detected only through the pooling of data across multiple studies employing a unique alternating-case stimulus presentation (Bodner and Masson 2001; Masson and Bodner 2003). This line of reasoning suggests a qualitatively distinct profile between masked and unmasked repetition priming, with the FAE more firmly established in the latter.

Conversely, one could argue that 15 out of 18 studies exhibit numerically larger repetition effect sizes for low-frequency words compared to high-frequency words —- a pattern that is challenging to reconcile with a genuine absence of interaction between frequency and masked repetition. Additionally, the average FAE across all studies stands at 13 ms, a modest yet non-negligible effect size. In fact, the naïve assumption that the two conditions are similar enough across experiments could justify the use of a t-test with statistically significant results: $M\_FAE = 13$, CI_95% = [7, 20]), $t(17) = 4.24$, $p = .0005$. These considerations suggest that a genuine FAE may exist in masked priming but might be smaller than the thresholds detectable by most previous experiments. This interpretation is supported by the results from Adelman et al. (2014) in a large scale, multi-site lab-based study on orthographic priming. They report a small but reliable FAE, but only when the frequency counts of Brysbaert and New (2009) are used, and caution that this effect could simply be an orthographic neighborhood effect masquerading as a frequency effect, due to the high correlations between the two variables.

Compounding this complexity, another potential contributor to past discrepancies is the reliance on the dated Kucera and Francis (1967) word frequency database, which 15 out of 18 studies have depended on. This poses a potential problem, as this frequency database has consistently demonstrated inferior predictive performance, particularly with low-frequency words, compared to more contemporary databases (Burgess and Livesay 1998; Zevin and Seidenberg 2002; Balota et al. 2004; Brysbaert and New 2009; Yap and Balota 2009; Brysbaert and Cortese 2011; Gimenes and New 2016; Herdağdelen and Marelli 2017; Brysbaert, Mandera, and Keuleers 2018). Both of these issues are addressed in the subsequent sections.

2.1. *Issues with frequency databases*

Due to the well-documented concerns over the reliability of the Kučera and Francis (1967) frequency database for psycholinguistic experiments (Burgess and Livesay 1998; Zevin and Seidenberg 2002; Balota et al. 2004; Brysbaert and New 2009; Yap and Balota 2009; Brysbaert and Cortese 2011; Gimenes and New 2016; Herdağdelen and Marelli 2017; Brysbaert, Mandera, and Keuleers 2018), our studies exclusively sourced materials from the HAL (Lund and Burgess 1996) and SUBTLEX$_{US}$ (Brysbaert and New 2009) databases, which reflect more recent linguistic usage and offer better validation in behavioral experiments (e.g., Balota et al. 2004; Brysbaert and New 2009; Yap and Balota 2009; Brysbaert and Cortese 2011; Gimenes and New 2016; Herdağdelen and

Marelli 2017). While these databases outperform Kučera and Francis (1967) in predicting psycholinguistic task outcomes, it is important to note potential discrepancies in individual frequency counts, particularly in the low and mid-frequency ranges. This variation, attributable to the primary genre of their sources (USENET groups for HAL and movie subtitles for SUBTLEX$_{US}$),[2] may have minimal impact on megastudies with large word samples (e.g., Balota et al. 2004; Brysbaert and New 2009; Yap and Balota 2009; Brysbaert and Cortese 2011; Gimenes and New 2016; Herdağdelen and Marelli 2017). However, corpus-specific frequency skew can become significant when dealing with smaller samples of words, as is the case in most masked priming studies (cf. Adelman et al. (2014)). Table 2 illustrates the potential discrepancy in considering words as high or low frequency based on the different aforementioned databases.

Table 2. Example of frequency count imbalances (in occurrences per million) across the frequency norms of Kucera & Francis (KF), HAL and SUBTLEX$_{\mathrm{US}}$ for 4 to 6 letter words.

Word	KF	HAL	SUBTLEX$_{\mathrm{US}}$
Skew in KF			
negro	104	3	5
poet	99	9	9
mercer	71	4	2
swung	48	3	2
mantle	48	8	2
Skew in HAL			
web	6	351	9
user	4	297	2
mint	7	211	5
format	9	198	1
warp	4	125	5
Skew in SUBTLEX$_{US}$			
daddy	4	16	185
bitch	6	24	169
cute	5	28	88
pardon	8	12	65
steal	5	28	53

2.2. *Issues with statistical power*

The inconsistency of past findings regarding the FAE in masked priming has been linked to a potential lack of statistical power in previous research (Bodner and Masson 1997, 2001; Masson and Bodner 2003; Adelman et al. 2014). This is a plausible concern, as interactions like the FAE often require larger sample sizes for statistical detection (Potvin and Schutz 2000; Brysbaert and Stevens 2018) compared to main effects. We outline be-

[2] A separate, though relevant issue which cannot be addressed here is to how to mitigate the discrepancies across the databases available, but see Yap and Balota (2009), and Brysbaert and Cortese (2011) for proposals about combining the frequency counts from different corpora.

low three ways in which neglecting statistical power might frustrate our understanding of FAE in masked repetition priming.

First, our literature review revealed crucial gaps in the reporting of relevant statistical information, which impedes the assessment of the statistical power attained by past experiments. The inconsistent reporting of each conditions' standard deviations (only 7 out of 18 studies) and the complete absence of reporting of the correlation structure between conditions complicates power assessments. Researchers are thus forced to explore a range of plausible values for standard deviations and correlation structures on their own. Table 1 details our attempt to conduct power simulations for two hypothesized frequency attenuation effect sizes: 15 ms (close to the averaged FAE of 13 ms) and 30 ms (close to the only three observed statistically significant FAE in English). Standard deviations (ranging between 60 ms and 180 ms, in 10 ms increments) and correlation between conditions (uniformly set to range between 0.6 and 0.9, with 0.1 unit increments) were simulated for each study's sample size, with 10,000 replications for each simulation. These range of values were derived from our literature review and previous in lab and online experiments (Petrosino 2024; Petrosino, Sprouse, and Almeida 2023). For each simulated dataset, a paired t-test was performed comparing the repetition effect for high frequency words and low frequency words. This calculation is mathematically identical to the interaction term in a 2x2 factorial within-subjects design (the squared t value is equal to the F value for the interaction calculated in the 2x2 rmANOVA), but it is less computationally expensive to perform in large scale simulations. Power to detect this interaction was then calculated as the proportion of significant tests obtained across replications. All else being equal, standard deviations and correlations between conditions have opposite effects on statistical power: increases in standard deviations lead to less power, while increases in correlation between conditions lead to more power. The results reported in Table 1 reveal a wide range of possible statistical power attained by previous studies, depending solely on the combination of plausible standard deviation and correlation across conditions. For instance, the study with the smallest sample size (Forster et al. 1987, $N = 16$) had a 2% to 24% chance of detecting a 15 ms frequency attenuation effect and a 4% to 84% chance to detect a 30 ms effect. Similarly, the study with the largest sample size (Rajaram and Neely 1992, $N = 48$) exhibited a range of 4% to 76% for a 15 ms frequency attenuation effect and 9% to 100% for a 30 ms effect. As a consequence of the limited reporting of relevant statistical information in past studies, it is nearly impossible to determine if any of them were adequately powered to detect the effect of interest.

A second concern arising from the ambiguity surrounding statistical power in the literature is the potential impact of a prevalence of low-powered experiments on the scientific record. An excess of such experiments increases the risk of observed statistically significant effects being spurious (Button et al. 2013). As highlighted in Table 1, only 4 out of 18 studies demonstrate a statistically significant FAE. The absence of clarity regarding the statistical power of previous research poses challenges in assessing the likelihood of these significant findings being spurious.

Finally, it is widely acknowledged that experiments with approximately 50% power are akin to a coin toss in their ability to detect a true effect (Cohen 1992). A less-appreciated fact is that, in the presence of even lower power (<25%), statistically significant results can substantially overestimate the effect size – a type-M error (Gelman and Carlin 2014). When power drops to levels below 10%, a statistically significant result may occur even when the observed effect goes in the opposite direction of the true effect – a type-S error

(Gelman and Carlin 2014). Our power simulations for within-subjects data revealed a similar relationship between statistical power, type-M, and type-S errors in line with the observations detailed by Gelman and Carlin (2014) for the independent samples t-test. For instance, at 10% power (a possibility for virtually all previous studies, as indicated in Table 1), a statistically significant result could indicate an overestimation of the magnitude of the frequency attenuation effect by a factor between 2 and 5, with a 5% chance of incorrectly determining the direction of the effect.

The two studies reported here were designed to mitigate these two confounding issues: the overreliance on the Kučera and Francis (1967) frequency data as well as a potential lack of statistical power observed in previous research. As a large increase in statistical power requires a large sample size, Experiment 1 aimed to assess the suitability of using *Labvanced* (Finger et al. 2017), an online platform for running web browser-based experiments, for running masked priming studies online.

3. Experiment 1

As evident in Table 1, conducting a properly powered experiment for a FAE close to the averaged value calculated from previous studies will require sample sizes that would be impractical to pursue in standard university research settings, typically quiet lab rooms with limited research computers. In response to this challenge, our study was exclusively conducted online, leveraging the growing trend in online behavioral research facilitated by HTML5 capabilities and the availability of advanced web software such as *jsPsych* (de Leeuw 2014), *PsychoJS* [the JavaScript counterpart of *PsychoPy*; Peirce et al. (2019)], *Gorilla* (Anwyl-Irvine et al. 2020), and *Labvanced* (Finger et al. 2017).

Notably, three recent studies have already demonstrated the viability of conducting masked priming experiments online, employing different software tools: Angele et al. (2023) with *PsychoJS*, Cayado, Wray, and Stockall (2023) with *Gorilla* and Petrosino, Sprouse, and Almeida (2023) with *Labvanced*. In this study, we opted for *Labvanced* (Finger et al. 2017), given our previous successful experience with it (Petrosino, Sprouse, and Almeida 2023). Similar to *Gorilla*, *Labvanced* eliminates local installation issues, ensures cross-platform consistency, and simplifies experimental design without necessitating proficiency in additional programming languages, while still providing comparable levels of versatility and capability.

3.1. *Methods*

3.1.1. Participants

Three hundred participants (145 females; *mean age* = 38; *sd* = 12) were recruited on the Prolific online platform (https://www.prolific.com). Several criteria were selected to ensure recruitment of native speakers of U.S. English. Participants had to be born in the Unites States of America, speak English as their first and only language, and have no self-reported language-related disorder. We encouraged participants to avoid any sort of distraction throughout the experiment, and to close any program that may be running in the background. Because the experiment was run online, participants could not be monitored during data collection. Finally, to further reduce variability across participants' devices, we restricted the experiment to be run on Google Chrome only, which is the most used browser worldwide (W3 Counter 2023), and reportedly performs better than any other across operating systems (likely thanks to the *Blink* engine; see Lukács and Gartus 2023).

3.1.2. Design

The masked priming procedured relied on a lexical decision task (LDT), in which a 3 (condition: *high* vs *low* vs *non-word*) x 2 (primetype: *repetition* vs *unrelated*) factorial design was used. Both factors were manipulated within-subjects. The dependent variables were lexical decision latency (in miliseconds) and error rate (in percentages).

3.1.3. Materials

Two hundred five-letter English words were selected from the English Lexicon Project (ELP; Balota et al. 2007), in which 100 words were selected from an upper and a lower frequency range, respectively.[3] It was not possible to identify two frequency ranges that were well separated from one another for both the HAL (Lund and Burgess 1996) and the SUBTLEX$_{US}$ (Brysbaert and New 2009) frequency databases. As Table 3 shows, we managed to do this only for the former, whereas some overlap was present in the latter. This is expected given the different source of the two databases (see above, and fn. 2). The two word subsets corresponded to the two word frequency conditions being tested: the high-frequency, and low-frequency conditions. In each condition, fifty words were randomly chosen to be presented as targets and related primes (for the related prime type condition), and the remaining fifty were presented as unrelated primes (for the unrelated prime type condition).

Table 3. Experiment 1. Descriptive statistics of the word item used. For both frequency databases, the word frequencies were converted to per-million count to ensure cross-comparison.

		HAL				**SUBTLEX$_{\mathbf{US}}$**			
frequency	N	min	max	mean	SD	min	max	mean	SD
high	100	169	1212	482	292	2.00	1168	129	201
low	100	3	23	9	5	0.12	13	3	3

Two-hundred five-letter phonotactically legal nonwords were randomly selected from the ELP database as well. Half of them were randomly selected to be presented as targets; the other half was instead used as unrelated nonword primes.

3.1.4. Procedure

Each recruited participant was assigned one of two word lists, which differed only in the relatedness of the prime with respect to the target; otherwise, the two lists presented the same set of target words and nonwords (300 items in total). In one list, the three conditions (high-frequency, low-frequency word conditions, and the non-word condition)

[3] The experiment also included an even lower frequency condition (HAL frequency range: [0.16 1]; HAL mean: 0.69, HAL SD: 0.28), thus summing up to six hundred trials being presented in the experiment. However, the average error rate for this condition was 44% and 33 (out of the 50) target words used in the same condition had a error rate higher than 30%. This suggested that they might have not known these words (see Kinoshita 2006). For this reason, this condition was completely removed from analysis.

had 25 target items being preceded by themselves (the *related* condition) and the remaining 25 target items being preceded by one of the unrelated primes belonging to the same frequency bin (the *unrelated* condition). In the other list, these assignments were reversed. The order of stimulus presentation was randomized for each participant.

After being recruited in the *Prolific* online platform, participants were asked to click on a link redirecting them to the Labvanced online service. During the experiment, they were asked to perform a lexical decision task by pressing either the 'J' (for word) or 'F' (for non-word) keys on their keyboard. Each trial consisted of three different stimuli appearing at the center of the screen: a series of hashes (#####) presented for 500 ms, followed by a prime word presented for 33 ms, and finally the target word; the target word disappeared from the screen as soon as a decision was made. The motivation for the choice of a very short prime duration (as compared to the literature, in which it is usually between 50 and 60 ms; see Table 1) is threefold. First, previous experiments on *Labvanced* (e.g., Petrosino, Sprouse, and Almeida 2023) showed that, due to the inherent difficulties in presenting stimuli for very short set durations in the browser, a longer set duration would increase the number of trials in which the prime duration would rise above the subliminal threshold (usually set at 60 ms) due to timing inaccuracies and missing screen refreshes, which could trigger the adoption of experiment-wide strategies in the task, and ultimately contaminate the masked priming response (Zimmerman and Gomez 2012). Second, Angele et al. (2023), Cayado, Wray, and Stockall (2023) and Petrosino, Sprouse, and Almeida (2023) have demonstrated that a 33 ms priming duration is sufficient to elicit repetition priming effects in online experiments. Finally, setting such a short prime duration prevents virtually everyone from consciously perceiving the prime word Nievas (2010), and thus presents a less contaminated estimate of early putatively automatic processes in word recognition.

Participants were given 5 breaks throughout the experiment. When the experiment was over, the participants were then redirected to Prolific in order to validate their submission. The median time to finish the experiment was 11 minutes. Each participant was paid with a standard rate of GBP 9/hour.

3.2. *Data analysis*

Analysis scripts and an abridged version of the data collected can be found on online (https://osf.io/ej8dh). We performed three different steps of analyses (in sequential order), with the goal of only keeping data that pass a set of stringent inclusion criteria (77,359 observations in total). After removing participants and items with high error rates, we inspected the durations of prime stimuli and removed those that did not fell within our desired range. Finally, we removed RT outliers.

3.2.1. Step 1: subject and item performance

Item and subject error rates were calculated, with a cutoff of 30%. Only 3 low-frequency words (*carte, parse, posit*), 5 non-words (*frick, gotch, phasm, pluff, venem*), and 8 participants were removed, with 291 participants remaining.

3.2.2. Step 2: prime durations

During the experiment, the duration of presentation of the prime word was recorded for every trial. Both the mean (mean = 37.88 ms) and the median (median = 35 ms) of prime durations were slightly larger than the intended value (33 ms). This distribution suggests some imprecision in prime duration during the experiment. This was expected and likely due to the inherent difficulty with timing precision of visual presentations in web browsers and the great variation of computer hardware and internet connections used by the participants. Both of these issues may be impossible to control, at least at the current state of browser development. However, in masked priming, in which the duration of the prime is an essential part of the design, such fluctuations may indeed hinder proper elicitation of the priming response. As a way to counteract the influence that such fluctuations might have on the priming response, we only kept trials whose prime durations were within a pre-set range from the intended prime duration of 33 ms. Taking a standard 60-Hz monitor as reference, the lower and the upper bounds were set respectively at 25 ms (i.e., the intended prime duration minus half of a full refresh cycle: $33 - 8$ ms; noting that Angele et al. (2023) already showed that no repetition priming effects are obtained with a 16.7ms prime duration) and 60 ms (i.e., the commonly accepted upper threshold of subliminal processing), in an attempt to remove any trial that could have been consciously perceived by participants. Only 4% of the trials were out of this duration range. We take this as evidence that *Labvanced* is able to consistently present stimuli at short durations. Prime duration fluctuations were however observed, and they were likely due to external factors outside of experimenter control (such as computer hardware, internet connection speed, and number of active operations in the background). The out-of-range trial removal was performed on the data after the error rate removal procedure. A total of 291 participants and 67,209 observations were included in the next steps of analysis.

3.2.3. Step 3: RT distribution

Finally, individual trials were excluded if their RT was below 200 ms and 1800 ms. 602 observations were excluded at this stage of analysis (i.e., 0.9% of the dataset). After removing incorrect trials, to ensure more accurate estimates, we also made sure that each condition for each each participant contained at least half of the total number of trials presented (i.e., 12). A total of 61,449 observations and 282 subjects were included in the statistical analysis below.

3.3. ***Results***

For each frequency bin, priming effects were calculated for each subject by subtracting the subject's mean RT to the repetition condition from the subject's mean RT to the unrelated condition. Unstandardized (in ms) and standardized effect sizes (i.e., Cohen's *d*) were then calculated for each condition. A 2x2 ANOVA (condition, 2 levels: *high* vs. *low*; primetype, 2 levels: *unrelated* vs. *repetition*) revealed significant main effects (condition: $F(1, 281)$=1167, $p < .0001$; primetype: $F(1, 281)$=198, $p < .0001$), and a trending-to-significance interaction ($F(1, 281)$=3.53, $p = .06$). Planned comparisons showed that significant repetition priming effects were triggered in both frequency conditions (MOP_HF = 23, CI_95% = [19, 27], $t(281)$ = 10.4, $p < .0001$; MOP_LF = 30, CI_95% = [24, 36], $t(281)$ = 9.75, $p < .0001$), with the low-frequency repetition priming effect being 7-ms larger than that of the high-frequency words, but only marginally significant (M_FAE = 7, CI_95% = [-1, 15]), $t(281)$ = 1.88, $p = 0.06$; Table 4). Non-word repetition priming effects were inhibitory, and marginally significant ($MOP\_$ = -4, CI_95% = [-8, 0], $t(281)$ = -1.91, $p = 0.057$). As for the error analysis, we found a significant priming effect in all conditions (high: $t(281)$=2.51, $p < .0001$; low: $t(281)$=6.39,

$p < .0001$; non-word: $t(281)$=-2.24, $p < .0001$).

Table 4. Experiment 1. Summary of the word priming results. *Legend.* MOP: magnitude of priming. SD: standard deviation. SD_p: standard deviation of the priming effects. ES: effect size (Cohen's d). cor: correlation.

	unrelated RT			repetition RT				priming effects				t-test		
factor	mean	SD	Error (%)	mean	SD	Error (%)	cor	MOP	95% CI	SD_p	ES	t	df	p
high	619	77	2	596	80	1	0.89	23	[19 27]	37	0.62	10.4	281	8.78e-22
low	699	93	10	669	91	7	0.84	30	[24 36]	52	0.58	9.75	281	1.51e-19
non-word	712	110	6	716	110	6	0.96	-4	[-8 0]	31	-0.11	-1.91	281	0.0567
frequency:primetype							-0.01	7	[-1 15]	64	0.11	1.88	281	0.0616

3.4. *Discussion*

The primary objective of Experiment 1 was to evaluate whether web browser-based stimulus delivery programs such as *Labvanced* can yield data comparable in quality to traditional lab-based experiments. The results indicate that this is indeed possible, but careful inspection of prime durations is nonetheless necessary.

Robust repetition priming was observed in both frequency conditions. The non-word condition triggered a small inhibitory repetition effect, in line with the previous literature Forster (1999), but this was not statistically significant. Crucially, we observed a 7 ms FAE effect that was marginally statistically significant. As noted elsewhere (Potvin and Schutz 2000), the absence of a significant interaction effect may easily arise due to low statistical power. The 95% CI for the FAE was between -1 ms and 15 ms. This interval suggests that the actual FAE is possibly a positive value that can be as large as 15 ms. This is in line with the results from previous literature, with the caveat that the majority of previous experiments used ~50 ms prime durations, while experiment 1 used a 33 ms prime duration. Prime durations have been suggested to be an upper bound on the size of the masked repetition priming effect (Forster 1998), and thus it is not entirely clear how much the FAE should vary as a function of the prime duration.

To address the concerns about the lack of statistical power and the substantial imprecision in the estimated FAE size observed in experiment 1, experiment 2 was designed to have a sample size that ensures acceptable statistical power to detect the an interaction between priming and frequency, as well as a sample size that reduces the width of the resulting confidence interval compared to experiment 1.

4. Experiment 2

The findings from Experiment 1, as well as those reported by Angele et al. (2023), Cayado, Wray, and Stockall (2023) and Petrosino, Sprouse, and Almeida (2023), establish the feasibility of obtaining masked repetition priming in online experiments with a 33 ms prime duration. However, a crucial question remains: can we reliably detect the FAE in web browser-based settings? Experiment 2 directly addresses concerns about the potential statistical power limitations observed in Experiment 1 and much of the prior literature. Specifically targeting what we construe as the smallest theoretically interesting FAE (5ms), we recruited a larger sample size, as determined by a power analysis. We simulated 10,000 datasets for each of the combinations of two statistical parameters: standard deviation and the correlation between conditions. The latter were kept equal across conditions to simplify the simulations. Based on our own pilot studies and previous published work (Petrosino 2024; Petrosino, Sprouse, and Almeida 2023), the simulations involved standard deviations ranging from 80 to 120 ms (with 10 ms increments), while the correlation between conditions ranged from 0.7 to 0.9 (with 0.1 increments). The sample size varied between 200 and 3,000 participants (with 150 unit increments). Three different FAE sizes were simulated: 15 ms, 10 ms and 5 ms. The first effect size (15 ms) is about half of the ones observed in previous studies that statistically detected the FAE (~30 ms). The second effect size (10 ms) is close to the size of the average frequency attenuation effect found in the literature (13 ms). The last effect size (5 ms) is our lower-bound estimate of a theoretically interesting effect size. The code used for the power simulations, along with the simulated datasets, is available online (https://osf.io/r7d2q/).

Our analysis identified a sample size of 1,250 participants as optimal, ensuring robust statistical power ($\geq$ 80) across various parameter combinations (Figure 1), especially for a raw FAE equal to or greater than 10 ms —- a value closely aligned with the average FAE calculated from previous studies (refer to Table 1). In light of the observed limitations in the temporal accuracy and precision of current online stimulus delivery programs (discussed in Section 3.2.2), which necessitated substantial subject and data exclusion in Experiment 1, we aimed for an intended sample size of 2,600. This decision was made to enhance the likelihood of obtaining a sample size of at least 1,250 participants after applying all the necessary exclusion criteria to the data. In addition, sample sizes exceeding 1,250 can only help increase the precision of the estimated effect size.

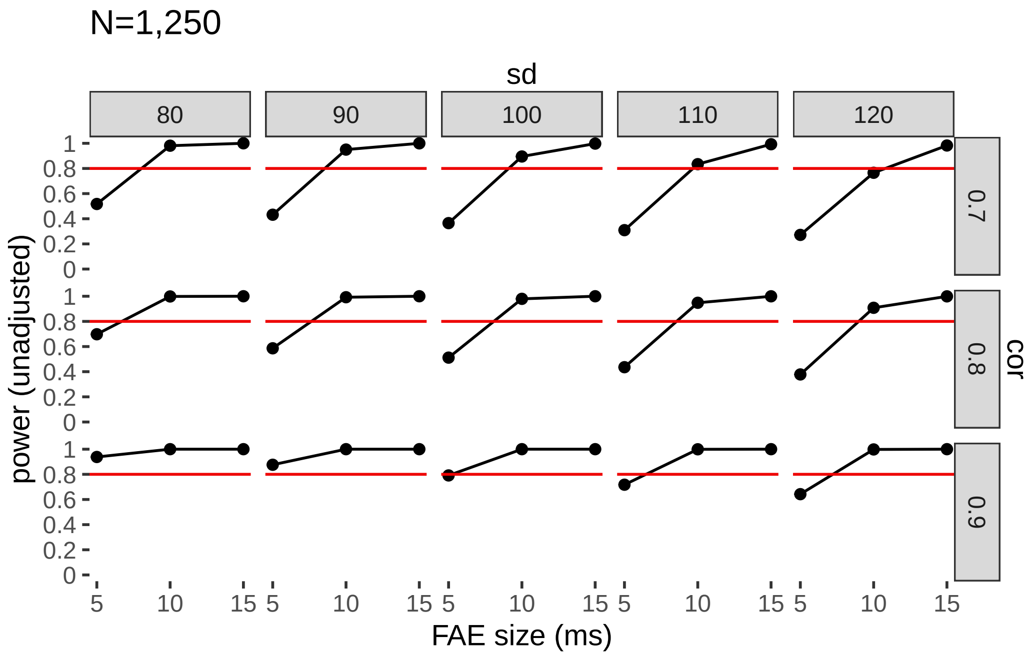

Figure 1. Power simulations with a sample size of 1,250, for all combinations of standard deviation (sd), pairwise correlation (cor), and interaction effect size. The red line identifies the threshold of 80% power.

4.1. *Methods*

4.1.1. Preregistration

We preregistered the results of the power analysis, the goals, the design and analysis plan for experiment 2 prior to data collection. The preregistration, detailing the experimental hypotheses, the desired sample size as well as the planned analyses is available online (https://doi.org/10.17605/OSF.IO/3NFQP).

4.1.2. Participants

Two thousand and six hundred participants (1,445 females; *mean age* = 42, *sd age* = 14) were recruited on Prolific (https://www.prolific.com) with the same criteria specified for experiment 1 (Section 3.1.1).

4.1.3. Design

The experimental design was identical to experiment 1.

4.1.4. Materials

One-hundred and four five-letter words, half of low frequency (between 7 and 24 in the SUBTLEX_{US} frequency per million) and half of high frequency (between 57 and 2,961 in the SUBTLEX_{US} frequency per million) were sampled from ELP (Balota et al. 2007), but this time based on the SUBTLEX_{US} frequency counts rather than HAL (which was used in experiment 1). Table 5 shows that although the SUBTLEX_{US} frequency ranges of the two conditions were very far from one another (similarly to what was done in Experiment 1; Section 3.1.3), they still show some overlap when HAL frequencies are used. As mentioned before, this seems to be a general problem when jointly considering different frequency databases for a smaller set of stimuli that need to be manipulated and controlled in different ways (see also fn. 2 and Adelman et al. 2014). From each condition, fifty words were randomly chosen to be presented as targets and related primes (the *related* condition), and the remaining fifty were presented as unrelated primes (the *unrelated* condition). All words used were monomorphemic nouns, adjectives, or verbs, thus excluding particles, prepositions, and derived or inflected forms.

Table 5. Experiment 2. Descriptive statistics of the word items used. For both frequency databases, the word frequencies were converted to per-million count to ensure cross-comparison.

		HAL				$\mathbf{SUBTLEX_{US}}$			
frequency	N	min	max	mean	SD	min	max	mean	SD
high	52	45	4984	573	808	57	2691	210	388
low	52	6	570	64	93	7	24	13	5

One-hundred and four five-letter, phonotactically legal nonwords were randomly selected from the ELP database as well. Half of them were randomly selected to be presented as targets; the other half was instead used as unrelated nonword primes. None of the nonwords contained any existing English morpheme. Both the words and non-words used in the experiments are reported in the appendix below.

4.1.5. Procedure

Experiment 2 followed the same procedures as experiment 1 (see Section 3.1.4). The median time to finish the experiment was 5 minutes.

4.2. *Data analysis*

Analysis scripts and an abridged version of the data collected can be found online (https://osf.io/vn3r2), and consisted of 297,598 observations in total. We performed the same three steps of analysis described for experiment 1 (Section 3.2).

4.2.1. Step 1: subject and item performance

Item and subject error rates were calculated. The item error rate was never below above 14%, so no item was excluded from analysis. 19 subjects were removed because their error rate was above 30%. Thus, a total of 269,652 observations and 2,593 participants were included in further analyses.

4.2.2. Step 2: prime durations

Prime fluctuations were dealt with in the same way as in experiment 1 (Section 3.2.2). The mean (mean = 32.32 ms, sd = 15) and the median (median = 33 ms) prime durations were closer to the intended value (33 ms). The same prime duration cut-off set for experiment 1 (i.e., any trial whose prime duration was out of the 25-60ms range) removed 13 % of the trials. No participant was excluded, for a total of 237,287 observations.

4.2.3. Step 3: RT distribution

After removing the incorrect responses, similarly to what we did for experiment 1 (Section 3.2.3), 0.51% of the trials were excluded if the relative RT was below 200 ms and above 1800 ms. Finally, 249 subjects were removed because the number of trials within the same condition was less than 7 (i.e., about half of the total number of trials being presented within the same condition, i.e. 13). A total of 210,889 observations and 2,341 subjects were included in the statistical analysis below.

4.3. *Results*

For each frequency condition, priming effects were calculated in the same way as experiment 1. The 2x2 ANOVA (condition, 2 levels: *high* vs. *low*; primetype, 2 levels: *unrelated* vs. *repetition*) revealed significant main effects (condition: $F(1, 2340)=1572$, $p < .0001$; primetype: $F(1, 2340)=1113$, $p < .0001$), and interaction ($F(1, 2340)=52.48$, $p < .0001$). Planned comparisons confirmed statistically significant repetition priming effects for both word conditions (MOP_HF = 18, CI_95% = [16 20], t(2340) = 19.7, $p < .0001$; MOP_LF = 28, CI_95% = [26 30], $t(2340)$ = 27.8, $p < .0001$), with the low-frequency word repetition priming effect being 10 ms larger than the high-frequency word repetition priming effect, and this FAE effect was statistically significant (M_FAE = 10, CI_95% = [7 13]), $t(2340)$ = 7.24, $p < .00011$; Table 6). Close to, yet different from zero negtive priming effects to the non-word condition were also significant (MOP_NW = -2, CI_95% = [-4 0], t(2340) = -2.33, $p < .0001$). As for the word error analysis, we found significant priming effects in the all conditions (high: $t(2340)=9.95$, $p<.0001$; low: $t(2340)=16.9$, $p<.0001$; non-word: $t(2340)=-3.27$, $p=.001$).

Table 6. Experiment 2. Summary of the word priming results. *Legend.* MOP: magnitude of priming. SD: standard deviation. SD_p: standard deviation of the priming effects. ES: effect size (Cohen's d). cor: correlation.

	unrelated RT			repetition RT				priming effects				*t*-test		
factor	mean	SD	Error (%)	mean	SD	Error (%)	cor	MOP	95% CI	SD_p	ES	t	df	p
high	573	83	3	555	85	2	0.860	18	[16 20]	45	0.41	19.7	2340	2.88e-80
low	605	88	6	577	88	3	0.850	28	[26 30]	49	0.58	27.8	2340	1.52e-147
non-word	623	103	4	625	103	4	0.910	-2	[-4 0]	43	-0.05	-2.33	2340	0.0197
frequency:primetype							0.029	10	[7 13]	66	0.15	7.24	2340	5.86e-13

4.4. *Discussion*

Experiment 2 was designed to investigate whether Frequency Attenuation Effects (FAE) can be detected under masked priming conditions (with SOA < 60 ms, here 33 ms). We employed a robust sample size to ensure adequate statistical power for detecting small to medium effect sizes. Our results not only replicated Experiment 1 in revealing statistically significant main effects of repetition for high and low frequency words alike, but also detected a statistically significant interaction: the low-frequency condition yielded priming effects that were 10 ms larger than the high-frequency condition. This value is within the 95% CI from experiment 1, making it a successful replication of that result. The 95% CI of experiment 2 ranges from 7 ms to 13 ms. This is notable because it includes the effect size of experiment 1, but is also quite narrow (a halfwidth of 3 ms). This indicates that, for the frequency ranges investigated in experiment 2, the FAE is unlikely to be smaller than 7 ms or larger than 13 ms when the prime duration is 33 ms. In contrast, experiment 1 had a halfwidth almost three times as large (8 ms).

The absence of a robust non-word masked priming response has been used as an additional piece of evidence supporting the view that the masked priming response is devoid of episodic influences (e.g., Forster 1999). The results of experiment 2 align with the previous evidence (including that of experiment 1) in showing at best very small inhibitory masked repetition priming for non-words, with very high precision: the 95% CI indicates the plausible range for the masked repetition priming effect for non-words to be between -4 and 0 ms when prime duration is 33 ms.

5. General discussion

The repetition priming response stands as a cornerstone in psycholinguistic investigations, offering insights into the mechanisms governing word recognition. An ongoing debate surrounds the interpretation of these effects, particularly concerning their source in the memory system. On the one hand, *interactive activation models* (McClelland and Rumelhart 1981; Grainger and Jacobs 1996; Coltheart et al. 2001) posit a lexical source for repetition priming effects, either in terms of temporarily raised resting activation levels for lexical nodes in unmasked priming, or as a head start in the retrieval process in masked priming. *Episodic* and *memory recruitment models* (Jacoby and Dallas 1981; Jacoby 1983; Bodner and Masson 1997; Masson and Bodner 2003; Bodner and Masson 2014) on the other hand, invoke a non-lexical source for the repetition effect, namely an episodic or episodic-like memory resource formed upon brief exposure to the prime word that can be recruited during the processing of the target item. Crucially, both models predict a single mechanism underlying masked and unmasked priming. Differential mechanisms between unmasked and masked repetition priming, however, are predicted by the *entry-opening model* (Forster and Davis 1984), which propose both lexical and episodic sources of priming effects.

Thus, the existence of qualitatively distinct outcomes in masked and unmasked priming presented a direct challenge to some, but not all of these models. One such finding is the *Frequency Attenuation Effect* (FAE), in which higher frequency words exhibit smaller repetition effects compared to lower frequency words. The FAE has been described as observable only in unmasked priming since the work of Forster and Davis (1984), who demonstrated that when the prime word is presented very briefly (SOA < 60 ms), it becomes masked by the target word, and this is hypothesized to prevent the conscious encoding of the prime. Under such conditions, the FAE purportedly disappears. Forster and Davis (1984) argued that this potentially shows that the FAE is subserved by a different type of memory source (perhaps episodic) than the masked repetition priming response. This conclusion, however, is the source of ongoing debates (see Table 1 for review of past findings), which the two experiments reported here were meant to address.

Within this research landscape, our experiments targeting the frequency sensitivity of the repetition effect under masked conditions contribute methodological and theoretical insights. Methodologically, our results help establish the viability and reliability of online data collection for the masked priming paradigm. Building on the work of Angele et al. (2023), Cayado, Wray, and Stockall (2023) and Petrosino, Sprouse, and Almeida (2023), we addressed pitfalls in implementing and analyzing masked priming data collected online, and by doing so offered a solution to the longstanding problem of low statistical power and lack of estimation precision when it comes to investigating phenomena with effect sizes that are harder to detect statistically, like interactions in factorial designs. However, this newfound opportunity necessitates careful data scrutiny.

In the same vein, the FAEs observed in experiments 1 and 2 have important theoretical ramifications. The historical belief in the non-observability of FAE in masked priming primarily arose from a lack of statistically significant results, possibly rooted in outdated frequency corpora or inadequate statistical power. Our design addressed these concerns, yielding statistically significant FAE results aligning with the literature's average effect (see Table 1; the 95% CI implies that the FAE is unlikely to be larger than 13 ms with a 33 ms prime duration). These results challenge the supposed qualitative distinction between masked and unmasked repetition priming cleaved by the FAE, complicating the rejection of single-mechanism theories, and suggesting that *interactive-activation models* and *memory recruitment models* may yet offer unifying explanations for masked and unmasked priming.

Similarly, our results also challenge the entry-opening model's prediction of the absence of FAE in masked priming. One potential way of dealing with this in the *entry opening model* is to claim that masked priming severely reduces, but does not entirely eliminate, the use of sources other than lexical memory (see Forster 1998; Forster, Mohan, and Hector 2003, for proposals along this line). Alternatively, within the entry-opening model, the results of experiment 2 may be explained by a frequency-based mechanism occurring in the fast search stage. Something along these lines was actually already hinted at by Forster and Davis (1984) themselves, and consisted of a procedure whereby the entry of a prime word is promoted to the top position of the search list. As a consequence, low-frequency words (which are fairly low in the search list) will benefit from such promotion procedure more than high-frequency words (which are instead already in higher positions), thus ultimately giving rise to the FAE.

While our findings present a compelling case for the presence of FAE in masked priming that is seemingly parallel to the unmasked case, questions about potential mechanistic differences persist. The larger sample size needed for masked FAEs raises intriguing considerations about the influence of memory sources and warrants further investigation. For example, there is independent evidence for different mechanisms in masked and unmasked repetition priming from RT distributional analyses (cf. Gomez, Perea, and Ratcliff 2013) that suggests that repetition priming under masked conditions affect primarily the encoding stage of the stimulus. Given that frequency is often associated with facilitation of encoding, our results could help support this view. Additionally, the trivially small inhibitory effect sizes of non-word masked repetition priming in experiments

1 and 2 align with the trend (overwhelmingly shown in the literature) that facilitatory effect may be exclusive to unmasked designs (Forster 1998; Forster, Mohan, and Hector 2003; but see Masson and Bodner 2003), and suggests avenues for future exploration on large-scale.

Finally, the finding that the FAE occurs under masked priming conditions may impact our understanding of masked morphological priming. In this literature, there is a unresolved question about the ability of affixes to elicit masked morphological priming results (for a review, Amenta and Crepaldi 2012). In English, the evidence seems to indicate that only stems, but not affixes, have the ability to prime entries across the lexicon. This finding can and has been used to support models in which affixes are initially stripped before stems are accessed in the lexicon (Taft and Forster 1975; Forster and Azuma 2000; Stockall and Marantz 2006). However, stems and affixes do also have a large frequency imbalance, with most affixes being substantially more frequent that most stems. The observation of FAE under masked priming can provide an alternative reason for why masked stem morphological priming is well attested but masked affix morphological priming is not: the latter could be due to a ceiling frequency attenuation effect. This is an intriguing possibility that must be left for future work to explore.

In summary, our study successfully replicated and expanded upon the work of Angele et al. (2023), Cayado, Wray, and Stockall (2023) and Petrosino, Sprouse, and Almeida (2023), confirming the viability of observing repetition priming effects in masked priming experiments conducted online with a brief Stimulus Onset Asynchrony (SOA) of 33 ms. Notably, we addressed a lingering question in the literature by establishing the presence of the Frequency Attenuation Effect (FAE) under masked conditions. The use of large online samples proved instrumental in overcoming the longstanding challenge of insufficient statistical power to detect interactions in factorial designs, which we believe had impeded previous investigations into detecting the FAE in masked priming.

These results not only contribute to our understanding of masked priming but also open up intriguing avenues for further research. The ability to harness extensive online samples provides a valuable opportunity to explore and illuminate unresolved issues across various domains where masked priming is a crucial research tool, underscoring the potential for online experimentation to advance our knowledge and resolve longstanding questions in the field.

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Wordlists

Experiment 1

unrelated prime	word	RT (to repetition) mean	RT (to repetition) SD	RT (to unrelated) mean	RT (to unrelated) SD
low frequency condition					
smash	chasm	714	216	831	250
manna	oxide	719	198	715	156
legit	vowel	655	139	694	152
blunt	clerk	617	157	635	133
slope	bleed	609	171	621	176
nasal	decor	654	140	694	204
forte	quirk	689	204	688	155
aloud	speck	732	208	739	187
nymph	stash	638	175	657	142
crass	ditch	671	173	678	157
squid	snare	684	168	722	164
swirl	budge	672	200	732	207
grunt	slack	608	129	664	157
taunt	sedan	711	197	705	122
cigar	tally	667	131	720	176
lunge	posit	–	–	–	–
negro	flock	654	141	716	166
exert	scorn	670	159	651	146
lathe	grail	697	206	718	171
viola	bloat	663	185	698	181
rival	tumor	627	159	651	152
dizzy	acute	662	174	660	142
hertz	sauna	652	132	706	154
haste	elect	640	162	650	144
poppy	spoof	706	185	759	201
clove	plush	615	138	669	175
guise	fiend	785	209	846	185
magma	knelt	744	213	814	225
lotto	privy	733	182	777	219
kayak	sigma	798	258	796	205
taint	parse	–	–	–	–
fanny	carte	–	–	–	–
rouge	verge	664	168	672	171
vitro	mourn	665	171	682	186
floss	shrug	687	175	682	132
tempt	clasp	658	128	701	178
flirt	bathe	659	159	701	197
fluff	linen	620	91	650	133
butch	stare	617	126	632	144
bowel	medic	637	166	663	218
aspen	weave	614	128	649	128

chime	flint	681	140	718	191
crust	flank	689	176	740	177
spunk	scrub	645	172	670	167
stoke	hoist	686	168	724	190
dairy	stout	667	148	707	166
stale	cough	588	147	629	157
gypsy	annex	744	197	798	169
gloss	plume	730	195	775	194
topaz	quart	662	159	715	205
high frequency condition					
shoot	proof	576	119	617	129
usual	clear	598	169	588	120
teach	audio	589	141	632	112
adult	apply	592	154	632	130
allow	phone	573	143	588	89
forum	class	656	162	682	197
whole	raise	611	154	598	116
often	civil	580	107	623	120
issue	match	590	119	619	169
style	local	589	141	580	113
coast	minor	600	137	632	157
reach	below	611	143	618	90
smith	extra	599	146	609	141
speed	court	585	115	638	141
sense	exact	592	127	590	113
write	bunch	647	140	646	130
trust	quick	554	104	616	134
sleep	birth	619	165	609	156
reply	truth	579	140	611	150
track	serve	611	136	649	168
dream	trade	606	185	602	106
image	heart	592	159	602	113
white	index	606	111	625	146
flame	cable	583	119	626	130
value	break	605	163	601	133
avoid	woman	576	119	609	153
short	front	587	138	619	140
aware	voice	562	127	585	116
large	stock	596	148	661	216
prove	seven	583	130	653	193
brand	blood	568	109	598	109
river	plain	596	115	617	123
guess	solid	643	158	612	140
month	limit	603	122	658	136
heard	scale	632	144	639	176
space	stuff	623	133	642	154
leave	major	599	139	585	123
agree	brown	591	120	632	167
metal	house	552	121	603	137

along	stage	590	138	619	160
print	built	628	155	664	166
worst	video	570	113	650	157
sound	story	594	129	614	176
faith	march	607	134	630	191
quote	clean	553	93	585	135
train	price	599	141	624	189
small	event	583	127	623	166
night	thank	656	190	607	128
shell	radio	577	131	604	162
alone	sorry	592	155	609	140
non-word					
strat	inurt	726	259	712	215
gleat	shawt	760	270	672	154
dolio	delax	758	182	767	195
cutch	thelp	745	242	687	199
greaf	isapt	645	181	628	160
broot	fopaz	660	196	628	125
lubic	fuxom	676	234	601	126
drirk	bloot	761	190	744	172
cooch	scart	768	220	726	162
motem	frint	720	203	685	148
abapt	ahuck	673	207	633	153
nigit	netro	734	217	721	169
hilac	moust	744	186	798	174
cojex	barsh	731	216	706	183
prilt	avort	710	196	725	199
whirp	venem	–	–	–	–
shino	grack	743	209	728	182
nelch	ranth	681	174	654	135
exulk	frick	–	–	–	–
morex	nohew	683	197	656	165
tamek	pramp	745	239	696	200
miant	altep	664	179	654	159
bloth	scrib	788	243	749	230
bumbo	tumph	785	204	768	210
occut	dorst	686	168	674	184
topec	thint	754	205	748	153
shoof	rourt	691	192	688	194
spack	smout	759	195	736	184
blenk	kayuk	823	289	772	237
silaf	drick	727	189	678	131
crunk	smoop	710	185	684	154
fluck	deirm	649	161	657	178
ghisk	ephic	787	223	751	212
chrik	glurp	731	209	727	236
cetup	blumb	746	183	733	220
firch	eicht	725	226	718	205
vasem	forim	736	214	690	185

earch	slent	840	207	773	178
blont	lepot	693	203	659	162
ecret	plock	763	222	734	195
wateb	ocheb	643	168	620	130
trook	febut	659	166	632	156
ruzak	coreb	656	169	643	133
theet	frath	738	193	699	148
blamp	eggem	705	190	681	160
lambo	gredo	700	217	689	182
aliom	brost	728	204	690	170
brust	ganic	712	178	660	117
cleot	polep	714	236	641	174
lindo	snock	766	194	776	206
driff	fomit	711	187	633	147
wrast	sholf	665	157	642	111
lidst	racef	668	167	658	171
huirk	thamp	711	188	708	226
pumbo	purso	702	196	665	168
whilo	glarm	765	210	748	184
murkt	fingo	707	164	683	179
steck	gotch	–	–	–	–
molax	spuff	745	198	692	151
ronch	schew	811	294	756	265
guesh	humot	690	175	674	149
snump	sgrew	706	212	724	175
fleak	fadio	713	175	678	141
recup	plint	768	246	735	225
loast	pheek	696	181	676	192
smalt	blasm	785	226	755	175
swimp	reash	780	187	754	181
tymph	chank	798	229	774	221
laget	septh	721	196	688	193
gluck	feeth	756	191	720	156
gatob	tosit	683	184	668	210
sauto	exuct	767	232	693	191
crunt	ethym	724	211	700	213
pranc	feght	718	187	723	203
twank	stoff	709	165	688	155
letap	cruck	742	197	812	229
alash	fatho	643	146	660	184
sharf	firsh	717	168	717	196
frimp	paltz	688	211	719	227
lumpo	thark	683	134	714	205
huilt	aufit	638	146	649	184
brosk	hinup	636	126	653	142
dulch	jongo	681	181	705	202
dealf	guast	670	178	687	210
drash	sunch	697	196	692	190
prock	cleak	766	177	819	214
spaft	stram	720	157	726	155

criex	etuip	620	138	635	177
phumb	opert	750	225	791	255
denet	keach	670	176	700	189
bluck	umarm	719	213	756	239
racet	tooch	739	213	741	234
phrap	chuth	682	152	726	208
wight	tedic	695	196	704	199
lorro	mutch	796	257	811	279
oorph	hilth	682	195	711	213
praph	pluff	–	–	–	–
aboot	widet	799	222	818	251
hoest	scook	721	168	749	201
polic	fisco	797	261	797	271
glunk	gamit	751	257	725	243
letch	phasm	–	–	–	–
spink	sondo	679	168	672	182
dippo	vuint	634	137	616	130
astef	rynic	629	123	658	161
tatch	waget	736	205	747	211
shoop	vooch	671	158	691	169
isloo	guilm	675	179	719	210
scack	elsom	686	195	704	248
bliff	crost	718	190	731	199
cempo	alept	754	186	780	216
glaim	robit	741	206	783	220
thunt	noast	658	122	688	160
plesh	bealm	740	175	759	204
thoop	hyrup	703	125	741	191
louth	chost	752	192	778	209
preak	borif	617	111	616	130
creck	starp	751	215	744	208
realp	valif	656	178	678	190
ferit	raceb	674	203	687	174
theep	dacit	642	171	649	179
murch	abert	733	190	765	233
blomp	paith	703	161	724	170
sloup	mough	710	145	719	145
strit	plick	768	218	793	232
skinp	toost	763	198	786	218
phock	tacao	751	217	778	285
cyrrh	kneak	790	212	826	228
ahack	vitch	682	155	717	238
saist	paxim	647	152	667	185
pheep	kingo	734	167	738	175
ehert	truff	767	218	771	246
spuck	fundt	655	162	700	183
antuc	bloam	719	155	741	191
shish	quilp	726	217	718	233
gijou	fotch	658	127	661	132
drarp	broup	674	161	690	213

stilp	krauf	683	183	683	200
doint	swaft	826	286	821	253
owlut	adoof	726	176	724	191
swant	meash	722	195	776	230
vepot	afent	660	151	651	180
ploic	setip	705	198	710	203
glick	linew	769	207	794	242
hatex	corax	696	162	755	218
framo	scock	811	233	807	232
praft	quast	733	193	763	211
minch	ipept	685	201	691	209
ragic	gonet	658	172	692	203
stabt	lertz	629	153	652	155

Experiment 2

		RT (to repetition)		RT (to unrelated)	
unrelated prime	word	mean	SD	mean	SD
low frequency condition					
hunch	arrow	590	130	587	124
sneak	pitch	576	126	612	122
widow	hatch	621	151	639	148
brief	shark	573	125	590	138
sharp	tooth	536	125	565	116
grief	booth	572	136	627	157
sting	pound	551	127	572	127
thief	weigh	593	167	636	164
avoid	blank	571	139	596	124
award	crush	554	128	592	136
smack	bench	573	132	601	129
brand	fetch	622	156	658	146
salad	cheek	561	141	602	142
swamp	brush	564	130	600	128
depth	march	559	125	580	123
flesh	bleed	560	148	577	146
harsh	cliff	602	130	645	137
creep	fraud	621	147	628	132
plead	cloud	536	115	551	101
thumb	fluid	605	140	678	162
creek	trash	554	127	560	128
blond	flush	576	123	617	140
stink	porch	587	136	620	160
patch	stiff	626	154	678	156
sweep	cough	564	142	601	141
squad	smash	570	129	587	126
high frequency condition					
chief	blood	541	130	551	104

child	bunch	585	148	617	145
board	catch	545	116	562	130
tough	stuff	555	119	585	137
stand	break	545	107	561	124
beach	speak	545	131	573	129
hotel	stick	562	128	598	138
angel	sleep	538	113	559	119
truth	wrong	563	143	565	132
quick	grand	571	127	582	143
world	mouth	543	125	556	119
extra	knock	560	134	631	136
think	guard	580	132	590	134
thing	small	557	130	577	125
round	check	558	135	562	121
proud	watch	541	128	546	110
smell	group	559	127	576	142
earth	month	555	120	572	123
relax	south	575	139	611	133
truck	lunch	547	119	557	125
throw	clock	548	132	574	124
death	sound	538	127	552	103
north	drink	559	129	556	122
young	touch	541	122	573	121
weird	laugh	546	119	568	121
reach	black	553	131	563	114
non-word					
grack	alkew	599	153	591	140
furob	agink	626	148	614	141
begro	ruzak	577	130	584	142
labok	sondo	625	142	612	149
gazzo	guesh	702	184	721	194
criam	fadio	618	149	604	146
coreb	plich	650	162	640	159
docab	sgrew	626	182	638	182
colob	sceak	675	154	683	171
isloo	ghisk	588	139	593	139
ahuck	deirm	589	142	596	139
flurb	villo	632	182	615	181
pikto	tidow	648	167	624	160
aliom	drick	684	168	681	172
purso	phick	643	160	637	165
borno	nello	625	156	612	151
pacaw	feach	730	201	720	192
rilth	tello	651	175	644	171
caveb	dolio	602	148	610	165
swysh	gorgo	643	164	619	170
lanjo	whilo	612	137	604	150
drief	stanf	611	134	617	133
ocheb	crulk	671	162	665	169

tunch	phumb	645	160	633	148
steaf	sirth	612	141	618	145
nohew	slerk	640	153	634	163
nualm	vitbo	593	151	596	154
ofium	sunch	665	165	665	161
croik	soeth	589	141	589	130
valuo	eltow	628	171	606	158
sorgo	framo	617	146	618	146
shavo	lumpo	630	162	635	172
oceab	spuff	672	169	667	183
tolio	gatob	599	139	606	155
theck	nosom	598	155	604	139
tooch	gezzo	592	136	586	131
slonk	afoub	582	133	589	128
salch	wateb	633	151	619	133
raceb	nelch	601	144	594	145
ahack	dahoo	598	132	595	146
fideo	driek	606	145	606	143
fluko	gnask	612	171	604	153
cyrrh	brosk	629	159	647	175
revuo	duvez	580	152	580	155
cempo	fielm	609	146	611	151
exulk	pumph	669	162	685	176
kleck	gerif	584	137	588	149
bonth	racef	618	151	622	156
scook	pheek	640	155	644	176
slork	pruaw	593	133	592	135
whilf	guilm	603	142	598	142
drosh	lairf	587	144	600	150