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 addresses the controversy surrounding whether masked repetition priming is sensitive to word frequency, the Frequency Attenuation Effect (FAE). Early findings suggested no such interaction, but recent studies have contradicted this. This uncertainty is consequential, as word recognition models differ in whether they predict this effect, and how they account for it in case it does exist. In two experiments employing very large sample sizes (Ns=2600), we show that the FAE does occur in masked conditions. In experiment 1, we observed a FAE of 10ms with a short prime duration (33ms). In experiment 2, we replicate and extended this finding with a longer prime duration (50ms). While the magnitude of the repetition priming effects increased with prime duration, the FAE remained constant. We contend that no word recognition model can currently account for this pattern of results, but the entry-opening model may be modified to accommodate these findings.

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

1. Introduction

Visual word recognition is a complex cognitive function, as written words engage various visual and linguistic processes and, in the case of fluent readers, do so in a remarkably fast and accurate manner. Among many tools that have been developed to study visual word recognition, one of the most influential is masked priming. Initially developed to help disentangle automatic and episodic contributions to priming in lexical decision tasks (Forster and Davis 1984), the masked priming procedure has since proved valuable to uncover the conditions under which orthographic, phonological, morphological, and semantic information impact access to visual word forms, by mitigating strategic effects and minimizing the influence of controlled processes (Forster 1998, for review). The masked priming procedure generally involves a forward mask (i.e., usually a string of

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hashes, #####), followed by a prime string presented in lowercase for very short time (SOA < 60 ms), and a target string presented in uppercase immediately after (##### - *prime* - *TARGET*) to which participants are expected to respond. Because the prime presentation is so brief and masked by preceding and subsequent stimuli, most participants report no awareness that a prime string has been presented (Forster, Mohan, and Hector 2003; Nievas 2010).

One of the most robust findings in the masked priming literature is the *repetition priming effect*: when the prime and target are the same string (albeit in a different case, e.g., *word-WORD*), the response to the target is facilitated compared to when the same target is preceded by an unrelated prime (e.g., *phone-WORD*). The fact that the size of masked repetition priming effect is proportional to the duration of the prime (Forster, Mohan, and Hector 2003; Angele et al. 2023) has led some researchers to posit that the masked repetition priming effect reflects a type of "head start" in the processing of the target.

In *Interactive-Activation* (IA) models (McClelland and Rumelhart 1981; Grainger and Jacobs 1996; Coltheart et al. 2001), this "head start" is conceived of as increased activation of the target entry as a function of its prior activation during the presentation of the prime. These raised activation levels in turn help the entry reach the recognition threshold faster.

Alternatively, *search models* like the *entry opening model* (cf. Forster, Mohan, and Hector 2003) posit an *entry opening time* (EOT) that is uniform across lexical items. In this model, the target's EOT is entirely or partially saved due to the fact that the entry opening process is effectively initiated during the presentation of the prime.

Finally, *retrospective models* of priming, such as the *memory recruitment model* (Bodner and Masson 1997; Masson and Bodner 2003; Bodner and Masson 2014), posit a non-lexical source for the masked repetition priming effect. Under this view, masked repetition priming stems from the exploitation, at post-access stages, of a memory resource created by the encounter with the prime word.

Table 1. Summary of the masked repetition priming effects as a function of word frequency reported in the literature. The statistical power range estimates were calculated by simulation with the corresponding sample size (N) and for two representative FAE magnitudes. Simulations were performed across a range of correlation values between conditions (from 0.6 to 0.9, in increments of 0.1) as well as plausible standard deviations per conditions (from 60 ms to 180 ms, in increments of 10 ms), with 10,000 simulated datasets for each combination of parameters.

				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]
Wu (2012), exp. 5	English	64	60	31	64	33	*	[0.04 0.87]	[0.12 1]
Wu (2012), exp. 5	English	64	40	33	51	18	*	[0.04 0.87]	[0.12 1]
Mean				40	55	15			
SD				10	14	13			
Correlation						0.41			

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

1.1. ***Does frequency interact with repetition in masked priming?***

The masked repetition effect is a robust finding, but there is still considerable uncertainty about whether lexical frequency impacts masked priming. In unmasked contexts, it is well established that high-frequency words often benefit less from repetition than low-frequency words (*frequency attenuation effect*, henceforth FAE; Scarborough, Cortese, and Scarborough 1977). The precise *locus* of this effect, however, remains the topic of debate. Three types of models have attempted to explain the FAE in different ways.

IA models (McClelland and Rumelhart 1981; Grainger and Jacobs 1996) capture the FAE by positing that once a word is recognized, it takes time for its activation levels to return to their normal resting state. If the same word is then encountered again while still highly activated, it will cross the recognition threshold much faster compared to when it is at its usual resting activation levels. In this case, the facilitation effect gained by repeating the same word is expected to be additive with word frequency, and should therefore amount to the same size for both high- and low-frequency words (Figure 1, A) . Alternatively, these models could propose that each access creates a small but long-term change in the resting activation levels of the lexical node, so that prior exposure will always lead to some amount of priming. In this type of model, the FAE does arise because low frequency words are hypothesized to have a much lower resting activation level compared to high frequency words. Thus, any increase activation level will benefit low frequency words more compared to high frequency words (Figure 1, B).

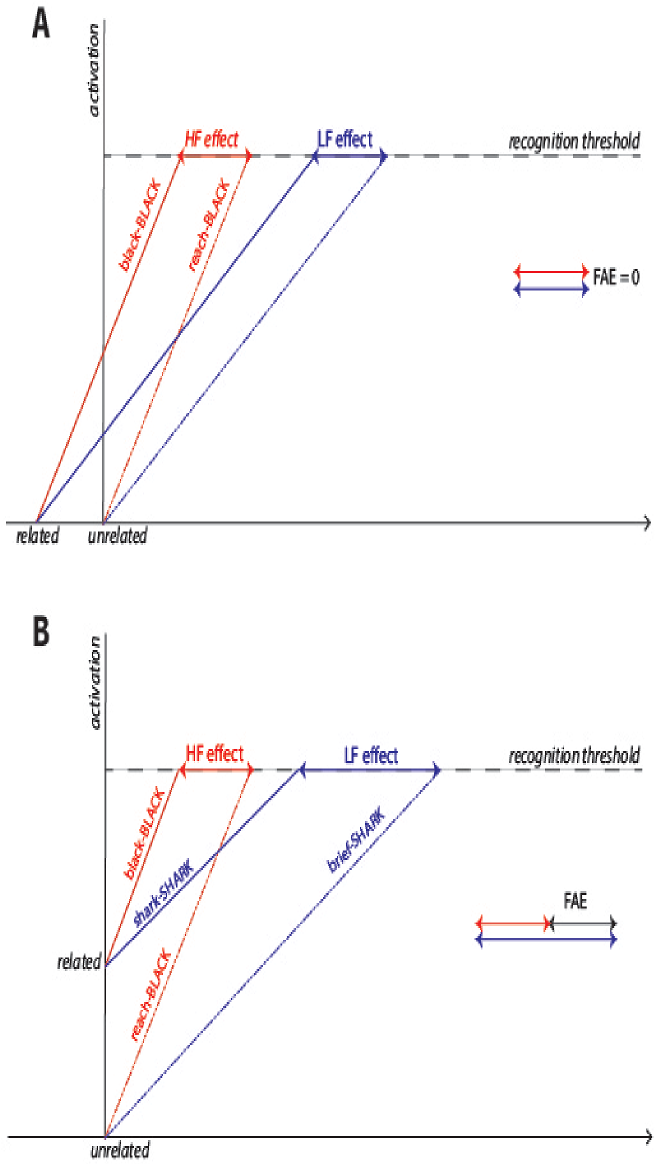


Figure 1. Additive (panel A) and interactive (panel B) effects of word frequency and repetition priming as explained in interactive activation models. In general, the lexical representations of high-frequency words (e.g., *black*) activate faster than those of low-frequency words (e.g., *shark*), and is triggered as soon as the corresponding activation value reaches the recognition threshold. Priming effects are encoded as the difference between the condition in which the presentation of the target is preceded by an unrelated word (unrelated condition, e.g. *brief-SHARK*) and the condition in which the target is instead preceded by itself (related condition, e.g. *shark-SHARK*). In panel A, priming arises just as a result of the prior presentation of the related prime, which provides a head start that is independent of word frequency. In panel B, frequency is assumed to modulate the resting activation level of the word, thus impinging on the size of the priming response and ultimately giving rise to FAEs.

Search models like the *entry opening model* (Forster and Davis 1984; Forster 1998), on the other hand, predict that candidate entries for recognition are evaluated according to their rank order of frequency (Figure 2). Because this is a relatively stable property of the lexical system, it is unlikely to radically change based on one or even a few entry accesses. Thus, these models predict that the FAE must have a different source that is not lexical. In this, they agree with *retrospective models* (e.g., Jacoby and Dallas 1981; Jacoby 1983; Bodner and Masson 1997; Masson and Bodner 2003; Bodner and Masson 2014), according to which it is the activation/retrieval of the episodic memory trace of the encounter with the prime word that is responsible for facilitating the behavioral response; the FAE under this view stems from the independently established fact that low-frequency words exhibit an advantage in episodic memory tasks (like *old-new* tasks; Scarborough, Cortese, and Scarborough 1977). Thus, a direct prediction of this view would be that, if it were possible to minimize episodic influences, then no FAE should be expected. Forster and Davis (1984) proposed that masking the priming stimulus could achieve this effect, adapting the procedure of Evett and Humphreys (1981). Forster and Davis (1984) established that no FAE arises when the primes are effectively masked, corroborating their prediction that, being a post-lexical, episodic memory effect, the FAE should only be observed when the primes are fully visible and their context is somehow relevant for the behavioral decision to the target. This finding has since often been replicated (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; Norris et al. 2018).

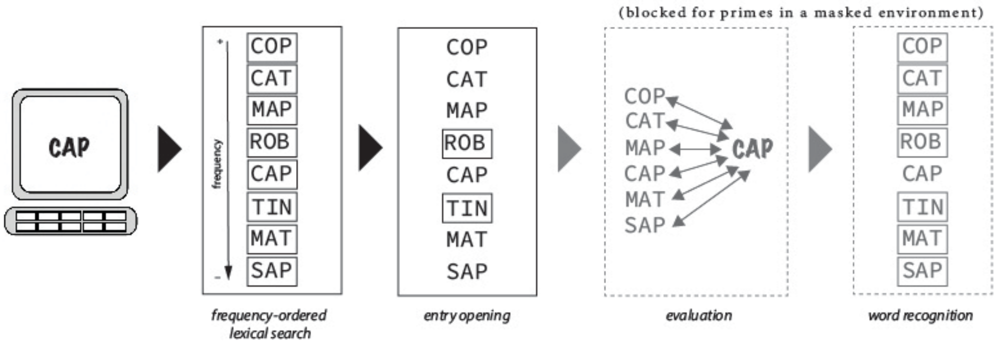


Figure 2. Graphic representation of the entry opening model (Forster 1998, 1999). As soon as a word is presented, the parser goes through a frequency-ordered lexical search and only the entries that are close and perfect orthographic matches with the stimulus are opened (entry opening stage). In the figure, the opened entries are represented as borderless text boxes. Subsequently, each opened entry is strictly compared to the stimulus (evaluation stage, which is blocked for prime words in a masked environment). All entries except the actual match are closed again, thus leading to word recognition.

However, as Table 1 shows, there are, contrary to the earlier literature, a number of recent studies that do report significant FAEs in masked repetition priming (Bodner and Masson 2001; Kinoshita 2006; Norris and Kinoshita 2008; Nievas 2010; Wu 2012). Bodner and Masson (2001) argues that when the processing of stimuli is made harder by an alternating case presentation (e.g., *pHoNe*), the associated increase in difficulty to perform the lexical decision 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, as predicted by their *memory recruitment* model.

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, 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, as were Wu (2012) (exp. 5) and Nievas (2010) in Spanish (exp. 1B).

This uneven pattern of findings is puzzling. However, as Table 1 shows (see also Kinoshita 2006), it is noteworthy that 17 out of the 20 experiments 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 15 ms larger when compared to that of high frequency words.

In summary, it is somewhat surprising that the status of the FAE in masked priming remains largely unresolved in the literature, given its non-negligible average magnitude across studies and its theoretical significance in elucidating the underlying cognitive processes of masked priming. The experiments reported here were designed put on firmer empirical footing whether frequency attenuation effects can be observed under masked priming, as well as whether such an effect is sensitive to the duration of the prime, which can help distinguish between competing frameworks of visual word recognition.

1.2. *This study*

One potential contributor to past discrepancies about the existence of the FAE in masked priming is the overreliance on the dated Kučera and Francis (1967)'s word frequency database, which 15 out of the 17 English experiments have depended on. This frequency database has consistently demonstrated inferior predictive performance in psycholinguistic experiments, 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).

Another factor that might have contributed to the conflicting past findings is the potential underpowered nature of many previous studies, as already noted by other researchers (Bodner and Masson 1997, 2001; Masson and Bodner 2003; Adelman et al. 2014). This is a reasonable concern, as interaction effects like the FAE often require larger sample sizes for reliable detection compared to main effects (Potvin and Schutz 2000; Brysbaert and Stevens 2018). 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 standard deviations (in only 7 out of 20 experiments) and the complete absence of reporting of the correlation structure between conditions complicates power assessments. In our own simulations (full details in supplementary material) 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. 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.

Thus, if a non-negligible number of low-powered experiments is in the scientific record, it may play a role in accounting for the uneven pattern of results reported thus far. First, if low-powered experiments are prevalent, it increases the risk of observed statistically significant effects being spurious (Button et al. 2013). At face value, only 6 out of 20 experiments report a statistically significant FAE; and two of these employed a unique alternating-case stimulus presentation (Bodner and Masson 2001; Masson and Bodner 2003). Among the studies reporting non-significant FAEs, eleven still exhibit numerically larger repetition effect sizes for low-frequency compared to high-frequency words. The observed pattern is difficult to reconcile with the purported absence of interaction between frequency and masked repetition priming. The average FAE across all studies stands at 15 ms, a non-negligible effect size.[1] These considerations suggest that a genuine FAE may indeed exist in masked priming, but might be smaller than the magnitudes that are statistically detectable in 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 caution this effect could simply be an orthographic neighborhood effect masquerading as a frequency effect, due to the high correlations between the two variables. The lack of clarity surrounding the statistical power of previous studies makes it difficult to discern whether past statistically significant findings may be spurious.

Second, 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 case. 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 up to a 5% chance of incorrectly determining the direction of the effect.

The study reported here was designed to mitigate these two major confounding issues: overreliance on the Kučera and Francis (1967) frequency counts, as well as the potential lack of statistical power in previous research. With respect to the concern about the unreliable nature of the Kučera and Francis (1967) corpus, we exclusively sourced materials from the SUBTLEX$_{US}$ database (Brysbaert and New 2009), which reflects more recent linguistic usage and offer better validation in behavioral experiments (e.g., Brysbaert and New 2009; Yap and Balota 2009; Brysbaert and Cortese 2011; Gimenes and New 2016; Herdağdelen and Marelli 2017). In order to avoid any issues with statistical

[1] And it is indeed significant, under the naive assumption that the two conditions are similar enough across experiments: $M_{FAE} = 15ms, CI_{95\%} = [8\ 21];\ t(19) = 4.89, p = .0001$.

power, we recruited two extremely large samples ($Ns = 2600$) to achieve high statistical power for an interaction effect like the FAE (see supplementary materials) and narrow margins of error around the estimated effect size (Maxwell, Kelley, and Rausch 2008).

1.3. ***Web browser-based masked priming***

Achieving the desired level of statistical power required a sample size that would be impractical to pursue in traditional lab settings, typically constrained by access to limited research computers and participant pools. In response to this challenge, the present studies were 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).

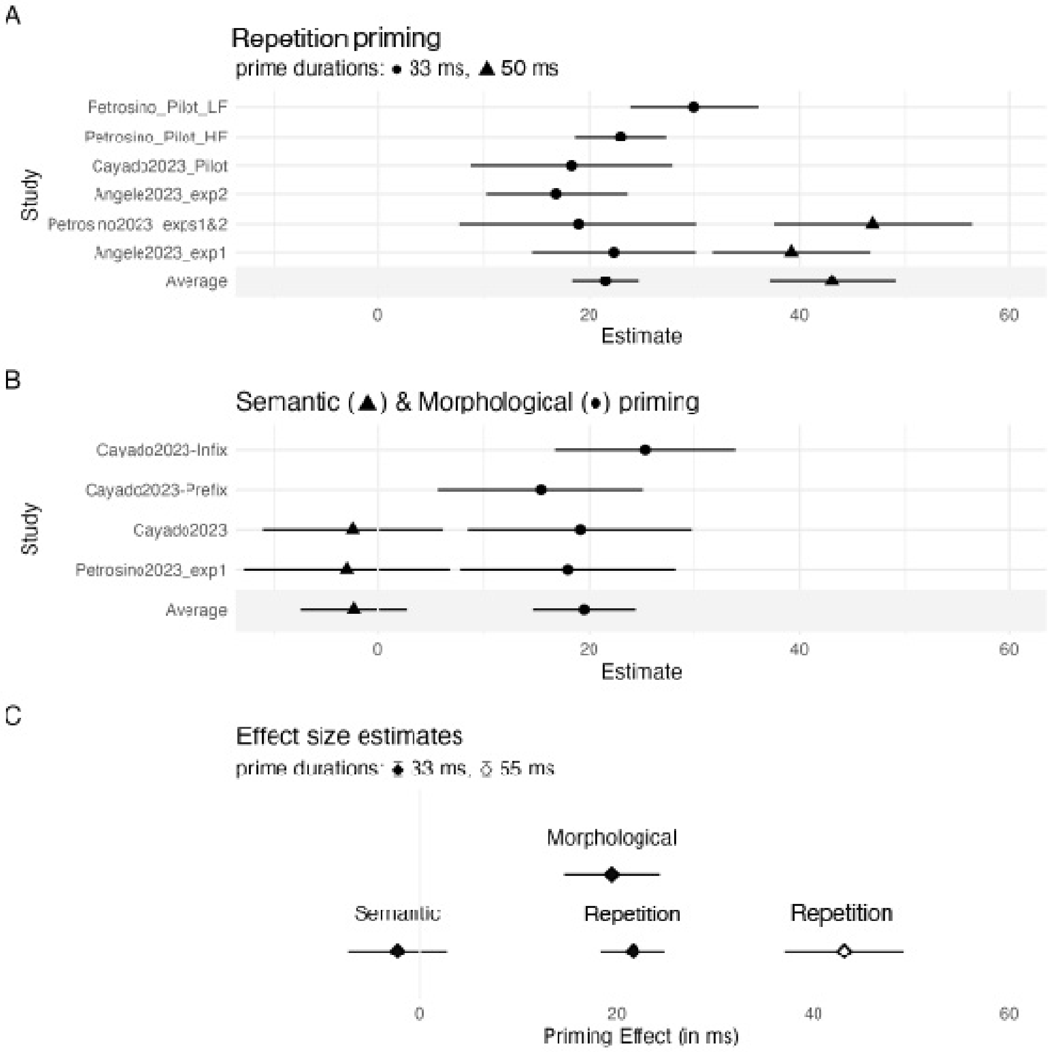


Figure 3. Meta-analysis of three recent masked priming experiments conducted online, together with unpublished pilot data from our lab, investigating three different phenomena: identity, morphological and semantic priming, at 33ms and 50ms prime durations.

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*. Crucially, a meta-analysis (Bonett 2009) of their results (Figure 3; full details in the supplemental material) show a clear replication of four basic benchmark results in masked priming: detection of (i) repetition and (ii) morphological priming with (iii) no evidence of semantic priming, as well as (iv) a proportionality effect of repetition priming and prime duration.

Building on these previous findings, experiment 1 attempts to determine whether frequency attenuation effects can be observed under masked priming. Experiment 2 focuses on whether frequency attenuation effects may vary as a function of prime duration.

2. Experiment 1

Experiment 1 was designed to elicit a masked repetition priming response to both high- and low-frequency words. The required sample size to ensure adequate statistical power (>80%) was determined through a power analysis simulation (details and code available as supplemental material at https://osf.io/r7d2q/). For a 10 ms FAE, a sample size of 1,250 participants was identified as necessary to maintain 80% statistical power across a wide range of plausible standard deviations and correlation structures between conditions. Given the relatively untested nature of web browser-based masked priming experiments, we opted for a larger sample size of 2,600 participants. This decision was made to increase the likelihood of retaining at least 1,250 participants after applying exclusion criteria, while also guaranteeing a narrow margin of error around the estimated effect size (Maxwell, Kelley, and Rausch 2008).

The prime duration was set at 33 ms for three main reasons. First, prior studies by Angele et al. (2023), Cayado, Wray, and Stockall (2023), and Petrosino, Sprouse, and Almeida (2023) have demonstrated that a 33 ms prime duration is sufficient to elicit reliable repetition priming effects in web browser-based experiments. Second, this brief prime duration ensures that the prime word is almost never consciously perceived by participants (Forster, Mohan, and Hector 2003; Nievas 2010), providing a more accurate measure of early, presumably automatic processes involved in word recognition. Lastly, previous research on the *Labvanced* platform (Petrosino, Sprouse, and Almeida 2023) has shown that timing inaccuracies and missed screen refreshes can lead to an increased number of trials with actual prime durations exceeding the subliminal threshold (typically considered to be around 60 ms) when the prime duration is set to 50 ms. If a significant number of trials exceed the subliminal threshold, it may prompt participants to adopt experiment-wide strategies, ultimately compromising the observed masked priming response (Zimmerman and Gomez 2012).

2.1. *Methods*

2.1.1. Preregistration

We preregistered the results of the power analysis, the goals, the design and analysis plan for this experiment prior to data collection. The preregistration is available online (https://doi.org/10.17605/OSF.IO/3NFQP).

2.1.2. Participants

Two thousand and six hundred participants (1,445 females; *mean age* = 42, *sd age* = 14) were recruited on Prolific (https://www.prolific.com). 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. However, 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 at the time of this writing is the most used browser worldwide (W3 Counter 2023), and reportedly performs better than any other across operating systems (see

Table 2. Descriptive statistics of the word items used.

		SUBTLEX$_{US}$				**Orthographic *N***				**Accuracy (%)**			
frequency	N	min	max	mean	SD	min	max	mean	SD	min	max	mean	SD
high	52	57	2691	210	388	0	10	3.98	2.60	91	100	98	2
low	52	7	24	13	5	0	11	3.92	2.79	97	100	99	2

Lukács and Gartus 2023).

2.1.3. Design

The masked priming procedure relied on a lexical decision task (LDT), in which a 3 (factor: *high* vs *low* vs *nonword*) x 2 (primetype: *repetition* vs *unrelated*) factorial within-subject design was used. The dependent variables were lexical decision latency (RT, in milliseconds) and error rate (in percentages).

2.1.4. Materials

One-hundred and four five-letter words, half of low frequency (between 7 and 24 occurrences per million in the SUBTLEX$_{US}$ corpus) and half of high frequency (between 57 and 2,961 occurrences per million in the SUBTLEX$_{US}$) were sampled from the English Lexicon Project (Balota et al. 2007). From each condition, 26 words were selected to be presented as targets and related primes (the *repetition* condition), and the remaining 26 were presented as unrelated primes (the *unrelated* condition). All word items were also controlled for orthographic neighborhood (i.e., Coltheart's N): $t \approx 0$. All words used were monomorphemic nouns, adjectives, or verbs, thus excluding particles, prepositions, and derived or inflected forms, with a reported accuracy above 90%. The descriptive statistics of the words used is reported in Table 2.

One-hundred and four five-letter, phono-orthographically legal non-words were randomly selected from the English Lexicon Project database as well. Half of them (i.e., 52) were randomly selected to be presented as targets; the other half was instead used as unrelated non-word primes. None of the non-words contained any existing English morpheme. In addition, all items had an error rate in the ELP smaller than 10%, to ensure that they would all be clearly distinguishable from words by participants. All items used in the experiments are reported in the appendix at the end of the paper (after the References section).

2.1.5. 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 (i.e., 104 pairs for each list). In one list, the three conditions (the high- and low-frequency word conditions, and the non-word condition) had half of the target items being preceded by themselves (the *repetition* condition) and half of the 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 five 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.

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 6 minutes and participants were paid a rate of 9 GBP/hour.

2.2. *Data analysis*

Priming effects were calculated by subtracting the mean RT to the related condition to the mean RT from the unrelated condition. We ran two different analyses, for both RT and error data. Our preregistered data analysis plan established that the data would be analyzed using a 2x2 repeated-measures ANOVA (factor, 2 levels: *high* vs. *low*; primetype, 2 levels: *unrelated* vs. *repetition*), with only subjects as random factors, followed by planned comparisons between related and unrelated primetype for the word conditions, as well as the non-word condition. The choice of this statistical model follows the recommendations of Raaijmakers, Schrijnemakers, and Gremmen (1999) and Raaijmakers (2003), and allowed us to carry out the prior power analysis simulations that informed our sample size choice, since models that include items as random factors or even as crossed random factors are too computationally expensive to run in large scale statistical power simulations. However, we also present an 2x2 analysis using Generalized Linear Mixed Model (GLMM) analysis on the raw RT and the raw accuracy data, since these models have become more widely adopted. Unlike linear mixed-effect models, GLMMs do not assume normal distribution of the data, and are therefore particularly useful for non-normally distributed data such as RTs and accuracy. For the RT data, a Gamma distribution was used; for the accuracy data, a binomial distribution was used instead. Both models used an identity link between fixed effects and the dependent variable (Lo and Andrews 2015). To prevent converge failure, the model was kept as simple as possible, with *factor* (2 levels: high vs. low), *primetype* (2 levels: unrelated vs related) and their interaction as fixed effects. Subjects and items were crossed as random effects. Before fitting the model, the contrasts were also set to sum-to-zero contrasts (i.e., by using the R function `contr.sum()`) to facilitate interpretation of main and interaction effects. The fitting was performed by using the `lme4` R-package (Bates et al. 2015) with the Laplace approximation technique, using 1 million iterations and the BOBYQA optimizer to help convergence. The function `Anova()` from the `car` R-package (Fox and Weisberg 2019) was used to obtain estimates and probability values for fixed effects calculated for Type-III sums of squares. The function `emmeans()` from the `emmeans` R-package (Lenth 2024) was used to calculate the estimated marginal means based on a separate 3x2 GLMM model, similarly defined as above, but including the non-word condition.

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. The pre-processing of the data consisted of three separate steps.

2.2.1. *Step 1: subject and item performance*

Item and subject error rates were calculated. The item error rate was never above 14%, so no item was excluded from analysis. Nineteen subjects were removed because their error rate was above 30%. A total of 269,652 observations and 2,593 participants were kept for analysis for this first step.

2.2.2. *Step 2: prime durations*

During the experiment, the duration of presentation of the prime word was recorded for every trial. On average, the prime duration was kept to its requested value (mean = 32 ms). However, the standard deviation of 15 ms indicated some imprecision in the presentation of the prime. 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. This cut-off removed 13% of the trials. No participant was excluded, for a total of 237,287 remaining observations.

2.2.3. *Step 3: RT analysis*

After removing the incorrect responses, 0.51% of the trials were excluded because their corresponding RT was below 200 ms or above 1800 ms. Finally, 249 subjects were removed because the number of trials within the same condition was less than 7 (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 remained avaialble for statistical analysis.

2.3. ***Results***

The preregistered 2x2 repeated-measures ANOVA revealed significant main effects (factor: F(1, 2340)=1572, p<.0001; primetype: F(1, 2340)=1113, p<.0001) and their interaction (F(1, 2340)=52.48, p<.0001). Planned comparisons confirmed statistically significant repetition priming effects for both word conditions (MOP_{HF}=18 ms, $CI_{95\%}$=[16 20], t(2340)=19.7, p<.0001; MOP_{LF} = 28 ms, $CI_{95\%}$=[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. This FAE effect was statistically significant (M_{FAE}=10 ms, $CI_{95\%}$=[7 13], t(2340)=7.24, p<.0001). A very small, but statistically significant inhibitory priming effect was observed in the non-word condition (MOP_{NW}=-2 ms, $CI_{95\%}$=[-4 0], t(2340)=-2.33, p<.0001). Similarly, the 2x2 GLMM analysis confirmed that both main effects (factor: $\chi^2(1) = 12.89, p = .0003$; primetype: $\chi^2(1) = 421.39, p < .0001$), and their interaction ($\chi^2(1) = 4.97, p = .03$) were significant. As reported in Table 3 and Table 4, the priming effects sizes based on the ANOVA and GLMM models are all comparable with one another.

In the error analysis, the 2x2 ANOVA revealed significant effects for both main effects (factor: F(1, 2340)=392.5, p<.0001; primetype: F(1, 2340)=380.5, p<.0001) and interaction (F(1, 2340)=55.47, p<.0001). Planned comparisons revealed significant priming effects in the form of fewer errors in the repeated compared to unrelated trials in the word condition (high: $M = 1.34, CI_{95\%} = [1.07\ 1.6], t(2340) = 9.95, p < .0001$; low: $M = 2.98, CI_{95\%} = [2.64\ 3.33], t(2340) = 16.9, p < .0001$), and an inverse effect in the non-word condition ($M = -0.42, CI_{95\%} = [-0.67\ -0.17]$, t(2340)=-3.27, p=.001). Similarly, the 2x2 GLMM analysis revealed significance for both the main effects (factor: $\chi^2(1) = 30.45, p < .0001$; primetype: $\chi^2(1) = 108.88, p < .0001$) and their interaction ($\chi^2(1) = 307.57, p < .0001$). The GLMM estimated marginal means from the corresponding 3x2 GLMM model indicated the same trends as the ANOVA planned comparisons: a facilitatory effect in both word conditions (high: $\beta = 0.58, SE = 0.056, z = 10.434, p < .0001$; low: $\beta = 0.74, SE = 0.042, z = 17.62, p < .0001$), and an inhibitory effect in the non-word condition ($\beta = -0.11, SE = 0.031, z = -3.50, p = .0005$).

Table 3. Experiment 1. Summary of the word priming results calculated within the ANOVA model. *Legend.* MOP: magnitude of priming.

	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

Table 4. Experiment 1. Summary of the word priming results based on the 2x3 GLMM model, with the same fixed and random effects as the 2x2 GLMM model, but including the non-word condition. *Legend.* MOP: magnitude of priming.

	unrelated RT			repetition RT			priming effects		
factor	mean	95% CI	SE	mean	95% CI	SE	MOP	95% CI	SE
high	604	[607 602]	0.85	585	[588 583]	0.90	19	[17 21]	0.63
low	634	[637 632]	0.73	606	[608 604]	0.83	28	[26 31]	0.84
non-word	645	[647 643]	0.75	647	[649 645]	0.84	-2	[-4 0]	0.62
frequency*primetype							9	[7 12]	1.03

2.4. *Discussion*

Experiment 1 was designed to investigate whether Frequency Attenuation Effects (FAE) can be detected in masked priming with a prime duration of 33 ms. Our findings replicated the results of Angele et al. (2023), Cayado, Wray, and Stockall (2023), and Petrosino, Sprouse, and Almeida (2023), showing statistically significant masked repetition priming effects in a web browser-based experiment for both high- and low-frequency words. Crucially, the results also confirmed the presence of an FAE, evidenced by a statistically significant interaction: the priming effect for low-frequency words was 10 ms larger than that for high-frequency words. Additionally, due to the large sample size, the FAE was estimated with high precision, yielding a narrow margin of error of 3 ms. Interestingly, in addition to the reliable FAE, our results align with previous studies (Cayado, Wray, and Stockall 2023; Petrosino, Sprouse, and Almeida 2023) in showing a minimal inhibitory masked repetition priming effect for non-words, with very high precision. The 95% confidence interval (CI) suggests that the plausible range for the masked repetition priming effect for non-words, at a prime duration of 33 ms, is between -4 ms and 0 ms.

These findings help dispel doubts about whether an FAE can be observed in masked priming, challenging earlier studies that failed to detect this effect. However, the results also raise questions about the size and malleability of the effect. The FAE observed in Experiment 1 (10 ms) is substantially smaller than those reported in other studies where significant FAEs were found (e.g., Kinoshita 2006), where the effect was around 30 ms. What could account for this discrepancy in effect size? One possibility is that the 30 ms estimate is inflated due to the lower statistical power in many previous experiments. As noted in the introduction, low power can lead to overestimated effect sizes when results are statistically significant. When examining the range of effect sizes in Table 1, we find that the average FAE is approximately 15 ms, which is half the size of those observed when statistically significant results were obtained. This 15 ms estimate is closer to the 10 ms effect size observed in Experiment 1, but still somewhat larger. Thus, there appears to be a potential discrepancy between the FAE effect sizes reported in the literature and the estimate from Experiment 1.

One possible explanation for this discrepancy is the difference in prime duration used in Experiment 1 (33 ms) compared to most previous studies (i.e., between 40 and 60 ms). It is plausible that the FAE scales linearly with prime duration in masked priming, similar to how repetition priming effects increase with longer prime durations. In fact, this is an actual prediction of *IA* models (see Grainger et al. 2012). Fortunately, Angele et al. (2023) and Petrosino, Sprouse, and Almeida (2023) have already demonstrated that web browser-based experiments can replicate the proportionality between prime duration and the magnitude of masked identity priming, consistent with Forster, Mohan, and Hector (2003). Both studies showed that increasing the prime duration from 33 ms to 50 ms resulted in larger identity priming effects. This provides an opportunity to test whether the discrepancy in FAE effect size between Experiment 1 and prior literature is due to the shorter prime duration used in our study. The goal of Experiment 2, therefore, is to leverage the established linear scaling of repetition effects with prime duration in masked priming to determine whether extending the prime duration also increases the magnitude of the FAE.

3. Experiment 2

Experiment 2 is an exact replication of experiment 1, with one difference: the prime duration is set to 50ms instead of 33ms. The predictions are straightforward: if the FAE scales with prime duration, then the FAE in experiment 2 should be larger than the one observed in experiment 1. This result would explain why the FAE observed in experiment 1 was smaller than the ones observed in prior studies.

In order to facilitate the interpretation of the results of experiment 2, we calculate the 95% *prediction intervals* (PI; cf. Patil, Peng, and Leek 2016; Spence and Stanley 2016) for a replication of experiment 1 with the effective sample size of experiment 2 (cf. Section 3.2.3). The logic of the *prediction interval* is to establish a range of possible effect sizes that can be expected in a replication due only to sampling error. In order words, if the only difference between the original study and the replication study is sampling variability, we should expect the effect sizes of the replication study to fall within the 95% PI set by the original study.

According to Table 5, we can make the following predictions for experiment 2: (1) Non-word repetition effects in masked priming are thought to be independent of prime duration (mostly because they are at best negligible to begin with). Thus, as far as the non-words are concerned, experiment 2 can be seen as a simple replication of experiment 1. The $PI_{95\%}$ for non-words from experiment 1 is [-5ms 1ms]. Thus, we expect that the effect size for non-word repetition in experiment 2 could be as low a -5ms or as high as 1ms; any result beyond these bounds would strongly imply that the differences between the effect sizes of the two experiments cannot be explained simply in terms of sampling error. (2) Masked repetition effects for words, on the other hand, are thought to linearly increase in magnitude as a function of prime duration. Thus, we expect that in experiment 2 the repetition effect size for the LF and HF conditions should be superior to the upper bounds of the $PI_{95\%}$ of experiment 1. The $PI_{95\%}$ from experiment 1 for the the repetition of HF words is [15 21] ms; the one for the LF condition is [25 31] ms. Thus, if our prime duration manipulation is successful in experiment 2, we should expect the repetition effect for the HF condition to be larger than 21ms while the same effect for the LF condition should be larger than 31 ms. (3) It is presently unclear whether the FAE interacts with prime duration. Given the FAE $PI_{95\%}$ [6 14] ms from experiment 1, we can interpret the results of experiment 2 in the following manner: if the FAE in experiment 2 falls within the [6 14] ms interval, we have no particular reason to think that FAE interacts with prime duration, as this result is exactly what would have been predicted if experiment 2 was a direct replication of experiment 1. However, if the FAE in experiment 2 is larger than 14 ms (as the previous literature implies), then we have good reason to believe that the FAE linearly scales with prime duration, much like masked repetition priming effects do.

3.1. *Methods*

3.1.1. *Participants*

Two thousand and six hundred participants (1,551 females; *mean age* = 39, *sd age* = 12) were recruited on Prolific (https://www.prolific.com) with the same criteria specified for experiment 1 (Section 2.1.2). Participants from experiment 1 were not allowed to enroll in this experiment.

Table 5. Prediction intervals calculated on the ANOVA-based means and standard deviations (SD) of the conditions tested in experiment 1.

			sample size		
factor	mean	SD	experiment 1	experiment 2	95% PI
high	18	45	2341	1924	[15 21]
low	28	49	2341	1924	[25 31]
non-word	-2	43	2341	1924	[-5 1]
frequency:primetype	10	66	2341	1924	[6 14]

3.1.2. Design

The experimental design was identical to experiment 1.

3.1.3. Materials

The experimental items were the same as experiment 1.

3.1.4. Procedure

Experiment 2 followed the same procedures as experiment 1 (see Section 2.1.5). The only difference was the prime duration, which was set to 50 ms. The median time to finish the experiment was about 6 minutes, like in experiment 1.

3.2. *Data analysis*

The analysis and data pre-processing plan were the same as in experiment 1 (Section 2.2). Analysis scripts and an abridged version of the data collected can be found online (https://osf.io/k3gpc), and consisted of 295,940 observations in total.

3.2.1. Step 1: subject and item performance

No item had an error rate superior to 12%, so no item was excluded from analysis. Thirty-nine subjects were removed because their error rate was above 30%. Thus, a total of 265,982 observations and 2,558 participants remained available for further analyses.

3.2.2. Step 2: prime durations

Prime fluctuations were dealt with in the same way as in experiment 1 (Section 2.2.2). The mean (50.11 ms, SD = 11.13 ms) and the median (50 ms) prime durations were close to the intended value (50 ms). Any trial whose prime duration was out of the 25-60ms range was excluded, resulting in the removal of 20% of the trials. As compared to experiment 1, experiment 2 had a 7% larger percentage of removed out-of-range trials, the majority of which (18%) were above the likely subliminal threshold of 60 ms. This is in contrast with the distribution of the prime durations in experiment 1, where the trials above the range amounted to 0.31% of the dataset. As observed in previous studies and pilots conducted in our lab, this distribution suggests that setting the prime duration closer to either limit of a given range has the side effect of allowing for more fluctuations beyond either limit, thus potentially leading to greater data loss. No participant was excluded, and a total of 213,078 observations remained available for analysis.

3.2.3. Step 3: RT analysis

After removing the incorrect responses, 0.59% of the trials were excluded because their corresponding RT was below 200 ms or above 1800 ms. Finally, 634 subjects were removed because the number of trials within the same condition was less than 7. A total of 168,195 observations and 1,924 subjects remained available for statistical analysis.

3.3. *Results*

In the word analysis, the 2x2 repeated-measures ANOVA model revealed significant main effects (factor: $F(1, 1923)$=987.4, p<.0001; primetype: $F(1, 1923)$=1447, p<.0001) and interaction ($F(1, 1923)$=36.82, p<.0001). Planned comparisons confirmed statistically significant repetition priming effects for both word conditions (MOP_{HF}=26 ms, $CI_{95\%}$=[24 28], t(1923)=24.6, p<.0001; MOP_{LF} = 35 ms, $CI_{95\%}$=[33 37], t(1923)=30.2, p<.0001), with the low-frequency word repetition priming effect being 10 ms larger than the high-frequency word repetition priming effect. This FAE effect was statistically significant (M_{FAE}=9 ms, $CI_{95\%}$=[6 12]), t(1920)=6.07, p<.0001). A very small but statistically significant inhibitory priming effect was observed in the non-word condition (MOP_{NW}=-4 ms, $CI_{95\%}$=[-6 -2], t(1923)=-3.53, p=.0004). The 2x2 GLMM analysis confirmed significant effects for condition ($\chi^2(1) = 834.43, p < .0001$), primetype ($\chi^2(1) = 3274.43, p < .0001$), and their interaction ($\chi^2(1) = 219.76, p < .0001$). Similarly to experiment 1, the estimates of both models were comparable to one another (see Table 6 and Table 7)

In the error analysis, the 2x2 ANOVA showed significant effects for both main effects (factor: $F(1, 2340)$=392.5, p<.0001; primetype: $F(1, 2340)$=380.5, p<.0001) and interaction ($F(1, 2340)$=55.47, p<.0001). Planned comparisons confirmed significant priming effects in the form of fewer errors in repeated compared to unrelated trials in the word conditions (high: $M = 1.29, CI_{95\%} = [1.02\ 1.56], t(1923) = 9.30, p < .0001$; low: $M = 3.05, CI_{95\%} = [2.67\ 3.43], t(1923) = 15.8, p < .0001$), and an inverse effect in the non-word condition ($M = -0.35, CI = [-0.64\ -0.054], t(1923) = -2.32, p = .002$). Similar results were obtained in the 2x2 GLMM error analysis (condition: $\chi^2(1) = 4.95, p = .02$; primetype: $\chi^2(1) = 101.53, p < .0001$; interaction: $\chi^2(1) = 7.18, p = .007$). The GLMM estimated marginal means from the corresponding 3x2 GLMM model indicated the same trends as the ANOVA planned comparisons: (high: $\beta = 0.64, SE = 0.065, z = 9.87, p < .0001$; low: $\beta = 0.87, SE = 0.051, z = 17.14, p < .0001$), and an inhibitory effect in the non-word condition ($\beta = -0.08, SE = 0.033, z = -2.96, p = .02$).

Table 6. Experiment 2. Summary of the word priming results within the ANOVA model. *Legend.* MOP: magnitude of priming.

	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	574	83	3	548	83	2	0.850	26	[24 28]	46	0.56	24.60	1923	2.27e-116
low	605	89	6	570	89	3	0.840	35	[33 37]	51	0.69	30.19	1923	3.51e-164
non-word	629	108	4	633	110	5	0.900	-4	[-6 -2]	50	-0.08	-3.53	1923	4.32e-04
frequency:primetype							0.024	9	[6 12]	68	0.13	6.07	1923	1.55e-09

Table 7. Experiment 2. Summary of the word priming results based on the 2x3 GLMM model, with the same fixed and random effects as the 2x2 GLMM model, but including the non-word condition. *Legend.* MOP: magnitude of priming.

	unrelated RT			repetition RT			priming effects		
factor	mean	95% CI	SE	mean	95% CI	SE	MOP	95% CI	SE
high	605	[608 603]	0.81	579	[582 577]	0.87	26	[24 28]	0.80
low	633	[635 631]	0.81	598	[600 596]	0.77	35	[32 37]	0.85
non-word	649	[652 647]	0.95	653	[655 650]	0.96	-3	[-5 -1]	0.73
frequency*primetype							8	[6 11]	1.05

3.4. *Discussion*

Experiment 2 aimed to replicate the FAE observed in Experiment 1 using an extended prime duration of 50 ms, and to assess whether this longer prime would amplify the effect. To achieve this, the same stimuli and sample size from Experiment 1 were employed, with the sole modification being the increased prime duration.

As predicted, as compared to experiment 1, we found an increase in the masked repetition priming effects for both word conditions in experiment 2, by approximately 8 ms for high-frequency ($M_{exp1} = 18ms$ [16 20], $M_{exp2} = 26ms$ [24 28]) and 7 ms for low-frequency ($M_{exp1} = 28ms$ [26 30], $M_{exp2} = 35ms$ [33 37]) words. Specifically, the repetition priming effect for high-frequency words was 26 ms, and for low-frequency words, 35 ms, both exceeding the upper bounds of the 95% prediction interval from Experiment 1 (20 ms and 30 ms, respectively; see Table 5). This indicates that the manipulation of prime duration was effective. Notably, and consistent with our predictions, the repetition effect for the non-word condition was -2 ms, which fell within the 95% prediction interval of Experiment 1 (i.e., [-5 1]). Furthermore, the larger priming response for the high- and low-frequency word conditions in Experiment 2 was driven entirely by faster recognition of the related word pairs. The RT estimates for the unrelated pairs, along with their standard deviations and error rates, were virtually identically to those observed in Experiment 1, despite being from a completely different set of participants. This finding further corroborates the long-standing observation that the magnitude of the masked priming response is influenced by prime duration (Forster, Mohan, and Hector 2003).

Crucially, we were also able to replicate the FAE in Experiment 2. However, the FAE in this experiment was 9 ms ($CI_{95\%} =$ [6 12]), which is nearly identical to the FAE observed in Experiment 1 and falls within its 95% prediction interval, i.e. [6 14] ms. This suggests that, unlike the repetition effect in masked priming, the FAE does not interact with prime duration. Therefore, the difference in prime duration between Experiment 1 and prior studies in the literature does not appear to account for the observed discrepancies in the magnitude of the FAEs.

4. General discussion

Establishing whether the FAE is observable in masked repetition priming is theoretically important, as different word recognition models make divergent predictions about whether the FAE should occur in masked priming, and, if it does, how it should be explained.

4.1. *Accounting for the FAE*

On one side, *interactive activation* (IA) models (McClelland and Rumelhart 1981; Grainger and Jacobs 1996; Coltheart et al. 2001) and *retrospective models* of priming, such as the *memory recruitment model* (Bodner and Masson 1997; Masson and Bodner 2003; Bodner and Masson 2014), predict the FAE in masked priming. However, these theories differ in where they locate the effect. *IA* models predict a lexical origin (see Grainger et al. 2012), while the memory recruitment model suggests a post-lexical source.

On the other side, *search models*, such as the *entry-opening model* (Forster and Davis

1984), propose a more complex interaction between lexical and episodic memory. Specially, this model posit that under unmasked conditions, repetition priming is driven largely by *episodic* memory, but under masked priming, *episodic* memory plays no role, and thus masked repetition priming reveals primarily *lexical* (and perhaps also *sublexical*) level processes. According to the *entry-opening model*, only two processes are at play within lexical structure: a frequency-ordered search and the time required to open an entry once a potential match is flagged (the *entry opening time*, EOT). The search is sensitive to word frequency, but the EOT is assumed to be uniform across all entries (and it is estimated at roughly 60 ms by Forster, Mohan, and Hector 2003). Consequently, the model predicts no interaction between frequency and repetition priming under masked conditions, meaning the FAE should not be observable in masked repetition priming.

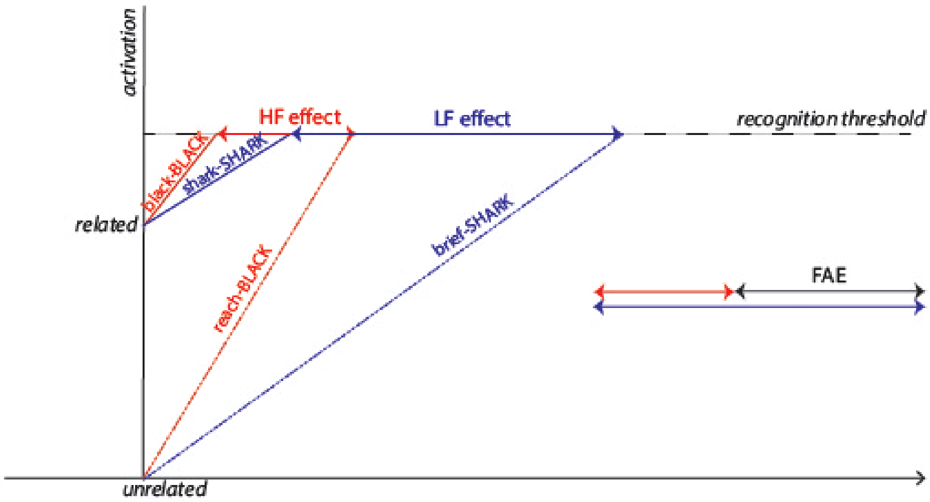
Clearly then, the FAE in experiments 1 and 2 empirically aligns with the predictions of *IA* and *memory recruitment* models, insofar as both models predict an FAE under masked conditions. These findings contradict the *entry opening model*, which predict no FAE in masked repetition priming.

However, further considerations suggest that the FAEs observed in our studies may be less supportive of the *memory recruitment model*, which predicts the FAE only when the context of the behavioral task justifies drawing on non-lexical memory resources post-lexically, such as when task difficulty is increased. In our experiments, task difficulty was deliberately minimized: both words and non-words had very high accuracy rates in the English Lexicon Project (>90%, see Table 2), leading to item error rates that were never higher than 14%, entirely avoiding the need for item exclusions. In addition, only 58 in 5200 participants (1.1% of the total sample) had an overall error rate above 30%. Finally, many researchers may find implausible the assumption that episodic memory resources are formed at the very short prime duration used in experiment 1 (33 ms).

Conversely, the *entry-opening model* predicts a null FAE because it assumes a fixed EOT. This assumption, based on the observation that the magnitude of masked repetition effects mirror prime duration up to 60 ms (Forster, Mohan, and Hector 2003), is contradicted by our findings. In Experiment 2, where the prime duration was 50 ms, the maximum repetition priming effect was 35 ms, suggesting that EOTs are not fixed at 60 ms, nor do they match the prime duration in general (see Wu 2012 for a similar argument). Moreover, repetition priming effects were consistently smaller for high-frequency words, further challenging the model's assumption of uniform EOTs. Thus, if we relax the assumption that EOTs are uniform across the lexicon and instead posit that lower-frequency words have longer entry opening times than higher-frequency words, the *entry-opening model*, like *IA* models, could account for the FAE.

4.2. *The independence of FAE from prime duration*

What about the finding that the FAE does not seem to interact with prime duration? Here, all models seem to make the wrong predictions. *IA* models assume that it is stimulus exposure that drives activation of lexical nodes, and that frequency is directly related with lexical nodes activation levels, either in terms of setting their resting levels or in how fast they accumulate information from the input. Grainger et al. (2012) explains how the FAE is predicted by such a model: low-frequency words accumulate evidence more slowly that do high-frequency words. If one posits that stimulus repetition provide a uniform boost to lexical nodes, then compared to a situation where lexical nodes are at their resting activation levels (the unrelated condition), lower-frequency repeated words will reach their recognition threshold comparatively faster than would repeated high-frequency words (cf. Grainger et al. 2012, fig. 1). Under this view, however, the amount of activation boost to lexical nodes in the related condition is a function of stimulus exposure, and thus a longer prime duration should deliver a stronger initial activation boost, which can only lead to a magnification of the FAE (**?@fig-ia-2**). Therefore, it seems that the basic mechanics of *IA* models, together with the assumptions that allow them to capture the FAE, also predict an interactive effect between FAE magnitude and prime duration, with larger FAE magnitudes predicted when prime durations are longer. In experiments 1 and 2, however, the increase from a 33ms to a 50ms prime duration elicited FAEs of same magnitude (approximately 10ms).



{#fig-ia-2 fig-pos="H"å}

The original *entry opening model* does not predict a FAE, and so it cannot make any predictions about its potential interaction with prime duration. **(I think a logic step is missing here:) This is because the original model assumes the size of identity priming to amount to the EOT or prime duration (whichever is smaller), but should not be modulated by prime duration (Forster, Mohan, and Hector 2003; Wu 2012). On the other hand, the modified version of the *entry opening model* sketched above would be able to. While still assuming that identity priming be equal to EOT or prime duration (whichever is smaller), the model we can be modified by arguing that the EOT is a function of word frequency. In this sense,** if the EOT is larger for lower-frequency words, then the relationship between prime duration and FAE is predicted to be somewhat non-linear (see Figure 4). While prime duration equals the EOTs of the higher frequency words, and is therefore smaller than the EOTs of the lower frequency words, no FAE should be observed, **since the priming magnitude of both high- and low-frequency words should equally amount to the prime duration (Figure 4, scenario A)**. While the prime duration exceeds the EOTs for the higher frequency words, but it is still shorter than the EOTs of the lower frequency words, the FAE should increase linearly with prime duration (Figure 4, scenario B), and should reach its maximum when the prime duration equals the EOTs of the lower frequency words. Beyond that point, any increase in prime duration should no longer lead to increases in the FAE, which should remain at its maximum magnitude, namely the difference between the EOTs of high-and low-frequency words (Figure 4, scenario C).

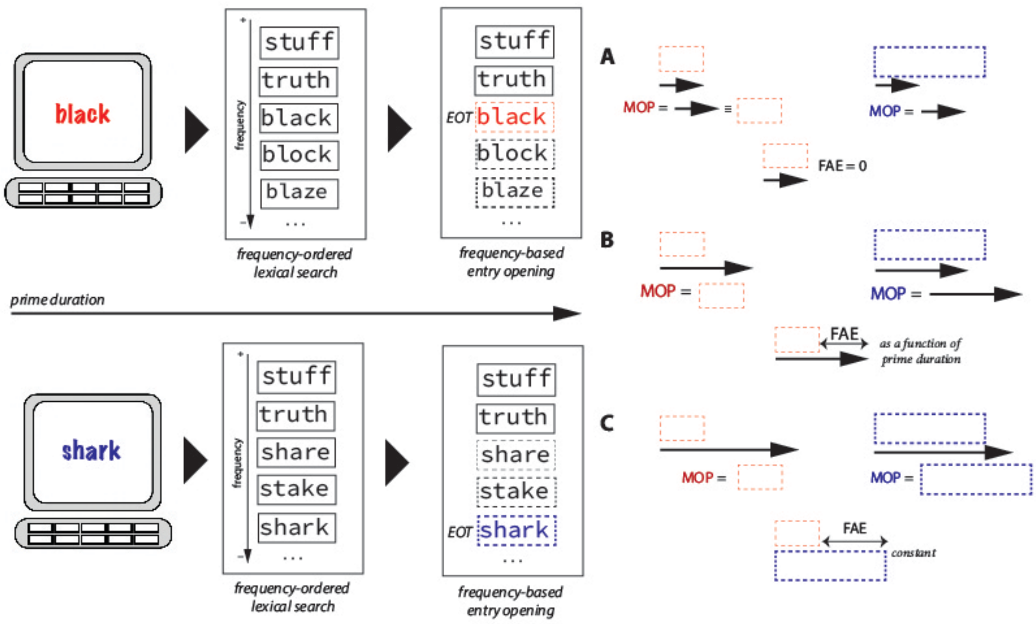


Figure 4. Graphic representation of a modification of the entry opening model, in which the entry opening time may vary as a function of word frequency. The size of the priming response to a word is argued to be equal to either the prime duration or the entry opening time (EOT) of the corresponding lexical entry, whichever is smaller. The interaction between prime duration and EOT of the prime word may trigger any of the three scenarios in panels A-C. The length of the black arrow graphically represents the length of the prime duration; the length and thickness of the text boxes represent the EOT for each word (shorter for high-frequency words such as *black*, and longer for low-frequency words such as *shark*).

Unfortunately, we do not observe this predicted pattern in experiments 1 and 2. In experiment 1, the repetition effect for low-frequency words is close to the prime duration (a 28ms effect for a 33ms prime duration), while the repetition effect for high-frequency words is 10ms shorter (a 18ms effect for a 33ms prime duration). If we assume that the LF effect of 28 ms is not significantly different from the prime duration of 33 ms, our modified *entry opening model* would predict that the prime duration has not yet exceeded the *opening times* of the low frequency words, while it clearly has exceeded the *opening times* of the high-frequency words. Thus, the model just sketched would predict that an increase in prime duration would also increase the magnitude of the repetition effect for the LF condition, but not for the HF condition, leading to a larger FAE (Figure 4, scenario B). However, while we did observe a 7ms increase in the magnitude of the repetition effect for the LF condition in experiment 2 ($MOP_{exp1} = 28ms; MOP_{exp2} = 35ms$), we also observed an 8ms increase for the HF condition ($MOP_{exp1} = 18ms; MOP_{exp2} = 26ms$), and a FAE that was virtually identical in magnitude across experiments ($FAE_{exp1} = 10ms; FAE_{exp2} = 9ms$). Conversely, if we take the results of experiment 1 at face value, and assume that the 33ms prime duration exceeds both the *opening times* of high- and low-frequency words (18 ms and 28 ms, respectively), then the modified *entry opening model* would indeed make the prediction that the FAE observed in experiment 1 reveals the maximum of the FAE as 10 ms, and that this value should not increase when prime duration increases (Figure 4, scenario C). While this is what our experiments show, the same model would also make the prediction that the repetition priming effects should remain stable across experiments, contrary to fact (they are both approximately 8ms longer in experiment 2).

Thus, it seems neither *IA* models nor the modified *entry opening model* make the correct predictions about the pattern of results observed in experiments 1 and 2, namely a stable FAE effect irrespective of prime duration, with a concomitant increase in repetition priming effects as a function of it. *IA* models would have predicted an increased FAE in experiment 2, and the modified *entry opening model* would have predicted either an increased FAE in experiment 2, or exactly identical results in terms of FAE and repetition priming effects magnitudes in experiments 1 and 2.

Can these models be amended to allow them to capture the results? On the one hand, given their core mechanics, *IA* models can capture either a situation where FAE does not arise (i.e., word frequency is purely additive to repetition effects), or a situation where FAE is generated, and its magnitude is necessarily a function of the prime duration. Neither of these patterns match the results of experiments 1 and 2. So the results of experiment 1 and 2 pose a real challenge for *IA* models.

On the other hand, a different modification to the original *entry opening model* may help explain both the existence of the FAE and its non-interaction with prime duration. If we maintain the assumption that the EOT is uniform for all words, we could assume that the exposure to the prime raises the rank order of the target word to the top of the search list, thus resulting in a “promotion boost” that is a function of the repeated presentation, and should not interact with prime duration (along the lines of a direction that was already hinted at by Forster and Davis 1984). This boost would also impact high- and low-frequency words differently: while both words would benefit from repetition by being raised to the top of the search list, low-frequency words would be retrieved comparatively faster than high-frequency words when contrasted with their first retrieval times during the presentation of the prime (see Figure 5). This simple mechanism explains at once the existence of the FAE and the fact that it does not interact with prime duration. For instance, in experiment 1, the 10ms FAE can be explained as follows: as the EOT is uniform, then it must be around 18 ms (the repetition priming effect of high-frequency words). The repetition priming effect for low-frequency words (28 ms) is then the summation of the EOT (18 ms) and the comparative speed up difference in retrieval time for the target words across high- and low-frequency condition (i.e, the FAE, which is 10 ms with our set of materials).

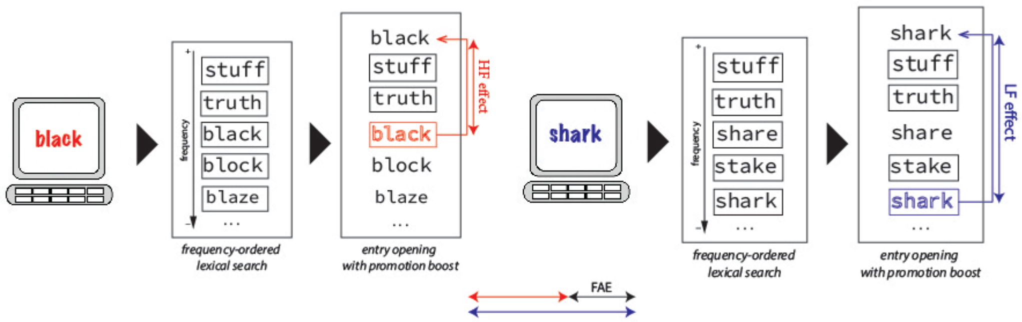


Figure 5. Graphic representation of an alternative modification of the entry opening model, in which the lexical entry of the prime stimulus is promoted to the top of the entry list in the entry opening stage. As being already relatively high in the list search, high-frequency prime words (such as *black*) may benefit from the promotion boost in a lesser extent as compared to low-frequency prime words (such as *shark*). The differential size of the relative promotion boost gives rise to the FAE.

Unfortunately, this logic would fail to account for the results of experiment 2: if the EOT is indeed 18 ms as shown in experiment 1, and the EOT is the only additional source of priming for high-frequency words, then this effect is already maxed out with a prime duration of 33 ms. Why is it then that increasing the prime duration to 50 ms in experiment 2 makes the effect approximately 8ms longer (26 ms)? **One potential explanation for this may be that the EOTs may vary across samples of individuals, in line with a strand of the masked form priming research showing that priming covaries with the individual reading and spelling skills of the sample recruited (Andrews and Hersch 2010; Andrews and Lo 2012). In our case, this would mean that the EOT for the sample in experiment 1 was 18 ms, but it was 26 ms for the sample in experiment 2, irrespective of the difference in the prime duration.** The problem with this claim is that it seems arbitrary to assume that the EOT can vary substantially (here ~8 ms) across our two very large samples, which are able to produce very narrow margins of errors around precise estimates for a number of relevant measurements, such as the mean and standard deviations RT for the unrelated conditions and non-word conditions and the magnitude of the FAE. Nonetheless, it may be a fruitful avenue for future studies to explore.

5. Conclusions

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 both a short (33ms) and longer (50ms) priming durations. Notably, we were able to successfully address a lingering empirical question in the literature by establishing that the Frequency Attenuation Effect (FAE) does arises under masked conditions. We were able to determine additionally that the FAE is also independent of the duration of the prime. We contend that, while this pattern of results presents a direct challenge to both *IA* and *entry opening* models, the latter offers a few promising avenues for small modifications might enable it to accommodate the results of our studies. The most promising is the assumption that after the encounter with the prime, its entry is temporarily raised to the top of the rank frequency ordered search list. This modification explains at once the existence of the FAE, and its independence of the prime duration.

(Here's my attempt to add some general implications of the results of the study:) 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. First, the larger sample size needed to detected the masked FAE 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 that suggests that repetition priming under masked conditions affect primarily the encoding stage of the stimulus (Gomez, Perea, and Ratcliff 2013). 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. 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 we anticipate to be fruitful to explore in the near future.

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Wordlists

Experiment 1

Experiment 2

			RT (to repetition)		RT (to unrelated)	
related	repeated prime	word	mean	SD	mean	SD
low frequency condition						
arrow	hunch	arrow	590	130	587	124
pitch	sneak	pitch	576	126	612	122
hatch	widow	hatch	621	151	639	148
shark	brief	shark	573	125	590	138
tooth	sharp	tooth	536	125	565	116
booth	grief	booth	572	136	627	157
pound	sting	pound	551	127	572	127
weigh	thief	weigh	593	167	636	164
blank	avoid	blank	571	139	596	124
crush	award	crush	554	128	592	136
bench	smack	bench	573	132	601	129
fetch	brand	fetch	622	156	658	146
cheek	salad	cheek	561	141	602	142
brush	swamp	brush	564	130	600	128
march	depth	march	559	125	580	123
bleed	flesh	bleed	560	148	577	146
cliff	harsh	cliff	602	130	645	137
fraud	creep	fraud	621	147	628	132
cloud	plead	cloud	536	115	551	101
fluid	thumb	fluid	605	140	678	162
trash	creek	trash	554	127	560	128
flush	blond	flush	576	123	617	140
porch	stink	porch	587	136	620	160
stiff	patch	stiff	626	154	678	156
cough	sweep	cough	564	142	601	141
smash	squad	smash	570	129	587	126
high frequency condition						
blood	chief	blood	541	130	551	104
bunch	child	bunch	585	148	617	145
catch	board	catch	545	116	562	130
stuff	tough	stuff	555	119	585	137
break	stand	break	545	107	561	124
speak	beach	speak	545	131	573	129
stick	hotel	stick	562	128	598	138
sleep	angel	sleep	538	113	559	119
wrong	truth	wrong	563	143	565	132
grand	quick	grand	571	127	582	143
mouth	world	mouth	543	125	556	119
knock	extra	knock	560	134	631	136
guard	think	guard	580	132	590	134
small	thing	small	557	130	577	125
check	round	check	558	135	562	121
watch	proud	watch	541	128	546	110
group	smell	group	559	127	576	142
month	earth	month	555	120	572	123
south	relax	south	575	139	611	133
lunch	truck	lunch	547	119	557	125
clock	throw	clock	548	132	574	124

			RT (to repetition)		RT (to unrelated)	
related	unrelated prime	word	mean	SD	mean	SD
low frequency condition						
arrow	hunch	arrow	573	130	591	128
pitch	sneak	pitch	565	132	608	146
hatch	widow	hatch	606	163	644	164
shark	brief	shark	560	132	587	124
tooth	sharp	tooth	533	126	560	111
booth	grief	booth	566	147	618	163
pound	sting	pound	552	139	570	134
weigh	thief	weigh	583	158	649	186
blank	avoid	blank	556	132	583	132
crush	award	crush	542	126	583	142
bench	smack	bench	565	143	601	138
fetch	brand	fetch	612	149	663	146
cheek	salad	cheek	556	138	597	148
brush	swamp	brush	559	136	593	125
march	depth	march	560	149	575	122
bleed	flesh	bleed	552	137	568	135
cliff	harsh	cliff	595	149	662	157
fraud	creep	fraud	596	131	634	113
cloud	plead	cloud	539	132	552	108
fluid	thumb	fluid	601	167	690	173
trash	creek	trash	564	157	566	126
flush	blond	flush	570	133	624	155
porch	stink	porch	584	155	617	153
stiff	patch	stiff	617	163	677	161
cough	sweep	cough	552	136	591	145
smash	squad	smash	553	133	590	142
high frequency condition						
blood	chief	blood	534	131	558	112
bunch	child	bunch	584	156	619	146
catch	board	catch	529	111	568	141
stuff	tough	stuff	556	128	576	133
break	stand	break	542	128	564	123
speak	beach	speak	543	131	570	132
stick	hotel	stick	556	136	599	130
sleep	angel	sleep	525	111	566	137
wrong	truth	wrong	555	142	569	137
grand	quick	grand	564	129	588	147
mouth	world	mouth	534	123	552	116
knock	extra	knock	549	131	615	139
guard	think	guard	565	118	590	118
small	thing	small	549	137	567	130
check	round	check	546	125	563	120
watch	proud	watch	531	115	545	133
group	smell	group	553	126	579	142
month	earth	month	549	126	576	128
south	relax	south	563	138	603	144
lunch	truck	lunch	549	141	563	124
clock	throw	clock	547	133	574	130