# 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, the so called Frequency Attenuation Effect (FAE). Many early findings suggested no such interaction, but more recent studies have contradicted this. In experiment 1, we provide evidence that the FAE does occur under masked conditions, with a short prime duration (33ms). In experiment 2, we replicate this finding with a longer prime duration (50ms), and show that the magnitude of the FAE is independent of prime duration. We contend that, while no word recognition models can currently account for this pattern of results, there may be possible modifications to the entry-opening model that might render it able to accommodate our set of results.

## 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 hashes, #####), followed by a prime string presented in lowercase for very short

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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 not being aware that a prime string has been presented, and can at most report a screen flicker just before the target presentation (cf. 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.

| Study | Language | N | SOA | MOP (ms) | | FAE (ms) | | Power range [min max] | |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| | | | | 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 or 33[1] | 44 | 65 | 21 | * | [0.03 0.52] | [0.06 0.99] |
| Nievas (2010), exp. 2a | Spanish | 30 | 50 | 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)

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## 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. Alternatively, these models could also propose that each access creates a small but long-term change in the resting activation levels of the lexical node, and so prior exposure will always lead to some amount of priming. The FAE arises in this type of model 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 (cf. Grainger et al. 2012).

*Search models* like the *entry opening model* (Forster and Davis 1984), on the other hand, predict that candidate entries for recognition are evaluated according to their rank order of frequency. 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). Across six experiments, 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 Norris et al. (2018).

However, as Table 1 shows, there are, contrary to the earlier literature, a number of 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 20 previous 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. *Our studies*

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) word frequency database, which 15 out of 20 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. Researchers are thus forced to explore a range of plausible values for standard deviations and correlation structures on their own. 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 corre-

lation across conditions. For instance, the study with the smallest sample size (Forster et al. 1987, N=16) had between 2% and 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 (Wu 2012, $N = 64$) exhibited a range of 4% to 87% power to detect a 15 ms FAE, and 12% 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.

The potential of a non-negligible number of low-powered experiments on the scientific record 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). While reporting non-significant FAEs, the remaining 14 studies still exhibit numerically larger repetition effect sizes for low-frequency words 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, which, under the naïve assumption that the two conditions are similar enough across experiments, seem to be statistically significant: $M\_FAE = 15$ ms, $CI\_95\% = [8\ 21])$, $t(20) = 4.89$, $p = .0001$. 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 absence of clarity regarding the statistical power of previous research poses challenges in assessing the likelihood of these significant findings being 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 two studies reported here were 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 first issue, we exclusively sourced materials from the SUBTLEX$_{US}$ database (Brysbaert and New 2009), which reflects more recent linguistic usage and offer better validation in behav-

ioral experiments (e.g., Brysbaert and New 2009; Yap and Balota 2009; Brysbaert and Cortese 2011; Gimenes and New 2016; Herdağdelen and Marelli 2017). As for the second issue, we recruited two extremely large samples ($N = 2600$) 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).
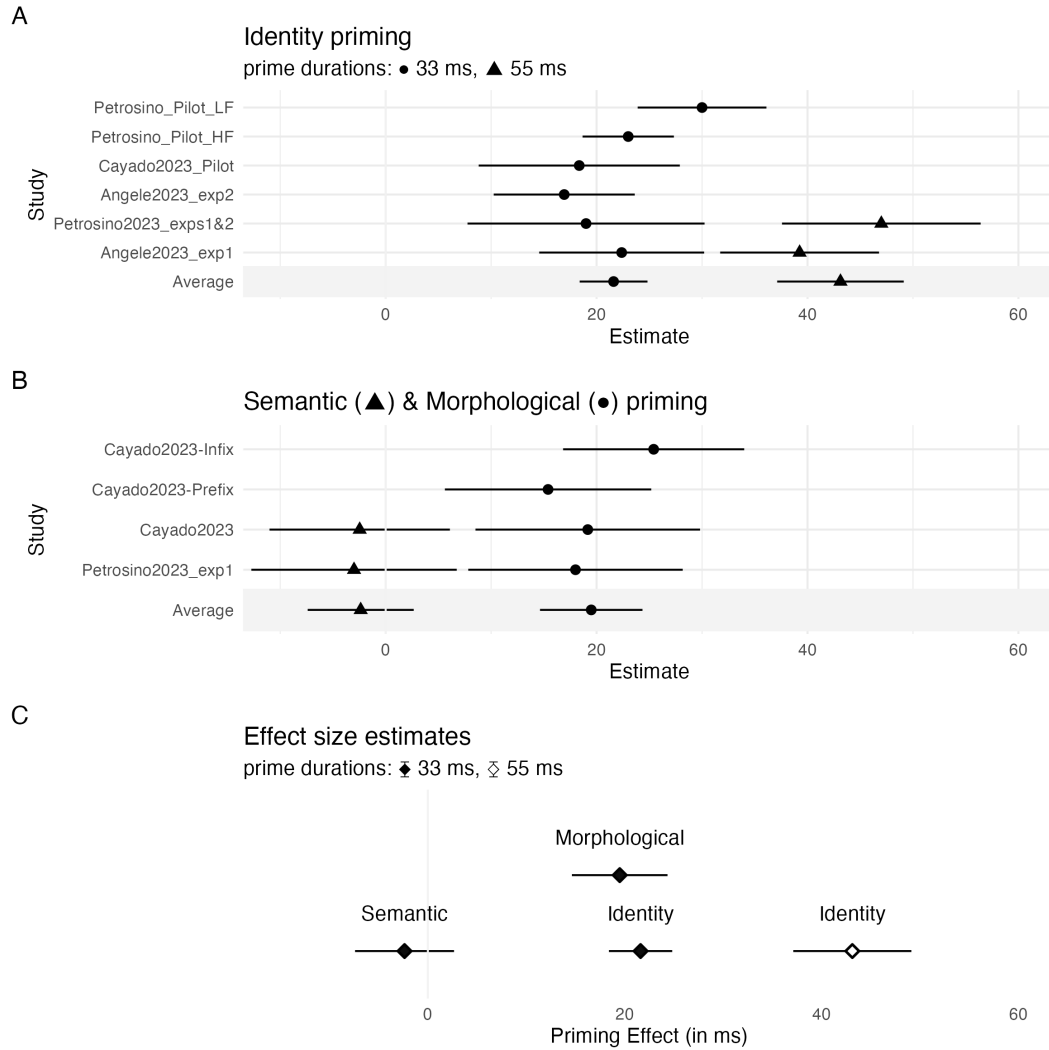
Figure 1. Meta-analysis of three recent masked priming experiments conducted online, together with unpublished pilot data from our lab, investigating three different phenomena: identity, morphological and semantic priming, at 33ms and 50ms prime durations.

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 1; the full details are available in the supplemental material) show a clear replication of four basic benchmark results in masked priming: detection of (i) identity and (ii) morphological priming with (iii) no evidence of semantic priming, as well as (iv) a proportionality effect of identity 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

Lukács and Gartus 2023).

### 2.1.3. Design

The masked priming procedure relied on a lexical decision task (LDT), in which a 2 (frequency: *high* vs *low*) x 2 (prime type: *repetition* vs *unrelated*) factorial design was used. Both factors were manipulated within-subjects. 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.

Table 2. Experiment 1. Descriptive statistics of the word items used. For both frequency databases, the word frequencies were converted to per-million count to ensure cross-comparison.

| frequency | N | HAL | | | | SUBTLEX$_{US}$ | | | | Orthographic $N$ | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | min | max | mean | SD | min | max | mean | SD | min | max | mean | SD |
| high | 52 | 45 | 4984 | 573 | 808 | 57 | 2691 | 210 | 388 | 0 | 10 | 3.98 | 2.60 |
| low | 52 | 6 | 570 | 64 | 93 | 7 | 24 | 13 | 5 | 0 | 11 | 3.92 | 2.79 |

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. Both the words and non-words used in the experiments are reported in the appendix at the end of the paper (after the References section). 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.

### 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*

Our preregistered data analysis plan established that the data would be analyzed using a **2x3** repeated factors ANOVA (condition, 3 levels: high vs. low vs. non-word; prime-type, 2 levels: unrelated vs. repetition), with only subjects as random factors, followed by planned comparisons between related and unrelated primetype for each 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 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, since these models have become more widely adopted, we also present an analysis using Generalized Linear Mixed Model (GLMM) analysis on the raw RT and the raw accuracy data. 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 *condition*, *primetype* 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. Estimated marginal means were then calculated with the function `emmeans()` from the the `emmeans` R-package (Lenth 2024).

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. 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.

### 2.2.2. Step 2: prime durations

During the experiment, the duration of presentation of the prime word was recorded for every trial. Both the mean (mean = 32 ms; sd=15 ms) and the median (33 ms) of the prime duration confirm some imprecision in the duration of the presentation of the prime 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. Thus, 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 observations.

### 2.2.3. Step 3: RT analysis

After removing the incorrect responses, 0.51% of the trials were excluded if 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 (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.

## 2.3. Results

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.

The preregistered 2x3 repeated-measures ANOVA (condition, 3 levels: high vs. low vs. non-word; primetype, 2 levels: unrelated vs. repetition) on the by-subject averaged RT data revealed significant main effects (condition: $F(1, 2340)$=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 GLMM analysis confirmed that

both main effects (condition: $\chi^2 = 2978.35, p < .0001$; primetype: $\chi^2 = 1531.12, p < .0001$), and their interaction ($\chi^2 = 870.27, p < .0001$) were significant. As reported in Table 3 and Table 4, the estimates based on the ANOVA and GLMM models overlap with one another, which suggest the two models to be substantially equivalent.

In the error analysis, the ANOVA revealed significant effects for both main effects (condition: $F(1, 2340)=392.5$, $p<.0001$; 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 all conditions (high: $t(2340)=9.95$, $p<.0001$; low: $t(2340)=16.9$, $p<.0001$; non-word: $t(2340)=-3.27$, $p=.001$). As in the ANOVA error analysis, both main effects (condition: $\chi^2 = 30.45, p < .0001$; primetype: $\chi^2 = 108.88, p < .0001$) and their interaction ($\chi^2 = 307.57, p < .0001$) were significant.

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

| factor | unrelated RT | | | repetition RT | | | | priming effects | | | | t-test | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 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 calculated within the GLMM model. *Legend.* MOP: magnitude of priming.

| factor | unrelated RT | | | repetition RT | | | priming effects | | |
|---|---|---|---|---|---|---|---|---|---|
| | 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 |

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## 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 FAE. 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 ($\geq 50 ms$). 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. Table 5). 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 previous 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 LF and HF conditions should be superior to the upper bounds of the *PIs_95%* of experiment 1. The *PI_95%* from experiment 1 for the the repetition of HF words is [15ms 21ms]; the one for the LF condition is [25ms 31ms]. 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%* [6ms 14ms] from experiment 1, we can interpret the results of experiment 2 in the following manner: if the FAE in experiment 2 falls within the [6ms 14ms] 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.

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

| factor | mean | SD | sample size 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] |

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| | | | | | |
|---|---|---|---|---|---|
| frequency:primetype | 10 | 66 | 2341 | 1924 | [6 14] |

## 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 excluded from the recruiment pool, to ensure an completely new set of participants.

### 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*

Item and subject error rates were calculated. Similarly to experiment 1, the item error rate was never above 12%, so no item was excluded from analysis. 39 subjects were removed because their error rate was above 30%. Thus, a total of 265,982 observations and 2,558 participants were included in 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 (mean = 50.11 ms, sd = 11.13 ms) and the median (median = 50 ms) prime durations were closer to the intended value (50 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 20 % of the trials. As compared to experiment 1, experiment 2 had therefore a 7% larger percentage of removed out-of-range trials, the majority of which (18%) were above the subliminal threshold. This is in contrast with the distribution of the prime durations in experiment 1, where the trials above the range amounted to only 0% of the dataset. As observed in previous studies and pilot conducted in our lab, this distribution suggests that setting the prime duration closer to either limit of a given range has the

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side effect of allowing for more fluctuations beyond either limit, thus potentially leading to greater data loss (see also further below). No participant was excluded, for a total of 213,078 observations.

### 3.2.3. Step 3: RT analysis

After removing the incorrect responses, similarly to experiment 1 (Section 2.2.3), 0.59% of the trials were excluded if 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 (i.e., about half of the total number of trials being presented within the same condition, i.e. 13). A total of 168,195 observations and 1,924 subjects were included in the statistical analysis below. The substantial number of subjects being removed at this final stage was the side effect of the increased number of out-of-range trials that were removed during the previous step of analysis, demonstrating that setting the prime duration closer to the upper bound of the subliminal range may incur in a higher degree of data loss in online masked priming experiments.

## 3.3. *Results*

In the word analysis, the 2x3 repeated-measures ANOVA model revealed significant main effects (condition: $F(1, 1923)$=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 GLMM analysis confirmed significant effects for condition ($\chi^2 = 2613.3, p < .0001$), primetype ($\chi^2 = 1700.5, p < .0001$), and their interaction ($\chi^2 = 1158.9, p < .0001$). Similarly to experiment 1, the estimates of both models were equivalent (see Table 6 and Table 7)

In the error analysis, the ANOVA showed significant effects for both main effects (condition: $F(1, 2340)$=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 all conditions (high: $t(1923)$=9.30, $p$<.0001; low: $t(1923)$=15.8, $p$<.0001; non-word: $t(1923)$=-2.32, $p$=.002). Similar results were obtained in the GLMM error analysis (condition: $\chi^2 = 51.50, p < .0001$; primetype: $\chi^2 = 97.42, p < .0001$; interaction: $\chi^2 = 283.77, p < .0001$).

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

| factor | unrelated RT | | | repetition RT | | | priming effects | | | | | t-test | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 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 | 24.595576 | 1923 | 2.27e-116 |
| low | 605 | 89 | 6 | 570 | 89 | 3 | 0.840 | 35 | [33 37] | 51 | 0.69 | 30.185474 | 1923 | 3.51e-164 |
| non-word | 629 | 108 | 4 | 633 | 110 | 5 | 0.900 | -4 | [-6 -2] | 50 | -0.08 | -3.525726 | 1923 | 4.32e-04 |
| frequency:primetype | | | | | | | 0.024 | 9 | [6 12] | 68 | 0.13 | 6.068254 | 1923 | 1.55e-09 |

Table 7. Experiment 2. Summary of the word priming results within the GLMM model. *Legend.* MOP: magnitude of priming.

| factor | unrelated RT | | | repetition RT | | | priming effects | | |
|---|---|---|---|---|---|---|---|---|---|
| | 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, we found that the masked repetition priming effect for both high- and low-frequency words increased by approximately 8 ms in Experiment 2 compared to Experiment 1. 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 (21 ms and 31 ms, respectively). 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 highly similar to those observed in Experiment 1. This finding lends additional support to the notion 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 ($CI\_95\%_{exp2} = [612]$), which is nearly identical to the FAE observed in Experiment 1 and falls within its 95% prediction interval, i.e. [6 ms 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

The masked priming paradigm stands as a cornerstone in psycholinguistic investigations, providing critical insights into the mechanisms of word recognition. However, there is still uncertainty about whether lexical frequency interacts with masked repetition priming, a phenomenon known as the Frequency Attenuation Effect (FAE). Many early findings suggested no such interaction, but more recent studies have contradicted this view (see Table 1). Resolving this issue 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. These models 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 lexical 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 60ms 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 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 a non-lexical memory resources post-lexically, such as increased task difficulty. In our experiments, task difficulty was deliberately minimized: both words and non-words had very high accuracy rates in the English Lexicon Project, leading to item error rates that never rose above 14%, entirely avoiding the need for item exclusions. In addition, only 58 in 5200 participants, or 1.1%, 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 for low-frequency words, 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, these 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. 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).

The original *entry opening model* does not predict a FAE, and so it cannot make any predictions about its potential interaction with prime duration. However, the modified version of the *entry opening model* sketched above would be able to. If it is indeed true that EOTs are larger for lower-frequency words, then the relationship between prime duration and FAE should be somewhat non-linear: while prime duration equals the EOTs of the higher frequency words, then no FAE should be observed. 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. It should reach its maximum when the prime duration equals the EOTs of the lower frequency words. Beyond that point, increases in prime duration should no longer lead to increases in FAE, which should remain at its maximum magnitude, namely the difference between the EOTs of high- and low-frequency words.

Unfortunately, we do not observe this predicted pattern in experiments 1 and 2. For instance, 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 28ms is not significantly different from 33ms, the 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 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. However, while we did observe an increase in the magnitude of the repetition effect for the LF condition in experiment 2 (28ms in Exp.1 and 35ms in Exp.2), the same was true for the HF condition (18ms in Exp.1 and 26ms in Exp.2), and thus the FAE was identical in magnitude across experiments. 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 (18ms and 28ms 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 10ms, and that this value should not increase when prime duration increases. However, 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 across increases in prime duration, with a concomitant increase in repetition priming effects as a function of prime duration. *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 (ie. 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 assume that the exposure to the prime raises the rank order of the target word to the top of the search list, this would result in a boost that is a simple function of the repeated presentation, and therefore should not interact with prime duration. 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. 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 18ms (the repetition priming effect of high-frequency words). The repetition priming effect for low-frequency words (28ms) is then the the summation of the EOT (18ms) and the comparative speed up difference in retrieval time for the target words across high- and low-frequency condition (i.e, the FAE, which with this set of materials is 10ms). Unfortunately, this logic would fail to account for the results of experiment 2: if the EOT is indeed 18ms as shown in experiment 1, and the EOT is the only source of priming for high-frequency words, then this effect is already maxed out with a prime duration of 33ms. Why is it then that increasing the prime duration to 50ms in experiment 2 makes the effect approximately 8ms longer (26ms)? The only way this could be true is if the EOTs can vary substantially across samples of individuals; for instance, if the EOT for the sample in experiment 1 was 18ms, but it was 26ms for the sample in experiment 2. The problem with this assumption is that it seems arbitrary to assume that the EOT can vary substantially (here ~8ms) 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. Conclusion

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.

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**Wordlists**

*Experiment 1*

| related | unrelated prime | word | RT (to repetition) | | RT (to unrelated) | |
|---|---|---|---|---|---|---|
| | | | 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 |
| sound | death | sound | 538 | 127 | 552 | 103 |
| drink | north | drink | 559 | 129 | 556 | 122 |
| touch | young | touch | 541 | 122 | 573 | 121 |
| laugh | weird | laugh | 546 | 119 | 568 | 121 |
| black | reach | black | 553 | 131 | 563 | 114 |

non-word

| | | | | | | |
|---|---|---|---|---|---|---|
| alkew | grack | alkew | 599 | 153 | 591 | 140 |
| agink | furob | agink | 626 | 148 | 614 | 141 |
| ruzak | begro | ruzak | 577 | 130 | 584 | 142 |
| sondo | labok | sondo | 625 | 142 | 612 | 149 |
| guesh | gazzo | guesh | 702 | 184 | 721 | 194 |
| fadio | criam | fadio | 618 | 149 | 604 | 146 |
| plich | coreb | plich | 650 | 162 | 640 | 159 |
| sgrew | docab | sgrew | 626 | 182 | 638 | 182 |
| sceak | colob | sceak | 675 | 154 | 683 | 171 |
| ghisk | isloo | ghisk | 588 | 139 | 593 | 139 |
| deirm | ahuck | deirm | 589 | 142 | 596 | 139 |
| villo | flurb | villo | 632 | 182 | 615 | 181 |
| tidow | pikto | tidow | 648 | 167 | 624 | 160 |
| drick | aliom | drick | 684 | 168 | 681 | 172 |
| phick | purso | phick | 643 | 160 | 637 | 165 |
| nello | borno | nello | 625 | 156 | 612 | 151 |
| feach | pacaw | feach | 730 | 201 | 720 | 192 |
| tello | rilth | tello | 651 | 175 | 644 | 171 |
| dolio | caveb | dolio | 602 | 148 | 610 | 165 |
| gorgo | swysh | gorgo | 643 | 164 | 619 | 170 |
| whilo | lanjo | whilo | 612 | 137 | 604 | 150 |
| stanf | drief | stanf | 611 | 134 | 617 | 133 |
| crulk | ocheb | crulk | 671 | 162 | 665 | 169 |
| phumb | tunch | phumb | 645 | 160 | 633 | 148 |
| sirth | steaf | sirth | 612 | 141 | 618 | 145 |
| slerk | nohew | slerk | 640 | 153 | 634 | 163 |
| vitbo | nualm | vitbo | 593 | 151 | 596 | 154 |
| sunch | ofium | sunch | 665 | 165 | 665 | 161 |
| soeth | croik | soeth | 589 | 141 | 589 | 130 |
| eltow | valuo | eltow | 628 | 171 | 606 | 158 |
| framo | sorgo | framo | 617 | 146 | 618 | 146 |
| lumpo | shavo | lumpo | 630 | 162 | 635 | 172 |
| spuff | oceab | spuff | 672 | 169 | 667 | 183 |
| gatob | tolio | gatob | 599 | 139 | 606 | 155 |
| nosom | theck | nosom | 598 | 155 | 604 | 139 |
| gezzo | tooch | gezzo | 592 | 136 | 586 | 131 |

| | | | | | | |
|---|---|---|---|---|---|---|
| afoub | slonk | afoub | 582 | 133 | 589 | 128 |
| wateb | salch | wateb | 633 | 151 | 619 | 133 |
| nelch | raceb | nelch | 601 | 144 | 594 | 145 |
| dahoo | ahack | dahoo | 598 | 132 | 595 | 146 |
| driek | fideo | driek | 606 | 145 | 606 | 143 |
| gnask | fluko | gnask | 612 | 171 | 604 | 153 |
| brosk | cyrrh | brosk | 629 | 159 | 647 | 175 |
| duvez | revuo | duvez | 580 | 152 | 580 | 155 |
| fielm | cempo | fielm | 609 | 146 | 611 | 151 |
| pumph | exulk | pumph | 669 | 162 | 685 | 176 |
| gerif | kleck | gerif | 584 | 137 | 588 | 149 |
| racef | bonth | racef | 618 | 151 | 622 | 156 |
| pheek | scook | pheek | 640 | 155 | 644 | 176 |
| pruaw | slork | pruaw | 593 | 133 | 592 | 135 |
| guilm | whilf | guilm | 603 | 142 | 598 | 142 |
| lairf | drosh | lairf | 587 | 144 | 600 | 150 |

## Experiment 2

| | | | 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 |
| sound | death | sound | 534 | 127 | 559 | 147 |
| drink | north | drink | 550 | 124 | 563 | 116 |
| touch | young | touch | 545 | 145 | 567 | 120 |
| laugh | weird | laugh | 535 | 113 | 556 | 114 |
| black | reach | black | 547 | 129 | 578 | 129 |
| non-word | | | | | | |
| alkew | grack | alkew | 603 | 150 | 606 | 160 |
| agink | furob | agink | 632 | 161 | 615 | 155 |
| ruzak | begro | ruzak | 580 | 149 | 582 | 155 |
| sondo | labok | sondo | 619 | 168 | 621 | 178 |
| guesh | gazzo | guesh | 707 | 189 | 748 | 195 |
| fadio | criam | fadio | 632 | 169 | 602 | 171 |
| plich | coreb | plich | 645 | 148 | 646 | 180 |
| sgrew | docab | sgrew | 637 | 197 | 648 | 184 |
| sceak | colob | sceak | 692 | 179 | 692 | 165 |
| ghisk | isloo | ghisk | 595 | 149 | 594 | 152 |
| deirm | ahuck | deirm | 599 | 147 | 595 | 147 |
| villo | flurb | villo | 637 | 210 | 615 | 178 |
| tidow | pikto | tidow | 651 | 175 | 641 | 177 |
| drick | aliom | drick | 696 | 194 | 685 | 186 |
| phick | purso | phick | 648 | 176 | 639 | 168 |
| nello | borno | nello | 631 | 178 | 637 | 189 |
| feach | pacaw | feach | 748 | 201 | 745 | 208 |
| tello | rilth | tello | 665 | 185 | 662 | 174 |
| dolio | caveb | dolio | 618 | 175 | 609 | 157 |

| | | | | | | |
|---|---|---|---|---|---|---|
| gorgo | swysh | gorgo | 651 | 179 | 631 | 178 |
| whilo | lanjo | whilo | 619 | 150 | 615 | 164 |
| stanf | drief | stanf | 613 | 147 | 627 | 134 |
| crulk | ocheb | crulk | 678 | 173 | 679 | 183 |
| phumb | tunch | phumb | 655 | 175 | 648 | 180 |
| sirth | steaf | sirth | 623 | 146 | 616 | 136 |
| slerk | nohew | slerk | 644 | 167 | 638 | 172 |
| vitbo | nualm | vitbo | 590 | 143 | 596 | 154 |
| sunch | ofium | sunch | 664 | 162 | 676 | 172 |
| soeth | croik | soeth | 590 | 131 | 589 | 146 |
| eltow | valuo | eltow | 638 | 170 | 603 | 153 |
| framo | sorgo | framo | 626 | 165 | 631 | 162 |
| lumpo | shavo | lumpo | 634 | 172 | 635 | 164 |
| spuff | oceab | spuff | 683 | 182 | 665 | 204 |
| gatob | tolio | gatob | 607 | 167 | 594 | 154 |
| nosom | theck | nosom | 603 | 156 | 608 | 160 |
| gezzo | tooch | gezzo | 601 | 160 | 600 | 164 |
| afoub | slonk | afoub | 583 | 136 | 587 | 133 |
| wateb | salch | wateb | 640 | 159 | 641 | 160 |
| nelch | raceb | nelch | 609 | 163 | 595 | 145 |
| dahoo | ahack | dahoo | 611 | 157 | 597 | 154 |
| driek | fideo | driek | 613 | 152 | 610 | 151 |
| gnask | fluko | gnask | 606 | 154 | 606 | 159 |
| brosk | cyrrh | brosk | 637 | 165 | 649 | 180 |
| duvez | revuo | duvez | 590 | 168 | 576 | 154 |
| fielm | cempo | fielm | 621 | 181 | 614 | 160 |
| pumph | exulk | pumph | 687 | 184 | 697 | 202 |
| gerif | kleck | gerif | 587 | 154 | 586 | 147 |
| racef | bonth | racef | 619 | 164 | 621 | 151 |
| pheek | scook | pheek | 641 | 157 | 635 | 169 |
| pruaw | slork | pruaw | 599 | 157 | 598 | 143 |
| guilm | whilf | guilm | 620 | 168 | 603 | 163 |
| lairf | drosh | lairf | 590 | 138 | 597 | 146 |