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Meaning is in the beholder's eye: Morpho-semantic effects in masked priming

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Abstract

A substantial body of literature indicates that, at least at some level of processing, complex words are broken down into their morphemes solely on the basis of their orthographic form (e.g., Rastle, Davis, & New, 2004). Recent evidence has shown that this process might not be obligatory, as indicated by the fact that morpho-orthographic effects were not found in a cross-case same-different task, i.e., when lexical access is not necessarily required (Duñabeitia, Kinoshita, Carreiras, & Norris, 2011). In this study we employed a task that requires to understand a series of words, and thus implies lexical access. Masked primes were shown very briefly right before the appearance of the target word; prime-target pairs entertained either a morpho-semantic (*dealer-DEAL*), a morpho-orthographic (*corner-CORN*), or a purely orthographic relationship (*brothel-BROTH*). Eye fixation times clearly indicate facilitation for transparent pairs, but not for opaque pairs (nor for orthographic pairs, which were used as a baseline). Conversely, the usual morpho-orthographic pattern was found in a control experiment, employing a lexical decision task. These results indicate that the access to a morpho-orthographic level of representation is not always necessary for lexical identification, which challenges models of visual word identification that cannot account for task-induced effects.

Keywords: masked priming; task effects; morpho-orthographic segmentation; eye-tracking

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There is wide agreement that morphologically complex words (like *bakery* or *incorrect* or *misunderstanding*) are decomposed into their morphemic constituents during visual word recognition. However, there is no mutual consent as to how exactly segmentation unfolds. In this respect, one issue that has ignited a strong debate over the last years concerns whether segmentation is influenced solely by orthographic factors or also by semantic information.

All studies adopting paradigms that tap into late processing stages, e.g., long stimulus–onset–asynchrony (SOA) or cross-modal priming, indicate that semantics plays a crucial role in morphological decomposition, as evidenced by processing facilitation only for genuinely related prime-target pairs (*punishment-PUNISH*) as opposed to pseudo-related pairs (*inventory-INVENT*; e.g., Marslen-Wilson, Tyler, Waksler, & Older, 1994; Rastle, Davis, Marslen-Wilson, & Tyler, 2000; Rueckl & Aicher, 2008). These results indicate a level of morphological analysis that is sensitive to semantic transparency. However, because the above paradigms tap into late processing levels, these data are not incompatible with an early stage of morphological processing where semantic transparency does not play any role.

Along these lines, data from masked priming experiments suggest the existence of a morpho–orthographic routine that parses letter strings into morphemes solely on the basis of their orthographic form. In two seminal studies, Longtin, Segui, and Hallé (2003) and Rastle et al. (2004) found significant priming effects when the relationship between prime and target is both semantically transparent (e.g., *dealer–DEAL*) and opaque (e.g., *corner–CORN*), but not in an orthographic control condition (e.g., *brothel–BROTH*). Although this pattern of results was confirmed in several other experiments (see Rastle &

Davis, 2008), it is still unclear whether priming is of the same magnitude in transparent and opaque pairs (Diependaele, Duñabeitia, Morris, & Keuleers, 2011; Feldman, O'Connor, & Moscoso del Prado Martín, 2009; Järvikivi & Pyykkönen, 2011). Critically, however, it is undisputed that opaque pairs yield more facilitation than orthographic pairs in lexical decision, masked priming experiments, which is the critical comparison to prove the existence of a morpho-orthographic segmentation routine. Because this process was supposed to be in action before word identification takes place (i.e., at a pre-lexical level), it was always considered to be an obligatory step in the analysis of (pseudo–)complex letter strings.

The obligatoriness of morphological segmentation has been recently questioned by Duñabeitia et al. (2011). These authors employed a cross–case same–different task, which requires participants to judge whether a lowercase and an uppercase letter string, presented one after the other, are the same. By presenting the prime word for a very brief time between the reference word and the target, the authors found no morpho–orthographic (nor morpho–semantic) effect, i.e., equivalent facilitation emerged for *dealer–DEAL*, *corner–CORN*, and *brothel–BROTH*. Although this study has the important merit of showing that morpho-orthographic effects are sensitive to task manipulations even in masked priming paradigms, the cross–case same–different task used by Duñabeitia et al. (2011) does not necessarily require lexical access (one can easily judge whether two nonwords are identical, for example), and so these data are hardly informative as to how morpho–orthographic segmentation serves word identification. In other words, Duñabeitia et al. (2011) showed that morpho–orthographic processing is not necessarily in action any time a letter string is presented to the visual identification system; but this process might still be necessary in order to achieve lexical identification (see Crepaldi, Rastle, Coltheart, & Nickels, 2010).

In order to address this issue, we devised a new task, adapted from Baayen and

Marelli (2010). In this task, participants are asked to read and understand a word and a number presented simultaneously at the two lateral extremities of a computer screen. No action is required while participants are processing the stimuli; comprehension is tested off–line (i.e., after the stimuli have disappeared) on a proportion of trials through YES/NO questions. The fixation point is located where the word will appear, so as to ensure that people will look at the word first. The stimuli are presented for a very brief time (700 ms) so that participants will be forced to move away their eyes from the word as quickly as possible in order to process the number before it disappears. By doing so, we make (reasonably) sure that the fixation time on the word is the shortest possible time necessary to gain all the relevant information from the stimulus: this measure is thus taken as an index of processing speed, and is used as the dependent variable in our task. Crucially, a masked priming paradigm has to be introduced in order to be sure to tap into those (early) levels associated to the morpho–orthographic segmentation. The priming manipulation applies as in the standard lexical decision paradigm: a forward mask is presented after the fixation point in the same location of the screen, immediately followed by the prime, which in turn is immediately followed by the target. Priming is measured as the difference between the fixation time on the target word when this was preceded by a related word and the fixation time on the target word when this was preceded by a matched, unrelated word.

An additional advantage of this procedure is that we measure an implicit index of word processing time (fixation duration). This makes sure that the measure adopted is associated to word processing *per se*, excluding processing time related to decision making. Moreover, because this task requires the reader to understand the critical word, it clearly implies lexical access.

In order to guarantee a direct comparison with previous masked priming studies, we applied to this task the same manipulation used in those studies (e.g., Longtin et al., 2003), i.e., we compared priming in semantically transparent morphological pairs (e.g., *dealer–DEAL*), semantically opaque morphological pairs (e.g., *corner–CORN*), and purely orthographic pairs (e.g., *brothel–BROTH*). If morpho–orthographic segmentation is obligatory for lexical access in reading toward understanding, we should observe the usual pattern according to which *dealer–DEAL* and *corner–CORN* yield more facilitation as compared to *brother–BROTH*.

Experiment 1

Materials and Methods

Participants. 27 students from the University of Milano-Bicocca were recruited to participate in the study, in exchange for either credit courses or 2 Euros. Participants were all skilled readers and native speakers of Italian. They had normal or corrected–to–normal vision and no history of learning disability or neurological impairment.

Apparatus. An EyeLink 1000 eye-tracker manufactured by SR Research Ltd. (Canada) was employed in order to monitor participants' eye–movements during the experiment. A chin–rest support was used to maintain the position of the head constant, while a desktop camera sampled the pupil position at a frequency of 1000 Hz. The recording was monocular.

Materials. 150 prime–target pairs were selected from the Italian database CoLFIS (Bertinetto et al., 2005), equally assigned to three conditions. In the transparent condition primes and targets entertained a genuine morphological relationship (e.g., *artista–ARTE*, artist–ART). In the opaque condition primes and targets were semantically unrelated, but entertained an apparent morphological relationship; in fact, primes were fully parsable in a leftmost portion (homograph to an existent root), which was shared with the target, and a rightmost portion (homograph to an existent suffix) (e.g., *retaggio–RETE*, legacy–NET;

an analogous example in English would be *corner–CORN*). In the form condition, primes and targets had a purely orthographic relationship, i.e., the stem of the target was homograph to the leftmost portion of the prime, whose ending did not correspond to a suffix (e.g., *corallo–CORO*, coral–CHOIR; an analogous example in English would be *dialog-DIAL*). 150 control primes were also chosen. These were existent Italian morphologically complex words, which did not entertain any relationship (either semantic, morphological or visual) with the corresponding targets. Targets were matched as closely as possible across conditions for frequency, length, and orthographic neighborhood size (N) (Table 1a). Related primes and paired control primes were matched as closely as possible for the same variables (Table 1b). Moreover, we also matched across conditions the orthographic overlap between targets and the corresponding related and unrelated primes, calculated using the spatial coding for letter position (Davis, 2010; Table 1b). Finally, we matched related primes for their letter transitional probabilities at the morphemic boundary (Table 1b).

INSERT TABLE 1 HERE

The assignment of word targets to the two priming conditions was counterbalanced over participants, so that all participants received both related and control primes, but saw each target only once. This was achieved by creating two experimental lists, which were submitted to different groups of participants.

Procedure. Trials began with a fixation point located in the upper-left quarter of a computer screen, in correspondence to the center of the following prime stimulus. A hash-mark mask followed and remained on the screen for 500 ms; this extended to the whole screen width, interrupted only by a cross superimposed to the fixation point. The mask was then substituted by the prime stimulus, which was presented in lowercase and remained on the screen for 35 ms, along with a "0" in the righthand portion of the screen.

The exact position of the prime word was determined dynamically on each trial so that is was centered on the fixation point. Finally, the screen including the target stimuli was presented for 700 ms. The uppercase target stimulus was presented in the same position as the prime, while a one-digit number (ranging from 1 to 8) was presented in the righthand portion of the screen, symmetrically to the screen center. The appearance of the target screen triggered the registration of eye movements. The trial timeline is represented in Figure 1.

INSERT FIGURE 1 HERE

All elements were presented in grey Courier New case (32 pt) on a black screen. Considering a mean viewing distance of 60 cm, each character subtended 1.33° of visual angle. About 15% of the trials were followed by a question regarding either the meaning of the word (e.g., does the word refer to a tool?) or the number (e.g., is the number odd?).

Participants were asked to read the word for comprehension, and then look at the number. They were told that questions could have been asked about either the word or the number at the end of each trial, and were instructed to answer them by pressing either a YES or a NO button on a response pad. No mention was made of the presence of the prime words.

Before the experiment, the eye-tracker was calibrated employing a three–point grid at the center of the screen. Before each trial, the fixation point was exploited to check fixation drift. In order to let the participants familiarize with the task, a practice session consisting of 10 trials was run at the beginning of the experiment. The whole experimental session lasted between 10 minutes and 15 minutes.

Data analysis. The durations of the first fixation and the gaze on the target word were adopted as dependent measures; gaze duration was defined as the total amount of time spent with the eyes on the stimulus, before fixating away from it. Mixed-effects models (Baayen, Davidson, & Bates, 2008) were employed as primary statistical tool. The effects of interest were those associated to the experimental manipulations, i.e., *relatedness* (control vs. related prime) and *condition* (transparent vs. opaque vs. form), and their mutual interaction. In order to account for more error variance, a number of covariates were also considered. Length, log-transformed frequency, and N of both prime and target, as well as trial number, were introduced in the initial model. Random intercepts for participants and items were also introduced. Effects were evaluated one by one on the basis of likelihood ratio tests: those whose inclusion did not increase significantly the model goodness of fit were removed from the analysis. After having identified the best model with this procedure, atypical outliers were identified and removed (employing 2.5 SD of the residual errors as a criterion). Statistics in the refitted models are reported. The statistical significance of the fixed parameters was evaluated using Markov chain Monte Carlo sampling (pMCMC).

Results

Only data about fixations on target stimuli were considered. Fixations that either preceded or followed a blink were excluded from the analyses, as well as refixations on the targets (i.e., fixations that went back to the word after having fixated away from it, observed in 13% of the trials). Datapoints that deviated from a normal distribution were also excluded. The analyzed data set consisted of 3905 valid datapoints.

The average number of fixations on the target was 1.6 (SD=.6), the average first fixation duration was 256 ms (SD = 71), and the average gaze duration was 330 ms (SD = 88). 50% of the targets required a single fixation for reading, 45% required two fixations, 5% required either three or four fixations.

First Fixation Durations.

INSERT TABLE 2 HERE

Table 2 summarizes the first fixation durations in the different experimental situations. The interaction between *relatedness* and *condition* was significant (F = 4.26, p=.0142). No significant priming effect emerged either in the form condition or in the opaque condition. However, a significant effect was found in the transparent condition: first–fixation durations were shorter for target words preceded by a morpho-semantically related prime, in comparison to target words preceded by a control prime. Table 3 reports the parameters of the significant effects included in the final model.

INSERT TABLE 3 HERE

Gaze Durations.

INSERT TABLE 4 HERE

Table 4 summarizes the gaze durations in the different experimental situations. The interaction between *relatedness* and *condition was* significant (F = 6.25, p=.0019). No significant priming effect emerged either in the form condition or in the opaque condition. However, a significant effect was found in the transparent condition: gaze durations were shorter for target words preceded by a morpho–semantically related prime, in comparison to target words preceded by a control prime. Table 5 reports the parameters of the significant effects included in the final model.

INSERT TABLE 5 HERE

Discussion

The present results ideally complements the evidence provided by Duñabeitia et al. (2011) in showing that morpho–orthographic effects are sensitive to task requirements. These authors employed a cross–case, same–different task, which arguably taps into early orthographic processing and does not necessarily involve lexical access. Therefore, one may argue that morpho–orthographic effects could not be found in Duñabeitia et al. (2011) experiment because the processing stage at which morpho–orthographic segmentation occurs was not even reached. On the contrary, our experimental conditions entail semantic processing, which obviously requires lexical access. Thus, the lack of priming among opaque pairs in our study does not only indicate that morpho–orthographic segmentation is not necessarily in place every time we are exposed to printed complex words, but also that this process is not always needed for lexical access.

Before taking this conclusion, however, we need to confirm that the pattern of results emerged in Experiment 1 would turn into the typical difference between morpho–orthographic and form condition in a classical lexical-decision, masked-priming experiment. This would be direct evidence that this lack of morpho–orthographic effect does not depend on the specific items that we used, or perhaps on some peculiar feature of the Italian language (in fact, pseudo-derived words were never shown to prime their pseudo–stems in this language). To this aim, we ran a traditional masked priming, lexical decision experiment to confirm that there is some *corn* in the *corner* also in Italian.

Experiment 2

Materials and Methods

Participants. 58 participants from the same population that took part in Experiment 1 were recruited to participate in the study. None of them had also taken part in Experiment 1.

Materials. The same 150 prime–target pairs that were used in Experiment 1 were also used in this study. They were also counterbalanced over participants as in Experiment 1.

Procedure. Participants were seated in front of a computer screen and instructed to decide whether the letter strings appearing on the screen were existing Italian words. They were informed that the target word would be preceded by a string of hash mark as a warning signal, but no mention was made about the presence of the prime word. In order to familiarize with the task, participants were given six practice trials. Moreover, each experimental session began with five warm-up trials that were not analysed so as to avoid outlier response due to unfamiliarity with the task.

Each trial started with a string of hashmarks displayed for 500 ms, which was followed by the prime word presented in lowercase for 35 ms. The target word appeared on the screen immediately after the prime offset and remained on the screen until participants' response.

Stimulus presentation and data recording was accomplished via Matlab and its Psychtoolbox functions. A response box was used to collect lexical decision times; the YES response button was always controlled by the participant's dominant hand.

Data analysis. Data were analysed as in Experiment 1, with the only exception that response times were negative inverse-transformed in order to make their distribution more Gaussian-like.

Results

INSERT TABLE 6 HERE

Table 6 reports mean response times in the experimental conditions. Mixed–effects models revealed an interaction between *relatedness* and *condition* ($F = 31.79$, p=.0001): priming emerged in the opaque condition and in the transparent condition, but not in the orthographic condition (Table 7). Priming was also larger in the transparent than in the opaque condition (Estimate =-.06, pMCMC=.0001).

INSERT TABLE 7 HERE

Discussion

Results confirm that the same prime–target pairs used in Experiment 1 give rise to the traditional morpho–orthographic effect in a standard lexical decision, masked priming study. Pseudo–derived words (e.g., *brother*) prime their pseudo–stems (e.g., *broth*) more than orthographic primes (e.g., *brothel*) do, thus proving that they are parsed into their morphemes on the basis of their orthographic form. Critically, this excludes that the lack of a morpho–orthographic effect in Experiment 1 was due to some peculiar features of Italian, or of these specific items. Moreover, these results confirm that the traditional pattern of morpho–orthographic priming is observed also in a language that, differently from English, does not present free stems in derived words.

General Discussion

The experiments described in this paper show that it is possible to set task requirements so that morpho–orthographic effects do not emerge *in a masked priming environment*, i.e., where they are reliably reported in classical lexical decision tasks. Critically, and contrary to Duñabeitia et al. (2011), this was achieved with a task *that requires lexical access*, thus showing that morpho–orthographic segmentation is not always necessary for the visual identification of complex words. This conclusion challenges substantially existing models of visual-word processing.

It is not clear which factor might have exactly determined the lack of morpho-orthographic effects in Experiment 1. In fact, as compared to the standard lexical decision experiment described in Experiment 2 (which led to the usual morpho-orthographic pattern), Experiment 1 procedure employed (i) a new dependent variable (eye fixation time, rather than decision time) and (ii) a new task (comprehension, rather than lexical decision). This latter factor is indeed more likely to be responsible for our data, since several experiments have documented task-induced changes of established patterns of results in masked priming (e.g., Norris & Kinoshita, 2008, Bueno & Frenck-Mestre, 2008). However, the crucial thing to note is that, independently of whether we blame the task or the dependent variable for the lack of morpho–orthographic effects, the main point illustrated by this experiment remains valid: morpho–orthographic segmentation is not always necessary to achieve lexical identification, because if it were, we would observe morpho–orthographic effects any time readers identify words, i.e., in *any* task involving word identification and measuring *any* index of word identification time.

Clearly, the present results do not speak against the reliability of morpho-orthographic effects in lexical decision: those data are solid and surely indicate the existence of a processing level where morphemes are primarily identified on orthographic grounds. However, our data shed new light on the interpretation of this phenomenon. For example, most one-route, localist models of the visual identification of complex words include a morpho-orthographic segmentation stage as an obligatory step in order to access the lexicon (Crepaldi et al., 2010, Taft, 2004), which is clearly not tenable given the present results. These models should be extended to account for the fact that morpho–orthographic segmentation is triggered by lexical decision task, but may be by–passed when trying to understand the meaning of a word (at least under the specific conditions imposed by our task).

Prima facie, models positing multiple processing routes (e.g., Kuperman, Schreuder, Bertram, & Baayen, 2009; Grainger & Ziegler, 2011) could account more easily for the present data, under the assumption that task requirements can modulate the relative importance of their different routes. However, the relationship between task requirements and processing strategies can be very intricate, and is often underspecificied in these models. For example, although Kuperman et al. (2009) predicts that both form–based and semantic–based cues can be activated in parallel even before the whole word is read (see

Marelli & Luzzatti, 2012), it is not clear how task settings might affect these dynamics. Similarly, with reference to the model by Grainger and Ziegler (2011), it is not clear why a morpheme-based route should operate when processing is mainly focused on lexicality, and shut down when processing is mainly focused on comprehension.

Alternatively, a different perspective could be adopted in the way models are conceived. In place of fixed modules and rigid architectures, we could hypothesize a processing style characterized by temporary representations, employed *ad-hoc* in relation to task requirements. For example, the present data might be explained in the context of the model for masked priming proposed by Norris and Kinoshita (2008) following the Bayesian Reader (Norris, 2006). In that framework, readers develop a series of hypotheses to keep under scrutiny in order to fulfill the requested tasks. These hypotheses are tested by processing the stimuli (both primes and targets) as evidence sources in accordance with the experimental conditions. Under these assumptions, it is reasonable to think that when people are to make lexical decisions, processing is focused on lexical and orthographic properties, with the semantic relationship between primes and targets being irrelevant. In these conditions, readers need to be quick and efficient in identifying words, and chunking frequent letter combinations – as (pseudo-)morphemes are – is an efficient strategy in this respect, no matter whether those chunks contribute to meaning (i.e., they are genuine morphemes) or not (i.e., they are pseudo-morphemes). Conversely, when the task implies semantic access, processing would be focused on word meanings, and thus the semantic relationship between primes and targets would be crucial, irrespective of the morphological properties of the two words: *dealer* will provide evidence for the meaning of *DEAL*, and the same will not happen for *corner* and *CORN*. Clearly, several details are lacking in this account, but its main principle fits well with the task sensitivity shown by morpho-orthographic effects.

In sum, the present data show that morpho-orthographic segmentation is not always necessary in order to achieve the lexical identification of complex words. By doing so, they challenge in several important ways all existing models of visual word identification. It is possible to see ways in which these models might be modified in order to account for this new evidence; however, it is less clear how these modifications should be specifically implemented computationally, which leaves room for the adjudication process to take place.

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Authors' contributions are as follows: Marco conceived the experiment; Elena and Simona created the stimuli, with contributions from Marco and Davide; Elena and Simona collected the data; Marco and Davide analyzed the data; Marco and Simona drafted the paper, which was critically revised by all authors; Davide supervised the project

Table 1

Matching of relevant variables for targets and primes. Frequency values refer to the raw number of occurrences in a corpus of 3,800,000 word forms (Bertinetto et al., 2005). Bigram trough depth was operationalized as $|\log BF_a - \log BF_b| + |\log BF_c - \log BF_b|$*, where* BF_a *is the frequency of the bigram immediately preceding the boundary,* BF_b *is the frequency of the bigram straddling the boundary, and* BF_c *is the frequency of the bigram immediatly after the boundary.*

		Condition		
		Transparent	Opaque	Form
(a) Targets				
Log-frequency		1.87±.56	1.45±.82	1.46±.79
Length		5.44±1.21	4.71±.81	5.12±1.02
Orthographic neighborhood size		5.22±3.56	6.96±4.71	7.58±4.31
(a) Primes				
Log-frequency	(related)	.96±.73	1.04±.74	1.03±.78
	(control)	.99±.67	1.08±.67	1.11±.64
Length	(related)	8.11±1.36	8.18±1.45	7.42±1.39
	(control)	8.11±1.36	8.16±1.47	7.46±1.41
Orthographic neighborhood size	(related)	.92±1.01	.91±1.15	1.24±1.44
	(control)	1.48±1.27	1.31±1.52	1.76±1.77
Orthographic overlap	(related)	.81±.11	.82±.11	.80±.12
	(control)	.21±.11	.21±.12	.18±.09
Bigram trough depth	(related)	.65±.46	.65±.41	.85±.51

Table 2

Mean first fixation durations and standard errors of the mean (in ms) in the different experimental situations

	Transparent		Opaque		Form	
	mean	SEM	mean	SEM	mean	SEM
Control prime	261	1.18	253	1.11	255	1.14
Related prime	251	1.03	258	1.16	254	1.19

Table 3

Fixed effects in the final model on first fixation durations. The included covariates had significant effects and significantly improved the model goodness-of-fit

Fixed Effect	Estimate	Std. Error	t value	pMCMC
Intercept	259.86	9.93	26.18	.0001
Relatedness:Related	.55	3.18	.17	.8668
Condition:Opaque	1.37	3.51	.39	.6984
Condition:Transparent	9.29	3.56	2.61	.0074
Relatedness:Related*Condition:Opaque	2.66	4.44	.61	.5514
Relatedness:Related*Condition:Transparent	-9.61	4.44	2.16	.0321
Target length	3.76	1.46	2.58	.0086
Trial number	.05	.02	2.38	.0208

Table 4

Mean gaze durations and standard errors of the mean (in ms) in the different experimental situations

	Transparent		Opaque		Form	
	mean	SEM	mean	SEM	mean	SEM
Control prime	345	1.45	329	1.36	325	1.41
Related prime	329	1.36	330	1.36	324	1.48

Table 5

Fixed effects in the final model on gaze durations. The reported covariates had significant effects and significantly improved the model goodness-of-fit

Fixed Effect	Estimate	Std. Error	t value	pMCMC
Intercept	263.14	13.99	18.81	.0001
Relatedness:Related	1.16	3.34	.35	.7328
Condition:Opaque	-3.12	3.74	.83	.4028
Condition:Transparent	5.89	3.88	1.52	.1202
Relatedness:Related*Condition:Opaque	-3.82	4.66	.82	.4224
Relatedness:Related*Condition:Transparent	-15.79	4.66	3.38	.0006
Prime length	3.51	1.21	2.89	.0034
Target length	11.28	1.79	6.31	.0001
Target N	-.98	.36	2.72	.0048
Target frequency	-3.48	1.72	2.03	.0384
Trial number	-.08	.02	3.72	.0002

Table 6

Mean response times and standard errors of the mean (in ms) in the different experimental situations

	Transparent		Opaque		Form	
	mean	SEM	mean	SEM	mean	SEM
Control prime	613	1.63	637	1.69	642	1.81
Related prime	575	1.49	620	1.82	641	1.93

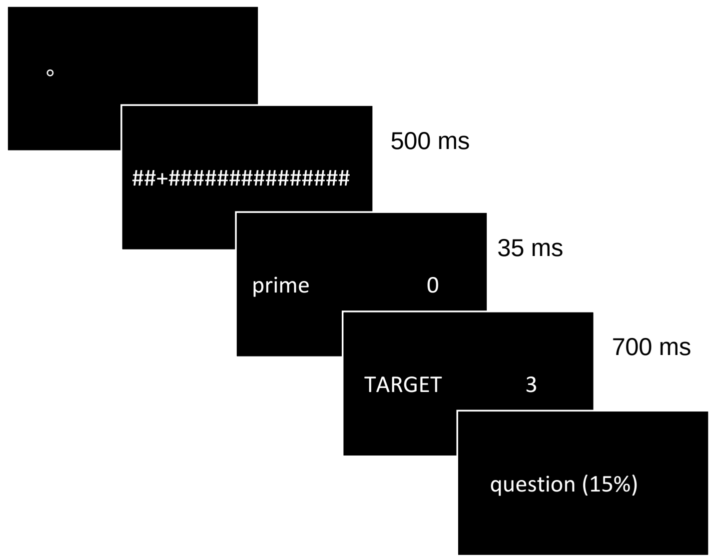
Table 7

Fixed effects in the final model on response times. The reported covariates had significant effects and significantly improved the model goodness-of-fit

Fixed Effect	Estimate	Std. Error	t value	pMCMC
Intercept	-1.48	.03	46.52	.0001
Relatedness:Related	-.02	.01	1.68	.0956
Condition:Opaque	-.01	.02	.63	.5038
Condition:Transparent	-.04	.02	2.11	.0304
Relatedness:Related*Condition:Opaque	-.05	.01	3.47	.0002
Relatedness:Related*Condition:Transparent	-.11	.01	7.94	.0001
Target *N*	-.01	.01	1.88	.0502
Target frequency	-.08	.01	8.24	.0001

Figure Captions

Figure 1. Timeline of trial presentation



°
##+###############
500 ms
prime 0
35 ms
TARGET 3
700 ms
question (15%)