MASKED SUFFIX PRIMING AND MORPHEME POSITIONAL CONSTRAINTS

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RUNNING HEAD: MASKED PRIMING AND SUFFIX IDENTIFICATION

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Abstract

Although masked stem priming (e.g., dealer–DEAL) is one of the most established effects in visual word identification (e.g., Grainger et al., 1991), it is less clear whether primes and targets sharing a suffix (e.g., kindness–WILDNESS) also yield facilitation (Giraudo & Grainger, 2003; Duñabeitia et al., 2008). In a new take on this issue, we show that prime nonwords facilitate lexical decisions to target words ending with the same suffix (*sheeter–TEACHER*) compared to a condition where the critical suffix was substituted by another one (*sheetal–TEACHER*) or by an unrelated non–morphological ending (*sheetub– TEACHER*). We also show that this effect is genuinely morphological, as no priming emerged in non–complex items with the same orthographic characteristics (*sportel–BROTHEL* vs. *sportic–BROTHEL* vs. *sportur–BROTHEL*). In a further experiment, we took advantage of these results to assess whether suffixes are recognized in a position–specific fashion. Masked suffix priming did not emerge when the relative order of stems and suffixes was reversed in the prime nonwords— *ersheet* did not yield any time saving in the identification of *teacher* as compared to either *alsheet* or *obsheet*. We take these results to show that *–er* was not identified as a morpheme in *ersheet*, thus indicating that suffix identification is position specific. This conclusion is in line with data on interference effects in nonword rejection (Crepaldi, Rastle, & Davis, 2010), and strongly constrains theoretical proposals on how complex words are identified. In particular, because these findings were reported in a masked priming paradigm, they suggest that positional constraints operate early, most likely at a pre–lexical level of morpho–orthographic analysis.

Keywords: visual word identification, suffix identification, position coding, masked suffix priming.

It has long been known that morphological structure plays a crucial role in the identification of printed words (e.g., Bradley, 1979; Taft & Forster, 1976). One critical piece of evidence in this respect is stem priming, that is, the fact that the recognition of a stem morpheme (e.g., *dark*) is facilitated by the prior presentation of a morphologically-related word (e.g., *darkness*) more than would be expected on the basis of semantic or orthographic information (e.g., Drews & Zwitserlood, 1995; Rastle, Davis, Marslen-Wilson, & Tyler, 2000). This phenomenon has been observed in a number of different paradigms, including masked (e.g., Feldman, 2000), unmasked (Raveh, 2002), long-lag (e.g., Stanners, Neiser & Painton, 1979) and cross-modal priming (e.g., Marslen-Wilson, Tyler, Waksler & Older, 1994). Stem priming has typically been taken to show that the prime is parsed into its morphemes, facilitating identification of the morpheme that is shared by the prime and the target.

On this account, one should observe facilitation whenever the prime and the target share a morpheme, including when they share an affix (e.g., rewrite-RETHINK, graceful-MINDFUL). However, the experimental evidence in this respect is rather mixed. Masked priming with prefixes has been reported in three different languages by Chateau, Knudsen and Jared (2002; English), Giraudo and Grainger (2003; French), and Dominguez, Alija, Rodriguez-Ferreiro and Cuetos (2010; Spanish). In all these experiments, prefix priming (e.g., bilateral–BILINGUAL) has been contrasted with prime–target pairs with similar orthographic overlap, but where either the prime or the prime and the target did not have a morphological structure (e.g., element–ELEVATOR, billiards–BILINGUAL). This strongly suggests that the effect is truly morphological in nature.

The evidence regarding suffix priming is less clear. Giraudo and Grainger (2003; Experiment 1) tested masked suffix priming in French (e.g., *veston*, little jacket, preceding

CHATON, little cat), both against a condition where primes shared a final pseudo–suffix with the targets (e.g., *béret*, beret, preceding *MURET*, little wall[1]) and against an unrelated baseline (e.g., *crabe*, crab, preceding *MURET*). They found no evidence of any morphological facilitation in either comparison. In their Experiment 4, Giraudo and Grainger (2003) increased the proportion of letters shared by primes and targets (e.g., *rouage*, cogwheel, and *PLIAGE*, folding) and used stimuli in which morphemic and syllable boundaries overlapped. Under these conditions, they were able to show masked suffix priming, but only against the unrelated baseline and only in the subject analysis (26 ms; $F1(1,27)=7.56$, $p<.05$, $F2(1,29)=2.01$, $p=.10$). No significant difference emerged in the more stringent comparison with pseudo-suffixed primes (4 ms). The contrast between the lack of suffix priming in Experiment 1 and the presence of the same effect in Experiment 4 suggests an important role for orthographic and phonological factors, which do not seem to affect stem priming. In addition, the effect emerged in Experiment 4 was statistically weak. These considerations cast doubt on the robustness of suffix priming in French, or at least on the fact that such an effect is fully morphological in nature.

In contrast, Duñabeitia, Perea, and Carreiras (2008) reported clear evidence for masked suffix priming in Spanish (SOA = 50 ms) independently of whether the primes comprised suffixes in isolation (e.g., *dad–IGUALDAD*, ity–EQUALITY; Experiment 1), nonsense symbol strings and suffixes (e.g., *%%%%dad–IGUALDAD*; Experiment 2), or other derived words (e.g., *brevedad-IGUALDAD*, *brevity–EQUALITY*; Experiment 3). These effects were always reliably larger than those emerging when primes and targets shared non–morphological letter clusters of comparable length (e.g., *men–CERTAMEN*, est–contest; *%%%%men–CERTAMEN*; *volumen–CERTAMEN*, volume–CONTEST), suggesting that suffix priming was

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genuinely morphological in these experiments and could not be interpreted in purely orthographic terms.

Overall, prefix priming seems to be robust—different experiments (in different languages) have found it to hold against a non–morphological, orthographically matched baseline. In contrast, evidence regarding suffix priming is inconclusive—the effect was found in Spanish against appropriately matched orthographic controls, but did not emerge in French against the same baseline (and even against a completely unrelated baseline, it depended on orthographic and phonological factors such as syllable overlap). Experiment 1 was therefore a new attempt at assessing suffix priming. The experiment was carried out in English, a language where this effect has never been investigated. Three important variants were introduced as compared with previous studies. First, we used nonword primes (e.g., sheet*er*–TEACH*ER*), thus minimizing any possible lexical competition between primes and targets (e.g., Davis & Lupker, 2006; Segui & Grainger, 1990) that may have obscured suffix priming in previous investigations (e.g., Giraudo & Grainger, 2003). Second, two different baselines were used, that is, one in which unrelated primes also had a (pseudo)morphological structure (e.g., sheet*al*–TEACHER) and one in which they had no such structure (e.g., sheet*ub*–TEACHER). This is important, because morphologically structured nonwords are entirely parsable into existing entities (morphemes), and thus may bias participants towards a YES response *independently* of their relationship with the target. By requiring that facilitation for sheeter–TEACHER must hold against both a morphological and a non–morphological baseline we ensured that any priming could be unequivocally attributed to the suffix shared between primes and targets. Of course, we also needed to make sure than any priming effect could not be entirely attributed to orthography—finding shorter response times in sheeter–TEACHER as compared to sheetal–

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TEACHER and sheetub–TEACHER may be simply due to the fact that related primes and targets share their final letters. So, the three morphological conditions were mirrored in three orthographic control conditions with monomorphemic targets (e.g., colour*el*–APPAR*EL* vs. colour*ic*–APPAR*EL* vs. colour*ut*–APPAR*EL*). Related pairs only share their final letters here, which do not constitute a suffix, thus tracking for purely orthographic effects.

Overall, the experimental design included six conditions coming from two crossed independent variables, that is, target type (suffixed vs. simple words) and prime type (related vs. suffixed control vs. unrelated control). In order to claim genuine suffix priming, we needed to obtain an interaction between target and prime type, reflecting a larger difference between sheeter–TEACHER and sheetal–TEACHER/ sheetub–TEACHER than between colourel–APPAREL and colouric–APPAREL/colourut–APPAREL.

Experiment 1

Method

Participants

Forty–five students at Royal Holloway, University of London participated in the experiment. Participants were native speakers of English and had normal or corrected-to-normal vision; they also had no history of learning disabilities and/or neurological impairments. Participants were paid £5 for their participation.

Materials

Thirty–six English derived words and thirty–six English monomorphemic words were selected as targets. The N-Watch program (Davis, 2005) was used to ensure that complex and

simple targets were matched for length in letters (pairwise), number of syllables, number of orthographic neighbours (*N*) and mean log bigram frequency (*MLBF*; see Table 1). Frequency values were taken from SUBTLEX–UK (Van Heuven, Mandera, Keuleers & Brysbaert, 2014), and were matched across conditions. Letter transition probabilities were also kept closely similar across simple and complex target words, both at the morphemic boundary (or at the corresponding point in monomorphemic controls) and averaged within suffixes/word endings (see Table 1).

TABLE 1 ABOUT HERE

Derived target words were paired with three different prime nonwords. In the suffix priming condition, primes constituted non-existing combinations of an existing stem and the suffix that was included in the target word (e.g., sheet*er* was chosen as a prime for teach*er*). In the suffix–control condition, nonword primes were composed of the same stems used in the related primes and of an unrelated suffix (e.g., sheet*al*). In the non–morphological control condition, the same stems were concatenated with unrelated, non–morphological endings (e.g., sheet*ub*). Stems in the nonword primes were always of the same length as the stems in the target words. Unrelated suffixes and non–morphological endings in the control primes were of the same length as their corresponding suffixes (e.g., *–ic* and *–fu* were paired with *–ly*, while *–ise* and *–olf* were paired with *–ary*).

Monomorphemic target words were paired with three different nonword primes constructed in the same fashion. In the word–ending priming condition, nonword primes were combinations of an existing stem and the non–morphological ending of the target word (e.g.,

poll*el* was paired with barr*el*). In the control conditions, nonword primes comprised the same stems used in the related primes together with either an unrelated suffix (e.g., poll*ic*) or an unrelated non–morphological ending (e.g., poll*ut*).

The length of the non–morphological endings was matched pairwise with the length of the suffixes in the suffix priming condition. Pairwise matching between related primes to complex and monomorphemic targets was also sought for syllabic structure. When the morphemic boundary overlapped with the syllabic boundary in the suffix priming condition (e.g., *passment*), we ensured that this was also the case for the non–morphological ending in the monomorphemic target condition (e.g., *falltude*, where *tude* was the non–morphological ending). Similarly, when the morphemic boundary did not coincide with the syllabic boundary in the suffix priming condition (e.g., *divertory*), we ensured that the same was the case for the corresponding monomorphemic target (e.g., *sportel*, where *el* was the non–morphological ending).

The stems used in the complex and monomorphemic conditions were matched pairwise for length in letters, and kept closely comparable for log frequency per million words and number of orthographic neighbours (see Table 2).

In order to make certain that the orthographic relationship between primes and targets was comparable across target types, we used the computer program MatchCalculator (freely available at http://www.pc.rhul.ac.uk/staff/c.davis/Utilities/MatchCalc/index.htm) to compute the degree of orthographic overlap for each prime–target pair using the spatial coding (e.g., Davis & Bowers, 2006) and the open bigram coding (e.g., Grainger & Whitney, 2004) models of letter position. These figures confirmed that the difference in orthographic overlap with the target between related, suffix control and unrelated control primes was comparable in complex and monomorphemic targets (see Table 2).

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Finally, we computed transition probabilities (i) at the boundary between prime stems and prime endings (morphological or not) and (ii) within prime endings (again, morphological or not). Transition probabilities at the boundary between stems and endings were closely matched across prime conditions, both in the complex–target condition and in the simple–target condition (see Table 2). Transition probabilities within word endings varied a bit more across prime types, but still remained very low (see Table 2).

TABLE 2 ABOUT HERE

Despite the rigorous matching of our materials, we included each of the variables described above in our statistical models as covariates (see below). The complete list of the prime and target stimuli used for word trials in Experiment 1 is given in Appendix A.

The stimulus set also included 72 legal nonwords. Half of these nonwords were morphologically structured (i.e., were non–existing combinations of an existing stem and an existing suffix, like *betage*), whereas half were not (e.g., *delktad* or *tostreb*)[2]. None of the suffixes or stems used in the word trials was also used for the preparation of the nonword targets. Simple nonwords were generated through the ARC nonword database (Rastle, Harrington, & Coltheart, 2002). Target nonwords were similar to the target words in length and number of orthographic neighbours.

Each nonword target was paired with a nonword prime. In order to create a nonword trial set that mirrored perfectly the word trial set, one third of the complex nonwords and one third of the simple nonwords were paired with nonword primes that were related to them as in the suffix priming (e.g., *fitage–BETAGE*) or word–ending priming conditions (e.g., *friskad–DELKTAD*); in another third of the trials, nonword targets were paired with suffix control

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primes (e.g., *jarion–MUDATE*, *zoonion–STIRDEK*); and in another third, they were paired with completely unrelated primes (e.g., *dirtak–LONGEN*, *fatok–TUDAP*). As for the nonword targets, we ensured that nonwords–trial primes were comparable to the word–trial primes for length and number of orthographic neighbours. Moreover, none of the stems used to create the word-trial set was also used in the preparation of the nonwords–trial primes.

The complete list of the prime and target stimuli used for nonword trials in Experiment 1 is given in Appendix B.

Procedure

Participants were tested in a dimly lit room. They were seated in front of a computer screen and instructed to decide whether or not the letter strings appearing on the screen were existing English words. They were also told that the letter strings would be preceded by a string of hash marks as a warning signal, but no mention was made of the presence of the prime words. Participants were given 8 practice trials to familiarize themselves with the task; further, each experimental session began with five warm-up filler trials that were not analysed.

Each trial started with a string of hash marks presented in the centre of the computer screen for 500 ms. The prime word was presented in lowercase after the warning signal offset and remained on the screen for 42 ms; it was then followed by the uppercase target string on which the subject had to make a lexical decision. The target string remained on the screen until the participant's response and was then replaced by a 1-second blank serving as inter-stimulus interval.

Stimulus presentation and data recording were accomplished via the DMDX software (Forster & Forster, 2003). A two-button response box was used to record lexical decisions, in which the YES response button was always controlled by the dominant hand. The assignment

of word targets to the three priming conditions was counterbalanced over participants through the creation of three parallel versions of the experiment; thus, all participants received primes from each condition, but saw each target only once. Within each version, trial presentation was pseudo-randomized so that no more than 4 consecutive word or nonword targets could occur in a row. This design also ensured that no more than four experimental items were presented in eight consecutive trials.

Results

Average response times and error rates are illustrated in Table 3. Prior to analysing the data, we removed three subjects who had high error rates on nonword trials (> 30%), and we removed one target word that also had a high error rate (40%). Remaining data were analysed through mixed effect models with crossed random intercepts for participants and target words. Any covariate that explained a significant amount of variance was included in the model. In the RT analysis, these covariates included target frequency (modelled non–linearly through restricted cubic splines) and number of letters, and in the accuracy analysis, they included target frequency (again modelled non–linearly). Inverse–transformed response times were used as the dependent variable in the analyses (Ulrich & Miller, 1994). Following Bates, Maechler, Bolker and Walker (2013), whole factor and parameter significance was assessed through chi-square tests and confidence intervals estimation based on the likelihood ratio test (see also Jaeger, 2008). The role of subject–related variance in the effects of interest was assessed through subject random slopes for relatedness and target type. Because this did not improve the model goodness of fit (thus showing that the significant effects were reliable across participants), these random slopes were not included in the final models.

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TABLE 3 ABOUT HERE

The analyses of response times revealed a significant effect of target type, Chi-square[1] = 4.92, p = .03, and an interaction between prime type and target type, Chi-square[2] = 7.30, p = .02. In order to characterise this interaction, separate models were fitted to complex and simple targets. No significant effect of prime type emerged with simple targets, Chi-square[2] = 1.63, p = .44, whereas this factor was significant for complex targets, Chi-square[2] = 18.90, p < .001, in reflection of the fact that suffix primes elicited shorter response times than both suffix controls (677 ms vs. 712 ms, 95% confidence interval for the relevant model parameter: .04 – .11[3]), and unrelated controls (677 ms vs. 700 ms, 95% confidence interval for the relevant model parameter: .004 – .07),

Accuracy analysis revealed a significant effect of prime type, Chi–square[2] = 6.07, p = .048. No other effect was significant (all ps > .12).

Discussion

The results of Experiment 1 show that suffix priming is observed when derived words (e.g., *teacher*) are primed by complex nonwords that include the same suffix (e.g., *sheeter*), compared to a condition where the target word is preceded by a nonword made up of the same stem and an unrelated letter cluster (e.g., *sheetub*) or a different suffix (e.g., *sheetal*). No facilitation is observed when the letter cluster that is shared by primes and targets does not constitute a morpheme (*pollel* does not prime *barrel* as compared to either *pollic* or *pollut*), demonstrating that suffix priming cannot be interpreted in terms of non–morphological orthographic overlap.

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To our knowledge, this is the first clear demonstration of a genuinely morphological suffix priming effect in English. These data contribute importantly to the debate about whether masked suffix priming exists at all. As illustrated in the Introduction, whereas Duñabeitia et al. (2008) showed very clear suffix priming in Spanish, data in French (Giraudo & Grainger, 2003) did not reveal such an effect. There seems to be no principled reason why suffix priming should emerge in Spanish, but not in French, although many co–varying variables might be responsible for the discrepancy (e.g., individual variability; Andrews & Lo, 2013; Beyersmann, Casalis, Ziegler, & Grainger, in press). We provided a new investigation of the issue with one important novelty, that is, we used nonword primes. This allowed us to avoid lexical competition between primes and targets (e.g., Davis & Lupker, 2006; Segui & Grainger, 1990), thus offering a more precise assessment of suffix priming as a purely morphological effect. Thanks to this variant, we were also able to contrast suffix priming with a new type of baseline, that is, control primes that share a stem and a full morphological structure with related primes, but include a suffix that is different from the target—shee*ter*–TEACHER was compared with shee*tal*–TEACH*ER*. This latter condition allows to assess for any priming that may come from the mere fact that the prime is parsable into morphemes, which, in itself, may bias participants towards a YES response to the target (e.g., Caramazza, Laudanna & Burani, 1988; Crepaldi et al., 2010; Taft & Forster, 1975). Because sheeter–TEACHER yielded shorter response times than sheetal–TEACHER, we can be sure that suffix priming does not depend on this possible confound, but it is genuinely due to the shared suffix between primes and targets.

Experiment 2

In addition to advancing our knowledge about the role of suffixes in complex word

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identification, the results of Experiment 1 provide the basis for addressing another important issue in complex word identification, that is, positional constraints in morpheme representations.

In line with previous masked priming (e.g., Duñabeitia, Laka, Perea, & Carreiras, 2009) and nonword interference data (Shoolman & Andrews, 2003; Taft, 1985), Crepaldi, Rastle, Davis and Lupker (2013) provided evidence that stem identification is relatively free of positional constraints. They showed that transposed–morpheme compounds (e.g., *moonhoney*) take longer to be rejected in a lexical decision task than compounds made up of free stems that never bind together in existing words (e.g., *moonbasin*). This effect was taken to show partial activation of the representation of the existing word *honeymoon* by the morphemic constituents *moon* and *honey*, an account based on the fact that the word recognition system identifies *moon* as a possible constituent of the compound *honeymoon* even when it occurs at the beginning of the nonword stimulus. Position–invariant stem identification was further tested using masked priming. Transposed–morpheme compounds were shown to yield time saving in the identification of their corresponding compound words (e.g., *fireback–BACKFIRE*). Importantly, this effect could not be accounted for by mere orthographic overlap, because it did not emerge in morphologically simple words, e.g., *roidaste–ASTEROID*.

Affix identification does not seem to show the same pattern. Crepaldi, Rastle, and Davis (2010) provided evidence suggesting that suffix identification occurs in a position–specific fashion. They first replicated the demonstration that nonwords made up of an existing stem and an existing suffix (e.g., *shootment*) take longer to be rejected in lexical decision than orthographic controls (*shootmant*) that do not comprise a morphological structure (e.g., Taft and Forster, 1975). This indicates that morphemic representations are activated in morphologically–structured nonwords in which the suffix occupies its usual position. Then,

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Crepaldi et al. (2010) demonstrated that this morphological effect disappears when the position of the morphemes is reversed (*mentshoot* elicits similar rejection latencies to *mantshoot*). These results suggest that suffixes are not recognised when they occur at the beginnings of nonwords. Thus, the conclusion that follows from Crepaldi et al.'s (2010; 2013) experiments is that different types of morphemes are coded by cognitive representations with somewhat different properties. These differences presumably reflect the different positional constraints within the language, that is, the fact that derivational and inflectional morphemes (e.g., *–ness*, *–ed*, *–ing*) always occur after a stem, whereas these latter can appear anywhere within complex words (e.g., *cat* in *cats*, *wildcat*, *catwalk*).

However, before drawing any firm conclusion from these data, it must be considered that the evidence reported by Crepaldi et al. (2010) relies critically on the interpretation of interference effects in lexical decisions for nonwords. Although this approach has a long history (e.g., Taft & Forster, 1975, 1976; Coltheart, Davelaar, Jonasson, & Besner, 1977), lexical decisions to nonwords are consistently slower than to words, and there is the possibility that these decisions are influenced by post–perceptual processes. Indeed, there continues to be some debate concerning how exactly lexical decisions to nonwords are made (for different theoretical hypotheses, see Coltheart et al., 1977; Davis, 2010; Dufau, Grainger & Ziegler, 2012; Grainger & Jacobs, 1996; Norris, 2006; Ratcliff, Gomez, & McKoon, 2004). In addition, supraliminal presentation of the critical stimuli may trigger task–specific strategies.

Masked priming avoids these problems. Now that we know (from Experiment 1) that, e.g., *boltness* primes *kindness*, it is possible to conceive of a masked priming experiment where different predictions arise according to whether suffix identification occurs in a position–invariant or a position–specific fashion. In this experiment, derived words (e.g., *kindness*) would be preceded by complex nonwords beginning with the same suffix (e.g., *nessbolt*). If the

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morphological representation of –*ness* is activated independently of its position, then *nessbolt*

should be processed as *ness + bolt*, thus facilitating responses to *kindness* (which is processed

as *kind + ness*; e.g., Baayen et al., 1997, 2007; New et al., 2004). Conversely, if –*ness* is

recognised as a suffix only when it follows an existing stem (as suggested by Crepaldi et al.,

2010), then it should not be identified within *nessbolt* and thus should not facilitate responses to

the target *kindness*. Critically, this evidence: (i) would not rely on nonword rejection times, thus

making sure that we are tapping into the mechanisms actually involved in lexical access; and

(ii) would be obtained in a paradigm where the critical manipulation is kept outside

participants' awareness, thus making sure that strategic, non–perceptual factors would have

little if any impact.

Methods

Participants

Fifty–one students from the same population as in Experiment 1 participated in the

experiment; none of them had also been included in Experiment 1. Participants were paid £5

for their participation.

Materials and Procedure

The stimulus set for Experiment 2 was identical to the set used in Experiment 1 except for

the fact that the relative position of the stems and the suffixes (or the stems and the non–

morphological endings) was reversed in all prime nonwords. For example, the complex target

teacher – that was primed by *sheeter*, *sheetal* and *sheetub* in Experiment 1 – was primed in

Experiment 2 by *ersheet*, *alsheet* and *ubsheet*. Similarly, the monomorphemic target *barrel* –

that was primed by *pollel*, *pollic* and *pollut* in Experiment 1 – was primed in Experiment 2 by

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elpoll, *icpoll* and *utpoll*.

Transition probabilities at the boundary between suffixes (or their non–morphological controls) and prime stems were checked also in this second Experiment, and were found to be closely matched across conditions (complex targets: suffix prime, .02 ± .03; suffix control, .02 ± .03; unrelated control: .02 ± .03; simple targets: word–onset prime, .02 ± .03; suffix control, .02 ± .04; unrelated control: .01 ± .02). Interestingly, transition probabilities are quite lower here than in Experiment 1, thus making suffixes even more detectable. Because suffixes and their non–morphemic controls were just moved to the prime onset position, average transition probabilities within them were identical to Experiment 1.

Nonword trials were derived from nonwords trials in Experiment 1 following exactly the same procedure that was used for word trials; they were thus identical to Experiment 1 but for the fact that suffixes and their non–morphemic controls were moved to the beginning of nonword primes.

Every other aspect of the experiment (including experimental design and procedure) was identical to Experiment 1.

Results

Data were trimmed as in Experiment 1. This procedure resulted in the exclusion of three participants, and one simple and two complex target words. The remaining data were analysed as in Experiment 1.

Mean response times and error rates are reported in Table 4. In contrast to Experiment 1, there was no effect of prime type and no interaction between prime type and target type, in either the response time or accuracy analyses (all ps > .19).

To test the strength of the evidence in favour of the null hypothesis – that is, no priming when suffixes are in different positions within primes and targets – we computed a Bayes Factor (BF) as suggested by, e.g., Masson (2011) and Wagenmakers (2007). The BF was 68.70, which means that the probability of observing the data is 68.70 times higher under the null hypothesis (no priming) than under the alternative hypothesis (cross-position suffix priming). On the basis of the BF, we can compute the probability that the null hypothesis itself is true, given the data—this amounts to .985. According to Raftery's (1995) classification of evidence based on posterior probability into *weak* (.5–.75), *positive* (.75–.95), *strong* (.95–.99) and *very strong* (>.99), this analysis provided *strong* evidence for the absence of cross–position suffix priming.

TABLE 4 ABOUT HERE

In order to confirm that results in this experiment were reliably different from those obtained in Experiment 1, we also carried out a cross–experiment analysis on response times. We included in the model any covariate that accounted for significant RT variance in either Experiment 1 or 2; every other aspect of the analysis was identical to those of Experiment 1 and Experiment 2. As in previous analyses, crossed random intercepts for targets and participants allowed us to control any spurious variance that might come from these variables. This was particularly important in this analysis, because different participants took part in Experiment 1 and Experiment 2. Critically, this analyses yielded a significant three–way interaction between experiment, target type and prime type, Chi-square[2] = 8.59, p = .01.

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Discussion

The results of this experiment demonstrate the absence of any cross–position suffix priming: when nonword primes (e.g., *ersheet*) begin with a suffix that is also included in the target word (*teacher*), they do not confer any advantage as compared to nonwords including the same stem preceded by an unrelated letter cluster, either morphemic or not (*alsheet* or *ubsheet*). Given that the stems, suffixes, and non–morphological clusters used in this experiment were identical to those employed in Experiment 1, the possibility that the null result depends on some specific feature of the materials is ruled out. We also checked through a cross–experiment analysis that this pattern of results was statistically different from that obtained in Experiment 1, where suffix priming emerged. Despite drawing together data from different experiments may have introduced some additional error variability, the outcome of this analysis was clear and suggested that the results of this experiment were different from those obtained in the previous experiment. One possible explanation of these results is of course that suffixes are not identified at the onset of morphologically–structured nonwords. This would clearly speak in favour of the hypothesis that suffix identification is position–specific, thus confirming the results reported by Crepaldi et al. (2010) in a lexical decision study that examined nonword interference effects.

However, before taking this conclusion we need to rule out an alternative account, i.e., that priming did not emerge in Experiment 2 simply because suffixes are not frequent word onsets (indeed, in some cases, they are impossible word onsets, e.g., *ify*), and thus the system rejected the primes as impossible words and processed the targets as if they were not preceded by any linguistic material. This is a logically possible account of the null effect described above, and clearly implies no role for morphology. A straightforward prediction of this account is that

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priming should be absent for primes containing suffixes that never occur at the beginning of existing words (e.g., *ify*), minimal in suffixes that occur very rarely as word onsets (e.g., *ory*, as in *oryx*), and larger in suffixes that occur more often as word onsets (e.g., *al*, as in *always, alumni, altruism, altitude*). More generally, priming should be proportional to the frequency of the suffixes-as-word-onsets.

In order to test this prediction, we computed type and token frequency for each suffix-as-a-word-onset based on the SUBTLEX-UK corpus (van Heuven et al., 2014). In order to avoid distortion in the frequency data due to typographical errors, we only considered SUBTLEX-UK entries that appeared in at least three different sources (i.e., three different movies, given that SUBTLEX is based on movie subtitles), which is to say that we only considered entries with a Contextual Diversity higher than 2 (Adelman et al., 2006). We then computed the average priming effect for each target word in Experiment 2, both against the morphological and the non-morphological baseline, that is, comparing alcrop-FORMAL against both ercrop-FORMAL and obcrop-FORMAL. Finally, we correlated priming with both type and token frequency. The results of this analysis are illustrated in Figure 1. Independently of the baseline and of whether we considered type or token frequency, there is clearly no correlation between the size of the priming effect and how often the critical suffix appears at the beginning of existing words in the language. This speaks strongly against the hypothesis that the lack of priming is due to unfamiliar prime onsets, and confirms that the most likely interpretation of the results of Experiment 2 is morphological in nature. That is, cross-position suffix priming does not emerge for primes like *alcrop* because suffixes are not identified as such when they precede a stem.

FIGURE 1 ABOUT HERE

General Discussion

The data described in the present paper provide two important contributions to the existing knowledge on how complex words are recognized in the visual system. First, they demonstrate suffix priming, that is, they show that complex words are recognized more quickly if they are preceded by a morphologically structured nonword with the same suffix (sheeter–TEACHER). Second, they show that facilitation disappears when that same suffix is moved to the beginning of the nonword prime (ersheet–TEACHER); that is, suffix priming does not hold cross–positionally.

The existence of suffix priming strongly suggests that suffixes play an active role in complex word identification. Given that there is also convincing evidence for prefix priming (Chateau et al., 2002; Dominguez et al., 2010; Giraudo & Grainger, 2003), this statement may be generalised to any kind of affix. Clearly, this does not sit well with models of complex word identification that have focused exclusively on stems. The idea that affixes are just stripped away from complex words and the lexical identification system is left with the task of identifying the stem has been popular for a number of years (e.g., Frost, Forster, & Deutsch, 1997; Taft, 1994; Taft and Forster, 1975). Here we show, instead, that although stems and affixes are indeed identified within complex words, both types of morphemes are actively processed in a way that allows prime–to–target facilitation even when the two stimuli only share a suffix.

The lack of cross–positional suffix priming also tell us something about the circumstances that give rise to affix recognition and processing. More specifically, they suggest that suffixes are identified when they follow a stem, but not when they occur at the beginning of morphologically structured nonwords (thus preceding a stem). These data confirm what reported by Crepaldi et al. (2010), who demonstrated that morphologically structured nonwords in agreement with morpheme positional constraints (e.g., *shootment*) are more difficult to reject than orthographic controls (*shootmant*); but the same interference effect does not arise when positional constraints are violated by having the suffix at the nonword onset (*mentshoot*). The present study goes beyond Crepaldi et al. (2010), however, in demonstrating the position specificity of suffix representations in a paradigm unlikely to be contaminated by strategic or episodic factors, and that is likely to reflect perceptual rather than decisional processes (Forster & Davis, 1984). Moreover, masked priming reflects early stages of visual word identification; in addition to the fact that both Crepaldi et al. (2010) and the present evidence rely on *nonword* manipulations, this strongly suggests that the analysis of positional information arises pre–lexically.

It is clear, then, that the data reported in this paper are consistent with models of visual word identification that include an early and fast–operating level of morphological analysis (e.g., Crepaldi et al., 2010; Grainger and Ziegler, 2011; Rastle et al., 2004; Taft & Nguyen–Hoan, 2010). These data also suggest that one factor that may facilitate the rapidity of early morphological parsing is its apparent sensitivity to position. That is, the parser may be able to segment suffixes very rapidly because it 'knows' that they can only occur at the end, and not the beginning of letter strings (symmetrically, this may be valid for prefixes; see Kazanina, 2011). Despite their compatibility with the idea that morpheme representations feature

positional information, none of the existing models take an explicit position on this issue. So, one important contribution of the present work is showing that these models need to be extended by considering how position is coded, in a way that allows both position–specific (affixes) and position–free (stems) representations within the system (see also Crepaldi et al., 2010, and Crepaldi et al., 2013).

Positional constraints clearly arise from morpheme distribution within the language: suffixes are not identified when they occur at the beginning of letter strings because they never occur at the beginning of letter strings in the real language. That is, positional constraints are a reflection within the word identification system of statistical regularities in the input. This suggests that one possible way to characterize morpho–orthographic analysis is as the result of a statistical learning mechanism devoted to capture these regularities. After all, one theoretical issue with morpho–orthographic segmentation has always been the purpose/nature of such a mechanism: if morphology captures form–meaning correlation, why should we ever break down *corner* into *corn* and *er*, given that a corner is clearly not someone who corns? This issue is nicely settled by adopting the 'statistical learner' approach: morpho–orthographic analysis would not be a genuinely morphological process, but a mechanism that facilitates rapid and effective analysis of the visual input by chunking low–level units (letters) that occur frequently together into larger units (e.g., Gobet et al., 2001; Miller, 1956; Simon, 1974). Because morphology clearly imposes regularities in letter co–occurrence (e.g., *m*, *e*, *n* and *t* occur often together because they are part of the suffix *–ment*), this mechanism takes the form of a morpho–orthographic parser in reading (Crepaldi et al., 2010; Davis, 1999; Rastle and Davis, 2003, 2008)

It is difficult to characterise morpheme positional constraints more specifically given the

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available knowledge. Positional constraints in the language are typically more complex than just not–at–the–beginning (for suffixes) or not–at–the–end (for prefixes). For example, there are several words where the morpheme *less* precedes the morpheme *ness* (e.g., carelessness, helplessness, hopelessness, homelessness), but no word where the contrary happens (e.g., Kiparsky, 1982). Does this mean that *less* and *ness* would be identified as morphemes only if they appear in that order? More generally, do positional constraints refer necessarily to word boundaries, or is local (within–word) contextual information also taken into consideration? And what happens to morphemes that occur much more frequently in one position (e.g., at the word onset in compounds) than in others (e.g., at the word ending in compounds): would positional constraints reflect this difference in prior probability? These issues require further testing and are of critical importance for determining what kind of coding scheme underpins the morpheme positional constraints demonstrated here and in Crepaldi et al. (2010, 2013). Also interesting is the question of whether/how facilitation is affected by the suffix lying in different positions across the prime and the target (e.g., central vs. final as in farm*er*like–BANK*ER*; or second vs. third as in careless*ness*–KIND*NESS*). By proving more clearly the existence of positional constraints in suffix representation, the present work sets the stage for the investigation of all these important issues.

Whatever the answer to these questions, the data presented here (together with those described in Crepaldi et al., 2010, 2013) clearly demonstrate that: (i) positional constraints are implemented in pre–lexical morpheme representations; (ii) these constraints inform visual word identification very quickly after word presentation; (iii) they reflects the distributional properties of the different types of morphemes within words in the language; (iv) in most Indo–European languages, this implies that stems and suffixes are coded differently, namely, stem

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codes are (relatively) position–free, whereas suffix codes are (relatively) position–locked.

These four statements represent a serious challenge to all existing models of complex word

identification, none of which has any explicitly– defined way of coding for morpheme position.

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Appendixes

Appendix A. Set of stimuli used in Experiment 1 for the word trials.

Target	Related prime	Suffix control	Unrelated control
(a) Complex targets			
steeply	altarly	altaric	altarfu
merely	fately	fateic	fatefu
yearly	flagly	flagic	flagfu
arabic	heiric	heirly	heirop
periodic	forestic	forestly	forestop
titanic	labelic	labelly	labelop
teacher	sheeter	sheetal	sheetub
seller	minker	minkal	minkub
washer	tooler	toolal	toolub
formal	cropal	croper	cropob
herbal	tactal	tacter	tactob
orbital	dozenal	dozener	dozenob
faithful	towerful	towerism	towerpak
cheerful	themeful	themeism	themepak
wishful	quizful	quizism	quizpak
humanism	worthism	worthful	worthork
alcoholism	treasonism	treasonful	treasonork
tourism	peakism	peakful	peakork
hazardous	helmetous	helmetify	helmetalg

Target	Related prime	Suffix control	Unrelated control
dangerous	barrowous	barrowify	barrowalg
pompous	tuskous	tuskify	tuskalg
justify	tombify	tombous	tombele
fortify	boltify	boltous	boltele
testify	wombify	wombous	wombele
dietary	tripary	tripise	tripolf
customary	maidenary	maidenise	maidenolf
honorary	witchary	witchise	witcholf
criticise	willowise	willowary	willowugg
realise	cartise	cartary	cartugg
visualise	hooverise	hooverary	hooverugg
directory	attackory	attackive	attackuff
transitory	inspectory	inspective	inspectuff
inventory	tamperory	tamperive	tamperuff
massive	curbive	curbory	curbull
festive	jailive	jailory	jailull
adoptive	batonive	batonory	batonull

(b) Simple targets

Target	Related prime	Suffix control	Unrelated control
defect	varyct	varyal	varysk
brothel	sportel	sportic	sportur
chapel	bellel	bellic	bellur
barrel	pollel	pollic	pollut

Target	Related prime	Suffix control	Unrelated control
cancel	limbel	limbic	limbut
parallel	colourel	colouric	colourut
apparel	bellyel	bellyic	bellyut
energy	pacegy	paceal	pacemu
formula	fencela	fenceic	fencene
symbol	baitol	baiter	baitum
epitaph	minceph	minceal	mincett
debate	tubete	tubely	tubepo
spinach	muskach	muskful	muskesp
curtain	pintain	pintous	pinteph
sarcasm	whipasm	whipive	whipurf
possible	graveble	graveous	gravecid
impeccable	cabbageble	cabbageous	cabbagecid
spectacle	cantoncle	cantonish	cantondut
pinnacle	fudgecle	fudgeish	fudgemut
intellect	autumnect	autumnish	autumnapp
intersect	thrillect	thrillish	thrilloul
sheriff	pilliff	pillous	pillath
manuscript	boycottipt	boycottous	boycottenk
inquire	helmire	helmful	helmoth
bracket	townket	townous	towncif
cockney	slabney	slabish	slabmut

Target	Related prime	Suffix control	Unrelated control
compost	vealost	vealful	vealerk
squirrel	heartrel	heartism	heartoan
emphasis	slopesis	slopeful	slopetet
consensus	littersus	litterive	litternan
brittle	whimtle	whimous	whimmak
ultimatum	pondertum	ponderive	pondersab
construct	filteruct	filterish	filteroof
syllable	sheerble	sheerism	sheeroor
costume	goodume	goodist	goodoph
destitute	patentute	patentish	patentarg

Appendix B. Set of stimuli used in Experiment 1 for the nonword trials.

Target	Prime
betage	fitage
lilyed	quided
angely	debity
shedage	zoomage
fighood	kidhood
valleyan	quarryan
swimable	tendable
pedalhood	blurbhood
veteraned	monarched
summitan	retalian
triggerable	abolishable
bleedable	soundable
bigen	rawic
mudate	jarion
tomban	palmen
oilate	hobish
scrumed	skullan
youngen	harshan
obeliskan	canteenen
repeatage	insertity
garmentate	dolphinful

Target	Prime
breathant	splashion
bridgehood	tycoonable
extractage	persistity
jewely	sirenu
surgeony	concertu
longen	dirtak
scratchion	sustainint
crystalant	counselimy
anthood	doterff
coastan	woundoc
planeted	buttonec
kissant	surfuth
sausagy	gazellu
penalen	scuffel
patrolable	compelnuss
dridge	vasege
attask	sodask
regbot	illbot
geefep	waspep
tostreb	puntreb
tracheck	venteck
maderick	noodlick

Target	Prime
zerntass	musetass
teandwig	immerseg
pirtaston	cherryton
stooverime	berserkime
britchlenk	pensionenk
tudap	fatic
pisilm	spilly
puroty	petion
prochap	stalkic
stirdek	zoonful
stoothac	gobbeted
almanick	lexicony
glaktean	chorusly
prostrom	switchal
tiwantilm	regattaly
steelterin	harrowness
kaintropoth	breatherment
lintig	copbor
shengel	setgel
whostle	cressna
bomplev	plumdan
harvist	dollyst

Target	Prime
delktad	friskad
pelfothom	spigoteel
geefindeam	hammockoog
skantrell	scriptell
thringoll	smashgoll
shrepothep	strifethep
jorthtibek	patriotdif

Authors' Note

Footnotes

1. It is not completely clear from the description of the items, but it is likely that Giraudo and Grainger's (2003) orthographic primes were actually a mix of truly non–morphological primes with pseudo–suffixes (e.g., murder–DEALER) and entirely parsable words made up of an existing pseudo–stem and an existing pseudo–suffix (e.g., corner–DEALER). We now know that entirely parsable pseudo–complex words can be analysed morphologically in masked priming experiments (e.g., Kazanina, 2011; Longtin et al., 2003; Rastle et al., 2004), so pure orthographic priming may be contaminated by some undue morphological facilitation, and thus overestimated. The same comment holds for the prefix priming experiment described in Giraudo and Grainger (2003).

2. Some of these nonwords could contain a stem or an affix, but they could never be decomposed into a stem–suffix combination.

3. Following Bates et al. (2013), the reliability of mixed model parameters is not established using p values, but through the computation of confidence intervals. Note, however, that the two methods are practically equivalent, given that significant p values correspond to CIs that do not include zero (as in this case).

Tables

Table 1. Lexical and sub–lexical features of complex and simple targets in Experiment 1 and 2. Standard deviations are reported in parentheses.

	Complex targets	Simple targets
Log frequency (per million words)	.41 (.74)	.59 (.70)
Length in letters	7.53 (1.18)	7.53 (1.18)
Number of syllables	2.86 (.87)	2.72 (.70)
Coltheart N	.64 (.87)	.50 (.48)
Mean log bigram frequency	2.57 (.33)	2.42 (.31)
Transition probability at the boundary	.09 (.09)	.09 (.09)
Mean transition probability within suffix/word ending	.07 (.04)	.10 (.09)

Table 2. Lexical and sub–lexical features of the primes used in Experiment 1. Standard deviations are reported in parentheses.

	Complex targets			Simple targets		
(a) Stems						
Length in letters	4.86 (.96)			4.86 (.96)		
Log word frequency	1.03 (.50)			1.09 (.66)		
Coltheart N	5.00 (3.96)			5.39 (4.11)		
	Related	Suffix control	Unrelated control	Related	Suffix control	Unrelated control
(b) Whole primes						
Orthographic overlap with target (spatial coding)	.46 (.08)	.16 (.06)	.15 (.07)	.45 (.08)	.14 (.08)	.14 (.07)

Orthographic overlap with target (bigram coding)	.15 (.09)	.04 (.05)	.03 (.04)	.14 (.09)	.03 (.04)	.03 (.05)
Transition probability at the boundary	.08 (.06)	.08 (.06)	.06 (.08)	.06 (.07)	.06 (.04)	.06 (.06)
Mean transition probability within suffix/word ending	.11 (.10)	.11 (.10)	.05 (.03)	.07 (.04)	.11 (.09)	.08 (.05)

Table 3. Average response times (in ms) and error rates in Experiment 1. Significant effects are marked with an asterisk.

		Related prime	Suffix control	Unrelated control	Related vs. suffix control	Related vs. unrelated control
RT	Complex targets	677	712	700	−35*	−23*
	Simple targets	688	704	697	−16	−9
Error rates	Complex targets	.030	.049	.038	−.019	−.008
	Simple targets	.037	.039	.065	−.02	−.028

Table 4. Average response times (in ms) and error rates in Experiment 2.

		Related prime	Suffix control	Unrelated control	Related vs. suffix control	Related vs. unrelated control
RT	Complex targets	718	713	720	+5	−2
	Simple targets	724	721	712	+3	+12
Error rates	Complex targets	.043	.059	.066	−.016	−.023
	Simple targets	.050	.046	.045	+.004	+.005

Figure captions

Figure 1. Scatterplots illustrating the lack of correlation between priming and frequency of the suffix as a word onset. Priming is calculated by item, considering either the morphologically structured (upper row) or the unrelated baseline (bottom row). Frequency is computed based on types (left column) or tokens (right column), and is plotted on a logarithmic scale.