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‘Fell’ primes ‘fall’, but does ‘bell’ prime ‘ball’? Masked priming with irregularly-inflected primes

Davide Crepaldi [a,*], Kathleen Rastle [b], Max Coltheart [c], Lyndsey Nickels [c]

[a] *Department of Psychology, University of Milano-Bicocca, piazza Ateneo Nuovo 1, 20126 Milano, Italy*
[b] *Department of Psychology, Royal Holloway University of London, UK*
[c] *Macquarie Centre for Cognitive Science, Macquarie University, Australia*

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ABSTRACT

Recent masked priming experiments have brought to light a morphological level of analysis that is exclusively based on the orthographic appearance of words, so that it breaks down corner into corn- and -er, as well as dealer into deal- and -er (Rastle, Davis, & New, 2004). Being insensitive to semantic factors, this morpho-orthographic segmentation process cannot capture the morphological relationship between irregularly inflected words and their base forms (e.g., fell–fall, bought–buy); hence, the prediction follows that these words should not facilitate each other in masked priming experiments. However, the first experiment described in the present work demonstrates that fell does facilitate fall more than orthographically matched (e.g., fill) and unrelated control words (e.g., hope). Experiments 2 and 3 also show that this effect cannot be explained through orthographic subregularities that characterize many irregular inflections, as no priming arose when unrelated words showing the same orthographic patterns were tested (e.g., tell–tall vs. toll–tall). These results highlight the existence of a second higher-level source of masked morphological priming; we propose that this second source of priming is located at the lemma level, where inflected words (but not derived words) share their representation irrespective of orthographic regularity.

Over the last 30 years a substantial body of literature has focused its attention on how morphologically-complex words are processed by the human language system. Since the pioneering work of Taft and Forster (1975, 1976) showed interference effects in a lexical decision task when nonwords contained embedded morphemes (e.g., desplicate, cleanmip), morphological effects have been reported frequently in word recognition experiments. For example, it is now well established that the recognition of a stem (e.g., depart) is speeded by the prior masked presentation of a morphologically related word (e.g., departure) in a way that cannot be traced back just to the semantic and orthographic relationships characteristic of morphological relatives (e.g., Drews & Zwitserlood, 1995; Rastle, Davis, Marslen-Wilson, & Tyler, 2000). The consistent report of stem frequency effects in lexical decision tasks (e.g., Bradley, 1979; New, Brysbaert, Segui, Ferrand, & Rastle, 2004) also suggests processing of the stems of complex words. These results have led to the belief that morphological structure plays a crucial role in printed word recognition, and have driven research aiming to describe these mechanisms in more depth.

In line with the traditional definition of a morpheme as the smallest meaning-bearing linguistic unit (e.g., Bloomfield, 1933; Spencer, 1991), substantial psycholinguistic research has focused on the role of semantic transparency in the processing of complex words. Until recently, most theories of morphological processing proposed that complex words are decomposed into their

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[*] Corresponding author. Fax: +39 02 64483706.
E-mail address: davide.crepaldi1@unimib.it (D. Crepaldi).

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constituents only if the complex word is related in meaning to its stem (e.g., Giraudo & Grainger, 2001; Marslen-Wilson, Tyler, Waksler, & Older, 1994; Plaut & Gonnerman, 2000; Rueckl & Raveh, 1999). These theories had been supported by results from cross-modal priming (e.g., Longtin, Segui, & Hallé, 2003; Marslen-Wilson et al., 1994) and visual priming with fully visible primes (Rastle et al., 2000) showing that morphologically-complex words prime their stems only if they are semantically related (e.g., *government* primes *govern*, but *department* does not prime *depart*). However, more recent work has homed in on an earlier stage of decomposition that appears to be guided primarily by the orthographic appearance of morphological complexity, a phenomenon that has become known as morpho-orthographic segmentation (after Rastle et al., 2004). The conclusion that decomposition is guided by orthographic rather than semantic factors is supported by masked priming studies using very brief stimulus onset asynchrony (SOA; e.g., 40 ms). With only a couple of exceptions (Diependaele, Sandra, & Grainger, 2005; Feldman, O'Connor, & Martin, 2009), these studies have shown that semantically-transparent pairs like darkness–DARK produce statistically indistinguishable priming effects from pseudo-morphological pairs like corner–CORN, and that both of these types of prime–target pair yield greater priming than non-morphological form controls like brothel–BROTH (*-el* never functions as a suffix in English), suggesting that the priming effects observed cannot be ascribed to simple orthographic overlap (Devlin, Jamison, Matthews, & Gonnerman, 2004; Kazanina, Dukova-Zheleva, Geber, Kharlamov, & Tonciulescu, 2008; Longtin et al., 2003; Marslen-Wilson, Bozic, & Randall, 2008; Rastle et al., 2004; see Rastle and Davis (2008) for a review).

The existence of an early morphological segmentation procedure guided purely by the orthographic appearance of morphological complexity, in which a meaning-bearing stem can be accessed rapidly from a longer stimulus, raises interesting issues regarding the visual recognition of irregular inflectional forms (e.g., *drove*, *bought*, *mice*). In contrast to cases like *darkness* and *government*, in which known stems can be segmented from known suffixes, the base stems in these irregular inflectional examples (e.g., *drive*, *buy*, *mouse*) cannot be extracted based on a simple orthographic analysis alone. Though it has been shown that morpho-orthographic segmentation survives the regular orthographic alterations that frequently characterize complex words (such as missing *e*, as in *adorable*; McCormick, Rastle, & Davis, 2008), the orthographic relationship between irregular inflectional forms and their base stems is far more idiosyncratic. Thus, it would seem that the morpho-orthographic segmentation process described by Rastle et al. (2004); also Rastle and Davis (2008) would predict that these kinds of forms should not be subject to rapid morphological analysis in visual word recognition. The strong prediction of this theory would appear to be that, in masked priming situations comparable to those used to study derivational and pseudo-derivational morphology (i.e., very short SOAs), irregular inflections should not prime their base stems (at least not more than would be expected on the basis of their simple letter overlap).[1]

However, in contrast to the apparent predictions of this theory, there is some evidence that such effects do occur. In the initial research into this issue, Forster and colleagues (Forster, Davis, Schoknecht, & Carter, 1987, Experiment 7) reported that irregularly inflected words prime their base forms as effectively as the base forms themselves, using a 60-ms SOA (drive–DROVE = drove–DROVE). In a somewhat more complex experiment conducted in French, Meunier and Marslen-Wilson (2004; Experiment 2) reported similar results, showing comparable masked priming effects on target base forms for regular and irregular inflectional forms. Unfortunately, it is difficult to draw strong conclusions from these studies because of the types of control primes used; both of these studies measured morphological priming effects against unrelated controls. In the case of Forster et al. (1987), controls were randomly chosen unrelated words of the same length: no attempt was made to balance primes across different conditions for frequency or neighbourhood density. In the case of Meunier and Marslen-Wilson (2004), controls consisted of unrelated words matched to the regular primes, but not to the irregular primes, on surface frequency, number of syllables, tense and person. Critically, neither study included non-morphological orthographic controls designed to establish to what extent these effects reflected simple letter overlap across morphologically-related primes and targets.[2]

This problem with orthographic controls was partially addressed by Pastizzo and Feldman (2002), who compared priming for irregular inflections with high (e.g., fell–FALL) vs. low (e.g., taught–TEACH) orthographic overlap against orthographically matched (e.g., fill–FALL, taunts–TEACH) and completely unrelated baselines (e.g., pair–FALL, slouch–TEACH). Results showed significant priming for the fell–FALL items against the orthographic baseline (but not against the unrelated baseline), and no priming for the taught–TEACH items against either baseline. One thing that is not clear is why the fell–FALL items showed priming while the taught–TEACH items did not, especially since Pastizzo and Feldman (2002) reported quite similar mean values of orthographic similarity for the two conditions (67.9% vs. 56.1% of position-specific letters shared across primes and targets), with no evidence provided that the groups of stimuli actually differed statistically on this factor. Further, it is not clear whether the orthographic control primes imple-

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[1] One possibility is that irregular inflections could prime their base stems as a result of their semantic overlap. However, it is well established that masked semantic priming effects on lexical decision are typically small and statistically unreliable (e.g., Rastle et al., 2000), unless the primes fall into the range of partial visibility (e.g., 70 ms SOA; Perea & Gotor, 1997).

[2] Forster et al. (1987) argued that their effects must have been morphological in nature because they had shown in another experiment that no priming is obtained solely on the basis of orthographic similarity. However, more recent evidence has shown that orthographic similarity does indeed influence masked priming (e.g., Davis & Lupker, 2006; Forster & Veres, 1998), thus calling into question the extent to which the drive–DROVE effect reported by Forster and colleagues was driven by morphological factors.

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mented by Pastizzo and Feldman (2002) were effective in accounting for the orthographic overlap between irregular primes and their targets. The problem here is a general one that relates to the assumptions that researchers must make about the nature of orthographic input coding when designing their orthographic controls. Pastizzo and Feldman (2002) reported designing their orthographic controls such that they preserved common letters in common positions across irregular primes and targets (i.e., they assumed a slot-based coding scheme in the design of their controls). However, recent evidence has shown that slot-based coding schemes provide a poor metric for assessing perceptual similarity (e.g., Davis & Bowers, 2006; Perea & Lupker, 2003). On slot-based coding schemes, prime–target pairs like ate–EAT have no orthographic overlap whatsoever, and so an orthographic control that preserves common letters in common positions like gin–EAT would be perfectly appropriate. Such a control would be inappropriate though, if orthographic similarity were determined using another coding scheme; for example, spatial coding (for which there is now considerable evidence; see Davis and Bowers (2006)) returns a match value of 0.34 for ate–EAT yet returns a match value of 0.00 for gin–EAT. Though extreme examples like ate–EAT are not frequent, this issue applies to any prime–target pair in which there shared letters in different positions (e.g., bought–BUY, taught–TEACH). Because no stimuli were provided in Pastizzo and Feldman (2002), it is not possible to gauge the extent to which their stimuli suffered from this potential problem.

More recently, Kielar, Joanisse, and Hare (2008) investigated irregular masked priming, emphasising in particular those irregular past tense forms ending with an alveolar consonant (e.g., *wept*, *heard*) under the hypothesis that these should behave similarly to regular forms as they "take a version of the regular alveolar past tense suffix" (p. 330). The authors found masked priming effects for these "suffixed irregular" forms (e.g., dealt–DEAL) when these were compared to unrelated primes (hung–DEAL), but, in apparent contrast to the results of Pastizzo and Feldman (2002), no effect emerged for pairs like fell–FALL characterized by a vowel change. However, in addition to suffering the same kinds of problems with orthographic controls as in previous studies, the theoretical construct on which "suffixed irregular" forms are defined is problematic. Specifically, there is no orthographic analogy to the "regular alveolar past tense morpheme": in fact, the prime–target pairs that made up this condition (e.g., wept–WEEP; meant–MEAN; sold–SELL) were not predictable on an orthographic basis, and it is not clear how phonological regularity might influence the orthographic representations thought to be tapped in masked priming experiments (see Rastle and Brysbaert (2006), for a discussion of the magnitude of phonological effects in masked priming). Overall, then, Kielar et al. (2008) did report evidence for irregular masked priming, at least as far as dealt–DEAL items were concerned; however, their results differed from those of Pastizzo and Feldman (2002) for vowel-change irregular inflections (e.g., fell–FALL) even though the characteristics of the items used in the two studies appear to be fairly comparable.

In summary, the evidence obtained so far is suggestive of masked priming effects for irregular inflectional forms, but does not allow a firm conclusion on this issue for two reasons. First, some methodological problems (e.g., the lack of orthographic controls, adoption of a slot-based coding scheme) have made the contribution of the experimental results obtained in previous literature somewhat unclear. Perhaps more importantly, contrasting results have emerged on a very frequent type of irregular inflection, with Pastizzo and Feldman (2002) observing masked priming between fell and fall against an unrelated baseline, and Kielar et al. (2008) reporting no effect in the same comparison.

Our Experiment 1 thus offered a new examination of irregular masked priming, while overcoming the problematic features of previous experiments; in particular, orthographic controls were matched with irregular inflectional forms using both the slot coding and the spatial coding scheme, and accurate matching was sought across experimental conditions for length, written frequency, spoken frequency and orthographic neighbourhood size. More specifically, Experiment 1 was designed to assess whether irregularly inflected forms (e.g., *fell*) prime their base forms (*fall*), as compared to orthographically matched (*fill*) and completely unrelated (*hope*) control words. On the theory of morphological decomposition put forward by Rastle et al. (2004) (see also Rastle and Davis (2008)), it is difficult to see how a morphological analysis of these kinds of items could be achieved in early visual perception, and thus this theory would appear to predict that no masked priming effects should be observed for irregularly inflected forms, over and above those expected on the basis of orthographic overlap alone. If instead previous suggestions of masked priming of irregular inflections prove to be robust, then priming effects should also emerge in the present, better controlled experiment, thus indicating that modifications are required to the model put forward by Rastle et al. (2004).

Experiment 1

Methods

Participants

Forty-two undergraduate 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. They were paid £5 (about $7.50) for their participation.

Materials

Thirty-nine English monomorphemic words were selected as targets. Thirty-four were verbs in their base form (e.g., *fall*) and five were singular nouns (e.g., *mouse*); their mean surface frequency was 146.06 (±266.11) occurrences per million, based on the CELEX database (Baayen, Pipenbrock, & van Rijn, 1993).

Each target word was paired with three different primes. In the +M+O condition, primes constituted ortho-

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graphically irregular inflected forms of the targets (i.e., the past tense form for the verbs, e.g., *fell*, and the plural form for the nouns, e.g., *mice*); primes in this condition were thus morphologically and, although to a variable extent, orthographically related to the target. In the –M+O condition, primes comprised monomorphemic words that were morphologically unrelated but orthographically related to the targets (e.g., *full* and *maze*). In the –M–O condition, primes comprised monomorphemic words that were completely unrelated to the targets (e.g., *hope* and *warn*). The three types of primes all had a monomorphemic surface structure (i.e., they were not composed of two morphemes clearly identifiable on orthographic grounds). Stimuli for this experiment are contained in Appendix A.

The irregularly-inflected primes (+M+O) varied in orthographic overlap with the targets, but were always at least partially related to them (i.e., no completely idiosyncratic forms like, e.g., *go–went*, were used). The degree of orthographic overlap between the primes and the targets was calculated through the MatchCalculator application (Davis, 2005) adopting both the left-aligned slot-coding approach (see McClelland and Rumelhart (1981)) and the spatial coding approach (Davis & Bowers, 2006). The figures shown in Table 1 reveal that –M–O control primes had significantly less orthographic overlap with targets than both +M+O and –M+O primes, but that +M+O and –M+O primes did not differ from each other in their overlap with targets, irrespective of the position coding scheme adopted.

The three sets of primes were also matched pairwise for length, and listwise for logarithmic written frequency, logarithmic spoken frequency, and number of orthographic neighbours (see Table 1).

The stimulus set also included 39 legal nonwords created through the ARC nonword database (Rastle, Harrington, & Coltheart, 2002). The nonwords were all one-letter different from at least one existing word and were matched in length and number of orthographic neighbours with the target words (length: nonwords, 4.28 ± .92, words, 4.23 ± .84; $t[76] = .26$, $p = .79$; orthographic neighbours: nonwords, 10.51 ± 5.83, words, 10.26 ± 5.69; $t[76] = .19$, $p = .84$).

The 39 nonword targets were paired with two new sets of word primes, either orthographically related or orthographically unrelated to the nonwords. The degree of orthographic overlap between primes and targets for the nonword trials was calculated as described previously for the word trials, and averaged .57 and .01 for the orthographically related and the orthographically-unrelated primes, respectively (.64 and .04 if adopting a spatial coding approach). The nonword-trial prime lists were matched pairwise with the word-trial prime lists for length; word- and nonword-trial primes were also matched listwise for logarithmic written frequency (word trials: 2.66 ± .72; nonword trials: 2.83 ± .76, $t[193] = 1.15$, $p = .13$), logarithmic spoken frequency (word trials: 1.28 ± .91; nonword trials: 1.39 ± .90, $t[193] = .89$, $p = .37$) and number of orthographic neighbours (word trials: 8.45 ± 5.35; nonword trials: 8.74 ± 5.58, $t[193] = .35$, $p = .72$).

The assignment of word targets to the three priming conditions was counterbalanced over participants, so that all participants received primes from each condition, but saw each target only once. The assignment of nonword targets to the orthographically-related and orthographically-unrelated primes was counterbalanced in a similar manner. In order to preserve the proportion of orthographically-related primes and targets across the manipulation of target lexical status, orthographically-related primes were presented with 26 of the nonword targets and orthographically-unrelated primes were presented with 13 of the nonword targets.

In order to reduce to proportion of orthographically-related pairs to 50%, 13 filler word trials and 13 filler nonword trials were created with completely unrelated primes and targets: primes and targets for the filler trials were comparable to their experimental counterparts for length, number of orthographic neighbours and written and spoken frequency.

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 eight practice trials to familiarize themselves with the task; as a further control over outlier responses due to unfamiliarity with the task, each experimental session began with six 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; this served both as a fixation point and as a forward mask for the incoming prime word. 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

Table 1
Length, logarithmic written and spoken frequency, orthographic neighbourhood size (*N*), and orthographic overlap with the targets for the three word-trial prime lists (+M+O, –M+O, –M–O). The mean values for orthographic overlap are calculated adopting either the left-aligned slot coding (upper row) or the spatial coding approach (lower row). Examples of primes are provided for the target word *fall*.

	+M+O (*fell*)	–M+O (*full*)	–M–O (*hope*)	*F*[2, 114]	*p*
Length	4.49 ± .97	4.49 ± .97	4.49 ± .97	0	1
Log (written frequency)	2.84 ± .78	2.81 ± .82	2.84 ± .66	.03	.97
Log (spoken frequency)	1.29 ± .87	1.47 ± .93	1.43 ± .91	.44	.65
N	8.67 ± 5.48	8.95 ± 5.99	8.59 ± 5.41	.04	.96
Orthographic overlap (slot coding)	.55 ± .21	.55 ± .22	.00 ± .00	125.77	<.001
Orthographic overlap (spatial coding)	.68 ± .17	.68 ± .17	.03 ± .08	245.26	<.001

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Table 2
Mean response times (RT; in ms) and error rates (ER) obtained by the participants in (a) Experiment 1, (b) Experiment 2 and (c) Experiment 3.

		+M+O/+C+O		−M+O/−C+O		−M−O/−C−O	
		RT	ER	RT	ER	RT	ER
(a) Exp 1	Genuine irregular	581 ± 47	.02 ± .06	602 ± 67	.04 ± .06	606 ± 53	.02 ± .06
(b) Exp 2	Pseudo-irregular	587 ± 68	.02 ± .04	588 ± 53	.04 ± .07	582 ± 52	.02 ± .04
(c) Exp 3	Genuine irregular	583 ± 55	.07 ± .16	610 ± 55	.08 ± .14	600 ± 66	.07 ± .11
	Pseudo-irregular	604 ± 65	.05 ± .11	603 ± 66	.05 ± .10	606 ± 68	.05 ± .12

the participants' response and was then replaced by a 1-s 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 controlled by the dominant hand.

Trial presentation within lists was pseudo-randomized, so that no more than four consecutive word or nonword targets could occur in a row; this design also ensured that no more than three experimental items were presented in eight consecutive trials.

Results

Data in all experiments were cleaned of aberrant data points according to the following procedure. First, items were excluded from the analyses if they elicited an overall error rate higher than 30%. Second, participants were excluded if one of the following conditions was satisfied: (i) their overall error rate on word or nonword trials was higher than 20%, (ii) their mean response time on word or nonword trials was more than three standard deviations higher than the relevant mean response time for all participants, (iii) their overall standard deviation in word trial response times was more than three standard deviations higher than the overall mean standard deviation. Third, individual data points that were excessively long[3] were also excluded from the analysis. This procedure resulted in the exclusion of two items, five subjects, and eight individual data points in this experiment.

Remaining data were analysed via by-subjects and by-items ANOVAs, with Prime Type (three levels) and Version (two levels) as factors. Following McCormick et al. (2008) and McCormick, Rastle, and Davis (2009), the ANOVA was carried out on inverse-transformed response times in order to make the Y-variable distribution more Gaussian-like.[4]

Mean response times and error rates are shown in Table 2. Lexical decision times were faster when targets were preceded by an irregular inflection than when they were preceded by an orthographically related or completely unrelated prime, an effect of Prime Type that reached statistical significance ($F_1[2, 68] = 5.55$; $p < .01$; $F_2[2, 68] = 7.33$; $p = .001$). Simple contrast analyses confirmed that irregular inflections (+M+O) triggered faster response times than orthographically-related primes (−M+O; $t_1[36] = 2.40$, $p < .05$, $t_2[36] = 2.80$, $p < .01$), and faster response times than unrelated primes ($t_1[36] = 2.85$, $p < .01$; $t_2[36] = 3.32$, $p < .01$). There was no difference between orthographically related and unrelated primes (−M−O; $t_1[36] = .54$, $p = .59$; $t_2[36] = .49$, $p = .63$).

No effect of Prime Type emerged in the accuracy analysis ($F_1[2, 68] = 1.03$; $p = .36$; $F_2[2, 68] = 1.66$; $p = .19$), and so these data were not considered any further.

Discussion

The results of Experiment 1 show that irregular inflectional forms facilitate the recognition of their stems in masked priming experiments as compared to both orthographically related and unrelated baselines. Although these results were suggested by Forster et al. (1987), Pastizzo and Feldman (2002), Kielar et al. (2008), and Meunier and Marslen-Wilson (2004), they are reported here for the first time: (a) with prime and control words carefully matched for orthographic overlap (with orthographic overlap also being determined both by slot-based coding and by spatial coding) and (b) with prime and control words also carefully matched for length, written frequency, spoken frequency and orthographic neighbourhood size across all experimental comparisons.

This pattern of results seems difficult to reconcile with the morpho-orthographic segmentation process proposed by Rastle et al. (2004). This proposal states that: (a) stimuli that can be parsed into orthographically identifiable morphemes (e.g., farmer, corner) are decomposed rapidly in visual word recognition and (b) semantic information plays no role in this process (otherwise, masked priming effects for semantically-related pairs like darkness–DARK would be significantly larger than for semantically-unrelated pairs like corner–CORN). Because irregular inflectional forms do not appear to satisfy the first condition, it seems hard to explain the masked morphological priming effects that we observed, unless we claimed that the effects were semantic in nature (which would in turn create difficulties in explaining the equivalence of darkness–DARK and corner–CORN priming).

However, one possibility remains that would allow us to explain irregular inflectional masked priming effects within the theory proposed by Rastle et al. (2004). Specifically, as it has been noted previously (Bybee & Slobin,

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[3] The threshold over which individual data points were considered to be outliers was determined independently for each experiment: a histogram of the reaction times was plotted and the first empty bin (i.e., the first zero of the density function) was taken as the cut-off value (see, e.g., Ratcliff, 1993; Sprent, 1998; van Zandt, 2002). This procedure determined the cut-off value to be 1275 ms in Experiment 1, 1350 ms in Experiment 2, and 1550 ms in Experiment 3.

[4] In all three experiments, the relevant main effects and interactions are also significant in the analyses carried out on non-transformed RTs.

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1982), irregular past tense forms are not completely idiosyncratic from an orthographic point of view, but tend to cluster in islands of sub-regularity. For example, the past tense forms of *meet*, *bleed*, *feed* and *breed* are *met*, *bled*, *fed* and *bred*; similarly, *spend*, *send*, *bend* and *lend* have inflected forms that are obtained by changing the final *-d* to a *-t* (*spent*, *sent*, *bent* and *lent*). Of course, these patterns cannot be considered as properly regular, as each of them applies only to a small group of base forms, and each also has counterexamples (e.g., intend–intended, extend–extended, blend–blended). However, they provide some statistical regularity in the very complex orthographic input that our recognition system normally encounters.

There is some evidence that these clusters of sub-regularity are identified, and even productively used, by normal speakers. For example, children occasionally make errors such as bring–brang (cf ring–rang) and bite–bote (cf write–wrote; e.g., Marcus et al., 1992) and adult speakers extend sub-regular patterns to nonword stems in experimental tests (e.g., spling–splung; Kim, Pinker, Prince, & Prasada, 1991). Thus, it is possible that the early morphological level of analysis proposed by Rastle et al. (2004) is in fact purely orthographic, and that the relationship between, e.g., *said* and *say* is captured not on the basis of a shared orthographic morpheme, but by the fact that this present–past alternation is sufficiently orthographically consistent to be exploited in early visual word recognition. If this were the case, then the morpho-orthographic level of analysis would be able to extract relationships between irregular inflections and their base forms, at least those clustering in some pattern of sub-regularity (this hypothesis does not hold, of course, for completely idiosyncratic forms, like go–went; notice though, that these words were not used in Experiment 1).

This hypothesis generates an intriguing prediction. If the early morphological analysis is really blind to semantic information, and if masked priming effects for irregular inflections (e.g., shook–SHAKE) are to be explained in terms of orthographic sub-regularities, then we must predict that similar masked priming effects will be observed for totally unrelated words that share these orthographic sub-regularities (e.g., book–BAKE; look–LAKE). This is in fact very similar to what the morpho-orthographic procedure is thought to do when it breaks down *farmer* into *farm-* and *-er*, as well as *corner* into *corn-* and *-er* (e.g., Rastle et al., 2004). This prediction will be tested in Experiment 2.

Experiment 2

In Experiment 2 we compared the response times to targets (e.g., *ray*) following the masked presentation of: (i) morphologically-unrelated words compatible with a sub-regular orthographic pattern (e.g., *raid*, in analogy to, e.g., paid–pay and laid–lay), (ii) orthographically-matched control words (e.g., *rain*), and (iii) completely unrelated words (e.g., *boon*). If the proposed morpho-orthographic segmentation procedure is sensitive to the sub-regular orthographic patterns characteristic of irregularly inflected forms, and if it is truly insensitive to semantic factors, then we expect *raid* to facilitate *ray* just as *laid* was shown to facilitate *lay* in Experiment 1.

Methods

Participants

Forty-eight students from the same population as in Experiment 1 participated in the experiment. They were paid £5 for their participation.

Materials

Thirty-nine monomorphemic English words were selected as targets; they were either singular nouns ($n = 19$), present tense verbs ($n = 15$) or adjectives ($n = 4$), and their average surface frequency was 139.04 (±333.04) occurrences per million (Baayen et al., 1993). Each target word was paired with three different prime words. In the +C+O condition, targets were paired with semantically unrelated monomorphemic words consistent with a sub-regular pattern of present–past tense alternation (e.g., *book* was paired with the target word *bake*, in analogy to shook–shake and took–take); these prime words were also orthographically related to the targets, as in the +M+O condition of Experiment 1. In the –C+O condition, targets were paired with orthographically-related prime words (e.g., bulk–BAKE), in analogy to the –M+O condition of Experiment 1. In the –C–O condition, targets were paired with completed unrelated control primes (e.g., poll–BAKE). Stimuli for this experiment are contained in Appendix B.

The degree of orthographic overlap with the target was calculated for each prime list as in Experiment 1, both considering a slot-coding approach for letter position representation and a spatial coding approach. The mean values obtained for the pseudo-sub-regular (+C+O), orthographic (–C+O) and control (–C–O) primes are reported in Table 3, together with their statistical comparisons; as with their counterparts in Experiment 1, +C+O and –C+O primes were significantly more similar to their targets than –C–O primes.

The three groups of primes were matched for length, logarithmic written frequency, logarithmic spoken frequency, and number of orthographic neighbours (see Table 3).

In order to make sure that the only difference between Experiments 1 and 2 was whether or not the sub-regular prime was a true irregular inflection of the target word (e.g., shook–SHAKE vs. book–BAKE), the +C+O prime–target pairs were matched with the +M+O prime–target pairs used in Experiment 1 for the consistency of the sub-regular present–past tense alternations. Pattern consistency was defined as the ratio between the total frequency of the present–past (or singular–plural) pairs following a specific sub-regular pattern (e.g., keep–kept, sleep–slept, sweep–swept, weep–wept and creep–crept) and the total frequency of all the pairs with the same base form pattern (e.g., all the verbs listed above, plus steep–steeped, peep–peeped and seep–seeped). Since 17 sub-regular patterns were used to generate the 39 trials in Experiment 2, while 32 were employed in Experiment 1, consistency was matched across experiments considering patterns both by type (Experiment 1: .44 ± .35; Experiment 2: .42 ± .35;

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Table 3
Length, logarithmic written and spoken frequency, orthographic neighbourhood size (*N*), and orthographic overlap with the targets for the three word-trial prime lists used in Experiment 2: sub-regular (+C+O), orthographic (–C+O) and control (–C–O). The mean values are calculated adopting either the slot coding (upper row) or the spatial coding approach (lower row). Examples of primes are provided for the word target 'bake'.

	+C+O (book)	–C+O (bulk)	–C–O (poll)	*F*[2, 114]	*p*
Length	4.18 ± .76	4.21 ± .83	4.21 ± .83	.01	.99
Log (written frequency)	1.24 ± .73	1.34 ± .55	1.29 ± .50	.29	.75
Log (spoken frequency)	.94 ± .89	.99 ± .63	.85 ± .65	.37	.69
N	10.26 ± 4.99	10.28 ± 6.10	9.79 ± 5.43	.09	.90
Orthographic overlap (slot coding)	.57 ± .22	.57 ± .20	.00 ± .02	140.88	<.001
Orthographic overlap (spatial coding)	.72 ± .15	.71 ± .15	.03 ± .08	362.38	<.001

t[45] = .23, *p* = .82) and by token (Experiment 1: .43 ± .34; Experiment 2: .39 ± .32; *t*[74] = .60, *p* = .55).

The nonword trials for this experiment were constructed exactly as in Experiment 1.

The 26 unrelated filler trials used in Experiment 1 were also utilized in Experiment 2 in order to bring the overall proportion of related and unrelated trials in each single stimulus list to .50.

Finally, counterbalancing of primes to word targets was achieved in the same manner as in Experiment 1, except that particular care was taken to distribute sub-regular prime–target pairs of the same type (e.g., tall–TELL, hall–HELL) evenly across the rotations.

Results

Data were trimmed as in Experiment 1; this resulted in the exclusion of two items, eight subjects and 10 individual data points. The by-item and by-subject datasets were then computed and analysed as in Experiment 1.

Mean response times and error rates are shown in Table 2. There was no effect of Prime Type on response times ($F_1[2, 72] = .46$; $p = .63$; $F_2[2, 68] = .21$; $p = .65$), demonstrating that pseudo-inflected sub-regular forms like 'book' do not facilitate the recognition of their corresponding pseudo-base forms (*bake*) as compared to orthographically matched or unrelated control primes. The accuracy analysis also revealed no effect of Prime Type ($F_1[2, 72] = 1.38$; $p = .26$; $F_2[2, 68] = 1.50$; $p = .23$).

Discussion

The results of Experiment 2 clearly suggest that the irregular masked priming effect observed between genuinely related forms (e.g., *shook* and *shake*) in Experiment 1 does not generalise to pairs of unrelated words characterized by the same orthographic pattern (e.g., *book* and *bake*). This result would appear to pose difficulty for the notion of an early morphological analysis that is blind to semantic information. However, two problems with this interpretation are: (a) that these two experiments were conducted using different groups of participants (and we have no statistical support that the results of the two experiments were reliably different from one another) and (b) that the sub-regular patterns used in Experiments 1 and 2 were not completely identical. Perfect pairwise matching of genuinely related irregular pairs and their orthographic sub-regular counterparts on this factor would certainly constitute stronger evidence that irregular masked priming holds only among genuine morphological relatives. Experiment 3 was designed to address this issue.

Experiment 3

Experiment 3 examined masked priming effects for two kinds of prime–targets pairs: pairs that had a genuine irregular morphological relationship (e.g., sworn–SWEAR) and pairs that had a pseudo-irregular morphological relationship using the same sub-regular orthographic patterns (e.g., porn–PEAR). Priming effects in each of these conditions were measured against both orthographic (e.g., swamp–SWEAR, port–PEAR) and unrelated baselines (e.g., pinch–SWEAR; fish–PEAR). The use of different control words for the genuine morphological relatives and the pseudo-morphological relatives allows tight control over length, frequency and orthographic neighbourhood size across all experimental conditions, as in Experiments 1 and 2.

Methods

Participants

Ninety students from the same population as in Experiment 1 participated in the experiment. Participants received either course credits or £5 for their participation.

Materials

For the genuine irregular condition, 30 monomorphemic English words were selected as targets; they were either singular nouns ($n = 4$) or present tense verbs ($n = 26$), and their mean surface frequency was 2296.48 (±3935.6) occurrences per million (Baayen et al., 1993). As in Experiment 1, each of these target words was paired with three different prime words: an irregularly inflected form of the target (+M+O: e.g., fell–FALL), an orthographically similar word (–M+O; e.g., full–FALL) and a completely unrelated word (–M–O; e.g., hope–FALL).

For the pseudo-irregular condition, an additional set of 30 target words was created by pairing each of the target words that gave rise to the genuine irregular +M+O, –M+O and –M–O conditions with one word sharing the same orthographic body (e.g., *tall* was paired with *fall*). The mean surface frequency of these target words was 780.13 (±1663.15) occurrences per million (Baayen et al., 1993). These 30 target words (16 nouns, 14 verbs and two adjectives) were then paired with three different primes, in analogy to Experiment 2: a semantically unrelated monomorphemic word that is coherent with a sub-regular pattern of present–past tense alternation (+C+O; e.g., tell–TALL, in analogy to fell–FALL), an orthographically-related word (–C+O; e.g., toll–TALL), and a completely unrelated control prime (–C–O; e.g., dome–TALL).

This design ensures a perfect pairwise matching between the genuine irregular and the pseudo-irregular trials for the sub-regular pattern they follow; obviously, this also implies that the genuine irregular and the pseudo-irregular conditions are matched perfectly for sub-regular pattern consistency. Stimuli for this experiment are contained in Appendix C.

The two sets of target words were carefully matched for length in letters (4.17 ± .87 vs. 4.00 ± .83; $t[58] = .76$, $p = .45$), logarithmic written frequency (2.82 ± .77 vs. 2.69 ± .80; $t[58] = .66$, $p = .51$), logarithmic spoken frequency (1.56 ± 1.02 vs. 1.44 ± .90; $t[58] = .49$, $p = .62$) and number of orthographic neighbours (10.67 ± 5.69 vs. 11.37 ± 5.61; $t[58] = .48$, $p = .63$).

The degree of orthographic overlap with the target was calculated for each prime list as in Experiments 1 and 2. Both for the genuine irregular and the pseudo-irregular conditions, the mean values obtained for the +M+O/+C+O and the –M+O/–C+O primes were significantly higher than those obtained for the –M–O/–C–O primes (see Table 4).

Prime type matching was carried out independently for the genuine irregular and the pseudo-irregular conditions; however, the ranges of length, written and spoken frequency, and orthographic neighbourhood size were comparable for the two conditions (see Table 4).

The nonword trials were constructed following the same procedure used in Experiments 1 and 2; the only difference was that in this experiment there were 60 nonword trials rather than 39, due to the higher number of experimental items.

Forty unrelated filler trials were created in order to bring the overall proportion of related and unrelated trials in each single stimulus list to .50.

The assignment of primes to the two sets of targets was counterbalanced across participants as in Experiments 1 and 2.

Procedure

Participants were tested using exactly the same procedure as in Experiments 1 and 2.

Results

Response time data were trimmed as in Experiments 1 and 2; this resulted in the exclusion of two target items, 13 subjects and 31 individual data points.

Remaining data were analysed using by-subjects and by-items ANOVAs with Morphological Status (two levels: genuine irregular vs. pseudo-irregular), Prime Type (three levels: +M+O vs. –M+O vs. –M–O), and Version (two levels) as factors. As in Experiments 1 and 2, the ANOVA was carried out on the inverse-transformed response times in order to make the Y-variable distribution more Gaussian-like.

The mean response times and accuracy levels shown by the participant in the genuine irregular and pseudo-irregular conditions are displayed in Table 2. The ANOVA revealed a marginally significant effect of Prime Type ($F_1[2, 148] = 4.46$; $p = .01$; $F_2[2, 52] = 2.50$; $p = .09$). More importantly, both the subject and the item analyses showed an interaction between Morphological Status and Prime Type ($F_1[2, 148] = 5.97$; $p < .005$; $F_2[2, 52] = 4.39$; $p < .05$). As in Experiments 1 and 2, no effect emerged in the accuracy analysis (all F values for the Prime Type effect and the interaction were lower than 1).

Table 4
Length, logarithmic written and spoken frequency, orthographic neighbourhood size (*N*), and orthographic overlap with the targets for the six prime conditions tested in Experiment 3. The mean values for orthographic overlap are calculated adopting either the slot coding or the spatial coding approach. Examples of primes are provided for the word target *speak* (genuine irregular conditions) and the word target *peak* (pseudo-irregular conditions).

	+M+O (*spoke*)	−M+O (*space*)	−M−O (*lunch*)	*F*[2, 58]	*p*
Genuine irregular conditions:					
Length	4.37 ± .93	4.37 ± .93	4.37 ± .93	0	1
Log written frequency	2.82 ± .82	2.74 ± .92	2.70 ± .67	.17	.84
Log oral frequency	1.31 ± .87	1.25 ± 1.02	1.29 ± .93	.03	.97
N	8.37 ± 5.22	9.13 ± 5.67	8.83 ± 5.62	.15	.86
Orthographic overlap (slot coding)	.51 ± .18	.52 ± .19	0	207.31	<.001
Orthographic overlap (spatial coding)	.67 ± .12	.70 ± .13	.01 ± .05	556.76	<.001
	+C+O (*poke*)	−C+O (*pace*)	−C−O (*slow*)	*F*[2, 58]	*p*
Pseudo-irregular conditions:					
Length	4.20 ± 1	4.20 ± 1	4.20 ± 1	0	1
Log written frequency	2.30 ± .67	2.51 ± .76	2.48 ± .64	.82	.44
Log oral frequency	.80 ± .83	1.10 ± .79	.92 ± .77	1.04	.36
N	10.60 ± 5.61	10.40 ± 6.26	9.83 ± 5.62	0.14	.87
Orthographic overlap (slot coding)	.48 ± .20	.47 ± .20	.01 ± .04	142.61	<.001
Orthographic overlap (spatial coding)	.67 ± .13	.66 ± .15	.03 ± .06	440.52	<.001

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The source of the interaction between Morphological Status and Prime Type was examined using planned comparisons. These analyses revealed that there was no difference whatsoever across the pseudo-irregular conditions (all comparisons yielded $t_1 < .20$ and $t_2 < .45$), whereas genuinely irregular +M+O primes facilitated target recognition significantly more than the matched orthographic –M+O primes ($t_1[76] = 3.95$, $p < .001$; $t_2[28] = 3.13$, $p < .005$) and the matched unrelated –M–O primes ($t_1[76] = 2.61$, $p = .01$; $t_2[28] = 2.03$, $p = .05$). These two latter conditions were not significantly different from one another ($t_1[76] = 1.48$, $p = .14$; $t_2[28] = 1.62$, $p = .12$).

Discussion

Experiment 3 confirms the pattern of results that emerged in Experiments 1 and 2. When the genuine morphological relatives and their orthographically sub-regular counterparts are tested on the same sample of participants, with a careful pairwise matching of the sub-regular patterns used, the masked presentation of ‘shook’ facilitates the recognition of the target ‘shake’ as compared to both an orthographic and an unrelated baseline, but the masked presentation of ‘book’ does not facilitate the recognition of ‘bake’, irrespective of the baseline used.

General discussion

The results of the experiments reported here seem to present us with a difficult problem. On the one hand, there is now considerable evidence for a rapid form of morphological decomposition that can be observed in masked priming, and that is based on the orthographic appearance of printed stimuli (i.e., also applies to pseudo-complex words); prime–target pairs like darkness–DARK and corner–CORN yield more priming than pairs without a morphological structure like brothel–BROTH (see Rastle and Davis (2008), for a review). On the other hand, our results demonstrate that irregular-inflectional pairs like fell–FALL yield significantly larger masked priming effects than: (a) comparable pseudo-irregular pairs like tell–TALL and (b) matched orthographic control pairs like full–FALL. Clearly, the results presented here cannot be attributed to the morpho-orthographic segmentation process described by Rastle et al. (2004): no shared stem is orthographically identifiable in *fell* and *fall*, and the recognition system does not appear to treat the orthographic patterns that characterize irregular inflections and their stems in a special way (or else we would have seen priming for tell–TALL also). This suggests that a second locus of early morphological priming must exist: the question then arises as to where this second source of priming must be located within the printed word recognition system.

If the fell–FALL effect cannot be attributed to morpho-orthographic overlap between prime and target, then one potential way of explaining it would be to argue that the priming observed for irregular inflections actually constitutes a semantic effect, or more likely, an effect of the combination of orthographic and semantic similarity (Gonnerman, Seidenberg, & Andersen, 2007). The main difficulty with this proposal is that masked priming studies typically show that priming effects for semantically-transparent derivational pairs like darkness–DARK do not differ significantly from priming effects for pseudo-morphological pairs like corner–CORN (see Longtin et al. (2003), Marslen-Wilson et al. (2008), Rastle et al. (2004)). If the combination of semantic and orthographic similarity were playing a strong role in masked priming, then it seems that a convincing effect should be apparent across this comparison. Of course, it is possible that the semantic relationships between inflected forms and their stems are stronger than those between derived forms and their stems. In fact, there are several reasons to believe that this might be the case. For example, theoretical linguists generally hold that inflectional processes never lead to the formation of an independent lexical entry, while derivational processes always do (e.g., Kuryłowicz, 1964). This claim is based on a number of arguments, including the fact that: (i) inflectional relatives always share the same grammatical class, while this is not always the case for derivations (e.g., heal–healer, dark–darkness), (ii) inflectional processes preserve the meaning of their (unaffixed) base morphemes, while this is not always the case for derivations (e.g., critic–critical, angel–angelic; e.g., Aronoff, 1976), and (iii) inflection implies a consistent and transparent semantic change (e.g., the relationship between cat and cats is perfectly comparable to the relationship between idea and ideas), while derivational relationships are much more idiosyncratic (e.g., a gardener is a professional who takes care of gardens, while a juicer is a kitchen appliance that makes juice). However, even if we accept that inflectional relatives are more semantically similar than derivational relatives, pursuing an account of the fell–FALL effect based on the combination of orthographic and semantic similarity would force us to explain why the combined effects of orthographic and semantic similarity yield virtually no benefit for darkness–DARK pairs relative to corner–CORN pairs,[5] but yield a robust 20-ms benefit for fell–FALL pairs over an equivalent baseline. In the absence of a computational simulation showing that this is possible, we would have difficulty making this kind of argument.

The hypothesis that we are left with is that the priming observed for irregular inflections arises at some intermediate level between the morpho-orthographic

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[5] There have now been three experiments reporting larger masked priming effects for darkness-DARK items than for corner-CORN items (Diependaele et al., 2005; Feldman et al., 2009). However, these constitute a small minority of the 17 studies that have tested this comparison (Rastle & Davis, 2008), and at least the two studies reported in Diependaele et al. (2005) consist of a relatively small number of data points, thus making the estimate of the population mean less reliable (Davis & Rastle, in press). More importantly, these studies differ from the others in potentially important ways, including the insertion of a backward mask between prime and target (Diependaele et al., 2005, Experiment 1), the repetition of primes and targets throughout the experiment in conditions of partial visibility (Diependaele et al., 2005, Experiment 2), and the use of a number of items in the pseudo-morphological condition that did not respect the orthographic rules of morphological combination (e.g., harness-HARP; blistery-BLISS; Feldman et al., 2009, see Davis and Rastle, in press, for discussion).

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segmentation stage and the semantic system. Fortunately, a model of the recognition of morphologically-complex words that incorporates such a level has already been proposed by Taft and colleagues (e.g., Taft, 2003, 2004; Taft & Kougious, 2004). The authors first propose the existence of a peripheral level of analysis (form code) in which words are decomposed into smaller morphemic or non-morphemic parts (e.g., on the basis of syllabic units or BOSS units; see Taft (2003)). On this level, *mending* would be parsed into *mend* and *ing*, and *picnic* would be parsed into *pic* and *nic*. This level of analysis then feeds information into a lemma level that contains representations of: (i) free stems (e.g., *dog*), (ii) bound stems (e.g., *vir*-, as in *virus* and *viral*), (iii) derivational morphemes (e.g., -*er* as in *dealer* and *viewer*), and (iv) polymorphemic words (e.g., *dealer*). This level of representation is organised in such a way that lemmas for polymorphemic words (e.g., *viewer*) are activated via the lemmas for their constituent morphemes (*view* and -*er*); so, the form code units for *view* and -*er* activate the lemma nodes for *view* and -*er*, which in turns contact the lemma for *viewer*. Notably, while there are lemmas for derived forms: (i) there are no lemmas for inflected forms (e.g., *cats*, *fell*); (ii) no whole-word representations exist for regularly inflected words whatsoever in the model; (iii) the model does not include an orthographic lexicon. Word identification (lexical decision) is based on activations at the lemma level and on a later stage of morphological recombination.

This model seems to provide a nice explanation for the results described in the present work. Irregular inflections such as fell activate the lemma representation of their base form (*fall*) just as the base form itself does. It is this activation of lemma units by the primes (which would not occur for either orthographic or unrelated controls) that permits savings in the processing of the target. Critically, these same savings would not be expected to arise when using pseudo-irregular words as primes (bell–BALL) because representations at the lemma level are not based on orthographic regularities, but reflect genuine morphological relationships, i.e., different orthographic forms access the same lemma node only if they truly are different inflected forms of the same lexical entry (like *fell* and *fall*, but unlike *bell* and *ball*). This model also accounts for a range of other word recognition data including findings reported by Taft and Kougious (2004) and Taft (2004). Unfortunately, this model is less successful in accounting for two key effects: (a) the finding that brother–BROTH items yield more priming than brothel–BROTH items; and (b) the finding that darkness–DARK items yield similar priming effects to corner–CORN items (Longtin et al., 2003; Marslen-Wilson et al., 2008; Rastle et al., 2004; see Rastle and Davis (2008) for a review).

In respect of the first problem, the priming for brother–BROTH items cannot occur at the lemma level, because this level of representation codes only genuine morphological relationships, and a brother is not someone who broths. One possibility is that this effect could be captured at the peripheral form level of representation. However, it is difficult to discern exactly how this level operates. Taft (2003) states that this form level breaks long words down on the basis of morphemic units, but possibly also on the basis of other units (e.g., syllables, BOSSes; so that it decomposes *picnic* into *pic* + *nic*). Thus, while this level of analysis would decompose *brother* into *broth* + *er*, it would also seem to decompose *brothel* into *broth* + *el*, leading to the prediction that this stimulus should also prime *broth*. This problem could be solved by postulating that the peripheral form level of analysis constitutes a morpho-orthographic decomposition procedure – a process restricted to semantically-blind decomposition of morphologically-structured stimuli – as described by Rastle et al. (2004).

The second problem arises because the lemma level codes not only genuine inflectional relationships but also genuine derivational relationships. Thus, even if we posit that *corner* and *darkness* are both decomposed at the form level, *darkness* activates the lemma nodes for *dark*, -*ness*, and *darkness* on this account, while *corner* only contacts the lemma node for *corner*. Because lexical decisions are made on the basis of lemma activations, the fact that *darkness* activates the lemma for *dark*, while *corner* does not activate the lemma for *corn* seems to predict that priming for darkness–DARK pairs should be greater than priming for corner–CORN pairs. This problem could be solved by postulating a different conception of the lemma level, i.e., one in which this level of representation does not have the primary role of capturing form-meaning covariations, but of storing individual lexical entries (i.e., vocabulary entries) as defined by: (i) a specific meaning and (ii) a set of lexical-syntactic properties (e.g., grammatical class; see Levelt, Roelofs, and Meyer (1999)). This conception of lemma has been mainly developed in the word production literature (e.g., Kempen & Hoenkamp, 1987; Levelt, 1993; Roelofs, 1992), where it has received strong experimental support (e.g., Garrett, 1980; van Turennout, Hagoort, & Brown, 1997; Vigliocco, Antonini, & Garrett, 1997). Critically, this conception of lemma proposes that derivationally-related words have independent representations at this level: while *fell* and *fall* (or *falls* and *fall*) have substantially overlapping meanings and identical lexical-syntactic properties (and thus share their lemma node as in Taft's model), the same is not true for *darkly* and *darkness* (e.g., they belong to different grammatical classes) and *corner* and *corn* (e.g., they are completely unrelated in meaning). Moreover, a lemma level defined in this way does not include representations for bound stems (in contrast to what is assumed in Taft's model), as bound stems are nonwords and thus do not constitute lexical entries.

These modifications to the model proposed by Taft and colleagues now permit us to account for the key findings obtained on morphological masked priming effects, namely: (i) *corner* primes *corn* more than *brothel* primes *broth* (e.g., Rastle et al., 2004); (ii) *corner* primes *corn* to the same extent as *darkness* primes *dark* (e.g., Rastle et al., 2004); and (iii) *fell* primes *fall* more than *full* primes *fall* or *tell* primes *tall* (present work). In respect of (i), the morpho-orthographic segmentation procedure activates {corn} when presented with the prime *corner*, and this prior activation yields savings in the processing of the target *corn*. The target *broth* does not benefit from prior pre-

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sentation of *brothel*, because this prime cannot be fully decomposed by the morpho-orthographic segmentation process and, thus, no activation is triggered in the morpho-orthographic node {broth} by the presentation of *brothel*. In respect of (ii), *darkness* primes *dark* via the same mechanism that *corner* primes *corn*; further, because semantic information does not inform morpho-orthographic segmentation, and because *darkness* and *dark* do not share a lemma, the priming effects are statistically equivalent in both cases. Finally, in respect of (iii), the prime *fell* activates {fell} at the morpho-orthographic stage; this in turn activates the {fall} lemma, which is the same that is contacted by the target *fall*. Because neither of the pairs full–FALL nor tell–TALL shares a lemma, the targets in these cases do not enjoy the same processing benefit.

One issue that we have not yet dealt with concerns the absence of an orthographic lexicon in the model proposed by Taft and colleagues. Though this does not appear to be required to account for the various masked morphological priming effects described above, the lack of an orthographic lexicon raises substantive other issues. For example, how is it that readers distinguish between existing inflected forms such as falls (to which, for example, they will answer YES in a lexical decision task) and non-existing, but grammatically legitimate, pseudowords such as *falled* (to which any skilled reader would easily say NO in lexical decision)? In the current model, *falls* and *falled* will be both decomposed into their morphemes at the morpho-orthographic segmentation stage, and will both activate the lemma {fall}, arguably to the same extent. Moreover, it is not possible to decide that *falls* is a word while *falled* is not using a syntax checking routine (such as those proposed by Taft (2004), and Schreuder and Baayen (1997), as both of these examples are characterized by the perfectly regular addition of an inflectional marker whose meaning fits well with the meaning of the stem. The model thus seems to need an orthographic lexicon in which existing forms like *falls* would be represented, while non-existing pseudowords like *falled* would not. If we posit that lexical decisions are made on the basis of activations at this level (e.g., Coltheart, Rastle, Perry, Langdon, & Ziegler, 2001; Perry, Ziegler, & Zorzi, 2007), then the rejection of *falled* would be easily explained by the fact that the activation coming from {fall} and {-ed} at the morpho-orthographic stage does not result in sufficient activation of any lexical entry.

Importantly, the introduction of an orthographic lexicon into the model does not impair its ability to account for: (i) the brother–BROTH vs. brothel–BROTH effect, (ii) the lack of differential masked priming in darkness–DARK vs. corner–CORN, and (iii) the irregular masked priming illustrated in the present work. (i) is completely explained at the morpho-orthographic segmentation stage, while (ii) is dependent on the lack of a shared lemma between *darkness* and *dark*, a feature that is unaffected by the existence of an orthographic lexicon. With respect to (iii), we need to hypothesise that the orthographic lexicon and the lemma level are connected interactively. If this is the case, then the prime *fell* activates {fell} in the orthographic lexicon; this in turn activates the {fall} lemma, which sends activation back to the {fall} node in the orthographic lexicon, thus effecting savings in the processing of the target *fall*. We would not like to take a position on the nature of the links between the morpho-orthographic segmentation stage and the orthographic lexicon, as we are aware of no

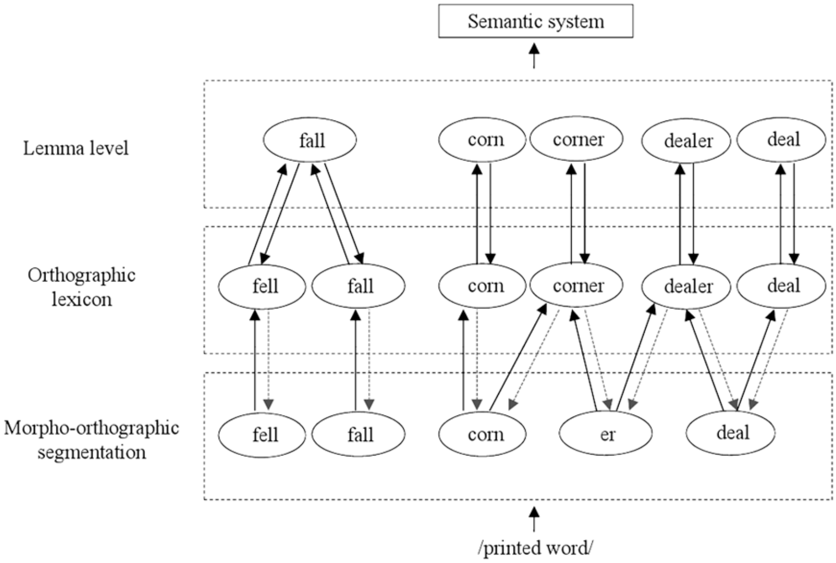

Fig. 1. Graphic representation of the extension of the morpho-orthographic segmentation hypothesis proposed to account for the results emerged in the present work, along with those previously obtained on masked morphological priming (e.g., Longtin et al., 2003; Rastle et al., 2004). The dashed links connecting the orthographic lexicon with the morpho-orthographic stage indicate that feed-back might occur at that level, but there are no compelling data showing that this is really the case.

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data that would clearly constrain the theory in either direction (feed-forward vs. interactive). A complete sketch of the model proposed here is offered in Fig. 1.

If derivational relationships are not coded at the lemma level in our model, one might ask where the system captures the concomitant semantic and orthographic overlap typical of these morphological ties. We suggest that this happens because genuine derivationally-related pairs of words (e.g., *darkness* and *darkly*) share both: (i) a pre-lexical morpho-orthographic node (*dark*) and (ii) some features in the semantic system. These two characteristics unequivocally define derivationally-related words as opposed to pseudo-derived words – e.g., *corner* and *corn*, which lack (ii) – and words that are semantically related – e.g., *dealer* and *trade*, which lack (i). The fact that morpho-orthographic representations are able to accommodate for slight modifications of the stem in derived forms (e.g., adorable–adore, dropper–drop; McCormick et al., 2008) also guarantees that the system is able to capture the relationship between allomorphic derived words (e.g., *sizable*) and their stems (*size*).

Two further differences between the model proposed here and the original model by Taft and collaborators (Taft, 2003, 2004) are that: (i) lemma nodes do not exist for bound morphemes (e.g., *vir-* as in *viral* and *virus*) on our account and (ii) there is no recombination stage in our model, i.e., processing dynamics arise only from node activations. One might then question whether the new model might have difficulties in explaining the results that originally seemed to require the existence of lemma nodes for bound stems (Taft & Kougious, 2004) and of the recombination stage (Taft, 2004).

In relation to the first issue of lemma nodes for bound stems, Taft and Kougious (2004) reported that words related in form and meaning, like *virus* and *viral*, yield larger masked priming effects than words related only in form (e.g., future–FUTILE) compared to an unrelated baseline (e.g., major–VIRAL, kettle–FUTILE). This result was interpreted as evidence that the bound stem *vir-* has an independent lemma node, and that this node is contacted while the system is recognising both *virus* and *viral*. Although this interpretation is certainly viable, these results can also be explained in the new model at the morpho-orthographic segmentation stage, where bound stems like *vir-* are likely to be represented, while non-morphological clusters like *fut-* are not; consequently, *virus* and *viral* will be parsed into *vir* + *us* and *vir* + *al*, but *future* and *futile* will not be chunked at all. Incidentally, the existence of pre-lexical representation for bound stems also explains why these items (e.g., *vive*, as in *revive* and *survive*) are more difficult to reject in lexical decision tasks than non-morphological controls (e.g., *lish*; Taft & Forster, 1975), another result that has been taken to indicate the existence of lemma nodes for bound stems (e.g., Taft, 2003).

In relation to the second issue regarding the presence of a recombination stage, Taft (2004) reported two experiments in which lexical decision times were faster for low base-frequency words (e.g., *fangs*) than for high base-frequency words (e.g., *moons*) when real-stem nonword distractors (e.g., *mirths*) were used as foils (the so-called reverse base frequency effect). Taft (2004) attributed this effect to a recombination stage in which the independently-identified stem and suffix are brought together; at this level, rare combinations of stems and suffixes – i.e., low-frequency words with a high-frequency stem, like *moons* – are particularly difficult to process, because *moon* is pluralised very rarely. Thus, when the recombination stage is critical for carrying out the lexical decisions (i.e., illegal combinations of existing stems and suffixes are included in the experiment), rare words with high-frequency stems elicit longer reaction times than rare words with low frequency stems. Though this interpretation works nicely, there is an alternative account that dispenses with a recombination stage, and is compatible with the more general model proposed in this work. This alternative account is based on two facts. First, rare plurals of high-frequency stems (*moons*) have by definition a very strong (i.e., higher-frequency) competitor within the lexicon – the stem itself (*moon*); this is not the case for rare plurals of rare stems (*fang* is not a very strong competitor for *fangs* because these two words are of similar frequency). Second, nonword distractors with existing stems (*mirths*) make the task more difficult and thus lengthen response times overall. On the basis of these considerations, one might argue that *moons* is identified more slowly than *fangs* because it suffers from its strong competitor *moon* (in computational terms, its node in the orthographic lexicon receives strong lateral inhibition from the node for moon). This *moon* vs. *moons* competition becomes more intense as time goes by because high-frequency words grow in activation faster than low-frequency word (see, for example, Coltheart et al. (2001), McClelland and Rumelhart (1981), Ratcliff and McKoon (2000), Wagenmakers, Zeelenberg, and Raaijmakers (2000)). Consequently, the reverse base frequency effect is most likely to emerge when responses are slower overall, as is the case when nonword distractors have real word stems (e.g., *trouts*). This account is also supported by the fact that participants responded much more slowly in Taft (2004) when they were presented with real-stem nonwords (overall mean RT: 680 ms), than when they were presented with non-morphological foils (overall mean RT: 505 ms).

Though this model appears to account for a variety of experimental data on the identification of morphologically-complex words, the theory in its current form raises some issues regarding its implementation. For example, we have reported above that the morpho-orthographic segmentation stage seems to have a "full decomposability" constraint, i.e., it only breaks down words that are entirely decomposable into morphemes. This is the general interpretation of the fact that *brothel* does not prime *broth*, while *corner* primes *corn* (e.g., Longtin et al., 2003; Marslen-Wilson et al., 2008; Rastle & Davis, 2008; Rastle et al., 2004), and we have endorsed this view in the present work. However, it is not clear how this constraint would be implemented in a computational version of the model, particularly if assuming

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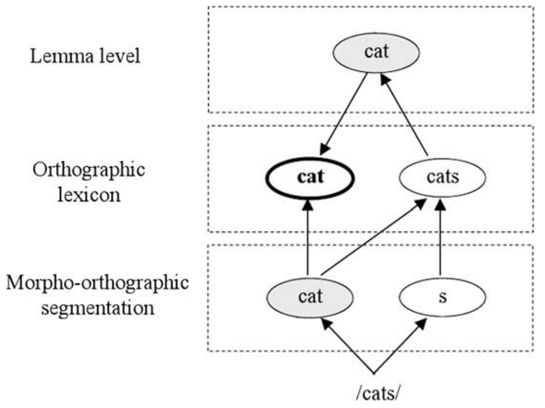

Fig. 2. The functional mechanisms that are expected to determine masked morphological priming between *cats* and *cat* (and, more in general, between any regular inflected word and its stem). *Cats* would be decomposed into *cat* and *-s* at the morpho-orthographic segmentation stage; moreover, *cats* and *cat* are held to share their lemma node {cat}. This implies that two concurrent sources of priming will be put in place (the pale grey nodes), thus determining larger masked priming effects than those observed between irregularly inflected words and their base forms (fell–FALL), between derived words and their stems (dealer–DEAL), and between pseudo-derived words and their pseudo-stems (corner–CORN).

that information on letter identity is fed to the word recognition system in a left-to-right, serial fashion (see Davis (1999), Harcum and Nice (1975), O'Regan and Jacobs (1992), Rumelhart and McClelland (1982); but also see Diependele, Sandra, and Grainger (2009) for the suggestion that morpho-orthographic decomposition occurs in parallel). If implemented as a classical interactive activation system, the model would now predict that a left-to-right parsing of the string *brothel* would lead to the activation of the morpho-orthographic unit for *broth*; it is hard to see how the subsequent processing of the cluster el could "block" the activation of {broth}, so as to prevent masked priming from arising in brothel–BROTH. Clearly, alternative computational approaches need to be explored if this model is to be implemented as a fully specified model.

The entire decomposability constraint is based on the proposal that no morpho-orthographic representation exists for non-morphological clusters of letters (like *-el* in *brothel*); one might ask why this is the case, given that this level of analysis is not meant to capture form-meaning correlations (as it is clear from the fact that it breaks down *corner* into *corn* + *er* even if a corner is not someone who corns). One plausible answer to this question is that the morpho-orthographic stage is exclusively focused on lexical identification and, thus, is insensitive to semantic factors; it exists for the purpose of allowing a more efficient (i.e., faster and less error prone) transfer of information to the next level through chunking (see Miller (1956), Nigrin (1993)). If this is the case, it is sensible to suggest that units at this level are built only for clusters of letters that are sufficiently frequent (see Davis (1999)), otherwise the cost of storing an additional representation is not compensated by the processing benefit that it confers. Morphology would inform this process because it generates statistical regularities in the orthographic input; with very few exceptions, morphemes are also recurrent clusters of letters. Because *-er* carries meaning and can be used productively with other morphemes (*dealer*, *buyer*, *cooler*, etc.), it will occur much more frequently in words than *-el*; this is why *-er* has a morpho-orthographic unit, while *-el* has not. Crucially, this account leaves open the possibility that rare morphemes might not be represented at this level, while very frequent non-morphological clusters might be; although this has never been addressed experimentally to our knowledge, items of these types would be very rare indeed.

Notwithstanding the theory outlined in the present work is not ready yet for implementation, it is falsifiable in its current form; in fact, it sets a clear prediction regarding masked priming effects with regular inflected words (e.g., cats–CAT). Because it proposes the existence of a pre-lexical, morpho-orthographic segmentation stage, which is expected to break down *cats* into *cat* and *-s*, and it also states that inflections share a lemma node, regular inflections should benefit from two concurrent sources of priming (see Fig. 2). This is not the case for irregularly inflected words because they do not benefit from morpho-orthographic segmentation, nor is it the case for genuine derivations and pseudo-derivations because they have no relationship with their stems at the lemma level. Thus, we would predict that pairs like cats–CAT would yield even larger masked priming effects than fell–FALL, darkness–DARK, and corner–CORN, provided that these conditions are carefully matched on all relevant factors.

Unfortunately, we are not aware of any direct evidence regarding this prediction in English, and while there are potentially relevant studies in Spanish (Dominguez, Segui, & Cuetos, 2002; Sànchez-Casas, Igoa, & García-Albea, 2003), these have yielded conflicting patterns of results, have suffered from difficulties with stimulus matching, and have not used comparable prime durations to those employed here. Further research is therefore needed to permit a sharp test of the theory laid down in the present paper.

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Appendix A. Target and prime words used in Experiment 1

Target	+M+O prime	–M+O prime	–M–O primes
BUY	Bought	Bounce	Slight
CATCH	Caught	Cancer	Bridge
DIG	Dug	Dog	Pop
EAT	Ate	Tea	Joy
FALL	Fell	Full	Hope
FLEE	Fled	Flex	Hint
FOOT	Feet	Fact	Weak
HIDE	Hidden	Hinder	Follow
LEAVE	Left	Less	Good
LEND	Lent	Lens	Bark
LOUSE	Lice	Life	Ping
MAKE	Made	Male	Post
MOUSE	Mice	Maze	Warn
PAY	Paid	Pain	Feel
PENNY	Pence	Penal	Broom
RIDE	Rode	Rude	Pack
RISE	Rose	Ruse	Tall
RUN	Ran	Ron	Top
SAY	Said	Same	Work
SEE	Seen	Seem	Room
SEEK	Sought	Smooth	Bright
SELL	Sold	Salt	Moon
SHAKE	Shook	Shock	Touch
SIT	Sat	Set	Cow
SLAY	Slew	Slip	Peek
SLEEP	Slept	Sleek	Route
SLING	Slung	Slang	Creed
SPEAK	Spoke	Space	Lunch
SPIT	Spat	Spot	Dose
STEAL	Stole	Stale	Wound
STICK	Stuck	Stock	Rough
STRING	Strung	Strong	Chance
SWEAR	Sworn	Swamp	Pinch
SWEEP	Swept	Sweet	Coast
THINK	Thought	Through	Breathe
TOOTH	Teeth	Truth	Drive
WEAR	Worn	Wire	Sink
WIN	Won	Wan	Job
WRING	Wrung	Wrong	Float

Appendix B. Target and prime words used in Experiment 2

Target	+C+O prime	–C+O prime	–C–O primes
BAKE	Book	Bulk	Poll
BALL	Bell	Bull	Rope
BEAR	Born	Bird	Kiss
BEE	Been	Beef	Jump
BIG	Bug	Bag	Cap
BIKE	Buck	Back	Warm
BOOT	Beet	Bait	Swim
BRAKE	Brook	Brick	Tough
BUN	Ban	Bin	Sag
CHICK	Chuck	Check	Dream
CLICK	Cluck	Clock	Stone

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Appendix B (*continued*)

Target	+C+O prime	–C+O prime	–C–O primes
DAY	Dew	Dip	Fig
DOUSE	Dice	Duck	Sail
FAKE	Fade	Fate	Hill
FUN	Fan	Fin	Wad
HALL	Hell	Hull	Pink
HEAL	Hole	Hale	Curt
LAKE	Look	Lock	Root
LIKE	Luck	Lick	Ward
MIND	Mound	Mouth	Force
PEAK	Poke	Pace	Slow
PICK	Puck	Peck	Flaw
PLAY	Plaid	Plain	Judge
PUN	Pan	Pen	Bog
RAY	Raid	Rain	Boon
RIG	Rug	Rag	Pad
SCREE	Screen	Screech	Stealth
SHALL	Shell	Skull	Crisp
SHOOT	Sheet	Shaft	Curve
SICK	Suck	Sock	Tame
SMALL	Smell	Skill	Crime
SPOUSE	Spice	Spite	Tight
STAY	Staid	Stain	Quilt
STEAK	Stoke	Stick	Crown
TALL	Tell	Toll	Dome
TEND	Tent	Tens	Hash
TIN	Ton	Ten	Hug
TRICK	Truck	Track	Sheep
WAKE	Wade	Wage	Dust

Appendix C. Target and prime words used in Experiment 3

Genuine irregular				Pseudo-irregular			
Target	+M+O prime	–M+O prime	–M–O primes	Target	+C+O prime	–C+O prime	–C–O primes
BID	Bade	Body	Free	GRID	Grade	Grave	Stone
BIND	Bound	Blend	Marsh	MIND	Mound	Mouth	Force
BREAK	Broke	Brick	Cloud	STEAK	Stoke	Stock	Crown
DIG	Dug	Dog	Pop	RIG	Rug	Rag	Pad
FALL	Fell	Full	Hope	TALL	Tell	Toll	Dome
FIGHT	Fought	Fright	Breeze	NIGHT	Nought	Naught	Hearth
FLEE	Fled	Flex	Hint	BEE	Bed	Bad	Car
FOOT	Feet	Fact	Weak	BOOT	Beet	Bait	Swim
GOOSE	Geese	Guise	Filth	CHOOSE	Cheese	Chaise	Sprout
LAY	Laid	Lake	Crop	PLAY	Plaid	Plain	Judge
LEND	Lent	Lens	Bark	TEND	Tent	Tens	Hash
LOUSE	Lice	Life	Ping	DOUSE	Dice	Duck	Sail
MAKE	Made	Male	Post	WAKE	Wade	Wage	Dust
MOUSE	Mice	Maze	Warn	SPOUSE	Spice	Spite	Tight
PAY	Paid	Pain	Feel	RAY	Raid	Rain	Boon
RUN	Ran	Ron	Top	PUN	Pan	Pen	Bog
SAY	Said	Same	Work	STAY	Staid	Stain	Quilt
SELL	Sold	Salt	Moon	BELL	Bold	Bolt	Dock
SHAKE	Shook	Shock	Touch	BAKE	Book	Bulk	Pint

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Appendix C (*continued*)

Genuine irregular				Pseudo-irregular			
Target	+M+O prime	–M+O prime	–M–O primes	Target	+C+O prime	–C+O prime	–C–O primes
SIT	Sat	Set	Cow	BIT	Bat	Bet	Tap
SLAY	Slew	Slip	Peek	DAY	Dew	Dip	Fig
SPEAK	Spoke	Space	Lunch	PEAK	Poke	Pace	Slow
SPIT	Spat	Spot	Dose	HIT	Hat	Hot	Low
STEAL	Stole	Stale	Wound	HEAL	Hole	Hale	Curt
STICK	Stuck	Stack	Rough	SICK	Suck	Sock	Tame
STRIKE	Struck	Stroke	Launch	BIKE	Buck	Back	Warm
SWEAR	Sworn	Swamp	Pinch	BEAR	Born	Bird	Kiss
THINK	Thought	Through	Breathe	DRINK	Drought	Draught	Scourge
WEAR	Worn	Wire	Sink	HEAR	Horn	Hurl	Fish
WIN	Won	Wan	Job	TIN	Ton	Ten	Hug

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