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NOTES AND COMMENT

Form and meaning in early morphological processing: Comment on Feldman, O'Connor, and Moscoso del Prado Martín (2009)

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Feldman, O'Connor, and Moscoso del Prado Martín (2009) reported evidence for differential priming of semantically transparent (talker–TALK) *and semantically opaque* (corner–CORN) *morphological pairs under masked presentation conditions. The present commentary argues that these data should not call into question the theory that morphologically structured words undergo a segmentation process based solely on form, because (1) these results do not contradict existing evidence for morpho-orthographic segmentation, (2) funnel plots suggest that the lack of priming observed for semantically opaque items in this study is inconsistent with findings in the existing literature, and (3) orthographic characteristics of the semantically opaque pairs in this study (rather than semantic factors) are the most likely explanation for these discrepant results.*

Behavioral evidence from repetition priming (Marslen-Wilson, Tyler, Waksler, & Older, 1994), frequency effects (Schreuder & Baayen, 1997), and eyetracking (Hyönä & Pollatsek, 1998) has converged in showing that morphologically complex words such as *departure* are recognized in terms of their constituent morphemes (i.e., {depart} + {ure}). Despite widespread agreement that word recognition involves the analysis of morphemic elements, it remains contentious whether morphemes contribute to lexical processing by virtue of their role in conveying the meanings of words (e.g., Marslen-Wilson et al., 1994; hereafter, *morpho-semantic decomposition*) or whether morphemic elements also have a privileged status at earlier levels of lexical processing involved in recognizing orthographic form (e.g., Taft, 1994; *morpho-orthographic decomposition*, after Rastle & Davis, 2008). Critical to this debate is whether morphemic analysis is confined to items (such as *departure*) in which the meaning of the whole form can be transparently derived from the combination of its constituent parts or also extends to semantically opaque morphemic items (such as *department*) in which there is no semantic relationship between the meaning of the whole word and the combined meanings of the constituent morphemes {depart} + {ment}.

The article by Feldman, O'Connor, and Moscoso del Prado Martín (2009; hereafter, FOM) presents evidence from masked priming that semantically opaque complex words both fail to prime their stems and elicit significantly less priming than do semantically transparent items. Similar results have been reported in a number of priming paradigms, including delayed repetition priming (Marslen-Wilson & Zhou, 1999), cross-modal priming (Marslen-Wilson et al., 1994), and long stimulus onset asynchrony (SOA) paired priming (Rastle, Davis, Marslen-Wilson, & Tyler, 2000). However, whereas these previous studies were characterized by overt presentation of complex words (and thus, adequate processing time for participants to access the meanings of prime words), the priming effects obtained by FOM were obtained using a masked visual-priming method usually thought to reflect the earliest form-based processing of written words (Forster & Davis, 1984). The semantic influences observed by FOM therefore

> call into question the autonomy of morpho-orthographic from morpho-semantic processing and the universality of the form-then-meaning assumption within models of word recognition. (p. 688)

This conclusion, in particular, runs contra to perhaps all accounts of the cognitive and neural stages involved in word identification, which propose that the initial stages of word recognition probed by visual masked priming are largely independent of word meaning (see, e.g., Dehaene, Cohen, Sigman, & Vinckier, 2005; Norris & Kinoshita, 2008). Must the form of written words be processed prior to the meanings of those words being accessed? Or is the meaning of written words processed concurrently with their orthographic form?

In this article, we consider whether the data presented by FOM merit rejecting form-then-meaning accounts of morphological processing (in particular) and word recognition (in general) by (1) reviewing prior empirical evidence that led to the proposal of form-based morpho-orthographic segmentation; (2) assessing whether the effects reported by FOM are consistent with this prior literature, using funnel plots (graphs of sample size against effect size) derived from a meta-analysis of masked morphological priming studies (Rastle & Davis, 2008); and (3) considering whether methodological aspects of FOM can explain their results.

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Evidence for Morpho-Orthographic Segmentation in Visual Word Recognition

Although a number of priming paradigms have demonstrated greater priming for semantically transparent than for matched semantically opaque pairs, there are few published demonstrations of semantic influences on short-SOA masked priming prior to FOM. For example, both Rastle et al. (2000) and Feldman, Soltano, Pastizzo, and Francis (2004) showed null effects of semantic transparency on stem priming with a short SOA (42 and 48 msec, respectively). The same items, however, show effects of semantic transparency in longer SOA priming (at 230- and 250-msec SOAs, respectively). However, this short-SOA null effect does not provide evidence that morphological structure contributes to visual word recognition, since priming effects might reflect orthographic overlap irrespective of morphological structure.

The first evidence that shared morphological units contribute to masked priming came from studies in Hebrew (Frost, Forster, & Deutsch, 1997) that showed greater priming for transparent and opaque derivations than for pairs with equivalent letter overlap but no shared root (see Boudelaa & Marslen-Wilson, 2005, for similar results in Arabic). Priming effects are determined by root letters and, hence, reflect the presence of shared morphological elements. However, it is unclear whether long-SOA priming and cross-modal priming show effects of semantic transparency on morphological priming in Semitic languages (Boudelaa & Marslen-Wilson, 2005; Frost, Deutsch, Gilboa, Tannenbaum, & Marslen-Wilson, 2000). These results could, therefore, reflect idiosyncratic properties of Hebrew or Arabic across many priming paradigms, rather than an initial morpho-orthographic segmentation process that is common to all languages.

An empirical resolution came from studies in French (Longtin, Segui, & Halle, 2003) and English (Rastle & Davis, 2003; Rastle, Davis, & New, 2004), which similarly dissociated the effects of morphological and orthographic overlap on masked priming with materials that showed semantic transparency effects under long-SOA priming conditions. Importantly, these studies added truly nonmorphological orthographic pairs such as *brothel*–BROTH as a third condition. These pairs have the same number of letters in common as opaque pairs like *whisker*–WHISK, but without morphemic structure, since the ending {-el} never functions as a derivational or inflectional affix in English. In these experiments (and in replications in Russian [Kazanina, Dukova-Zheleva, Geber, Kharlamov, & Tonciulescu, 2008] and English [Marslen-Wilson, Bozic, & Randall, 2008]), the presence of an orthographic ending that never functions as an affix significantly reduces the magnitude of masked priming: Items like *brother* prime their pseudostems (BROTH) more effectively than do equivalent items without morphemic affixes, such as *brothel*.

These priming effects therefore demonstrate a form of decomposition based only on the surface appearance of morphological structure. Although these findings were obtained under short-SOA conditions in which semantic influences on priming were weak or null, this positive effect of morphological surface structure does not require a null effect of semantic transparency. It is logically possible for repetition priming effects to reflect both morpho-orthographic and morpho-semantic decomposition in the same experiment. One illustration of how this might occur comes from fMRI and EEG experiments, which (arguably) show that neural correlates of both morpho-orthographic and morpho-semantic decomposition can co-occur in a single experiment, but in different brain regions (fMRI: Devlin, Jamison, Matthews, & Gonnerman, 2004; Gold & Rastle, 2007) or different time windows (EEG: Lavric, Clapp, & Rastle, 2007; Morris, Frank, Grainger, & Holcomb, 2007). By analogy, then, both semantic transparency and orthographic morphological structure might modulate masked visual priming in a single set of behavioral data.

We therefore argue that the significant effect of semantic transparency on masked morphological priming reported by FOM does not entail rejecting the theory that morphologically complex words undergo a segmentation process based solely on form. Since FOM did not compare priming effects for opaque morphological (*corner–corn*) and nonmorphological (*brothel–broth*) form pairs, their results are irrelevant to the question of whether morphemic and nonmorphemic elements are processed differently during early stages of visual word recognition. Although it is striking that priming effects for opaque items were small and statistically nonsignificant in FOM, this null result is insufficient evidence for rejecting morpho-orthographic segmentation. Even accepting FOM's demonstration of semantic influences on masked priming at face value, the balance of evidence suggests that orthographic factors are more important than semantic factors in producing masked morphological priming. We therefore conclude that morphological processing (like other aspects of visual word recognition) is achieved by initial processing of orthographic form, followed by access to semantic properties. In the remainder of this commentary, we consider how best to interpret the results of FOM in the context of the existing empirical literature.

Reconciling Evidence for Morpho-Orthographic and Morpho-Semantic Effects

In psycholinguistics, as in other sciences, contradictory empirical observations can be difficult to interpret. It is tempting to believe that some unusual characteristics of the studies concerned can explain the discrepant results. However, given the variability of behavioral data, no specific explanation may be necessary; a single aberrant data set might be expected, given intrinsic variation in the normal population of participants and items and the possibility of 1 in 20 null data sets delivering a statistically significant difference at $p < .05$. Although this variability is challenging for psycholinguistics, it is even more critical for clinical and medical studies, where assessing evidence for the likelihood of different outcomes can be a grave matter. Here, we apply funnel plots—a graphical technique for depicting multiple data sets commonly used in the meta-analysis of clinical trials—to the meta-analysis of morphological effects in masked priming introduced by Rastle and Davis (2008) and revisited by

FOM. For a comprehensive introduction to funnel plots, we direct the reader to Light and Pillemer (1984) or, for a shorter review, Sterne and Harbord (2004). A more accessible and entertaining presentation can be found in Goldacre (2008).

A funnel plot represents two aspects of each study in a meta-analysis: (1) the effect size, plotted on the *x*-axis, and (2) the precision or accuracy of the effect size estimate, plotted on the *y*-axis. Less precise studies produce a range of effect sizes, with data points spread along the *x*-axis. Studies that report higher quality data (typically due to larger sample sizes) will be further away from the *x*-axis and will converge on a more accurate estimate of the true effect size. Scatterplots of precision against effect size typically produce a triangular or inverted funnel shape, pointing toward an estimate of the true effect size. We favor funnel plots over the scatterplots of response time (RT) in the related and unrelated priming conditions presented in Figure 1 of FOM for two reasons. First, they offer a direct depiction of the dependent variable of interest (i.e., the magnitudes and/or differences in priming effects), instead of representing this information indirectly as the vertical distance of any study from a diagonal identity line. The unrelated RTs on the *x*-axis of the FOM plots are correlated with priming due to mathematical coupling (Oldham, 1962) and do not provide any additional explanation of differences in the magnitude of priming. A further advantage of funnel plots is that they guide the reader toward those studies that provide more reliable evidence by virtue of collecting larger quantities of data. All other things being equal, discrepant observations are more likely to come from small studies that inaccurately estimate the underlying population mean (Sterne & Harbord, 2004). Conversely, larger studies presenting discrepant findings have clearer methodological or theoretical implications.

The effect size for masked morphological priming studies is the magnitude or differences between priming effects. We focus here on the size of these effects in milliseconds, which is independent of the number of observations or statistical significance and, hence, can be readily combined over studies with different numbers of participants and items.

The precision of priming estimates is more difficult to compute but is directly related to the number of participants and items tested (i.e., sample size) and inversely related to the amount of variability among these participants and items. These two measures are combined in the standard error of the magnitude of priming. Whereas some authors strongly argue for the use of standard error, rather than sample size, in funnel plots (Sterne & Egger, 2001), others describe circumstances in which sample size is the favored measure (Peters, Sutton, Jones, Abrams, & Rushton, 2006). These discussions, however, concern clinical data with binary outcome measures, for which a dependent measure (log odds ratio) and standard error can be computed directly from the data reported in studies. In contrast, an appropriate standard error measure is impossible to derive from published reports of psycholinguistic studies, for two reasons. First, the standard error over participants that is most often reported in studies is irrel-

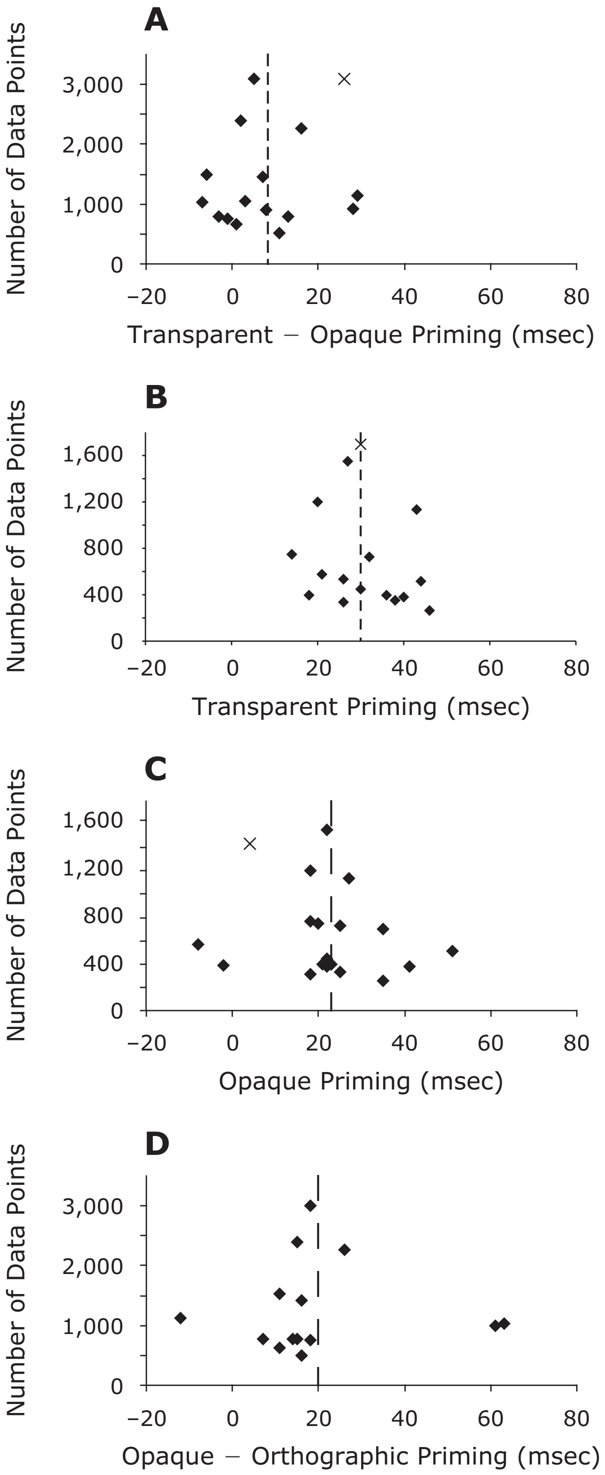

Figure 1. (A) Funnel plot showing the difference between transparent and opaque priming as a function of the total number of data points entered into this comparison. Data from studies included in Rastle and Davis (2008) are shown as solid diamonds, with the vertical broken line indicating the mean over all studies. Data from Feldman, O'Connor, and Moscoso del Prado Martín (2009) are shown as X. (B) Funnel plot showing the absolute magnitude of transparent priming, plotted as before. (C) Funnel plot showing the absolute magnitude of opaque priming. (D) Funnel plot showing the difference between opaque and orthographic priming, which was not tested by Feldman et al. (2009).

evant for assessing priming in typical repeated measures designs.[1] Second, in any psycholinguistic study, there are two sources of variability that are relevant when statistical significance is assessed (derived from analysis by participants and items, respectively). These two sources of variance cannot be accurately combined without access to the original data, which is impractical for a meta-analysis. For this reason, we used sample size, rather than standard error, in our funnel plots, reporting the product of the numbers of participants and items included in the analysis, after exclusion of error-prone participants and items, divided by the number of experimental versions in the experiment.[2] In Figures 1A–1D, we depict the results of four critical comparisons from a meta-analysis of masked morphological priming (Rastle & Davis, 2008).

Figure 1A plots the difference between transparent and opaque morphological priming (*talker*–TALK vs. *corner*–CORN; cf. FOM) and clearly shows the triangular shape expected. Smaller studies with fewer than 1,000 data points show a range of outcomes, with differences between 29 and −7 msec in the magnitude of priming. The top of the triangle converges on an effect size close to the mean over studies: approximately 7 msec greater priming for transparent than for opaque items. However, the findings of FOM differ from those in previous reports in showing a 26-msec transparency effect, an effect size that has been reported only in two small studies by Diependaele, Sandra, and Grainger (2005), which introduced several methodological innovations, such as using a longer SOA, a backward mask, written and spoken targets in the same experiment, and three repetitions of each prime–target pair, one of which may have been partially visible (67-msec exposure duration). In contrast, the FOM study used a more conventional design but collected 2,000 more data points. All other things being equal, FOM should have observed an effect size more similar to the mean value. These observations do not invalidate FOM's findings; however, they do challenge FOM's statement that "the present data are nearly prototypical of the published literature" (p. 688). The funnel plot in Figure 1A shows this statement to be inaccurate. The difference between transparent and opaque morphological priming reported by FOM is 1.53 standard deviations greater than the mean in previous studies, or 3.12 standard deviations greater if the Diependaele et al. studies are excluded on methodological grounds. Hence, we would argue that the FOM result is both unexpected, given the quantity of data collected, and clearly different from the results in the majority of the existing literature.

A funnel plot showing the magnitude of transparent and opaque priming (Figures 1B and 1C, respectively) confirms that FOM reported a typical magnitude of transparent priming but reduced opaque priming. Smaller, less precise studies have reported between 51 and −8 msec of facilitation for opaque pairs, whereas larger experiments have shown over 20 msec of consistent and reliable priming (cf. mean priming of 23 msec in the Rastle & Davis, 2008, meta-analysis). The findings reported by FOM are exceptional, not only due to the small absolute magnitude of opaque priming (Diepiendale et al., 2005, reported similarly weak priming), but more by the failure to observe significant priming despite a large number of observations. We will argue that methodological differences between FOM and previous work provide the best explanation for this discrepancy.

To assess evidence for morpho-orthographic segmentation in masked priming, Figure 1D shows the comparison of opaque morphological and orthographic priming. The spread of priming values observed in less precise studies is even broader than before (a 63- to −12-msec difference), yet once again, the exit of the funnel converges on the average effect size in the literature: Approximately 20 msec of additional priming is observed for item pairs like *corner*–CORN, as compared with matched items without an affix ending, such as *brothel*–BROTH. Thus, there is consistent evidence in support of morpho-orthographic segmentation at the stages of visual word recognition tapped by masked priming. Furthermore, masked morphological priming studies have shown a 20-msec influence of form-based morphological structure, as compared with a 7-msec influence of semantic factors. This nearly threefold difference is statistically reliable [$t(28) = 2.37$, $p < .05$], confirming that morpho-orthographic influences on masked priming are significantly larger than morpho-semantic influences and, hence, that morphological decomposition is based initially on orthographic analysis (cf. Rastle & Davis, 2008).

Do the Results Reported by FOM Provide Evidence of Semantic Influences on Masked Morphological Priming?

How then can we explain (1) the unexpectedly substantial (26-msec) difference between transparent and opaque priming and (2) the unexpectedly weak effect for opaque priming (4 msec) reported by FOM? FOM present a number of methodological differences between their work and previous work that are potentially responsible for this outcome. However, many of these seem likely to change average RTs or the overall magnitude of priming, without differentially affecting transparent and opaque items. For instance, FOM used shorter targets with denser orthographic neighborhoods, which may reduce mean lexical decision latencies (Andrews, 1989) and may reduce the overall amount of priming (Forster, Davis, Schoknecht, & Carter, 1987). Similarly, FOM included large numbers of identity-priming items as fillers, which might decrease mean response latencies, since more items are primed, but conversely, may increase the overall magnitude of priming (see Bodner & Masson, 2003). In the absence of specific empirical evidence, however, it seems unlikely that either of these manipulations should produce a *difference* between the magnitude of priming in transparent and opaque conditions.

Rather, we believe that factors that were unmatched between the transparent and the opaque item pairs are more likely to have been responsible for the effects observed by FOM. Of course, the most significant difference between the two sets was in semantic relatedness; however, form-based factors may also have influenced the magnitude of priming observed. For example, FOM reported a marginally significant difference in target family size with larger families in the transparent than in the opaque condition. Family size has long been known to facilitate lexical decision (Schreuder & Baayen, 1997), and in recent work, we showed enhanced masked morpho-orthographic priming for stems with larger family sizes (McCormick, Rastle, & Davis, 2009). Thus, the family size confound in the FOM items is a nonsemantic factor that would increase differential priming for transparent and opaque items in the observed direction. Previous studies that reported null effects of semantic transparency used either transparent and opaque materials matched for family size (Rastle et al., 2004) or a repeated-stem design that removed this confound (Feldman et al., 2004; Rastle & Davis, 2003).

The more worrying problem with FOM's materials concerns the presence of important orthographic differences between the transparent and opaque pairs. In particular, their opaque condition includes a substantial number of prime–target pairs characterized by nonsystematic, arbitrary orthographic changes:

bliss–blistery (swapping an *s* for a *t*)
cell–celery (removing a double *l*)
coin–coyness (swapping *i* for *y*)
cute–cuttable (swapping *e* for *t*)
harp–harness (delete a final *p*)
relay–relation (swapping *y* for *t*)
sack–saccade (swapping *k* for *c*)

Although the authors stated that "instances of spelling or sound mismatch (*huskiness*–HUSK) were equated across the semantically transparent and opaque stems" (p. 687), an inspection of the transparent item pairs reveals a substantially different set of orthographic alterations:

bury–burial (swapping stem-final *y* for *i*)
bride–bridal (deletion of a stem-final *e*)
dim–dimmish (final consonant reduplication)
bake–bakery (a stem-final *e* is shared with the affix -*ery*)

Unlike the orthographic alterations found in FOM's opaque items, these four orthographic changes occur systematically in a large number of morphemic combinations (e.g., *y*-deletion occurs not just for the stem *bury*, but also for *copy*, *defy*, *dry*, *icy*, *jury*, and *ply*). Previous studies have demonstrated that the magnitude of masked morphological priming is unaffected by three of the four regular orthographic alterations found in FOM's transparent items (McCormick, Rastle, & Davis, 2008; *y–i* alternations were not tested in this study). In contrast, this same study demonstrated that the kinds of nonsystematic, arbitrary orthographic changes found in FOM's opaque items block morpho-orthographic priming. The key point is that a word like *blistery* could never be derived from the stem *bliss*, since word-final *s* never changes to a *t* in English morphology. For this reason, McCormick et al. (2008) considered these kinds of items to be nonmorphological form controls for which 0 msec of priming was observed. McCormick et al.'s (2008) results therefore show a discrepancy between tolerance of morphologically governed changes and intolerance of other arbitrary orthographic changes. This is an aspect of morpho-orthographic segmentation, since semantically opaque pairs with regular orthographic changes (like *fetish*–FETE) are processed in the same way as equivalent transparent pairs, such as *bridal*–BRIDE (McCormick et al., 2008). We would argue, then, that the failure to find priming for pairs like *blistery*–BLISS in the FOM study is a predictable consequence of the computational properties of morpho-orthographic processing demonstrated by McCormick et al. (2008, 2009). We conclude, then, that the presence and nature of orthographic changes is not matched between transparent and opaque items in FOM's materials and, further, that this difference would be expected to suppress opaque priming.

Four additional pairs in FOM's opaque materials give some cause for concern:

bee–beery
earl–earless
husk–huskiness
pit–pitiless

In these items, morpho-orthographic decomposition yields ambiguous results, since there are multiple possible affixes and stems present. For instance, removing the affix *y* from *beery* yields the stem *beer*, not the target *bee*; removing the affix *less* from *earless* leaves *ear*, not *earl*; and segmenting *huskiness* produces *huski/y*, rather than *husk*. Cross-modal priming studies (de Almeida & Libben, 2005) have suggested ordering constraints that block priming of certain constituents of double-affixed items such as *unlockable* (*unlock+able*, or *un+lockable*). It therefore seems possible that segmentation ambiguities such as in *beery* could reduce the magnitude of priming of the potential stem BEE. Although further experimental work is required to test the impact of these segmentation ambiguities on masked morphological priming, we note that pairs of this nature were included only in the opaque condition of FOM, once again raising the possibility that the large transparency effect observed arose due to uncontrolled aspects of the materials.

Thus, we believe that it is premature to conclude from FOM's data that there are true semantic influences on masked morphological priming. We have documented an unfortunate combination of factors—differences in family size, nonmorphological orthographic changes, and segmentation ambiguity—all of which seem likely to reduce the magnitude of priming specifically in the opaque condition. Further data will be needed to ensure that these confounds are not responsible for the results obtained.

Conclusions

In the present commentary, we have sought to interpret the surprising data presented by FOM concerning semantic influences on masked morphological priming. We believe that funnel plots are a useful method for assessing findings such as these, since they highlight the size of each study, as well as the outcome. All other things being equal, smaller studies are more likely to give discrepant answers than are larger studies. Existing theoretical accounts are, therefore, most strongly challenged by large studies (such as FOM) that give results at odds with those in the existing literature. In such circumstances, then, it is prudent to consider whether methodological differences or confounding factors provide an alternative explanation of the data. We believe that in the present case, other nonsemantic factors provide the best explanation of the results obtained by FOM.

To conclude, we are not persuaded that the data presented by FOM provide evidence for challenging form-then-meaning accounts of morphological processing (in particular) or visual word recognition (in general). Nonsemantic factors might yet explain both the lack of priming for opaque pairs and the additional priming for semantically transparent items in their study. Although the meta-analysis reported by Rastle and Davis (2008) and reanalyzed by FOM provides some support for a semantic transparency effect in masked morphological priming, this 7-msec effect is carried primarily by less precise studies and is dwarfed by a significantly larger and more consistent 20-msec difference between priming for morphological-structured opaque and nonmorphological form pairs. It is this finding of significantly greater priming for pairs like *brother*–BROTH than for *brothel*–BROTH that we believe demonstrates that early visual processing of written words is initially concerned with extracting orthographic morphological elements (such as the affix *-er*). Hence, the present evidence would suggest that this initial orthographic processing stage operates largely independently of semantics. It will be for future studies to determine under what circumstances semantic influences on decomposition are enhanced (as in overt priming studies) and, hence, whether these semantic effects arise through top-down influences or through distinct representations at a later, morpho-semantic processing stage.

AUTHOR NOTE

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NOTES

1. It is common practice in experimental psychology to report the standard error of the mean over participants. However, for repeated measures priming studies, the standard error of the mean conveys no information concerning the statistical significance or otherwise of priming (see Loftus & Masson, 1994, for a discussion of confidence intervals and standard errors for repeated measures designs).

2. This slightly overestimates the total number of data points included in the analysis, since lexical decision errors (typically, $<5\%$) will be included.

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