The importance of awareness for understanding language.

---- In Press at *Journal of Experimental Psychology: General* ----

Hugh Rabagliati, Alexander Robertson, David Carmel

School of Philosophy, Psychology and Language Sciences, University of Edinburgh

Correspondence:
Hugh Rabagliati, School of Philosophy, Psychology and Language Sciences, University of Edinburgh, EH8 9JZ.
hugh.rabagliati@ed.ac.uk

All experimental presentation scripts, data and analyses can be found at
https://github.com/hughrabagliati/CFS_Compositionality

Is consciousness required for high level cognitive processes, or can the unconscious mind perform tasks that are as complex and difficult as, for example, understanding a sentence? Recent work has argued that, yes, the unconscious mind can: Sklar et al. (2012) found that sentences, masked from consciousness using the technique of continuous flash suppression (CFS), broke into awareness more rapidly when their meanings were more unusual or more emotionally negative, even though processing the sentences' meaning required unconsciously combining each word's meaning. This has motivated the important claim that consciousness plays little-to-no functional role in high-level cognitive operations. Here, we aimed to replicate and extend these findings, but instead, across 10 high-powered studies, we found no evidence that the meaning of a phrase or word could be understood without awareness. We did, however, consistently find evidence that low-level perceptual features, such as sentence length and familiarity of alphabet, could be processed unconsciously. Our null findings for sentence processing are corroborated by a meta-analysis that aggregates our studies with the prior literature. We offer a potential explanation for prior positive results through a set of computational simulations, which show how the distributional characteristics of this type of CFS data, in particular its skew and heavy tail, can cause an elevated level of false positive results when common data exclusion criteria are applied. Our findings thus have practical implication for analyzing such data. More importantly, they suggest that consciousness may well be required for high-level cognitive tasks such as understanding language.

Keywords
consciousness; language; semantics; continuous flash suppression; replication

Language and awareness are inextricably linked. We can only talk about things we are aware of, and it would seem paradoxical to say that we can understand a sentence without being aware of what it means. But the precise role played by awareness in understanding language is unclear. While there has been a long history of investigating whether consciousness is critical for extracting the shape, sound or meanings of individual words (for review see Kouider & Dehaene, 2007), that work has only rarely examined whether or how consciousness influences the processes by which word meanings are combined into phrases (Draine, 1997; Marcel, 1980). Indeed, many prominent theories of consciousness, such as global workspace theory (Baars, 1997, 2005; Dehaene & Naccache, 2001), have implicitly assumed that combinatorial sentence processing demands awareness, because it relies heavily on two things which have been proposed to be diminished without awareness: the use of complex working memory operations (e.g., to bind together the meanings of distant words, Daneman & Carpenter, 1980) and world knowledge (e.g., to interpret the meanings of vague or ambiguous words, Hobbs, Stickel, Martin, & Edwards, 1988).

Against the background of this conservative interpretation, a prominent recent body of work has argued that consciousness is not, in fact, required for high level cognitive operations such as understanding sentences. According to the so-called 'Yes It Can' principle (Hassin, 2013), "unconscious processes [are able to] perform the same fundamental, high-level functions that conscious processes can perform" (p.195), as evidenced by experiments that use a technique called continuous flash suppression. Continuous Flash Suppression, or CFS, is a form of binocular rivalry, in which a monocularly-presented target stimulus is masked from awareness by presenting a dynamic high-contrast mask to the other eye (Tsuchiya & Koch, 2005), an effect that can last from a few hundred milliseconds to tens of seconds. The time it takes a stimulus to "break through" suppression is known to depend on unconscious processing of lower-level visual properties (e.g., larger or noisier stimuli break through suppression faster (Carmel, Arcaro, Kastner, & Hasson, 2010; Gray, Adams, Hedger, Newton, & Garner, 2013; Sterzer, Stein, Ludwig, Rothkirch, & Hesselmann, 2014; Tsuchiya & Koch, 2005; E. Yang & Blake, 2012). Moreover, this technique of "breaking CFS" (b-CFS) – explicitly measuring time to breakthrough – has provided some evidence that high level properties may also be processed without awareness (cf. Stein & Sterzer, 2014), for instance, negative facial expressions break through faster than neutral ones (e.g., E. Yang, Zald, & Blake, 2007). However, the high-level features in such work have not been symbolic in nature.

Excitingly, Sklar and colleagues found that the time taken for a masked sentence to “break” through suppression was also affected by a property that is both high level and symbolic: its meaning (Sklar, Levy, Goldstein, Mandel, Maril & Hassin, 2012). Masked sentences with unusual meanings (*I ironed the coffee*) were faster to break through suppression than control sentences (*I drank coffee*), while phrases that had negative emotional valence (e.g., *electric chair*) broke through suppression faster than neutral phrases (*dining table*), even though each word in the negative phrase was itself unvalenced (requiring combinatorial processing of word sequences for valence to be parsed).

These results – alongside the same paper’s additional demonstrations that certain arithmetic operations can be carried out without awareness – provide an important challenge to current theories of how awareness and high-level cognition inter-relate. For instance, they suggest that awareness might play a much more limited role in high-level processing than previously assumed, or that our assumptions about language processing are incorrect, and that working memory and world knowledge are not required to understand sentences.

However, before making such large changes to theories of consciousness or language, it is important to be confident that these results are robust, that is to say, that they are replicable, that they generalize across a variety of environmental conditions and situations, and that they cannot be accounted for by simpler explanations. For example, one concern about experiments of this type is the difficulty of fully disentangling a sentence’s high level properties (e.g., its meaning) from its low level properties (e.g., its shape and form, c.f., Farmer, Christiansen, & Monaghan, 2006). While Sklar and colleagues made efforts to rule out lower level explanations of their results, doing so is difficult because sentences with different meanings necessarily have different forms.

A second concern, perhaps more important, is that Sklar et al’s results run counter to other findings about semantic access to word meanings during continuous flash suppression. For example, they contrast with an older literature on binocular rivalry which had concluded that high-level conceptual or semantic processing was diminished for suppressed stimuli (Zimba & Blake, 1983). More recently, using CFS, Kang, Blake and Woodman (2011) found no evidence that the meanings of individual words were accessed when they were suppressed (although see Heyman and Moors, 2012, for a critique of that procedure), while Yang and Yeh (2011) found that emotionally negative Chinese words were in fact *slower* to break suppression than neutral words, a finding in the opposite direction to the effect found by Sklar and colleagues.[1] Recent work on statistical inference and measurement has emphasized that such inconsistent results might be expected when experiments with low statistical power are used to test for small or null effects (Gelman & Carlin, 2014), and these worries are particularly marked in this instance given concerns that data from the b-CFS method are potentially very noisy (e.g., breaking times often have a very long right tail, Moors, Stein, Wagemans, & van Ee, 2015; Stein, Hebart, & Sterzer, 2011).

A final concern relates to data analysis strategies. Shanks (2016) has argued that many reported findings from the field of unconscious cognition are likely to be false positives, caused by common practices for excluding data or participants. As one example, participants in these studies are often excluded if they pass an awareness test (demonstrating that suppression from awareness had failed). But if the awareness test is a noisy measure, and if performance on the awareness test is correlated with performance on the critical task, then regression to the mean implies that those participants who receive extremely high scores on the awareness test will likely have less extreme scores on the critical task and, importantly, vice versa, i.e., seemingly "unaware" participants should likely have more extreme scores on the critical task. Indeed, Shanks showed that Sklar et al.'s demonstration of unconscious arithmetic processing might be explainable this way. While Sklar et al. found priming effects for participants who did not show awareness during their task, a reanalysis showed that priming effects were in fact smaller for participants who *did* show awareness, consistent with regression to the mean. Building on this, Moors and Hesselmann (in press) provide additional reanlyses of that dataset which suggest the evidence for unconscious arithmetic is only equivocal.

Given the potential theoretical importance of Sklar et al's results, but also these concerns about the replicability of their findings, we decided to conduct a series of highly powered partial replications and extensions of their experiments. Our replications were high powered

[1] Other studies also indicate that breaking suppression times are not sensitive to some non-semantic lexical factors such as word frequency (Heyman & Moors, 2014).

in that we used a considerably larger number of participants and stimuli than in prior work, but we label them as "partial" because the original experiments were conducted in Hebrew, whereas ours were conducted in English. In Studies 1 and 2 we test whether anomalous phrases (*I ironed the coffee*) break suppression faster than control phrases (*I ironed the clothes*), using English translations of the original sentences as well as novel sentences that better control for low-level visual properties. In Studies 3 and 4 we test whether emotionally valenced phrases (*electric chair*) break suppression faster, again using both translated stimuli and novel stimuli designed to control for low-level factors. Study 5 tests whether low level visual properties of the original Hebrew might offer an alternative explanation of the original findings. Studies 6 through 9 replicate Studies 1 through 4, but using the same stimulus presentation scripts used in Sklar et al (2012). Finally, Study 10 is an English language test of Yang and Yeh's (2011) finding that emotionally negative single words break suppression more slowly than neutral words.

To preview, across our experiments we find no evidence that participants process the combinatorial semantics of suppressed sentences, or even the semantics of a single suppressed word. We do find that the time taken for a sentence to break suppression is influenced by certain visual factors, such as the physical length of the sentence, the luminance contrast between the sentence and its background, and the participant's familiarity with the writing system (English/Hebrew, see Jiang, Costello, & He, 2007), but we find no evidence of unconscious combinatorial semantic processing. We offer an explanation for this failure to replicate through a set of statistical simulations, which show that the type of data produced in experiments using the breaking continuous flash suppression method, with high variability and a heavy right tail, generate an increased rate of false positives when combined with data exclusion practices that are commonly used in psychology.

As we lay out in the General Discussion, these results thus have implications for both theory and practice. They provide evidence against the hypothesis that language processing occurs without awareness, and they motivate an approach to statistical analysis that pays more attention to the distributional form of the data.

Studies 1 and 2

Sklar et al. (2012)'s Experiment 1 found that anomalous sentences (*I ironed the coffee*) broke suppression faster than neutral sentences (*I ironed the clothes*). In our Study 1, we attempted a higher-powered English language replication of this finding, using 53 participants rather than the original 32. Study 2 was an extension of that experiment: we attempted to test whether effects of unconscious semantic processing might be found when we better-controlled for lower-level visual and lexical properties of the stimuli. In particular, we contrasted neutral sentences (*Mike ate the steak*) with reversed sentences that were anomalous (*the steak ate Mike*), i.e., we held the words constant while drastically changing the sentence's meaning.

Although we report Studies 1 and 2 separately, the data were collected in the same testing session. Since the parameters of the two studies were identical, their trials were randomly intermingled.

Study 1

Method

Participants

Fifty-three members of the University of Edinburgh community (39 female, mean age 21 years, range 18-41) participated in the study, and were paid £8 an hour. All identified English as their native language from birth and had normal or corrected vision with no colour blindness. This number of participants, combined with the 105 items used (with different items used between conditions), gave us 80% statistical power to detect an effect of size 0.39 or larger based on the procedure described in Westfall, Kenny, and Judd (2014). When we conducted this experiment we were not able to calculate an effect size from the original experimental report, as we did not know the correlation between participants' response speeds in the two conditions. Whilst writing this paper we received the original data, and calculated the original effect size to be only 0.11. Westfall et al's (2014) procedure indicates that it is not possible to achieve 80% power to detect such a small effect using this experimental design, even with an infinite number of participants.

Apparatus

Stimuli were presented on a 19" CRT monitor in a dimly lit room, connected to a computer running PsychoPy2 software (Peirce, 2007). A chin rest and mirror stereoscope were positioned 57cm from the monitor, with a vertical divider splitting the display so that each eye only saw half of the screen.

Procedure

Each trial (Figure 1) began with a fixation cross, presented binocularly at the center of each eye's visual field between two textured vergence bars, allowing participants to reach stable binocular vergence. After 2000ms, the fixation cross remained super-imposed on both screens, but a changing Mondrian mask was presented to one eye. 200ms later a sentence was presented to the other eye, positioned slightly above or below the fixation cross, and continuously ramping up in contrast from 0% to 50% over 700ms. The visual mask consisted of a field of squares which randomly changed in size, colour, contrast, rotation and position at a rate of 60Hz (we had intended to use Sklar et al's original presentation rate of 10Hz, but a technical error caused the changes to occur at the screen's refresh rate; Studies 6 through 10, in which we used a 10Hz rate, show that this error does not explain the divergence between our findings and those of Sklar et al.).

Sentences were presented in random order; sentence position (above or below fixation) and the eye it was presented to varied randomly between trials. Participants were instructed to focus on the fixation cross with both eyes open, without blinking or looking around, and to quickly press the "up" arrow key if they detected text above the cross, or the "down" arrow key for text below the cross. If the participant did not respond within 8s, the trial timed out. This time-out was not included in Sklar et al.'s original method, but we did not think it was likely to greatly affect our results, as average by-condition response times in their experiments were less than one second. As such, response times as long as 8s would be considered outliers.

After participants made their detection report, they also reported their subjective experience of the trial. Using a modified version of the perceptual awareness scale (Sandberg, Timmermans, Overgaard, & Cleeremans, 2010), participants rated the clarity with which they had experienced the lexical stimulus, choosing from the levels 'No Text', 'Blurry Text', 'Almost Clear Text' and 'Absolutely Clear Text' (which were assigned numerical values of 0 to 3, respectively, in our analysis).

Before beginning the experiment, participants completed five training trials to ensure that the stereoscope was properly calibrated and that they understood the task. Participants were given the opportunity to pause after every 75 trials.

All of the studies reported here were approved by the Psychology Research Ethics Committee of the University of Edinburgh.

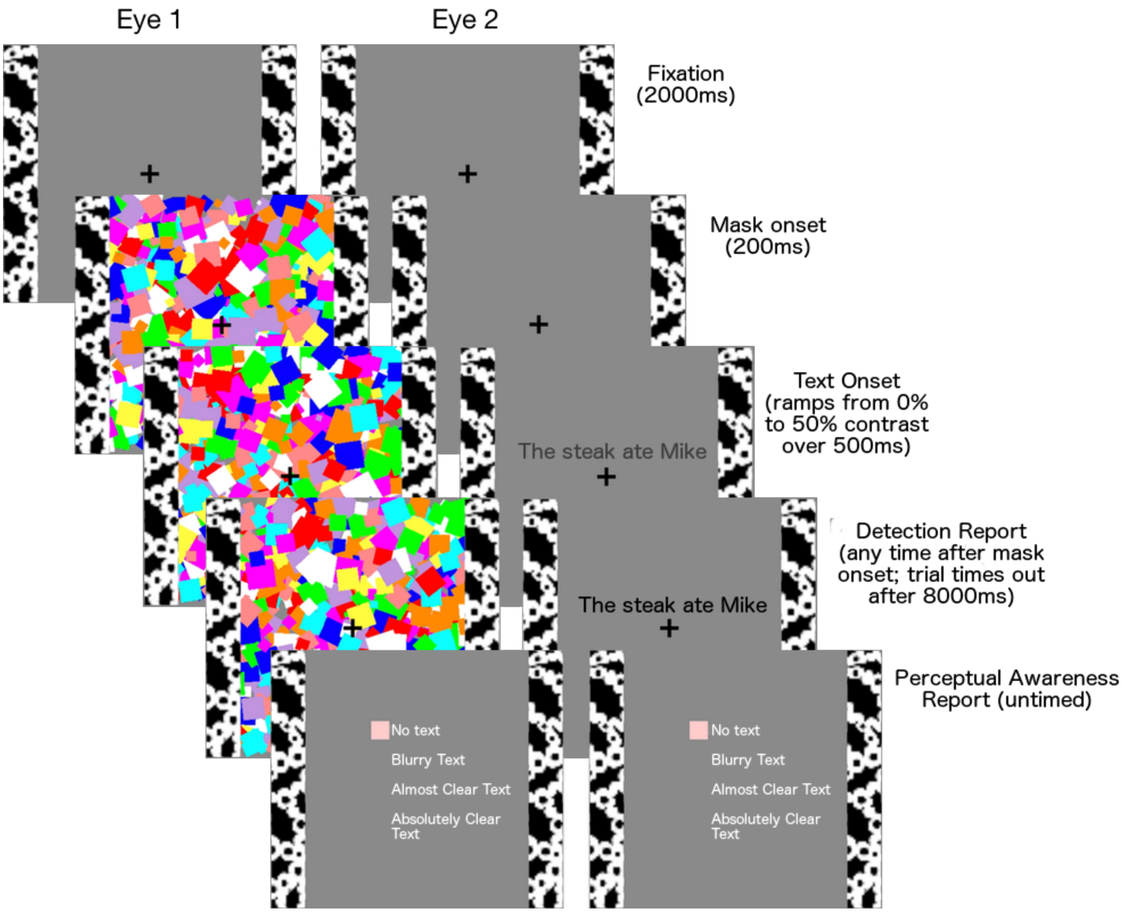

Figure 1. Schematic of the procedure for Studies 1 through 5. Text contrast is enhanced for clarity of presentation. On each trial, we randomly assigned which eye would receive the mask or stimulus.

Materials

Stimuli were English translations of the Hebrew expressions used in Sklar et al's Experiment 1, provided in the appendix to that paper. These consisted of 34 critical Violation sentences, in which an animate actor performed an implausible action on an object (e.g. *I ironed the coffee*); 68 Control sentences, in which the action or object from the Violation condition was used in a sensible way (e.g. *I made the coffee, I ironed the clothes*); and 34 semantically felicitous Filler sentences (e.g. *I washed the cup*). Each stimulus was presented once. A full list of stimuli is presented in the appendix.

Analyses

We first removed all trials that timed out (median = 0 trials per participant [range = 0 - 44]). We then followed the complex set of criteria that Sklar and colleagues used for excluding participants and trials from analysis. First, participants were excluded if their sentence localization accuracy was below 90%. Second, participants were excluded if their mean response time was greater than 3 standard deviations away from the grand mean of all participants. Third, trials were excluded if they were answered incorrectly or timed out. Fourth, trials were excluded for each participant if their response time was greater than 3 standard deviations away from the participant's grand mean. Fifth, trials were excluded for each condition if their response time was greater than 3 standard deviations from the condition's grand mean (across participants). Finally, trials were excluded as anticipatory if their response time was less than 200ms. In total, 5 participants were excluded along with 6% of the remaining trials (median = 5 trials per participant [range=1 - 20]). With these exclusions, we still had 80% power to detect an effect of size 0.4.

We then analyzed the resulting data in three different ways. First, we replicated the original analysis by Sklar and colleagues, comparing mean raw response times to the different sentence types (Violation/Control) using paired *t*-tests.

Second, because visual inspection of the response times prior to exclusions suggested that they had severe positive skew (estimated at 2.5 using the method of moments), we carried out the same analysis on log transformed response times, which reduced skew to 0.9 (for this analysis we re-ran the exclusion criteria on the log-transformed data).

Finally, we conducted a mixed effects regression analysis on the log transformed data. Mixed effects analyses (Baayen, Davidson, & Bates, 2008; Gelman & Hill, 2007) are an extension of standard regression analyses, that are designed for modeling datasets that can be decomposed into different subgroups, such that regression coefficients might vary to account for these different subgroups; the resulting coefficient estimates in the regression are thus a mixture of fixed effects, that apply generally across the dataset, and random effects that modulate the fixed effects within each subgroup. For example, in the present dataset, a mixed effects regression analysis can be used to model variation in the dataset such as whether each participant's response times will have a consistent bias away from the mean, whether each different item's response times will have a consistent bias away from the mean, and whether the effect of sentence type (i.e., the difference in response speed for violation and control sentences) will vary from participant to participant. By contrast, more traditional analyses are unable to simultaneously account for variation across multiple subgroups (like participants and items).

As well as being able to model important variance within datasets, an important advantage of mixed effects models is that they can account for experimental designs that contain unbalanced data, a property that is particularly important here (and in Sklar et al., 2012) because the design used twice as many Control sentences as Violation sentences. In addition, because mixed effects regressions are an extension of multiple regression, they allow us to simultaneously assess the effect of sentence type while accounting for control variables, such as sentence length (longer sentences may break suppression faster because they are composed of more pixels, and the overall probability of a stimulus breaking suppression is a weighted sum of the probability of each pixel breaking suppression).

In the remainder of this paper, we describe the structure of our mixed effects regression using the syntax designed for lme4 regressions (Bates, Mächler, Bolker, & Walker, 2015) in the statistical analysis software package R. For Study 1 our regression had the structure RT ~ 1 + Sentence Type + Sentence Length+ (1+Sentence Type|Participant) + (1|Item), where the dependent variable (response time, RT) precedes the tilde, fixed effects immediately follow the tilde, and random effects are placed in brackets. Our model of response times was thus composed of a fixed effect intercept term, fixed effects of Sentence Type (Violation/Control sentences) and of Sentence Length (in number of characters), random by-participant adjustments to the intercept and the effect of Sentence Type, and random by-item adjustments to the intercept. In plain English, this means that we tested whether there was a significant effect of Sentence Type on response times, while accounting for how response times vary by Sentence Length, for how response times vary across different items, for how response times vary across different participants, and for how the effect of Sentence Type might vary across those participants. In this analysis, and all other mixed effects analyses, all predictor variables were centered (factorial predictors used contrast coding) and continuous predictor variables (e.g., sentence length) were standardized by one standard deviation. We calculated p values for predictors in the mixed effects models by approximating the t distribution with the z distribution.

Finally, we also analyzed whether participants gave different ratings on the Perceptual Awareness Scale depending on the experimental condition.

Results

When the raw response time data were analysed using a paired *t*-test, we found a marginal effect of sentence type ($t(47) = 1.79$, p=.08). However, as can be seen in Figure 2, this effect was in the opposite direction to that found by Sklar et al. (2012): In the original study, Violation sentences were faster to break suppression than Controls, but we found that Violation sentences were marginally *slower* to break suppression ($M_{control}$=1466ms (SD=505), $M_{violation}$=1501ms (566)).

However, when we analyzed log-transformed response times we no longer found even a marginal effect of sentence type ($t(47) = 1.2$, p=.22), which perhaps suggests that the marginal result was not indicative of a true effect of semantics on suppression times ($M_{control}$=7.21 log ms (SD=0.36), $M_{violation}$= 7.23 log ms (0.38)). Consistent with this, when response times were analyzed with a mixed effects model, accounting for sentence length, there was again no reliable effect of sentence type (B = 0.01(0.01), t=1.02, p=.31).

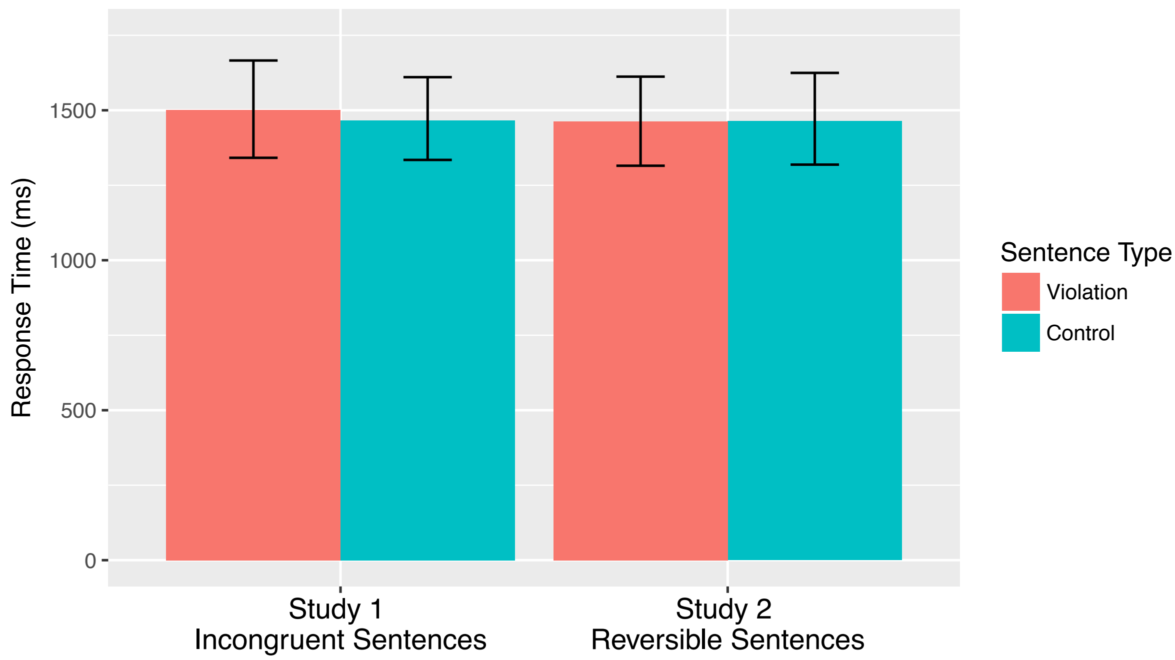

Figure 2. Mean raw response times in Studies 1 and 2. Error bars indicate bootstrapped 95% confidence intervals around the mean.

Interestingly, when we analyzed the Perceptual Awareness Scale ratings, we found that Control sentences were reported as having been seen slightly (but significantly) more clearly than Violation sentences ($\underline{M}_{control}$=1.86 (SD=0.49), $M_{violation}$= 1.81 (0.50), $t(47)=2.8$, $p=.007$). This could imply that participants used a different detection criterion for concluding that Violation sentences had been perceived clearly (for instance, violation sentences may have required more evidence to recognize because of their unusual meanings). This criterion difference could potentially explain the reaction time finding, that Control sentences were perceived more quickly, because participants would have been more willing to conclude that they were there. However, this result could also imply that Control sentences emerged more vividly into awareness than Violation sentences, which might implicate unconscious semantic processing. Because of its ambiguity, we do not interpret this result further for now; importantly, we used Studies 3 and 4 to test whether it would replicate for conceptually similar conditions.

Discussion

Unlike in Sklar et al (2012), the semantically anomalous sentences in Study 1 did not break suppression faster than neutral sentences. Although there was a marginal effect of semantics (in the opposite direction to that reported by Sklar et al., 2012) when raw data were analyzed, this effect did not hold in the log transformed analysis, and was potentially also explained by a criterion effect, evidenced by a reliable difference in judgments on the perceptual awareness scale.

One potential cause of this failure to replicate could be a low-level factor: differences in visual co-occurrence statistics between words in English and Hebrew (i.e., the frequency with which words tend to appear together in a sentence). For instance, English lexical co-occurrence statistics might have worked against any effect of semantics. Study 2 contrasted sentences that were better matched.

Study 2

In Study 2 participants were again presented with sentences under suppression, and we again varied whether or not the sentences were semantically anomalous. But this time, we aimed to control for a variety of lower-level features (e.g., the shapes of individual words, lexical co-occurrences, and so forth) by using reversible pairs of sentences. In Control sentences, an animate actor performed a plausible action, e.g., *Mike ate the steak*; the Semantically Anomalous sentences reversed the order of actor and theme (e.g., *The steak ate Mike*). As such, these pairs of sentences varied in meaning, but their lower-level properties were tightly matched. In addition, compared to Study 1, this study had higher power to detect any effect of unconscious semantic processing because we used 150 pairs of reversed sentences (i.e., 150 sentences per condition and 300 trials in total per participant), which gave us 80% power to detect an effect of size 0.29. This study used the same participants, procedure, and analysis as Study 1.

Analysis and Results

After removing timeouts (median = 0 trials per participant [range = 0 – 119; two participants were extreme outliers]), we applied the same exclusion criteria as Study 1. 5 participants were excluded along with 6% of the remaining trials (median = 12.5 trials per participant [range=2 - 59]). With these exclusions, we still had 80% power to detect an effect of size 0.3. Our mixed effects analysis had the form RT ~ Sentence Type + Sentence Length+ (1+Sentence Type|Participant) + (1+Sentence Type|Item); this is the same structure as in Study 1, except that we treated each reversible pair of sentences as a single item, and included a random by-item predictor to account for how the effect of Sentence Type might vary across different pairs of sentences (analogously to how it might vary between participants).

Figure 2 shows that, just like Study 1, Study 2 produced no evidence for unconscious sentence processing. When the raw response time data were analysed using a paired *t*-test, we found no effect of sentence type: the sentence's meaning did not influence response times ($M_{control}$=1463ms (SD=549), $M_{violation}$= 1463ms (536), $t(47) = 0.02$, p=.98). Our subsequent analyses confirmed this. There was no effect of sentence type for log-transformed response times analyzed using a *t*-test (($M_{control}$=7.20 log ms (SD=0.37), $M_{violation}$= 7.20 log ms (0.37), $t(47) = 0.86$, p=.39), nor under the mixed effects analysis (B = 0.005(0.006), t=0.8, p=.42).

In this study, and similarly to the reaction time analysis, we found no difference between conditions in the Perceptual Awareness Scale ratings ($M_{control}$=1.84 (SD=0.50), $M_{violation}$= 1.84 (0.50), $t(47)$=.25, p=.81).

Discussion

Study 2, like Study 1, failed to replicate the previous demonstration that sentences could be interpreted without awareness, even while using a new, larger set of stimuli that were designed to more-precisely control for any low-level perceptual features. However, one concern about the findings of Studies 1 and 2 is that our continuous flash suppression manipulation may have been too strong: Perhaps no aspect of the suppressed stimuli used here, including their lower level properties, would have influenced participants' responses, thus blocking any access to semantics. To test this idea, we drew on prior findings that suppression times are sensitive to the amount of low level information that is present in a suppressed stimulus. For instance, suppression times are shorter when suppressed images are more complex or larger (e.g., for words containing more characters, Gayet, Van der Stigchel and Paffen, 2014; Heyman and Moors, 2014, E. Yang & Blake, 2012). If our flash suppression manipulation had been too strong, then lower-level factors should not affect suppression times. We used a mixed effects model to regress response time against the length of each stimulus in characters (centered and standardized), collapsing across Studies 1 and 2. Longer sentences were indeed faster to break suppression (B = -0.02(0.007), t=3.2, p=.002), indicating that low level features of our stimuli influenced participants' response times, even if high level features of those stimuli, like meaning, did not.

A second reasonable worry about the choice of stimuli in this study – semantically unusual versus neutral sentences – is that they may not provide the strongest test of unconscious language processing and the proposed (Hassin, 2013) Yes It Can principle of unconscious cognition. In their Experiments 4 and 5, Sklar et al found that phrases with negatively-valenced meanings, like *electric chair,* broke suppression faster than neutral phrases like *dining table*, even though the individual words in each phrase were always neutral. Because that finding echoes demonstrations that fearful faces break suppression faster than neutral faces (E. Yang et al., 2007), it may be more robust, and we therefore attempted to replicate this effect of valence in Studies 3 and 4.

Studies 3 and 4

Study 3 was a replication of Sklar et al's Experiment 4, using English versions of their stimuli. In Study 4, like in Study 2, we used reversible sentences to assess whether suppression times were affected by a sentence's valence while controlling for lower level visual and lexical properties (e.g., comparing *the baby hit the brick* versus *the brick hit the baby*). As with Studies 1 and 2, the data for Studies 3 and 4 (as well as Study 5, described below) were collected in the same testing session, and trials for the different studies were randomly intermingled.

Study 3

Methods

Participants

73 students (47 female, mean age 21, range 18-24) from the University of Edinburgh community participated, and were paid £7.10 an hour. All identified English as their native language from birth and had normal or corrected vision with no colour blindness. This number of subjects gave us 80% power to detect an effect of size 0.49. The original study's effect size was 0.38 but, given the number of stimuli used, we would not have been able to reach 80% power even with infinite participants (Westfall et al., 2014). In addition, testing 73 participants ensured that we followed the 'Small Telescopes' recommendation (Simonsohn, 2015) that, when a study is replicated using a sample 2.5 times larger than the original, it has 80% power to reject the original study's effect size as the true underlying effect size (the original study used only 28 participants).

Materials

Each participant saw 50 English phrases, 24 with neutral emotional affectivity and 26 negative. 34 phrases were taken from the English translations of the 45 Hebrew stimuli used in Sklar et al's experiments 4a, 4b and 5. Those stimuli judged to be too specific to Israeli culture or composed of more than two words in translation (e.g. "a stake in the eye") were replaced with suitable alternatives.

To confirm the valence of the phrases, they were rated by 649 workers on Amazon Mechanical Turk (AMT). Each worker was paid to rate up to 15 stimuli, using an affective scale ranging from -5 to 5, where negative scores were given to emotionally negative stimuli and positive scores to positive stimuli. On average, each stimulus received 50 ratings. An independent samples t-test performed on the mean ratings of the stimuli confirmed a significant difference between the phrases that we had pre-labeled as negative and neutral

(Neutral M = 0.6 (SD = 0.5), Negative M = -2.9(1.0), $t(25) = 14.6$, $p < .001$). However, the mean ratings for the constituent words of the negative and neutral phrases were overall neutral (0.33, SD=1.0)[2]. A full list of experimental items is available in the appendix.

Procedure

The procedure was the same as Study 1, except for a modification of the perceptual rating scale, with participants' experiences being rated as 'No Text', 'Blurry Text', 'Almost Clear Text' and 'Absolutely Clear Text'.

Analyses

After removing trials that timed out (median = 0 trials per participant [range = 0 - 22]), we followed Sklar et al's exclusion criteria. Two participants were excluded along with 3% of remaining trials (median = 2 trials per participant [range=0 - 6]). With these exclusions, we still had 80% power to detect an effect of size 0.49.

We again analyzed the data in three ways. First, we followed Sklar and colleagues by conducting a by-items regression of raw response time against valence score. Second, we conducted the same analysis on log transformed response times. Finally, we analyzed the log response times using a linear mixed effects models to simultaneously account for length, participants and items. Our model had the structure RT ~ Valence Score + Phrase Length + (1+Valence Score|Participant) + (1|Item); this accounts for the fact that each participant produced responses to many different items with different valences, but that each item only had a single valence score.

Results

Our results were very similar to those of Study 1. Again, raw suppression times varied slightly as a function of affective valence (see Figure 3), but in the opposite direction to that found by Sklar et al, and not in a statistically significant fashion (B = -0.018(0.012), t=1.5,

[2] However, the words we used in negative phrases were probably more negative than those words used by Sklar et al. (2012). This was because the English translations of their stimuli, provided in their appendix, had a stronger negative valence than their Hebrew counterparts, a fact that we only discovered after testing. Because the presence of negative words should slightly increase the overall negativity of the negative sentences, it should also increase the chances of negatively valenced phrases breaking suppression faster in this study, and so this error cannot explain any failure to replicate.

p=.14). There was also no effect in the log transformed data (B = -0.008(0.007), t=1.3, p=.19). When analyzed using mixed effects models accounting for length, there was, however, a marginal effect of semantics (B = -0.011(0.006), t=1.9, p=.06), but still in the unexpected direction.

Unlike Study 1, where phrase meaning had affected ratings on the Perceptual Awareness Scale, here we did not find any such effect (B = -0.007(0.01), t=0.5, p=.59).

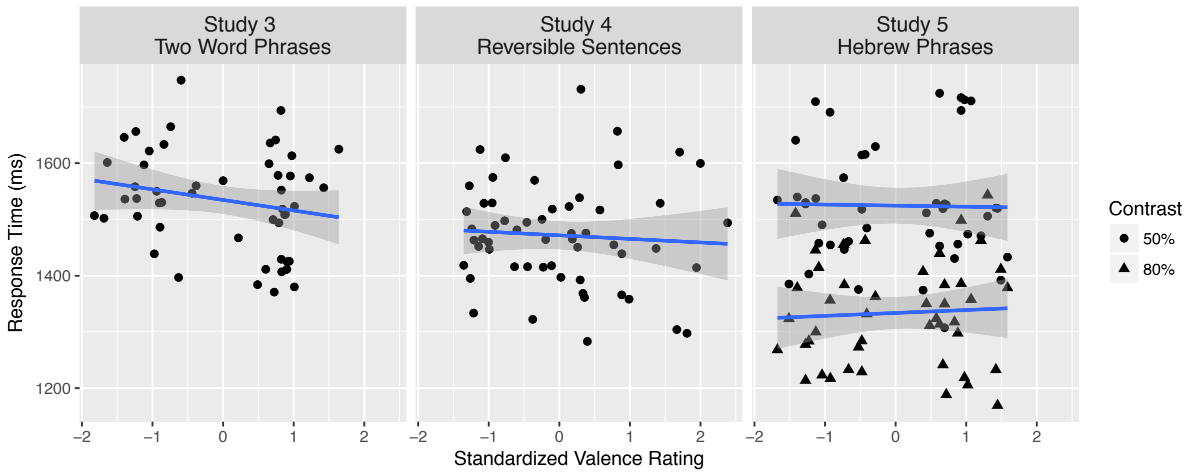

Figure 3. Effects of sentence valence (centered and standardized) on raw response times in Studies 3 through 5. Blue lines show the estimated linear fits of response time to valence rating for each study and contrast level, and ribbons around lines indicate bootstrapped 95% confidence intervals around the estimates.

Study 4

Study 4 was analogous to Study 2, including in its motivation: We aimed to test whether emotional valence might affect the suppression time of sentences when their lower level features were more precisely controlled. To do this, we again compared reversible pairs of phrases which had very different valences.

Methods were the same as Study 3, except that we used 28 pairs of English sentences whose valence strongly changed when their agent and theme were reversed, as rated by Amazon Mechanical Turk users (e.g. *The baby hit the brick* versus *The brick hit the baby*). Affectivity ratings were significantly different between negative and neutral sentences (Neutral M = -0.1

(SD = 1.7), Negative M = -3.5(1.1); $t(27) = 9.9$, $p<.001$). This number of stimuli and subjects gave us 80% power to detect an effect of 0.41 or larger.

Analysis and Results

After removing trials that timed out (median = 0 trials per participant [range = 0 - 12]), we used the same exclusion criteria as Study 3. 5 participants were excluded along with 3% of the remaining trials (median = 2 trials per participant [range=0 - 4]). With these exclusions, we still had 80% power to detect an effect of size 0.41. Our mixed effects analysis had the form RT ~ Valence Score + Length + (1+Valence Score|Participant) + (1+Valence Score|Item), accounting for the fact that each participant responded to multiple different items with different valence, and that each item (a pair of reversible sentences) could have two valences.

As in Study 2, we found no effect of affective valence on response time once low-level factors were controlled for (see Figure 3). This was true for raw data (B = -0.006(0.01), t=0.5, p=.62) and log-transformed data (B = -0.003(0.006), t=0.5, p=.62), and also held under a mixed effects model analysis that accounted for the pairings between items (B = -0.002(0.006), t=0.39, p=.69). Again, therefore, the data provided no evidence for unconscious semantic combination.

As in Study 3, phrase meaning did not affect ratings on the Perceptual Awareness Scale (B = 0.002(0.01), t=0.2, p=.81).

Study 5

Given that Studies 1 through 4 did not uncover evidence for combinatorial semantic processing, we considered alternative causes of Sklar et al's original results. For example, might the semantic characteristics of their Hebrew stimuli have been unwittingly correlated with certain lower-level visual characteristics? We therefore conducted a further replication of their original Experiments 4 and 5 (i.e., our Study 3), but in which English speakers saw phrases in the original Hebrew script. If lower-level visual differences explained the original findings, then we might expect negatively valenced words to break suppression faster in our sample, even though participants could not possibly have processed their meaning.

In addition, to confirm our participants' sensitivity to the visual properties of the suppressed words in Studies 3 through 5, we conducted two further tests. First, we assessed whether suppression times would be shorter when the contrast of the suppressed phrases of Study 5 was higher. We presented each Hebrew stimulus twice; on one presentation it was shown at a maximum of 50% contrast, and on one presentation it was shown at a maximum of 80% contrast.

Second, we tried to replicate a finding from Jiang et al. (2007), that words in familiar written scripts (i.e., English) break suppression faster than words from unfamiliar scripts (in this case, Hebrew). We compared response times to the Hebrew phrases from this study with response times to the English phrases from Studies 3 and 4, while controlling for both length and contrast.

Methods

Study 5 used the same participants and procedure as Studies 3 and 4, and its trials were intermingled with those studies. Participants saw 45 Hebrew phrases twice each, at two different contrast levels (order of presentation was randomized). Low contrast phrases ramped up from 0 to 50% over 700ms (as in our previous studies), and high contrast phrases ramped up from 0 to 80% over 700ms.

We conducted separate power analyses for each of our planned assessments. For the valenced Hebrew phrases assessment, we had 80% power to detect an effect of size 0.51, for the assessment of visual contrast we had 80% power to detect an effect of size 0.34, and for the comparison of English and Hebrew we had 80% power to detect an effect of size 0.33.

Analysis and Results

After removing trials that timed out (median = 0 trials per participant [range = 0 - 21]), we used the same exclusion criteria as Study 3. 5 participants were excluded along with 4% of the remaining trials (median = 3 trials per participant [range=1 - 10]). These exclusions did not importantly change the effect sizes that we could detect at 80% power (Hebrew valence: 0.52; Visual contrast: 0.35; Hebrew-English: 0.33).

We analyzed the effects of Hebrew valence and Contrast simultaneously using a linear regression, as in Study 3, predicting response times for each item as a function of its Valence and Contrast. We also used a mixed effects analysis of the form RT ~ Contrast * Valence +

Length + (Contrast * Valence|Participant) + (Contrast|Item); this analysis accounts for how each participant responded to multiple different items of different contrasts and valence, and for how each individual item was seen at two contrast levels.

There was no evidence that the original findings were confounded by low-level visual features. As shown in Figure 3, phrases with lower affective valence scores were no faster to break suppression. This was true for raw data (B = 0.0016(0.011), t=0.16, p=.88) and log-transformed data (B = 0.003(0.006), t=0.49, p=.62), and also held under a mixed effects model analysis (B = 0.002(0.007), t=0.28, p=.78). This null effect did not differ across the contrast levels (all p values > .70 across all analyses).

Our subsequent analyses confirmed that participants were sensitive to the visual properties of the suppressed words. As expected, we found that participants responded faster to phrases that were presented in higher contrast (B = -0.072(0.007), t=10.4, p<.0001).

Interestingly, we also found evidence consistent with Jiang et al's (2007) claim that suppression times are longer for words in unfamiliar scripts. We compared responses times to the 45 low-contrast Hebrew phrases used in Study 5 with response times to the 106 English phrases used in Studies 3 and 4, using a mixed effects regression of the form RT ~ Language + Phrase Length + (1+Language+Phrase Length|Participant) + (1|Item). This model accounted for the fact that each participant saw different items of different lengths from each language, but that each item had only a single length and language. The model indicated that English phrases broke suppression faster than Hebrew phrases (English: M = 1.58s (SD = 1.0), Hebrew: 1.67s (1.1)) when matched on contrast and controlling for length (B = 0.03(0.01), t=2.8, p=.005).

Phrase meaning did not affect ratings on the Perceptual Awareness Scale (B = -0.004 (0.008), t=0.55, p=.58), but high contrast stimuli were perceived more clearly (B= 0.05 (0.008), t=5.8, p<.001); this is unsurprising due to the continuous ramp-up of stimulus contrast in each trial: even if a suppressed stimulus had broken into awareness before its contrast had reached maximum, in the time it took the participant to press the key to report detection, contrast would have continued to increase (and do so to a greater extent in the high-contrast condition, leading to higher visibility). Contrast and semantics did not interact (B=0.005(0.008), t=0.6, p=.54).

Study 5 therefore shows that participants are sensitive to a number of low level visual properties of suppressed sentences, but that these properties cannot easily explain the results of Sklar et al (2012).

Discussion

Like Studies 1 and 2, Studies 3 and 4 provided little evidence that word meanings could be combined without awareness. This was the case for English phrases that were highly similar to those used by Sklar et al (Study 3), and for sentences with better-controlled visual properties (Study 4). However, the results of Study 5 rule out a simple alternative explanation of the original Hebrew findings based on low level visual properties, and also confirm that our participants could process the visual properties of the suppressed stimuli (as indicated by the fact that stimulus length and familiarity of the alphabet used did affect breakthrough time). In addition, Studies 3 and 4 provided no evidence that participants used different criteria to respond to more versus less emotionally valenced phrases, which contrasts with the findings of Study 1; we return to this point in the General Discussion.

In combination, Studies 1 through 5 fail to provide consistent support for unconscious semantic composition, but also provide no insight into why those results might have been found in the original report of Sklar and colleagues. However, a hint of a pattern did emerge across these studies: when data were log transformed prior to analysis, the p values of our statistical tests tended to move away from the significance level of 0.05. We return to this point later.

The following studies assess two further explanations of our failure to find evidence that combinatorial semantics is processed outside awareness. Studies 6 through 9 test whether the failure can be explained by simple differences between our Continuous Flash Suppression procedure and Sklar et al's procedure, by replicating Studies 1 through 4 using their stimulus presentation scripts. Study 10 tests whether participants may find it easier to unconsciously process semantics when words are presented closer to fixation.

Studies 6 through 9

We are grateful to the authors of Sklar et al (2012) for generously sharing their stimulus presentation scripts with us. These presented stimuli in a roughly similar fashion to ours;

perhaps the most important difference was that participants did not complete a Perceptual Awareness Scale at the end of each trial. Our use of the PAS in the first five studies may have reduced participants' 'flow', or engagement with the task, by interrupting transitions from one trial to the next, thus potentially impairing the processing of stimuli during the experiment (R. Hassin, personal communication, June 2016). We used these scripts to conduct a single session comprising four studies that replicated the key contrasts tested in our previous studies. Study 6 retested Sklar et al's semantic violation stimuli, replicating our Study 1 (*I ironed the clothes/I ironed the coffee*); Study 7 retested our reversible violation stimuli (*Mike ate the steak/the steak ate Mike*), replicating our Study 2; Study 8 tested Sklar et al's valenced phrases (*dining table/electric chair*) replicating our Study 3; and Study 9 tested our reversible valenced sentences (*the baby hit the brick/the brick hit the baby*), replicating our Study 4. Again, we used a large number of participants (74) to ensure that these studies had sufficient power to detect any effect.

General Methods

Participants

74 students (50 female, mean age 21, range 18-30) from the University of Edinburgh community participated, paid £7.10 an hour. All identified English as their native language from birth and had normal or corrected vision with no colour blindness. This gave us 80% power to detect an effect of size 0.37 in Study 6, of size 0.25 in Study 7, of size 0.49 in Study 8, and of size 0.40 in Study 9.

Procedure and Materials

The procedure was implemented in Matlab using the Psychophysics Toolbox extensions (Brainard, 1997; Kleiner et al., 2007; Pelli, 1997). It was similar to our previous studies, but Mondrian masks now alternated at 10Hz (rather than 60Hz), trials timed out after 20s, and there was no perceptual rating scale at the end of each trial. We used the same sentence stimuli as in Studies 1 through 4, but did not intermingle these stimuli. Instead, participants received the studies in one of two orders: Study 6-Study 8-Study 7-Study 9 or Study 8-Study 6-Study 9-Study 7, with orders split equally between participants. We reasoned that this procedure, in which studies using Sklar et a's original stimuli were presented before participants became fatigued, had the highest likelihood of replicating Sklar et al.'s (2012) original findings.

Analysis and results

The data were analyzed in the same fashion as Studies 1 through 4. As in Studies 1 and 2, Studies 6 and 7 compared conditions (violation vs. control sentences) using *t*-tests on raw and log transformed data, as well as mixed effects regressions. As in Studies 3 and 4, Studies 8 and 9 used by-items regressions to assess whether suppression time varied as a function of sentence meaning (comparing raw data and log transformed data), and also used an additional mixed effects regression.

Exclusions were calculated per study, based on the procedures described in Studies 1 and 3. For Study 6, we excluded timeouts (median 0 trials per participant [range = 0 – 51; the extreme high value came from an outlier who was subsequently excluded]), and 12 participants, along with 5.7% of the remaining trials (median = 4 trials per participant [range=0 - 27]). For Study 7, we excluded timeouts (median 0 trials per participant [range = 0 – 4]), 8 participants (plus four further participants who had dropped out before Study 7 began), and 5.4% of the remaining trials (median = 11 trials per participant [range=2 - 82]). For Study 8, we excluded timeouts (median 0 trials per participant [range = 0 – 26]), and 14 participants, along with 4% of the remaining trials (median = 2 trials per participant [range=0 - 11]). For Study 9, we excluded timeouts (median 0 trials per participant [range = 0 – 16]), 8 participants (plus two participants who dropped out before Study 9) along with 4% of the remaining trials (median = 1 trials per participant [range=0 - 10]). With these exclusions, we still had 80% power to detect an effect of size 0.38 in Study 6, of size 0.27 in Study 7, of size 0.5 in Study 8, and of size 0.48 in Study 9.

Study 6 (replication of Study 1)

When we analyzed participants' raw response times, we found that they did in fact vary as a function of unconscious semantics, a result that had been marginal in our Study 1 (Figure 4). Again, however, this effect was in the opposite direction to that found by Sklar et al (2012): Violation sentences were slower to break suppression than control sentences ($M_{control}$=1825ms (SD=663), $M_{violation}$= 1913ms (SD=784), *t*(61)=2.4, *p*=.02). However, when we accounted for the positive skew of the data via a log transformation, this effect was no longer significant ($M_{control}$=7.39 log ms (SD=0.36), $M_{violation}$= 7.41 log ms (0.39), *t*(61)=1.7, *p*=.09), and it was also not significant in the mixed effects regression that included length as a covariate (B=0.02(0.02), *t*=1.4, *p*=.17).

Study 7 (replication of Study 2)

Our original Study 2 provided no evidence that sentences were processed unconsciously, and the same was true here (Figure 4) for raw response times ($\underline{M}_{control}$=1323ms (SD=345), $M_{violation}$= 1323ms (353), *t*(61)=0.09, *p*=.93), for log transformed response times ($\underline{M}_{control}$=7.13 log ms (SD=0.28), $M_{violation}$= 7.13 log ms (0.29), *t*(63)=0.41, *p*=.67), and under the mixed effects regression analysis (B=-0.002(0.006), *t*=0.37, *p*=.71). Note that response times were much shorter in Study 7 than Study 6, presumably because participants had adapted to the CFS by the time Study 7 was run (in both possible orders, Study 6 preceded Study 7).

Study 8 (replication of Study 3)

While the results of our original Study 3 were marginally significant, we found no evidence that semantics affected response times in this replication (Figure 4). This was true for both raw response times (B=0.02(0.03), *t*(48)=0.88, *p*=.38), log response times (B=0.009(0.01), *t*(48)=0.83, *p*=.41), and the mixed effects regression (B=0.005(0.009), *t*=0.54, *p*=.58). Indeed, the effect was numerically in the opposite direction to that found in our Study 3.

Study 9 (replication of Study 4)

While our original Study 4 did not find an effect of semantics, participants in this replication were reliably slower to respond to neutral sentences (B = 0.033(0.014), *t*=2.4, *p*=.02), at least in the raw data (Figure 4). However, this effect was entirely driven by the positive skew in participants' reaction times, and it disappeared when the data were log transformed before analysis (B = 0.007(0.008), *t*=0.9, *p*=.40), and also when analyzed in the mixed effects regression (B=0.01(0.02), *t*=0.61, *p*=.54). Note that response times were much shorter in Study 9 than Study 8 (which always preceded Study 9), presumably because participants had adapted to the CFS.

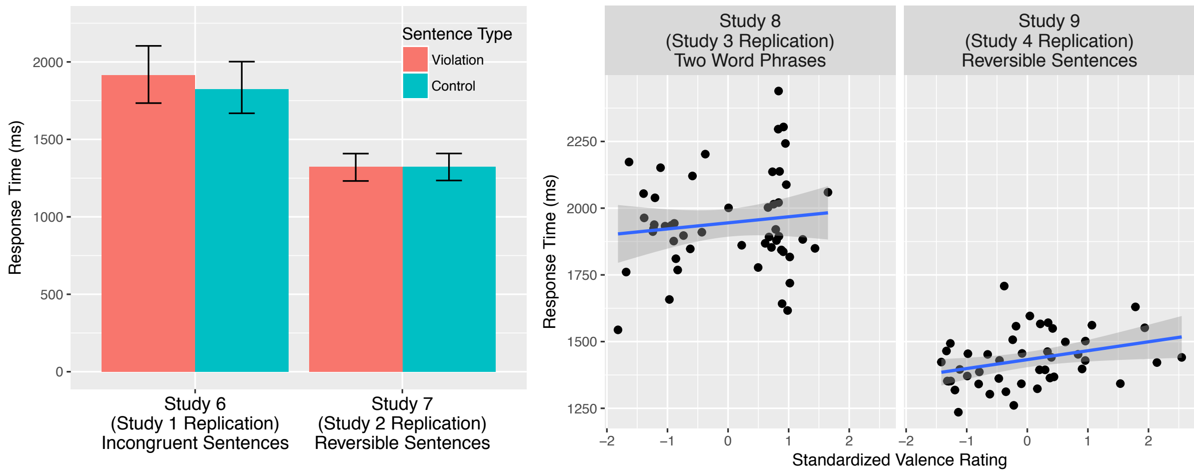

Figure 4. Results of Studies 6-9 (replicating Studies 1 through 4) using the original presentation scripts of Sklar et al. (2012). Error bars indicate 95% confidence intervals around the mean, blue lines indicate estimated linear fits of valence to response times, and ribbons around lines indicate 95% confidence intervals around the estimates.

Order of presentation analysis

We also examined if participants showed greater evidence of unconscious processing before becoming fatigued, i.e., when completing the very first task (recall that half of our participants completed Sklar et al's semantic violation task [Study 6] before the valenced phrases task [Study 8], and vice versa). This new analysis, however, also provided no evidence of unconscious processing. When Study 6 was completed first (n = 30 participants after exclusions), raw response times did not vary as a function of unconscious semantics ($M_{control}$=1880ms (SD=760), $M_{violation}$= 1990ms (SD=970), *t*(29)=1.5, *p*=.15) and nor did log response times ($M_{control}$=7.39 log ms (SD=0.38), $M_{violation}$= 7.42 log ms (0.43), *t*(29)=1.1, *p*=.43). When Study 8 was completed first (n=32 after exclusions), raw responses times did not vary as a function of valence (B=0.03(0.04), *t*(48)=0.84, *p*=.41) and nor did log response times (B=0.01(0.02), *t*(48)=0.52, *p*=.41).

Discussion

Although Studies 6 through 9 used the same presentation parameters and Matlab presentation scripts as Sklar et al (2012), they still failed to produce evidence that the meanings of words are composed together without awareness. While two of the studies did produce marginal or significant effects when the raw data were analyzed (one of which was in the opposite direction to the findings in Sklar et al, 2012), these effects disappeared when the data were log transformed to normalize the response times. We return to the importance of this point in the General Discussion.

Study 10

The phrases and sentences used in Sklar et al (2012) were typically shorter (in terms of characters) than those used in our studies, because vowels in Hebrew words are typically inferred rather than written. One consequence of this is that their Hebrew stimuli would have been presented, on average, closer to the fovea, and the increased resolution with which such stimuli are perceived could, potentially, have made unconscious processing easier.

To test whether semantic access occurs for stimuli that can more easily be fixated or foveated, we moved from sentences to single words. Study 10 followed up the experiments of Yang and Yeh (2011), who demonstrated that the meanings of Chinese words influence response times in a breaking CFS paradigm. However, their task had two potential problems. First, they only used 12 different negative and neutral words per condition (repeated four times), which increases the possibility that semantics may have been confounded with lower level visual features. Second, participants' responses in the task (indicating when a stimulus appeared) were not independent of the nature of the stimulus[3].

In Study 10, instead, we used a higher power test (150 words per valence condition and 28 participants, compared to 12 words and 12 participants in the original report), and repeated the same procedure used in our previous studies. On each trial, participants saw a single word that had either a neutral or negatively valenced meaning, and indicated whether it lay above or below a fixation cross. We crossed the semantic manipulation with a manipulation of length: words were either short (3 or 4 letters) or long (7 to 12 letters). If distance from the fovea affects unconscious processing such that shorter words are perceived more clearly, then valence may modulate breakthrough times for short words to a greater extent than for long words.

Methods

[3] Participants responded when they saw a word, rather than indicating whether the word lay above or below fixation. This raises the possibility that participants may have applied different detection-report criteria to stimuli from different categories.

Participants

28 participants (16 female) were recruited under the same terms as our previous studies. Due to an experimenter error, age was not recorded, but the participants were drawn from the same University of Edinburgh population as our previous studies. This gave us 80% power to detect any effect larger than 0.39; the two studies in the original paper had effect sizes of 0.43 and 0.41.

Materials and Procedure

Participants saw a set of 300 words that crossed length (short/long) with meaning valence (neutral/negative, based on the 9-point valence scale taken from the Affective Norms for English Words (ANEW) database of the University of Florida, Bradley & Lang, 1999). Short neutral and short negative words were matched for length (Neutral mean: 3.8 characters (SD=0.4), Negative: 3.7(0.5)) but not for rated valence (Neutral: 5.3(0.3), Negative: 2.7(0.5)). Long neutral and long negative words were also matched for length (Neutral: 8.1(1.2), Negative: 8.2(1.2)) but not rated valence (Neutral:5.3(0.3), Negative: 2.7(0.5)). Long words were significantly longer than short words, and negative words were rated significantly lower than neutral words (both $p<.001$). Order of presentation was randomized. The CFS display parameters were otherwise similar to the procedure in Studies 6 through 9 (i.e., no perceptual rating scale, and mondrians presented at 10Hz), but we set the trial timeout to be 8s and programmed the study in PsychoPy rather than the Psychophysics Toolbox.

Analysis and Results

We excluded timeout trials (median 0 trials per participant [range = 0 – 137; the outlier participant who yielded the upper extreme of this range was subsequently excluded]), and two participants who were accurate on less than 90% of trials or whose mean response time was greater than 3 standard deviations from the group mean (as a result, we had 80% power to detect an effect of size 0.40). We then excluded trials on which participants answered incorrectly, and trials on which response time was less than 200ms (removing 1% of the data, median = 3.5 trials per participant [range=0 - 6]). We analyzed the resulting data using a linear mixed effects model of the form RT ~ Length * Valence + (1+Length*Valence|Participant) + (1|Item), accounting for the fact that each participant saw items that crossed length with valence, but that each item only had one length and valence. We analyzed both raw data (as in Yang and Yeh, 2011) and log transformed data.

The resulting analyses (Figure 5) were consistent with our previous findings. When we analyzed the raw data, we found that suppression times were reduced for longer words (M_{short}=1307ms (SD=584), M_{long}= 1476ms (SD=614), B=0.18(0.04), *t*=4.2, *p*<.001) but were unaffected by the meanings of those words ($M_{neutral}$=1412ms (SD=622), $M_{negative}$= 1372ms (SD=589), B=0.04(0.03), *t*=1.3, *p*=.18), and these factors did not interact (B=-0.01(0.05),*t*=0.2, *p*=.82). The analysis of the log transformed data reinforced this point. Again, length had a significant effect (M_{short}=7.12 log ms (SD=0.35), M_{long}= 7.01 log ms (SD=0.35), B=0.11(0.02), *t*=6.4, *p*<.001) but semantics had no effect ($M_{neutral}$=7.08 log ms (SD=0.36), $M_{negative}$= 7.06 log ms (SD=0.35), B=0.02(0.02), *t*=1.1, *p*=.27) and these two factors did not interact (B=-0.0003(0.02), *t*=0.012, *p*=.99).

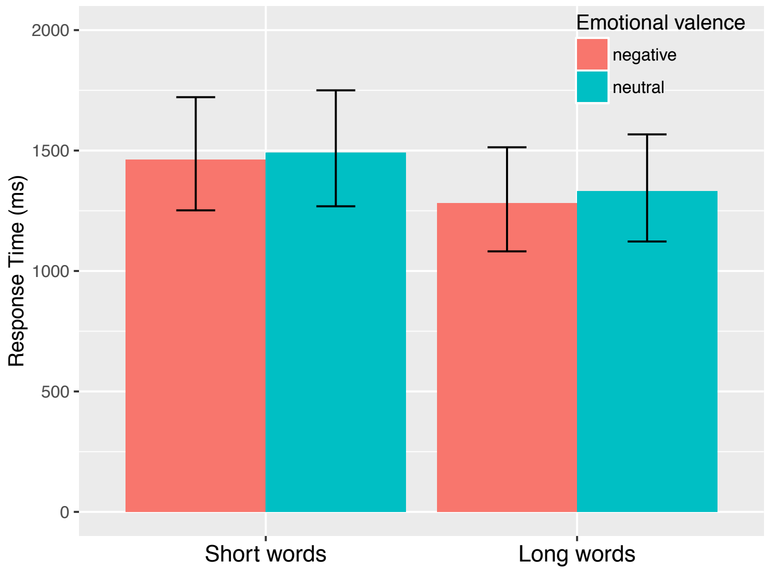

Figure 5. Mean response times in Study 10. Error bars show bootstrapped 95% confidence intervals around the mean.

In summary, even when single words were viewed near to fixation, their meanings did not affect suppression times. It therefore seems unlikely that the null findings from Studies 1 through 9 can be explained through a failure to foveate the critical stimuli.

Discussion and meta-analysis

Our ten studies consistently failed to find evidence that participants could process the meanings of English words that were masked from awareness using continuous flash suppression. This was true for sentences, for two-word phrases, and for single words. But while each study in isolation may not have contributed much evidential value, might the full combination of studies reveal a more subtle effect?

We conducted two meta-analyses. First we assessed the effect of semantics on raw response times. Although we were skeptical of the raw response time analyses given the data's severe positive skew, this analysis allowed us to combine our studies with the relevant findings of Sklar et al (2012) and Yang and Yeh (2011), to provide a more precise estimate of any effect size. Then, in a second meta-analysis, we assessed log transformed response times, with the dataset restricted to the studies reported here.

Raw response time meta-analysis. We calculated the effect of semantics in each of our studies, along with Sklar et al's Experiments 1, 2, 4a and 4b, and Yang and Yeh's Experiments 1 and 2. For ease of interpretation, our Study 10 was divided into two: an effect of semantics on recognizing long words (10a), and an effect on recognizing short words (10b). These effect sizes were entered into a multilevel random effects meta-regression (Viechtbauer, 2010); this type of model assumes that each effect size is a randomly drawn estimate of a true population effect size, and weights each effect size by its sample size. We used a multilevel regression in order to model each effect size as being drawn from a different lab (in Scotland, Israel or Taiwan). Across all 16 studies, the estimated overall effect size was -0.068(SE=0.18), which was not significantly different from zero (Z=0.38, p=0.70, 95%CI=-0.41 – 0.28).

We then used a meta-analytic Bayes Factor analysis (Rouder and Morey, 2011) to evaluate whether the estimated overall effect was more consistent with the null hypothesis, in which there is no effect of unconscious language processing, or with a less informative hypothesis, that is agnostic as to whether or not there is an effect of unconscious language processing, such that the true effect size could lie anywhere on a uniform distribution between 1 and -1, including 0. We calculated a meta-analytic Bayes Factor (i.e., the ratio of evidence for the alternative hypothesis over evidence for the null hypothesis) using the function *meta.ttestBF* in the R package BayesFactor (Morey & Rouder, 2015; we edited this function to account for comparisons between paired samples). A Bayes Factor over 3 is typically taken as evidence for the alternative hypothesis, while a Bayes Factor under 0.33 is typically taken as evidence for the null. The resulting Bayes Factor was 0.23, suggesting that this set of experimental results was four times more likely under the null hypothesis than the alternative.

We thus estimate that there is no overall effect of semantics on suppression times. However, we note that our meta-regression did also show considerable heterogeneity in its estimates (Cochran's Q(15)=64.6, *p*<.001), so it remains possible that subtle differences in method and procedure, in population, or in the language used, may have led to important differences in the results of the three different labs under test.

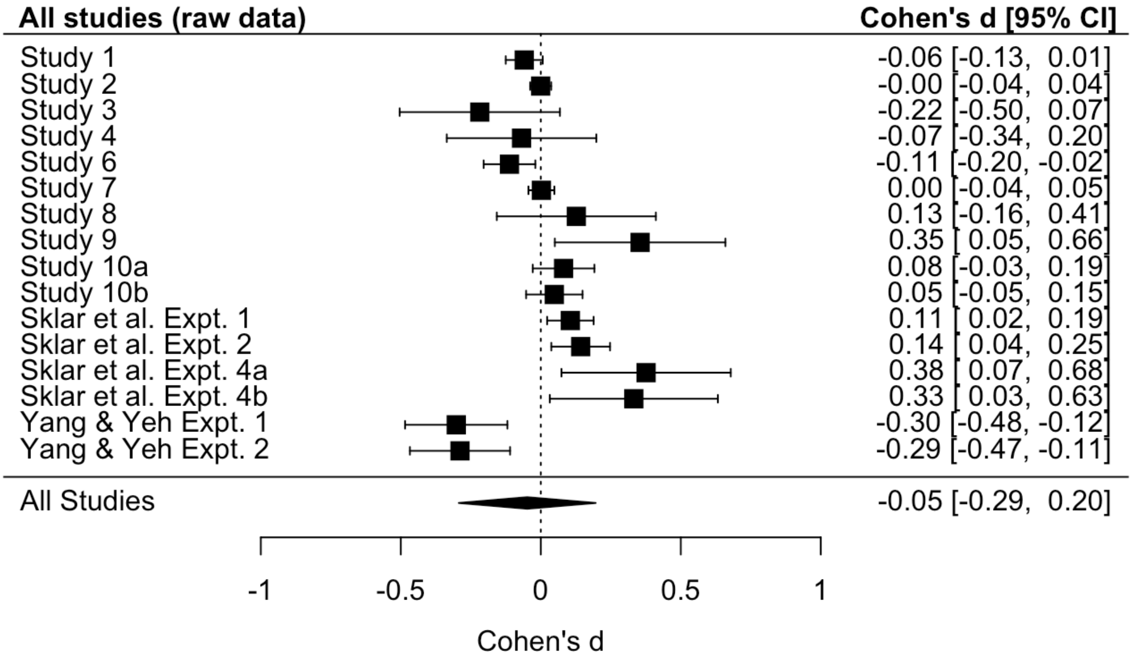

Figure 6. Forest plot of effect sizes calculated from the raw data analyses from the current studies, from Experiments 1, 2, 4a and 4b of Sklar et al. (2012), and from Experiments 1 and 2 of Yang and Yeh (2011). Cohen's *d* effect size estimates were calculated from the relevant *t* and *F* statistics of each study. For comparisons between paired samples, we followed Dunlap, Cortina, Vascow and Burke (1996) and adjusted for the correlation between participants' responses in each condition. We calculated these correlations exactly for our studies (range from 0.94-0.99) and estimated the correlations to be 0.975 for the remaining studies. The "All Studies" estimate is taken from a random effects meta-regression model.

Log response time meta-analysis. Our second meta-analysis was restricted to the log transformed response times, which had reduced the effects of skewed data and provided results that were more stable and consistent across our studies. A random effects meta-regression (Figure 7) estimated that the overall effect size was -0.0049(SE=0.018). This estimate was also not significantly different from 0 (Z=0.27, p=0.78, 95%CI = -0.04 – 0.03), and again a Bayes Factor analysis, with the same parameters as before, favored the null hypothesis over the uninformative one (a Bayes Factor of 0.12, suggesting that the data is almost 8 times more likely under the null).

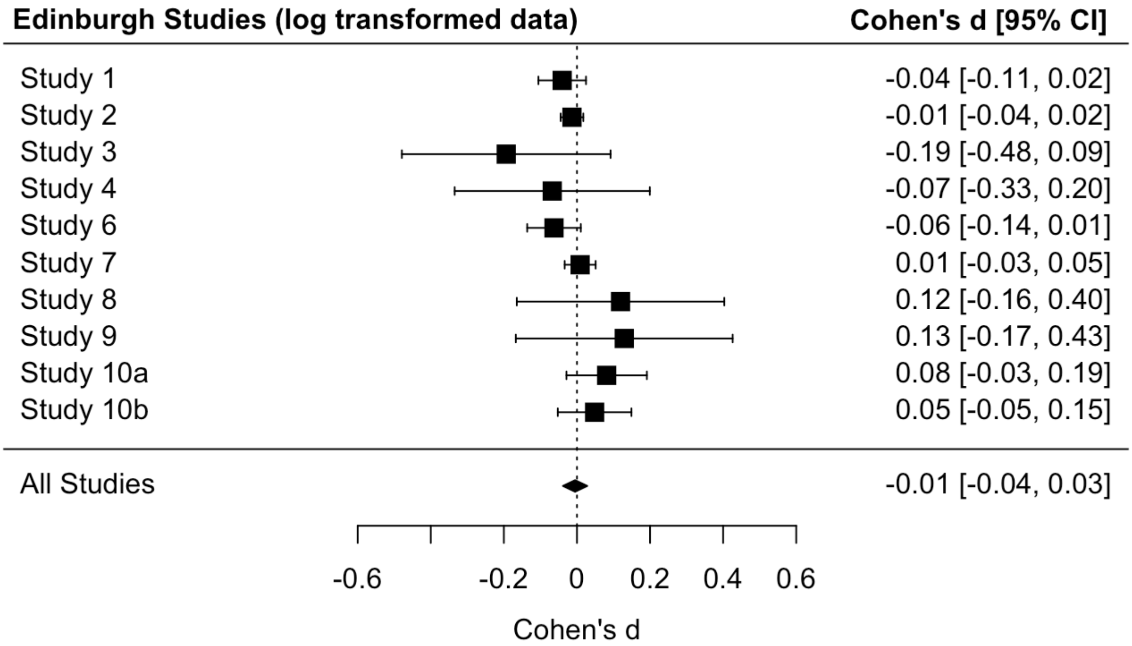

Figure 7. Forest plot of effect sizes calculated from the log transformed data analyses from the studies reported here, with all calculations as described in the Legend for Figure 6.

In combination, these two meta-analyses suggest that there is unlikely to be an effect of semantics on suppression times, but that if one does exist then (1) that effect is likely to be very small; (2) the sign of that effect is uncertain (i.e., whether it is facilitory or inhibitory); and (3) that effect may not generalize across languages.

General Discussion

Across ten high power continuous flash suppression studies, we consistently found no evidence that the semantics of an English phrase (or word) affects the efficacy with which that phrase can be suppressed from awareness. This contrasts with previous findings, from

Hebrew and from Chinese, in which semantic information appeared to be processed during continuous flash suppression.

Why did our studies produce such different results? One possibility is that the English alphabet or language is simply not amenable to unconscious processing. We do not believe that this possibility is particularly plausible. Both Hebrew and English use alphabetic writing systems, meaning that orthographic processing is relatively similar in both (indeed, it may be harder in Hebrew as vowels must usually be inferred) and there is no good reason to assume that unconscious processing should be more attuned to the grammatical and lexical features of Hebrew than features of English.

Chinese words are perhaps slightly different; in particular, it is possible that individual Chinese characters are more amenable to unconscious processing than Hebrew or English letters, as their symbolism is more iconic and less arbitrary. However, this factor also makes it harder to rule out the hypothesis that the effects found in Yang and Yeh (2011) were not in fact caused by the semantics of the presented words, but by their lower level visual features. Indeed, the small number of stimuli used in their experiments (12 words per condition in Experiment 1, 16 words per condition in Experiment 2) make it very plausible that correlations may have existed between high-level semantics and low-level features. We thus conclude that there are few good reasons to believe that different languages are differentially susceptible to unconscious *semantic* processing.

Another possibility is that confounding aspects of the experimental paradigm may have caused prior results. For example, participants may have set different detection criteria for judging when phrases of different meaning emerged into consciousness. However, the Perceptual Awareness Scale included in our first five studies uncovered minimal evidence for this: While there was some evidence that different criteria were used in Study 1, that finding did not replicate in subsequent studies.

Finally, it is also possible that our failure to replicate was driven by subtle methodological differences between our apparatus/testing conditions, and the methods used in prior work. For example, although our Studies 6 through 9 used the same presentations scripts as Sklar et al (2012), the task was carried out under slightly different conditions: the level of illumination in our testing room was lower and the viewing distance between observer and stimuli was greater (Ran Hassin, January 2017, personal communication). But while factors like this might explain why suppression times in our study were longer than those in Sklar et al (2012)[4], they do not naturally explain why we failed to find the expected differences between stimulus categories, given that the text used in our studies was perfectly legible (when read without suppression).

We thus wish to focus on an alternative explanation, which is not based on any experimental factor or confound, but rather on the distributional characteristics of response data in experiments that use the "breaking continuous flash suppression" (b-CFS) method.

The potential for false positives in the breaking CFS paradigm

b-CFS data are well known to have positive skew and kurtosis, and it is surprising that more studies do not analyze the resulting data with a log transformation to ensure a better approximation to a normal distribution. Indeed, our analyses of raw response times produced more statistical comparisons that were marginal or significant than our analysis of log transformed response times, which may be expected because strong skew or kurtosis are likely to invalidate key assumptions of parametric statistical tests (e.g., the normality of residuals).

But while parametric assumptions are important, here we want to propose a different idea: Strongly skewed data can lead to a high rate of false positives when combined with fairly common procedures for excluding outlier trials from a dataset. In particular, in Experiments 1 and 2 of Sklar et al (2012), data points were excluded from analysis if they fell more than 3 standard deviations from the grand mean of each condition (i.e., separate standard deviations were calculated for each condition). As we show below, this type of exclusion criterion can bias the results of statistical tests.

Of particular concern is that the values of a skewed dataset's outliers strongly influence the size of that dataset's standard deviation, much more so than the values of points that are not outliers. In a skewed dataset, there will almost certainly be large differences between conditions in the specific values of outliers, which will cause commensurate differences in the size of a standard deviation, and, if those standard deviations are then used for exclusions,

[4] Which were extremely short – less than 1 second on average.

this effect will cascade up to cause differences in the means of the resulting samples. Importantly, this means that excluding outliers based on their distance from each condition's overall mean leads to applying different exclusion criteria to different conditions. In other words, outliers are removed from different numerical ranges in different conditions; this, in turn, results in differential (and entirely artefactual) effects on each condition's post-exclusion mean. The direction of this artifact – which condition's mean ends up being larger or smaller – is random, meaning that significant effects may fail to replicate, or that replication efforts would be just as likely to find the opposite effect as the original one.

This mechanism can potentially explain the marginally significant result we found in our Study 1, which compared Control sentences (*I drank coffee*) to Violation sentences (*I ironed the coffee*). Applying the exclusion process to these two conditions meant that Control trials were excluded if they fell more than 3030ms from the mean, whereas Violation trials were excluded if they fell more than 3140ms from the mean. This extremely unequal exclusion criterion difference, of more than 100ms, should cause the mean of Control trials to be lower than the mean of Violation trials. This problem is particularly marked for skewed datasets compared to normally distributed datasets, because in the former case more data points will lie close to the exclusion boundary, and so the exclusion point's value will have a larger effect on the resulting mean of the distribution.

This difference could thus easily explain the actual mean difference we found in our study: Control sentences were seen 35ms faster than Violation sentences. If the condition-specific-outlier exclusion criterion had not been applied, then Control sentences would only have been seen 20ms faster (a non-significant difference, p=.53).

We illustrate the generality of this problem through a computational simulation. We generated a dataset in which 30 simulated participants took part in a two-condition experiment, each generating 50 trials per condition. Each participant's trials were drawn from a single exponential distribution whose rate was jittered (across participants) around 0.3, and the rate did not differ between the two conditions. The resulting data thus have positive skew but should not have a mean difference between conditions. We applied the by-condition exclusion procedure and analyzed the data using a t-test. Then, to assess the effects of the by-condition exclusion procedure on this dataset, we analyzed the distribution of p values from the simulated dataset when it was resampled and retested. To do this resampling, we took the original dataset (prior to any exclusions), shuffled the mapping between condition labels and data points for each participant, applied the by-condition exclusion procedure again, and analyzed the data using a *t*-test. We repeated this procedure 500 times in total. We then carried out the same overall procedure on a further 999 simulated datasets, to ensure the generality of the results.

As there is no difference between the conditions, the distribution of the resulting half a million *p* values should be roughly uniform between 0 and 1 (as *p* values are uniformly distributed under the null hypothesis). However, when we combined skewed data with a by-condition exclusion criterion, we instead found a spike of *p* values close to 0, i.e., an increase in the rate of false positives. This can be seen in the density plots of p values in Figure 8. The solid black line in Figure 8a shows the resampled p values from a skewed dataset combined with a by condition exclusion, and it clearly spikes close to 0. The dashed line shows results from the same simulated dataset, but analyzed without the by-condition exclusion procedure, and it does not spike close to 0. Figure 8b shows what happens when the same skewed data are log transformed prior to analysis. In this case, the high rate of false positives disappears, whether or not data are excluded. These qualitative impressions were confirmed through statistical analyses. We first used one-sample Kolmogorov-Smirnov tests to assess whether the distributions of *p* values in Figures 8a and 8b diverged from uniformity. When the data was untransformed and the by-condition exclusion was applied, then the resulting distribution significantly diverged from uniformity ($D=0.08$ $p<.001$), but when the by-condition exclusion was not applied, then the resulting distribution did not significantly diverge from uniformity ($D=0.002$, $p=.18$). When the data was log transformed prior to analysis, then the resulting distributions did not differ from uniformity whether the exclusion criteria were applied ($D=0.002$, $p=.14$) or not ($D=0.0008$, $p=.92$). We then used two-sample Kolmogorov-Smirnov tests to assess whether the two distributions of *p* values in each of Figures 8a and 8b differed. When the data was untransformed, the two distributions significantly differed ($D=0.08$, $p<.001$), but when the data was log transformed, the two distributions did not significantly differ ($D=0.001$, $p=.73$).

In sum, combining a by condition exclusion procedure with skewed data appears to enhance the false positive rate. This effect could potentially explain the marginal results we found in our own datasets: Figures 8c-f apply the same resampling logic to the data from Study 1 and its replication in Study 6 (for each study, we shuffled condition labels, excluded data, and conducted *t*-tests 5000 times to generate a large number of *p* values). For both studies, there is a spike in *p* values when the by-condition exclusion is applied to positively skewed data, and this spike is less prominent when the data have been log transformed. Applying a by-condition exclusion significantly affected the distribution of *p* values when data were positively skewed (Study 1: D=0.14, $p<.001$; Study 6: D=0.073, $p<.001$) but not when data were log transformed (D=0.025, $p=.08$; D = 0.01, $p=.96$). This raises the possibility that other studies that use raw response times in their analyses of b-CFS data may have been similarly affected.

To test this, we reanalyzed the data from Sklar et al (2012)'s Experiments 1 and 2, which served as the basis for our own Studies 1 and 6. As expected, the response time data from that paper had positive skew; in fact the skew was much greater (9.5) than in our own Study 1 (where skew was 2.5). There was a significant difference between conditions when Experiment 1 (n=31 participants) was analyzed with the by-condition exclusion procedure ($\underline{M}_{control}$=959.1ms(SD=203.1), $\underline{M}_{violation}$=939.9ms(202.8), $t(30)=2.4$, $p=.02$) but not when it was analyzed without the exclusion procedure ($\underline{M}_{control}$=984.5ms(261.8), $\underline{M}_{violation}$=968.6ms(266.7), $t(30)=1.7$, $p=.11$). A log transformation reduced positive skew, but did not eliminate it (1.5), and so we similarly found effects of condition with a by-condition exclusion procedure ($\underline{M}_{control}$=6.83(SD=0.19), $\underline{M}_{violation}$=6.82(0.19), $t(30)=2.2$, $p=.04$) but not without ($\underline{M}_{control}$=6.84(0.21), $\underline{M}_{violation}$=6.83(0.21), $t(30)=1.6$, $p=.12$). However, a reciprocal transformation reduced positive skew entirely (in fact introducing slight negative skew of -0.34), and under this transformation there was no effect of condition either with the by-condition exclusion ($\underline{M}_{control}$=0.0011(SD=0.0002), $\underline{M}_{violation}$=0.001 (0.0002), $t(30)=1.4$, $p=.18$) or without ($\underline{M}_{control}$=0.001(0.0002), $\underline{M}_{violation}$=0.001(0.0002), $t(30)=1.3$, $p=.21$). Interestingly, when Experiment 2 was re-analyzed (n = 21 participants), the comparison between conditions was significant whether the by-condition exclusion procedure was applied ($\underline{M}_{control}$=1107 (348), $\underline{M}_{violation}$=1063(318), $t(20)=2.7$, $p=.01$) or not ($\underline{M}_{control}$=1148 (434), $\underline{M}_{violation}$=1091(364), $t(20)=2.3$, $p=.03$), and the same held whether or not logarithmic or reciprocal transformations were applied (all $p<.05$). These results thus do leave open some possibility that perhaps, in Hebrew, unconscious sentence processing is possible.

Nevertheless, given the results of these simulations, we recommend that response time data from breaking Continuous Flash Suppression experiments always be analyzed with a transformation to reduce skew (e.g., a log or reciprocal transformation), and we strongly caution against using by-condition exclusion procedures, because this increases the potential for false positives.

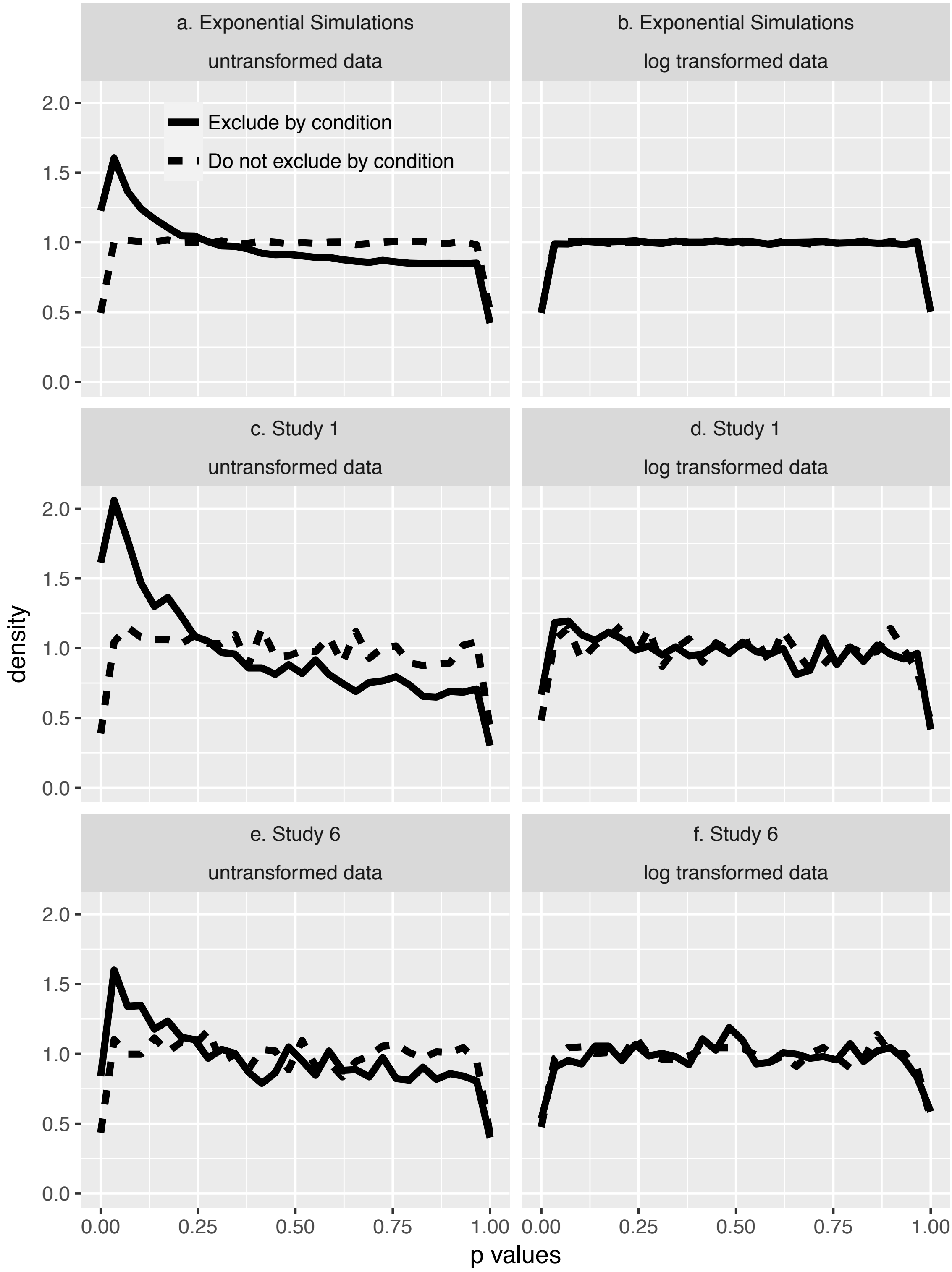

Figure 8. Density plot of *p* values generated through simulations. Panels (a) and (b) show the distributions of 500,000 simulated *p* values, panels (c) and (d) each show distributions of

5,000 *p* values from resampling of Study 1, panels (e) and (f) show distributions of 5,000 *p* values from resampling of Study 6.

Evidence for unconscious sentence processing from other methods.

Our data provide no evidence that the meanings of English sentences and words are processed once they have been rendered unconscious through continuous flash suppression. This could be because consciousness is required for processing sentences, or because the present method – breaking continuous flash suppression – is not an adequately sensitive measure. Indeed, evidence for unconscious sentence processing has been uncovered using CFS in different ways (i.e., other than b-CFS), or through using other techniques altogether. Axelrod, Bar, Rees and Yovel (2015) used CFS to mask sentences that were presented during fMRI scans, and found activity in left hemisphere language areas. Van Gaal et al. (2014) and Armstrong and Dienes (2013) used visual masking to investigate the processing of multiword expressions, and found that the meanings of such masked expressions influenced both measures. Finally, Batterink and Neville (2013) used ERPs to show that readers automatically calculate whether words fit their grammatical context, even when the word is read during an attentional blink (which, unlike CFS or masking, does not render the word invisible, but rather unattended).

However, we suggest that much of this evidence is in fact amenable to other interpretations, and should not be taken as conclusive demonstrations that combinatorial semantic and syntactic processing occur without awareness. For example, Axelrod et al (2015) compared the fMRI response to suppressed sentences with the response to suppressed strings of randomly combined letters. Such stimuli differ in a multitude of ways: random strings of letters lack syntax and semantics, but also fail to obey the orthographic properties of their language and lack familiarity, and it would be premature to conclude that it was the high-level properties that drove their finding.

Other work, relying on temporal masking, has used more minimal linguistic contrasts as stimuli, but remains hard to interpret. Van Gaal et al (2014) investigated how behavioral and ERP responses to valenced nouns (e.g., *peace*, *murder*) were affected by unconscious multi-word primes (e.g., is the response to *murder* differentially affected by the primes *not good* versus *very good*). However, while these primes did have a small effect on the ERP response to the valenced words, it is unclear what, precisely, the cause of that ERP effect might have been, because the prime did not have any effect on participants' behavioral response even when they were presented consciously. Indeed, it is unclear whether the negated phrases could even have been processed during the time window of the ERP analysis: While van Gaal et al found that negation affected the ERP response within 500ms, prior work has shown that the conscious processing of a negated phrase is extremely slow, and takes longer than 500ms unless the discourse context leads the reader to expect that the phrase will contain negation (Clark & Chase, 1972; Nieuwland & Kuperberg, 2008). In van Gaal et al's (2014) experiment there was no discourse context, and so no expectation of negation.

The van Gaal (2014) work is also subject to a recently raised methodological criticism (Sand & Nilsson, 2016; Vadillo, Konstantinidis, & Shanks, 2016), that the study's explicit measure of awareness lacked statistical power compared to the main manipulation (80 trials in the awareness test, more than 1000 trials in the main experiment), implying that some participants were likely to have been aware of the stimuli, but failed to statistically demonstrate this awareness in the awareness test. A similar point can be raised about Armstrong and Dienes (2013), who also examined the effect of subliminal negation, and also used a low power procedure to measure individual masking thresholds. In addition, the threshold in their studies was defined subjectively rather than objectively, which raises the possibility that participants applied overly stringent criteria to reporting awareness under conditions of degraded visibility, but were actually aware, to an extent, of the masked stimuli.

Finally, Batterink and Neville (2013) suggested that *syntactic* processing might occur without awareness. Participants read sentences while simultaneously completing a cross-modal distraction task, such that they frequently failed to notice grammatical errors (e.g., *we drank Lisa's by brandy…*). Interestingly, the early ERP response to the ungrammatical word (*by*) was enhanced even when participants failed to notice the grammatical error. Batterink and Neville argued that this was indicative of unconscious syntactic processing (e.g., detecting that *by* is ungrammatical in its syntactic context) because early-occurring ERP components have traditionally been proposed to index modular syntactic processing (Friederici, 2002; Friederici, Pfeifer, & Hahne, 1993; Neville, Nicol, Barss, Forster, & Garrett, 1991). However, those traditional interpretations have recently been challenged. One alternative interpretation argues that these early components instead index mismatches between predicted sensory input and actual sensory input (e.g., the wordform *by* is visually surprising in a context in which a noun was expected, because this wordform is only used as a preposition, Dikker, Rabagliati, Farmer, & Pylkkänen, 2010; Dikker, Rabagliati, & Pylkkänen, 2009; Kim & Lai, 2012); under this account, the ERP response to *by* does not reflect combinatorial syntactic processing, but a sensory prediction error. A second, more deflationary account, has argued that early ERP responses to ungrammatical words might also result from artefacts caused by how ERP data are baseline corrected (Steinhauer & Drury, 2012). Importantly, if responses to the preceding context words are used as a baseline, but those words are processed in systematically differently ways (e.g., because they are drawn from different parts of speech), then this type of correction can potentially cause a downstream difference in the ERP response to the target. Note that the three accounts mentioned here are not necessarily exclusive, as it is possible that, e.g., both prediction errors and baseline correction could explain the quite long-lasting effects that Batterink and Neville found, but the existence of such diverse interpretations indicates that the precise cognitive implications of these ERP results remain unclear.

Moving away from sentence processing, our results are also inconsistent with some of the conclusions from a recent paper by Eo et al. (2016). They found that a consciously presented prime did not affect participants' behavioral responses to a CFS-masked word, but did affect the N400 ERP component – although only when participants' attention was directed away from that suppressed word. Eo et al. argue that this feature – focusing attention away from the suppressed stimulus – may account for studies in which semantics affects CFS suppression times (as in Sklar et al., 2012 as well as Costello et al., 2009 and Yang &Yeh, 2011), because in those studies participants did not know the location of the suppressed stimulus and could not attend to it. However, participants in our experiments also did not know the location of the suppressed stimuli, and yet showed no evidence that they had accessed the meanings of those stimuli. In addition, Eo et al's proposed mechanism predicts increased breakthrough of unattended stimuli into awareness, which they do not report. We suggest that Eo et al's finding, while extremely interesting, require replication, as the effect of attention on semantic access has only been shown in a single study with a relatively small sample size (an n of 24).

Our arguments against unconscious sentence processing may well be rather deflationary, but they are also consistent with a growing body of work that has questioned the reliability and interpretation of studies of unconscious high level cognition (Hesselmann, Darcy, Sterzer, Knops, 2015; Hesselmann & Knops, 2014; Moors, Boelens, van Overwalle, & Wagemans, 2016; Newell & Shanks, 2014; Shanks, 2016; Vadillo et al., 2016), and specifically of Hassin's (2013) proposed "Yes It Can" principle (Hesselmann & Moors, 2015). Given this, we suggest that there is currently no strong reason to believe that syntactic and combinatorial semantic processing can occur without awareness. The data that we have collected and reviewed are either not consistent with combinatorial processing, or they can be equally well explained by "classical" theories of consciousness (Baars, 1997, 2005), in which complex tasks such as sentence processing do in fact require awareness, due to their requirements for global processing and sustained access to working memory.

Still, hypotheses such as the "Yes It Can" principle (Hassin, 2013), as well as other work demonstrating previously unexpected computational power in the unconscious mind (see Dehaene, Charles, King, & Marti, 2014 for review), are interesting, important, and deserving of further test and scrutiny. As such, we wish to end by acknowledging the collegial and friendly interaction we have had with Ran Hassin and his research group (the authors of Sklar et al., 2012), and their generosity in sharing materials, code scripts and data with us in the process of carrying out this work; their openness to discussion of our findings, and advice on our methods, have been consistent with the highest scientific standards (even if we remain in disagreement about the conclusions). In the long run, it will be the cumulative evidence collected by the community as a whole, rather than any individual set of results, that will yield a consensus on the boundary conditions for unconscious processing.

Acknowledgments

This work was funded by an ESRC Future Research Leaders award (No. ES/L01064X/1) to HR. Many thanks to Daisy King and Dani Mathieu for help collecting data.

Supplementary Materials

All experimental presentation scripts, data and analyses can be found at https://github.com/hughrabagliati/CFS_Compositionality

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Appendix

Study 1

Violation condition	Control A	Control B	Filler
I broke the water	I heated the water	I broke the glass	I baked the pie
I drank the chair	I moved the chair	I drank the chocolate milk	I cooked the tomatoes
I crumpled the river	I photographed the river	I crumpled the paper	I imagined the well
I shampooed the key	I found the key	I shampooed the hair	I imagined the border
I laundered the refrigerator	I opened the refrigerator	I laundered the scarf	I lifted the squash
I lit the rain	I drew the rain	I lit the lamp	I changed the sock
I wore the way	I looked for the way	I wore the gloves	I changed the computer
I built the rice	I cooked the rice	I built the building	I prepared the stew
I smelled the melody	I played the melody	I smelled the pie	I sweetened the juice
I scrubbed the thunder	I heard the thunder	I scrubbed the house	I sweetened the chocolat milk
I cut the sand	I sprinkled the sand	I cut the string	I heard the sound
I spilled the moon	I photographed the moon	I spilled the juice	I saw the light
I turned off the door	I closed the door	I turned off the computer	I sold the string
I burnt the clouds	I saw the clouds	I burnt the stew	I wore the vest
I locked the ground	I watered the ground	I locked the cabinet	I warmed up the food
I gathered the shower	I fixed up the shower	I gathered the dice	I threw the bread
I swallowed the shed	I fixed up the shed	I swallowed the food	I watered the plant

I cut the wine	I tasted the wine	I cut the bread	I fixed the gloves
I woke up the steam	I wiped the steam	I woke up the child	I liked the scarf
I interviewed the vinegar	I tasted the vinegar	I interviewed the woman	I found the exercise
I solved the peas	I threw the peas	I solved the exercise	I cleaned the house
I tied the chasm	I found the chasm	I tied the shoelace	I cleaned the floor
I tore the juice	I wiped the juice	I tore the map	I drew the map
I broke the hat	I moved the hat	I broke the border	I closed the cabinet
I washed the dance	I liked the dance	I washed the floor	I met the child
I planted the hammer	I lifted the hammer	I planted the plant	I met the woman
I sewed the smoke	I smelled the smoke	I sewed the sock	I scattered the dice
I fried the shirt	I wore the shirt	I fried the squash	I painted the building
I dug the paint	I looked for the paint	I dug the well	I colored the paper
I cooked the stairs	I fixed the stairs	I cooked the tomatoes	I smelled the hair
I turned up the bread	I baked the bread	I turned up the volume	I bought the clothes
I ironed the coffee	I made the coffee	I ironed the clothes	I bought the shoe lace
I folded the diamond	I sold the diamond	I folded the vest	I washed the cup
I shaved the glue	I spread the glue	I shaved the head	I washed the head

Study 2

Violation	**Control**
The ball squeezed Josh	Josh squeezed the ball
The room tidied Sam	Sam tidied the room
The tomato sliced Liz	Liz sliced the tomato
The book read Sean	Sean read the book
The floor cleaned Tim	Tim cleaned the floor
The garlic crushed Erin	Erin crushed the garlic
The plans cancelled Ian	Ian cancelled the plans
The fruits mixed Jack	Jack mixed the fruits
The money offered Matt	Matt offered the money
The room searched Jen	Jen searched the room
The tree hugged Amy	Amy hugged the tree
The lolly sucked Leah	Leah sucked the lolly
The kettle got Lily	Lily got the kettle
The parcel mailed Rob	Rob mailed the parcel
The lesson taught Eve	Eve taught the lesson
The museum visited Mary	Mary visited the museum
The fence jumped Jon	Jon jumped the fence
The milk sniffed Phil	Phil sniffed the milk
The bucket lent Max	Max lent the bucket
The paper cut Jon	Jon cut the paper
The spoon licked Will	Will licked the spoon
The pill swallowed Luke	Luke swallowed the pill

The mirror kissed Max	Max kissed the mirror
The papers filed Lara	Lara filed the papers
The jumper wore Joe	Joe wore the jumper
The trolley pushed Matt	Matt pushed the trolley
The herbs chopped Phil	Phil chopped the herbs
The potatoes mashed Will	Will mashed the potatoes
The coins polished Jon	Jon polished the coins
The folder carried Dan	Dan carried the folder
The toffee chewed Ben	Ben chewed the toffee
The bushes trimmed Jon	Jon trimmed the bushes
The movie watched Ella	Ella watched the movie
The puzzle solved Joe	Joe solved the puzzle
The picture painted Ryan	Ryan painted the picture
The card posted Erin	Erin posted the card
The laptop fixed Jen	Jen fixed the laptop
The sofa sold Carl	Carl sold the sofa
The bike kicked Matt	Matt kicked the bike
The table wiped Beth	Beth wiped the table
The fur stroked Rob	Rob stroked the fur
The pen described John	John described the pen
The towels passed Phil	Phil passed the towels
The cake iced Mary	Mary iced the cake
The lever pulled Hugh	Hugh pulled the lever
The house built Kate	Kate built the house
The shelf dusted Jake	Jake dusted the shelf
The sheets counted Bill	Bill counted the sheets
The letter typed Phil	Phil typed the letter
The wall punched Jane	Jane punched the wall
The thread measured Amy	Amy measured the thread
The wine stored Leo	Leo stored the wine
The photo took Beth	Beth took the photo
The garden watered Jeff	Jeff watered the garden
The shirt ironed Josh	Josh ironed the shirt
The bowl scrubbed Liz	Liz scrubbed the bowl
The dishes washed Mark	Mark washed the dishes
The room entered Ryan	Ryan entered the room
The bill paid Mike	Mike paid the bill
The juice drank Matt	Matt drank the juice
The jar labeled Liz	Liz labeled the jar
The eggs whisked Dan	Dan whisked the eggs
The number dialed Tom	Tom dialed the number
The flour weighed Jen	Jen weighed the flour
The cheese grated Andy	Andy grated the cheese

The scarf knitted Tom	Tom knitted the scarf
The ham ate Tim	Tim ate the ham
The hill climbed Luke	Luke climbed the hill
The box lifted Kate	Kate lifted the box
The dish served Kate	Kate served the dish
The lorry dented Lily	Lily dented the lorry
The teeth brushed Carl	Carl brushed the teeth
The handle pulled Tim	Tim pulled the handle
The story wrote Ewan	Ewan wrote the story
The gun carried Ryan	Ryan carried the gun
The lemon squeezed Jay	Jay squeezed the lemon
The garage tidied Erin	Erin tidied the garage
The onion sliced Liam	Liam sliced the onion
The poster read Luke	Luke read the poster
The bath cleaned Jack	Jack cleaned the bath
The ice crushed Josh	Josh crushed the ice
The party cancelled Phil	Phil cancelled the party
The nuts mixed Liam	Liam mixed the nuts
The sweets offered Tom	Tom offered the sweets
The internet searched Max	Max searched the internet
The pillow hugged Kate	Kate hugged the pillow
The candy sucked Sam	Sam sucked the candy
The radio got Ben	Ben got the radio
The invite mailed Ian	Ian mailed the invite
The rules taught Josh	Josh taught the rules
The hospital visited Tom	Tom visited the hospital
The hedge jumped Sara	Sara jumped the hedge
The perfume sniffed Tim	Tim sniffed the perfume
The car lent Fred	Fred lent the car
The ribbon cut Lucy	Lucy cut the ribbon
The plate licked Jane	Jane licked the plate
The gum swallowed Owen	Owen swallowed the gum
The rose kissed Hugh	Hugh kissed the rose
The report filed Ian	Ian filed the report
The dress wore Kate	Kate wore the dress
The button pushed Ian	Ian pushed the button
The carrots chopped Fred	Fred chopped the carrots
The turnip mashed Ella	Ella mashed the turnip
The trophy polished Anna	Anna polished the trophy
The kettle carried John	John carried the kettle
The fat chewed Adam	Adam chewed the fat
The hair trimmed Bill	Bill trimmed the hair
The opera watched Adam	Adam watched the opera

The problem solved Phil	Phil solved the problem
The portrait painted Jay	Jay painted the portrait
The gift posted Jack	Jack posted the gift
The radio fixed Luke	Luke fixed the radio
The bed sold Jay	Jay sold the bed
The door kicked Max	Max kicked the door
The desk wiped Lucy	Lucy wiped the desk
The blanket stroked Ryan	Ryan stroked the blanket
The church described Erin	Erin described the church
The salt passed Leo	Leo passed the salt
The donut iced Joe	Joe iced the donut
The rope pulled Emma	Emma pulled the rope
The shed built Rose	Rose built the shed
The house dusted Alex	Alex dusted the house
The shoes counted Lucy	Lucy counted the shoes
The message typed Eve	Eve typed the message
The monitor punched Erin	Erin punched the monitor
The curtains measured Rob	Rob measured the curtains
The rice stored Fred	Fred stored the rice
The ticket took Tim	Tim took the ticket
The plants watered Luke	Luke watered the plants
The trousers ironed Eve	Eve ironed the trousers
The pot scrubbed Josh	Josh scrubbed the pot
The pan washed Beth	Beth washed the pan
The office entered Joe	Joe entered the office
The debt paid Zoe	Zoe paid the debt
The beer drank Jack	Jack drank the beer
The box labeled Ian	Ian labeled the box
The cream whisked Tom	Tom whisked the cream
The hotline dialed Hugh	Hugh dialed the hotline
The sugar weighed Liz	Liz weighed the sugar
The carrots grated Tom	Tom grated the carrots
The socks knitted Ryan	Ryan knitted the socks
The steak ate Mike	Mike ate the steak
The stairs climbed Ben	Ben climbed the stairs
The crate lifted Amy	Amy lifted the crate
The dessert served Fred	Fred served the dessert
The bowl dented Ian	Ian dented the bowl
The hair brushed Dan	Dan brushed the hair
The rope pulled Andy	Andy pulled the rope
The report wrote Alex	Alex wrote the report
The wallet carried Leo	Leo carried the wallet

Study 3

Stimulus	Condition	Source	Mean Valence
black head	Negative phrase	New	-1.952380952
computer bug	Negative phrase	New	-2.731707317
cow pat	Negative phrase	New	-1.090909091
dentist drill	Negative phrase	New	-2.545454545
frost bite	Negative phrase	New	-3.534883721
kidnap	Negative phrase	New	-4.302325581
missing teeth	Negative phrase	New	-2.790697674
runny nose	Negative phrase	New	-2.325581395
spotty face	Negative phrase	New	-1.840909091
water boarding	Negative phrase	New	-3.139534884
bad breath	Negative phrase	Sklar	-2.933333333
black eye	Negative phrase	Sklar	-2.860465116
concentration camp	Negative phrase	Sklar	-4.395348837
electric chair	Negative phrase	Sklar	-3.813953488
facial hair	Negative phrase	Sklar	-0.136363636
foot fungus	Negative phrase	Sklar	-3.488372093
human trafficking	Negative phrase	Sklar	-4.658536585
overdose	Negative phrase	Sklar	-3.833333333
root canal	Negative phrase	Sklar	-3.511111111
skinhead	Negative phrase	Sklar	-3.285714286
stomach pump	Negative phrase	Sklar	-3
upside-down car	Negative phrase	Sklar	-2.840909091
vegetative state	Negative phrase	Sklar	-3.465116279
weak knees	Negative phrase	Sklar	-2.255813953
cardboard box	Neutral phrase	New	0.090909091
desk lamp	Neutral phrase	New	0.772727273
dining table	Neutral phrase	New	1.302325581
letterbox	Neutral phrase	New	0.522727273
tin opener	Neutral phrase	New	0.555555556
traffic light	Neutral phrase	New	0.363636364
apartment building	Neutral phrase	Sklar	0.522727273
backpack	Neutral phrase	Sklar	0.744186047
body wash	Neutral phrase	Sklar	1.697674419
carpark	Neutral phrase	Sklar	0.181818182
clothes closet	Neutral phrase	Sklar	0.636363636
cogwheel	Neutral phrase	Sklar	0.214285714
computer screen	Neutral phrase	Sklar	0.886363636
front door	Neutral phrase	Sklar	0.88372093
gear shift	Neutral phrase	Sklar	0.325581395
hand rail	Neutral phrase	Sklar	0.681818182
helicopter pad	Neutral phrase	Sklar	0.674418605

high ceiling	Neutral phrase	Sklar	0.813953488
ironed shirt	Neutral phrase	Sklar	2.11627907
lined paper	Neutral phrase	Sklar	0.533333333
paper clip	Neutral phrase	Sklar	0.431818182
pedestrian crossing	Neutral phrase	Sklar	0.613636364
speed bumps	Neutral phrase	Sklar	-0.666666667
tile roof	Neutral phrase	Sklar	0.511111111
windshield	Neutral phrase	Sklar	0.454545455
wooden stool	Neutral phrase	Sklar	0.292682927

Study 4

Stimulus	**Semantics**	**MeanAffectivity**
The nurse injected the patient	Neutral sentence	-0.511627907
Bobby ate the boar	Neutral sentence	-0.955555556
The fireman killed the fire	Neutral sentence	3.545454545
The toddler slapped dad	Neutral sentence	-2.585365854
Harry caught the octopus	Neutral sentence	0.181818182
Baby chased the dog	Neutral sentence	0.414634146
The elephant caught the poacher	Neutral sentence	1.272727273
The officer crushed the car	Neutral sentence	-3.222222222
The baby hit the brick	Neutral sentence	-2.348837209
The dog jumped on Steve	Neutral sentence	-0.909090909
Claire dragged the car	Neutral sentence	-1.414634146
The baby kicked john	Neutral sentence	-1.75
The baby hit Mum	Neutral sentence	-2.046511628
The kitten chewed the lawnmower	Neutral sentence	-1.159090909
Sam hit the ball	Neutral sentence	1.931818182
Andy cut the paper	Neutral sentence	0.186046512
The pig was a male	Neutral sentence	0.071428571
The photographer shot the actor	Neutral sentence	-1.488372093
The bridge went over Claire	Neutral sentence	-1.113636364
The journalist followed the attacker	Neutral sentence	0.045454545
The bunny outwitted the hunter	Neutral sentence	2.681818182
The soldiers captured the terrorists	Neutral sentence	2.558139535
The cat licked Sally	Neutral sentence	2.255813953
The people ate the fish	Neutral sentence	1.404761905
The child scared the dog	Neutral sentence	-1.136363636
The child spun the washer	Neutral sentence	-0.068181818
The policeman tackled the criminal	Neutral sentence	2.022727273
The man ate the snake	Neutral sentence	-1.219512195
The patient injected the nurse	Negative sentence	-3.511627907
The boar ate Bobby	Negative sentence	-4.658536585
The fire killed the fireman	Negative sentence	-4.837209302

Dad slapped the toddler	Negative sentence	-4.363636364
The octopus caught Harry	Negative sentence	-3.146341463
The dog chased baby	Negative sentence	-2.833333333
The poacher caught the elephant	Negative sentence	-4.048780488
The car crushed the officer	Negative sentence	-4.581395349
The brick hit the baby	Negative sentence	-4.243902439
Steve jumped on the dog	Negative sentence	-2.311111111
The car dragged Claire	Negative sentence	-4.627906977
John kicked the baby	Negative sentence	-4.522727273
Mum hit the baby	Negative sentence	-4.195121951
The lawnmower chewed the kitten	Negative sentence	-4.744186047
The ball hit Sam	Negative sentence	-2.644444444
The paper cut Andy	Negative sentence	-2.022727273
The male was a pig	Negative sentence	-2.23255814
The actor shot the photographer	Negative sentence	-3.911111111
Claire went over the bridge	Negative sentence	-1
The attacker followed the journalist	Negative sentence	-4.023255814
The hunter outwitted the bunny	Negative sentence	-1.386363636
The terrorists captured the soldiers	Negative sentence	-4.317073171
Sally licked the cat	Negative sentence	-1.048780488
The fish ate the people	Negative sentence	-3.933333333
The dog scared the child	Negative sentence	-2.80952381
The washer spun the child	Negative sentence	-3.536585366
The criminal tackled the policeman	Negative sentence	-3.840909091
The snake ate the man	Negative sentence	-4.511627907

Study 5

Stimulus	**Condition**	**MeanAffectivity**
מנת יתר	Negative Hebrew Phrase	-3.54
ליל הבדולח	Negative Hebrew Phrase	-3.11
לקפוץ מהגג	Negative Hebrew Phrase	-2.7
מצב צמח	Negative Hebrew Phrase	-3.85
נרדם בנהיגה	Negative Hebrew Phrase	-3.85
מגולח ראש	Negative Hebrew Phrase	-2.15
סוף העולם	Negative Hebrew Phrase	-2.25
סחר בנשים	Negative Hebrew Phrase	-4.1
פיטריות ברגליים	Negative Hebrew Phrase	-3.35
פיק ברכיים	Negative Hebrew Phrase	-1.74
פנס בעין	Negative Hebrew Phrase	-2.25

טיפול שורש	Negative Hebrew Phrase	-2.68
ריח מהפה	Negative Hebrew Phrase	-3.45
רכב הפוך	Negative Hebrew Phrase	-3.55
שטיפת קיבה	Negative Hebrew Phrase	-2.55
שיער פנים	Negative Hebrew Phrase	-2.05
כלבה בן	Negative Hebrew Phrase	-2.0
יתד בעין	Negative Hebrew Phrase	-4.15
מחנה ריכוז	Negative Hebrew Phrase	-4.35
תינוק בתנור	Negative Hebrew Phrase	-4.7
מנוחת עולמים	Negative Hebrew Phrase	-3.1
כיסא חשמלי	Negative Hebrew Phrase	-3.75
תשעה באב	Negative Hebrew Phrase	-2.15
שרפרף מעץ	Neutral Hebrew Phrase	0.35
ארגז חול	Neutral Hebrew Phrase	1.65
ארון בגדים	Neutral Hebrew Phrase	2.05
בניין דירות	Neutral Hebrew Phrase	0.2
רעפים גג	Neutral Hebrew Phrase	1.15
גלגל שיניים	Neutral Hebrew Phrase	-0.2
דלת כניסה	Neutral Hebrew Phrase	1.05
דף שורות	Neutral Hebrew Phrase	0.85
חולצה מגוהצת	Neutral Hebrew Phrase	1.9
ידית אחיזה	Neutral Hebrew Phrase	0.4
מגרש חנייה	Neutral Hebrew Phrase	0.2
מהדק נייר	Neutral Hebrew Phrase	0.95
מוט הילוכים	Neutral Hebrew Phrase	-0.1
מנחת מסוקים	Neutral Hebrew Phrase	0.35
מסך מחשב	Neutral Hebrew Phrase	0.75
מעבר חצייה	Neutral Hebrew Phrase	0.65
סבון גוף	Neutral Hebrew Phrase	1.95
פסי האטה	Neutral Hebrew Phrase	0.1
שמשה קדמית	Neutral Hebrew Phrase	-0.3
תיבת דואר	Neutral Hebrew Phrase	0.3
תיק גב	Neutral Hebrew Phrase	0.85

תיקרה גבוהה	Neutral Hebrew Phrase	1.45

Study 10

Condition	Word	Valence
long negative	aggravated	2.66
long negative	aggravation	2.1
long negative	allergy	3.07
long negative	anguished	2.12
long negative	annoyance	2.97
long negative	apprehension	3.03
long negative	avalanche	3.29
long negative	bankrupt	2
long negative	bastard	3.36
long negative	blackmail	2.95
long negative	blister	2.88
long negative	burdened	2.5
long negative	burglar	2.52
long negative	carcass	3.34
long negative	cigarette	2.46
long negative	cocaine	3.37
long negative	confusion	3.46
long negative	corrupt	3.32
long negative	criminal	2.93
long negative	cripple	2.89
long negative	crushed	2.21
long negative	decapitate	2.45
long negative	decompose	3.2
long negative	deformed	2.41
long negative	depressed	1.83
long negative	despise	2.03
long negative	destroy	2.64
long negative	discouraged	3
long negative	disgusting	2.96
long negative	distress	2.67

long negative	enraged	2.46
long negative	frustrated	2.48
long negative	furious	1.96
long negative	handicap	3.29
long negative	handicapped	3.23
long negative	headache	2.02
long negative	homeless	2.06
long negative	horrible	2.28
long negative	hostile	2.73
long negative	hurricane	3.34
long negative	immature	3.39
long negative	impotent	2.81
long negative	infection	1.66
long negative	invader	3.05
long negative	irritate	3.11
long negative	irritated	2.23
long negative	jealous	2.86
long negative	jealousy	2.51
long negative	mastectomy	2.89
long negative	measles	2.74
long negative	missiles	3.17
long negative	missles	3.33
long negative	mistake	2.86
long negative	murderer	1.53
long negative	nuclear	3.25
long negative	nuisance	3.27
long negative	pervert	2.79
long negative	problem	2.74
long negative	punishment	2.22
long negative	repulsed	2.48
long negative	ridicule	3.13
long negative	robbery	2.42
long negative	sadness	2.21
long negative	scalding	2.82
long negative	selfish	2.42
long negative	shoplifter	2.93
long negative	smallpox	2.52
long negative	starving	2.39
long negative	stricken	3.29
long negative	suicide	1.25
long negative	surgery	2.86
long negative	terrified	1.72
long negative	toothache	1.98

long negative	unfaithful	2.05
long negative	violent	2.29
long neutral	activate	5.46
long neutral	aggressive	5.1
long neutral	antique	5.39
long neutral	anxious	4.81
long neutral	appliance	5.1
long neutral	battleship	4.93
long neutral	blowdryer	5.31
long neutral	boyfriend	5.74
long neutral	busybody	5.17
long neutral	cabinet	5.05
long neutral	calender	5.42
long neutral	candlestick	5.43
long neutral	ceiling	5.45
long neutral	chimney	5.72
long neutral	clarinet	5.7
long neutral	cliffdiver	5.8
long neutral	cognition	5.57
long neutral	complex	5.25
long neutral	concentrate	5.2
long neutral	conquest	5.85
long neutral	consoled	5.78
long neutral	corridor	4.88
long neutral	crackers	5.8
long neutral	curtains	4.83
long neutral	elderly	4.86
long neutral	empathy	5.32
long neutral	erasure	4.84
long neutral	explosion	5.18
long neutral	freezer	4.96
long neutral	furnace	4.81
long neutral	glacier	5.5
long neutral	grafitti	4.82
long neutral	greyhound	5.42
long neutral	hairdryer	4.84
long neutral	herring	5.43
long neutral	history	5.24
long neutral	hospital	5.04
long neutral	inhabitant	5.05
long neutral	intoxicated	5
long neutral	kerchief	5.11
long neutral	ketchup	5.6

long neutral	knitting	4.94
long neutral	lightbulb	5.61
long neutral	lighthouse	5.89
long neutral	location	5.36
long neutral	machine	5.09
long neutral	masturbate	5.45
long neutral	military	5.54
long neutral	mushroom	5.78
long neutral	newspaper	5.52
long neutral	nursery	5.73
long neutral	passage	5.28
long neutral	prairie	5.75
long neutral	president	5.2
long neutral	privacy	5.88
long neutral	propeller	5.43
long neutral	religion	5.07
long neutral	reserved	4.88
long neutral	reverent	5.35
long neutral	scrotum	5.85
long neutral	society	5.03
long neutral	subject	5.04
long neutral	surgeon	5.36
long neutral	sympathy	5.33
long neutral	teacher	5.68
long neutral	temptation	4.94
long neutral	terrace	5.55
long neutral	testicles	5.38
long neutral	tourist	5.66
long neutral	treatment	5.24
long neutral	trumpet	5.75
long neutral	umbrella	5.16
long neutral	volcano	4.84
long neutral	whistle	5.81
long neutral	zealous	5.67
short negative	ache	2.46
short negative	bad	2.56
short negative	ban	3.48
short negative	bawl	3.41
short negative	bomb	2.1
short negative	burn	2.73
short negative	clot	3.13
short negative	cram	3.16
short negative	cult	2.48

short negative	dead	1.94
short negative	debt	2.22
short negative	dent	2.93
short negative	die	2.48
short negative	dumb	3
short negative	dump	3.21
short negative	end	3.44
short negative	envy	3.41
short negative	evil	3.23
short negative	exam	2.76
short negative	fail	1.79
short negative	fake	3.1
short negative	fat	2.28
short negative	fear	2.76
short negative	feud	3.07
short negative	fire	3.22
short negative	flaw	3.28
short negative	flee	3.29
short negative	flu	2.52
short negative	foul	2.81
short negative	fury	3.1
short negative	gang	2.59
short negative	germ	3.35
short negative	gun	3.47
short negative	hate	2.12
short negative	hell	2.24
short negative	hive	3.18
short negative	hurl	3.12
short negative	hurt	1.9
short negative	jail	1.95
short negative	lack	3.21
short negative	lie	2.79
short negative	lose	2.81
short negative	loss	1.89
short negative	lost	2.82
short negative	mad	2.44
short negative	mob	3.27
short negative	nag	2.9
short negative	odor	2.52
short negative	old	3.31
short negative	owe	3.25
short negative	pale	3.17
short negative	pout	2.83

short negative	pry	3.47
short negative	pus	2.86
short negative	quit	2.46
short negative	rape	1.25
short negative	rash	2.54
short negative	rat	3.02
short negative	riot	2.96
short negative	rob	2.8
short negative	rot	2.68
short negative	rude	2.5
short negative	sad	1.61
short negative	shun	2.97
short negative	sick	1.9
short negative	sin	2.8
short negative	slap	2.95
short negative	slob	3.12
short negative	slum	2.39
short negative	sob	2.03
short negative	tomb	2.94
short negative	ugly	2.43
short negative	war	2.08
short negative	wasp	3.37
short negative	weep	1.8
short neutral	arch	5.37
short neutral	area	5.45
short neutral	arm	5.34
short neutral	army	4.72
short neutral	barn	5.48
short neutral	body	5.55
short neutral	bolt	5.42
short neutral	bulb	5.17
short neutral	calf	5.39
short neutral	card	5.61
short neutral	cat	5.72
short neutral	clay	5.33
short neutral	cord	5.1
short neutral	dare	5.76
short neutral	daze	5.04
short neutral	defy	5.4
short neutral	dusk	5.4
short neutral	egg	5.29
short neutral	fact	5.55
short neutral	fad	5.08

short neutral	feat	5.62
short neutral	fern	5.1
short neutral	gate	5.52
short neutral	ham	5.31
short neutral	hat	5.46
short neutral	herd	5
short neutral	hose	5.25
short neutral	hour	5.1
short neutral	howl	4.9
short neutral	huge	5.39
short neutral	jar	5.21
short neutral	job	5.83
short neutral	jug	5.24
short neutral	jugs	5.65
short neutral	lamb	5.89
short neutral	lamp	5.41
short neutral	land	5.66
short neutral	lane	5.39
short neutral	law	5.1
short neutral	leg	5.71
short neutral	lid	5.03
short neutral	lung	5.21
short neutral	mile	5.24
short neutral	monk	5.18
short neutral	mood	5.6
short neutral	name	5.55
short neutral	net	4.8
short neutral	nose	4.71
short neutral	nun	4.93
short neutral	oath	5.57
short neutral	pail	5.04
short neutral	path	5.74
short neutral	pee	5.41
short neutral	pig	5.07
short neutral	pint	5.41
short neutral	raft	5.72
short neutral	rely	5.27
short neutral	rink	5.7
short neutral	rock	5.56
short neutral	soak	5.38
short neutral	stun	4.93
short neutral	task	4.79
short neutral	taxi	5

short neutral	tire	4.97
short neutral	tray	5.1
short neutral	tusk	5.41
short neutral	unit	5.59
short neutral	van	4.97
short neutral	vest	5.25
short neutral	vine	5.36
short neutral	week	5.35
short neutral	wolf	5
short neutral	wood	5.86
short neutral	wool	5.27
short neutral	yolk	5.48