Making it harder to 'see' meaning: The more you see something, the more its conceptual representation is susceptible to visual interference

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Abstract

Does the perceptual system for *looking* at the world overlap with the conceptual system for *thinking* about it? In two experiments (N = 403), we show that (1) when making simple semantic judgments on words, interference from a concurrent visual task scales in proportion to how much visual experience people have with the things the words refer to, and (2) when making the same judgments on the very same words, interference from a concurrent manual task scales in proportion to how much manual (but critically, not visual) experience people have with those same things. These results suggest that the meanings of frequently visually-experienced things are represented (in part) in the visual system used for actually *seeing* them, that this visually represented information is a functional part of conceptual knowledge, and that the extent of these visual representations is influenced by visual experience.

Keywords: semantic memory, concepts, embodied cognition, vision, interference

Introduction

As you search through your fridge for a bottle of salad dressing, a voice calls: "Wasn't last night's sunset amazing?" It takes you a moment to consider, and before you can respond, your partner expresses disappointment in your inability to recall a memorable evening. While your attention was certainly absorbed by your search for that elusive bottle of dressing, viewing the items in the fridge might have made it especially challenging to think about a *sunset*. Why? According to sensorimotor-based models of cognition, conceptual knowledge is grounded in the same sensorimotor systems used while *experiencing* the world (e.g., Allport, 1985; Barsalou, 1999). So, if your visual system is engaged when you are trying to think about something predominantly experienced visually, like a sunset, some of the resources that normally help you consider (or *simulate*) the sunset will be unavailable. If your partner had instead asked about something experienced more in other modalities, like the ocean breeze, responding might have been easier.

Evidence supporting sensorimotor-based models has been mounting. Brain areas involved in sensing, perceiving, and acting are also active when we read/hear language related to sensation, perception, and action (for review, see Barsalou, 2016; Meteyard et al., 2012), suggesting that these brain areas support conceptual processing. However, sensorimotor activation during conceptual processing does not necessarily show that activity in sensorimotor systems forms a functional *part of* conceptual representations.

Rather, sensorimotor activation may be simply *associated with* (or "peripheral to") conceptual processing (Mahon & Caramazza, 2008). That is, visual system activation when people think about sunsets may be a consequence, rather than a functional part, of conceptualizing sunsets. A complementary argument has been made in response to demonstrations that priming the sensory modality associated with a concept can partially activate that concept (e.g., Vermeulen et al., 2009; Helbig et al., 2010; Connell et al., 2012). That is, sensory information could, in principle, elicit a cascade of activation, which in turn activates an “abstract” concept representation (Mahon & Caramazza, 2008). Each of these arguments—that sensorimotor activation *is a consequence of* and that such activation *leads to* “abstract” concept processing—would suggest that this activation is not *part of* our concept representations.

To demonstrate unequivocally that activity in the visual system (or any sensory system) is part of a conceptual representation requires showing that when that system is disrupted (e.g., via brain damage, stimulation, or an interfering task that relies on that system), processing that concept is also disrupted. Yet despite the theoretical importance of such evidence, surprisingly little exists. Most comes from the neuropsychological literature. Patients with difficulty accessing concepts thought to primarily rely on a particular sensorimotor system (e.g., the ventral/visual-processing stream for animals and dorsal/object-related action stream for tools) tend to have brain damage affecting that system (for reviews see, Gainotti, 2000, 2015). Some studies have examined effects of interference on conceptual processing (primarily for actions or action-associated objects) in unimpaired individuals. These studies have generally found that interfering with (e.g., via a concurrent interfering task) sensorimotor activity thought to be associated with an action or object makes conceiving of it more difficult (e.g., Witt et al., 2010; Pobric et al., 2010; Yee et al., 2013; Shebani & Pulvermuller, 2013; Vukovic et al., 2017; Ostarek & Huettig, 2017 Experiment 1; but cf. Matheson et al., 2014).

However, most studies compare interference between categories of concepts (e.g., living/non-living; concrete/abstract). Thus, it remains possible that some categorical property (e.g., concreteness in Ostarek & Huettig, 2017), not sensorimotor experience, is responsible for the interference. Distinguishing among these possibilities is critical—a fundamental prediction of sensorimotor-based models is that sensorimotor *experience* with things determines the extent to which sensorimotor information is part of their representations. So, although, for example, concreteness is correlated with sensorimotor experience, it is sensorimotor experience that should account for the conceptual processing difficulty produced by sensorimotor interference.

Furthermore, if sensorimotor-based information is truly *part of* conceptual knowledge, this crucial relationship between sensorimotor experience and interference may be detectable not only during deliberate verification or retrieval of a concept’s features (as shown, e.g., by Edmiston & Lupyan, 2017, Ostarek & Huettig, 2017, and Amsel et al., 2014 for visual features, and Chrysikou, Casasanto, & Thompson-Schill, 2017, for manipulation information) but also when the conceptual task does not require accessing sensorimotor properties. Yet very few studies have obtained evidence that the relationship between experience and interference holds when the task does not require explicit retrieval of sensorimotor information (auditory: Bonner & Grossman, 2012; Trumpp et al., 2013; manipulation: Wolk et al., 2005; Yee et al., 2013). And in the visual modality this issue is almost entirely unexplored (but see Rey et al., 2017, discussed below).

Why might vision be different? One reason could be the importance of visual search. If representations of visually-experienced things share resources with the visual system, then what you are currently looking at could interfere with your ability to think about the very thing you are searching for. Animals that rely heavily on vision may therefore have developed a method of storing conceptual information that originated via visual experience in a format (one that is presumably more abstract; see Ungerleider & Mishkin, 1982) that is not susceptible to interference from the current visual percept. Indeed, some studies have failed to find evidence that concept-related visual shape information is stored in certain visual-perception regions (e.g., Yee, Drucker, & Thompson-Schill, 2010; see also Ostarek et al., 2019).

Here, we directly test whether conceptual representations of visually-experienced things share resources with the visual system. There is some evidence that they may. A tone associated with a complex visual mask delayed semantic judgements (relative to a tone associated with a blank square) on words as a function of visual experience with their referents (Rey et al., 2017). However, because an interference control was lacking (e.g., a tone associated with a complex non-visual stimulus), this study leaves open the possibility that the visually-experienced stimuli may have been particularly susceptible to interference generally. Here, we examine whether, when making simple semantic judgments on heard words, interference from a concurrent visual task (Figure 1a) scales in proportion to how much visual experience people have with the things the words refer to (Experiment 1). We also include a control interference task previously used to demonstrate the role of motor information in object representations (Yee et al., 2013). This control allows us to test whether visually-experienced things are simply unusually susceptible to interference generally (Experiment 2). Furthermore, critical words all referred to non-living things, the semantic judgment was orthogonal to amount of visual experience, and our analyses controlled for concreteness. These controls enabled testing of a fundamental prediction of sensorimotor-based models—*conceptual representations are grounded in sensorimotor experience*.

Importantly, the visual task used nonsense shapes unrelated to the appearance of the words' referents—that is, the shapes were intended to interfere with, not prime, the stimulus concepts. If the visual system supports representation of visually-experienced things, then occupying it with unrelated shapes should interfere more with thinking about visually-experienced things (e.g., *sunset*) than infrequently visually-experienced things (e.g., *breeze*).

Experiment 1

Methods

Participants. We conducted a power analysis in G*Power 3.1 based on pilot data and determined that to detect an estimated small effect size at a power of $\beta = .80$, we would need 199 participants in our within-subjects design. To account for potential experimenter and participant error, we recruited 205 undergraduate participants from the University of Connecticut. Ultimately, all participants were included in the analyses ($N = 205$). Participants were compensated with course credit, and the study was approved by the University of Connecticut Institutional Review Board.

Word stimuli. Words were rated for degree of visual experience in an online norming study. Norming participants ($N = 58$) did not partake in Experiments 1 or 2, but were drawn from the same undergraduate participation pool as participants in the main studies. They were asked to rate each word on “How much visual experience have you had with this?” on a scale from 1 (*very little*) to 7 (*very much*). Ratings were Winsorized at the 5th and 95th percentile in order to minimize the influence of extreme responses. The mean visual rating was 4.48 ($SD = 1.33$, range = 2.41–6.76). We calculated word frequency and concreteness based on Brysbaert, Kuperman, and Warriner’s (2014) norms. All of the stimuli and associated ratings can be found in the supplementary online material (SOM-U). Correlations among stimulus characteristics are shown in Table 1.

Table 1

Pearson’s Correlations among Stimulus Characteristics

	Visual rating	Manual rating	Duration	Log frequency	Concreteness
Visual rating	-				
Manual rating+	.50***	-			
Duration	.06	-.19*	-		
Log frequency	.19*	.23*	-.35***	-	
Concreteness	.63***	.19*	-.04	.10	-

+Manual ratings were collected as part of Experiment 2. * $p < .05$; ** $p < .01$; *** $p < .001$

Procedure. Participants underwent a two-phase (interference and no-interference) experiment. Under interference, participants saw an array of four nonsense geometric shapes randomly sampled from a set of 32 possible shapes (for examples, see Figure 1a). The shapes were intended to be unrelated to the concepts being elicited (i.e., the shapes were selected so as to not resemble real-world things). Then, 500 ms after the shapes appeared, while they were still on the screen, participants heard a word (e.g., *sunset*, *breeze*), upon which they performed a semantic judgment (“Is this an animal or not?”). Two seconds after the shapes appeared, they were replaced with a red cross for 250 ms. Then, a single shape appeared and participants indicated whether that shape had been present in the prior array. Figure 1a shows a schematic of the task. Half of the participants were given feedback (i.e., a ‘beep’) when they answered incorrectly on the shape-array interference task.[1] In the no-interference condition, no shapes were

[1] We were interested in exploring whether paying greater attention to the shape-array task would affect interference for visually-experienced words. Thus, we provided feedback to half of the participants in an effort to increase attention to the shape-array task. While feedback did increase response times, it did not interact with our fixed effects ($t < 1$), and so we have omitted feedback from our models.

presented: participants simply performed the word judgment, a blank screen was shown instead of the array of shapes, and then participants had to click a button when a fixation cross appeared. Phase order was counterbalanced across participants. Two-hundred forty words were presented (120 non-living things that varied with respect to visual experience, and 120 animals—the animals were unanalyzed foils, included so that half of the trials would elicit a "yes" response; we did not collect visual experience ratings on these). Importantly, the answer to the critical word judgment task is the same ("no") for all of our experimental items, i.e., the word judgment is orthogonal to amount of visual experience. The word lists presented in the interference and no-interference conditions were counterbalanced across participants. Critically, the exact same words were used (between-subjects) in the interference and no-interference conditions, so that any interference effects should not be due to properties of the words themselves.

Data analysis. Data were analyzed in R using the *lme4* package (Bates et al., 2007). The raw data and analysis scripts are available in the SOM-U. Models were constructed with visual experience with a given item (measured as a continuous variable) and interference condition (interference vs. no interference) as the primary fixed effects, and random slopes for both subject $(1 + exp\_cond|subject)$ and word $(1 + exp\_cond|word)$ as a function of interference condition, where exp_cond refers to interference condition. Word length (i.e., duration of the sound file), word frequency (log-transformed SUBTLEX frequency; Brysbaert et al., 2014), and concreteness were included in the models as control factors. The critical effect of interest was the interaction between visual experience and visual interference. This interaction, where the effect of interference increases as a function of visual experience, can be interpreted as showing an overlap in systems for performing the visual task and representing visual knowledge about a concept. While we also initially included random slopes for visual experience, concreteness, and their respective interactions (Barr et al., 2013), these models did not converge. Accordingly, we removed random effects until convergence was achieved, starting with the random slopes for the interactions, then for the continuous word-level variables, and finally the random intercepts for subject and word. We were able to retain the interference condition random slopes crossed with word and subject, which were most theoretically important to retain due to subject- and word-level variability in response to interference (see Matuscheck et al., 2017). The primary dependent measures were response time (assessed using linear mixed effects models) and accuracy (assessed using logistic mixed effects models) on the word judgment task. Because response times were measured from the onsets of the auditory words, response times < 150 ms were unlikely to reflect processing of the auditory words and were removed.

The models were constructed in three steps: (1) a control main effects model with interference condition, each of the control variables (duration, frequency, concreteness), and visual experience, (2) a control model testing the concreteness × visual interference interaction, and (3) the critical visual experience × visual interference interaction model, which tested whether visual experience accounted for visual interference *over and above* any effects of concreteness. Within a given model, we report effects as the model estimates (or for logistic models, odds ratios; OR). When OR < 1, we report the inverse odds for interpretability (simply 1/OR). For linear and logistic models, respectively, $|t|$ and $|z|$ values > 2 were considered statistically significant. Likelihood ratio tests were used to evaluate the statistical significance of each successive model, and here, p-values < .05 were considered statistically significant.

Results

Word judgment task. Descriptively, on the word judgment task, participants were faster and more accurate in the no-interference condition than in the interference condition (RTs ± 1

SD: $M = 925 \pm 222$ ms and $M = 1072 \pm 265$ ms, respectively; Accuracy ± 1 SD: $M = .988 \pm .108$ and $M = .981 \pm .183$, respectively).

The models assessed effects on these word judgments. The control main effects model showed significant effects of interference condition, such that RTs were estimated as about 161 ms slower under visual interference (95% CI [140, 182]; $\beta = 0.63$, $SE = 0.04$, $t(203) = 15.17$, $p < .001$)[2]. There was also an effect of word duration such that for each 100 ms of increased duration, RTs were about 26 ms slower (95% CI [18, 34]; $\beta = 0.11$, $SE = 0.02$, $t(112) = 6.48$, $p < .001$), and an effect of concreteness such that greater concreteness (in units concreteness on a 1–7 scale) was associated with RTs that were about 20 ms slower per unit (95% CI [4, 35]; $\beta = 0.05$, $SE = 0.02$, $t(113) = 2.48$, $p = .015$).[3] The control model testing for an interaction between concreteness and visual interference did show an interaction, such that the response-slowing effect of visual interference increased with concreteness, by about 12 ms per unit (95% CI [3, 21], $\beta = 0.03$, $SE = 0.01$, $t(108) = 2.57$, $p = .012$). A likelihood ratio test comparing the concreteness model to the model with only the main effects showed that this concreteness model was more predictive than the main effects model ($\chi^2(1) = 6.41$, $p = .011$).

The model testing our critical prediction—that visual *experience* with things influences the extent to which they are represented in the visual system—showed an interaction between

[2] Although the methods for estimating *p*-values and degrees of freedom in mixed models remain under debate (e.g., Bates et al., 2015), to facilitate comparison of our findings with existing literature, we provide approximations of these values using the *lmerTest* package (Kuznetsova, Brockhoff, & Christensen, 2017) to approximate degrees of freedom for linear models, and *p*-values for both linear and logistic models.

[3] This effect of concreteness on RTs (and the effect on accuracy described in the next paragraph, as well as the concreteness effects described in Experiment 2) runs counter to typical concreteness effects where responses are facilitated for more concrete items. This reversal is due in part to the interference condition, where it is attributable to the correlation between visual experience and concreteness (see Table 1), i.e., more concrete items also tend to be more visually experienced and so are particularly susceptible to visual interference. However, we suspect that this finding may also be partially attributable to our "animal judgment" task—many of our more concrete items (e.g., *asteroid, aquarium*) move or have moving features, making it harder to reject them as animals (Goldberg & Thompson-Schill, 2009) compared to more abstract items that do not move (e.g., *bitterness, chill*).

visual experience and visual interference, where the response-slowing effect of visual interference increased as the amount of visual experience (in units visual experience, on a 1–7 scale) increased, by about 8 ms per unit (95% CI [2, 14]; $\beta = 0.04$, $SE = 0.02$, $t(115) = 2.80$, $p = .006$). Importantly, this model was significantly more predictive than the model with the concreteness interaction alone ($\chi^2(1) = 7.67$, $p = .006$)[4]. To show the effect of visual experience, Figure 1b uses the raw data to plot, for each item, the size of the “interference effect” (RT in the interference condition – RT in the no-interference condition) against the visual experience rating for that word.

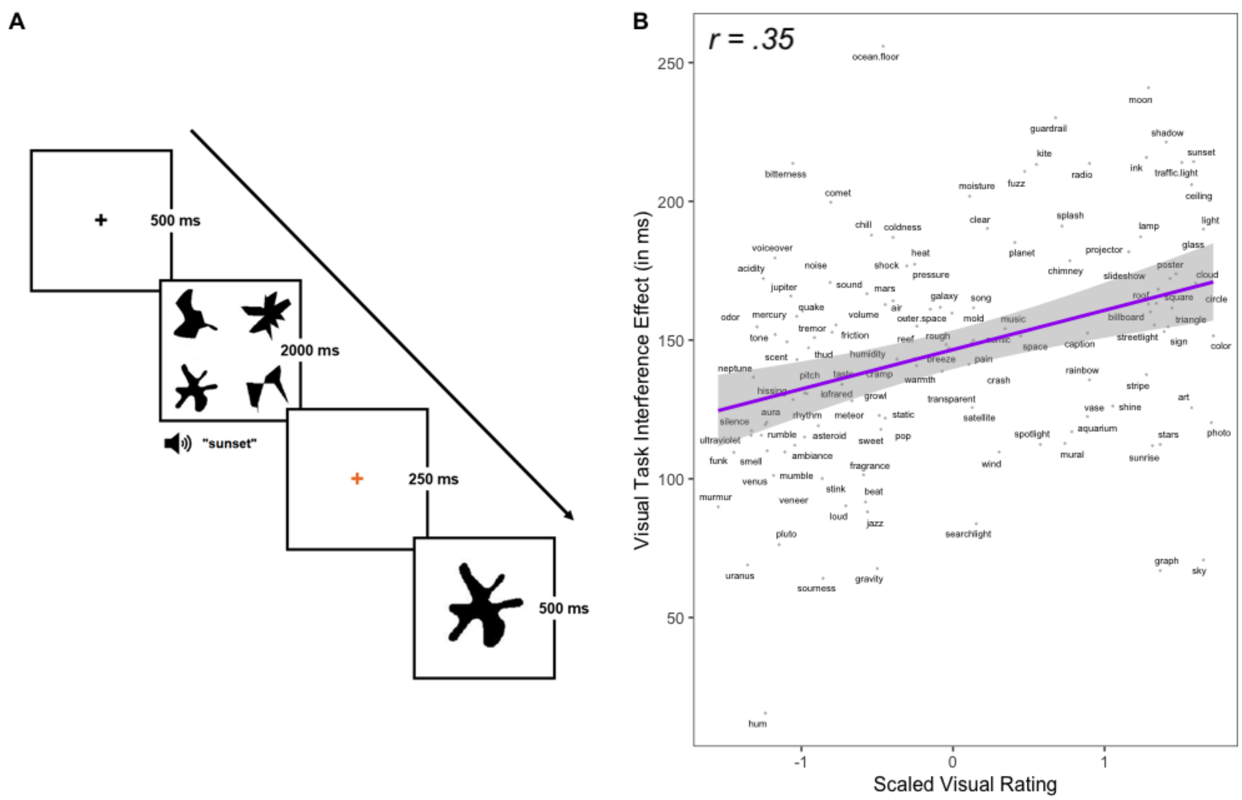

Figure 1. (a) Schematic of the interfering visual task. (b) The correlation between scaled visual experience ratings and the visual task “interference effect” (RT on word judgments in the

[4] In contrast, when we reversed models 2 and 3, such that the concreteness interaction was added to a model including the visual experience interaction, it was no more predictive than the model with the visual experience interaction alone ($\chi^2(1) = 0.05$, $p = .821$).

interference condition – RT on word judgments in the no-interference condition). Word ratings are scaled (mean-centered) for figure interpretability across experiments, individual points reflect the actual data points (word labels are displaced for visibility), and error bands show 95% confidence intervals. The superimposed correlation value is the bivariate Pearson's *r* coefficient.

The accuracy data were analyzed in the same way as the RT data.[5] There was a significant main effect of visual interference, where participants were 1.69 times less likely to respond accurately under interference (OR = .59, 95% CI [.47, .74]; $z = -4.52$, $p < .001$). There were also main effects of duration and concreteness, such that participants were 1.37 times more likely to respond accurately for every 100 ms increase in duration (OR = 1.37, 95% CI [3.25, 175.08]; $z = 3.15$, $p = .002$)) and 1.92 times less likely to respond accurately with each unit increase in concreteness (OR = 0.52, 95% CI [0.34, 0.77]; $z = -3.25$, $p = .001$). However, there were no interactions with visual interference (*z*s < 1, *ns*) and thus the accuracy data are reported here for completeness but will not be discussed further.

Visual shape judgment task. The interfering visual shape judgment task was quite difficult, as reflected by low accuracy ($M \pm 1$ SD = .656 ± .475). Using models analogous to those described above, neither accuracy nor RT ($M \pm 1$ SD = 830 ± 480 ms) on the task was predicted by visual ratings, nor were there any effects of duration, frequency, or concreteness.

Overall, Experiment 1 showed that visual interference slowed semantic judgements on words as a function of the degree of visual experience with the words' referents. This effect was attributable to visual *experience*, over and above the effect of concreteness.

[5] In models with accuracy as a dependent measure, the random slopes models did not converge. Thus, we used random intercepts in these cases. While this can lead to inflation of Type 1 error (Barr et al., 2013), other alternatives to logistic mixed effects models (e.g., separate by-subject and by-item analyses, also known as $F_1 \times F_2$ analysis) can lead to spurious results (Jaeger, 2008), and so we have used intercepts-only models. Ultimately, the results of these accuracy models have no bearing on our conclusions.

Experiment 2

Experiment 1 suggests that visual experience drives the extent to which concepts for visually-experienced things are grounded in the visual system. However, it remains possible that among the tested items, those involving more visual experience were, for some unanticipated reason, especially susceptible to interference generally (rather than particularly susceptible to visual interference). To investigate this possibility, in Experiment 2 we employed a manual interference task on the same stimuli. Including this condition also allowed us to test, for the same words, the predictions of sensorimotor-based models in a different modality. That is, that the processing difficulty elicited by manual interference should be accounted for by *manual* experience (as in Yee et al., 2013).

Methods

Participants. Participants were 198 undergraduate students from the University of Connecticut who had not participated in Experiment 1. Again, all participants were included in the analyses ($N = 198$). They were compensated with course credit.

Word stimuli. Words were the same as those used in Experiment 1. Because Experiment 2 used a manual interference task, we collected manual interference ratings to explore whether manual—rather than visual—experience ratings might explain any interference effects observed in Experiment 2. Norming participants ($N = 60$) did not partake in Experiments 1 or 2, but were drawn from the same undergraduate participant pool as participants in the main studies. They rated each word for manual experience: "How much experience have you had touching this with your hands?" Manual ratings were collected and treated in the same way as the visual ratings collected alongside Experiment 1. The mean manual rating was 2.94 ($SD = 1.43$, range = 1.00–6.22).

Procedure. In the interference condition, participants performed a concurrent manual task from Yee et al., (2013) throughout the experiment (Figure 2a) while making the word judgment with their feet on a button-box placed on the floor. In this task, participants repeatedly move their hands through a series of three simple motions performed on a table. The motions were selected by Yee et al. to be unlikely to be associated with any objects, particularly when performed as a continuous sequence, and the task has been shown to disrupt processing of concepts experienced predominantly in the manual modality (e.g., *hammer*; Yee et al., 2013). While participants performed the hand movements at their own pace, they were encouraged to speed up if they did not complete at least one cycle of the three hand movements per word. Otherwise, the procedure was identical to that used in Experiment 1, except that in the no-interference condition, like in the manual interference condition, participants used their feet to respond.

Data analysis. Data were analyzed in the same way as in Experiment 1.

Results

In Experiment 2, participants again responded more quickly and accurately on the word judgment task in the no-interference condition than in the interference condition (RTs ± 1 SD: M = 955 ± 227 ms and M = 1021 ± 284 ms, respectively; Accuracy ± 1 SD: M = .973 ± .164 and M = .949 ± .219, respectively).

In the main effects model, interference condition, duration, and concreteness had statistically significant effects on RTs. Responses were estimated to be about 77 ms slower in the interference condition (95% CI [59, 94]; $\beta = 0.30$, $SE = 0.03$, $t(205) = 8.70$, $p < .001$), and were also slower with increasing duration by about 15 ms per 100 ms of duration (95% CI [79, 231]; $\beta = 0.07$, $SE = 0.02$, $t(112) = 4.24$, $p < .001$) and with increasing concreteness by about 14 ms per unit concreteness (95% CI [0, 29]; $\beta = 0.04$, $SE = 0.02$, $t(112) = 2.01$, $p = .047$), the same pattern as observed in Experiment 1. The control model testing for an interaction between interference and concreteness ($\beta = 0.002$, $SE = 0.01$, $t(113) = .11$, $p = .912$) was no more predictive than the main effects model, $\chi^2(1) = .01$, $p = .911$.

The critical model with the *visual* experience × manual interference interaction ($\beta = 0.03$, $SE = 0.02$, $t(113) = 1.56$, $p = .121$) was, as expected, no more predictive than the concreteness interaction model, $\chi^2(1) = 2.42$, $p = .120$. This, together with follow-up analyses (described below), suggests that Experiment 1’s results did not simply reflect items involving more visual experience being especially susceptible to interference in general, rather than visual interference in particular. When analogous models were tested using errors, the same main effects were observed for accuracy, such that participants were 2.04 times less likely to respond accurately under interference (OR = 0.49, 95% CI [0.42, 0.57]; $z = -9.30$, $p < .001$), 1.16 times more likely to respond accurately per 100-ms duration (OR = 1.16, 95% CI [1.01, 1.31]; $z = 2.52$, $p = .012$), and 1.45 times less likely to respond accurately per unit concreteness (OR = 0.69, 95% CI [0.54, 0.88]; $z = -3.10$, $p = .002$). As in Experiment 1, in accuracy there were no interactions between interference (here, manual) and either concreteness, visual experience, or the manual experience measure described below, and thus, the accuracy data are reported here for completeness but not discussed further.

Returning to RTs, to learn if, in line with Yee et al. (2013), *manual* experience could explain the effect of manual interference, we tested a further interaction model with a manual experience × manual interference interaction. Consistent with the results of this prior study, this interaction was significant, such that the response-slowing induced by manual interference increased as a function of manual experience (in units manual experience, on a 1–7 scale), by about 8 ms per unit (95% CI [1.92, 13.82]; $\beta = 0.04$, $SE = 0.02$, $t(113) = 2.61$, $p = .010$). Furthermore, including this manual experience interaction made the model significantly more predictive than the model with only the concreteness and visual experience interactions, $\chi^2(1) = 6.63$, $p = .010$, and importantly, when manual experience was included, the effect of visual experience was reduced from marginal ($p = .121$) to nonexistent ($\beta = 0.005$, $SE = 0.02$, $t(114) = 0.24$, $p = .81$) suggesting that the marginal effect of visual experience was due to shared variance between manual and visual experience ratings, rather than visual experience per se. Supplementary Figure 2 (in SOM-U) shows the relation between manual interference and manual experience after the latter has been residualized on visual experience (Supplementary Figure 1, on the other hand, shows the relation between visual interference and visual experience after the latter has been residualized on manual experience).

Thus, items involving more visual experience were not as susceptible to interference in a different modality (i.e., manual; see Figure 2b) as they were to visual interference, and the non-significant positive relationship between visual experience and manual interference was entirely accounted for by manual experience. We also conceptually replicated[6] Yee et al. (2013) by showing that interference induced by a manual task increases as a function of the degree of manual experience associated with a concept (see Figure 2c).

[6] Yee et al. (2013) used different conceptualization tasks and verbal, rather than button press responses. In that study, RT effects trended in the direction observed in the current work, and significant interactions between manual interference and experience were observed in accuracy.

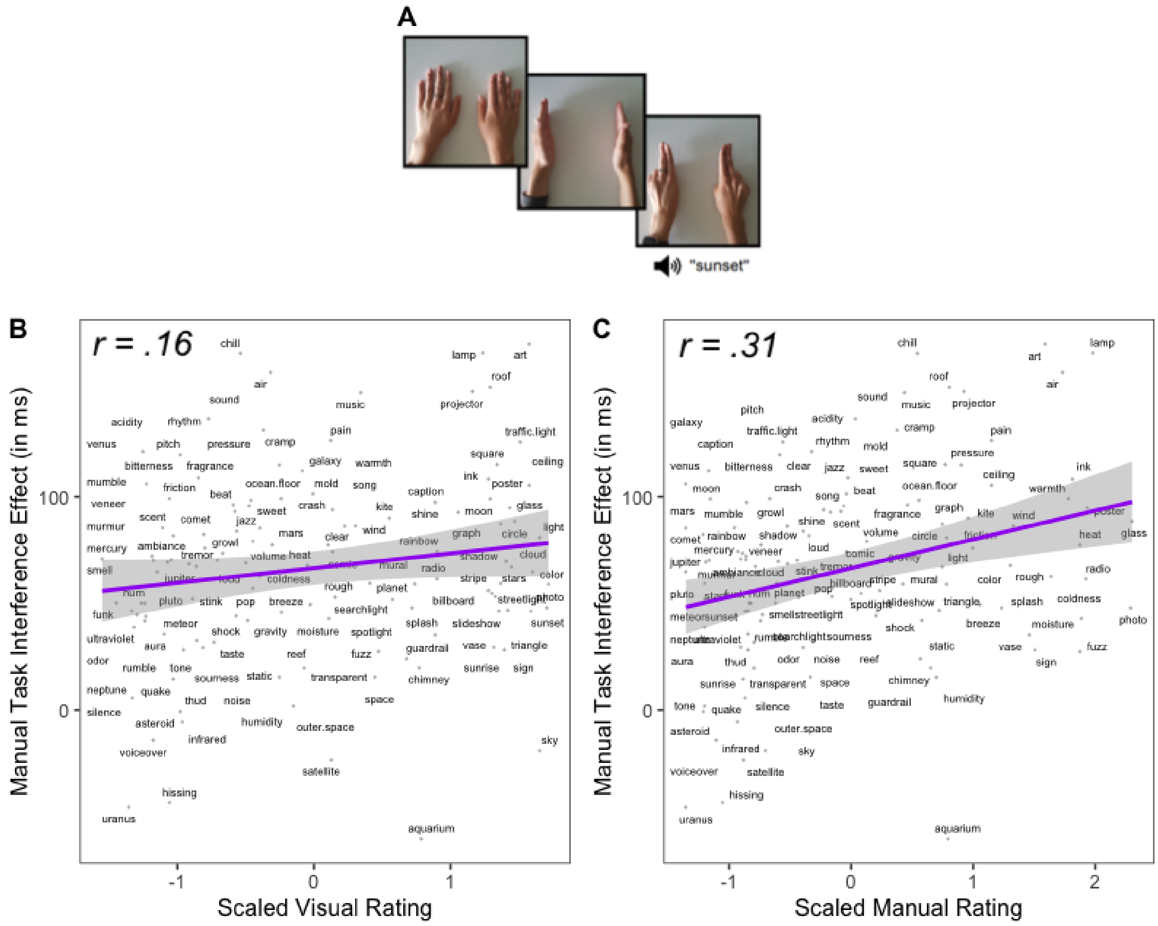

Figure 2. (a) Schematic of the concurrent manual task adapted from Yee et al. (2013). (b) The (non-significant) relation between scaled visual experience ratings and the manual task “interference effect” (again, RT to word judgments in the interference condition – RT to word judgments in the no-interference condition). (c) The relation between scaled *manual* experience ratings and the manual task “interference effect.” In (b) and (c), word ratings are scaled (mean-centered) for figure interpretability across experiments, individual points show the actual data points (word labels are displaced for visibility), and error bands reflect 95% confidence intervals. The superimposed correlation value is the bivariate Pearson’s *r* correlation for each.

For completeness, we also conducted an exploratory analysis using the manual ratings that we collected in Experiment 2 to test whether the effect of visual interference observed in Experiment 1 could be predicted by *manual* experience—our account predicts that the effect of visual interference should be better explained by experience in the same (i.e., visual) modality than by experience in a different modality. As expected, the effect of manual experience on visual interference was not significant (i.e., a response-slowing effect of about 4 ms, 95% CI [-0.69, 8.81; $\beta = 0.02$, $SE = 0.01$, $t(110) = 1.69$, $p = .093$). Further, when we used a likelihood ratio model to compare the model that included only the visual experience × visual interference interaction (described in Experiment 1) to the model that included both a visual experience interaction *and* a manual experience × visual interference interaction, the inclusion of the manual experience interaction did not significantly increase the model's fit to the data ($\chi^2(1) = 2.83$, $p = .093$).

Evaluating the evidence for modality-specificity in the effects of experience on interference

Although the effect of manual experience on visual interference was non-significant, it approached significance, raising an important question: How strong is the evidence that interference from a visual task is better predicted by visual than by manual experience?[7] To assess this, inspired by Ostarek et al. (2019), we conducted follow-up Bayesian analyses using the *brms* package in R, which allows one to use identical fixed and random effect structures to those implemented in *lme4* (Bürkner, 2017). Briefly, we used the higher-order models from Experiments 1 and 2, including both the visual and manual experience interactions in each to directly compare the visual and manual experience interactions. Because Bayesian models are less susceptible to convergence issues than linear mixed effects models, we were able to include by-participant random slopes for the interactions between our item-level variables (visual experience, manual experience, and concreteness) and interference condition. Priors for the interactions between concreteness, manual, and visual experience and interference were set

[7] We thank an anonymous reviewer for raising this key issue.

based on similar previous research (e.g., Edmiston & Lupyan, 2017; Ostarek et al., 2017, 2019; Yee et al., 2013). Importantly, to be conservative, the prior for the secondary experience variable (e.g., *manual* experience in the visual interference experiment) was positive (it was the same as that for the concreteness interaction).[8] Following Wagenmakers et al. (2010), for each fixed effect, we used the Savage–Dickey method to generate estimates and 95% credible intervals (CrIs), and then computed Bayes factors (BFs) to describe the evidence in favor of both the null (BF_{01}) and directional (BF_{dir}) hypothesis. In both cases, BFs larger than 1 indicate evidence in favor of the hypothesis (i.e., in favor of the null or directional, respectively), while BFs less than 1 indicate evidence against the hypothesis. Greater distance from 1 reflects stronger evidence, e.g., $BF_{01} = 2$ reflects two times as much evidence for the null as compared to the alternative, or $BF_{01} = 0.25$ reflects four times (1/.25 = 4) as much evidence for the alternative as compared to the null (Wagenmakers et al., 2010).

Importantly, using Bayesian analyses we can directly compare two fixed effects and evaluate the evidence that one is greater than the other. This analysis (based on 100,000 samples from the posterior distribution for each of four model chains) produced an estimate of the visual experience interaction (9.34, SE = 1.95, 95% CrI [6.11, 12.57], $BF_{01} < 0.01$, $BF_{dir} > 2000$) that was larger than that of the manual experience interaction (3.16, SE = 1.90, 95% CrI [-0.03, 6.14], $BF_{01} = 1.36$, $BF_{dir} = 18.42$). Most critically, the 95% CrI for the difference between the visual and manual experience interactions did not contain zero (estimate of difference = 6.18, SE =

[8] Specifically, we approximated the average (standardized) effect size in these studies (a small-to-medium effect), and because *brms* priors are specified in coefficient units, we translated these effect sizes to the expected *unstandardized* coefficients that would be returned by the linear mixed effects model. Priors for the main effect of interference and control variables were simply derived from the other experiment—that is, because interference in Experiment 2 produced an RT-slowing effect of 100 ms with a standard error of 10, the corresponding prior in Experiment 1 was set as 100 (10).

3.16, 95% CrI [1.13, 11.54], BF_{01} = 1.99, BF_{dir} = 34.71), providing considerable evidence that interference from a visual task is better predicted by visual than by manual experience.

Similar results (in the opposite direction) were observed for Experiment 2. The estimate of the manual experience interaction (8.91, SE = 2.10, 95% CrI [5.58, 12.44], BF_{01} < 0.01, BF_{dir} > 2000) was larger than the estimate of the visual experience interaction (-2.58, SE = 2.01, 95% CrI [-5.97, 0.66], BF_{01} = 2.34, BF_{dir} = 0.10; note that there was evidence against the directional visual experience interaction). Most importantly, the 95% CrI for the difference between the manual and visual experience interactions did not contain zero (estimate of difference = 11.48, SE = 3.34, 95% CrI [6.31, 17.05], BF_{01} = 0.09, BF_{dir} = 1999), providing strong evidence that interference from a manual task was better predicted by manual than by visual experience.

A final test of whether concepts predominantly experienced through vision are particularly susceptible to *visual* interference should compare across experiments to evaluate whether visual experience predicts visual better than manual interference. To do this, we compiled the data from Experiments 1 and 2 into one dataset, and computed an *interference effect* (RT in the interference condition – RT in the no-interference condition; as presented in Figures 1 and 2) for each item in each experiment (averaged over subjects). The model was constructed with *interference effect* as the dependent measure and experiment (visual, manual), duration, frequency, concreteness, visual and manual experience, and the interactions between concreteness, visual, and manual experience and experiment as the predictors. Priors for the control variables were set to 0 (*SE* = 1) because we did not expect them to predict interference. The prior for the concreteness interaction was also set to 0 (*SE* = 1) because it should equally predict visual and manual interference. Priors for the visual and manual experience interactions were equal to the difference between the priors used in the Bayesian analyses described above for Experiments 1 and 2 (e.g., prior for visual experience × visual interference interaction – prior for visual experience × manual interference interaction), as this difference should reflect the experience × experiment interaction. There was substantial evidence that visual experience better predicted visual than manual interference (estimate = 10.17, SE = 2.25, 95% CrI [6.57, 13.98], BF_{01} = 0, BF_{dir} > 2000), and substantial evidence that manual experience better predicted manual than visual interference (estimate = -8.05, SE = 2.33, 95% CrI [-12.80, -3.25], BF_{01} = 0.8, BF_{dir} = 665.67)

Thus, Bayesian follow-up analyses provide support for the conclusion that more visually experienced things are particularly susceptible to interference in the visual modality, and more manually experienced things are particularly susceptible to interference in the manual modality.

General Discussion

Performing a concurrent visual task slowed concept processing as a function of the *amount of visual experience* people have with a word’s referent. Returning to our opening scene, if your visual system is occupied by the complex array of items in the fridge, it will be harder to think about something with which you have substantial visual experience, like *sunset*, than something less visually-experienced, like *breeze*.

In contrast, for the same judgments on the very same words, a concurrent manual task produced a different pattern: conceptual processing slowed more as a function of *manual* experience. Thus, the relationship between visual interference and visual experience in Experiment 1 was not simply an effect of interference broadly. If you had been rolling out dough when asked to think about the sunset, you would have been no slower to think about it than the ocean breeze, as the motor system resources used for rolling out dough do not overlap with the visual resources required to think about sunsets. You might, however, be slower to think about a shovel you had used to build a sandcastle, as the motor resources for rolling out dough overlap with those that represent the meaning of *shovel*.

These findings have several implications. First, they provide strong evidence for a fundamental prediction of sensorimotor-based models (e.g., Allport, 1985; Barsalou, 1999)—conceptual representations are grounded in sensorimotor *experience*. Second, aspects of the representations of visually-experienced things share resources with the visual system. Finally—and critically—these aspects are a *functional part of* representations. While earlier neuroimaging work suggested that the visual system is *active* when processing the meaning of visually-experienced things (for review, see Meteyard et al., 2012), the present findings show that this activity plays a functional role in understanding their meaning.

Our results converge with evidence that when making explicit judgments about visual properties of things (e.g., judging whether “Does it have stripes?” applies to *tiger*), viewing a visual noise pattern is more disruptive than when making judgments about encyclopedic properties (Edmiston & Lupyan, 2017). They are also consistent with evidence that visual noise can disrupt concreteness judgments for concrete concepts more than abstract ones (Ostarek & Huettig, 2017, Experiment 1), presumably because concrete—but not abstract—things can be experienced visually. These studies, however, required judgments explicitly related to the featural dimension of interest (i.e., explicit judgments about visual properties, or concreteness judgments wherein all concrete items were highly visually imageable), whereas we show that visual experience can affect conceptual processing even when the task is orthogonal to that experience.[9] Relatedly, we show that (as also suggested by Rey et al., 2017) it is *amount of*

[9] When performing a task where conceptual processing is *not* required (e.g., lexical decision), viewing a visual noise pattern does not delay responses more for concrete than abstract words (Ostarek & Huettig, 2017, Experiments 2 & 3). We concur with Ostarek and Huettig that such task differences suggest that a concept’s visual features may not be activated in every context (for discussion of this issue, see Yee & Thompson-Schill, 2016).

visual experience, not concreteness, that determines the visual system's involvement in conceptualization.

Interestingly, in exploratory analyses, we observed a trend whereby *manual* experience with things may be associated with slowed processing under *visual* interference. While Bayesian analyses provided substantial evidence that visual interference was better predicted by visual than manual experience, the trend still merits consideration. In particular, it is important to consider which components of the visual system the visual interference task may have engaged. Given that visual information is processed in both a ventral ("what") and dorsal ("how") stream (Milner & Goodale, 1992), one possibility is that our visual task also, to some degree, occupied the "how" stream, which supports processing of object manipulability (e.g., Chao & Martin, 2000; Almeida, Mahon, & Caramazza, 2010), and interference of this dorsal stream may have driven the (trend towards) interference of our visual task on processing frequently manually-experienced things. And in fact, there is fMRI evidence that a shape task very similar to ours engages not only the lateral occipital complex, a "higher level" region of the ventral stream involved in encoding object shape, but also the superior parietal lobe, which is part of the dorsal stream (Song & Jiang, 2006). Future work should investigate whether the effects observed here persist with lower-level visual interference. They may not—during sentence comprehension, perceptual simulation is not affected by interference targeting low-level visual processing, although progressively modifying the interference task to target higher levels of the visual system appears to gradually increase its effect on perceptual simulation (Ostarek et al., 2019).

Conclusions

As is predicted by sensorimotor-based models of cognition (e.g., Allport, 1985; Barsalou, 1999), the more something is experienced visually, the more its conceptual representation shares resources with those involved in visual processing. This has the perhaps counterintuitive implication that when you are looking for something, having to scan through unrelated things could interfere with your ability to think about the very thing you are searching for. That is, we do not represent the target concept using only abstract information divorced from the visual substrates used to concurrently process visual input—rather, we rely on those substrates for its conceptual representation, in proportion to the amount of visual experience that contributed to that representation. Perhaps this contributes to why it can be so hard to keep what you wanted in mind once you open the refrigerator door and look inside.

Author Contributions

PB and EY developed the original study concept. All authors contributed to study design. CPD analyzed the data. CPD and EY wrote the manuscript. PB, GHJ, and CD contributed to revising the manuscript. All authors approve the final version of the manuscript.

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Open Practices Statement

Neither of the experiments reported in this article was formally preregistered. The data and materials are available as Supplementary Online Material. Requests for more information or clarification can be made to charles.davis@uconn.edu.

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