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Bindings Between Stimuli and Multiple Response Codes Dominate Long-Lag Repetition Priming in Speeded Classification Tasks

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Repetition priming is often thought to reflect the facilitation of 1 or more processes engaged during initial and subsequent presentations of a stimulus. Priming can also reflect the formation of direct, stimulus–response (S-R) bindings, retrieval of which bypasses many of the processes engaged during the initial presentation. Using long-lag repetition priming of semantic classification of visual stimuli, the authors used task switches between study and test phases to reveal several signatures of S-R learning in Experiments 1 through 5. Indeed, the authors found surprisingly little, if any, evidence of priming that could not be attributed to S-R learning, once they considered the possibility that stimuli are simultaneously bound to multiple, different response codes. Experiments 6 and 7 provided more direct evidence for independent contributions from at least 3 levels of response representation: the action (e.g., specific finger used), the decision (e.g., yes–no), and the task-specific classification (e.g., bigger–smaller). Although S-R learning has been discussed previously in many contexts, the present results go beyond existing theories of S-R learning. Moreover, its dominant role brings into question many interpretations of priming during speeded classification tasks in terms of perceptual–conceptual processing.

Keywords: stimulus–response learning, instances, perceptual, semantic, event files

When a stimulus is encountered in the context of a classification task, the appropriate response is normally assumed to be generated through a number of separate processes, such as perceptual identification of a visual object and retrieval of its task-relevant semantic properties. The facilitation of one or more of these processes is often assumed to be the cause of the behavioral priming (e.g., faster reaction times [RTs]) that is observed when the stimulus is repeated in the same or a related task. One useful heuristic has been the broad distinction between perceptual and conceptual processes (Blaxton, 1989; Roediger & McDermott, 1993), although it is likely that there are multiple component processes that have the potential to be facilitated (Tenpenny & Shoben, 1992; Witherspoon & Moscovitch, 1989). When the stimulus or task changes, the amount of priming is generally believed to mirror the degree of overlap between processes performed on the initial and repeated presentations (*transfer appropriate processing*; Kolers & Roediger, 1984; Morris, Bransford, & Franks, 1977; Roediger, Weldon, & Challis, 1989), related perhaps to the degree of overlap in the neural pathways traversed (Henson, 2003).

An alternative cause of priming is the formation and retrieval of direct bindings between the stimulus and the prior responses elicited by (or even just contemporaneous with) that stimulus. Such bindings might be associations between stimulus and response representations that increase or decrease in associative strength over presentations. Alternatively, every co-occurrence of the stimulus and response might be represented in a unique episode, or *instance* (e.g., Logan, 1990). Such episodes may also include additional information, such as representations of the task context (Hommel, 1998). We refer to this general view as *stimulus–response (S-R) learning*. Note that the precise nature of the stimulus (e.g., a specific visual image vs. a more abstract perceptual representation; Schnyer et al., 2007) and of the response (e.g., a specific finger press vs. a task-specific interpretation or category label; Abrams, Klinger, & Greenwald, 2002; Logan, 1990; Schnyer et al., 2007) are yet to be fully characterized (see later).

S-R learning has been shown to contribute to long-lag repetition priming of RTs and/or errors in classification tasks (e.g., Dennis & Schmidt, 2003; Schnyer et al., 2007), although its contribution is normally believed to supplement that from facilitation of component processes. S-R learning has also been used to explain aspects of short-lag, subliminal priming (Abrams et al., 2002; Damian, 2001; Kiesel, Kunde, & Hoffmann, 2006, 2007; Klauer, Eder, Greenwald, & Abrams, 2007; Kunde, Kiesel, & Hoffmann, 2003) and negative priming (Frings, Rothermund, & Wentura, 2007; MacDonald & Joordens, 2000; Rothermund, Wentura, & De Houwer, 2005). Interest in S-R learning has increased following claims that it is not exhibited by individuals with amnesia, despite their intact long-lag priming (Schnyer, Dobbins, Nicholls, Schacter, & Verfaellie, 2006), and that S-R learning may underlie the repetition-related response decreases observed by fMRI during priming paradigms (Dobbins, Schnyer, Verfaellie, & Schacter, 2004), rather than the facilitated neural processing that is typically assumed (Henson, 2003).

S-R learning effects have also been studied extensively within the context of task switching (Allport & Wylie, 1999; Koch &

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Allport, 2006; Waszak, Hommel, & Allport, 2003). These experiments show that RTs are usually slower after a task switch, the so-called *task-switch cost* (Rogers & Monsell, 1995). More important for present purposes, RTs are also modulated by the prior task and/or response history of a repeated stimulus (Allport & Wylie, 1999; Koch & Allport, 2006; Waszak et al., 2003). In particular, RTs tend to be slower (relative to novel stimuli) when a stimulus is repeated in a task that is incongruent with the task performed on its prior presentations. Such stimulus-specific task-switch costs are thought to result from the retrieval of information relating to the previous task and/or response, which interferes with the selection of a new response. Note, however, that such research has focused primarily on the behavioral costs associated with S-R learning, rather than on the benefits (e.g., faster RTs) associated with long-lag repetition priming (although see Koch & Allport, 2006; Waszak et al., 2003).

The importance of further investigation of S-R learning contributions to repetition-priming paradigms is that repetition effects are often used as a tool with which to investigate the nature of stimulus representations. For example, priming has been used for many years to inform psycholinguistic theories (e.g., Marslen-Wilson, Tyler, Waksler, & Older, 1994) and theories of object perception (e.g., Biederman & Gerhardstein, 1993). Some of these inferences might be questioned if some, or all, of the priming observed reflected S-R learning rather than facilitated processing of the representations of interest. This question is particularly pertinent to many recent neuroimaging studies, which have used repetition-related response decreases to infer localization of different representations in the brain (Grill-Spector, Henson, & Martin, 2006). Indeed, an influential study by Dobbins et al. (2004) claimed that many repetition-related decreases, even in relatively early perceptual regions, reflect the bypassing of processing in those brain regions through direct retrieval of S-R mappings, rather than facilitated processing within them. This claim was based on the abolition of such decreases when the classification task was simply reversed, from deciding whether the object depicted was bigger than a shoebox to deciding whether it was smaller than a shoebox.

The same paradigm of speeded semantic classification of visual objects has been used in a number of further behavioral (Schnyer et al., 2007) and fMRI (Horner & Henson, 2008) studies. Indeed, it was the same logic of comparing priming when the task was repeated to priming when it was reversed that Schnyer et al. (2006) used to claim that S-R learning was impaired in amnesia: Those with amnesia did not show the greater priming in repeated than reversed tasks that controls showed. Nonetheless, those with amnesia showed reliable (albeit equivalent) priming in both conditions, which was attributed to facilitation of component perceptual and/or conceptual processes, facilitation that can presumably occur without explicit memory for repeated stimuli. However, it is possible that the residual priming in the reversed task condition still reflects S-R learning. For example, a bigger response might come to mind when a stimulus is repeated (from either intentional or incidental retrieval), and this could be used to respond quickly (e.g., to respond *no* if the task is now to decide whether an object is smaller than a shoebox). Thus, the comparison of repeated versus reversed tasks may not be sufficient to separate S-R learning from the component-process account.

In allowing for S-R learning to multiple response codes, we can distinguish at least three levels of response representation: a particular motor *action* (e.g., left–right finger press), a particular binary *decision* (e.g., yes–no), and a particular *classification* (e.g., bigger–smaller). Each of these levels of abstraction has been proposed in previous research (Abrams et al., 2002; Damian, 2001; Dobbins et al., 2004; Koch & Allport, 2006; Logan, 1990; Schnyer et al., 2007), but normally they are contrasted with one another, under the assumption that only one true level of response representation needs to be identified. Here, we suggest that these three response levels are learned in parallel.

The present series of experiments explored the influence of S-R learning within the shoebox paradigm of Dobbins et al. (2004) and Schnyer et al. (2006, 2007) but allowed for multiple response codes. These studies assessed S-R learning by means of a same-question task, where participants were asked the same question at study and test (e.g., "Is the object bigger than a shoebox?"), and a reverse-question task, where participants were asked the opposite question at test and study (e.g., "Is the object smaller than a shoebox?"). Our initial idea was that adding an orthogonal-question condition to the same-question (repeated task) and reverse-question (reversed task) conditions would offer a better means to separate the S-R learning and component-process accounts. In the orthogonal-question condition, the classification at study (e.g., "Is the object bigger than a shoebox?") was changed completely (not just reversed) relative to that at test (e.g., "Is the object man-made?"), such that any bigger or smaller classification responses that are bound to stimuli in the study task would be irrelevant to the test task. Moreover, the tasks and stimuli were carefully selected so that half of the correct yes–no responses in the test task were congruent with those in the task performed at study (i.e., repeated), and half of the responses were incongruent (i.e., reversed). Thus, we assumed that any effects of S-R learning at the level of decisions or actions would cancel on average.[1]

Contrary to our expectations, we found it difficult to find any reliable priming in the orthogonal-question condition (Experiments 1–3). Even when we did find reliable priming on average in this condition, by various manipulations of the stimuli (Experiments 4 and 5), it remained unclear whether this was carried solely by retrieval of prior decisions or actions for congruent trials. In other words, once we allowed for multiple levels of response representation, it appeared that S-R learning was able to explain almost all of the priming across our same-, reverse-, and orthogonal-question conditions. Indeed, any residual priming not attributable to S-R learning was small, suggesting that S-R learning dominates in speeded classification paradigms like the present one. Direct evidence for simultaneous coding of multiple response representations was established by Experiments 6 and 7. These findings not only question the dominant component-process account of priming in classification tasks like the present one, but also require extension of existing theories of S-R learning.

[1] We used these task manipulations in a previous fMRI experiment (Horner & Henson, 2008); however, this study was primarily concerned with repetition-related neural responses and did not consider the nature of S-R learning in detail (such as the role of congruent vs. incongruent responses in the orthogonal-question condition).

Experiment 1

Experiments 1 through 5 used the same basic design, within Dobbins et al.'s (2004) paradigm of speeded semantic classification of pictures of everyday objects. The main interest was to compare priming across three conditions—the same-, reverse-, and orthogonal-question conditions—in which the task was repeated, reversed, or changed, respectively, across initial and repeated presentations of an object. The tasks were administered with a study–test design, in which priming was measured by comparing at test stimuli that were previously seen at study (repeated) with stimuli that were not (novel). All tasks were phrased in terms of a yes–no question, to which participants responded with a finger press, with the mapping between yes–no and finger being constant for a given participant. The three different task conditions were administered within participants, with their order counterbalanced across participants. Finally, the experiments included a further factorial manipulation of prime level in which stimuli were classified either once or three times at study (analogous to Dobbins et al., 2004). Novel stimuli at test were randomly assigned to two equal-sized groups to provide separate unprimed baselines for high- and low-primed conditions. This resulted in a $3 \times 2 \times 2$ factorial design, with task (same, reverse, orthogonal), prime level (low primed, high primed), and repetition (novel, repeated; see Figure 1A).

What predictions would component-process and S-R learning accounts make? First, if priming is a consequence of perceptual facilitation only, equivalent positive priming would be expected across all three task conditions (because all three tasks require object identification). A similar pattern might be expected from facilitation of conceptual processes, although one might expect reduced priming in the orthogonal-question condition if the test task does not engage all of the same conceptual processes as the study task (a question addressed in Experiments 3 and 4). In both cases, priming should generally increase for high- versus low-primed stimuli.

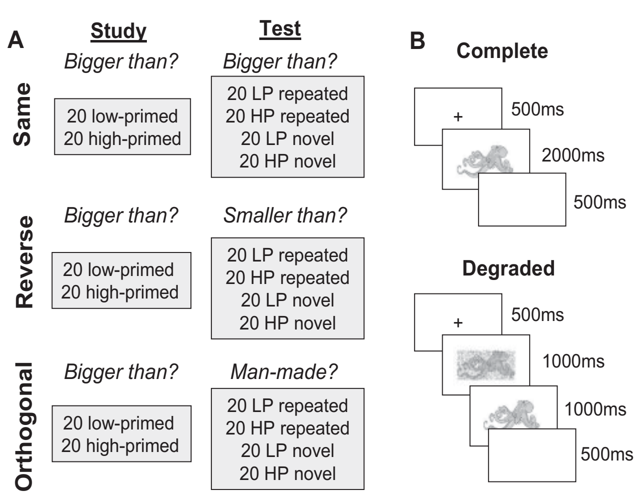

Figure 1. Details of (A) experimental design for Experiments 1 through 5 and (B) of the trial sequence for complete objects (used at study and test in Experiments 1, 2, 3, 6, and 7) and degraded objects (used at test in Experiment 5). LP = low primed; HP = high primed.

The predictions of an S-R account depend on the nature of the response. If responses are coded at the level of yes–no decision or a specific finger press (action), there should be positive priming in the same-question condition but reduced priming in the reverse-question condition because the response given at study is not repeated at test. Indeed, the reverse-question condition might be expected to produce negative priming (i.e., slower RTs for repeated than for novel stimuli) because of interference from previously learned responses. If multiple study presentations lead to a strengthening of S-R associations, or the formation of more S-R episodes, high-primed stimuli should produce greater priming in the same-question condition and a greater reduction in priming in the reverse-question condition than low-primed stimuli (i.e., high- vs. low-primed stimuli should interact with same-question vs. reverse-question tasks). In the orthogonal-question condition, because the same numbers of stimuli require response repetition (congruent trials) as require response reversal (incongruent trials), the mean amount of priming should be intermediate between that in the same- and reverse-question conditions (and greater priming should be seen for congruent than incongruent trials). Indeed, if the amount of facilitation owing to response repetition matches the amount of interference owing to response reversal, there should be zero net priming in the orthogonal-question condition. If, on the other hand, responses are coded only at the more abstract level of the classification (e.g., as either bigger or smaller in the Shoebox Task), then S-R learning would predict similar priming for the same- and reverse-question conditions but would predict no priming in the orthogonal-question condition, for either congruent or incongruent trials.

Of course, the actual pattern of priming could be a combination of several of these causes. Given the relative importance of the component-process view (e.g., for inferring the nature of psychological and neural representations; see the introductory section), we decided to concentrate on trying to refute the S-R learning account, for which reliable positive priming in the orthogonal-question condition, particularly for incongruent trials, was deemed critical.

Method

Participants. Participants in all experiments were recruited from the subject panel of the Medical Research Council's Cognition and Brain Sciences Unit or from the student population of Cambridge University; all participants had normal or corrected-to-normal vision. All experiments were of the type approved by a local research ethics committee (Cambridge Psychological Research Ethics Committee reference 2005.08).

Twelve participants (7 male, 5 female) gave informed consent to participate in Experiment 1. The mean age across participants was 23.3 years ($\sigma = 4.5$). By self-report, all participants were right-handed.

Materials. Stimuli were 240 colored images of everyday objects, taken from a set used by Dobbins et al. (2004). They were selected so that 25% were bigger than a shoebox and man-made, 25% were bigger than a shoebox and natural, 25% were smaller than a shoebox and man-made, and 25% were smaller than a shoebox and natural, according to norms taken from independent raters ($N = 6$). Each picture was randomly assigned to 1 of 12 groups relating to the 12 experimental conditions, resulting in 20 stimuli per group. The assignment of groups to experimental conditions was rotated across participants.

Procedure. Prior to the experiment, participants performed a practice session using the Bigger-Than-a-Shoebox Task, where it was made clear that this comparison referred to the object's typical size in real life. They responded using a *yes* or *no* key with their right or left index finger, respectively. Although participants were told the question may change during the course of the experiment, the other test tasks were explained to the participants only prior to each test block. They were told to respond as quickly as possible without sacrificing accuracy. No error feedback was provided either in the practice session or the main experiment.

The experiment consisted of three alternating study–test cycles, with each cycle lasting approximately 10 min. During each study phase, 40 stimuli were shown; 20 were presented once (low primed), and 20 were presented three times (high primed), resulting in 80 trials. Apart from ensuring no immediate repetitions, the stimulus presentation order within each study block was randomized so that high-primed stimuli were approximately evenly distributed throughout. During each test phase, the 40 stimuli from the study phase (repeated) were randomly intermixed with 40 novel stimuli. The order of the three test conditions (task) was counterbalanced across participants.

An example trial sequence is shown in Figure 1B. A central fixation cross was presented for 500 ms, followed by a stimulus for 2,000 ms, followed by a blank screen for 500 ms. Images subtended approximately 6° of visual angle. Participants were able to respond at any point up to the start of a new trial (i.e., the presentation of another fixation cross).

Behavioral analyses. Trials in which RTs were less than 400 ms, or two or more standard deviations above or below a participant's mean for a given task, were excluded. Given that there is some subjectivity in determining whether an object is bigger than a shoebox or man-made, (i.e., some variability of opinions across participants for some stimuli), accuracy was defined by the modal response across participants for each object. Although we report analyses of such errors in the Appendix, these results should be interpreted with caution given that the definition of an error is less clear than in some other priming experiments. RTs for correct trials at test constituted the main dependent variable. Given the focus on S-R learning, RTs were further restricted to objects also given a correct judgment on every occurrence at study. Repeated measures analysis of variance (ANOVA) was applied to mean RTs, with a Greenhouse–Geisser correction for all F values with more than one degree of freedom in the numerator. All statistical tests had alpha set at .05; t tests were two tailed except where stated otherwise.

Priming was defined as the difference in mean RTs between novel and repeated trials. To control for possible novel RT differences across task conditions, an additional proportional measure of priming was calculated by dividing the difference between novel and repeated trials by the mean RT for novel trials (Schnyer et al., 2006).

To investigate whether responses made at study had a significant effect on RTs at test, repeated-question trials in the orthogonal-question condition were split further according to whether the participant had given the same (congruent) or opposite (incongruent) response at study, regardless of accuracy. For high-primed stimuli, such trials were restricted to objects for which the same response was given across all three study presentations. Note that this means that effects in this response congruency analysis of the orthogonal-question condition (such as effects of high- vs. low-primed stimuli) can differ from those in the main analysis, because incorrect (as defined by modal response over participants) trials may be included in the former but not the latter.

Results

After excluding 0.3% of trials with outlying RTs, the percentages of errors are shown in Table 1. Note that most errors were likely to reflect a degree of subjectivity, particularly for the Shoebox Task (see the *Method* section). Analyses of errors revealed no significant effects of repetition (see the Appendix), suggesting the RT priming effects reported later are unlikely to reflect a speed–accuracy trade-off. A further 4.1% of repeated trials were excluded from RT analysis because of incorrect responses given at study (see the *Method* section).

Table 2 displays mean RTs, together with subtractive (novel−repeated) and proportional ([novel−repeated]/novel) measures of priming. A $3 \times 2 \times 2$ ANOVA on RTs revealed a significant Task × Repetition interaction, $F(1.5, 17.0) = 7.15$, $p < .01$, plus main effects of task, $F(1.9, 21.0) = 14.39$, $p < .001$, and repetition, $F(1, 11) = 21.82$, $p < .001$. Given no reliable effects involving prime level, Fs < 2.0, ps $> .16$, subsequent tests collapsed across this variable. Pairwise tests of priming across tasks revealed significantly greater priming in the same- relative to the reverse-question condition, $t(11) = 2.48$, $p < .05$, and in the same- relative to the orthogonal-question condition, $t(11) = 3.90$, $p < .01$; reverse versus orthogonal, $t(11) = 1.57$, $p = .14$. Furthermore, although priming was significantly greater than zero in the same- and reverse-question conditions, ts > 2.92, ps $< .01$, it was not reliable in the orthogonal-question condition, $t(11) = 0.60$, $p = .56$. Thus, switches in task decreased priming in the reverse condition and prevented reliable priming in the orthogonal condition.[2] Analogous ANOVAs on the proportional measure of priming showed the same pattern of results (see the Appendix), suggesting that the difference in priming between the same/reverse and orthogonal conditions was not a range effect owing to the shorter overall RTs in the orthogonal (man-made) task.

The orthogonal trials were split according to response congruency between study and test (i.e., objects given the same *yes* or *no* response at study and test, regardless of accuracy, vs. those given differing responses). The resultant priming data were entered into a 2×2 (Response Congruency × Prime Level) ANOVA. Only the main effect of response congruency was reliable, $F(1, 11) = 7.73$, $p < .05$, demonstrating greater priming for congruent (50 ms) than incongruent (−6 ms) trials. Despite this main effect, priming did not reach significance for either congruent, $t(11) = 1.48$, $p = .17$, or incongruent, $t(11) = 0.25$, $p = .81$, trials alone.

Discussion

The results of Experiment 1 suggest that S-R learning plays an important role within the present paradigm. The binding of stimuli

[2] Analyses conducted on untrimmed mean RTs (i.e., including all trials irrespective of RT or response given) showed a similar pattern of results (as was the case in all the present experiments).

Table 1
Mean Percentage of Errors and Error Priming (Plus Standard Deviations) Across Task, Prime Level, and Repetition for Experiments 1 Through 5

	Same		Reverse		Orthogonal	
	Bigger than shoebox?		Smaller than shoebox?		Man-made?	
Experiment	LP	HP	LP	HP	LP	HP
Experiment 1						
Novel	13.3 (3.9)	12.9 (7.5)	9.6 (4.5)	10.8 (5.6)	2.9 (3.3)	1.3 (2.3)
Repeated	8.3 (4.9)	10.8 (7.9)	12.5 (6.9)	14.6 (7.8)	2.1 (2.6)	2.1 (4.5)
Priming	5.0 (7.4)	2.1 (11.0)	−2.9 (8.9)	−3.8 (10.0)	0.8 (3.6)	−0.8 (4.2)
	Man-made?		Natural?		Bigger than shoebox?	
	LP	HP	LP	HP	LP	HP
Experiment 2						
Novel	2.8 (3.9)	2.8 (2.6)	1.7 (4.9)	1.7 (2.4)	9.7 (6.1)	13.1 (7.1)
Repeated	0.8 (2.6)	1.7 (3.0)	3.9 (4.0)	2.2 (3.9)	10.0 (6.4)	11.4 (5.1)
Priming	1.9 (3.5)	1.1 (2.7)	−2.2 (5.2)	−0.6 (3.8)	−0.3 (9.3)	18.7 (17.1)
	Bigger than shoebox?		Smaller than shoebox?		Taller than wide?	
	LP	HP	LP	HP	LP	HP
Experiment 3						
Novel	7.8 (7.3)	8.9 (4.4)	11.9 (8.4)	11.1 (7.2)	20.0 (9.2)	19.7 (12.4)
Repeated	6.7 (4.5)	9.2 (7.7)	11.9 (8.1)	15.6 (8.4)	25.8 (13.1)	18.1 (11.1)
Priming	1.1 (8.5)	−0.3 (7.2)	0.0 (9.1)	−4.4 (10.6)	−5.8 (13.9)	1.7 (12.7)
	Bigger than shoebox?		Smaller than shoebox?		Taller than wide?	
	LP	HP	LP	HP	LP	HP
Experiment 4						
Novel	11.9 (7.5)	12.8 (6.5)	16.4 (6.8)	10.6 (6.8)	23.6 (9.8)	21.4 (10.5)
Repeated	9.7 (4.4)	10.6 (7.5)	17.8 (8.1)	16.1 (9.9)	20.6 (11.5)	20.8 (10.7)
Priming	2.2 (8.6)	2.2 (10.2)	−1.4 (9.0)	−5.6 (10.6)	3.1 (9.7)	0.6 (10.8)
	Bigger than shoebox?		Smaller than shoebox?		Taller than wide?	
	LP	HP	LP	HP	LP	HP
Experiment 5						
Novel	11.1 (5.8)	11.1 (7.8)	12.8 (6.5)	13.1 (8.9)	19.4 (10.3)	21.1 (13.1)
Repeated	10.8 (7.7)	10.3 (5.8)	11.4 (7.0)	10.0 (6.4)	22.2 (12.6)	19.7 (9.2)
Priming	0.3 (8.8)	0.8 (9.9)	1.4 (9.2)	3.1 (10.0)	−2.8 (12.0)	1.4 (11.1)

Note. The division of novel stimuli into high and low primed is based on an arbitrary, equal split. LP = low primed; HP = high primed; priming = novel − repeated.

to a particular yes–no decision and/or to a specific left–right action is apparent from two reliable effects: (a) a reduction in priming for the reverse-question condition relative to same-question condition and (b) greater priming for congruent than incongruent trials in the orthogonal-question condition. It is also consistent with the lack of reliable net priming in the orthogonal condition, where there were approximately equal numbers of congruent and incongruent trials. The lack of priming in the orthogonal condition surprised us and suggests that other causes traditionally linked to perceptual priming (such as faster object identification for repeated pictures) play a negligible role in the present paradigm (see also Bruce, Carson, Burton, & Ellis, 2000). The lack of evidence for any modulation of priming by high- versus low-primed stimuli also surprised us, although there was a numerical pattern consistent with S-R learning that was reproduced and reached significance in subsequent experiments (see later).

Only one aspect of the data is difficult to explain in terms of bindings between stimuli and decisions/actions, namely, the residual priming in the reverse condition, where the prior response given at study might be expected to interfere with the opposite response required at test (possibly even producing negative priming). One possibility is that this residual priming reflects facilitation of conceptual processing, given that the same semantic information about the everyday size of an object

Table 2
Mean Reaction Times, Reaction Time Priming, and Proportional Priming (Plus Standard Deviations) Across Task, Prime Level, and Repetition for Experiments 1 Through 5

	Same		Reverse		Orthogonal	
	Bigger than shoebox?		Smaller than shoebox?		Man-made?	
Experiment	LP	HP	LP	HP	LP	HP
Experiment 1						
Novel	914 (161)	903 (157)	1,021 (228)	1,018 (215)	882 (165)	863 (184)
Repeated	781 (154)	758 (192)	956 (210)	923 (191)	834 (202)	874 (228)
Priming	133 (75)	145 (99)	65 (118)	94 (102)	48 (75)	−11 (157)
Proportional priming	.15 (.07)	.16 (.11)	.06 (.10)	.09 (.10)	.06 (.08)	−.02 (.16)
	Man-made?		Natural?		Bigger than shoebox?	
	LP	HP	LP	HP	LP	HP
Experiment 2						
Novel	757 (154)	759 (135)	773 (148)	781 (176)	960 (209)	969 (214)
Repeated	677 (138)	651 (128)	746 (150)	754 (160)	946 (180)	935 (194)
Priming	79 (60)	108 (77)	27 (72)	27 (79)	13 (91)	34 (165)
Proportional priming	.10 (.08)	.14 (.09)	.03 (.09)	.03 (.10)	.00 (.10)	.02 (.16)
	Bigger than shoebox?		Smaller than shoebox?		Taller than wide?	
	LP	HP	LP	HP	LP	HP
Experiment 3						
Novel	915 (124)	914 (140)	1,028 (177)	986 (181)	1,177 (232)	1,220 (211)
Repeated	788 (115)	722 (90)	949 (154)	958 (182)	1,196 (239)	1,176 (183)
Priming	128 (107)	192 (126)	79 (117)	28 (96)	−19 (154)	44 (112)
Proportional priming	.13 (.11)	.20 (.11)	.07 (.12)	.02 (.10)	−.02 (.14)	.03 (.09)
	Bigger than shoebox?		Smaller than shoebox?		Taller than wide?	
	LP	HP	LP	HP	LP	HP
Experiment 4						
Novel	954 (141)	944 (151)	1,035 (219)	1,030 (200)	1,373 (229)	1,384 (200)
Repeated	827 (119)	769 (122)	1,000 (199)	969 (187)	1,305 (237)	1,322 (201)
Priming	127 (69)	175 (96)	36 (116)	61 (73)	68 (176)	63 (94)
Proportional priming	.13 (.06)	.18 (.09)	.03 (.10)	.06 (.07)	.04 (.13)	.04 (.07)
	Bigger than shoebox?		Smaller than shoebox?		Taller than wide?	
	LP	HP	LP	HP	LP	HP
Experiment 5						
Novel	1,169 (143)	1,169 (148)	1,332 (192)	1,305 (235)	1,497 (246)	1,545 (220)
Repeated	987 (106)	910 (123)	1,124 (171)	1,146 (183)	1,455 (309)	1,434 (280)
Priming	182 (95)	259 (82)	207 (106)	159 (148)	42 (134)	111 (139)
Proportional priming	.15 (.07)	.22 (.06)	.15 (.07)	.11 (.10)	.03 (.09)	.07 (.10)

Note. The division of novel stimuli into high and low primed is based on an arbitrary, equal split. LP = low primed; HP = high primed; proportional priming = (novel − repeated)/novel.

is required in the study and test phases of the reverse-question condition (but not the orthogonal-question condition). We return to this possibility in Experiment 3. However, an alternative S-R account is that the yes–no decision from the study phase is retrieved rapidly at test but that the participant develops a strategy of simply reversing this response, which may still take less time than recomputing a decision from semantic knowledge (as necessary for novel stimuli). A second alternative S-R account is that the positive priming in the reverse-question condition reflects stimuli becoming bound with a particular classification (e.g., bigger–smaller), in addition to being bound to the yes–no decision or the left–right action. Rapid retrieval of this classification for a repeated stimulus would also enable a decision without recomputing the typical size of the object. We return to the different levels of response representation in Experiments 6 and 7. First, though, we wanted to check that the apparent dominance of S-R learning in this paradigm was not peculiar to the rather arbitrary classification entailed by the

Shoebox Task and that the lack of priming in the orthogonal-question condition was not simply because the Man-Made Task was easier.

Experiment 2

Experiment 1 used the Shoebox Task on the basis of previous work by Dobbins et al. (2004) and Schnyer et al. (2006). Such a task is likely to represent a rather ad hoc categorization (Barsalou, 1983). In other words, it is likely to involve a considerable strategic/executive component, which may encourage, or leave greater scope for, S-R learning. In Experiment 2, we therefore switched to the Man-Made Task as the main categorization task. We thought this categorization would be a more natural categorization, in that it is more likely to be a distinction represented within semantic memory (Farah & McClelland, 1991). This is consistent with the faster RTs for this task than for the Shoebox Task in Experiment 1. The Man-Made Task is also one that has been used in many previous priming studies (Bruce et al., 2000; Franks, Bilbrey, Lien, & McNamara, 2000; Vriezen, Moscovitch, & Bellos, 1995; Zeelenberg & Pecher, 2003). Thus, the design of Experiment 2 was a mirror reflection of Experiment 1, in the sense that the Man-Made Task and the Shoebox Task were swapped (i.e., the Man-Made Task was used in all phases, except for the orthogonal test phase, when the Shoebox Task was used). We were particularly interested in whether we would still see no priming in the Orthogonal-question condition.

Method

The experimental design of Experiment 2 was identical to that of Experiment 1, with the following exceptions.

Participants. Eighteen participants (5 male, 13 female) gave informed consent to participate in the experiment. The mean age across participants was 22.6 years ($\sigma = 3.0$). By self-report, 3 participants were classified as left-handed, and 15 were right-handed.

Design. Participants were always asked whether an object was man-made at study. At test, in the same-question condition, the Man-Made Task was repeated; in the reverse-question condition, the opposite question was presented (“Is the object natural?”); in the orthogonal-question condition, the bigger-than-a-shoebox question was asked.

Results

After excluding 0.6% of trials with outlying RTs, the percentages of errors are shown in Table 1. Consistent with expectations, RTs for novel stimuli were faster in the Man-Made Task (i.e., same-question and reverse-question conditions) than in the Shoebox Task (i.e., orthogonal-question task). Analyses of errors revealed no significant effects of repetition (see the Appendix). A further 1.9% of repeated trials were excluded from RT analysis because of incorrect responses given at study (see the *Method* section).

Table 2 displays mean RTs and priming effects. A $3 \times 2 \times 2$ ANOVA revealed a significant Task $\times$ Repetition interaction, $F(1.6, 26.5) = 5.29$, $p < .05$, plus main effects of task, $F(1.4, 23.5) = 42.57$, $p < .001$, and repetition, $F(1, 17) = 18.85$, $p < .001$. Given no reliable effects involving prime level, Fs < 0.84, ps $> .37$, subsequent tests collapsed across this variable. These tests revealed significantly greater priming in the same-question relative to the reverse-question condition, $t(17) = 3.80$, $p < .01$, and in the same-question relative to the orthogonal-question condition, $t(17) = 2.83$, $p < .05$; reverse versus orthogonal, $t(17) = 0.12$, $p = .91$. Furthermore, although priming was significantly greater than zero in the same- and reverse-question conditions, ts > 2.0, ps $< .05$, it was not reliable in the orthogonal-question condition, $t(17) = 0.94$, $p = .36$. The proportional priming measure revealed a similar pattern of results (see the Appendix).

A 2×2 (Response Congruency $\times$ Prime Level) ANOVA for the orthogonal condition showed only a main effect of response congruency, $F(1, 17) = 23.18$, $p < .001$, with greater priming for congruent (73 ms) than incongruent (-18 ms) trials. Indeed, priming was reliable for congruent trials, $t(17) = 3.42$, $p < .01$, but not incongruent trials, $t(17) = 0.71$, $p = .49$.

Discussion

The priming results of Experiment 2 replicate those of Experiment 1, most notably with an absence of reliable priming in the orthogonal-question condition. Again, most of the results appear explicable in terms of S-R learning: (a) greater priming in the same-question condition than in the reverse-question condition and (b) greater priming for congruent than incongruent trials in the orthogonal-question condition. This suggests that S-R learning plays an important role even with the relatively easier (and less ad hoc) decisions required by the Man-Made Task (relative to the Shoebox Task).

Experiment 3

Experiments 1 and 2 revealed clear evidence of S-R learning effects but no results that could be explained only in terms of the facilitation of other component processes (e.g., object identification or semantic access). Most notable was the failure to observe priming in the orthogonal-question condition, which was intended to provide a baseline measure of priming with which to compare the same-question and reverse-question conditions. Experiments 3 through 5 were designed to try to increase the contributions of facilitation of other component processes, such as perceptual and/or semantic processes.

In Experiment 3, we decided to use a new task for the orthogonal condition, one likely to be matched more closely to the Shoebox Task in terms of semantic processing. We opted for the Taller-Than-Wide Task previously used by Vriezen et al. (1995, Experiment 6), reasoning that this categorization at least requires access to similar size semantics about objects. Again, we selected objects so that half of those larger than a shoebox and half of those smaller than a shoebox were generally taller than they were wide, such that, on average, the response given at test was not predicted by the response given at study in the orthogonal-question condition.

Method

The experimental design of Experiment 3 was identical to that of Experiment 1, with the following exceptions.

Participants. Eighteen participants (11 male, 7 female) gave informed consent to participate in the experiment. The mean age across participants was 21.7 years ($\sigma = 2.5$). All participants were right-handed (self-report).

Design. Experiment 3 used the same Shoebox Task as Experiment 1 in the same-question and reverse-question conditions and in the study phase of the orthogonal-question condition; the only difference was in the orthogonal test phase, where participants were asked whether the object was taller than it was wide in real life (i.e., not based on the picture's on-screen dimensions). Again, correct responses for a given object were based on the modal response across participants.

Results

After excluding 0.9% of trials with outlying RTs, the percentages of errors are shown in Table 1 (the higher error rates in the orthogonal-question task reflected greater individual differences in the taller-than-wide judgment; see *Method* section). Analyses of errors revealed no significant effects of repetition (see the Appendix). A further 4.9% of repeated trials were excluded from RT analysis because of incorrect responses given at study.

Table 2 displays mean RTs, together with measures of priming. A 3 $\times$ 2 $\times$ 2 ANOVA revealed a significant Task $\times$ Repetition interaction, $F(1.4, 23.0) = 15.91$, $p < .001$, plus main effects of task, $F(1.7, 29.6) = 69.52$, $p < .001$, and repetition, $F(1, 17) = 43.95$, $p < .001$. Collapsing across prime level, subsequent tests revealed significantly greater priming in the same-question relative to the reverse-question condition, $t(17) = 6.63$, $p < .001$, and in the same-question relative to the orthogonal-question condition, $t(17) = 4.41$, $p < .001$; reverse versus orthogonal, $t(17) = 1.44$, $p = .17$. Furthermore, although priming was significantly greater than zero in the same-question and the reverse-question conditions, ts > 2.86, ps $< .05$, it was not reliable in the orthogonal-question condition, $t(17) = 0.56$, $p = .58$. RTs in the Taller-Than-Wide Task were longer than in the Shoebox Task, but the proportional measure of priming showed the same pattern of results (see the Appendix).

Of interest, there was a trend toward a Task $\times$ Prime Level $\times$ Repetition interaction, $F(1.8, 30.4) = 2.83$, $p = .08$. Given our predictions regarding possible facilitation and interference in the same-question and reverse-question conditions, respectively, we conducted a further 2 $\times$ 2 $\times$ 2 (Task $\times$ Prime Level $\times$ Repetition) ANOVA on the mean RT data from the same-question and reverse-question conditions only. This revealed a significant Task $\times$ Prime Level $\times$ Repetition interaction, $F(1, 17) = 5.60$, $p < .05$, reflecting numerically greater priming for high- than low-primed stimuli in the same-question condition (64 ms) and numerically less priming for high-primed stimuli in the reverse-question condition (-51 ms).

Priming in the orthogonal-question condition was split according to congruent and incongruent responses and entered into a 2 $\times$ 2 ANOVA. Despite numerically greater priming for congruent (23 ms) than incongruent (-18 ms) trials, as in Experiments 1 and 2, the main effect of congruency did not reach significance, $F(1, 17) = 1.37$, $p = .26$ (nor did any other effects). Priming was significant for high-primed congruent trials (86 ms), $t(17) = 2.49$, $p < .05$, but not for the other trial types, ts < 1.02, ps $> .32$.

Discussion

Experiment 3 replicated Experiments 1 and 2; in particular, there was still no reliable net priming in the orthogonal-question condition, despite trying to maximize the overlap in semantic processing required by the study and test tasks. As in Experiments 1 and 2, most of the results are explicable in terms of S-R learning, namely (a) greater priming in the same-question condition than other conditions and (b) a trend toward greater priming for congruent than incongruent trials in the orthogonal-question condition. Furthermore, Experiment 3 was the first to show a reliable interaction between task and prime level, suggesting that increasing the number or strength of S-R bindings at study can significantly increase the difference in priming across the same-question and reverse-question conditions. It is unclear, however, whether this effect is primarily driven by facilitation in the same-question condition (i.e., greater priming for high- than low-primed stimuli), interference in the reverse-question condition (i.e., less priming for high- than low-primed stimuli), or a combination of both. A response-facilitation effect for congruent trials together with a response-interference effect for incongruent trials may explain the lack of net priming in the orthogonal-question conditions of Experiments 1 through 3. We return to this point in the combined analysis across Experiments 1 through 5.

Experiment 4

Experiment 3 still failed to produce significant net priming in the orthogonal-question condition. These results were particularly surprising given that significant priming was seen in Vriezen et al.'s (1995) Experiment 6, which used the same Shoebox and Taller-Than-Wide Tasks. One important difference, however, is that Vriezen et al. used words rather than pictures. Experiment 4 was therefore a replication of Experiment 3, except that we replaced the pictures of objects with the object names.

Method

The experimental design of Experiment 4 was identical to that of Experiment 3, with the following exceptions.

Participants. Eighteen participants (11 male, 7 female) gave informed consent to participate in the experiment. The mean age across participants was 22.9 years ($\sigma = 3.7$). All participants were right-handed (self-report).

Materials. The same objects were used as in Experiment 3, except that the stimuli were the names of the objects rather than pictures of them.

Results

After excluding 1.3% of trials with outlying RTs, the percentages of errors are shown in Table 1. Analyses of errors revealed no main effect of repetition, although there was a significant repetition effect in the reverse-question condition, reflecting greater errors for repeated than for novel stimuli (see the Appendix). Given the failure to find this effect in previous and subsequent experiments, it is not discussed further. A further 5.2% of repeated trials were excluded from RT analysis because of incorrect responses given at study.

Table 2 displays mean RTs, together with the mean RT priming effect (novel – repeated) and proportional priming. A 3 × 2 × 2 ANOVA revealed a significant Task × Repetition interaction, $F(1.9, 32.2) = 8.86$, $p < .01$. There were also main effects of task, $F(1.7, 28.3) = 94.88$, $p < .001$, and repetition, $F(1, 17) = 42.49$, $p < .001$. Given no reliable effect involving prime level, Fs $<$ 2.09, ps $>$ .15, subsequent tests collapsed across this variable. These tests revealed significantly greater priming in the same- relative to the reverse-question condition, $t(17) = 4.17$, $p < .01$, and in the same- relative to the orthogonal-question condition, $t(17) = 3.50$, $p < .01$; reverse versus orthogonal, $t(17) = 0.58$, $p =$ .57. As in Experiments 1 through 3, significant priming was present in the same-question and reverse-question conditions, ts $>$ 2.40, ps $<$ .05. Unlike Experiments 1 through 3 though, significant priming was also present in the orthogonal-question condition, $t(17) = 2.53$, $p < .05$. The proportional measures of priming showed the same pattern of results (see the Appendix).

A 2 × 2 ANOVA on the orthogonal priming data revealed a main effect of response congruency, $F(1, 17) = 14.26$, $p < .01$, showing greater priming for congruent (103 ms) than incongruent (−29 ms) stimuli. Indeed, priming was reliable for congruent trials, $t(17) = 3.56$, $p < .01$, but not for incongruent trials, $t(17) =$ 1.09, $p = .29$. A main effect of prime level was also present, $F(1, 17) = 5.26$, $p < .05$, revealing greater priming for low- than high-primed stimuli. This latter finding was unexpected, but given that it was not found in the other experiments here, it is not considered further.

Discussion

The use of words (object names) rather than pictures in Experiment 4 produced, for the first time in the present series of experiments, significant net priming in the orthogonal-question condition. This cross-task priming in Experiment 4 replicates that found by Vriezen et al. (1995). One possibility is that significant priming can be induced even after controlling for S-R learning effects, by facilitation of one or more component processes. Why would this be the case for words but not for the pictures in Experiments 1 through 3? One reason may be that performing the Shoebox Task and the Taller-Than-Wide Task with words requires the participant to imagine a specific (or prototypical) exemplar of the object, perhaps even forming a visual image. These processes of exemplar selection and/or image generation may be particularly prone to facilitation if they have been performed in the recent past (e.g., during the study phase) producing a savings effect for repeated stimuli (i.e., priming). Because a picture of an object provides direct access to a specific exemplar, no such selection/image-generation processes would be necessary in Experiments 1 through 3. Indeed, previous research has shown priming during imagery tasks that require participants to form a mental image of an object (McDermott & Roediger, 1994). Another reason for priming in the orthogonal condition for words but not for pictures may be facilitation of phonological access, given evidence that phonological representations are automatically accessed during word processing (Bowers & Turner, 2003) but not object processing (Damian & Bowers, 2003).

Nonetheless, priming in the orthogonal condition was driven primarily by the congruent trials, which could reflect retrieval of the previous decision or action associated with a word repeated from the study phase. Priming was not reliable for incongruent trials. Indeed, this was also the case in Vriezen et al. (1995, Experiment 6). In other words, the present results could still be explained by S-R learning, particularly if it is assumed that facilitation due to response repetition is greater than any interference due to response reversal. Thus, the results from the present experiment cannot be taken as unequivocal evidence for the component-process view of priming. We return to these issues in the General Discussion section; in the next experiment, we returned to pictures and tried another method to increase the potential for measurable facilitation of a more perceptual component process.

Experiment 5

In Experiment 5, we sought further evidence for the existence of perceptual/semantic contributions to priming. One reason for the failure to see evidence of perceptual facilitation in Experiments 1 through 3 may be that recognition of the objects depicted in the colored pictures was already as efficient as possible (i.e., could not be facilitated appreciably by repetition). To tax object-recognition processes to a greater extent, we visually degraded stimuli at test in Experiment 5, anticipating more scope for perceptual facilitation owing to prior exposure of intact versions at study.

Method

The experimental design of Experiment 5 was identical to that of Experiment 3, with the following exceptions.

Participants. Eighteen participants (10 male, 8 female) gave informed consent to participate in the experiment. The mean age across participants was 22.6 years ($\sigma = 4.4$). One participant was ambidextrous; all other participants were right-handed (self-report).

Procedure. Images were displayed in exactly the same manner as in Experiment 3 during study blocks; however, at test they were degraded (see Figure 1B). At stimulus onset, the image was completely masked by setting 100% of pixels to gray. The amount of this noise was reduced gradually over 25 steps by randomly removing gray voxels from 100% at onset to 0% after 1,000 ms. The unmasked stimulus then remained on screen for a further 1,000 ms. Participants performed the same study and test tasks as in Experiment 3 and were given exactly the same instructions (i.e., to respond as quickly and as accurately as possible).

Results

After excluding 0.8% of trials with outlying RTs, the percentages of errors are shown in Table 1. Analyses of errors revealed no significant effects of repetition (see the Appendix). A further 4.0% of repeated trials were excluded from RT analysis because of incorrect responses given at study.

Table 2 displays mean RTs, together with measures of priming. Inspection of both subtractive priming and proportional priming scores suggests that priming was greater in Experiment 5 than in previous Experiments. Tests confirmed that mean subtractive priming, $t(17) = 4.48$, $p < .001$, and mean proportional priming, $t(17) = 3.26$, $p < .01$, were indeed greater in Experiment 5 than in Experiment 3 with intact pictures. A 3 × 2 × 2 ANOVA revealed a significant Task × Repetition interaction, $F(1.5, 26.3) = 16.28$, $p < .001$, plus main effects of task, $F(1.3, 22.8) = 45.27$, $p < .001$, and repetition, $F(1, 17) = 156.85$, $p < .001$. Collapsing across prime level, subsequent tests revealed significantly greater priming in the same-question condition, $t(17) = 6.23$, $p < .001$, and the reverse-question condition, $t(17) = 3.28$, $p < .01$, relative to the orthogonal-question condition. Note that although the same- versus reverse-question contrast did not reach significance, $t(17) = 1.73$, $p = .10$, analysis of the proportional measure of priming did reveal significantly greater priming in the same-question condition, $t(17) = 3.15$, $p < .01$ (see the Appendix). As in Experiments 1 through 4, significant priming was seen in the same- and reverse-question conditions, $t\text{s} > 9.82$, $p\text{s} < .001$, and as in Experiment 4, there was also significant priming in the orthogonal-question condition, $t(17) = 3.25$, $p < .01$.

As in Experiment 3, a trend toward a Task × Prime Level × Repetition interaction was present, $F(1.5, 25.6) = 2.83$, $p = .09$. A 2 × 2 × 2 ANOVA for the same- and reverse-question conditions revealed only a significant Task × Prime Level × Repetition interaction, $F(1, 17) = 5.19$, $p < .05$ (as seen in Experiment 3). This interaction reflected greater priming for high- than low-primed stimuli in the same-question condition (77 ms), $t(17) = 3.00$, $p < .01$, a pattern that was not present in the reverse-question condition (−49 ms), $t(17) = 1.01$, $p = .33$.

A 2 × 2 ANOVA on the orthogonal priming data revealed a main effect only of congruency, $F(1, 17) = 16.51$, $p < .001$, with greater priming for congruent (134 ms) than incongruent (21 ms) stimuli, as in previous experiments. Indeed, priming was reliable for congruent trials, $t(17) = 5.51$, $p < .001$, but not for incongruent trials, $t(17) = 0.89$, $p = .39$.

Discussion

Experiment 5 showed that a second type of experimental manipulation—visual degradation of object pictures—(in addition to the use of words rather than pictures in Experiment 4) can also reveal reliable net priming in the orthogonal-question condition. Indeed, this manipulation seemed to increase priming across all conditions relative to the nondegraded pictures in Experiment 3. One possible explanation is that, by slowing down object identification at test through the gradual removal of visual noise, there was more scope for facilitation of this process by prior identification of objects at study.

As in Experiment 4, however, priming in the orthogonal-question condition was reliable only for congruent trials. It is, of course, possible that there was a positive priming effect caused by perceptual facilitation for both congruent and incongruent trials, which was augmented by S-R contributions for congruent trials but was counteracted by response interference for incongruent trials. Such interference may have resulted in no net priming for incongruent trials. However, it is also possible that there was no contribution of perceptual facilitation at all, and S-R learning causes greater facilitation (for congruent trials) than it does interference (for incongruent trials), such that there was positive priming for congruent trials but no negative priming for incongruent trials. This is consistent with multiple study exposures (high primed) increasing priming in the same-question condition but having little affect on priming in the reverse-question condition. It is also consistent with the *instance theory* of (Logan, 1990), in which response retrieval can only cause facilitation (such that RTs for primed stimuli can never be slower than the algorithmic route required on initial presentation of a stimulus—see the General Discussion section). The greater overall priming across all conditions when degrading stimuli (i.e., in Experiment 5 relative to Experiment 3) might simply be explained by the longer RTs allowing greater influence of S-R learning. Thus, to unequivocally rule out S-R learning as the explanation for cross-task priming, reliable positive priming needs to be demonstrated for incongruent trials in the orthogonal-question condition, and this was not found.

Interexperimental Analyses Across Experiments 1 Through 5

Experiments 1 through 5 provided strong evidence for S-R learning, which appeared sufficient to explain most if not all priming effects. Three different signatures of S-R learning were revealed: (a) significantly greater priming in the same-question than in the reverse-question condition, (b) significantly greater priming for congruent than for incongruent trials in the orthogonal-question condition, and (c) an increase in priming for high- rather than low-primed stimuli in the same-question condition but not the reverse-question condition. These three effects are highlighted in Figures 2A and 2B, where the effects have been averaged across Experiments 1 through 5.

A final ANOVA was conducted on all of the data from Experiments 1 through 5 to test for possible effects of task order on priming. It was a 3 × 2 × 2 × 5 × 6 (Task × Prime Level × Repetition × Experiment × Order) ANOVA, where the between-subjects variable of order refers to the six counterbalancing orders of the three task conditions, with a total of 14 participants per counterbalancing order. No main effect of order was present, $F(5, 54) = 0.38$, $p = .86$, nor did this variable interact significantly with any other variable, $F\text{s} < 1.7$, $p\text{s} > .17$. Thus, there was no evidence for any task order effects.

The ANOVA also confirmed the significant Task × Repetition interaction seen across Experiments 1 through 5, $F(1.6, 87.2) = 43.08$, $p < .001$, and showed a trend toward a significant Task × Prime Level × Repetition interaction, $F(1.8, 96.2) = 2.64$, $p = .08$. Because Experiments 3 and 5 demonstrated a significant three-way interaction when the analysis focused specifically on the same-question and reverse-question tasks, we performed a similar analysis across Experiments 1 through 5. A 2 × 2 × 2 × 5 (Task × Prime Level × Repetition × Experiment) ANOVA with only the data from the same-question and reverse-question conditions demonstrated a significant Task × Prime Level × Repetition interaction, $F(1, 79) = 7.91$, $p < .01$, which did not interact significantly with experiment, $F(4, 79) = 1.9$, $p = .11$. Further tests revealed significantly greater priming for high- than for low-primed stimuli in the same-question condition, $t(83) = 3.32$, $p < .01$, but showed no difference in the reverse-question condition, $t(83) = 0.76$, $p = .45$. These results suggest that a greater number of presentations during study increases priming when those responses are repeated (in the same-question condition) but does not decrease priming when those responses are reversed (in the reverse-question condition). This brings into question the degree of interference arising from S-R learning in the present paradigm (though see Experiment 7 and the General Discussion section).

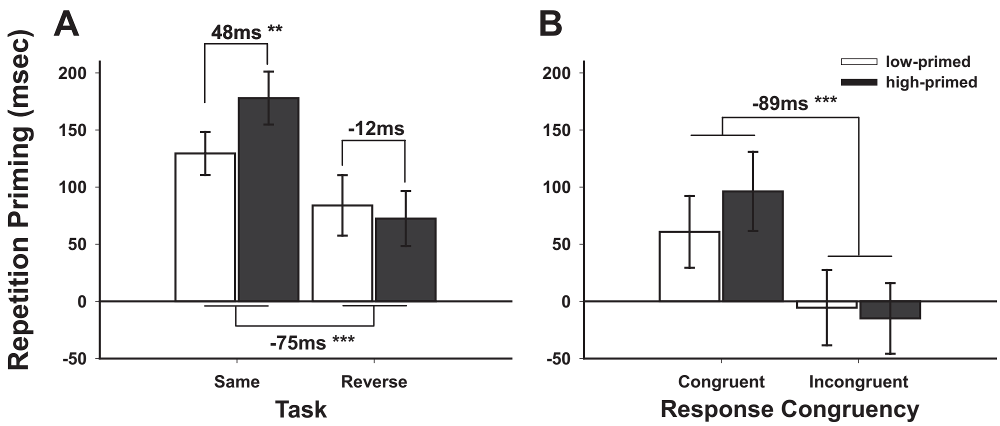

Figure 2. Main effects and interactions of interest across Experiments 1 through 5. (A) Greater priming in the same-question than in the reverse-question condition (collapsed across experiment and prime level) and greater priming for high- than low-primed stimuli in the same-question condition (collapsed across experiment). (B) Greater priming for congruent than incongruent stimuli in the orthogonal-question condition (collapsed across experiment and prime level). Errors bars represent 95% confidence intervals (two-tailed). $^{**}p < .01$. $^{***}p < .001$.

Experiment 6

As discussed in Experiment 1, the pattern of priming across the same-, reverse-, and orthogonal-question conditions of Experiments 1 through 5 can be explained fully by S-R learning, if it is assumed that stimuli can become bound simultaneously to more than one level of response representation. Bindings between stimuli and decisions and/or actions are necessary to explain the greater priming in the same- than in the reverse-question condition and in congruent than incongruent trials in the orthogonal-question condition. To explain the reliable positive priming in the reverse-question condition, however, stimuli would also need to be bound to classifications. Retrieval of the prior classification could decrease RTs for repeated relative to novel stimuli in the reverse-question condition (but would not affect the orthogonal-question condition, where the task is changed). This is illustrated in Table 3. If retrieval of responses at each of these levels occur in parallel and if such retrieval primarily accelerates RTs (with little or no deceleration of RTs when the response retrieved is incorrect), then priming for the same-question condition should approximate the sum of priming for the reverse-question condition (classification retrieval) and priming for congruent orthogonal-question trials (decision/action retrieval). This appeared to be the case (cf. Figures 2A and 2B).

The conditions in Experiments 1 through 5 could not distinguish S-R learning at the level of decisions from that at the level of actions (see Table 3), given that the assignment of yes–no responses to keys was fixed for a given participant, nor could the conditions provide direct evidence for S-R learning at the level of classifications. The aim of Experiments 6 and 7 was to distinguish these and to provide more direct evidence that stimuli become bound simultaneously to each of these three levels of response representation.

In Experiment 6, participants always performed the Shoebox Task at test. There were three different conditions at study. In two conditions, the Shoebox Task was also performed, but participants either responded with keypresses (same action and same decision as at test) or with vocal yes–no responses (different action but same decision as at test); in the third study condition, participants were required to vocalize the object's name (different action and different decision from test). This design therefore allowed us to separate learning of an action (finger press vs. vocal response) from learning of a decision (yes–no vs. object naming), as illustrated in Table 3.

Table 3
Schematic of All Conditions Across Experiments 1 Through 7 in Terms of Three Different Levels of Response Representation (Classification, Decision, and Action)

Experiment and condition	Classification	Decision	Action
Experiments 1–5			
Same	S	S	S
Reverse	S	R	R
Orthogonal			
Congruent	D	S	S
Incongruent	D	R	R
Experiment 6			
Same action, same decision	S	S	S[a]
Different action, same decision	S[b]	S	D[a]
Different action, different decision	D[b]	D	D
Experiment 7			
Classification congruent, decision congruent	S	S	S[a]
Classification congruent, decision incongruent	S[b]	R	R[a]
Classification incongruent, decision congruent	R	S	S
Classification incongruent, decision incongruent	R[b]	R	R

Note. S = same response; R = reverse response; D = different response. [a] Indicates the critical differences in Contrast 1 of the interexperimental analysis of Experiments 6 and 7, related to changes in action (see text). [b] Indicates the critical differences in Contrast 2 of the interexperimental analysis of Experiments 6 and 7, related to changes in classification (see text).

Method

The experimental design of Experiment 6 was identical to that of Experiment 1, with the following exceptions.

Participants. Eighteen participants (7 male, 11 female) gave informed consent to participate in the experiment. The mean age across participants was 24.7 years ($\sigma = 6.0$). Four participants were left-handed, the remaining 14 participants were right-handed (self-report).

Design. Experiment 6 involved three study–test cycles. At test, participants always performed the Bigger-Than-a-Shoebox Task, using keypress responses. At study, participants completed one of three tasks: (a) the Bigger-Than-a-Shoebox Task with a keypress response (same-action same-decision condition), (b) the Bigger-Than-a-Shoebox Task with a verbal yes–no response (different-action same-decision condition), or (c) a verbal naming task (different-action different-decision condition).

Results

After excluding 0.3% of trials with outlying RTs, the percentage of errors, together with mean RTs, mean RT priming, and proportional priming, are shown in Table 4. Analyses of errors revealed no significant effects of repetition (see the Appendix). A further 2.1% of repeated-question trials were excluded from RT analysis because of incorrect responses given at study.

A 3 × 2 × 2 ANOVA revealed significant Task × Repetition interaction, $F(1.6, 27.2) = 17.04$, $p < .001$, and a significant Prime Level × Repetition interaction, $F(1, 17) = 6.95$, $p < .05$, plus main effects of task, $F(1.8, 30.3) = 10.15$, $p < .001$, prime level, $F(1, 17) = 6.09$, $p < .05$, and repetition, $F(1, 17) = 79.31$, $p < .001$. The Prime Level × Repetition interaction reflected greater priming for high- than low-primed stimuli irrespective of task. To further investigate the Task × Repetition interaction, subsequent pairwise comparisons across tasks were collapsed across prime level. These revealed significantly greater priming in the same-action same-decision condition than in the different-action same-decision condition, $t(17) = 2.85$, $p < .05$, and significantly greater priming in the different-action same-decision condition than in the different-action different-decision condition, $t(17) = 3.28$, $p < .01$ (see Figure 3A). Note that there were no reliable task differences in RTs for novel stimuli across task (as expected because the test task was the same across conditions); as a result, the proportional priming data show a pattern similar to those of the main analysis (Table 4). Therefore, changes (not reversals) in both action and decision caused a significant decrease in priming (see Figure 3A). The proportional measures of priming showed the same pattern of results (see the Appendix).

Of interest, priming was still significant in the different-action different-decision condition, $t(17) = 2.48$, $p < .05$. Indeed, this priming effect was significant even for participants who performed the different-action different-decision condition first, $t(5) = 2.65$, $p < .05$, suggesting that it was not simply because participants who performed the different-action different-decision condition last continued to (covertly) categorize objects as bigger–smaller at study.

Discussion

Experiment 6 produced two important findings for S-R learning: (a) significantly greater priming when an action is repeated than when it is not (from contrasting keypresses with yes–no vocalization at study) and (b) significantly greater priming when a decision is repeated than when it is not (from contrasting yes–no vocalization with object name vocalization at study). These suggest that responses are coded at both the level of the action and the level of the decision, possibly explaining some of the discrepancies in this regard across previous studies (Dobbins et al., 2004; Koch & Allport, 2006; Logan, 1990; Rothermund et al., 2005; Schnyer et al., 2007; Waszak & Hommel, 2007). Note also that the different-action same-decision and different-action different-decision conditions did not entail any response reversal, unlike the reverse-question condition or the incongruent trials in the orthogonal-question condition of Experiments 1 through 5. Therefore, there was no opportunity for a decrease in RTs owing to response interference, consistent with the greater overall priming for high- than low-primed stimuli in Experiment 6, but no interaction of this effect with task condition.

A third finding was reliable residual priming even when neither the finger press nor the yes–no decision was repeated (i.e., in the different-action different-decision condition, when objects were

Table 4
Mean Percentage Errors, Error Priming, Reaction Time (RT), RT Priming, and Proportional Priming (Plus Standard Deviations) Across Task, Prime Level, and Repetition for Experiment 6

	Same action, same decision		Different action, same decision		Different action, different decision	
Errors and RTs	LP	HP	LP	HP	LP	HP
Errors						
Novel	11.1 (5.0)	12.2 (8.3)	12.5 (6.9)	11.1 (7.6)	10.3 (6.7)	12.2 (7.5)
Repeated	9.7 (6.3)	9.4 (5.4)	12.2 (10.2)	11.9 (7.3)	10.8 (6.2)	13.3 (7.5)
Priming	1.4 (7.2)	2.8 (10.9)	0.3 (10.4)	−0.8 (6.5)	−0.6 (6.8)	−1.1 (10.2)
RTs						
Novel	857 (131)	832 (133)	813 (126)	834 (119)	862 (174)	885 (176)
Repeated	708 (101)	667 (99)	734 (127)	674 (102)	832 (141)	812 (151)
Priming	150 (77)	165 (87)	80 (59)	161 (73)	30 (105)	73 (100)
Proportional priming	.17 (.08)	.19 (.09)	.10 (.07)	.19 (.08)	.02 (.11)	.08 (.10)

Note. The division of novel stimuli into high and low primed is based on an arbitrary, equal split. LP = low primed; HP = high primed; proportional priming = (novel − repeated)/novel.

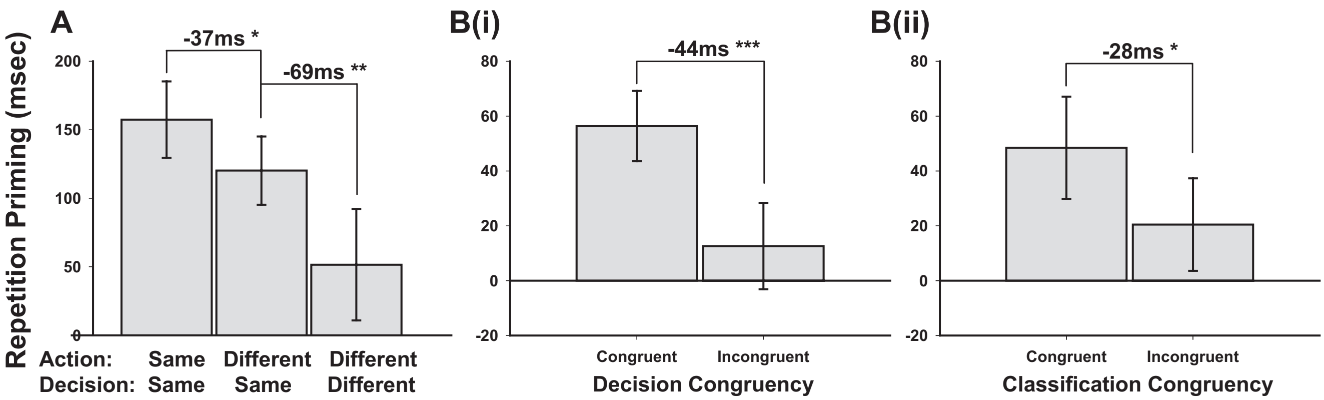

Figure 3. Main effects of interest across Experiments 6 and 7. (A) Priming across task in Experiment 6 (collapsed across prime level). (B) Priming across (i) decision congruency (collapsed across classification congruency) and (ii) classification congruency (collapsed across decision congruency) in Experiment 7 (collapsed across prime level). Errors bars represent 95% confidence intervals (two-tailed). * $p < .05$. ** $p < .01$. *** $p < .001$.

named only at study). This finding cannot be explained by responses at the level of classification either, because naming an object has nothing to do with its subsequent classification as bigger or smaller, nor did it appear to reflect covert performance of the Shoebox Task at study, given that it was reliable even for participants who performed this condition first (though covert classification might have been encouraged by the practice phase). Repeating this condition with a group of participants who are never informed about the subsequent Shoebox Task would be informative in this regard. If reliable priming remains, this would be strong support for some form of facilitated perceptual processing (e.g., object identification). Nonetheless, our main focus here was on S-R learning, for which Experiments 1 through 6 taken together suggest simultaneous coding of at least three levels of responses: action, decision, and classification. This proposal was tested further in Experiment 7.

Experiment 7

Given the evidence from Experiment 6 that stimuli become bound with both overt actions and covert decisions, we wanted to find analogous evidence that stimuli can become bound with both yes–no decisions and task-specific classifications. Although we appealed to the distinction between action/decision and classification to explain the results of Experiments 1 through 5, this was rather post hoc and indirect. Furthermore, we wanted to distinguish S-R learning of classifications from facilitation of conceptual processes, given that the implication of classification response codes in Experiments 1 through 5 was based partly on comparing the reverse-question condition with the orthogonal-question condition. Because this comparison entailed a change in task, it is difficult to guarantee that the same degree of overlap in conceptual processing occurred in the orthogonal-question condition as in the reverse-question condition (even with the Taller-Than-Wide Task in Experiments 3 through 5 being as similar as possible to the Shoebox Task). Experiment 7 was, therefore, designed to contrast the use of classification codes (and decision codes) within the context of a constant task.

We achieved this by a combination of task reversals (e.g., from bigger than *X* to smaller than *X*) and changes in the size referent (e.g., bigger than *X* to bigger than *Y*) between study and test, resulting in a factorial manipulation of decision congruency versus classification congruency (see Figure 4). For instance, when asked whether a monkey is bigger than a shoebox at study, the participant is likely to classify it as bigger (the classification) and, therefore, to answer *yes* (the decision). When asked at test whether a monkey is bigger than a wheelie bin, the classification is now reversed (from bigger to smaller), as is the decision (from *yes* to *no*).[3] This would correspond to a classification-incongruent, decision-incongruent trial because both responses were reversed. However, when asked at test whether a monkey is smaller than a wheelie bin, the correct decision would now be *yes* because the monkey is smaller. This would correspond to a trial that is decision congruent (because the participant answers *yes* at both study and test) but classification incongruent (because the participant classifies the monkey as bigger at study but smaller at test).

Method

The study–test design in Experiment 7 was similar to that of previous experiments; however, there were several key differences.

Participants. Twenty-four participants (7 male, 17 female) gave informed consent to participate in the experiment. The mean age across participants was 21.7 years (σ = 3.7). Four participants were left-handed, the remaining 20 participants were right-handed (self-report).

Design. Participants performed four study–test cycles. At study, participants always performed the Bigger-Than-a-Shoebox Task. At test, the referent was changed from a shoebox to either a wheelie bin or a pencil case. Of importance, half the stimuli seen

[3] *Wheelie bin* is a common term in the United Kingdom that refers to a large trash can (with wheels), which has a standard size that would be well-known by our participants.

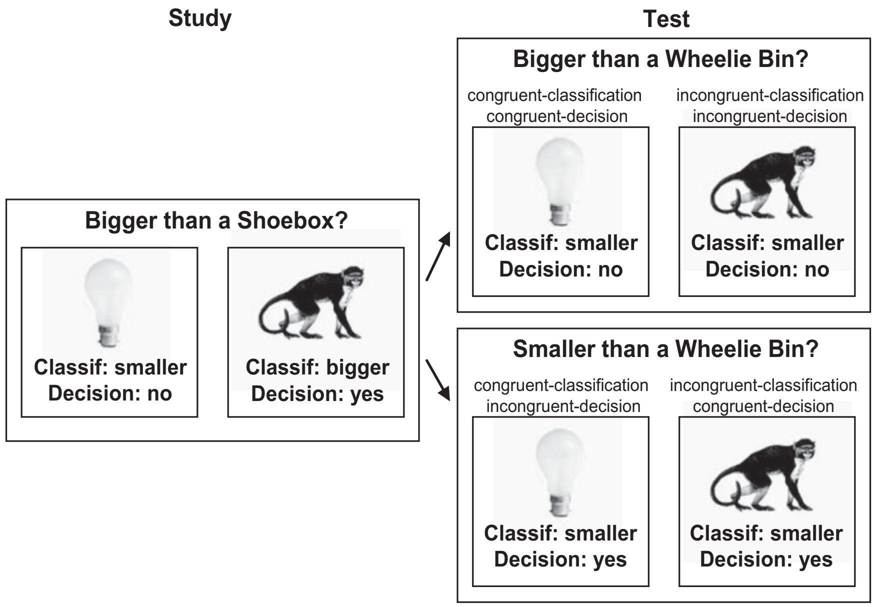

Figure 4. Schematic of experimental design for Experiment 7. Reversals in task (bigger–smaller) coupled with changes in size referent (shoebox to wheelie bin/pencil case) resulted in a 2 × 2 (Classification Congruency × Decision Congruency) factorial design. Note that a referent change was also made to a smaller referent (a pencil case) as well as the larger wheelie bin referent change shown here. Classif = classification.

at study that were bigger than a shoebox were smaller than a wheelie bin (for the wheelie bin referent change); equally, half the stimuli that were smaller than a shoebox were bigger than a pencil case (for the pencil case referent change). The other half of the stimuli were split evenly so that 50% were bigger than a shoebox and bigger than a wheelie bin, and 50% were smaller than a shoebox and smaller than a wheelie bin. The same was true for the pencil case referent change condition. This design meant that, for 50% of repeated stimuli, a congruent classification was given between study and test (e.g., bigger–bigger), and for the remaining 50% an incongruent classification was given (e.g., bigger–smaller).

Crucially, however, participants were asked whether the object was either bigger or smaller than a wheelie bin or a pencil case at test. In other words, four possible questions were posed at test: Was the object (a) bigger than a wheelie bin? (b) smaller than wheelie bin? (c) bigger than a pencil case? (d) smaller than a pencil case? These manipulations factorize the decision (yes–no) and classification (bigger–smaller). For example, an object that is smaller than a shoebox and smaller than a pencil case has a congruent classification and a congruent decision in both bigger-than tasks at test (i.e., smaller–smaller and no–no, respectively). On the other hand, for the smaller-than tasks at test, the object has a congruent classification but an incongruent decision (i.e., smaller–smaller and no–yes, respectively). However, an object that is smaller than a shoebox but bigger than a pencil case has an incongruent classification but a congruent decision for the Smaller-Than-a-Pencil-Case Task at test (i.e., smaller–bigger and no–no, respectively). On the other hand, for the Bigger-Than-a-Pencil-Case Task at test, that object has both an incongruent classification and an incongruent decision (i.e., smaller–bigger and no–yes, respectively; see Figure 4).

This results in a 2 × 2 × 2 factorial design of classification congruency (congruent–incongruent), decision congruency (congruent–incongruent), and test referent (wheelie bin, pencil case). Two further factorial manipulations were also added: (a) repetition (novel, repeated) and (b) prime level (low primed, high primed). Following the logic of previous experiments, the novel stimuli were arbitrarily split into groups equal in size to the repeated conditions. Order of test tasks was counterbalanced across participants.

Materials. The 384 stimuli (a superset of those used in Experiment 1) were split between the two test referents (wheelie bin, pencil case) so that, of the 192 stimuli per referent, 48 were bigger than a shoebox and bigger than a wheelie bin (or pencil case), 96 were bigger than a shoebox and smaller than a wheelie bin (or smaller than a shoebox and bigger than a pencil case), and 48 were smaller than a shoebox and smaller than a wheelie bin (or pencil case). Therefore, 96 stimuli were classification congruent, and 96 were classification incongruent. These 96 stimuli were then randomly assigned to 1 of 8 groups relating to the remaining 8 experimental conditions (a 2 × 2 × 2 Decision Congruency × Prime Level × Repetition design), resulting in 12 stimuli per group. The assignment of groups to experimental conditions was rotated across participants.

Results

After excluding 6.2% of trials with outlying RTs, the percentage of errors, together with mean RTs, mean RT priming, and proportional priming, are shown in Table 5. Analyses of errors revealed no significant main effect of repetition; however, a Repetition × Prime Level interaction was present, reflecting fewer errors for repeated low-primed than high-primed stimuli (see the Appendix).

Table 5
Mean Percentage Errors, Error Priming, Reaction Time (RT), RT Priming, and Proportional Priming (Plus Standard Deviations) Across Classification Congruency, Decision Congruency, Prime Level, and Repetition

	Congruent classification				Incongruent classification			
	Congruent decision		Incongruent decision		Congruent decision		Incongruent decision	
Errors and RTs	LP	HP	LP	HP	LP	HP	LP	HP
Errors								
Novel	3.5 (4.0)	3.6 (3.5)	4.2 (3.7)	4.0 (4.7)	4.9 (4.0)	3.0 (3.6)	5.6 (5.3)	3.0 (4.2)
Repeated	2.4 (3.2)	3.5 (3.2)	3.0 (2.9)	4.0 (4.2)	2.8 (3.2)	3.0 (3.6)	4.0 (3.6)	3.6 (3.7)
Priming	1.0 (4.6)	0.2 (4.5)	1.2 (4.7)	0.0 (6.5)	2.1 (5.5)	0.0 (5.4)	1.6 (6.6)	−0.7 (5.4)
RT								
Novel	838 (132)	845 (139)	887 (106)	849 (92)	939 (118)	918 (114)	910 (144)	926 (136)
Repeated	775 (120)	755 (96)	835 (104)	849 (94)	886 (115)	900 (115)	893 (134)	932 (153)
Priming	60 (87)	91 (67)	47 (75)	−4 (67)	57 (88)	18 (59)	20 (69)	−12 (76)
Proportional priming	.07 (.04)	.10 (.02)	.05 (.03)	−.01 (.03)	.06 (.04)	.02 (.02)	.02 (.03)	−.01 (.03)

Note. The division of novel stimuli into high and low primed is based on an arbitrary, equal split. LP = low primed; HP = high primed; priming = novel − repeated; proportional priming = (novel − repeated)/novel.

A further 7.7% of repeated trials were excluded from RT analysis because of incorrect responses given at study.

Given that there was no significant difference in RTs across the two test referents (wheelie bin, pencil case), $t(23) = 1.12$, $p = .28$, and given the lack of theoretical interest in this manipulation, the RT data were collapsed across test referent for all further analyses. The resulting RT data were entered into a 2 × 2 × 2 × 2 (Classification Congruency × Decision Congruency × Prime Level × Repetition) ANOVA, which revealed several significant interactions and main effects. A similar pattern of results was seen for the proportional measure of priming (see the Appendix).

The highest order interaction was a Decision Congruency × Prime Level × Repetition interaction, $F(1, 23) = 5.63$, $p < .05$. Further tests revealed a significant decrease in priming for high-primed decision-incongruent than for low-primed decision-incongruent trials, $t(23) = 3.64$, $p < .01$, which was not present for the decision-congruent trials, $t(23) = 0.37$, $p = .72$. Therefore, increasing the number of repetitions at study resulted in greater interference when the decision was incongruent at test. Although the Classification Congruency × Prime Level × Repetition interaction did not reach significance, $F(1, 23) = 2.38$, $p = .14$, there was a trend in the same direction described earlier, with significantly decreased priming for high-primed classification-incongruent than for low-primed classification-incongruent trials, $t(23) = 2.91$, $p < .01$, which was not present for classification-congruent trials, $t(23) = 0.98$, $p = .34$.

The main ANOVA also revealed a significant Decision Congruency × Repetition interaction, $F(1, 23) = 19.19$, $p < .001$, as well as a trend toward a Classification Congruency × Repetition interaction, $F(1, 23) = 3.66$, $p = .07$, and no evidence for a three-way Decision Congruency × Classification Congruency × Repetition interaction, $F(1, 23) = .83$, $p = .37$. Given that we predicted greater priming for congruent than incongruent stimuli, one-tailed t tests revealed significantly greater priming for decision-congruent than for decision-incongruent trials, $t(23) = 4.38$, $p < .001$, and for classification-congruent than for classification-incongruent trials, $t(23) = 1.91$, $p < .05$ (collapsed across prime level, classification congruency, and decision congruency, respectively). Thus, congruency of both the decision and the classification significantly affected priming; see Figure 3B(i) and 3B(ii). There were also significant two-way interactions between prime level and repetition, $F(1, 23) = 8.32$, $p < .01$, and between classification congruency and prime level, $F(1, 23) = 4.92$, $p < .05$, as well as main effects of classification congruency, $F(1, 23) = 77.04$, $p < .001$, decision congruency, $F(1, 23) = 21.38$, $p < .001$, and repetition, $F(1, 23) = 41.59$, $p < .001$. However, there were no further interactions involving repetition.

Finally, priming in the incongruent-classification incongruent-decision condition was not reliable (4 ms), $t(23) = 0.42$, $p = .68$, suggesting that once S-R learning is controlled at all three levels of response representation, no additional contributions to priming (i.e., facilitation of component processes) remained.

Discussion

The two main findings of Experiment 7 were (a) significantly greater priming for decision-congruent than for decision-incongruent trials (Figure 3B[i]) and (b) significantly greater priming for classification-congruent than for classification-incongruent trials (Figure 3B[ii]), with no reliable interaction between these two effects. These findings support our prior hypothesis that responses are coded at the level of the classification, separately and simultaneously from the levels of decision and/or action. Of importance, these findings were in the context of conditions that appeared to be matched in their semantic requirements (i.e., differing only in the direction of the comparison—bigger than *X* vs. smaller than *X*—and in the referent, *X*). This makes the congruency effects unlikely to reflect differential levels of conceptual processing.

When both classification and decision were incongruent, there was no reliable priming, consistent with the lack of any contribution from facilitation of conceptual processes. This is unlike Experiment 6, where there was evidence of priming despite no overlap in the classification, decision, or action (in the different-decision different-action condition). However, the third notable finding of Experiment 7 was a significant reduction in priming for high- relative to low-primed stimuli given an incongruent response at test. This would suggest greater amounts of response interference when a stimulus–response pairing has occurred three times at study. This is the first appreciable evidence in the present series of experiments for the presence of interference in S-R learning. We return to this point in the General Discussion section.

Interexperimental Analyses of Experiments 6 and 7

The results of Experiments 6 and 7 suggest that S-R bindings can form at the level of action, decision, and classification. In Experiment 6, we manipulated action and decision. Note, however, that the change in decision also entailed a change in classification (e.g., from *monkey* to *bigger* in the different-decision different-action condition). The decrease in priming associated with this change may therefore reflect the change in classification rather than decision. In Experiment 7, on the other hand, we manipulated decision and classification. Here, however, the change in decision also entailed a change in action (e.g., a switch from *yes* to *no* also entailed a switch from *right* to *left*). As such, the decrease in priming associated with this change may have been due to a change in action rather than decision. It might, therefore, be possible to explain the results of Experiments 6 and 7 by proposing just two levels of response representation, namely, action and classification.

To address this concern, we calculated the difference in proportional priming (to control for baseline RT differences across experiments) between certain conditions from Experiments 6 and 7 (collapsing across prime level). For each experiment, two difference scores were calculated across pairs of conditions. For Experiment 6, these were (a) the difference between the same-action same-decision condition and the different-action same-decision condition and (b) the difference between the different-action same-decision condition and the different-action different-decision condition. For Experiment 7, they were (a) the difference between the classification-congruent decision-congruent condition and the classification-congruent decision-incongruent condition and (b) the difference between the classification-congruent decision-incongruent condition and the classification-incongruent decision-incongruent condition. For both experiments, as can be seen from Table 3, Contrast 1 is a measure of action change, whereas Contrast 2 is a measure of classification change. Of importance, however, Contrast 1 is a pure measure of action change in Experiment 6 but is a measure of both action and decision change in Experiment 7. Similarly, Contrast 2 is a pure measure of classification change in Experiment 7 but is a measure of both classification and decision change in Experiment 6.

Thus, to test for an effect of decision change, a 2 × 2 (Action vs. Classification × Experiment 6 vs. Experiment 7) ANOVA was conducted. If responses form at the level of the decision, Contrast 1 should be greater in Experiment 6 than in Experiment 7, but Contrast 2 should be greater in Experiment 7 than Experiment 6 (i.e., there should be a significant Response Level × Experiment interaction). Such an interaction was indeed significant, $F(1, 40) = 6.05$, $p < .05$. This reflected a larger change in priming for Contrast 2 in Experiment 6 than in Experiment 7, $t(40) = 2.75$, $p < .01$, and a numerical trend for a larger change in priming for Contrast 1 in Experiment 7 than in Experiment 6, $t(40) = 1.20$, $p = .24$. This result is therefore consistent with all three levels of response representation having an effect on priming.

General Discussion

The present series of experiments demonstrates that S-R learning plays a dominant role in long-lag repetition priming of speeded semantic classification tasks. This dominance is revealed once one allows stimuli to become simultaneously bound to multiple different response codes, from the level of the action (e.g., left–right finger press) to the decision (yes–no) to the task-specific classification (e.g., bigger). This dominance was such that there was little evidence remaining for any other contributions to priming, contrary to the common assumption that priming reflects the facilitation of one or more component processes (e.g., faster object identification or semantic retrieval). This brings into question prior interpretations of priming in speeded classification tasks, in both healthy participants (e.g., Bowers & Turner, 2003; Bruce et al., 2000; Light, Prull, & Kennison, 2000; Thompson-Schill & Gabrieli, 1999; Vaidya & Gabrieli, 2000; Vriezen, Moscovitch, & Bellos, 1995), and in individuals with amnesia (Schnyer et al., 2006). It also questions (as originally pointed out by Dobbins et al., 2004) the interpretation of stimulus repetition effects that have been observed in the many neuroimaging experiments that have used such tasks (e.g., Buckner et al., 1998; Henson et al., 2003; Koutstaal et al., 2001; Sayres & Grill-Spector, 2006; Simons, Koutstaal, Prince, Wagner, & Schacter, 2003; Vuilleumier, Henson, Driver, & Dolan, 2002). These repetition effects have been associated with facilitated neural processing and are often used to infer the localization of different representations of stimuli in the brain (Henson, 2003). Instead, they may reflect a bypassing of neural activity by direct retrieval of various response codes (see Dobbins et al., 2004; Horner & Henson, 2008; Race, Shanker, & Wagner, in press).

It is important to note that we are not claiming that all examples of priming reflect S-R learning (i.e., that there is never a role for the facilitation of perceptual or conceptual processing). Indeed, given the evidence for interference effects from previously learned S-R associations (Experiment 7), it is plausible that significant perceptual and/or conceptual contributions may have been masked in the present series of experiments (i.e., for incongruent trials in the orthogonal-question condition). Furthermore, the robust priming of accuracy or response times found in identification paradigms (rather than classification paradigms), such as picture-fragment or word-fragment completion tasks, would appear difficult to explain in terms of S-R learning. In these data-driven tasks (Jacoby, 1983; Roediger & McDermott, 1993; Roediger, Srinivas, & Weldon, 1989), a degraded version of a stimulus is often difficult to identify unless an intact version has been seen previously, offering little opportunity for a prior response to be retrieved until the stimulus is identified through priming (e.g., the Dalmatian dog example in Roediger & McDermott, 1993; Roediger, Srinivas, & Weldon, 1989).

The role of S-R learning in priming has been highlighted previously, in long-term repetition-priming paradigms like the present one (e.g., Dennis & Schmidt, 2003; Dobbins et al., 2004; Logan, 1990), in negative-priming paradigms (e.g., Frings, Rothermund, & Wentura, 2007; MacDonald & Joordens, 2000; Rothermund et al., 2005), and in subliminal-priming paradigms (e.g., Abrams et al., 2002; Damian, 2001; Kiesel et al., 2006, 2007; Klauer et al., 2007; Kunde et al., 2003). S-R learning has also been an important factor in the task-switching literature (e.g., Allport & Wylie, 1999; Koch & Allport, 2006; Waszak et al., 2003). Nonetheless, we are not aware of any existing S-R theory that is sufficiently well specified to explain the present findings. Later, we review our findings and then relate them to two such theories: the instance theory of Logan (1990) and the event file theory of Hommel (1998).

Signatures of S-R Learning

Experiments 1 through 5 revealed three different signatures of S-R learning: (a) significantly greater priming in the same-question than in the reverse-question condition (Figure 2A), (b) significantly greater priming for congruent than incongruent trials in the orthogonal-question condition (Figure 2B), and (c) an increase in priming for high- relative to low-primed stimuli in the same-question condition but not in the reverse-question condition (Figure 2A). Furthermore, Experiments 6 and 7 offered direct evidence of simultaneous bindings between stimuli and (a) actions, from the greater priming in the same-decision same-action condition than in the same-decision different-action condition of Experiment 6 (Figure 3A); (b) classifications, from the greater priming in the classification-congruent conditions than in the classification-incongruent conditions of Experiment 7 (Figure 3B[ii]); and (c) decisions, from the combined analysis of Experiments 6 and 7, where contributions from actions and classifications were effectively subtracted. Note also that the reliable positive priming that was found in the reverse-question condition of Experiments 1 through 5 could also be attributed to retrieval of stimulus-classification bindings, and the net priming in the orthogonal-question condition of Experiments 4 and 5 could be attributed to retrieval of stimulus-decision or stimulus-action bindings on the subset of congruent trials (priming was never reliable for incongruent, orthogonal trials). Indeed, the only condition of all 27 conditions in the present study (collapsing across high- and low-primed stimuli) in which there was reliable priming that would appear difficult to explain in terms of S-R learning was the different-decision different-action condition of Experiment 6 (a finding that may warrant replication with a group of participants completely naive to the subsequent classification task during study).

One question relating to S-R learning is whether retrieval of congruent responses speeds up RTs, whether retrieval of incongruent responses slows down RTs, or both. S-R theories from the long-lag repetition-priming literature have tended to focus on facilitation by congruent responses (e.g., Logan, 1990), whereas theories from task-switching literature have tended to focus on interference from incongruent responses (e.g., Waszak et al., 2003). The data from Experiments 1 through 5 suggest that only congruent responses affect RTs, in that there was no reliable negative priming (e.g., for incongruent trials in the orthogonal-question condition). More important, high-primed stimuli were associated with increased positive priming in the same-question condition but with no change in priming in the reverse-question condition (Figure 2A). The comparison of high- versus low-primed stimuli in Experiment 7, however, revealed reliably less priming for incongruent decisions (and a similar numerical trend for incongruent classifications), suggesting that interference can also occur.

One variable that might affect the amount of response interference is the strength of the task set (Monsell, 1996). Experiments 1 through 5 involved only one task switch between the study phase and the test phase in which priming was measured.[4] The more typical paradigms in which interference is noted in the task-switching literature use multiple switches (Allport & Wylie, 1999; Koch & Allport, 2006; Rubin & Koch, 2006; Waszak & Hommel, 2007; Waszak et al., 2003). Indeed, interference effects are decreased in pure relative to mixed task blocks (Waszak & Hommel, 2007). More frequent task switches may weaken the task set on a given trial, making it more prone to interference from retrieval of responses from a different task (or from retrieval of representations of that task itself; Waszak & Hommel, 2007). It is possible that the test phases in which interference was found in the present Experiment 7 were associated with a weaker task set, possibly because of the changes in both direction (bigger vs. smaller) and referent (e.g., shoebox vs. pencil case) of the task. It is less clear, however, why the strength of task set affects interference by incongruent responses differentially from facilitation by congruent responses. The precise circumstances under which response retrieval tends to facilitate more than it interferes, or vice versa, would appear to deserve further investigation.

Extending Instance Theory?

One concrete example of an S-R theory of priming is the instance theory proposed by Logan (1990), as an extension of his theory of automaticity (Logan, 1988). This theory assumes that the response to the initial presentation of a stimulus is generated by an algorithmic processing route but that this response also becomes stored together with the stimulus in a separate instance. When the stimulus is repeated, there is a race between the algorithmic route and retrieval of any previous instances. When retrieval of a previous instance wins the race, the RT is shorter, producing priming. By assuming that each S-R repetition leads to the formation of a new instance, Logan's (1988, 1990) theory provides an elegant account of both the mean and variance of RTs as a function of number of repetitions.

In situations in which participants realize that responses from previous tasks are likely to be inappropriate (i.e., in the present reverse- and orthogonal-question conditions, respectively), Logan (1988) proposed that they revert to algorithmic processing, ignoring retrieval of previous instances. That is, the system should "run off the relevant algorithm and compute . . . a response" (Logan, 1988, p. 495). Because processing in the algorithmic route is assumed to be unaffected by repetition (unlike the component process view of priming), such situations should therefore not show any priming. However, the reliable priming for our reverse-question condition and congruent trials in the orthogonal-question condition would suggest otherwise.

[4] There were, of course, more task switches across the whole experiment, when participants moved from the study–test phases of one condition to those of another. However, we found no evidence for effects of task order in the combined analysis across Experiments 1 through 5, suggesting that the number of task switches had little detectable effect on the amount of priming.

To accommodate the significant priming in the reverse-question condition, instance theory could assume that retrieval of previous responses was relevant to this task and that such responses were coded solely at the level of the classification.[5] That is, retrieval of an instance might provide a classification of bigger, which is simply remapped to a *no* response when the task is reversed to the Smaller-Than-a-Shoebox Task. If this remapping took some time, this would explain why priming was less in the reverse-question than in the same-question condition. However, it is less clear why high-primed stimuli (coded by three instances) did not then produce greater priming than low-primed stimuli in the reverse-question as well as the same-question condition. More important, one would not expect a congruency effect in the orthogonal-question condition, where decisions and actions are repeated but the classification is quite different (e.g., bigger vs. man-made in Experiments 1 and 2). These data would seem to require either instances that encode multiple levels of response or multiple separate instances for each level of response.

How would such an extended instance theory explain the congruency effect in the orthogonal-question condition? In this condition (unlike the reverse-question condition), responses from previous tasks are completely irrelevant, in which case participants should revert to algorithmic processing, and no priming should occur. One would need to assume instead that previous responses are retrieved, perhaps automatically, even if they are not obviously relevant. This would explain the positive priming for congruent trials. It is less clear, however, how this would explain the lack of reliable priming for incongruent trials (where an incorrect action/decision is retrieved).[6] The only way to account for this congruency effect would seem to be if there is some dynamic interaction between the instance retrieval route and the algorithmic route. During congruent trials, for example, there may be mutual reinforcement between the response retrieved from instances and the response currently favored (even if not yet selected) from the algorithmic route, thus speeding up RTs. During incongruent trials, on the other hand, there may be interference between the action/decision retrieved from instances and the response currently favored by the algorithmic route, thus slowing down RTs. As mentioned earlier, such interference was already implicated by the reduction in priming for high- rather than low-primed incongruent stimuli in Experiment 7. In the case of incongruent trials in the orthogonal-question conditions of Experiments 1 through 5, the interference did not seem sufficient to produce reliable negative priming but may have led to the algorithmic route running to completion, resulting in no net priming (alternatively, interference may, in fact, have slowed RTs, but this was counteracted by a small speeding of RTs from facilitation of perceptual processing). In any case, such interaction between the algorithmic and instance routes would reflect a major departure from the original instance theory.

Extending Event Files?

The simultaneous encoding of multiple levels of response would seem consistent with the event file theory proposed by Hommel (1998). This theory focuses more on interference effects of prior encounters, whereby discrepancies between the present circumstances and retrieved event files tend to slow RTs (Hommel, 2004). Although it does allow for the presence of multiple, separate event files (Waszak & Hommel, 2007), it is generally conceived that such bindings are temporary, and the theory has more often been applied to short-lag priming paradigms (although see Posse, Waszak, & Hommel, 2006). Furthermore, it does not specify a mechanism (such as the race in Logan's, 1988, model) by which multiple records interact to generate a response (e.g., for high-primed stimuli).

More important, it is unclear how the theory predicts positive priming, given its focus on interference effects arising from prior experience. Waszak and Hommel (2007) have presented evidence of positive priming under certain experimental conditions. Although they attributed the increase in priming to a disruption of S-R associations by an intervening task, it is unclear how such a reduction in interference can lead to significant positive priming without a separate mechanism (and, indeed, Waszak & Hommel, 2007, appealed to some form of additional facilitation of perceptual processing). As with instance theory, the present results would seem to require some form of interaction between episodes retrieved from previous trials and the component processes (algorithm) that compute the response in completely novel circumstances.

Other S-R Theories

Although we talked earlier about multiple separate instances resulting from each trial in which an object is repeated (e.g., in our high-primed condition) or possibly for each level of response representation within a single trial, the current data do not imply such episodic representations. Another possible mechanism for S-R learning are the *action triggers* of Kunde et al. (2003), which have been used to explain subliminal priming effects, such as the transfer of priming to different stimuli of the same category (see also Denkinger & Koutstaal, 2009). A third possibility is an associative mechanism, whereby S-R associations become strengthened on each trial in which a stimulus and response are repeated within a given task and become weakened when a stimulus occurs with a different response. Distinguishing episodic versus associative accounts may require testing whether S-R learning effects depend on the precise order or history of S-R pairings.

Note, however, that episodic representations lend themselves naturally to explaining other variables that affect priming, such as the binding of incidentally co-occurring stimuli (McKoon & Ratcliff, 1986), incidentally co-occurring responses (generated by other stimuli on a particular trial, as in negative-priming paradigms; Rothermund et al., 2005), incidentally co-occuring stimulus attributes (Rubin & Koch, 2006), or representations of the task

[5] Indeed, Logan (1990) originally performed an experiment like the present Experiment 6, concluding that responses form at a more abstract level than a simple finger press. Logan referred to such mappings as *stimulus-interpretation mappings,* although it is unclear whether they refer, in the present context, to a decision or a classification.

[6] One might suggest that the system waits for the answer from the algorithmic route during incongruent trials, resulting in no priming. However, given that the system cannot know the correct response until the algorithm finishes, this would also predict no priming for congruent trials. Note that there was no consistent increase in errors for incongruent trials, and in any case, RTs for these trial types were conditioned on the same response at study and test, whether or not those responses were correct.

itself (Koch & Allport, 2006; Waszak & Hommel, 2007). Although the present results do not speak to this issue, experiments by Hommel and colleagues (Keizer, Colzato, & Hommel, 2008) suggest that the records encoding concurrent stimuli may be distinct from (though linked with) those coding concurrent responses. Indeed, it is noteworthy that removal of hippocampal formations in the Macaque impairs formation of arbitrary S-R mappings, whereas formation of stimulus–stimulus mappings is relatively preserved (Wise & Murray, 1999).

Our demonstrations that priming is greatest when the responses at test are consistent with those at study at multiple levels of response representation may appear generally consistent with the idea of transfer appropriate processing (Morris et al., 1977; Roediger, Weldon, & Challis, 1989), that is, that priming is greatest when the overlap between processes engaged at study and test is greatest. However, it is important to note that one cannot explain our findings in terms of general overlap among classifications, decisions, and actions, in that a general strengthening of task-specific connections between a classification (e.g., bigger), a decision (e.g., yes), and an action (e.g., left finger press)—that is, variables related to task set—would affect both repeated and novel stimuli and, therefore, would not produce priming. The key aspect of the multiple levels of response representations in the present context is that they are bound to a specific stimulus.

Facilitation of Perceptual/Semantic Processes

Although the main point of our article has been to see how many of our priming effects can be explained by S-R learning—and most of them can—it is possible that some of the priming effects do reflect facilitation of one or more component processes, rather than S-R learning. One result mentioned earlier that is difficult to explain by S-R learning was the reliable priming in the different-decision different-action condition of Experiment 6. Another effect that might seem more naturally explained by perceptual priming is the overall increase in priming when we degraded our stimuli in Experiment 5 (e.g., Bowers, Vigliocco, & Haan, 1998; Waszak & Hommel, 2007). Note, however, that effects of stimulus degradation do not necessarily imply a purely perceptual locus of priming, because the prior activation of semantic codes also aids recognition when bottom-up input is poor (Eger, Henson, Driver, & Dolan, 2007), and the gradual nature of the stimulus clarification, resulting in slower overall RTs, may have increased the opportunity of influences from explicit (conscious) memory for the prior trial. Another situation was the significant priming in the orthogonal-question condition in Experiment 4, where we switched from pictures to words. As mentioned in the *Discussion* section of this experiment, this significant priming could reflect facilitated phonological/semantic processing of words and/or self-generation of a specific exemplar of an object, through mental imagery.

These component process interpretations of our findings would be consistent with prior claims for perceptual and/or semantic contributions to priming in classification tasks (Bowers & Turner, 2003; Bruce et al., 2000; Light et al., 2000; Thompson-Schill & Gabrieli, 1999; Vaidya & Gabrieli, 2000; Vriezen et al., 1995). Separate perceptual and conceptual contributions to priming would also be consistent with neuropsychological research showing that damage to the occipital lobe can impair perceptual forms of priming (Keane, Gabrieli, Mapstone, Johnson, & Corkin, 1995), whereas damage to the lateral temporal lobe can impair semantic forms of priming (Bondi & Kaszniak, 1991), and would be consistent with disruptions to subsequent priming caused by transcranial magnetic stimulation to the occipital (Pobric, Schweinberger, & Lavidor, 2007) and temporal lobes (Blaxton, 1999) during stimulus encoding. However, it is unclear whether such research sufficiently controlled for possible S-R learning effects, particularly when response coding is extended to the more abstract level of task-specific classification.

Finally, the potential dominance of S-R learning also calls into question many of the neural repetition effects recently observed with fMRI, which have often used classification paradigms (see Dobbins et al., 2004, who first showed likely effects of S-R learning on neural repetition effects). If the typical decreases in neural response (so-called *repetition suppression*; Henson, 2003) reflect bypassing of activity in brain regions by response retrieval, rather than more efficient processing in those regions, then some of the claims that repetition suppression offers a more sensitive tool for probing stimulus representations in the brain (e.g., Naccache & Dehaene, 2001) become questionable. Note, however, that repeated presentation of a stimulus may in fact result in more efficient neural processing of that stimulus, even if this increased neural efficiency does not translate directly (or only negligibly) into the final behavioral measure of priming. This might explain the repetition suppression for visual objects that has been observed in ventral occipitotemporal regions by fMRI studies in which effects of S-R learning are unlikely (Henson, Shallice, & Dolan, 2000), and yet why the size of such repetition suppression across participants often does not correlate with the amount of behavioral priming that they show (Horner & Henson, 2008; Maccotta & Buckner, 2004; Race et al., in press; Sayres & Grill-Spector, 2006).

Conclusion

We have provided evidence for the contribution of S-R learning to long-lag repetition priming in speeded classification tasks. Although such effects have been reported previously, we present novel evidence suggesting that S-R associations can form at three distinct levels of response representation: the action (e.g., left–right finger press), the decision (yes–no), and the task-specific classification (e.g., bigger). Once one allows stimuli to be simultaneously bound to such multiple response codes, one is able to explain most (if not all) of the priming effects seen across the present experiments. These results are contrary to the common assumption that priming reflects facilitation of one or more component processes, and they bring into question prior studies using speeded classification paradigms that have interpreted priming and repetition-related signal decreases (as measured by fMRI) in terms of perceptual/conceptual processing.

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Appendix

Analyses of Error Rates and Proportional Measures of Priming

Experiment 1

The error data were entered into a $3 \times 2 \times 2$ ANOVA that revealed a significant effect of task, $F(1.9, 21.2) = 59.74$, $p < .001$, reflecting greater accuracy for the orthogonal-question Man-Made Task compared with the Bigger-Than-a-Shoebox Task in both the same-question condition, $t(11) = 8.66$, $p < .001$, and the reverse-question condition, $t(11) = 10.78$, $p < .001$; same versus reverse, $t(11) = 0.50$, $p = .63$. No main effect of repetition was present, $F(1, 11) = 0.01$, $p = .93$, although the Repetition $\times$ Prime Level interaction was close to significance, $F(1.7, 18.8) = 3.53$, $p = .06$. Despite this trend for a significant interaction, no repetition effects were seen for any of the three tasks individually, ts < 1.96, ps $> .08$, when collapsed across prime level.

(*Appendix continues*)

The mean RT data for novel stimuli alone were entered into a 3 × 2 (Task × Prime Level) ANOVA revealing a main effect of task, $F(1.7, 18.4) = 10.12$, $p < .01$. Further tests revealed longer RTs for the same-question and reverse-question conditions compared with the orthogonal-question condition, ts > 3.61, ps $< .01$, presumably because participants found the Man-Made Task easier than the Bigger-Than-a-Shoebox Task. To control for these baseline RT differences, a proportional priming measure—(novel−repeated)/novel—was used in a 3 × 2 (Task × Prime Level) ANOVA. This revealed a main effect of task, $F(1.6, 17.2) = 10.42$, $p < .001$, replicating the Task × Repetition interaction for the subtractive measure of priming reported in the main article. Proportional priming was greater in the same-question condition relative to the reverse-question condition, $t(11) = 3.64$, $p < .01$, and the orthogonal-question condition, $t(11) = 4.61$, $p < .01$; reverse versus orthogonal, $t(11) = 1.42$, $p = .18$. Proportional priming was significant in both the same-question condition, $t(12) = 8.60$, $p < .001$, and the reverse-question condition, $t(12) = 2.82$, $p < .05$, but not the orthogonal-question condition, $t(12) = 0.72$, $p = .49$.

Experiment 2

A 3 × 2 × 2 ANOVA on errors revealed a significant main effect of task, $F(1.5, 25.8) = 92.69$, $p < .001$, reflecting greater accuracy in the orthogonal-question condition compared with both the same-question condition, $t(17) = 11.67$, $p < .001$, and the reverse-question condition, $t(17) = 9.56$, $p < .001$; same versus reverse, $t(17) = 0.66$, $p = .52$. No main effect of repetition was present, $F(1, 17) = 0.53$, $p = .48$, nor did this variable interact significantly with any other, Fs < 2.33, ps $> .13$.

An ANOVA on the mean RT data for novel items in Experiment 2 showed a main effect of task, $F(1.4, 24.5) = 24.44$, $p < .001$. Further tests revealed shorter RTs for the same-question and reverse-question conditions compared with the orthogonal-question condition, ts > 4.70, ps $< .001$. Therefore, a 3 × 2 (Task × Prime Level) ANOVA was performed on the proportional priming scores. This showed a significant main effect of task, $F(1.7, 29.3) = 9.74$, $p < .001$, replicating the Task × Repetition interaction for the subtractive measure of priming reported in the text. Proportional priming was greater in the same-question condition relative to the reverse-question condition, $t(17) = 4.18$, $p < .01$, and the orthogonal-question condition, $t(17) = 4.28$, $p < .01$; reverse versus orthogonal, $t(17) = 0.56$, $p = .58$. Proportional priming was significant in the same-question condition, $t(17) = 5.98$, $p < .001$, but not the orthogonal-question condition, $t(17) = 0.60$, $p = .56$, although it failed to reach significance in the reverse-question condition, $t(17) = 1.10$, $p = .29$, unlike when using the subtractive measure.

Experiment 3

A 3 × 2 × 2 ANOVA on errors revealed a main effect of task, $F(1.3, 22.7) = 23.66$, $p < .001$, reflecting greater accuracy in the same- than the reverse-question condition, $t(17) = 4.42$, $p < .001$, and in the reverse- than the orthogonal-question condition, $t(17) = 3.72$, $p < .01$; same versus orthogonal, $t(17) = 5.92$, $p < .001$. The greater accuracy in the same-question compared with the reverse-question condition may reflect the greater amount of experience participants had with the Bigger-Than-a-Shoebox Task compared with the Smaller-Than-a-Shoebox Task (given that the task at study was always the Bigger-Than-a-Shoebox Task). No main effect of repetition was present, $F(1, 17) = 1.13$, $p = .30$, nor did this variable interact with any other, Fs $< .81$, ps $> .44$.

An ANOVA on mean RTs for novel items in Experiment 3 revealed a main effect of task, $F(1.6, 27.8) = 39.62$, $p < .002$. Further tests revealed shorter RTs for the same-question condition than for the reverse-question condition, $t(17) = 3.59$, $p < .01$, and for the reverse-question condition than for the orthogonal-question condition, $t(17) = 6.08$, $p < .001$; same vs. orthogonal, $t(17) = 7.29$, $p < .001$. Although the longer RTs in the orthogonal-question condition were expected, given the difficulty of the Taller-Than-Wide Task, it is unclear why the reverse-question condition produced longer RTs than the same-question condition. In any case, a 3 × 2 ANOVA on the proportional priming data revealed a significant main effect of task, $F(1.5, 25.3) = 23.55$, $p < .001$—the Task × Prime Level interaction also approached significance, $F(1.9, 32.3) = 2.66$, $p = .09$—replicating the Task × Repetition interaction for the subtractive measure of priming reported in the text. Priming was greater in the same-question condition compared with the reverse-question condition, $t(17) = 6.74$, $p < .001$, and the orthogonal-question condition, $t(17) = 5.44$, $p < .001$; reverse versus orthogonal, $t(17) = 1.78$, $p = .09$. Proportional priming was significant in the same-question condition, $t(17) = 8.79$, $p < .001$, and was a trend in the reverse-question condition, $t(17) = 1.69$, $p = .12$, but not in the orthogonal-question condition, $t(17) = .37$, $p = .72$.

Experiment 4

A 3 × 2 × 2 ANOVA on errors revealed a main effect of task, $F(1.5, 26.1) = 19.58$, $p < .001$, reflecting greater accuracy in the same-question than the reverse-question condition, $t(17) = 3.05$, $p < .01$, and in the reverse-question than the orthogonal-question condition, $t(17) = 3.10$, $p < .01$; same versus orthogonal, $t(17) = 6.67$, $p < .001$. No main effect of repetition was present, $F(1, 17) = 0.05$, $p = .83$; however, a significant Task × Repetition interaction was seen, $F(1.7, 28.3) = 3.53$, $p < .05$. Post hoc tests revealed significantly greater errors for repeated stimuli in the reverse-question condition compared with the same-question condition, $t(17) = 3.17$, $p < .01$. Indeed, there was a significant effect of repetition in the reverse-question condition, $t(17) = 2.75$, $p < .05$, that was not present in the same-question condition, $t(17) = 1.42$, $p = .17$, or the orthogonal-question condition, $t(17) = 0.92$, $p = .37$. These results reinforce the effect of priming, in that repetition of stimuli in the reverse-question task both reduces RTs and increases errors (even if both effects arise from a speed–accuracy trade-off).

An ANOVA on mean RTs for novel items in Experiment 4 revealed a main effect of task, $F(1.8, 30.2) = 63.27$, $p < .001$. Further tests revealed shorter RTs for the same-question than the reverse-question condition, $t(17) = 2.40$, $p < .05$, and for the reverse-question than the orthogonal-question condition, $t(17) = 7.37$, $p < .001$; same versus orthogonal, $t(17) = 11.08$, $p < .001$. A 3 × 2 (Task × Prime Level) ANOVA on the proportional priming data revealed a significant main effect of task, $F(1.8, 29.8) = 21.65$, $p < .001$, replicating the Task × Repetition interaction for the subtractive measure of priming reported in the main article. Priming was greater in the same-question compared with the reverse-question condition, $t(17) = 5.32$, $p < .001$, and the orthogonal-question condition, $t(17) = 7.11$, $p < .001$; reverse versus orthogonal, $t(17) = 0.03$, $p = .98$. Proportional priming was significant in the same-question condition, $t(17) = 10.03$, $p < .001$, a trend in the reverse-question condition, $t(17) = 1.52$, $p = .16$, and significant in the orthogonal-question condition, $t(17) = 2.24$, $p < .05$.

Experiment 5

A $3 \times 2 \times 2$ ANOVA on errors revealed a main effect of task, $F(1.5, 24.9) = 15.73$, $p < .001$, reflecting greater accuracy in the same-question condition, $t(17) = 4.77$, $p < .001$, and the reverse-question condition, $t(17) = 3.82$, $p < .001$, than in the orthogonal-question condition; same versus reverse, $t(17) = 0.78$, $p = .48$. No main effect of repetition was present, $F(1, 17) = 0.30$, $p = .59$, nor did this variable significantly interact with any other, $F\text{s} < 1.47$, $p\text{s} > .24$.

An ANOVA on mean RTs for novel items in Experiment 5 revealed a main effect of task, $F(1.4, 23.3) = 31.11$, $p < .001$. Further tests revealed shorter RTs for the same-question than the reverse-question condition, $t(17) = 5.59$, $p < .001$, and for the reverse-question than the orthogonal-question condition, $t(17) = 3.71$, $p < .01$; same versus orthogonal, $t(17) = 7.33$, $p < .001$. A 3×2 ANOVA on the proportional priming data revealed a significant main effect of task, $F(1.7, 28.4) = 27.90$, $p < .001$—the Task × Prime Level interaction also approached significance, $F(1.6, 26.5) = 3.48$, $p = .06$—replicating the Task × Repetition interaction for the subtractive measure of priming reported in the text. Further tests revealed significantly greater priming in the same-question than the reverse-question condition, $t(17) = 3.15$, $p < .01$, and in the reverse-question than the orthogonal-question condition, $t(17) = 3.67$, $p < .01$; same versus orthogonal, $t(17) = 8.98$, $p < .001$. Proportional priming was significant in the same-question, $t(17) = 16.45$, $p < .001$, reverse-question, $t(17) = 11.03$, $p < .001$, and orthogonal-question, $t(17) = 3.13$, $p < .05$, conditions.

Experiment 6

A $3 \times 2 \times 2$ ANOVA on errors did not reveal any significant main effects or interactions: repetition, $F(1, 17) = 0.11$, $p = .75$.

An ANOVA on mean RTs for novel items in Experiment 6 did not reveal any reliable differences, $F\text{s} < 2.28$, $p\text{s} > .12$. Nonetheless, analysis of proportional priming was performed for completeness. A 3×2 ANOVA on the proportional priming data revealed significant main effects of task, $F(1.5, 24.7) = 23.62$, $p < .001$, and prime level, $F(1, 17) = 8.20$, $p < .05$. The main effect of task replicates the Task × Repetition interaction for the subtractive measure of priming reported in the text. The main effect of prime level replicates the Prime Level × Repetition interaction for the subtractive measure of priming, showing greater priming for high- than low-primed items irrespective of task. Further tests revealed significantly greater priming in the same-action same-decision than the different-action same-decision condition, $t(17) = 3.00$, $p < .01$, and in the different-action same-decision than the different-action different-decision condition, $t(17) = 3.99$, $p < .01$. Proportional priming was significant in the same-action same-decision, $t(17) = 13.13$, $p < .001$, the different-action same-decision, $t(17) = 10.14$, $p < .001$, and the different-action different-decision, $t(17) = 2.43$, $p < .05$, conditions.

Experiment 7

A $2 \times 2 \times 2 \times 2$ ANOVA on errors did not reveal a main effect of repetition, $F(1, 23) = 1.39$, $p = .25$; however, a Repetition × Prime Level interaction was present, $F(1, 23) = 5.00$, $p < .05$, reflecting fewer errors for low-primed repeated stimuli (compared with novel stimuli), $t(23) = 2.08$, $p < .05$, which was not present for high-primed repeated stimuli, $t(23) = .21$, $p = .84$. A Classification Congruency × Prime Level interaction was also present, $F(1, 23) = 9.37$, $p < .001$, reflecting greater errors for high- than low-primed classification-incongruent stimuli, $t(23) = 2.37$, $p < .05$, which was not present for classification-congruent stimuli, $t(23) = 1.47$, $p = .19$.

An ANOVA on mean RTs for novel items in Experiment 7 revealed a significant Classification Congruency × Decision Congruency × Prime Level interaction, $F(1, 23) = 10.30$, $p < .01$, plus a main effect of classification congruency, $F(1, 23) = 37.00$, $p < .001$. A $2 \times 2 \times 2$ (Classification Congruency × Decision Congruency × Prime Level) ANOVA on proportional priming showed a significant Decision Congruency × Prime Level interaction, $F(1, 23) = 6.80$, $p < .05$—Classification Congruency × Prime Level, $F(1, 23) = 2.15$, $p = .16$—plus main effects of decision congruency, $F(1, 23) = 23.37$, $p < .001$, and prime level, $F(1, 23) = 6.25$, $p < .05$; the main effect of classification congruency also approached significance, $F(1, 23) = 3.94$, $p = .06$. Further analyses (as in the main text) revealed significantly greater priming for low- than high-primed incongruent trials for both decision congruency, $t(23) = 3.73$, $p < .01$, and classification congruency, $t(23) = 2.68$, $p < .01$, which was not present for congruent trials, $t\text{s} < .95$, $p\text{s} > .35$. Furthermore, priming was significantly greater for congruent than incongruent trials for both decision congruency, $t(23) = 4.88$, $p < .001$, and classification congruency, $t(23) = 2.08$, $p < .05$. Therefore, effects of both classification and decision congruency were still present despite making allowances for baseline RT differences.

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