Journal of Experimental Psychology:
Learning, Memory, and Cognition
1991, Vol. 17, No. 1, 3–19

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0278-7393/91/$3.00

Implicit Memory for Possible and Impossible Objects: Constraints on the Construction of Structural Descriptions

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Four experiments examined implicit memory or priming effects on an object decision task in which subjects decided whether structurally possible or impossible novel objects could exist in three-dimensional form. Results revealed equivalent levels of priming for possible objects after 1 vs. 4 5-s exposures to the same structural encoding task (Experiment 1) and when objects were studied with a single structural encoding task or 2 different structural encoding tasks (Experiment 3). Explicit memory, by contrast, was greatly affected by both manipulations. However, priming of possible objects was not observed when Ss were given only a single 1-s exposure to perform a structural encoding task (Experiment 2). No evidence for priming of impossible objects was observed in any of the 4 experiments. The data suggest that object decision priming depends on a presemantic structural description system that is distinct from episodic memory.

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Implicit memory refers to unintentional retrieval of previously acquired information on tests that do not require conscious or explicit recollection of specific previous experiences (Graf & Schacter, 1985; Schacter, 1987). Perhaps the most extensively investigated type of implicit memory is known as *direct priming*: facilitated performance on an implicit memory test following exposure to a specific stimulus (e.g., Cofer, 1967; Tulving & Schacter, 1990). Although there is considerable evidence that priming and explicit memory can be dissociated by various experimental manipulations and subject factors (Richardson-Kavehn & Bjork, 1988; Schacter, 1987; Shimamura, 1986), most of this evidence is based on studies that have used words and other verbal materials. There has been considerably less research concerning implicit memory for nonverbal information, and much of this work has examined priming on tasks that include a significant verbal component, such as naming or identifying pictures of common objects (cf. Durso & Johnson, 1979; Jacoby, Baker, & Brooks, 1989; Mitchell & Brown, 1988; Warren & Morton, 1982; Warrington & Weiskrantz, 1968; Weldon & Roediger, 1987; for review and discussion, see Schacter, Delaney, & Merikle, 1990).

As Schacter et al. (1990) pointed out, research on priming of nonverbal information is important for a number of reasons: (a) It is necessary to provide a broad empirical picture of the nature and characteristics of priming, (b) it will help to ensure that theorizing about implicit memory is not overly constrained by idiosyncratic properties of verbal materials, and (c) it can suggest links between the study of memory and the study of perception. In addition, because memory for nonverbal information must have developed earlier in phylogeny than memory for verbal information, research concerning priming of nonverbal information is significant from evolutionary and ecological perspectives (e.g., Sherry & Schacter, 1987; Tulving & Schacter, 1990).

In a recent article, Schacter, Cooper, and Delaney (1990a) reported a series of experiments concerned with priming of newly acquired nonverbal information that does not have a preexisting memory representation (see also Bentin & Moscovitch, 1988; Gabrieli, Milberg, Keane, & Corkin, 1990; Kroll & Potter, 1984; Musen & Treisman, 1990). More specifically, Schacter et al. (1990a) developed a paradigm to examine implicit and explicit memory for novel three-dimensional objects. Target materials in these experiments were line drawings such as those displayed in Figure 1. All of the target objects are novel or unfamiliar in the sense that they do not represent actual objects that exist in the three-dimensional world. However, one half of the objects are structurally possible; their surfaces and edges are connected so that they could exist in three-dimensional form. The other half of the objects, in contrast, are structurally impossible and could not exist in three dimensions: They contain ambiguous lines and planes that create impossible relations between surfaces and edges within the figure (e.g., Draper, 1978; Penrose & Penrose, 1958).

To assess implicit memory for these objects, an object decision test was devised in which subjects, given 100-ms exposures to possible and impossible objects, decided whether each drawing was structurally possible or impossible (for a different type of object decision priming task, see Kroll & Potter, 1984). Schacter et al. (1990a) argued that accurate performance on the object decision test requires access to

This research was supported by Air Force Office of Scientific Research Grant 90-0187 to Daniel L. Schacter and Lynn A. Cooper.

We wish to thank Judith Kroll, Gail Musen, and Henry Roediger for their useful comments on an earlier draft of the manuscript. Copies of stimuli used in the experiments described in this article are available from Daniel L. Schacter or from Lynn A. Cooper.

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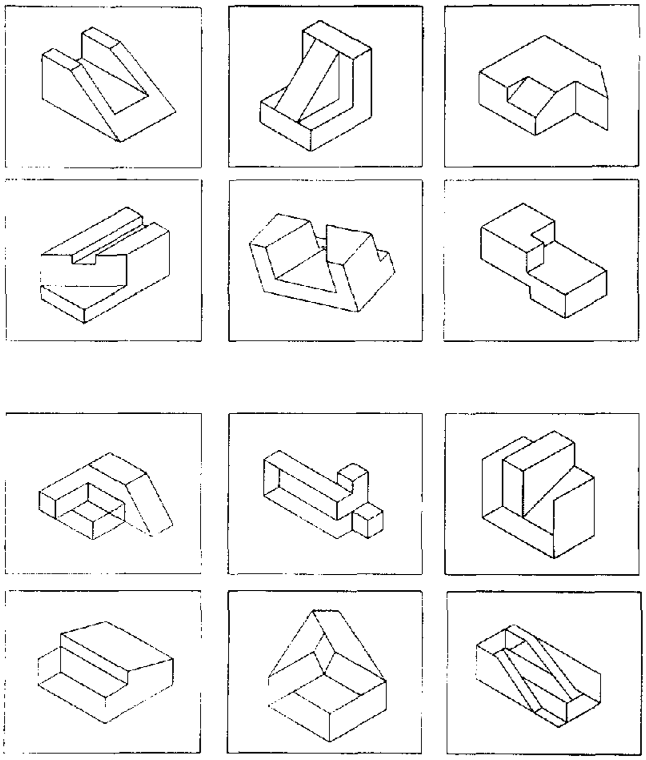

Figure 1. Representative examples of target objects. (The figures in the upper two rows depict structurally possible objects that could exist in three-dimensional form; figures in the lower two rows depict structurally impossible objects that could not exist in three-dimensional form.)

information about the global, three-dimensional structure of each object. In conformity with the principles of transfer-appropriate processing and encoding specificity (e.g., Morris, Bransford, & Franks, 1977; Roediger, Weldon, & Challis, 1989; Tulving & Thomson, 1973), it follows that prior encoding of such information should produce priming on the object decision task. Pilot work indicated that without any prior exposure to the drawings, object decision accuracy was about 65% correct for both possible and impossible objects. To examine priming, one half of the drawings on the object decision test were presented to subjects on a prior study list, and the other half were new items that had not been previously presented. Priming in this paradigm is indicated by more accurate object decision performance for previously presented objects than for nonpresented objects. Explicit memory for the objects were assessed with a conventional yes/no recognition test.

An initial experiment yielded four noteworthy results. First, significant priming was observed after a study task that required encoding of information about the global three-dimensional structure of target objects (indicating whether each object faced primarily to the left or to the right), but no significant priming was found following a study task that required encoding of the local features of target objects (indicating whether each object had more horizontal than vertical lines). Second, priming was observed only for structurally possible objects; no priming was observed for structurally impossible objects. Third, the magnitude of priming for possible objects in the left/right encoding condition did not differ significantly when the object decision test was preceded by a recognition test in which all target objects were exposed, and when the object decision test alone was given. Fourth, priming showed stochastic independence from explicit memory—that is, the probability of recognizing a previously studied figure was uncorrelated with the probability of making a correct object decision about that figure (cf. Hayman & Tulving, 1989a; Tulving, Schacter, & Stark, 1982).

In a second experiment, implicit and explicit memory were compared after the left/right encoding task and an elaborative encoding task in which subjects were required to think of a familiar object from the real world that each drawing reminded them of most. Performance in the left/right condition provided a close replication of the results of the first experiment. As expected, the elaborative encoding task produced significantly higher recognition memory performance than did the left/right task. By contrast, there was no priming on the object decision task following elaborative encoding, thus indicating that implicit and explicit memory for novel objects can be dissociated experimentally. A subsequent experiment showed that significant object decision priming could be observed following elaborative encoding when the task ensured that subjects generated a three-dimensional elaboration for target objects by requiring them to classify each object into one of three categories of three-dimensional objects.

On the basis of these results, Schacter et al. (1990a) argued that priming on the object decision task depends on initial encoding of, and subsequent access to, a structural description (e.g., Marr, 1982; Marr & Nishihara, 1978; Palmer, 1975; Reed, 1974; Sutherland, 1968) of target objects—that is, a representation of the structural relations that define an object. It was argued further that this kind of information is handled by a presemantic structural description system (Riddoch & Humphreys, 1987) that is distinct from the episodic memory system that underlies explicit remembering of objects (see also Schacter, 1990). The structural description system, which can be viewed as one subsystem of a more general perceptual representation system (Schacter, 1990; Schacter, Rapcsak, Rubens, Tharan, & Laguna, 1990; Tulving & Schacter, 1990; cf. Johnson, 1983), is dedicated to the representation or retrieval of information about the form and structure of visual objects. This system is not, however, involved in the representation or retrieval of semantic information about objects—that is, functions that an object can perform or associative properties of an object, such as where it can be found or other objects to which it is functionally related.

Independent evidence for the existence of a structural description system has been provided by neuropsychological research on visual object agnosia that has shown that access to structural knowledge of objects can be preserved in patients whose access to functional and associative knowledge of objects is severely impaired (e.g., Riddoch & Humphreys, 1987; Warrington, 1982; Warrington & Taylor, 1978; see Schacter, 1990, for further discussion). Moreover, a good deal of research on visual perception has examined structural representations of objects, independent of their functional or associa-

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tive properties (e.g., Biederman, 1987; Palmer, 1975; Sutherland, 1968; for review, see Pinker, 1984). With respect to the object decision task, the structural description hypothesis is consistent with the observed independence of implicit and explicit memory for novel objects and also accounts for the finding that priming does not require any semantic or elaborative study processing of target objects. This idea also suggests an interesting explanation for the failure to observe priming of impossible objects: It may be difficult and perhaps impossible to compute a structural description that preserves global, three-dimensional information about an impossible object. If forming a global representation of an impossible object exceeds the computational capacity of the structural description system, and object decision priming depends on gaining access to a previously encoded global description of an object, then it follows that priming of impossible objects will not be observed.

In this article we explore further priming of novel visual objects, with a view toward both elucidating the properties of the phenomenon and clarifying its theoretical implications. Experiment 1 examines the effects of repetition of structural encoding operations on object decision and recognition performance; it also assesses the idea that lack of priming for impossible objects reflects a limitation on the computational capacities of the structural description system by attempting to rule out various alternative explanations of the phenomenon. Experiment 2 explores the conditions under which structural descriptions are formed by assessing whether encoding objects from brief study exposures provides a basis for priming. Experiment 3 attempts to determine whether priming of possible objects can be increased, and priming of impossible objects observed at all, when subjects are induced to encode different types of structural information about target drawings. Experiment 4 investigates whether priming of impossible objects is observed when size differences among target objects are eliminated.

Experiment 1

The main purposes of Experiment 1 were twofold: (a) to assess alternative explanations of the failure to find priming of impossible objects, and (b) to replicate previous findings of priming for possible objects under different task conditions and delineate additional characteristics of the phenomenon.

Consider first the lack of priming for impossible objects. Although this phenomenon may reflect computational constraints on the structural description system, several other interpretations can be offered. One possibility discussed by Schacter et al. (1990a) concerns the criteria that were used to select target objects for inclusion in the initial experiments. In a pilot study, Schacter et al. gave 20 subjects unlimited time to judge whether candidate objects were possible or impossible; one half of the objects had been drawn to appear possible, and the other half had been drawn to appear impossible. An attempt was made to select as target items only those objects that yielded high levels of agreement across subjects. There was 97% agreement concerning the possible objects that were selected, but only 87% agreement concerning impossible objects. Failure to observe priming of impossible objects in the subsequent experiments may thus be attributable, at least in part, to the fact that there was relatively low agreement about whether these objects were indeed impossible. In fact, Schacter et al. noted that the object decision data for impossible objects showed marked fluctuation both within and between experiments and suggested that this unstable pattern might be related to the low intersubject agreement about impossible objects. To address this issue, we constructed a new and expanded set of possible and impossible objects and selected as target materials only those objects on which there was 95% or more intersubject agreement (see materials section of Experiment 1 for details).

A second potential reason for the lack of priming of impossible objects concerns the instructions and response requirements of the object decision task used in the earlier experiments. Specifically, instructions for the object decision test emphasized detection of possible objects; subjects were instructed to press one response key if an object "could be a possible object" and another response key if it "could not be a possible object." With these instructions, an impossible response was effectively a negative response. As discussed in the context of failures to observe priming of pseudowords or nonwords, lack of priming may sometimes be attributable to the influence of a negative response set (e.g., Feustel, Shiffrin, & Salasoo, 1983). Accordingly, we altered task instructions so that an *impossible* response was no longer defined explicitly as a negative response, and subjects were encouraged to process the impossible objects carefully.

A third issue is whether lack of priming for impossible objects was simply a consequence of generally weak or degraded memory for these objects. As noted earlier, explicit memory was consistently lower for impossible objects than for possible objects, thereby suggesting that the memory representation for impossible objects was simply not strong enough to support priming. This account seems unlikely in view of the fact that we observed stochastic independence between priming and explicit memory for possible objects. Nevertheless, we attempted to increase the likelihood of observing priming for impossible objects by including a condition in which subjects were given four successive exposures to the study list. On each exposure, they performed the left/right encoding task that has yielded priming of possible objects in previous studies. We expected that four repetitions of the left/right task would yield high levels of explicit memory for impossible objects. The question is whether priming of impossible objects will be observed under these conditions.

The repetition manipulation was also intended to provide further information concerning priming of possible objects. In our previous experiments, a second exposure to previously studied objects on a yes–no recognition test failed to produce more priming on the subsequent object decision test than did a single study exposure, hence suggesting that priming of possible objects may be insensitive to the number of prior exposures. However, a similar lack of priming was also documented in several experimental conditions when an object was first exposed on the recognition test (as a lure item). This latter finding suggests that the absence of test-induced priming may be attributable to the type of processing in which subjects engage on the recognition test and may not reflect some sort

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of general insensitivity of object decision priming to the number of prior exposures. Comparison of priming for possible objects following one versus four study exposures should illuminate the matter.

Method

Selection of target materials. In order to create a set of materials in which there was equivalent intersubject agreement concerning possible and impossible objects, a set of 50 possible and 50 impossible objects similar to those displayed in Figure 1 was created. All 50 impossible objects were drawn by one of the experimenters (S.M.D.). Of the 50 possible objects 40 were modified by the same experimenter from a set used originally by Cooper (1990), and 10 possible objects were taken directly from this latter set. Impossible objects all contained ambiguous lines and planes that produced impossible relations between surfaces and edges within the figure. Possible objects, on the other hand, did not have any ambiguities that suggested impossible relations among surfaces and edges; each plane in the figure depicted a surface, each line an edge.

To assess intersubject agreement, a pilot study was performed in which line drawings of the 50 possible and 50 impossible objects were randomly intermixed and shown to 20 subjects (University of Arizona undergraduate and graduate students); they were given unlimited time to classify each object as either possible or impossible. Objects were drawn in black outline on white 8-1/2 in. × 11 in. (21.59 cm × 27.94 cm) sheets and shown to subjects individually. Our criterion for considering an individual object for inclusion in the experimental set was an agreement rate of 95% or higher—that is, either 19 or 20 subjects had to classify a possible object as possible or an impossible object as impossible. We then created computer-generated line drawings of all objects, using a Compaq 386 Deskpro computer and 12 in. (30.48 cm) Princeton Ultrasync Monitor, randomly mixed them, and presented the drawings on an object decision test to a new sample of 20 undergraduates under the same conditions used in the experiments described later in this article. Specifically, each object was presented for 100 ms, followed by a darkened screen. The objects subtended a mean visual angle of 8° when viewed from 60 cm. The drawings were presented in medium resolution, and they appeared white against a uniform dark gray background. Presentation of each drawing was preceded by a fixation point that appeared in the middle of the screen. Subjects initiated presentation of the object by pressing the center key on a three-key personal computer (PC) mouse that they controlled with their right hand. Once the item appeared, subjects pressed either the left or the right response key to indicate whether the object was possible or impossible; one half of the subjects used the left key to indicate a possible response and the right key to indicate an impossible response; this response mapping was reversed for the other half of the subjects. A total of 10 practice items were presented at the 100-ms rate before presentation of the 100 critical items.

Subjects were told that they would be viewing a series of briefly exposed drawings and deciding whether each figure could actually exist in the real world. They were told that some of the drawings represented valid, possible three-dimensional objects that could exist in the world, whereas other drawings represented impossible figures that could not exist as three-dimensional objects in the real world, and that their task was to decide which objects were possible and which were impossible. Examples of possible and impossible objects were then shown to subjects. They were informed that all possible objects had to have volume and be solid, that every plane on the drawing represented a surface of the object, that all surfaces could face in only one direction, and that every line on the drawing necessarily represented an edge on the object. The experimenter then asked the subject to indicate why several sample impossible objects were impossible and explained the impossibilities to the subjects as needed. Subjects were then instructed in the use of a three-button mouse to make their responses and told to focus on the central fixation point before each trial.

In all, 20 possible and 20 impossible objects were selected for inclusion in the experimental set. As noted earlier, there was either 95% or 100% intersubject agreement about each selected object, yielding overall agreement rates of 99% for possible and impossible objects. In addition, we attempted to select objects that yielded an overall baseline classification rate of .60 to .65 in the 100-ms exposure condition, as in our previous experiments. The baseline rate was .61 for the selected possible objects and .64 for the selected impossible objects; baseline rates for individual objects ranged from .51 to .80.

Subjects. A total of 80 University of Arizona undergraduates participated in the main experiment in return for course credits or a payment of $5; 20 subjects were randomly assigned to one of the four between-subjects conditions.

Design. The main design consisted of a 2 (one vs. four study exposures) × 2 (object decision test vs. recognition test) × 2 (possible vs. impossible objects) × 2 (studied vs. nonstudied drawings) mixed factorial. The first two factors, number of study exposures and type of test, were between-subjects variables; the latter two factors, object type and item type, were within-subjects variables. In addition, the object decision test was either given alone or after the recognition test, thus creating a test order variable for the object decision analysis.

The target set of 20 possible and 20 impossible objects described earlier was randomly divided into two subsets, A and B. Each subset consisted of 10 possible and 10 impossible objects. The experiment was completely counterbalanced so that each subset appeared equally often as studied and nonstudied drawings in the main experimental conditions.

Procedure. All subjects were tested individually under conditions of incidental learning: They were told that the experiment concerned object perception, and no mention was made of any subsequent memory test. Subjects in both the one- and the four-exposure groups were told that a series of drawings would appear on the computer monitor for 5 s, and that their task was to judge whether each object appeared to be facing primarily to the left or primarily to the right. Subjects were told to use the entire 5 s to inspect each object carefully and to make an accurate left/right judgment because the objects were often not as simple as they appeared. The task began with the presentation of 5 practice items, followed by the presentation of the 10 possible and 10 impossible target objects in random order. For the four-exposure group, the study list was presented three more times after the initial exposure, each time in a different random order.

Immediately after the study list presentation, one half of the subjects were given the instructions for the object decision test described earlier, and the other half were given instructions for the recognition test. The object decision instructions included three modifications of the instructions used by Schacter et al. (1990a). First, to reduce the likelihood that the previous failure to observe priming of impossible objects is attributable to inadequate comprehension of what constitutes an impossible object, the instructions were modified to include different examples of structural impossibility and subjects were required to point out specifically the impossible aspects in several impossible objects. Second, instead of being told to press one response key if a drawing could be a possible object and another if it could not be a possible object, subjects were instructed to press one response key if a drawing appeared to be a possible object and another key if the drawing appeared to be an impossible object. Third, whereas in the previous experiments we used a randomly determined response mapping (subjects pressed the left key for "could be possible" and right key for "could not be possible"), in the present study we counterbalanced response mappings. One half of the subjects in each

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experimental condition pressed the left key to indicate a possible response and the right key to indicate an impossible response, whereas the reverse response mapping was used for the other half of the subjects.

Administration of object decision instructions took approximately 2 min. The object decision test was then given, with studied and nonstudied objects appearing for 100 ms under the same conditions described earlier with respect to the baseline study. The test began with 10 practice drawings, 5 that had appeared on the study list and 5 that had not appeared on the study list. These were followed in an uninterrupted sequence by the 20 studied and 20 nonstudied target drawings. Each test trial was initiated by the appearance of a fixation point in the middle of the computer screen.

Subjects who were given the yes/no recognition test were told that they would be shown a series of drawings, some of which had just been presented during the study task and some of which had not been shown previously. These subjects were further instructed to press one response key if they remembered seeing the object during the left/right encoding task and another response key if they did not remember seeing the object; response mappings were counterbalanced so that the left and the right keys were used equally often for yes and no recognition responses. As in the object decision test, each test trial was initiated upon the appearance of the fixation point.

The same 10 practice items that were used on the object decision test (5 studied, 5 not studied) were presented initially, followed by presentation of 20 studied and 20 nonstudied target drawings in random order. About 2 min intervened between conclusion of the study task and appearance of the first practice item. Drawings remained on the computer screen for 5 s, until subjects made their recognition responses. The recognition test was generally completed in about 3 to 4 min. Immediately after conclusion of this test, subjects were given the same object decision instructions and test described earlier.

After the conclusion of testing, all subjects were debriefed concerning the nature of the experiments.

Results

Object decision. The object decision data are displayed in Table 1. Consider first the findings in the one-study-exposure condition. Overall, these results provide a close replication of the critical patterns of data reported by Schacter et al. (1990a). Object decision accuracy was higher for studied than for nonstudied possible objects, thereby indicating the presence of priming; in contrast, there was no evidence of priming for impossible objects. Performance was similar whether the object decision test was given first or second (after the recognition test), although the difference between studied and nonstudied possible objects was greater in the first than in the second test condition. Note that a virtually identical pattern of results was observed in the four-exposure condition: There was robust priming for possible objects and no difference between studied and nonstudied impossible objects, both when the object decision test was given first and when it was given second. Performance was higher for impossible objects in the second than in the first test condition, but as indicated later, this trend was not statistically significant.

A preliminary analysis of variance (ANOVA) was performed that included response mapping as a factor, and no main effects or interactions approached significance (all Fs < 1). Accordingly, all subsequent analyses were collapsed across this variable. The key outcome of the ANOVA was a significant interaction of Object Type (possible vs. impossible) × Item Type (studied vs. nonstudied), $F(1, 76) = 17.86$, $MS_e = .017$, $p < .001$, indicating that priming was observed for possible but not for impossible objects. The main effect of study exposures was not significant $F(1, 76) < 1$, and this variable did not enter into any significant interactions (all Fs < 1.90). Similarly, there was a nonsignificant main effect of test order $F(1, 76) = 1.18$, $MS_e = .098$, and test order did not interact with any other variable (all Fs < 2.81).[1]

Recognition memory. The recognition data (hits and false alarms), presented in Table 2, contrast sharply to the object decision data: Explicit memory was considerably higher after four than after one study exposure for both possible and impossible objects. An ANOVA was performed on the hit rates in the main experimental conditions, and also on a corrected recognition measure (hit rate minus false alarm rate). These two types of analyses led to identical conclusions, indicating that the false alarm rate was relatively constant across experimental conditions. We therefore report the results of the hit rate analysis only.

The ANOVA revealed a highly significant main effect of study exposures, $F(1, 38) = 16.87$, $MS_e = .057$, $p < .001$. There was also a main effect of object type, $F(1, 38) = 6.51$, $MS_e = .017$, $p < .02$, reflecting the fact that recognition memory was more accurate for possible than for impossible objects. The Object Type × Study Exposures interaction was not significant, $F(1, 38) = 2.34$, $MS_e = .017$.

The foregoing analyses suggest that number of study exposures affects recognition but not object decision performance. Two ANOVAs that included type of test as a variable were performed on studied items (i.e., proportion correct for object decision and hit rate for recognition). The first compared recognition and object decision performance, with type of test as a within-subjects variable, and revealed a significant Study Exposures × Type of Test interaction, $F(1, 38) = 7.27$, $MS_e = .050$, $p < .01$. The second ANOVA compared recognition and first test object decision performance, with type of test as a between-subjects variable. It also showed a highly significant Study Exposures × Type of Test interaction, $F(1, 76) = 13.65$, $MS_e = .053$, $p < .001$. These interactions confirm that the one- versus four-exposure manipulation influenced recognition but not object decision performance.

To examine further the relation between object decision and recognition performance, we performed contingency analyses to determine whether priming and recognition of possible objects exhibits stochastic independence, as was observed in our earlier article. Only the data from the one-exposure condition were considered because there were too few recognition errors in the four-exposure condition to per-

[1] This ANOVA and all others in this article were performed on data from individual subjects and not from individual items. However, because a restricted item set was used in the present experiments and because type of item (i.e., possible vs. impossible) was a factor, it is important to know whether the results hold across items as well as across subjects. Analysis of the data across items revealed the same patterns as were observed across subjects, but only the subject-based analyses are reported.

Table 1
Object Decision Performance: Experiment 1

	Number of study exposures/test order					
	One exposure			Four exposures		
Item type	First	Second	*M*	First	Second	*M*
Possible objects						
Studied	.76	.72	.74	.71	.71	.71
Nonstudied	.57	.65	.62	.56	.60	.58
M	.66	.69	—	.64	.66	—
Impossible Objects						
Studied	.68	.66	.67	.61	.72	.67
Nonstudied	.67	.68	.68	.59	.71	.65
M	.68	.67	—	.60	.72	—

Note. Each study exposure consisted of a 5-s left/right judgment.

mit a meaningful contingency analysis. We constructed 2 × 2 contingency tables in which each of the four cells represent the probability of the joint outcome of success or failure on successive recognition and object decision tests for studied possible objects. The contingency analysis indicated that the conditional probability of a correct object decision being given successful recognition (.73) was essentially identical to the overall probability of a correct object decision (.72), thereby indicating independence between the two tests. These data replicate our earlier findings of independence with a new set of materials and different test instructions. Issues concerning the analysis and interpretation of stochastic independence will not be discussed further in this article (see Schacter et al., 1990a, for more extensive analysis and discussion of stochastic independence between recognition and object decision, and Hayman & Tulving, 1989a, for more general discussion).

Discussion

Experiment 1 yielded three new results concerning implicit and explicit memory for novel visual objects. First, there was no priming on the object decision task for structurally impossible objects even following four study exposures. Second, significant and comparable amounts of object decision priming were observed for structurally possible objects after one and four study exposures. Third, recognition performance was significantly higher in the four- than in the one-study exposure condition for both possible and impossible objects.

Table 2
Recognition Performance: Experiment 1

	Number of study exposures		
Item type	One	Four	*M*
Possible objects			
Studied	.65	.91	.78
Nonstudied	.24	.21	.23
Impossible objects			
Studied	.62	.80	.71
Nonstudied	.29	.18	.24

Note. Studied = proportion of studied items called *old* (hit rate). Nonstudied = proportion of nonstudied items called *old* (false alarm rate). Each study exposure consisted of a 5-s left/right judgment.

These results confirm our previous findings on object decision priming and provide additional evidence that implicit and explicit memory for novel visual objects can be dissociated experimentally. In addition, we replicated our previous findings of stochastic independence between object decision and recognition performance.

The fact that we did not observe any evidence of priming for structurally impossible objects under the present experimental conditions extends our previous observations and helps to rule out several interpretations of these findings. Whereas in the earlier experiments there was relatively low intersubject agreement under unlimited viewing conditions that impossible objects are indeed impossible (.87), there was near-complete intersubject agreement (.99) that the present set of impossible objects are impossible. Accordingly, lack of priming cannot be attributed to low intersubject agreement about the impossible nature of these figures. Our results also provide evidence against the idea that no priming of impossible objects is observed because impossible responses are treated as negative responses. Although, as discussed earlier, the task instructions in our previous experiments did effectively turn impossible responses into negative responses, the instructions in the present study were altered so that this was no longer the case. In addition, mappings between response keys and possible or impossible responses were counterbalanced in this experiment instead of being randomly assigned, as they were in the earlier studies. Despite these procedural modifications, we replicated our previous findings of no priming for impossible objects.

Our data also provide evidence against the idea that the memory representation of impossible objects is simply too weak to support priming. As in previous experiments, recognition memory for possible objects was higher than for impossible objects. The critical data, however, emerge from a comparison of performance for possible objects after a single exposure and impossible objects after four study exposures. Even though recognition of impossible objects after four exposures was considerably higher than recognition of possible objects after a single exposure (Table 2), priming was observed in the latter but not in the former condition. Thus, even under conditions in which the explicit memory data suggest a strong episodic representation of impossible objects—one that supports higher levels of recognition perform-

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ance than does the episodic representation of possible objects—we still failed to observe priming for impossible objects. Nevertheless, we have not of course ruled out the possibility that significant priming of impossible figures on an object decision task could be demonstrated under some set of conditions. We will explore further issues concerning priming of impossible objects in Experiments 3 and 4.

An additional issue that merits brief commentary concerns the possible role of response bias in the priming effects that we observed. It is conceivable that exposure to objects during the left/right study task simply produces a generalized bias to make a possible response to all previously studied items on the object decision test—possible and impossible—thus producing priming of possible but not of impossible objects. We considered this issue at length in our previous study (Schacter et al., 1990a) and showed that response bias could not account for the priming that we observed following the left/right encoding task. To evaluate the role of response bias in the present data, we used the same measure that was used by Schacter et al. (1990a): Yule's *Q*, a special case of the gamma correlation for analyzing association in 2 × 2 contingency tables (See Goodman & Kruskal, 1954; Hayman & Tulving, 1989a, 1989b; Nelson, 1984, 1990). *Q* provides an estimate of the strength of relation between two variables that can vary from −1 (negative association) to +1 (positive association). We created 2 × 2 contingency tables for each subject in which the four cells were defined by the orthogonal combination of subjects' responses (possible/impossible) and object type (possible—impossible). We then computed *Q*s separately for studied and for nonstudied items according to procedures described by Nelson (1984, 1990) and Reynolds (1977). The larger the *Q* value within an experimental condition, the greater the strength of association between subjects' responses and object type—that is, a more positive *Q* value indicates more accurate object decision performance. The question for our purposes is whether the *Q* for studied objects is larger than the *Q* for nonstudied objects. If priming reflects an increase in the accuracy of object decision performance for studied objects relative to nonstudied objects—and not some sort of generalized bias to use the possible response more frequently for studied than for nonstudied objects—then the *Q* value for studied items should be higher than the *Q* value for nonstudied items. If, on the other hand, priming simply reflects a study-induced response bias to say possible to old items (both possible and impossible), then *Q* values should not differ for studied and nonstudied items. In the single-exposure condition, the *Q* value for studied items (+.56) was significantly higher than the *Q* value for nonstudied items +.41; $t(38) = 2.18$, $p < .01$; the same pattern of results was observed in the four-exposure condition, studied $Q = +.55$, nonstudied $Q = +.31$; $t(38) = 3.73$, $p < .01$. These results show that object decision performance was more accurate for studied than for nonstudied items. (The fact that positive *Q* values were obtained even for nonstudied items simply indicates that baseline performance on the object decision task exceeds chance levels of accuracy.) Accordingly, these data indicate that the priming that we observed cannot be attributed to a generalized bias to use the possible response more frequently for studied objects than for nonstudied objects.

The foregoing analyses are thus consistent with the proposal that priming of possible objects is mediated by newly acquired structural descriptions of target drawings. Viewed from this perspective, the failure to find an effect of number of study exposures on priming—despite large effects on explicit memory—suggests that a single 5-s left/right judgment about a possible object is sufficient to establish a structural description that preserves the sort of global, three-dimensional information that supports object decision priming. We have thus far used a 5-s-exposure duration because we think that the analyses entailed in computing a global structural description—determining depth relations among surfaces and edges, assessing the orientation of the object in space, and so on—require some time to be completed. Thus, our encoding instructions have emphasized that subjects should use the full 5 s to inspect each object carefully before making a left/right judgment, and we have assumed that it is important for subjects to make use of this time in order to observe priming. It is conceivable, however, that object decision priming does not require such extensive structural analysis and that even a snap left/right judgment is sufficient to support priming.

To examine this issue and to provide more information about the kinds of encoding activities that are needed to support priming of novel visual objects, we examined object decision performance following two different study conditions in Experiment 2. One group of subjects made left/right judgments on the basis of a single 1-s exposure to each object. If priming is observed in this condition, it would indicate that the structural analyses required to support object decision priming require considerably less time and are perhaps less extensive than we had initially supposed. A second group of subjects was given five successive 1-s exposures to target objects—as much total exposure time as subjects in previous experiments who were given a single 5-s exposure.

The implications of the priming data in this latter condition depend to some extent on the results in the single 1-s exposure condition. If significant priming is observed following a 1-s exposure, then we will be in a position to assess the generality of the finding from Experiment 1 that repetition beyond a single exposure fails to increase the magnitude of priming. On the other hand, if no priming is observed following a single 1-s exposure, then a failure to find priming following five 1-s exposures would suggest that object decision priming is largely or entirely immune to the effects of repetition. If, however, significant priming is observed following five 1-s exposures—even though no priming is found after a single 1-s exposure—there would be evidence that object decision priming could benefit from repetition and that structural representations could be formed on the basis of temporally distributed encoding operations.

Experiment 2

Method

Subjects. A total of 80 University of Arizona undergraduates participated in the experiment in exchange of class credits; 20 subjects were assigned randomly to each of the four between-subjects conditions in the experiment.

Design, materials, and procedure. The same set of 20 possible and 20 impossible objects that was used in Experiment 1 constituted the target materials. The design of the experiment consisted of two between-subjects variables, study exposures (one vs. five) and type of test (object decision vs. recognition), and two within-subjects variables, object type (possible vs. impossible) and item type (studied vs. nonstudied). The object decision test was either given first or second (after the recognition test), thus creating a test order variable for the object decision analysis.

The study and test instructions as well as the procedures used in Experiment 2 were identical to those described in Experiment 1, with two exceptions. In the single-exposure condition, objects appeared on the computer monitor for 1 s, and subjects then made their left/right decision. In the five-exposure condition, the same 1-s presentation rate was used, except that subjects were given five successive exposures to the study list; objects were presented in a random order on each pass through the list, and subjects made a left/right judgment on each exposure to an object.

Results

Object decision. The object decision data are presented in Table 3. First, consider the results for structurally possible objects. In the single-exposure condition, object decision accuracy was virtually identical for studied and nonstudied items in both the first and second test conditions. By contrast, in the five-exposure condition, object decision accuracy was greater for studied than for nonstudied objects on both tests. There was no evidence of priming for structurally impossible objects in any experimental condition.

An overall ANOVA that included study condition as a between-subjects variable revealed a significant main effect of object-type (possible vs. impossible), $F(1, 76) = 7.03$, $MS_e = .053$, $p < .01$, and a marginally significant Object Type × Item Type (studied vs. nonstudied) interaction, $F(1, 76) = 3.93$, $MS_e = .022$, $p = .053$, indicating that priming was observed for possible but not for impossible objects. There was also a significant Study Condition × Object Type interaction, $F(1, 76) = 4.69$, $MS_e = .053$, $p < .05$. This interaction indicates that object decision performance for possible objects was more accurate after five exposures than after one exposure—presumably because of priming effects in the former but not in the latter condition—whereas performance for impossible objects was comparable in the two conditions. However, the Study Condition × Object Type × Item Type interaction was not significant, $F(1, 76) = 1.92$, $MS_e = .023$. No other main effects or interactions were significant (all $Fs < 2.55$).

Separate ANOVAs were performed for the one-exposure and for the five-exposure conditions. In the one-exposure condition, there was a trend for priming of possible but not for impossible objects from the recognition test: Object decision accuracy was higher in the second than in the first test condition for both studied and for nonstudied possible objects. However, neither the main effect of test order nor any interactions involving test order were significant ($Fs < 2.18$). No other effects were significant (all $Fs < 1$).

In the five-exposure condition, there was a significant effect of object type, $F(1, 38) = 18.59$, $MS_e = .033$, $p < .001$. More important, there was a significant Object Type × Item Type interaction, $F(1, 38) = 6.61$, $MS_e = .019$, $p < .02$, indicating that priming was observed for possible but not for impossible objects. No other main effects or interactions were significant, all $Fs < 1.63$.

Recognition. Recognition accuracy was considerably greater in the five-exposure condition than in the single-exposure condition, and there was also a trend for greater recognition accuracy of possible than of impossible objects. An ANOVA performed on the hit rates revealed a highly significant main effect of study condition, $F(1, 38) = 14.90$, $MS_e = .045$, $p < .001$. The main effect of object type approached but did not attain significance, $F(1, 38) = 2.83$, $MS_e = .032$, $P = .10$, and the Study Condition × Object Type interaction was not significant, $F(1, 38) = 1$. An analysis of corrected recognition scores (hits minus false alarms) revealed a similar pattern of results, except that now the effect of object type was significant, $F(1, 38) = 6.84$, $MS_e = 0.40$, $p < .02$.

Discussion

Experiment 2 has shown that priming of structurally possible objects is observed after five 1-s left/right judgments, but not after a single 1-s left/right judgment. The failure to observe priming in the single 1-s exposure condition indicates that several seconds are required to perform the encoding operations necessary to build a structural description of a novel object that is sufficient to support priming on the object

Table 3
Object Decision Performance: Experiment 2

	Number of study exposures/test order					
	One exposure			Five exposures		
Item type	First	Second	*M*	First	Second	*M*
Possible objects						
Studied	.60	.72	.66	.75	.80	.78
Nonstudied	.59	.71	.65	.64	.73	.68
M	.60	.71	—	.70	.77	—
Impossible objects						
Studied	.62	.65	.64	.57	.61	.59
Nonstudied	.62	.67	.64	.62	.61	.62
M	.62	.66	—	.60	.61	—

Note. Each study exposure consisted of a 1-s left/right judgment.

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decision task. (It is of course conceivable that a 1-s exposure would be sufficient to support priming of novel objects on an implicit test other than object decision.) This result is also consistent with the finding in the present experiments and those of Schacter et al. (1990a) that the appearance of studied or nonstudied objects on a recognition test does not produce robust priming. Test priming effects have generally been either weak or absent in most experimental conditions, although there was a trend for test priming in the single-exposure condition of Experiment 2, and a similar trend was observed when performance was assessed at long delays (see Schacter, Cooper, & Delaney, 1990b). However, the fact that significant test priming has not been observed makes good sense in view of the fact that subjects' recognition latencies are generally on the order of 1 to 1.5-s in our experiments. The lack of priming in the 1-s-exposure condition and the failure to observe consistent test priming effects suggest that priming on the object decision tests depends on careful and extensive structural analysis of an object at the time of study. If appropriate structural analyses are not performed, either because the task does not require them or because insufficient time is given to perform the necessary computations, object decision priming apparently will not be observed.

The foregoing considerations suggest that a 5-s left/right judgment allows subjects to encode the various kinds of structural information about an object that are needed to facilitate subsequent object decision performance. The fact that significant priming was observed following five 1-s exposures suggests that some of the necessary structural information can be acquired from successive and temporally separate brief exposures to an object. These considerations, when coupled with the finding from Experiment 1 that four 5-s exposures do not produce more priming than a single 5-s exposure, suggest that when an adequate or complete structural description has been formed on the basis of a 5-s left/right judgment, further repetitions are redundant and do not add to priming. However, when an incomplete structural description has been formed on the basis of a 1-s exposure, further repetitions are beneficial, perhaps because they allow the necessary structural information to be acquired.

It is important to note, however, that the overall magnitude of the priming effect after five 1-s exposures is somewhat smaller than the priming effects observed after one or four 5-s exposures condition in Experiment 1. Indeed, when we performed the Q analysis described in Experiment 1, we found that the Q value for studied objects (+.52) was higher than for nonstudied objects (+.46), but we also found that the difference did not achieve statistical significance, $t(38) < 1$. This analysis suggests that the component of priming attributable to a newly acquired structural description—as opposed to response bias—may be less robust after five 1-s exposures than after a single 5-s exposure and, hence, that a single 5-s exposure may produce a more useful or complete structural description than five separate 1-s exposures. However, it is not entirely clear whether a nonsignificant difference between Qs for studied and for nonstudied objects signals that priming should be attributed to response bias (see discussion of Experiment 3), so it is probably reasonable to conclude that priming in the five 1-s exposures condition is based at least in part on a stored structural description. Note that explicit memory was considerably higher following five 1-s exposures than following a single 5-s exposure (as is indicated by comparing data in Table 4 and Table 2), thus indicating again that implicit and explicit memory for novel objects can be dissociated experimentally.

Experiment 3

Experiments 1 and 2 suggest that a 5-s left/right judgment may be sufficient to encode a reltively complete structural description of an object, at least with respect to the demands of the object decision test. However, the repetition manipulations used in these experiments involved performing the same encoding operations (i.e., left/right judgment) on each exposure to target objects. This kind of repetition may have provided redundant structural information about the objects and, hence, did not increase the size of the priming effect (although the same repetition manipulation did improve explicit memory). Thus, it is possible that priming could be enhanced if, in addition to the left/right task, subjects performed a different encoding task that yielded nonredundant structural information about studied objects.

To examine this issue, we compared priming in the left/right condition with priming in a condition in which subjects performed both the left/right task and a three-dimensional classification task. In the three-dimensional classification task, subjects are asked to classify each target object in terms of which of three categories of real-world, three-dimensional objects the target would best fit: type of furniture, household object, or type of building. In previous research (Schacter et al., 1990a) we found that the three-dimensional classification task produced significant priming effects on the object decision test. Because this task supports priming, we assume that it provides a basis for establishing a three-dimensional structural description of an object. However, the encoding operations required by this task differ at some level from the encoding operations required by the left/right task. Performing both the left/right and three-dimensional classification tasks (we will refer to this task as the *left/right+* condition), then, should add nonredundant information to the encoded representation of target objects. The question is whether this information is useful for the object decision test and thus increases the magnitude of priming.

Table 4
Recognition Performance: Experiment 2

Item type	Number of study exposures: One	Five	M
Possible objects			
Studied	.60	.77	.68
Nonstudied	.28	.10	.19
Impossible objects			
Studied	.52	.72	.62
Nonstudied	.31	.19	.25

Note. Studied = proportion of studied items called *old* (hit rate). Nonstudied = proportion of nonstudied items called *old* (false alarm rate). Each study exposure consisted of a 1-s left/right judgment.

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In Experiment 3 we also explore further whether priming of impossible objects can be observed. The left/right+ condition should provide useful information in this regard because it will allow us to determine whether performing two different types of structural encoding tasks supports priming of impossible objects. Moreover, we have already observed a trend for priming of impossible objects following the three-dimensional classification task alone (Schacter et al., 1990a, Experiment 3, object decision first condition).

We also modified our materials and paradigm in an attempt to produce equivalent levels of baseline performance for possible and impossible objects. A potentially problematic feature of Experiment 1 is that baseline levels of performance for nonstudied possible objects were consistently lower than for nonstudied impossible objects (see Table 1). Stated slightly differently, there was a bias to use the impossible response more frequently than the possible response for nonstudied items. (This trend was also evident in the first test object decision data from Experiment 2, but it was not apparent in the second test data.) By contrast, in our previous experiments using the left/right task (Schacter et al., 1990a, Experiments 1 and 2), performance on nonstudied items was nearly identical for possible and for impossible objects.

Because comparison of the relative amounts of priming for possible and impossible objects can be made most readily when equivalent baselines are obtained, it would be desirable to replicate the results of Experiment 1 under conditions in which baseline performance for possible objects is higher than was observed in Experiment 1 and, hence, more nearly equivalent to the baseline level for the impossible objects. To achieve this objective, we made two small changes in our experimental paradigm. First, we used the set of possible objects from the Schacter et al. (1990a) study, which generally yielded higher levels of baseline performance than did the possible object set used in Experiments 1 and 2. Second, we explicitly informed subjects that one half of the figures on the object decision test were possible and the other half were impossible; in previous experiments we had simply indicated that some objects would be possible and that some would be impossible. We reasoned that providing this information would reduce the likelihood of any generalized bias to use the impossible response more often than the possible response.

Method

Subjects. A total of 80 University of Arizona undergraduates participated in the experiment in return for course credits.

Materials, design, and procedure. The critical items consisted of the 20 impossible objects from Experiments 1 and 2 and the 20 possible objects used in Schacter et al. (1990a). As noted earlier, this set of possible objects yielded generally higher levels of baseline performance than did the possible objects used in Experiments 1 and 2, and these baseline levels were nearly equivalent to those obtained with the impossible objects.

The main design consisted of a 2 × 2 × 2 × 2 mixed factorial, with two between-subjects variables (left/right vs. left/right+ encoding and object decision vs. recognition test) and two within-subjects variables (studied vs. nonstudied items and possible vs. impossible objects). In addition, the object decision test was either given alone or after the recognition test, thereby creating a test order variable for the object decision analysis. For the left/right+ encoding task, one half of the subjects performed left/right judgments on all target objects and then performed the three-dimensional classification task on the same objects; the other half of the subjects performed the elaborative classification task first and the left/right task second. Possible and impossible objects were each randomly divided into two subsets of 10 items, and the subsets were completely counterbalanced across experimental conditions.

In the left/right condition, task instructions and item presentations were the same as those described in Experiments 1 and 2. In the left/right+ condition, one half of the subjects were first given left/right encoding instructions and then were given 5 s to make left/right judgments about all target objects. After making left/right judgments for all studied objects, these subjects were then told that they would be shown the same objects again, but would be asked to make a different judgment. They were instructed to classify each object into one of three categories, depending on what the object most reminded them of: a type of furniture, a household object, or a type or part of a building. They were further asked to generate a specific exemplar from the category that was chosen (e.g., a desk, a bottle, or a wall); 5 s were allowed for each classification. The other half of the subjects in the left/right+ condition performed the three-dimensional classification task first and the left/right task second.

One half of the subjects in the left/right and left/right+ conditions were then given either the object decision test or the recognition test; subjects in the latter condition were given the object decision test after the recognition test. All aspects of testing were the same as described in previous experiments.

Results

Object decision. Consider first the results from the left/right condition (see Table 5). These data replicate the major trend observed in previous experiments—robust priming for possible but not for impossible objects—under conditions in which the overall baseline levels of performance for the two types of objects are virtually identical (.63 for possible objects and .62 for impossible objects). In the left/right+ encoding condition, the magnitude of priming for possible objects was about the same as in the left/right condition, and there was no evidence for priming of impossible objects; in fact, there was a trend for less accurate classification of studied than of nonstudied impossible objects in both the first and second test conditions (see Discussion section). There was no clear evidence of test-induced priming in either encoding condition.

An ANOVA revealed main effects of both item type (studied vs. nonstudied), $F(1, 76) = 4.67$, $MS_e = .023$, $p < .05$, and object type (possible vs. impossible), $F(1, 76) = 17.55$, $MS_e = .027$, $p < .001$. There was also a significant interaction between these two variables, $F(1, 76) = 25.26$, $MS_e = .018$, $p < .001$, thus confirming that priming was observed for possible but not for impossible objects. There was also an unanticipated Test Order × Object Type × Item Type interaction, $F(1, 76) = 5.12$, $MS_e = .018$, $p < .05$, indicating that the magnitude of priming for possible objects relative to impossible objects was greater when the object decision test was given first than when it was given second. We have not observed such an interaction previously in similar experiments, and we will not discuss it further.

The main effect of encoding condition was nonsignificant, $F(1, 76) < 1$, and this variable did not enter into any signifi-

Table 5
Object Decision Performance: Experiment 3

	Type of encoding task/test order					
	Left/right			Left/right+		
Item type	First	Second	*M*	First	Second	*M*
Possible objects						
Studied	.80	.69	.75	.80	.78	.79
Nonstudied	.66	.60	.63	.64	.71	.67
M	.73	.65	—	.72	.75	—
Impossible objects						
Studied	.67	.62	.65	.65	.60	.63
Nonstudied	.67	.57	.62	.74	.68	.71
M	.67	.60	—	.70	.64	—

Note. Subjects in the left/right condition were given 5 s to make a left/right judgment; subjects in the left/right+ condition were given 5 s to make a left/right judgment and 5 s to make a three-dimensional classification judgment.

cant interactions (all $Fs < 3.01$). No other main effects or interactions were significant.

Recognition. The recognition data are presented in Table 6. In contrast to the object decision results, type of encoding condition had a large effect on recognition performance: Explicit memory was much more accurate in the left/right+ condition than in the left/right condition. An ANOVA performed on the hit rates revealed significant main effects of encoding condition, $F(1, 38) = 41.04$, $MS_e = .029$, $p < .001$, and object type, $F(1, 38) = 4.70$, $MS_e = .018$, $p < .05$, with no interaction between these two variables, $F(1, 38) < 1$. When the same analysis was performed on hit rates minus false alarm rates, a highly significant effect of encoding condition was again observed, $F(1, 38) = 40.44$, $MS_e = .052$, $p < .001$, and the Encoding Condition × Object Type interaction was nonsignificant, $F(1, 38) < 1$. However, the main effect of object type failed to reach significance in this analysis, $F(1, 38) = 2.38$, $MS_e = .036$, thus indicating that the difference between recognition of possible and impossible objects was not robust in the present experiment.

Two further analyses were performed that included type of test (object decision vs. recognition) as a between-subjects variable (the results of these analyses were the same when hit rate and hit rate minus false alarm rate were used for the recognition data, so only the hit rate analyses are reported). For the first analysis, in which type of test was a between-subjects variable, the critical outcome was a significant Encoding Condition × Type of Test interaction, $F(1, 76) = 12.82$, $MS_e = .053$, $p < .001$. For the second analysis, in which Type of Test was a within-subjects variable, a similar Encoding Condition × Type of Test interaction was observed, $F(1, 76) = 11.14$, $MS_e = 0.42$, $p < .01$. These analyses confirm that recognition but not object decision performance was influenced by the encoding task manipulation.

Table 6
Recognition Performance: Experiment 3

	Type of encoding task		
Item type	Left/right	Left/right+	*M*
Possible objects			
Studied	.70	.94	.82
Nonstudied	.23	.17	.20
Impossible objects			
Studied	.63	.88	.76
Nonstudied	.25	.15	.20

Note. Studied = proportion of studied items called *old* (hit rate). Nonstudied = proportion of nonstudied items called *old* (false alarm rate). Subjects in the left/right condition were given 5 s to make a left/right judgment; subjects in the left/right+ condition were given 5 s to make a left/right judgment and 5 s to make a three-dimensional classification judgment.

Discussion

The left/right+ condition greatly enhanced explicit memory for possible and impossible objects relative to the left/right encoding task alone. Nevertheless, we still failed to observe any evidence for priming of impossible objects in the left/right+ condition, and priming of possible objects was no greater in the left/right+ condition than in the left/right condition. In addition, we observed priming of possible but not impossible objects under conditions in which baseline levels of object decision accuracy were essentially identical for the two types of objects.

With respect to the possible objects, our data are consistent with the idea that the encoding operations performed during a 5-s left/right judgment allow subjects to form a relatively complete structural description of a novel object with respect to the demands of the object decision test. The results of an earlier experiment (Schacter et al., 1990a, Experiment 3) showing significant priming following the three-dimensional classification task indicate that similar conclusions also apply to this task. With respect to the impossible objects, the absence of priming in the left/right+ condition provides further evidence that the general failure to observe priming for these objects is not simply a function of some sort of generally weak memory representation because explicit memory for impossible objects was quite robust in the left/right+ condition. These results also indicate that performing two nominally different structural encoding tasks apparently does not produce a global structural description of an impossible object that can support object decision priming.

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The foregoing points should be considered in light of one possibly problematic feature of our data: the trend toward less accurate object decision performance on studied than on nonstudied impossible objects in the left/right+ condition (see Table 5). This trend suggests that the left/right+ condition may have produced a strong response bias to call all previously studied objects *possible*. The observed priming for possible objects and lack of priming for impossible objects may thus be partly or entirely attributable to this response bias. To assess the issue, we computed Yule's Q for studied and nonstudied objects in both the left/right+ and left/right conditions. Not surprisingly, in the left/right+ condition, Q for studied objects (+.57) and nonstudied objects (+.55) did not differ significantly, $t(39) < 1$. By contrast, in the left/right condition, Q for studied objects (+.64) was significantly higher than Q for nonstudied objects (+.51), $t(39) = 1.67$, $p < .05$.

Although this analysis suggests that priming of possible objects in the left/right+ condition is largely attributable to a bias to use the possible response more frequently for all studied objects (possible and impossible) than for nonstudied objects, we think that there are both logical and empirical grounds on which to question this conclusion. The logical argument follows from the previously established experimental fact that the left/right and three-dimensional classification tasks, when performed separately, each produce priming of possible objects that is not attributable to response bias: Q for studied objects is significantly higher than Q for nonstudied objects in both tasks (Schacter et al., 1990a, and Experiments 1 and 3 of the present article). Because each of these tasks produces significant structurally based priming when performed alone, it makes little sense to conclude that they do not produce significant structurally based priming when performed successively.

An empirical argument against the notion that priming in the left/right+ task is largely or entirely attributable to a study-induced bias to use the possible response emerges from consideration of assumptions underlying the analysis of response bias in our experimental paradigm. Specifically, a response bias to say *possible* to studied objects is assumed to operate when subjects provide more possible responses to studied than to nonstudied possible objects and provide more possible responses to studied than to nonstudied impossible objects. It is this latter tendency that takes the form of what we will call *negative priming* of impossible objects—lower object decision accuracy for studied than for nonstudied impossible objects. The key question is whether it is necessary to assume that such a trend in the impossible object data indicates the presence of bias in the possible object data. If this assumption were correct, then a simple empirical consequence would follow: The magnitude of priming effects for possible objects should be correlated significantly with the magnitude of negative priming for impossible objects. That is, larger amounts of positive priming for possible objects should be accompanied by larger amounts of negative priming for impossible objects.

To evaluate this issue, we analyzed data from 18 separate between-subjects experimental conditions (drawn from Schacter et al. (1990a) and Experiments 1–3 of the present paper) in which priming of possible objects has been observed. We computed the difference between studied and nonstudied possible objects (which was always positive) and the difference between studied and nonstudied impossible objects (which was sometimes positive and sometimes negative). According to the response bias argument, these two difference scores should be significantly negatively correlated: As the difference score for possible objects becomes increasingly positive, the difference score for impossible objects should become increasingly negative. However, analysis of difference scores from the 18 experimental conditions revealed essentially no correlation ($r = -.06$) between the two sets of scores. These data indicate that the presence of negative priming with impossible objects is unrelated to the magnitude of positive priming for possible objects.

The general implication of this result is that response bias should not necessarily be invoked as an explanation of priming for possible objects whenever negative priming of impossible objects is observed. Although the occasional trend for negative priming may signal the presence of some response bias, it might also reflect the nature of encoding processes elicited at the time of study. For example, if an encoding task induces subjects to attend to only certain parts of an object, which themselves may form a structurally possible subset of a globally impossible object, the resultant structural description might increase the likelihood of making a possible response on the object decision task (see Peterson & Gibson, in press, for evidence that allocating spatial attention within an object can influence the form of the structural description).

It is also tempting to speculate that the phenomenon of negative priming of impossible objects is for the most part an expression of random variability around a mean priming score of zero. Consistent with this idea, we computed the overall levels of object decision accuracy for impossible objects from the 18 experimental conditions included in the foregoing analysis. Performance was virtually identical for studied (.64) and for nonstudied (.65) objects, thus indicating zero priming of impossible objects across conditions (the corresponding proportions for possible objects in the same 18 experimental conditions were .77 for studied objects and .64 for nonstudied objects).

With respect to Experiment 3, the foregoing considerations support the argument that priming of possible objects in the left/right+ condition is at least partly attributable to encoding of a structural description—and not solely to response bias—despite the trend for negative priming of impossible objects. This conclusion makes sense in view of the fact noted earlier that the left/right and three-dimensional encoding tasks, when given alone, produce structurally based priming, thus making it difficult to understand why performing both tasks would simply produce a response bias to say possible.

However one views the response bias issue, Experiment 3 provides no support for the idea that performing two different encoding tasks yields structural representations of impossible objects that support significant priming and provides further evidence that priming of impossible objects is not observed with the modified materials and task instructions developed in Experiment 1. These observations lead us to question whether any other features of our task or materials could be responsible for the failure to observe priming of impossible objects. One conceivably relevant feature concerns the size of our target drawings. For the impossible objects used in Experiments 1–3, the mean visual angle subtended was 8.2° (when viewed from 60 cm), with a range of 6.9°–10.6°. For the possible objects used in Experiment 1, the mean visual angle was 7.7°, with a range of 6.5°–8.9°; for the possible objects used in Experiments 2 and 3 the corresponding mean was 6.0° with a range of 4.7°–6.7°. Thus, impossible objects were on average larger than possible objects in all experiments.

Although there is no strong reason to suppose that size differences among objects are responsible for the pattern of priming data, we confront the issue directly in Experiment 4 by examining performance with target drawings of equal size. To accomplish this objective, we equated possible and impossible objects with respect to a reference frame of standard size and then examined object decision and recognition performance following the left–right encoding task.

Experiment 4

Method

Subjects. A total of 40 University of Arizona undergraduates participated in the experiment in return for course credits.

Design, materials, and procedure. The main design consisted of a 2 × 2 × 2 mixed factorial, with one between-subjects variable, type of test (object decision vs. recognition), and two within-subjects variables, object type (possible vs. impossible) and item type (studied vs. nonstudied). In addition, for the object decision analysis, test order (first or second) was included as a between-subjects variable.

Target materials consisted of the same 20 possible and 20 impossible objects that were used in Experiment 3. To equate these objects for size, we constructed an 8.6-cm circular reference frame. All figures were scaled to fit within the reference frame. When viewed from 60 cm, all objects subtended a visual angle of 8.16°.

Subjects initially performed the left/right encoding task, followed by either the object decision or recognition test; immediately after completion of the recognition test, subjects in this group were given the object decision test. All aspects of instructions, counterbalancing, and procedure were exactly as described for the left/right group in Experiment 3.

Results and Discussion

Object decision. The pattern of object decision performance was quite similar to that observed in previous experiments using the left/right encoding task: There was strong evidence for priming of possible objects, little evidence for priming of impossible objects, and no systematic effect of test order (see Table 7). An ANOVA revealed significant main effects of object type, $F(1, 38) = 7.89$, $MS_e = .041$, $p < .01$, and item type, $F(1, 38) = 10.71$, $MS_e = .013$, $p < .01$. More important, there was a significant Object Type × Item Type interaction, $F(1, 38) = 14.92$, $MS_e = .009$, $p < .001$, thereby confirming that priming was observed for possible but not for impossible objects. There was also a marginally significant Test Order × Object Type × Item Type interaction, $F(1, 38) = 4.06$, $MS_e = .009$, $p = .051$, reflecting a trend for priming of impossible objects on the first test, together with a trend toward negative priming of impossible objects on the second test, whereas similar levels of priming for possible objects were observed on both tests. However, neither of the trends observed with the impossible objects approached significance (both ts < 1). Moreover, neither the main effect of test order nor any other interactions involving test order were significant (Fs < 1.51). Accordingly, the major result of Experiment 4 is that priming of possible but not of impossible objects was observed, thus indicating that previous failures to observe priming of impossible objects cannot be attributed to the variable size of target drawings because size was equated in the present experiment.

Table 7
Object Decision Performance: Experiment 4

Item type	Test order: First	Second	M
Possible objects			
Studied	.82	.79	.81
Nonstudied	.71	.67	.69
M	.77	.72	—
Impossible objects			
Studied	.71	.61	.66
Nonstudied	.65	.66	.66
M	.68	.64	—

Note. The encoding task consisted of a 5-s left/right judgment about size standardized objects.

In light of our earlier discussion of response bias and negative priming, it is perhaps worth noting that the data in Table 7 once again illustrate the independence of positive priming of possible objects and negative priming of impossible objects. In the object decision first condition, there was a +.11 priming effect for possible objects together with a +.06 effect for impossible objects; in the object decision second condition, there was a +.12 priming effect for possible objects together with a −.05 effect for impossible objects. Thus, the magnitude of priming for possible objects was virtually identical whether there was positive or negative priming of impossible objects. Nevertheless, we computed Yule's Q for studied and for nonstudied objects to determine whether significant differences were observed. The Q for studied items (+.65) was significantly higher than the Q for nonstudied items (+.51), $t(39) = 2.67$, $p < .01$.

Recognition memory. Recognition performance showed a relatively small difference between hit rates for possible objects (.70) and impossible objects (.66), together with a lower false alarm rate for the possible objects (.19) than for the impossible objects (.31). Analysis of the hit rate data alone failed to show a significant difference between the two types of objects, $t(39) < 1$, but combined analysis of hits minus false alarms did, $t(39) = 2.96$, $p < .01$.

General Discussion

Our experiments have provided new information about the properties and characteristics of implicit memory for novel visual objects, as indexed by priming effects on the object decision task, and have provided further evidence that implicit and explicit memory can be dissociated. Priming for structur-

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ally possible objects was equivalent after one or four 5-s left/right judgments (Experiment 1) and was also about the same in the left/right condition and the left/right+ condition, in which subjects performed the left/right task and a three-dimensional classification task (Experiment 3). By contrast, explicit memory was significantly higher after four repetitions than after one, and it was also higher in the left/right+ condition than in the left/right condition. Experiment 2 showed that a single 1-s left/right judgment did not produce priming on a subsequent object decision test, whereas five 1-s left/right judgments did. No evidence for priming of structurally impossible objects was observed in any experiment, despite (a) inclusion of only those objects that elicited nearly perfect intersubject agreement, (b) modification of task instructions from our previous experiments to avoid the identification of impossible responses with negative responses, (c) provision of four or five repetitions of the left/right encoding task (Experiments 1 and 2) or different structural encoding tasks (Experiment 3), and (d) use of size-standardized objects (Experiment 4).

The failure to document priming of impossible objects, despite numerous experimental variations, indicates that it is unlikely that this finding is attributable to some spurious or idiosyncratic feature of our instructions, materials, or procedures. Moreover, the absence of priming, even when explicit memory for impossible objects was quite high, indicates that attempts to explain our results in terms of a generally weak memory representation for impossible objects are unlikely to be useful. Of course, the fact that we have not found priming of impossible objects on the object decision task need not imply that such priming cannot be observed on this task under some set of experimental conditions. Our findings do indicate, however, that there is a wide range of conditions in which priming of possible objects is robust, whereas priming of impossible objects is absent.

In view of the foregoing considerations, we think that our data can be most readily interpreted in terms of our previously stated *structural description system* hypothesis (Schacter et al., 1990a): Priming on the object decision task depends on prior encoding of structural descriptions that preserve global, three-dimensional information about novel objects, and the structural description system that is involved in such priming either cannot compute, or has great difficulty computing, a global representation of an impossible object. That is, the system cannot settle in on a single global interpretation of an impossible object, precisely because there is no globally consistent interpretation of the structure of such an object. The structural description system can, however, compute a globally consistent interpretation of a possible object, and it is this representation that we assume provides a basis for priming. This hypothesis suggests that explicit memory for impossible objects—which was quite high in several experimental conditions—must be based on information other than a global structural description, such as representations of salient parts of the object.

These ideas led to a prediction concerning task conditions in which it should be possible to observe priming for impossible objects. Specifically, such priming should occur when an implicit test is used that requires access to information about individual parts of an object. If an implicit test does not require access to information about structural relations—in contrast to the demands of our object decision task—then there should be robust priming with impossible objects because such priming would be based on access to representations of possible parts rather than impossible wholes. An important task for future research is to develop appropriate implicit tests in which priming is supported by prior encoding of component parts of possible and impossible objects.

Turning to the results on priming of structurally possible objects, the pattern of data from Experiments 1–3 suggests, on the one hand, that the encoding activities entailed in making a 5-s left/right judgment (and a 5-s three-dimensional classification) produce a complete structural description of an object with respect to the demands of the object decision test: The magnitude of priming is not increased by additional repetitions of the left/right task nor by combining the left/right and three-dimensional classification tasks. On the other hand, the data indicate that a 1-s left/right judgment does not enable subjects to acquire the sort of structural information needed to support priming, thus suggesting that it takes time to carry out the sort of extensive analyses necessary to produce a global structural description.

The failure to observe priming in the 1-s encoding condition is consistent with, and helps to make sense of, our repeated failure to observe significant priming from the appearance of studied and nonstudied objects on the recognition test in the present experiments and previous ones (Schacter et al., 1990a) because subjects' recognition latencies are on the order of 1 s in our paradigm. We did observe trends toward test priming in individual conditions of particular experiments, but we also observed trends in the opposite direction (i.e., more priming when object decision was given first than when it was given second) in other experimental conditions, perhaps reflecting variability associated with a between-subjects comparison. Combined across studied and nonstudied items from the four experiments, however, overall performance for possible objects was .69 when the object decision test was given first, and .71 when the object decision test was given second; performance for impossible objects was .65 in both conditions. Clearly, there is no compelling evidence that processing an object on the recognition test produces priming. In view of the data from the 1-s encoding condition, this is probably because recognition judgments are made too quickly to permit the necessary structural analyses to be carried out. It is interesting to note in this regard that strong test priming effects have been observed on the fragment completion test (e.g., Tulving, Schacter, & Stark, 1982). Any number of differences in tasks and materials could account for the contrasting test priming data from object decision and fragment completion. However, one speculative possibility is that this pattern is produced by different characteristics of the structural description subsystem that we assume is involved in object decision priming and the word-form sub-system that appears to be involved in fragment completion priming (see Schacter, 1990).

One potential objection to our suggestion that a 5-s left/right judgment produces a complete structural description of a possible object concerns the potential role of ceiling effects

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in the data that are relevant to this claim. Perhaps there was no more priming after four left/right judgments than after one, or in the left/right+ task relative to the left/right task, because performance in all conditions was at or close to ceiling levels. Two points about this possibility are worth noting. First, any argument for a ceiling effect would have to invoke some sort of a functional ceiling because object decision accuracy was under 90% in all conditions. Second and more important, appeals to a functional ceiling effect are not satisfactory, either. Consider, for example, the data from Experiment 1 indicating that object decision performance for studied items did not differ following one left/right judgment (.74) and four left/right judgments (.71). An argument for ceiling effects would hold that under the present task conditions (i.e., 100-ms exposure), object decision accuracy cannot exceed approximately 75% accuracy; hence the failure to observe an effect of four exposures versus one exposure. This argument fails, however, because in other experimental conditions we have observed levels of object decision accuracy for studied items over 80% (e.g., Experiment 4 in the present paper, and Experiment 1 in Schacter et al., 1990a) and as high as 87% (see Schacter et al., 1990b). We therefore think that ceiling effects are not relevant to the pattern of data that we obtained and that the failure to increase priming with multiple exposures and tasks indicates that a 5-s left/right judgment yields all the structural information necessary to support object decision priming. Explicit memory, on the other hand, benefits from repetitions beyond a single 5-s left/right judgment, perhaps reflecting an important difference between the episodic system that we assume is involved in recognition and the structural description system that we assume is involved in object decision priming.

Consistent with these ideas, similar patterns of data have been reported with another task that taps priming of novel nonverbal information. In an experiment by Musen and Treisman (1990), subjects were given 3-s exposures to novel dot patterns and were given an additional 7 s to "rehearse" each pattern. Implicit memory was assessed with a test in which subjects attempted to copy dot patterns from a brief masked exposure. Musen and Treisman found that subjects correctly produced more studied than nonstudied patterns, thereby indicating the presence of priming. Most important with respect to the present concerns, repetitions of the studied dot patterns did not increase the magnitude of priming relative to the single-exposure condition, even though explicit memory performance was sensitive to additional repetitions. If we assume that priming in the Musen and Treisman paradigm depends on the structural description system—an assumption that is entirely consistent with the authors' data and their interpretation of it—then we have additional evidence that priming effects in this system are not increased by repeated exposures to an object or pattern.

As noted in the beginning of the article, we view the structural description system as a presemantic system—distinct from episodic memory—that is dedicated to representing information about the form and structure of objects and that does not handle semantic, functional, or associative information about them. This idea is based partly on neuropsychological studies of patients with object agnosia who demonstrate relatively intact access to structural knowledge about objects, despite severe impairments in gaining access to information about their functions or associative properties (Riddoch & Humphreys, 1987; Warrington, 1975, 1982; see Schacter, 1990, for more extensive discussion). The idea also receives support from our experiments showing that requiring subjects to relate target objects to their semantic knowledge of real-world objects either produces no priming (Experiment 2 in Schacter et al., 1990a) or no more priming that the left/right task (Experiment 3 in the present article and in Schacter et al., 1990a), even though these same manipulations greatly enhance explicit memory. This is precisely the pattern of results that would be expected if object decision priming were mediated by a presemantic system that can function independently of episodic memory.

Recent experiments have extended the finding that object priming does not require semantic study processing to another implicit task, in which we think that priming is mediated by the structural description system. Schacter and Merikle (1990) showed subjects line drawings of familiar objects and required them either to think of functions that each object performs (semantic study task) or to count the number of vertices in each object (structural study task). Priming was assessed with an object completion task in which subjects were briefly exposed to perceptual fragments of studied and nonstudied objects and were required to complete them with the first object that came to mind (for further discussion of the logic of this test, in contrast to traditional picture fragment completion tests, see Schacter, Delaney, & Merikle, 1990). Explicit memory was assessed by providing the same fragment cue and by instructing subjects to try to remember the correct object from the study list. Results indicated that explicit memory was higher after semantic encoding than after structural encoding, whereas priming was equivalent in these two conditions. Thus, priming in this paradigm did not require any semantic study processing.

This overall pattern of results, then, is consistent with the notion that a presemantic structural description system is involved in object priming on object decision, completion, and identification tasks, whereas episodic memory is responsible for explicit recall and recognition of objects. Converging evidence on this point is provided by the finding that amnesic patients show normal priming on the object decision task (Schacter, Cooper, Tharan, & Rubens, in press).Of course, it is no doubt possible to offer an account of these results that does not involve postulating distinct memory systems (e.g., Jacoby, 1983; Masson, 1989; Roediger et al., 1989). Nevertheless, our data are entirely consistent with a multiple systems account, and in addition there are a variety of heuristic and theoretical reasons for adopting such a stance (for discussion, see Hayman & Tulving, 1989b; Schacter, 1990; Schacter et al., 1990a; Squire, 1987; Tulving & Schacter, 1990).

With respect to future research, conceptualizing implicit memory for visual objects in terms of a presemantic structural description system sets the stage for studies that exploit priming effects as tools for investigating the nature of structural descriptions: Precisely what kinds of information are preserved in structural descriptions of objects? Does changing the size, the color, or the orientation of an object between study and test reduce or eliminate priming? How are structural descriptions used for purposes of object identification? We have already begun to investigate such issues with the object decision task, and others have reported similar investigations with related priming tasks (e.g., Biederman & Cooper, 1989). These investigations should provide insight into the mechanisms of implicit memory and are also likely to elucidate fundamental issues concerning the representation and identification of visual objects (cf. Biederman, 1987; Humphreys & Quinlan, 1987; Kosslyn, 1987; Marr, 1982).

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Received May 7, 1990
Revision received June 28, 1990
Accepted June 29, 1990 ■

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