

## Bias in the Priming of Object Decisions

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Seven experiments examined priming effects for 3-dimensional line drawings in the object decision task. One of the most important previous findings about object decisions has been that the decision about a possible object is primed by previous presentation of the object, but the decision about an impossible object is not. Through the use of manipulations that can eliminate processes that retrieve episodic information (response time deadlines, memory load, forced choice, and similarity), equal size effects on impossible and possible objects were obtained. This is interpreted to mean that priming effects reflect a bias to respond "possible," which can be opposed for impossible objects by episodic information so as to yield the approximately null priming effect for impossible objects found in past experiments.

Over the past few years, considerable research has been devoted to constructing a framework for the description of implicit memory systems. The object decision task using 3-dimensional line drawings of possible and impossible figures, developed by Schacter, Cooper, and Delaney (1990; see also Kroll & Potter, 1984), has provided a major piece of this framework. The task requires participants to decide whether a line drawing of a three-dimensional object, flashed for only a brief amount of time, depicts a possible object that could exist in the real world or an impossible object that could not exist (see Figure 1 for examples). Participants have viewed some of the objects before the object decision test, but object decision does not require direct use of explicit memory for an earlier presentation, so the task is labeled an indirect, or *implicit*, test of memory. Despite not requiring the use of explicit memory, object decisions for possible objects show facilitative effects of earlier presentations: The response to a possible test item is more likely to be correct if it was previously viewed than if it was not. However, only decisions about possible objects, not impossible ones, are affected by earlier presentation. This priming effect on possible but not impossible objects is taken as evidence for an implicit memory system, a system that is supposed to exist separately from the explicit memory that would be used for conscious recollection in such tasks as recall and recognition. Priming effects are thought to come from systems other than explicit memory for several reasons: The instructions in indirect tasks do not require the use of explicit

information; priming on indirect tests of memory is typically affected by different variables than performance on tasks that explicitly ask participants to make use of prior information; priming is stochastically independent from performance on direct explicit tasks; and finally, priming effects seem to be preserved in amnesic and brain-damaged participants who are impaired on tasks such as recall and recognition. Priming in object decision is a particularly important piece of the implicit memory framework because, unlike most other implicit priming effects, it shows memory for novel information in that the objects have never been seen by participants before the experiment.

Schacter, Cooper, and their colleagues (Cooper, Schacter, Ballasteros, & Moore, 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, Peterson, & Tharan, 1991; Schacter, Cooper, Tharan, & Rubens, 1991) interpreted object decision priming effects in normal participants as evidence for a particular implicit memory system, the *structural description system*. In their experiments, participants first studied line drawings of objects under instructions to make some judgment about each object, for example, whether it was left or right facing. Then at test, the participants were given either the object decision task or a recognition task (or sometimes both). When performance in the object decision task is facilitated by prior study, this is said to be due to the representation of the studied objects in the structural description system. Performance in recognition is also facilitated by prior study, but this facilitation is said to depend on information stored in explicit, episodic memory. Information in the two systems is assumed to be separate, first, because the object decision task does not require the use of information from explicit memory, and second, because performance on the two tasks dissociates. Variables that greatly affect performance on object decisions have little effect on recognition, and variables that affect recognition have little effect on object decisions (Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991). For example, if participants are given instructions at study to think up relations between the line drawings and common everyday objects, then later performance on recognition is improved much more than later performance on object decision.

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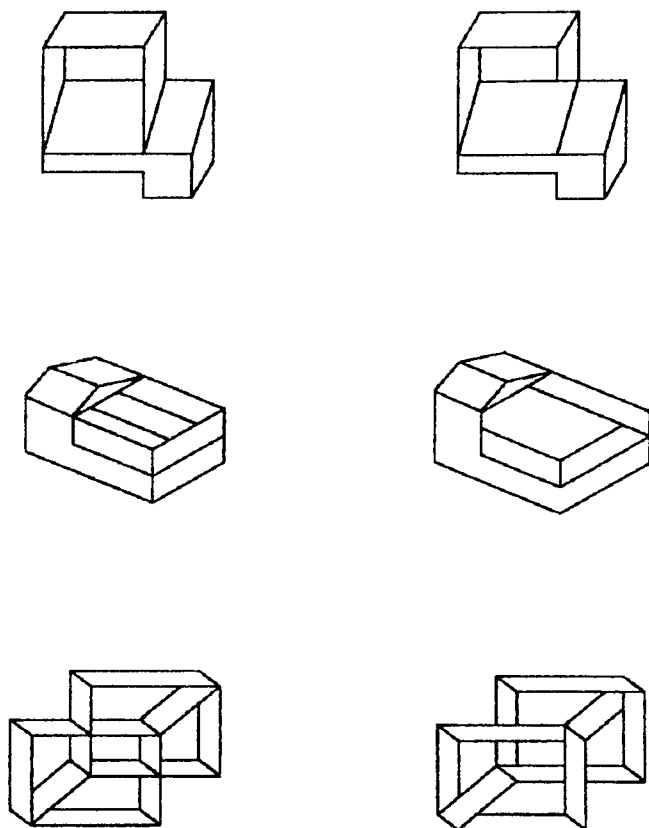


Figure 1. Examples of stimuli used in experiments.

Critical features of the structural description system as it is defined by Schacter, Cooper, and their colleagues (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991), features that separate it from other implicit memory systems, are that it encodes three-dimensional representations of objects and that it encodes representations that support priming only for objects that can exist in the real world. A representation is described as "a globally consistent interpretation of a possible object"; the structural description system "cannot compute, or has great difficulty in computing, a global representation of an impossible object" (Schacter, Cooper, Delaney, et al., 1991, p. 16). Postulating that the system represents only real, possible objects allows Schacter, Cooper, and colleagues (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991) to explain the failure to find priming in object decision for impossible objects. In their experiments, there was no significant effect of prior study on object decisions about impossible objects.

A shortcoming of the implicit memory enterprise, exemplified by the structural description system explanation of object decision priming, is the relative sparseness of theoretical specification of process and representation. For explicit episodic information, there are comprehensive models of recognition, recall, categorization, frequency judgments, and other

tasks (e.g., Anderson, 1983, 1990; Eich, 1982; Gillund & Shiffrin, 1984; Hintzman, 1986, 1988; Lewandowsky & Murdock, 1989; Murdock, 1982, 1983; Ratcliff, 1978), and various kinds of search processes have been proposed, ranging from direct access processes to serial and parallel searches and global memory matches. For semantic memory, there have been many efforts to develop models of representation, including feature models and network models (e.g., Collins & Quillian, 1969; Smith, Shoben, & Rips, 1974). However, for implicit memory systems, there seems to be only the priming effect: what kinds of encoding manipulations increase priming, what kinds of encoding manipulations reduce priming, what variables affect recognition or recall but not priming, what variables affect priming but not recognition or recall, and how amnesia affects priming. The findings are all mainly empirical, with little formal theory (although see Humphreys, Bain, and Pike, 1989, for an attempt to consider one implicit task in the context of explicit tasks and an explicit memory model). There are few theoretical suggestions for representation and process in implicit memory, and no proposals for a theoretical mechanism to explain why a prior representation should produce a priming effect. For example, there are no proposals for how a stimulus contacts information in the implicit system, how episodic information can override implicit information, how decisions are reached, or how responses are made. Certainly the database is rich enough to support theoretical efforts, and it is only the development of formal theories that will allow strong tests of the notions contained in the increasing number of qualitative theories about implicit memory.

The research described in this article has two goals. First, with a series of experimental studies, we demonstrate that an earlier presentation does not "improve the identification of perceptual objects" (Tulving & Schacter, 1990), and we propose an alternative explanation of the priming effects obtained in the studies. Our proposal is a specific hypothesis about how earlier presentation of an item affects its subsequent processing. Our second goal is to show that priming data and our hypothesis to explain them can potentially be incorporated into formal, explicitly testable, information-processing models.

Our explanation of object decision priming results begins with the claim that there is no need to postulate the existence of a separate implicit memory system such as the structural description system. The differences between performance on recognition and performance on the object decision task are held to be the reflections of different processes operating in a single memory processing system (see Ratcliff & McKoon, 1994). For the object decision task, we propose that priming results can reflect a combination of two processes: one is a tendency, or *bias*, to respond "possible" to an object that was previously studied, and the other is the retrieval of explicit episodic information about some feature or combination of features of a previously studied object that cue whether the object was possible or impossible. In the typical object decision experiment, these two processes combine to produce the typical pattern of results: If a test item is possible and it was previously studied, then both the bias to respond possible to more familiar objects and information from explicit memory

increase the probability of a *possible* response (relative to a possible test item that was not previously studied). However, for an impossible item that was previously studied, the two processes work against each other. The bias to respond possible to a familiar object increases the probability of a possible response, whereas the explicit memory associated with particular combinations of features cues an impossible response. Generally, in Schacter, Cooper, and colleagues' data (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney et al., 1991; Schacter, Cooper, Tharan, et al., 1991), it appears that the two processes cancel each other; the effect of previous study on responses to impossible test items was rarely significant. However, across 35 pairs of previous study versus no previous study conditions (Cooper et al., 1992; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991), there was a significant bias effect, a 2.2% increase in the probability of responding possible (incorrectly) for impossible objects ( $t = 2.72, p < .05$ ).

From a standard, information-processing, single memory system viewpoint, the bias process and the retrieval of explicit episodic information might be speculated to arise from different kinds of processes and representations (e.g., LaBerge & Samuels, 1974; McClelland & Rumelhart, 1981; Posner, 1978). Specifically (e.g., Winston, 1977, chap. 3), features of the objects (lines or angles) might be combined into partial representations (e.g., corners) that might, at higher levels, be combined into more complete representations (e.g., boxes for Winston or geons for Hummel & Biederman, 1992) that might, in turn, be combined into complex objects with associated information about possibility or impossibility. The complex objects might be further combined with explicitly retrievable semantic elaborations. Bias because of previous study of an object could be based on modifications to processes engaged in building these representations, and these biases might be sensitive to different variables than the explicit representations stored with episodic information. Bias manifested in earlier processing stages might be independent of the information encoded at later processing stages that is accessible to direct recollection. As an outline of how bias and explicit retrieval might be imagined to operate, this analysis suffers from the current lack of formal, detailed, psychological theories of perception and memory for complex visual objects (although see Hummel & Biederman, 1992). Nevertheless, it is analogous to the sorts of information-processing systems generally accepted in other areas of perception and information processing (e.g., LaBerge & Samuels, 1974; McClelland & Rumelhart, 1981; Posner, 1978).

Our explanation of priming effects in object decision includes retrieval of episodic information as a component of the object decision process. If episodic information is to be included in the decision process, then it follows directly that two of the reasons for separating an implicit memory system for three-dimensional objects from explicit memory must be rejected (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991). First, we believe that participants do make use of explicitly remembered information even when the task instructions do not require them to do

so (and we demonstrate this in the experiments reported below). Second, following previous arguments, we believe that neither functional independence nor stochastic independence provides sufficient grounds to establish independent memory systems. It is clear that functional independence and dissociations do not require a different memory system (see Hintzman, 1990), and it is clear that models can produce dissociations from different retrieval processes operating on a single memory system (Gillund & Shiffrin, 1984; Nosofsky, 1988). Stochastic independence likewise has not provided incontrovertible evidence for separate memory systems (e.g., Hintzman & Hartry, 1990; Ostergaard, 1992).

According to our proposal, there are two processes that work against each other to produce the approximately null effect of previous study on object decisions for impossible test items. For Schacter, Cooper, and colleagues (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991), this null effect is important support for the existence of a structural description system for real, possible objects. For the experiments described below, the goal was to show that our two proposed processes could be separated. In contrast to most previous object decision experiments, we manipulated retrieval conditions rather than encoding conditions. By manipulating retrieval conditions, we hoped to show that retrieval of explicit information for studied objects could be eliminated so that bias could be observed for all studied objects: Both possible and impossible test objects would show an increased probability of possible responses as the result of previous study.

In assuming an explicit retrieval process operating for impossible objects in opposition to a bias to respond possible to previously studied objects, we echo analyses put forward by Jacoby and colleagues (Jacoby, 1991; Jacoby, Woloshyn, & Kelley, 1989) for other tasks in which explicit information interferes with correct performance. For example, in one study (Jacoby et al., 1989), participants were asked to judge whether each name in a list was the name of a famous person. The judgments were preceded by study of some of the nonfamous names. Participants were told that previously studied names were all nonfamous, and they were able to use this information plus explicit retrieval of names from the studied list to reduce their likelihood of making an error of judging a nonfamous name famous. However, when explicit retrieval was made difficult by adding a concurrent task to be performed during the fame judgments, the familiarity produced by prior study of nonfamous names led to an increased probability of previously studied nonfamous names being judged famous. We designed our experiments with a similar aim, to manipulate retrieval conditions so as to eliminate or substantially reduce explicit retrieval and thus reveal a more complete picture than would otherwise be possible of the influences of prior study on performance.

### Experiment 1: Replication

We designed Experiment 1 to replicate the standard procedure and results of experiments by Schacter, Cooper, and colleagues to ensure, before we introduced new manipula-

tions, that our materials, participants, and equipment were comparable to those used in previous studies. A typical experiment (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991) began with the presentation of a series of drawings of three-dimensional objects (examples are shown in Figure 1); for each object, participants were asked to judge whether it was left or right facing. This study phase was followed by a test phase in which participants were required to judge whether briefly flashed drawings represent possible or impossible objects. Participants were not informed at study that there would be a later test phase, and they were not instructed at test to use information from the previous study phase for their object decisions. The variable of interest was whether a test item was previously studied: For possible test items, prior study increased the probability of a (correct) possible response by about 10% to 15%, but for impossible test items, prior study typically did not significantly affect the probability of an (incorrect) possible response (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991).

## Method

**Participants.** There were 24 participants who, like the participants in all of the succeeding experiments, participated in the experiment for credit in an introductory psychology class at Northwestern University.

**Materials.** A set of 42 drawings was created for use in Experiments 1–7. The drawings depicted three-dimensional objects like those shown in Figure 1. Each object was drawn in two versions: one intended to depict a possible object, and one intended to depict an impossible object. In some experiments, only one version of each object was used, and in others experiments, both versions were used. The two versions were very similar to each other, different only in a few lines that made one of them appear to be possible and the other impossible. The impossible objects were judged impossible by at least five out of a group of six judges. We also had 16 participants (from the same population as for the experiments) judge whether each version of each item was possible or impossible. For the objects we had intended to be seen as possible, participants agreed with a probability of .78; for the objects we had intended as impossible, they agreed with a probability of .76. These probabilities are lower than the corresponding probabilities Schacter et al. (1990) obtained for their objects. We believe that the difference comes from our requirement that each of our impossible objects have a very similar possible version, which probably makes the possible and impossible objects more like each other than might be the case for the Schacter et al. materials. To show that the impossible versions did differ appropriately from the possible versions, we had 10 participants choose which of the two versions of an item was the impossible one; they chose the intended version with a probability of .94. It should be noted that we use the term *impossible* loosely. Even some classic impossible objects can actually be constructed physically (Brouwer & Rubin, 1979), and this was true of our impossible objects (and those of Schacter et al., 1990, Figure 1).

The objects were drawn for presentation on the IBM PC clones used for stimulus display in the experiments. The monitor used was a monochrome Goldstar 1403PLUS with a fast decay phosphor (P4K), and it measured about 36 cm on the diagonal. The objects averaged about 10 cm × 10 cm, and they were drawn in white lines of about 1-mm thickness on a black background. When a single object was displayed, it was centered on the monitor. When two objects were

simultaneously displayed (Experiment 6), they were shown side by side, separated by about 3.5 cm.

**Design.** In Experiment 1, only one version of each object was used. There were four, within-subject conditions, which used 40 of the 42 objects: Either the possible or the impossible version of an object was flashed in the test phase, and that same version was either presented for study in the first phase of the experiment or it was not. The 40 objects were divided into groups of 10, and these groups were combined with 4 groups of 6 participants each in a Latin square design. Twelve different random orders for presentation of stimuli were used, and each order was used for 2 participants.

**Procedure.** In the first phase of the experiment, participants were shown objects, one at a time, and asked to judge whether the object “faced right or faced left” (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991). Further definition of the task was left up to the participants. Four practice objects preceded the 20 objects used for the experimental design. The 20 objects, half possible and half impossible, were shown in random order. Each object was displayed for 5 s, during which time participants pressed the ?/key on the PC keyboard if they thought the object faced right or the Z key if they thought the object faced left. There was a 200-ms blank interval between successive objects. Participants were not given any information about the possible versus impossible nature of the objects, nor were they told that this first phase would be followed by a test.

After the first phase, the possible versus impossible nature of the objects was explained to participants; they were told that the impossible objects represented objects that could not be constructed in the real world, and they were shown examples of both possible and impossible objects. They were instructed that they would be shown test objects like the examples and they would have to decide whether the test objects were possible or impossible. Then they were given 20 practice tests, followed by the 40 tests for the experimental design. Each test item was displayed in the following manner: To begin, a row of plus signs was shown in the middle of the PC display screen for 500 ms; then the instruction to “press the space bar” when ready was displayed until the participant pressed the space bar of the PC keyboard to indicate readiness for the test item; after a 500-ms pause, the test item was flashed on the screen for 200 ms and then erased from the screen; the screen remained blank (dark) until the participant responded, pressing the ?/key for a possible response and the Z key for an impossible response; and then the plus signs for the next test item were displayed. The 40 objects of the experimental design (half studied in the first phase and half not studied and half possible and half impossible) were presented in random order.

The flash time we used was 200 ms, longer than the 100-ms time typically used by Schacter, Cooper, and colleagues (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991). The two times are not directly comparable. We used a monochrome monitor with a rapid decay rate. If the decay rate of the phosphor on the monitor used by Schacter, Cooper, and colleagues was slower, then the two effective stimulus presentation times might have been more equivalent. For direct comparison, either actual measurements are needed or the monitors must be of the same type with identical phosphors. Note that in both cases, the test stimuli were not masked so persistence in the visual system would have to be added to the flash time to estimate the “effective” amount of time the item was available to the visual system.

## Results

For the data from all of Experiments 1–7, we calculated the mean probability of responding possible for each participant in each condition, and means of these means are presented in the

tables. For analyses of variance (ANOVAs),  $p < .05$  was used throughout.

We expected that the results of the experiment would replicate results obtained by Schacter, Cooper, and colleagues (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991), and they did. The probabilities with which participants responded possible to the test objects are shown in the first column of data in Table 1. For possible objects that were not presented in the study phase, the probability of responding possible was .58, which was greater than the probability of responding possible (incorrectly) to impossible objects that were not studied (.41). This baseline difference between possible and impossible objects is smaller than that typically found by Schacter, Cooper, and their colleagues (Cooper et al., 1992; Schacter et al., 1990; Schacter, Cooper, Delaney et al., 1991; Schacter, Cooper, Tharan, et al., 1991), but about the same size as that obtained by Schacter and Cooper (1993) when they reduced the exposure time of test items to increase the magnitude of priming effects. For possible objects, study in the first phase of the experiment increased the probability of responding possible from .58 without prior study to .67 with prior study. For impossible objects, the probability of responding possible increased slightly with study, from .41 to .42. Although Schacter, Cooper, and colleagues (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991) on average found a small increase in responding possible for impossible objects, the increase was variable across experiments and conditions and usually not significant. The small increase that we obtained was also not significant.

An ANOVA on the probabilities of responding possible showed the interaction representing the large increase in possible responses for possible but not for impossible objects was significant,  $F(1, 63) = 8.40$ . The increase for possible test objects was significant by planned test,  $F(1, 63) = 16.13$ . The main effect of possible versus impossible test item was of course significant,  $F(1, 63) = 72.25$ . The standard error of the means was 0.02.

Our interpretation of this pattern of data was described earlier in the introduction. First, we assume that there is no separate "structural description system" for possible objects. Instead, there is a bias to respond possible to an object that is familiar because of previous study. However, bias by itself would lead to the same effect of previous study for both possible and impossible objects. We hypothesized that the

reason impossible objects do not show significant bias is that participants sometimes explicitly remember some particular configuration of corners, angles, or twists from an object that is associated with information about whether the object is possible or impossible and so serves, for impossible objects, to counteract the bias to respond possible.

If it is assumed that the retrieval of explicit information takes some time, then there should be a greater effect of the explicit information on slow responses than on fast responses. To test this, we split the data in half at approximately the mean response time, 900 ms (the mean for correct responses calculated across all four conditions). The results are shown in Table 1. First, as might be expected, slow response items were those for which participants were less able to discriminate between possible and impossible objects: The difference in the probability of responding possible for possible versus impossible nonstudied items was only .08 for slow responses (.56 – .48), compared with .23 (.59 – .36) for fast responses. In addition to this baseline difference, the data also show the greater effect of explicit information for the slow responses. Slow responses showed a discrimination pattern (probability correct increased with study), whereas fast responses showed a bias pattern (the probability of responding possible increased with study for both possible and impossible studied objects). The suggestion from the data for the slow responses is that participants did not have much information from the stimulus to discriminate a possible from an impossible object, that bias had dissipated, and that participants took extra time to examine explicit information. These results suggest that bias can be separated from explicit retrieval on the response time dimension, but this might not always be the case; for example, it might be that some explicit retrieval could occur quickly. We designed the following experiments to eliminate explicit retrieval by manipulations of retrieval conditions.

## Experiments 2 and 3: Deadline Procedure

One time honored way to eliminate explicit retrieval from decision processes is to place participants under a time deadline (e.g., Pachella, 1974; Reed, 1973). The reasoning is that at least a major component of explicit retrieval is likely to be a relatively slow process, and participants know it to be such. If they must respond by a deadline set sufficiently fast, they should not be able to execute slow retrieval processes, and they might not even attempt such processes. The elimination of late emerging kinds of information from decision processes by the imposition of a response deadline has been documented for a number of kinds of information, including general knowledge (Ratcliff & McKoon, 1982), associative information (Gronlund & Ratcliff, 1989), syntactic-semantic relations (Ratcliff & McKoon, 1989), and suppression of semantic (preexperimental) information (Doshier, 1984). For the object decision task, we predicted that a response deadline would eliminate the retrieval of explicit information, changing the pattern of priming results to a bias pattern. Without explicit information to counteract the bias to respond possible to previously studied objects, impossible test objects should show priming just as possible objects do; that is, prior study should

Table 1  
*Replication—Probabilities of Possible Responses in Experiment 1*

Study form	Test form	Pr (possible)		
		Total sample	RT < 900 ms	RT > 900 ms
Possible	Possible	.67	.67	.65
Not presented	Possible	.58	.59	.56
Impossible	Impossible	.42	.42	.41
Not presented	Impossible	.41	.36	.48

Note. Pr = probability; RT = response time.

increase the probability of responding possible (incorrectly) to impossible objects.

The alternative account of priming in object decision is the implicit memory explanation. Prior study leads to priming of possible objects because they can be remembered in the structural description system. Impossible objects are not encoded in this system, and so they show no priming. This explanation predicts that a response deadline will not affect the pattern of priming results. The structural description system is a memory system used in perception of objects (cf. Tulving & Schacter, 1990); it is presemantic and its function is to "improve identification of perceptual objects" (Tulving & Schacter, 1990, p. 301). To achieve this function, information from the system must quickly be retrieved, much more quickly than would be affected by the deadlines imposed in our experiments (or else the system would not be useful in perception).

Experiments 2 and 3 were the same as Experiment 1, except that in the test phase participants were required to initiate each possible-impossible judgment by a deadline after the flashed test object. The deadline was 200 ms in Experiment 2 and 800 ms in Experiment 3. We used two deadlines for generality. We anticipated that the faster deadline would force responses faster than responses in Experiment 1, which were in the 900-ms range.

### Method

In both Experiments 2 and 3, an object was tested in either its possible or impossible version, and that same version either had or had not been presented in the study list. The same 40 objects were used in the same design as in Experiment 1. There were 24 participants in Experiment 2 and 32 participants in Experiment 3.

At the beginning of the experimental session, participants were given practice at responding to a deadline. For this practice, a list of 60 true-false test sentences was displayed, one sentence at a time on the PC screen. All of the sentences expressed information about which participants would certainly know the truth or falsity. Some examples are "all boys are girls, conflict is always a gallop, beef is meat, and all trains are buses." Each sentence was preceded by a row of plus signs as a warning signal (for 250 ms), and then the sentence was displayed. One second after the sentence, a row of asterisks was displayed underneath it; this was the signal to respond, and participants were instructed to indicate immediately whether the sentence was true or false, pressing the ?/key for true or the Z key for false. Participants were instructed to try to make their response exactly 200 ms after the asterisks appeared. Once the response key was pressed, the time that had elapsed from presentation of the asterisks to keypress was displayed for 600 ms. The 1-s deadline was used in Experiment 2; it proved to be quite fast for the sentence materials, and a somewhat longer deadline, 1,600 ms, was used in Experiment 3.

After the true-false sentence practice, the study phase for the three-dimensional objects was presented. This was the same left-right judgment task as in Experiment 1. The study phase was followed by the test phase, with the same explanation of possible-impossible objects as in Experiment 1. The test items for the experimental design were preceded by 20 practice items. All of the items were presented in the following manner: to begin, a row of plus signs was displayed for 500 ms, followed by the test object flashed on the screen for 200 ms; then the screen remained blank for 200 ms (in Experiment 2) or 800 ms (in Experiment 3). After the blank interval, a row of asterisks was displayed as the signal to respond. The asterisks remained on the

screen until a response key was pressed (?/ key for a possible decision, Z key for an impossible decision). Then the amount of time elapsed since the presentation of the asterisks was displayed for 900 ms, followed by the row of plus signs for the next test item. Participants were encouraged to respond immediately when the asterisks were displayed, aiming to make their responses 200 ms after the asterisks.

### Results

Table 2 shows the results for Experiments 2 and 3. To explain the typical pattern of object decision priming (shown in our replication, Experiment 1), we hypothesized that the bias to respond possible for a previously studied test item is offset for impossible objects by explicit memory for some of their salient features or combinations of features. Assuming that it takes some processing resources to make use of this explicit information during the object decision process, it should be possible to eliminate its retrieval by restricting time to respond, with the result that impossible objects should show the same bias as possible studied objects. The data confirm this prediction. For both deadlines, the probability of responding possible to an impossible test item was increased by previous study. This is the same result as was obtained for fast responses in Experiment 1. In fact, when considering both the baseline no-study conditions and the bias effects from those baselines, the data look markedly similar to the fast response data from Experiment 1.

With the 200-ms deadline, the bias to respond possible was even larger for impossible objects (.15) than for possible objects (.10), although not significantly so. With the 800-ms deadline, some explicit memory may have entered the decision process for impossible objects: The bias for the impossible objects was .07, compared with .10 for the possible objects (although, again, the difference in these two amounts of bias was not significant). We tested the bias effects with an ANOVA. For the faster deadline, Experiment 2, the bias to respond possible to a previously studied object was significant,  $F(1, 23) = 11.70$ . The bias held for both possible and impossible test objects, as evidenced by the lack of interaction,  $F < 1.00$ . Also, as expected, participants were more likely to respond possible to a possible test object than an impossible test object,  $F(1, 23) = 35.20$ . The standard error of the means was 0.03. For the 800-ms deadline, Experiment 3, the results were essentially the same. The main effect of study was significant,  $F(1, 31) = 11.20$ , as was the main effect of possible versus impossible test object,  $F(1, 31) = 60.30$ . The interaction

Table 2  
Deadlines—Probabilities of Possible Responses in Experiments 2 and 3

Study form	Test form	Pr (possible)	
		200-ms deadline	800-ms deadline
Possible	Possible	.64	.64
Not presented	Possible	.54	.56
Impossible	Impossible	.48	.40
Not presented	Impossible	.33	.33

Note. Pr = probability.

$F$  was again less than 1.0. The standard error of the means was 0.03.

#### Experiment 4: Regular and Deadline Tests, Mixed

The question in this experiment was whether explicit retrieval could be eliminated for regular, no-deadline, test items by mixing into the test list deadline test items. For each test item, participants did not know, at the time it was flashed, whether they would be given a signal to respond. If there was a signal, it appeared 200 ms after the flash. If there was no signal, participants responded in their own time. The hypothesis was that participants might be unlikely to attempt to engage in slower explicit retrieval processes on any trial, either regular or deadline, because the attempt would necessarily fail on the large number of deadline trials. In other words, the inability to use episodic retrieval on many trials would carry over to a failure to use episodic retrieval on all trials (see Poulton, 1975, for a discussion of this type of context or range effect). This result would be consistent with the result obtained in Experiment 3, in which the use of a deadline appeared to cause participants not to use episodic information even though the 800-ms deadline might have been long enough to allow it to be available.

#### Method

The experiment was almost identical to Experiment 2. There was one additional variable, whether a 200-ms deadline was imposed on responses, crossed with the study and test variables of Experiment 2; all variables were within-subject variables. For a deadline trial, the procedure was the same as in Experiment 2; for a no-deadline trial, the procedure was the same except that the response signal was not displayed. There were 25 participants.

#### Results

Table 3 shows the probability that participants responded possible in each condition. The data show similar patterns to the data of Experiments 2 and 3 and the fast responses from Experiment 1. For both deadline and no-deadline trials, there was a bias to respond possible for previously studied objects for both possible and impossible objects. The mean response probabilities suggest a larger bias effect for the deadline trials than for the no-deadline trials, but this interaction was not significant. The effect of previous study was significant,  $F(1, 24) = 5.49$ , and this effect did not interact with the other

Table 3  
*Deadlines and Regular Trials—Probabilities of Possible Responses in Experiment 4*

Study form	Test form	Pr (possible)	
		No deadline	200-ms deadline
Possible	Possible	.60	.56
Not presented	Possible	.56	.43
Impossible	Impossible	.43	.47
Not presented	Impossible	.38	.32

Note. Pr = probability.

Table 4

*Memory Load—Probabilities of Possible Responses in Experiment 5*

Study form	Test form	Pr (possible)
Possible	Possible	.66
Not presented	Possible	.61
Impossible	Impossible	.42
Not presented	Impossible	.34

Note. Pr = probability.

factors,  $F < 1.00$ . The main effect of possible versus impossible test item was significant,  $F(1, 24) = 11.31$ , and the main effect of deadline versus no deadline approached significance,  $F(1, 24) = 2.75$ . (Overall, participants were slightly less inclined to respond possible in the deadline condition.) The standard error of the means was 0.06.

#### Experiment 5: Memory Load

A second standard method for eliminating explicit retrieval processes is to give participants a memory load while they are making decisions (cf. Baddeley, 1976). In Experiment 5, participants were given a list of seven digits to keep in mind while responding to object decision test items. We predicted that retaining this memory load during the tests would, like imposing a deadline, inhibit explicit retrieval strategies and so lead to bias for the impossible objects.

#### Method

Experiment 5 was similar to Experiments 1–4: Participants were tested with objects that were either possible or impossible and either had or had not been studied previously. There were 40 participants in the experiment.

The first, left–right judgment study phase was presented as in the previous experiments, and the test phase began with the same explanation of possible–impossible objects. The test items were divided into two sets of practice items and four sets of experimental items, 10 items in each set. Before each set, seven digits were presented all at once for 2 s. Participants were instructed to keep the digits in mind while they made their responses to the object decision test items. The test items were displayed and responses to them collected with the same procedure as in Experiment 1. At the end of each set of test items, participants were asked to type the seven digits on the PC keyboard. Participants were instructed that it was very important to remember the digits, but it was also important to respond accurately on the object decision task.

#### Results

The data (displayed in Table 4) show a bias to respond possible to test objects that had been studied previously. There was a 5% increase in the probability of responding possible to previously studied possible objects, and there was an approximately equivalent increase in the probability of responding possible to previously studied impossible objects (8%). It appears that, like a deadline, a processing load eliminates the retrieval of explicit information so that impossible objects reveal the same bias as possible objects. An ANOVA showed the effect of previous study to be significant,  $F(1, 39) = 7.40$ ,

and the interaction of previous study with test item type to be nonsignificant,  $F < 1.00$ . The main effect of possible versus impossible test items was significant,  $F(1, 39) = 169.30$ . The standard error of the means was 0.02.

For their typical results (like those in our Experiment 1), Schacter, Cooper, and colleagues (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991) argued against bias as a possible explanation. However, the bias to respond possible to previously studied items was obscured in the typical object decision procedure because, we argue, the bias process was mixed with a retrieval process that makes available explicit information. When bias is revealed by manipulations that separate the two processes, it becomes apparent that the increased probability correct for possible objects is balanced (almost exactly in Experiments 2, 3, 4, and 5, and for fast responses in Experiment 1) by the decreased probability correct for impossible objects.

### Experiment 6: Similarity

The results of Experiments 2, 3, 4, and 5 support our hypothesis that the typical object decision pattern of data comes from two processes: a tendency to respond possible to familiar (previously studied) objects combined with retrieval of explicit information about those objects. The experiments all support our hypothesis in the same way, namely by imposing processing constraints to eliminate explicit retrieval and reveal bias operating alone. We designed Experiment 6 to eliminate the effects of explicit retrieval in a different way, with a design in which explicit information was not indicative of a decision in the object decision task. We made explicit information of no use by including in the experiment test conditions in which the test object was very similar to a previously studied object, but different in terms of whether it was possible or impossible (see the pairs of objects in Figure 1). The conditions of the experiment are shown in Table 5. For a possible test object, that exact same object was studied, the impossible (but very similar) version of the object was studied, or neither version of the object was studied. The analogous three conditions were used for impossible test objects. At test, participants should not perceive retrieval of information from explicit memory to be useful to the object decision task; just because parts of a test object are similar to or even identical to parts of a studied object, there is no reason to think the object is necessarily the same as the studied object in terms of being possible versus impossible. If the similarity manipulation does eliminate the use of explicitly retrieved information, then the pattern of data should show bias, equally for possible and impossible studied objects.

### Method

Both versions of all 42 objects were used in this experiment. There were two variables: In the first phase, an object was presented for right-left judgment in its possible version, its impossible version, or it was not presented at all. In the second, test phase, an object was flashed in either its possible or impossible version. These variables (both within subject) were crossed to form six conditions, combined with six groups of participants and six groups of items in a Latin square

design. There were 30 participants in the experiment. The procedure was the same as in Experiment 1, except that the amount of time for which a test item was flashed was different for different participants (an attempt was made to adjust the display time for different participants): for 6 participants, the display duration for the test object was 150 ms; for 12 participants, it was 200 ms; and for the remainder of the participants, it was 250 ms. We could see no difference in the patterns of performance across these participants, so they were combined for analysis of the results.

### Results and Discussion

As with Experiments 2, 3, 4, and 5, the most notable aspect of the data (Table 5) is that studying impossible objects had a large effect on test performance, in clear contradiction of what would be expected if studied objects were stored in an implicit memory system that could represent only possible objects. For an impossible test item that was studied in the first phase of the experiment, the probability of responding possible was .51, compared with .45 when the item was not studied. Similarly, when an impossible object was studied and then tested in its possible version, probability of responding possible was .70, up from .62 when the object was not studied. Study of an impossible object increased the likelihood that, in the test phase, participants would respond possible both to that object and to a similar possible object.

In fact, in all conditions, study in the first phase increased the probability of responding possible, consistent with our prediction that participants would be biased to respond possible for all studied test items because explicit episodic information was not predictive of whether a flashed test object was possible or impossible. An ANOVA showed the main effect of having studied an object to be significant,  $F(2, 58) = 3.60$ . The main effect of whether the test object was possible or impossible was also significant,  $F(1, 29) = 32.50$ . The interaction of the two factors was not significant,  $F < 1.0$ , showing no difference in the effect of study on impossible versus possible objects. The standard error of the means was 0.04.

### Experiment 7: Similarity and Forced Choice

We have defined bias for the object decision task as a tendency to respond possible to a test object that is familiar because of previous study. We designed Experiment 7 to examine this bias by combining the similarity manipulation of Experiment 6 with a forced-choice decision task. The study phase and design of Experiment 7 were the same as for Experiment 6; participants studied objects either not at all or

Table 5  
*Similarity—Probabilities of Possible Responses in Experiment 6*

Study form	Test form	Pr (possible)
Possible	Possible	.70
Impossible	Possible	.70
Not presented	Possible	.62
Possible	Impossible	.53
Impossible	Impossible	.51
Not presented	Impossible	.45

Note. Pr = probability.



in their possible or impossible versions and then were tested on either the possible or impossible versions (see Table 6). However, the decision required in the test phase was different. In Experiments 1–6, when a test object was flashed, participants were asked to decide whether it was possible or impossible. In Experiment 7, a test object was flashed and then that object and its other version were displayed for a forced choice: Participants were asked to decide which of the two displayed objects was the one that had been flashed. The forced-choice pair consisted of the object that had been flashed along with its very similar other version (like the pairs in Figure 1), so one of the forced-choice objects was possible, and one impossible.

From the framework of a structural description store as an implicit memory, there is no reason to expect the results of this experiment to differ in any interesting way from the typical pattern (the results we obtained in Experiment 1). Studied possible objects will be encoded in the structural description store, but impossible objects will not be. At test, if a studied possible object is flashed, processing will be facilitated, and so participants should be better able to choose it from the two alternatives. In contrast, if a studied impossible object is flashed, there will be no facilitation, and so performance on the forced choice should be no better than if the object had not been studied.

From the bias point of view, the pattern of results might change substantially from that obtained previously. When a participant is faced with the two objects and asked to choose between them, they could both seem equally familiar because they are so similar, even though only one of them was studied previously. That is, both the possible and impossible versions of an object could increase in familiarity as a result of previous study, not just the one that was actually studied. In fact, the increase in familiarity might be expected to be about equal for the two objects because, in Experiments 2–6, the bias for possible and impossible objects was about equal in size. If this is the case, then familiarity will not contribute any tendency toward one or the other of the responses—there will be no effect of prior study. Bias from prior study will not distinguish between the two alternatives. As in Experiment 6, retrieval of explicit information will also not distinguish between the two alternatives because similarity leads to participants' perceiving explicit information as relatively useless.

## Method

The first, study phase of the experiment was the same as in Experiment 6, but the second, test phase was different. In Experiment

**Table 6**  
*Similarity and Forced Choice—Probabilities of Possible Responses in Experiment 7*

Study form	Test form	Pr (possible)
Possible	Possible	.65
Impossible	Possible	.65
Not presented	Possible	.68
Possible	Impossible	.31
Impossible	Impossible	.28
Not presented	Impossible	.34

*Note.* Pr = probability.

6, participants were asked to indicate whether a flashed test item was a possible or impossible object. In Experiment 7, a flashed test item was followed by two objects displayed side by side, and participants were asked to choose which of them matched the flashed object. One of the two forced-choice objects was identical to the one that had been flashed, and the other was the other version of it. In other words, if a possible object was flashed, then that object and its impossible version were presented for forced choice, and if an impossible object was flashed, then that object and its possible version were presented for forced choice.

Each test item was preceded by a row of plus signs for 700 ms, then the test object was flashed for 200 ms, then there was a blank interval of 400 ms, and then the two objects for forced choice were displayed. They remained on the screen until participants pressed a response key, and then a blank interval of 300 ms preceded the next test item. Which version of an object was presented on the right-hand side in the forced-choice display and which was presented on the left-hand side was decided randomly. Participants indicated which version they believed matched the object that had been flashed by pressing the */* key for the right-hand object, the *Z* key for the left-hand object. There were 10 practice items preceding the 42 items from the experimental design.

The design of the experiment was the same as for Experiment 6: An object was presented for study in its possible version, its impossible version, or not studied at all, and an object was tested in either its possible or impossible version. There were 30 participants.

## Results

The prediction was that the two versions of a studied object would be so similar when presented for forced choice as to seem equally familiar, even though only one of the versions had actually been studied. As a result, familiarity would offer no basis on which to choose one of the objects over the other in the forced-choice test. From the results of Experiment 6, it was also predicted that explicit memory for specific configurations of features would also be perceived by participants as generally useless because of the high degree of similarity of the two objects. Thus, we anticipated that there might be no significant effect of prior study at all, and this is what the data show (Table 6). An ANOVA showed the probability of responding possible greater for possible than impossible objects,  $F(1, 29) = 136.40$ . No other effects approached significance. The standard error of the means was 0.03.

## General Discussion

The results of Experiments 2–7 are easily summarized in accordance with the proposal that priming in object decisions typically involves two processes: a tendency, or bias, to respond possible to previously studied objects plus the retrieval of explicit information about those objects. In the experiments reported by Schacter, Cooper, and colleagues (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991) and in our Experiment 1, these two processes worked in opposition to produce no significant effect of prior study on decisions about impossible objects. In Experiments 2–7, we used various manipulations to eliminate the retrieval of explicit information or its usefulness, and we were thus able to demonstrate the existence of the bias to respond possible for

both possible and impossible studied objects. Moreover, when we used the forced-choice decision task to ask participants to choose between a nonstudied object and a very similar studied object, it appeared that the bias for the two objects was sufficiently equated that participants were left with neither the bias nor explicit information to influence their decisions; the result was no significant effect at all of prior study.

The data from the experiments reported here raise a number of problems for the interpretation that object decision priming is the product of an implicit memory system. First and most obviously, the data cannot be attributed to a structural description system defined as Schacter, Cooper, and their colleagues defined it—a system that encodes possible but not impossible objects (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991). With three different retrieval manipulations—imposing a deadline, imposing a memory load, and introducing highly similar versions of each object—prior study of impossible objects was shown to affect later object decisions. In each case, all encoding conditions up to the point of test were the same as in the standard procedure, and only the retrieval test was manipulated.

On the one hand, it is noteworthy that it was so simple to use standard retrieval manipulations to produce data inconsistent with predictions from Schacter, Cooper, and colleagues' basic definition of an entire memory system. On the other hand, the definition of the structural description system was already severely limited because of its lack of theoretical specificity. Although the store has been said to have functional utility (Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991) "to improve identification of perceptual objects" (Tulving & Schacter, 1990, p. 301) and to have "evolved to perform only ecologically valid computations" (Tulving & Schacter, 1990, p. 303), virtually nothing has been said about how objects might be represented in the system or what kinds of processes might operate on those representations. Even for priming, the single finding of central interest, no specific mechanisms have been offered. The most that has been said is that "simply encoding the object as a whole was apparently not sufficient to produce facilitation of object decision performance"; what is required is "encoding of, and subsequent access to, a structural description" of the object (Schacter et al., 1990, p. 13) that will be "useful for making a correct object decision" (Schacter et al., 1990, p. 21).

These problems undermine the use of a structural description system to explain priming in the object decision task with normal participants. The notion of a structural description store may be on firmer ground when data from patients with certain deficits resulting from brain injuries are considered. For example, a patient reported by Riddoch and Humphreys (1987) showed intact access to structural knowledge about objects (the patient could distinguish real from make-believe objects) but impairment in naming the objects and impairment in making reference to semantic information about the objects. However suggestive such data might be, there is no obvious or easy way to connect the performance of impaired patients on identification of real, familiar objects to the performance of

normal participants on priming for unfamiliar objects in the object decision task.<sup>1</sup>

The difficulties of defining some task as a test of implicit memory have been recognized, and Schacter, Bowers, and Booker (1989) have proposed a solution. As Schacter, Cooper, and colleagues put it, it is not sufficient to claim that the object decision task is a test of implicit memory by virtue of the fact that "it does not make explicit reference to, or require conscious recollection of, any specific previous encounter with a presented object" (Schacter et al., 1990, p. 7). Instead, they proposed the "retrieval intentionality criterion" (Schacter et al., 1989, p. 53). By this criterion, object decision qualifies as a task that taps implicit but not explicit memory if it can be shown that there is some explicit task for which the test stimuli are exactly the same as for object decision, and it can be shown that there is some experimental manipulation that selectively affects performance on one of these two tasks but not the other. Object decision meets the criterion because recognition (judging whether test objects were or were not previously studied) uses exactly the same test stimuli, and there exist manipulations (e.g., encoding instructions in the study phase of the experiment) that affect performance on recognition but not object decision, and vice versa (Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991). What our data indicate (as do the data of Bowers and Schacter, 1990) is that even the retrieval intentionality criterion is insufficient to guarantee that a task does not involve the retrieval of explicitly encoded episodic information.

Previous interpretations of results from the object decision task assumed a one-to-one mapping between the task and the implicit memory system. The object decision task showed priming for possible but not impossible objects, and this result was combined with the assumption that the task was "factor-pure with regard to the type of processing it measured" (Jacoby, 1991, p. 515). As Jacoby pointed out, and as our results confirm, a mistaken picture of processing, memory systems, or both, can easily emerge from the factor-pure assumption.

<sup>1</sup> To bolster the connection between the structural description store that has been proposed for brain-damaged participants and priming effects in normal participants, Schacter, Cooper, Tharan, et al. (1991) conducted an experiment analogous to Experiment 1 with three groups of participants: amnesic patients, matched-age control participants, and normal college students. However, the results of the experiment were not easily interpretable: For nonstudied objects, both amnesic patients and students showed object decision performance near chance; amnesic patients showed the typical object decision priming effect for possible but not impossible objects, but matched-age control participants showed a large priming effect for impossible objects; amnesic patients performed worse on recognition of impossible objects than matched-age control participants, but about equally well on recognition of possible objects. In another experiment with amnesic participants, the critical interaction between possible versus impossible test objects and previous study versus no previous study was apparently not significant (Schacter, Cooper, & Treadwell, 1993). What is needed is to replicate these experiments with more participants, with retrieval manipulations such as a deadline or a memory load, and with different objects for the two tasks so as to rule out between-task memories for specific items.

Showing that the object decision task met the retrieval intentionality criterion was only one of the ways that Schacter, Cooper, and colleagues (Cooper et al., 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991; Schacter, Cooper, Tharan, et al., 1991) argued for explaining object decision priming in terms of an implicit memory system. They also argued that object decision priming must reflect a memory system independent of explicit episodic memory because performance on object decision can be shown to be independent of performance on recognition. However, the argument that independent performance on two tasks necessitates the assumption of two memory systems has come under frequent attack.

"Functional" independence describes the finding that performance on one but not the other task is affected by some variable (it is this kind of independence that makes up part of the retrieval intentionality criterion). For object decision, this dissociation is manifested in performance on priming of an implicit task versus performance on one episodic task, recognition. For example, changing the encoding judgment for object decision from "is the object left or right facing" to "does the object have more horizontal or vertical lines" affects later performance on object decision but not recognition (Schacter et al., 1990). However, as stressed by Hintzman (1990) and Roediger (1990; see also McKoon, Ratcliff, & Dell, 1986; Ratcliff & McKoon, 1986; Tulving, 1986), dissociations do not require the assumption of two different memory systems. Recognition and recall, for example, are two episodic tasks that dissociate as a function of the frequencies with which the words to be remembered occur in natural language, but no model postulates separate memory systems to explain behavior in these two tasks. Furthermore, a single dissociation could not describe the amount of overlap in processing between two tasks (Shimamura, 1993). Again using recall and recognition as an example, these two tasks behave in the same ways as a function of many variables other than word frequency. In the main, we agree with Hintzman (1990, p. 121), who suggested that once a model of performance is developed for some set of tasks, distinctions among memory systems will become less important as "the explanatory burden is carried by the nature of the proposed mechanisms and their interactions and not by what they are called."

"Stochastic" independence describes the relation between performance on an implicit task and performance on an explicit task in another way: The two tasks are said to be stochastically independent if performance on one of the tasks is not correlated with performance on the other (cf. Schacter et al., 1990; Schacter, Cooper, Delaney, et al., 1991). A number of problems have been identified with inferring different memory systems from stochastic independence. For one, intercorrelations are not always examined across a range of tasks. When Perruchet and Baveux (1989) did test a range of tasks, the correlations did not split along an explicit-implicit dimension. For example, anagram solution was correlated with recall and perceptual identification but not with recognition or fragment completion.

Another problem is that a measure of stochastic independence between two tasks can represent a kind of artifact that comes about because of averaging over items; Hintzman and

Hartry (1990) have illustrated this problem by showing that the stochastic independence between fragment completion and recognition found by Tulving, Schacter, and Stark (1982) can come about because of averaging over different subsets of items, some that negatively correlate across the two tasks and some that positively correlate. In other words, performance for some individual item is determined by the difficulty of that item in the particular task, and what makes the item difficult in one task may correlate positively or negatively with what makes it difficult in another task, and these correlations can differ across items. As a result, the overall correlation between two tasks may be meaningless and so offer no reason to postulate separate memory systems.

A third problem with efforts to examine correlations between performance on an implicit task and performance on an explicit task is that significant dependence between the tasks would be difficult to demonstrate even if it actually existed. Ostergaard (1992) has shown that, for a number of task comparisons in which stochastic independence was found, there were insufficient numbers of subject-item observations to provide the power necessary to detect dependence. For example, even if every item that was primed in fragment completion was also correctly recognized, there would not be sufficient power in typical experiments to show dependence. As Hintzman and Hartry (1990, p. 968) pointed out, "Finding stochastic independence really means accepting the null hypothesis of zero correlation," and Ostergaard has made clear that previous experiments have not had sufficient power to make convincing the acceptance of the null hypothesis.

Lack of power is especially likely to be a problem with the object decision task. There are many different ways to encode the objects, with lines and angles making up smaller objects, and these in turn making up more complex objects, with some objects obscuring other objects, and so on, and there is likely to be large variability in the way different items are encoded for different participants and for different tasks and large variability in the kind of encoded information that is relevant to different tasks (see Tulving & Thomson, 1973). To demonstrate this problem, we tested the objects used in Experiments 1-7 in two explicit tasks: yes-no recognition and forced-choice recognition. The yes-no recognition experiment was made up of a study phase (as in Experiments 1-7) and a test phase; each object in the test phase was displayed for 500 ms and participants were asked to respond yes or no regarding whether it had appeared in the study list (only one version of each figure was used so there were no highly similar distractors). For forced-choice recognition, the study phase was also the same as in the previous experiments. In the test phase, the two versions of an object were presented together and participants had to decide which of them, the possible one or the impossible one, had previously been studied. For 40 participants and 40 items, performance on these two tasks was only on the border of being significantly correlated: The correlation was only .31, about two standard deviations from zero (see Underwood, Boruch, and Malmi, 1978, for a study producing reliable correlations for a range of episodic and semantic tasks). For the three-dimensional object stimuli, if performance on two closely related, episodic recognition tasks with typical numbers of participants and materials is not strongly

correlated, it is unlikely that two such different tasks as object decision and recognition could show dependence, even if they depended on information from the same cognitive system.

The lack of strong correlation between performance on two different recognition tasks indicates that we should not expect positive correlations between either the bias component of object decision and recognition or the explicit retrieval component and recognition. Not only is there the general problem of variability across items and tasks, there is also the likelihood that the features of an object that facilitate processing for one component of one task will not be the same features that facilitate processing on some other component of the same task or a different task (cf. recall and recognition, Tulving & Thomson, 1973). Our data indicate that the information that produces bias becomes available early in processing and this suggests that it might depend on quickly encoded general configurations of features. The explicit slower retrieval component of the object decision task might depend on retrieval of the specific configurations associated with whether an object is possible or impossible. Different recognition tasks might depend on either this same information or on still other aspects of the encoded representation of an object; for instance, that it looks like a house or a dog or appeared early in the study list. For example, performance on a recognition task in which participants were asked to pick which of two versions of an object, the possible version or the impossible version, had occurred on a previous study list might depend, to some extent, on the same information as object decision. A recognition task that simply required old–new decisions for each item might depend more on semantic elaborations. Unfortunately, the explicit component of processing in object decision appears to affect only a small fraction of performance so that the typical 10%–15% priming effect will not provide the power to allow assessment of correlations or stochastic dependence (see Ostergaard, 1992).

In summary, we see no compelling reason to suppose that priming in the object decision task is mediated by an implicit memory system. The original motivations for implicit memory as an explanatory device for object decision can all be discounted: We have shown that memory for both possible and impossible objects affects performance in object decision and that object decision can involve retrieval of explicit information (despite fulfilling the retrieval intentionality criterion); others have shown numerous problems with the logic underlying functional and stochastic independence arguments. As an alternative to the implicit memory explanation, we have proposed that priming is the result of a bias to respond possible to objects previously studied.

Bias also describes the pattern of data in other implicit tasks (Jacoby, 1983; Ratcliff & McKoon, 1994; Ratcliff, McKoon, & Verwoerd, 1989). For example, in perceptual identification, words are briefly flashed and the typical result is that participants are more likely to name them correctly if they have been studied previously (Jacoby, 1983; Jacoby & Dallas, 1981). Ratcliff et al. (1989) showed that this was a bias effect by using a forced-choice testing procedure. Asked to choose which of two similar words (e.g., *died* or *lied*) matched the word that had been flashed, participants tended to choose the one that had previously been studied, with the result that probability correct

improved as a function of prior study for words that had actually been studied but suffered when the word flashed was similar to but different from the one studied earlier.

The bias effect in perceptual identification is different in a crucial way from the bias effect in object decision. In perceptual identification, bias is a tendency to choose a particular previously studied individual word as a response. In object decision, bias is a tendency to make the same response (possible) to all objects that have been studied and to all objects that are very similar to studied objects. This difference is pointedly apparent with the forced-choice testing procedure. The general bias to respond possible to anything that is highly similar to something that was studied cannot guide a decision toward one over the other of two similar objects presented for forced choice. In the situation of forced choice between highly similar objects, memory gives only the general bias and not sufficiently specific information about whether the possible or impossible object was previously encoded. In perceptual identification, on the other hand, memory does give specific information that, for example, *died* but not *lied* was studied.

More important, the differences between the object decision and perceptual identification biases illustrate the different kinds of biases that could occur in the many levels of a single cognitive system, each of which could lead to a different kind of bias in a different empirical measure. For perceptual identification, a model like the interactive activation model proposed by McClelland and Rumelhart (1981) might be appropriate, with bias operating at a word node level (see Ratcliff et al., 1989) or perhaps at the levels of individual features or letters. For object decision, the model proposed by Hummel and Biederman (1992) might be appropriate. In this model, there are seven layers of elements that take as input a representation of a line drawing of an object and give as output a unit representing the object. Input from lower levels activates higher level representations and relations among them. The higher level representations are simple primitive volumes called *geons*. The geons are assembled at the highest level to produce object representations. The lower levels of the model are hard wired, but the upper levels can be trained by weight modifications to the connections in the upper two levels, so the model can exhibit learning. If the model were adapted to make possible–impossible judgments, and to be able to respond to brief presentations of stimulus objects, then it is easy to see several ways that prior presentation might lead to a bias to respond possible. Prior presentation might modify connections between nodes in the upper levels so that a repeated presentation would increase the probability of constructing a representation of a possible object. Alternatively, bias might be implemented as a reduction in a response criterion that would lead to increased probability of responding possible.

What is shared across a number of tasks (stem completion, fragment completion, and picture naming as well as object decision and perceptual identification; Ratcliff and McKoon, 1994), and potentially across information-processing models of those tasks, is the tendency for participants to make their responses consistent with information that was previously encoded. This tendency improves performance if the test item is exactly the same as the one that was encoded or requires the same decision, but it hurts performance if the item requires a

competing decision. In the object decision experiments reported here, the costs to performance on impossible studied items were about equal to the benefits for possible studied items. However, in the general case, bias will not be implemented in a single way always in the same model; the implementation will depend on the particular processing model and the particular empirical measure (reaction time, probability correct, probability of producing a particular response or item, etc.).

It is the implication of costs offsetting benefits that distinguishes the bias explanation of priming effects from the transfer of processing view espoused by Roediger and his colleagues (e.g., Roediger, 1990; Roediger & Blaxton, 1987). The transfer appropriate processing view rejects the postulation of memory systems as an explanatory mechanism and proposes instead that priming occurs to the extent that processing on a current test of an item overlaps the processing that occurred on an earlier presentation of the item. To the extent that processing fails to overlap (because of changes in, for example, modality of presentation), priming is reduced. Although the transfer appropriate processing view and our view are similar, we add the specific mechanism of bias with its associated costs as well as benefits, and we stress that the processes that mediate costs and benefits need to be specified individually within a theoretical framework for each of the implicit tasks that exhibit priming. Researchers should begin to examine whether these implicit priming effects can be explained within existing models of information processing.

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