ORIENTATION, SIZE, AND RELATIVE SIZE INFORMATION IN SEMANTIC AND EPISODIC MEMORY

by

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Abstract

The time required to identify a common object depends on several factors, especially pre-existing knowledge and episodic representations newly established as a result of a prior study. My research examined how these factors contribute to identification of objects (both studied and non-studied) and to performance on explicit memory tests. The overall goal was to explore the link between memory and object perception.

One series of experiments examined influences due to object orientation in the plane of the page. Subjects were shown color photos of objects, and memory was assessed either with an old/new recognition test or with a test that required them to identify objects that were slowly faded in on a computer monitor. The critical variables were the type of photo -- each showing either an object with a predominant or *cardinal* orientation (e.g., helicopter) or a non-cardinal object (e.g., pencil), and the orientation at which the photos were displayed at study and at test (e.g., rotated 0°, 120°, or 240°). For non-studied targets, identification test performance showed a large effect due to display orientation, but only for cardinal objects. For studied targets, study-to-test changes in orientation influenced priming for both non-cardinal and cardinal objects, but orientation specific priming effects (larger priming when study and test orientations matched rather than mismatched) were much larger with cardinal than non-cardinal objects, especially when their display orientation at test was unusual (i.e., 120°, 240°).

A second series of experiments examined influences due to object size (size of an object presented alone) and relative size (size of an object relative to another object). Size manipulations had a large effect on identification of non-studied objects but study-to-test changes in size had only a minimal effect on priming. In contrast, study-to-test changes in relative size influenced recognition decision speed which is an index of priming.

The combined findings suggest that both semantic and episodic representations behave as if they coded orientation but only for cardinal objects. They also suggest that episodic representations code relative size but not size information. The findings are explained by the instance views of memory.

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Chapter 1

General introduction

The past decade has seen a lot of research on what is today called implicit and explicit memory. *Explicit memory* is the mode of remembering that we usually think of when talking about memory; it refers to situations in which prior events or experiences are recollected in an intentional and deliberate manner. In contrast, *implicit memory* refers to situations in which prior events or experiences facilitate performance of a task in the absence of intentional recollection. Implicit memory is indexed by *priming* -- an improvement in performance, in, for example, identification speed or response accuracy due to a prior encounter with to-be-remembered targets. The distinction between these two ways of recollecting is interesting for a number of reasons. One of them is that amnesic patients, who by definition are impaired on explicit memory tests, can perform as well as normal healthy subjects on implicit memory tests. The second reason is that children and elderly individuals who perform sub-normally on explicit memory tests show normal performance on implicit memory tests. The third reason is that performance on implicit and explicit memory tests can also be dissociated in normal healthy adults by a variety of experimental manipulations.

One of the most emphasized findings from this research is that implicit memory is much more sensitive to sensory cues than is explicit memory; this finding is revealed by surprisingly specific effects on implicit but not explicit memory tests. Research has emphasized that priming on implicit memory tests, but not performance on explicit

memory tests, tends to be reduced by study-to-test changes in presentation modality (e.g. , auditory to visual vs. visual to visual), as well as by changes in symbolic format (e.g., word to picture vs. picture to picture), word fragments cues (e.g., F_R_S_ to F_R_S_ vs. _O_E_T to F_R_S_), typography (e.g., QUICKLY vs. quickly), or object views (e.g., unusual vs. usual).

The emphasis on the specificity of priming contrasts with well-known demonstrations of object constancies (e.g., shape constancy, size constancy) in research on object perception. Object perception researchers tend to use the same methods as memory researchers, various identification tests and priming paradigms, but their aim is to investigate how we identify/recognize objects across changes in vantage point, in illumination, or in setting or context. In contrast to memory research, therefore, object perception research highlights the fact that variations in sensory and perceptual cues produced by changes in orientation, size, and color have little or no effect on identification of familiar objects.

My research focuses on the bridge between these domains: memory research and object perception research. It explores the possibility that the different emphases of these two domains -- specificity vs. constancy -- stem from the fact that perception and memory researchers proceed on different levels of analysis: Memory research is primarily concerned with specific targets as instances or tokens, whereas the targets for perception are types.

My work proceeds on the assumption that these two approaches are commensurate with each other. It is possible that each encounter with a target is encoded separately, and establishes a new episode-specific representation (a new instance) in memory. Thus, a target that is encountered more frequently has many more instances in long term memory than a target encountered less frequently. In turn, if these pre-existing representations are recruited for and facilitate identification of the target on a subsequent encounter, it follows that the impact of any single target representation on identification depends on how many other representations of the same target are available in long term memory. The impact of a single episode-specific representation is largest when there are no other representations available in memory to facilitate target identification whereas its impact decreases with an increase in the number of pre-existing representations. By this chain of assumptions, specificity effects can arise due to the lack of prior experience with identifying targets under a particular set of study/test conditions. In contrast, the object constancies and the absence of specific priming effects result from extensive experience with identifying targets under a wide variety of orientations, sizes, or illumination conditions, and therefore, from diminished influence of an episode specific representation.

Objectives and hypotheses. To explore this possibility and to gain insight into the relative contribution of episode-specific representations (episodic representations) vs. pre-existing representations in memory, my research investigates the relationship between

baseline identification, the magnitude of priming, and performance on explicit memory tests.

Consistent with the instance views of memory that guide my work, in my dissertation, the term *episodic representation* is used to refer to an episode-specific representation -- an instance -- established at prior study. In contrast, the term *semantic representation* is used as a convenient shorthand label for the processes that mediate performance on semantic memory tasks. Semantic memory representations are not different types of representations; they are a function, an average computed across the relevant set of episodic representations or episodic instances.

My first three experiments focused on the orientation of objects in the plane of the page; specifically, they examined how orientation influences baseline identification (i.e., identification of non-studied targets) as well as how it influences performance on implicit and explicit memory tests. The experiments include two kinds of objects: *cardinal objects* -- objects which are usually encountered in a single predominant or *cardinal orientation* (e.g., a helicopter), and *non-cardinal objects* -- objects that tend to occur in many different orientations (e.g., a scissors). Even though these objects' sets are treated as different kinds in my research, cardinality is a continuum, which at one end is defined by objects that are always encountered in the same orientation (e.g., house), and at the other end is defined by objects that are frequently encountered in many different orientations (e.g., a beach ball).

The cardinal and non-cardinal objects included in my experiments were selected to be at the respective end-points of the cardinality continuum. Cardinal object were assumed to have pre-existing representations that are largely specific to their predominant orientation, and thus, baseline identification and priming for these objects should be affected by study-to-test changes in orientation. In contrast, non-cardinal objects have pre-existing representations in many different orientations or views, and therefore, identification and priming should be only minimally affected by orientation changes between study and testing. The fourth and fifth experiments focused on objects' size (i.e., size of an object encountered alone) and on the relative size of objects (i.e., size of an object encountered next to another object). The primary question was how these variables affect baseline identification, priming, and explicit memory test performance.

Overview. My thesis is divided into four additional chapters. The second chapter reviews measures of implicit and explicit memory, existing experimental findings, and theories proposed to account for the findings. It presents a selective review in the sense that it considers only major trends, issues, and insights in the literature that are closely relevant to my research.

Chapter three begins with a brief review of the most recent research on identification and implicit and explicit memory for common objects. The bulk of the chapter is an article on *Object Orientation Information In Semantic and Episodic Memory* (Uttl & Graf, in press) that describes a series of three experiments on the relative contribution of episodic and semantic representations to implicit and explicit memory test

performance. For each of these experiments, subjects were shown color photos of objects, and memory was assessed either with an old/new recognition test or with a test that required them to identify objects that were slowly faded in on a computer monitor. The critical variables were the type of photo -- each showing either an object with a predominant *cardinal* orientation (e.g., helicopter) or a *non-cardinal* object (e.g., pencil) -- and the orientation at which the photos were displayed at study and at test (i.e., on the plane of the page at 0°, 120°, 180°, 240°). For each subject, half of the targets were shown at study and all appeared on the test, with targets displayed either in the same orientation as at study or in a different orientation. For non-studied targets (i.e., in the baseline condition), identification test performance showed a large effect due to display orientation, but only for cardinal objects. For studied targets, identification test performance showed substantial priming in all conditions, with more priming on cardinal than on non-cardinal targets, especially when their display orientation at test was unusual (i.e., 120°, 240°) and the same as at study. These findings are used to discuss the extent to which orientation information is coded in the semantic and episodic memory representations of different kinds of objects.

Chapter four focuses on object size and relative size information in semantic and episodic memory. It reports two experiments. The first experiment examined how object size (i.e., object displayed alone) influences baseline identification, priming, and performance on an explicit memory test. For study, subjects saw color photos of objects that were displayed in one of three sizes: small, medium, and large. For memory testing,

photos of objects were slowly faded in on a computer monitor and subjects were required to identify each object (identification test) or to decide whether they had encountered either each object in prior study (old/new recognition test). For each subject, half of the targets were shown at study and all appeared on the test, with targets displayed in the same size as at study or in one of the other two sizes. Identification test results showed that object size influenced identification but not priming. Size also influenced old/new recognition accuracy.

The second experiment examined whether the relative size of objects (i.e., size of an object relative to another object) influences priming and performance on an explicit memory test. In this experiment, subjects were shown pairs of objects displayed next to each other: One object was designated the target and the other the context. The context appeared immediately at the beginning of each trial whereas the target was always slowly faded-in until subjects made a response. For study and implicit memory testing, subjects rated the relative size of the target vs. the context (relative size rating test). For explicit memory testing, they rated whether each target was old or new (recognition test). For each subject, targets appeared either in the same or different relative size and they appeared either with the same or different contexts. The results confirmed that size influences baseline performance, and they failed to show influence due to the relative size manipulation on the relative size rating test. However, the relative size manipulation influenced recognition decision speed, which is often considered an index of priming.

The last chapter of my thesis (Chapter 5) discusses theoretical and methodological contributions, and the limitations of this line of work. The chapter also suggests avenues for future research.

Chapter 2

Implicit and explicit memory: Selective review of measures, data, and theory

Remembering is usually thought of as involving conscious recollection of specific events or experiences. This kind of remembering is usually referred to as *episodic memory*. The last decade of research on episodic memory has shown convincingly, however, that memory for prior events can also be expressed in the absence of intentional recollection, for example, by an increased ability to identify words, objects, or other kinds of stimuli. To distinguish between intentional and non-intentional recollection of episodes, Graf and Schacter (1985) coined the terms *implicit* and *explicit* memory, respectively. These terms refer to different ways of engaging memory; they specify *the mental states initiating and guiding performance* on various memory tests. Explicit memory refers to test performance that is initiated and guided by subjects' intention to recollect specific prior events; implicit memory refers to influences of prior events on test performance in the absence of subjects' intention to recollect such events.

The impetus for current research on implicit and explicit memory was the finding that patients with organic amnesia who have severe difficulties intentionally recollecting prior events can nevertheless show normal performance on implicit memory tests (e.g., Graf & Mandler, 1984; Warrington & Weiskrantz, 1968, 1970, 1974; for reviews see Moscovitch, Vriezen, & Goshen-Gottstein, 1993; Squire, 1992). This finding demonstrated that implicit memory is spared by neurocognitive trauma that causes amnesia. Another impetus for research was findings showing that implicit memory may

function normally in young children whose explicit memory has not yet fully developed, and it may also function normally in older adults whose explicit memory has declined with advancing age (for reviews see La Voie & Light, 1994; Mitchell, 1993; Naito & Komatsu, 1993; Parkin, 1993). A final impetus for recent research on the nature of implicit and explicit memory was the demonstrations that similar patterns to those found with amnesic patients can be simulated in normal healthy adults. Graf, Mandler, and Haden (1982) demonstrated impaired intentional recollection together with normal priming in normal healthy adults when they were prevented from meaningful encoding of studied materials by means of a study task.

The finding that memory functions may be dissociated by neurocognitive trauma, by lifespan development, and by study task manipulations in normal healthy adults has motivated widespread research into the nature of the processes or mechanisms that mediate implicit as opposed to explicit memory. This chapter reviews measures of implicit memory, existing experimental findings, and theories proposed to account for the findings. It is a selective review in the sense that it considers only major trends, issues, and insights in the literature that are closely relevant to my research. In addition, the forthcoming Chapters 3 and 4 include a more detailed review of the research that investigated the influence of orientation and size on implicit and explicit memory.

This chapter is divided into three sections. The first section concerns the measurement of implicit memory. The second section reviews well established experimental findings from investigations of amnesic patients, from studies of children

and the elderly, and from experiments with normal healthy adults. Finally, the third section outlines theoretical accounts that have been proposed to explain the findings.

Measures

This section introduces various implicit and explicit memory tests, it discusses different ways of classifying memory tests, it highlights the measurement problem inherent in the definition of implicit and explicit memory, and it outlines various strategies to minimize the measurement problem.

Measures of implicit and explicit memory

Table 2.1 provides examples of widely used implicit and explicit memory tests, and it lists their most important characteristics, including type of cues, availability of cues, test instructions specifying a subject's task, and types of dependent variables. The majority of the empirical research findings have been obtained with only a subset of the implicit memory tests: word identification (e.g., Feustel, Shiffrin, & Salasoo, 1983; Jacoby & Dallas, 1981), word stem completion (e.g., Graf & Mandler, 1984; Graf et al., 1982; Schacter & Graf, 1986), word fragment completion (e.g., Roediger, Weldon, Stadler, & Riegler, 1992; Tulving, Schacter, & Stark, 1982), picture identification and naming (e.g., Durso & Johnson, 1979; Warren & Morton, 1982), picture fragment completion (e.g., Gollin, 1960; Warrington & Weiskrantz, 1968; Weldon & Roediger, 1987; Weldon, Roediger, Beitel, & Johnson, 1995), and object decision (e.g., Kroll & Potter, 1984; Schacter, 1990). Similarly, researchers have typically used only a few

Table 2.1
Examples of the implicit and explicit memory tests and their characteristics.

Modality/ Materials	Test	Cues	Cue availability	Instructions	Dependent measure	Reference
			IMPLICIT MEMORY TESTS			
Visual/Verbal						
	word identification	intact words, e.g., ASSASSIN	~ 40 ms	identify each word	accuracy	Jacoby & Dallas (1981); Feustel et al. (1983)
	continuous threshold word identification	intact words, e.g., ASSASSIN	repeated exposures 2 ms – 100 ms	identify each word as quickly as possible	threshold	Feustel, et al. (1983)
	word stem completion	the first three letters of each word, e.g., ASS____	unlimited	complete each cue with the first word that comes to mind	accuracy	Graf et al. (1982); Graf & Mandler (1984); Graf et al. (1984)
	word fragment completion	words with deleted letters, e.g., A_S_SS_N	15 – 30 s	complete each fragment with a legal English word	accuracy	Tulving et al. (1982); Roediger et al. (1992)
	lexical decision	words and nonword letter strings, e.g., ASSASSIN vs. ARSASSIN	unlimited	decide whether or not each letter string is a legal word as quickly as possible	latency	Scarborough et al. (1977)
	anagram solution	scrambled words, e.g., SSANIASS	unlimited	generate a word from letters	accuracy	Srinivas & Roediger (1990)

Table 2.1 continued
Examples of the implicit and explicit memory tests and their characteristics.

Modality/ Materials	Test	Cues	Cue availability	Instructions	Dependent measure	Reference
	free associations	cue words, e.g., *ANIMAL -- ?*	unlimited	provide a single word associate	accuracy	Gardner et al. (1973)
	category instance production	category names, e.g., *FRUITS -- ?*	unlimited	say 8 items that belong to each category	accuracy	Gardner et al. (1973); Graf et al. (1985)
	general knowledge questions	questions, e.g., *Which city is the capital of Australia?*	unlimited	answer each question	accuracy	Roediger & Blaxton (1987b)
Auditory/Verbal						
	word identification	intact words embedded in a white noise, e.g., ASSASSIN	~ 1 s (single exposure)	identify each word	accuracy	Morton (1979)
	word stem completion	the first three letters of each word, e.g., ASS___	single presentation	complete each cue with the first word that comes to mind	accuracy	Bassili et al. (1989)

Table 2.1 continued
Examples of the implicit and explicit memory tests and their characteristics.

Modality/ Materials	Test	Cues	Cue availability	Instructions	Dependent measure	Reference
Visual/Pictorial						
	picture identification	line-drawings of familiar objects	~ 50 – 100 ms	identify each object	accuracy	Warren & Morton (1982)
	picture naming	line-drawings of familiar objects	unlimited	name each object as quickly as possible	latency	Durso & Johnson (1979); Biederman & Cooper (1992)
	picture fragment completion	fragmented line-drawings of familiar objects	unlimited	identify each object	accuracy	Gollin, 1960; Weldon et al. (1995)
	classification	line-drawings of familiar objects	unlimited	decide whether each object is man-made or natural as quickly as possible	latency	Durso & Johnson (1979)
	object decision	line-drawings of possible and impossible objects	~ 100 ms	decide whether each object could exist in the real world	accuracy	Kroll & Potter (1984); Schacter et al. (1990)
		EXPLICIT MEMORY TESTS♣				
Visual/Verbal, Auditory/Verbal, Visual/Pictorial						
	old/new recognition	target copies (e.g., spoken words, written words, pictures)	unlimited	decide for each target whether or not it is old (i.e., was studied)	accuracy	

Table 2.1 continued

Examples of the implicit and explicit memory tests and their characteristics.

Modality/ Materials	Test	Cues	Cue availability	Instructions	Dependent measure	Reference
	cued recall	target fragments (e.g., word stems, word fragments, picture fragments)	unlimited	complete fragments with studied targets	accuracy	Graf & Mandler (1984)
	free recall	no target cues	N/A	recall studied targets	accuracy	

♣Many of the implicit memory tests can be readily converted into the explicit memory tests by simply changing the instructions. Thus, for explicit memory tests, the table only illustrates three classes of the most often used explicit memory tests.

explicit memory tests, especially free recall, cued recall, and old/new recognition. This section focuses only on these most frequently used tests.

Classifying memory tests

One of the motivations for classifying memory tests has been a desire to explain performance dissociations between implicit and explicit memory tests. Memory tests can be arranged in a number of different ways. Table 2.1 arranges the tests by their implicit/explicit instructions, by the modality in which materials are presented at test (visual, auditory), and by the symbolic format of the materials (verbal, pictorial). Within these classifications, the tests are arranged by the extent to which the test cues are direct copies of targets (e.g., word identification tests), partial or fragmented copies of targets (e.g., word stem and word fragment completion tests), or only indirectly related to targets (e.g., free association tests). The extent to which test cues are direct copies of targets forms the basis of two classification schemes proposed in the literature.

One of these classification schemes was proposed by Roediger and his colleagues (Blaxton, 1989; Roediger, 1990; Roediger & Blaxton, 1987a; Roediger & McDermott, 1993; Roediger & Srinivas, 1993; Roediger, Weldon, & Challis, 1989). Roediger et al. (1989) argued that memory tests can be ordered on a continuum ranging from tests relying primarily on *data driven* processing to tests dependent primarily on *conceptually driven* processing. Data driven processing is initiated, guided, and determined by the data, for example, by the visual marks that define a printed word or by the lines that form a picture. Conceptually driven processing is initiated and guided by information already

registered in episodic and semantic memory; that is, by the knowledge and by the concepts acquired from previous experience (Norman & Bobrow, 1975). Roediger and his colleagues have argued that memory tests such as lexical decision, word identification, and picture identification engage primarily data driven processing; they assumed that on these tests performance depends primarily on processing that is driven by direct or partial copies of the targets provided as test cues. In contrast, memory tests such as free association, category instance production (implicit memory tests), and free recall (explicit memory tests) engage primarily conceptually driven processing, and performance depends on the strength of associations between targets and indirect cues provided at testing. Other memory tests, such as word stem completion and cued recall, fall between these extremes.

This classification scheme has been used widely to explain differences between various implicit memory tests (e.g., Blaxton, 1989) and also between implicit and explicit memory tests (e.g., Roediger & Blaxton, 1987a; Weldon et al., 1995). The classification captures nicely the distinction between, for example, word identification tests and free association tests. For a word identification test, subjects are briefly shown intact targets as test cues (i.e., printed words) and they are required to identify them. Test performance is determined primarily by data driven processing of the intact targets and only minimally by conceptually driven processing because performance does not require processing of associations between the targets and other mental contents. In contrast, for a free association test, subjects are shown only indirect cues, usually semantically related words

(e.g., *hot* for *cold*), and they are asked to free associate to these cues. Test performance requires some data driven processing of the indirect cues, but it depends primarily on conceptually driven processing, that is, on processing of semantic associations between the test cues and the targets. This intuition about the processing requirements of these two kinds of tests is supported by the findings showing that various manipulations of the data at study, for example, presentation modality (e.g., visual vs. auditory), influence performance on word identification tests (e.g., Jacoby & Dallas, 1981), whereas the same manipulations have no effect on performance of category instance production tests (e.g., Srinivas & Roediger, 1990). In contrast, study tasks (e.g., semantic processing and elaborative processing tasks) that build associations between targets and other mental contents influence performance on category instance production tests (e.g., Srinivas & Roediger, 1990) but not on word identification tests (e.g., Jacoby & Dallas, 1981). For a more detailed review of this evidence, see the section on 'Sensory and perceptual features effects'.

Roediger and his colleagues (Roediger & Weldon, 1987; Roediger et al., 1989) suggested that their classification scheme also captures the contrast between implicit word identification tests and explicit free recall tests. For a free recall test, subjects are provided with no direct or indirect target cues; instead, they are merely told to recollect previously studied items. Thus, performance on such a test requires primarily conceptually driven processing since subjects must retrieve previously studied targets with the aid of associations between their reconstruction of the study phase and the to-be-

remembered targets. Consistent with this suggestion, free recall test performance is typically higher following semantic vs. nonsemantic study tasks (e.g., Craik & Lockhart, 1972; Graf & Mandler, 1984; Graf et al., 1982; Lockhart & Craik, 1990), and it is unaffected by manipulations of the data at study, for example, by manipulations of presentation modality (e.g., Graf, Shimamura, & Squire, 1985).

Classifying tests on the basis of cues alone, however, does not capture the differences between implicit and explicit memory tests that have exactly the same cues and differ only in terms of instructions (Graf & Ryan, 1990). Graf and Mandler (1984) presented subjects with the first three letters of each target word as cues and gave either implicit or explicit memory test instructions. The implicit memory instructions required subjects to complete each stem with the first word that came to mind (word stem completion test), whereas the explicit memory instructions required them to complete the stems with previously studied words (cued recall test). The results showed that manipulating conceptual processing at study -- by requiring subjects either to count visual features of each target word or to rate how much they liked each word -- did not affect performance on the word stem completion test whereas it had a large effect on performance of the cued recall test. What is critical is that this performance dissociation occurred despite the fact that the test cues were exactly the same for both tests.

One way to avoid the problem associated with classifying tests on the basis of cues alone is to take into account both the test cues and the instructions. Toward this goal, Craik (1983, 1986) arranged memory tests on a continuum that reflects the degree to

which test performance is supported or guided by the environment vs. the degree to which it is dependent on subject-initiated activities. What is meant by environmental support and by subject initiated activities? If one includes both the cues and instructions in the environmental support provided by a test, this classification scheme captures differences between various implicit and explicit memory tests better than the distinction between data driven vs. conceptually driven tests (see Graf, 1990; Uttl & Graf, 1993). In exchange, however, this classification scheme trades off the precision afforded by arranging tests only on the basis of cues as suggested by Roediger and his colleagues. Consider, for example, whether an explicit old/new recognition test or an implicit free association test is closer to the environmentally supported vs. subject initiated end of the continuum. The degree of environmental support provided by particular test cues and by instructions is likely to depend on subjects' preexisting knowledge and on their task relevant skills, including their familiarity with the cues and with the requirements of the task.

Measurement problem

Implicit and explicit memory are defined in terms of the mental sets initiating and driving test performance. A mental set is, however, under the subject's rather than the experimenter's control and this creates a measurement problem concerning the validity of a test: How do we know that subjects' performance on a nominally implicit or explicit test was initiated and guided by the presence vs. absence of intentional attempts to recollect previous experiences or events? In an attempt to establish a proper mental set

for initiating and guiding test performance, the experimenter can manipulate various aspects of the testing situation, especially test cues and test instructions, but a mental set is ultimately a private affair. Subjects may never instantiate test instructions as required or they may switch their mental set during the course of a test.

Consider how the mental set used to initiate and guide performance can shift in the course of testing. For the word stem completion test, an implicit memory test, subjects are presented with the first three letters of each target word as cues (e.g., FOR___) and the instructions require them to complete each stem with the first word that comes to mind (e.g., FOREST). However, while working through the test, subjects may realize that they previously studied some of the completed words and, following this realization, they may change their strategy and engage in intentional recollection to boost performance (see Bowers & Schacter, 1990; Schacter, Bowers, & Booker, 1989). Similarly, on the closely related cued recall test, an explicit memory test, subjects may be given exactly the same cues (e.g., FOR___) but with instructions that require them to complete the stems with previously studied words (cf. Graf & Mandler, 1984). When subjects cannot recollect previously studied words, they may switch their strategy and start guessing in an attempt to improve their performance.

Susceptibility of memory tests to strategy shifts

Are some tests more liable to strategy shifts than others? We can identify at least some aspects of memory tests that make strategy shifts more or less likely. Tests seems to be more liable to strategy shifts when the test situation (cues and test task) present

subjects with an unstructured and unfamiliar situation, for example, when subjects are given scrambled letter strings and asked to make English words out of them (anagram solution test). Subjects who experience difficulties on this test may look for alternative strategies, especially if they were given no or only little opportunity to practice such a novel task and if they are given plenty of time to think of alternative strategies. Shifts in strategy may also be more likely when the test requirements severely limit the search set size, that is, the number of alternative responses in the specified domain. Search set size is large for a word stem completion test with single letter cues (e.g., F___), it is more restricted for a word stem completion test with three letter cues (e.g., FOR___), and it is severely restricted for a word fragment completion test with word fragments (e.g., F_R_S_) that usually allow only one valid response (for full discussion see Graf & Birt, in press; Roediger & McDermott, 1993).

To illustrate the influence of these characteristics on the susceptibility of various tests to strategy shifts, consider object naming vs. word fragment completion tests as examples of tests that are least and most susceptible to strategy shifts, respectively. In the object naming test, subjects are presented with displays of familiar items -- pictures of objects -- as test cues and required to perform a familiar, well practiced task -- to name each displayed object. For this test, a viable strategy for performing the task is specified by the instructions and it seems unlikely that subjects would search for alternative strategies such as trying to recollect previously studied items in order to name each picture. In contrast, word fragment completion may be more susceptible to shifts in

mental states. For this test, subjects are presented with word fragments (e.g., _E_E_R__) and asked to complete them with any English words that come to mind. Both the cues and task are unfamiliar. The search set size is severely restricted because only one as opposed to many solutions typically exists for each fragment, thereby making completion very difficult. Finally, there is no clear, experimenter supplied strategy that subjects may follow to complete the fragments. Consequently, if no words come to mind immediately, some subjects may be puzzled as to which strategy would allow them to complete the word fragments. Because they have a quite a lot of time, up to 20 seconds per a fragment, they may try to discover alternative strategies to complete the fragments, including intentionally trying to recollect previously studied items and to use these to complete the fragments.

In light of the possibility of these and similar strategy shifts, a number of methods and approaches have been developed to deal with them. These methods can be divided into two broad classes. The first class consists of methods built into the design of an experiment to induce a proper mental set and to minimize the possibility of mental set shifts. The second class includes inferential methods that are applied at the conclusion of the experiment to increase one's confidence in the implicit nature of a test.

Dealing with strategy shifts: Methods built into the design

A wide variety of methods can be employed to induce a proper mental set and to prevent mental set shifts during the experiment. To induce a proper mental set, researchers arrange events and distractor tasks so as to disguise that memory for

previously studied items will be tested. For example, they present the implicit memory test as a filler task between study and memory testing (Graf & Mandler, 1984; Schacter & Graf, 1986), or the implicit memory test is embedded within a study session by repeating some items. In addition, various cover tasks can be used for inducing an appropriate mental set. Schacter and Graf (1986) asked subjects to generate names of cities in response to single letter cues and then administered a word stem completion test disguised as a second filler task.

Other methods are designed to prevent mental set shifts during the course of the test. Researchers may include a large number of items on the test (100 or more) and keep the ratio of old to new items very low. Alternatively, they may use speeded tests, or they may employ test tasks that make use of alternative strategies superfluous, for example, by requiring subjects to rate target items along various dimensions on the implicit memory test (e.g., Durso & Johnson, 1979; Feustel et al., 1983; Schacter, Cooper, & Delaney, 1990). This last strategy was also used in my Experiment 5.

Opposition procedure. An entirely different approach for controlling the measurement problem has been advocated by Jacoby and his colleagues (Jacoby, 1991; Jacoby, Lindsay, & Toth, 1992). Jacoby introduced a new method, called *the process dissociation procedure* or *the opposition procedure*, and argued that this method avoids the measurement problem inherent in implicit memory testing. It has been pointed out elsewhere, however, that the opposition procedure focuses on memory with and without awareness rather than on intentional versus nonintentional recollection (Graf & Komatsu,

1994). Consequently, the opposition procedure is not directly relevant to research on implicit and explicit memory.

Dealing with strategy shifts: Inferential methods

A number of inferential methods have been developed for increasing confidence that performance on an implicit memory test was initiated and guided primarily in the absence of intentional retrieval strategies. These methods are applied at the conclusion of the experiment. The most widely used methods of this type are functional dissociations, stochastic independence, and questionnaires.

Functional dissociations. One way to increase confidence that subjects adhered to the instructions is to show that performance on an implicit test can be dissociated from performance on an explicit memory test, thereby suggesting that the two tests depend, at least in part, on different components or processes. The logic of this method is as follows: If an implicit memory test depends, at least in part, on a different set of processes than an explicit memory test, then it should be possible to dissociate performance on these two tests by manipulating a variable which selectively influences the processes required for performance on one but not on the other test. For a concrete illustration, consider a study by Graf and Mandler (1984) who manipulated levels of processing at study, and then tested memory with an explicit cued recall test and with an implicit word completion test. The results showed higher performance on the cued recall test following the semantic vs. nonsemantic study task, whereas performance on the word stem completion test was not influenced by the nature of the study task, thereby demonstrating

a dissociation between performance on the two tests. Since semantic/nonsemantic study task did not influence performance on the implicit word completion test, Graf and Mandler inferred that performance on this test was initiated and guided primarily in the absence of intentional recollection.

Different kinds of dissociations can occur between performance on two tasks. A single dissociation occurs when a variable selectively affects performance on one task but not on the second task. A double dissociation occurs when performance on two tasks is influenced by manipulations of different variables. A special case of a double dissociation, a cross-over double dissociation, occurs when the manipulation of a single variable increases performance on one test but reduces performance on the second test in such a way that performance levels on the two tasks reverse as a result of the manipulation.

Different kinds of dissociations are more or less informative; they provide various degrees of support for an inference that performance on two tests depends on different component processes (Dunn & Kirsner, 1988; Shallice, 1988; Shimamura, 1993). One problem with interpreting dissociations stems from the fact that they may arise as a by-product of floor or ceiling effects. A second interpretive problem comes from the fact that two tests may be differentially sensitive to a common process and thereby give rise to a dissociation. To illustrate, a single dissociation -- an independent variable influencing performance on Test 1 but not on Test 2 -- may arise from the lack of sensitivity of Test 2 to a manipulation of the independent variable that nevertheless influences the processes

mediating Test 2 performance. For these and similar reasons (see Dunn & Kirsner, 1988), the demonstration of functional dissociations alone does not necessarily demand the conclusion that two tests rely, at least in part, on different component processes. Rather, the demonstration of functional dissociations merely makes such an interpretation more plausible.

To deal with the inferential limits of single and double dissociations, Dunn and Kirsner (1988) proposed a new way of looking at performance dissociations, called the method of reversed associations. This method relies on the fact that if performance is a monotonic (i.e., either non-increasing or non-decreasing) function of a common process, performance on two tasks will also be monotonically related. Therefore, when performance on one task is plotted as a function of performance on the second task, the resulting function should be either monotonically increasing or monotonically decreasing. If the plot shows any reversals (i.e., changes from increasing to decreasing function), this suggests that performance on the two tasks must be mediated, at least in part, by distinct processes.

Stochastic independence. Another strategy to increase one's confidence in the validity of memory tests is based on correlational analysis of performance on explicit and implicit memory tests. If the set of processes required for performance on implicit and explicit memory tests are entirely different, the correlation between performance on the two tests should be zero. Thus, a finding of stochastic independence could be used to argue that performance on implicit and explicit memory tests is mediated by different

processes. Tulving et al. (1982) tested subjects' memory with an old/new recognition test followed by a word fragment completion test, and they found a zero correlation between recognition and priming, that is, subjects showed equal amounts of priming for the items that they did vs. did not recognize on the preceding recognition test. Based on this finding, Tulving et al. (1982) argued that performance on the two tests is mediated by different mechanisms, one mediating intentional memory and another mediating priming (see also Jacoby & Witherspoon, 1982).

The problem with this method is that a lack of correlation between performance on two tests may arise for a number of different reasons, thereby limiting the explanatory power of the correlations. First, computing correlations requires the use of two tests, and they may appear stochastically independent due to the additional priming that occurs when targets are re-encountered on the second test, thereby confounding performance (Shimamura, 1985). Second, a significant correlation between the tests may fail to emerge because priming scores are generally low, restricted in their range by floor effects (Ostergaard, 1992). Third, stochastic independence may arise as an artifact due to low reliability of scores (either priming or recognition scores, or both). The reliability of test scores places an upper limit on any possible correlation between two measures.

Equally problematic is any attempt to draw conclusions from nonzero correlations between the two tests. A nonzero correlation does not necessarily imply that tests are invalid measures of implicit and explicit memory, respectively. Such a correlation merely suggests that performance on the two tests was influenced by some common processing

components. For another closely related correlation-based statistical method -- the method of triangulation -- see Hayman and Tulving (1989).

Questionnaires. A more direct way to gain insight into the mental state that guides subjects' performance is to query them after the test about the strategies they used (see Bowers & Schacter, 1990; Richardson-Klavehn, Lee, Joubran, & Bjork, 1994). For example, Richardson-Klavehn et al. (1994) assessed implicit memory by means of a word identification test and then they queried subjects about the strategies they had used on the test. They found that 6.3% of the subjects were unaware that the test included studied items, 15.6% suspected prior to the study that some of the targets would appear in the test, 78.1% realized during the test that some of the items were previously studied, and 40.6% said that they attempted to intentionally recollect previously studied targets to improve their performance. These results indicate that many subjects may realize at some point that some of the test items were previously studied, yet only some of them believe that they used this knowledge to engage in intentional remembering.

The results gathered by means of such questionnaires must be interpreted with caution, however. First, questionnaires have all the problems generally associated with any self-report measures, including inaccurate memory for queried events and non-compliance with the experimenter's demands. Second, subjects may not have sufficient insight into their mental states, and in post-experimental questioning, they may interpret awareness that some test items appeared in the study list as an indication that they intentionally retrieved them. Third, it is next to impossible to find out at which stage

during the course of an experiment subjects started to use intentional retrieval strategies, whether they persisted in their use until the end of the experiment, and whether such strategies were used with all or only some items. Fourth, if higher test performance increases awareness of the study/test relation and use of intentional strategies (reasonable assumptions for tests such as word identification), any post-hoc analysis based on the questionnaires will be necessarily confounded with the overall level of test performance.

Summary and implications

Researchers have used a wide variety of implicit and explicit memory tests, and they have arranged these tests in various ways to gain insight into performance dissociations between them. The proposed classification systems are preliminary, however, and they offer only limited insights into differences between implicit and explicit memory tests. The fact that mental sets initiating and guiding test performance are under the subjects' rather than the experimenter's control causes a measurement problem concerning the validity of implicit and explicit memory tests. For this reason experimenters try to induce a proper mental set by various means, including type of test cues and clear, direct instructions. Memory tests differ in terms of their liability to retrieval strategy shifts, and various design methods can be used to minimize shifts. In addition, investigators may also use various inferential methods to increase their confidence that test performance was initiated and guided by test-appropriate retrieval strategies. However, none of these methods allows the conclusions that performance was 'pure' in a sense that it was guided exclusively by the appropriate retrieval strategy. What

is more important is that an awareness of methods for shaping subjects' retrieval sets is of immediate practical value; it permits a more critical evaluation of the findings from previous experiments. An experiment that used a number of methods for minimizing the measurement problem is more trustworthy.

To minimize the measurement problem in my research, I also used a number of these methods. In my experiments, the test materials were familiar items -- color photographs of common objects. In the first four experiments, the implicit test required subjects to execute a familiar task -- to identify each of the photographed objects as quickly as possible. In the last experiment, the implicit test required subjects to rate the size of depicted target objects relative to one another, and this task can be accomplished without resorting to more effortful intentional retrieval of previously studied targets. The cover story in my work presented the implicit memory tests as perception tasks, and in addition, in the last experiment, the implicit test was disguised as a continuation of the study task. To make the use of explicit strategies still more unlikely, each implicit memory test had a large number of target items (over 100) and each included an equal number of old (i.e., previously studied) and new (i.e., nonstudied) items. Finally, most of my experiments included an explicit old/new recognition test, thereby allowing me also to use some inferential methods for increasing the confidence in the validity of my tests.

Empirical research findings

This section identifies main trends in existing experimental research findings on the nature of implicit vs. explicit memory. The section is divided into five subsections.

The first reviews research showing that implicit memory is somehow special because it can function normally in various subject groups -- amnesic patients, children and elderly adults -- that show less than optimal performance on explicit memory tests. The remaining four subsections review research that investigated how implicit memory differs from explicit memory. First, is priming short lived or long lasting? Is it longer lasting than explicit memory? Second, in contrast to explicit memory, is priming influenced similarly or differently by manipulations of study instructions? Third, can priming occur for new materials (e.g., pseudowords, novel objects) that have not been encountered before, that is, that do not have preexisting representations in memory? Fourth, is priming specific to particular sensory and perceptual features of to-be-remembered (TBR) targets? Is it more specific to sensory/perceptual cues than explicit memory?

Subject group effects

Research on the nature of implicit memory began with the finding that patients with anterograde amnesia can perform as well as normals in a variety of situations that are today called implicit memory tasks (e.g., Warrington & Weiskrantz, 1968, 1970). This suggested that implicit memory is somehow special, mediated by different neurocognitive mechanisms than those mediating explicit memory. Equally important, other findings showed that, in contrast to performance on explicit memory tests, performance on implicit memory tests is only minimally influenced by development in childhood and by aging (for reviews see Graf, 1990; La Voie & Light, 1994; Mitchell, 1993; Naito & Komatsu, 1993; Parkin, 1993), thereby providing additional evidence that

implicit memory can function independently of explicit memory. This section reviews research showing that implicit memory is spared in amnesic patients and that implicit memory seems completely functional even in young children, and remains intact in elderly adults despite their sub-normal performance on explicit memory tests.

Amnesic patients. In a series of landmark papers, Warrington and Weiskrantz (1968, 1970, 1974) showed that despite their inability to recollect recently learned new information, amnesic patients can show some form of memory for the information learned previously in a single study trial. Warrington and Weiskrantz (1970, 1974) presented amnesic patients with the first three letters of previously studied words and asked them to complete the stems with English words. Although the patients could not recollect the previously studied words on conventional memory tests such as recall and recognition, they completed the stems using the previously studied words with greater likelihood than would be expected in the absence of prior study.

Graf and Mandler (1984) showed that whether subjects' performance is impaired on various memory tests depends critically on test instructions. Subjects were required to study a list of words, and they were asked to either rate how much they liked each word (semantic task) or to count the number of enclosures and T-junctions in each word (non-semantic task). For testing, all subjects were presented with the first three letters of studied words and they were given either completion or cued recall instructions. The completion test instructions asked subjects to complete each cue with *the first word that comes to mind*, whereas the cued recall instructions asked subjects to use the cues to help

recall the words from the presented list. The results showed that completion test performance was comparable in the semantic (23.3%) and non-semantic study groups (20%), whereas cued recall performance was greatly affected by semantic vs. non-semantic study manipulations (37.2% and 20.6%, respectively).

Graf, Squire, and Mandler (1984) presented both amnesic and normal control subjects with a list of words for study, and they instructed subjects either to rate their liking of each word (semantic study task) or to decide whether two successive words on the study list shared any of their vowels (non-semantic study task). Implicit memory was tested with a word stem completion test and explicit memory was assessed with a free recall test. The study showed that, compared to control subjects, amnesic patients performed poorly on the free recall test (11.9% vs. 26.6%), whereas their performance on the completion test was comparable to that of controls (49.2% vs. 43.1%), regardless of the study task. In another experiment, Graf et al. (1984) tested amnesic and control subjects with cued recall instructions and found that control and amnesic subjects performed similarly following the non-semantic study task (39.9% vs. 39.6%), but controls outperformed amnesics following the semantic study task (69.0% vs. 57.7%).

In combination, these and similar findings (for recent reviews see Cohen & Eichenbaum, 1993; Moscovitch, Goshen-Gottstein, & Vriezen, 1994; Moscovitch et al., 1993; Squire, 1992) demonstrate that amnesic patients who are characteristically impaired on explicit memory tests such as recall, cued recall, and old/new recognition can perform as well as normal subjects on a variety of implicit memory tests including word

identification, word stem completion, word fragment completion, free association, and category instance production. This pattern of findings indicates that implicit memory is mediated by different cognitive processes or mechanisms than those mediating explicit memory test performance.

Children, adults, and the elderly. Explicit memory develops in childhood, peaks in early adulthood, and then declines in late adulthood (Kail, 1990; Kausler, 1991; Salthouse, 1982). In contrast to explicit remembering which shows large changes across the life-span, the findings from studies on lifespan changes in implicit memory suggest that development and aging have only minimal or no effects on performance on a variety of implicit memory tests (for recent reviews see Graf, 1990; La Voie & Light, 1994; Mitchell, 1993; Parkin, 1993).

Greenbaum and Graf (1989) presented 3-, 4-, and 5-year olds with pictures of common objects and then assessed their implicit and explicit memory with a word-production test and a recall test, respectively. For the word-production test, children were told a story about going some place such as zoo and then were asked to name what is typically found there. In this paradigm, implicit memory or priming is demonstrated when children produce names of previously studied animals more often than in the absence of prior study. The results showed that priming was comparable across the age groups, but that the older children recalled more animals than the younger children.

Parkin and Streete (1988) studied implicit and explicit memory in groups of 3-, 5-, 7-, and 20-years olds using a picture fragment identification test and an old/new

recognition test. For the picture identification test, subjects were shown line-drawings of familiar objects that were degraded to various degrees by deleting a proportion of their line segments. On each trial, the subjects were presented with the most degraded version of each line-drawing first and were then asked to identify the object. If they could not identify the object, they were shown a less degraded version of the same picture, and this procedure was repeated until the object was correctly identified. The results showed that previously studied objects could be identified from more fragmented versions (i.e., with less picture information) than objects seen for the first time, but that the amount of improvement in identification (due to prior study) was similar across the age groups. In contrast, performance on an old/new recognition test improved with age. Other researchers have extended these findings to different implicit memory tests, and also showed that there are only minimal or no developmental changes in priming on word completion (Naito, 1990), and on picture naming (Carroll, Byrne, & Kirsner, 1985).

The research also indicates that priming shows only minimal or no changes with aging. To illustrate, Mitchell, Brown, and Murphy (1990) investigated implicit and explicit memory in young adults (18 to 34 years of age) and in older adults (57 to 83 years of age) using picture naming and old/new recognition tests, respectively. The results showed that naming times were faster for previously named pictures than for newly presented pictures, thereby demonstrating priming. More importantly, the magnitude of priming was comparable in young and older adults. In contrast, old/new recognition test performance was significantly lower for older adults than for young adults.

In a recent meta-analysis, La Voie and Light (1994) compared the magnitude of priming on verbal implicit memory tests for young and older adults. They reviewed all studies that included at least one group of young adults and one group of older adults (60 years of age and over). The implicit memory tests included spelling bias, word stem completion, word fragment completion, word identification, word naming, as well as tests for newly acquired associations between previously unrelated word pairs. Based on an analysis of 39 effect sizes, La Voie and Light concluded that performance on verbal implicit memory tests declined with age but that the size of this decline is small, relative to age-related declines in performance on explicit memory tests such as recognition and recall.

In another meta-analysis, Mitchell (1993) reviewed research on lifespan changes in performance on implicit and explicit memory tests that used pictorial as opposed to verbal materials. The review included 5 developmental studies investigating implicit and explicit memory in children and 4 studies investigating implicit and explicit memory in late adulthood. The implicit tests included picture naming, picture fragment completion, and object decision. The results of the meta-analysis indicated that age-related differences in implicit memory test performance were small and not statistically significant. In contrast, performance on the explicit memory tests showed the large typical age-related differences.

The studies reviewed here and elsewhere (Graf, 1990; La Voie & Light, 1994; Mitchell, 1993; Parkin, 1993) allow the following generalization: Young children whose

explicit memory has not yet fully developed, and older adults whose explicit memory has declined with aging, can show normal performance on at least some implicit memory tests or they show only minimal age-changes. These findings lend support to the notion that implicit memory is mediated by different processes or mechanisms than explicit memory.

Study/test delay effects

One of the early explanations of priming effects was that priming is mediated by automatic activation of preexisting representations in long-term memory (e.g., Diamond & Rozin, 1984; Morton, 1969). This activation process was likened to the 'hot' tubes in the old radio that remain warm for some time after the radio itself has been turned off (cf. Diamond & Rozin, 1984). Thus, one of the reasons for investigating the time course of priming was to find out whether priming is indeed short lived as suggested by the 'hot tubes' analogy. Another reason for investigating the time course of priming was to find out whether it declines at the same rate or at a different rate than performance on explicit memory tests. The following paragraphs review previous research showing that priming can be much longer long-lasting than suggested by the 'hot tubes' analogy, but that its time course is variable, and that it may decline at a slower rate than explicit memory test performance.

Early evidence indicated that priming on word stem completion returns to baseline level within about 2 hours (e.g., Graf & Mandler, 1984; Graf et al., 1984; Rozin, 1976; Squire, Shimamura, & Graf, 1987; but see Squire et al., 1987), whereas priming on other

implicit memory tests, such as word fragment completion and word identification, persists for at least 1 week (Jacoby & Dallas, 1981; Komatsu & Ohta, 1984; Light, Singh, & Capps, 1986; Roediger & Blaxton, 1987a; Tulving et al., 1982) or longer (Sloman, Hayman, Ohta, Law, & Tulving, 1988). These findings indicate that the time course of priming is variable, possibly dependent on the particular test that is used to measure it.

To explore this hypothesis, Roediger et al. (1992) compared the time course of priming on word stem and word fragment completion tests within a single experiment. Subjects studied a list of words and were tested immediately, and then again either after 90 min. or after a 48-hour delay. Implicit memory was assessed with word fragment completion, word stem completion, or anagram solution tests. Word-fragments were selected to have mostly only one solution, whereas word stems had at least 4 solutions (on the average more than 10 solutions). Despite these differences, priming on word stem and word fragment completion tests showed the same pattern of decline across test delays: Priming was equally large on immediate and 2-hours delay tests, and it declined significantly between the 2-hour delay and 48-hours delay tests. In another experiment, Roediger et al. (1992) confirmed that priming on both kinds of tests declined from 1.5, to 2, to 48, and to 168 hours. Importantly, however, significant priming was observed in all conditions, even after a delay of 168 hours (7 days). These results indicate that long lasting priming can be observed on both word-stem and word-fragment completion tests. Long lasting priming has also been found with a variety of other implicit memory tests

(e.g., Moscovitch, Winocur, & McLachlan, 1986; Scarborough, Cortese, & Scarborough, 1977; Weiskrantz & Warrington, 1970; for a recent review see Roediger & McDermott, 1993).

A number of studies suggest that priming declines at a slower rate than performance on explicit memory tests, but this inference is burdened by serious interpretive problems including the fact that it involves a comparison between different measures. Tulving et al. (1982) investigated the time course of priming using a word-fragment completion test by testing subjects either a few minutes after study or after a one week delay. Although explicit memory measured by old/new recognition declined substantially over this retention interval, priming did not decline significantly over the same interval (although it did decline in terms of absolute values from .17 to .14, or down to about 82% of immediate priming).

Mitchell and Brown (1988) dissociated priming from performance on an explicit memory test for non-verbal materials, line-drawings of common objects. For implicit memory testing, subjects were shown 50 previously studied and 50 new pictures and they were again required to name them as quickly as possible. For explicit memory testing, subjects were shown the same 50 studied and 50 nonstudied pictures, but they were required to decide whether each picture had been presented at study. The results showed that priming did not change between a 1- and 6-week delay; the magnitude of priming was 87, 72, and 83 ms after a 1-, a 4-, and 6-week delay respectively (but see Carroll et

al., 1985; Lachman & Lachman , 1980). In contrast, recognition declined (in terms of hits-minus-false alarms scores) from .50 to about .23 across the same 6-week interval.

These findings show that priming can be long lasting, can be quite variable, and is often longer lasting than explicit memory. Long lasting priming suggests, first, that a simple activation view such as the one suggested by the 'hot tubes' analogy can be excluded as a viable explanation of priming. Second, it suggest that priming reflects the influence of newly established, episode-specific representations rather then temporary modification of pre-existing representations. Variability in priming has been attributed to a number of factors, including type of materials, their frequency or familiarity, test cues, and the number of completions (e.g., Graf & Mandler, 1984; Sloman et al., 1988). Longer lasting priming than explicit memory suggests that priming is mediated by different processes or mechanisms than explicit memory.

Study instructions effects

A lot of research has focused on the influence of various study task manipulations on performance of implicit and explicit memory tests. The majority of these experiments investigated the effects of semantic vs. nonsemantic study tasks (levels of processing effects, cf. Craik & Lockhart, 1972), and the effects of generating vs. reading words at study (generation effects, cf. Slamecka & Graf, 1978). The idea behind these investigations was to see whether, in contrast to explicit memory, priming is automatic, independent of subject controlled processing of to-be-remembered materials.

Levels of processing (LOP) manipulations effects. Perhaps the most popular study trial manipulation has been comparison between semantic vs. nonsemantic processing. To illustrate, Graf and Mandler (1984; Experiment 3) required subjects to study a list of words under either semantic (i.e., rating whether they liked or disliked each word) or non-semantic (i.e., counting number of enclosures and T-junctions in each word) conditions. For testing, subjects were given the first three letters of studied words as cues and were instructed either to complete the stems with the first word that comes to mind (implicit, word-stem completion test), or to use the cues to recall the words from the presented list (explicit, cued recall test). The results showed that there was only a minimal, non-significant effect due to semantic/non-semantic study task manipulation on the word stem completion test (23.3% vs. 20.0%) and, consistent with previous research, that the LOP manipulation had a large effect on the cued recall test (37.2% vs. 20.6%). This and similar findings (e.g., Graf et al., 1982; Jacoby & Dallas, 1981) indicate that, in contrast to explicit memory, priming occurs independently of semantic vs. nonsemantic study trial processing. More generally, it appears that priming occurs as an automatic consequence of encountering targets at prior study.

The results of several recent meta-analyses (Brown & Mitchell, 1994; Challis & Brodbeck, 1992; Chiarello & Hoyer, 1988) indicate, however, that priming is influenced to some degree by levels of processing manipulations. The most extensive meta-analysis is by Brown and Mitchell (1994); it included results from 38 articles of LOP effects on performance of implicit memory tests. The 38 articles resulted in 166 outcomes that

compared performance on implicit tests following semantic vs. nonsemantic study trial processing. The results showed that priming was greater following semantic than nonsemantic study trial processing in 131 out of 166 outcomes, thereby suggesting that priming is not entirely automatic.

The automaticity of priming may not be all-or-none: It may be variable and related to target items' familiarity, and this may explain the finding of small LOP effects in priming. It is possible that priming for unfamiliar items benefits from extra processing conferred by semantic study tasks. Some evidence that is consistent with this possibility is discussed in more detail in the section on novelty effects.

Generation effects. One of the widespread assumption in the literature is that performance on implicit memory tests and priming is dependent primarily on the data, on processing sensory and perceptual features of targets. By this view, priming should be reduced following study tasks that prevent or minimize processing of targets' sensory and perceptual features. With this goal in mind, Jacoby (1983) investigated the influence of reading vs. generating words as responses to cues. At study, subjects read a word without context (e.g., short), read the word in the context of an antonym (e.g., tall-short), or generated the word as an antonym of a context word (e.g., tall-??). This last condition minimizes processing of targets' sensory and perceptual features because the targets themselves are never seen (although subjects may try to imagine what they would look like). Jacoby found higher old/new recognition performance for generated words (.78) than for read words (.72), replicating the familiar generation effect. In contrast,

identification performance showed more priming in the read (.16) than generate condition (.07). Thus, generating vs. reading words at study had an opposite influence on explicit and implicit memory test performance. Such findings support the view that in contrast to explicit memory, priming is more or less an automatic consequence of processing targets at study, and that it reflects primarily the processing of targets' sensory and perceptual features.

Novelty effects

One possible explanation for priming phenomena is that they reflect modification of target items' preexisting representations in memory. To examine this possibility, researchers have investigated whether priming is limited to target items that have preexisting representations in memory or whether it can also occur for new materials (e.g., pseudowords, new associations between unrelated word pairs, novel objects) that do not have any preexisting representations in memory.

A closely related issue is whether priming is an automatic consequence of encountering targets. If, as suggested earlier, priming is more or less automatic, depending on the amount of practice with targets, then we would expect that priming for new materials may not occur automatically. Instead, priming for new materials may require subject-controlled processing which leads to the constructions of new episodic representations.

Frequency of occurrence effects. One way to examine whether priming is related to familiarity (i.e., related to the amount of practice with specific targets) is to look at

priming for high vs. low frequency words. Previous research has demonstrated that high frequency (HF) words (e.g., apple) are identified faster than low frequency (LF) words (e.g., aadwark) (e.g., Jacoby & Dallas, 1981). By contrast, the magnitude of priming is inversely related to baseline identification speed. Jacoby and Dallas (1981) required subjects to identify briefly displayed high and low frequency words. When seen for the first time (in the baseline condition), high frequency (more than 50 occurrences per million) words were identified with a greater accuracy (65%) than low frequency (1 to 5 occurrences per million) words (34%),. The opposite was true for priming; priming was lower (18%) for HF words than for LF words (34%) (see also Komatsu, Graf, & Uttl, 1995; MacLeod, 1989; Roediger et al., 1992). Similarly, Oldfield and Wingfield (1965) showed that naming latencies of line-drawings of familiar objects was related to word frequency counts for their names, but that priming was larger for unfamiliar than familiar pictures (see also Kirsner, Milech, & Stumpfel, 1986).

This pattern of findings points to a positive relation between targets' familiarity and subjects' skill at processing targets, and to an inverse relation between familiarity and the magnitude of priming. This latter relation between priming and familiarity may result either from a modification of targets' preexisting representations or from the relative ease of establishing new representations at the time of study. To find out whether priming can in fact be mediated by newly established representations as opposed to modifications of preexisting representations, researchers have explored priming for new materials such as pseudowords (e.g., Bowers, 1994; Feustel et al., 1983; Haist, Musen, & Squire, 1991;

Musen & Squire, 1991; Salasoo, Shiffrin, & Feustel, 1985), new associations (e.g., Graf and Schacter, 1985, 1987, 1989; Schacter & Graf, 1986, 1989), and novel objects (e.g., Cooper & Schacter, 1992; Schacter & Cooper, 1993; Schacter et al., 1990; Schacter, Cooper, Delaney, Peterson, & Tharan, 1991; Cooper, Schacter, Ballesteros, & Moore, 1992).

Pseudowords. A number of investigations examined whether priming can be observed for novel letter strings -- pseudowords (i.e., pronounceable letter strings like *numdy*). Feustel et al. (1983; see also Bowers, 1994; Haist et al., 1991; Salasoo et al., 1985; Squire, 1991) presented familiar words or unfamiliar pseudowords on an oscilloscope, with display duration ranging from 24 ms to 68 ms, and the subjects' task was to identify and name these items. In a series of experiments, Feustel et al. demonstrated that words were identified both faster and more accurately than non-words during the first presentation, and more importantly, that both words and non-words were identified faster and more accurately on the second and subsequent presentations. To illustrate, in their Experiment 2, threshold identification time for nonstudied words was 50 ms whereas it was 57.5 ms for non-words, and both words and non-words were identified approximately 5 ms faster on the second presentation. Feustel et al. interpreted the faster identification times on nonstudied words as opposed to nonwords as showing that words but not nonwords have preexisting representations in memory that are automatically recruited for identification test performance. The findings of faster identification times for words and nonwords that had been in the study list indicates that

priming cannot depend entirely on automatic activation of preexisting representations. Rather, these priming effects must reflect the contribution of new episodic representations that were established at study.

New associations. In a series of studies, Graf and Schacter (1985, 1987, 1989; Schacter & Graf, 1986, 1989) investigated whether priming can be observed for newly acquired associations between preexperimentally unrelated word pairs. To illustrate, subjects were required to study unrelated context-target word pairs (e.g., WINDOW-REASON, OFFICER-??), and for a subsequent test, they were presented with the first three letters of each target word accompanied either by the same context word as at study (e.g., WINDOW-REASON) or by a context word that had been studied with a different target (e.g., OFFICER-REASON). The implicit memory test instructions required subjects to complete word stem cues with the first words to come to mind, whereas the explicit memory test instructions required subjects to complete the same word stems with the previously studied targets. In a number of experiments, Graf and Schacter found greater priming when targets were tested with the same rather than a different context. Because both words of each pair were previously studied, the advantage of intact over recombined pairs demonstrated priming for newly acquired associations. In contrast to the findings from studies that investigated priming for target items presented alone, Graf and Schacter obtained priming for new associations only when using a study task that required at least some degree of elaborative processing of paired words. They found evidence of contextual priming when the study task required subjects to create a sentence

connecting two paired words together in a meaningful way, but no such priming effects were found with tasks that required subjects, for example, to rate each word separately for pleasantness (Graf & Schacter, 1985; Schacter & Graf, 1986; but see Micco & Masson, 1991).

The finding that priming for new associations depends on elaborative processing suggests that the phenomenon may reflect subjects' use of intentional recollection strategies. This possibility, however, is countered by a number of demonstrations showing that priming for new associations on stem completion tests and performance on cued recall tests (i.e., the same cues but different instructions) can be dissociated by a number of different variables. First, in contrast to cued recall test performance, priming for new associations was unaffected by the degree and type of elaborative study trial processing activities. For example, subjects who were required to rate whether an experimenter-supplied sentence meaningfully connects each context-target pair showed a comparable level of priming to subjects who were required to generate a sentence that meaningfully connects the context-target pair. In contrast, performance on the cued recall test was higher following the generate than the rate condition (Schacter & Graf, 1986). Second, neither proactive nor retroactive interference manipulations influenced the magnitude of priming for new associations, despite having large effects on cued recall test performance (Graf & Schacter, 1987). Third, study-to-test changes in modality of presentation substantially reduced priming for new associations whereas they had no effect on cued recall test performance (Schacter & Graf, 1989). If priming for new

associations is mediated by explicit memory strategies (intentional retrieval), then such dissociations should not occur, especially not under the conditions where the same cues are used for the implicit and explicit tests.

Novel objects. Schacter and his colleagues (Cooper & Schacter, 1992; Schacter & Cooper, 1993; Schacter, Cooper, & Delaney, 1990; Schacter et al., 1991; Cooper et al., 1992) investigated implicit and explicit memory for line-drawings of two kinds of objects: structurally *possible* and structurally *impossible* novel objects. For possible objects, surfaces and edges were drawn in such a way that made it possible for these objects to exist in a three dimensional world, whereas for impossible objects, surfaces and edges were drawn in such a way that, similarly to various Escher-like drawings, these objects could not exist in a three dimensional world. During the study phase, subjects were shown both possible and impossible objects and they were required to make some kind of judgment about each. In the structural encoding condition, subjects decided whether each object faced to the left or to the right. In the local encoding condition, subjects guessed whether each object contained more vertical lines or more horizontal lines. Finally, in the elaborative encoding condition, subjects were asked to generate the name of a real object that seemed most like each line-drawing. Following study, subjects received either an implicit or an explicit memory test. For the implicit test, they were given brief exposures to the previously studied and non-studied objects, and they were required to classify each object as possible or impossible. For the explicit memory test, subjects were required to decide whether they had seen each object at study.

The following findings are critical: First, there was significant priming for novel objects but only for those that were physically possible. In contrast, recognition was above chance for both possible and impossible objects. Second, priming was found only following the task that required processing of the global aspects of each object. Neither tasks that required processing of selective object features (i.e., counting lines) nor tasks that required generating names of similar objects resulted in significant priming effects. In contrast, recognition performance was higher following generating names than following processing of global aspects or processing of local features. One interpretation of these findings is that priming for novel objects occurs only when a study task leads to the construction of new study trial representations for novel objects, and that construction of such representations was difficult when objects were impossible and when study tasks did not require processing of each object as a coherent unit.

Summary. The magnitude of priming is related to the amount of practice with identifying familiar targets such as words or pictures; priming is larger for low frequency targets than high frequency targets. Priming can occur for novel materials such as pseudowords, new associations, and novel objects, that do not have preexisting representations in memory. However, priming for new materials occurs only following study tasks that require processing of each new target as a coherent unit. Priming for novel materials (e.g., new associations) is, however, not affected by levels of processing manipulations, or by interference manipulations. In contrast, performance on a free recall test, an explicit memory test, is often higher for high frequency rather than low frequency

targets (e.g., Gregg, 1976), and it improves depending on the degree of levels of processing manipulations.

Sensory and perceptual features effects

A widespread generalization is that implicit memory and priming are specific to particular sensory and perceptual features of to-be-remembered materials, and that study-to-test changes in these features reduce priming effects. In contrast, explicit memory is insensitive to such changes; it is not specific to sensory and perceptual features. This general assumption about the specificity of processing has come from studies that investigated the influence of modality (e.g., visual vs. auditory) and symbolic format (e.g., words vs. pictures, different languages in bilinguals) manipulations on implicit and explicit memory test performance.

These claims, however, have been questioned by many contradictory findings from studies that focused on manipulations of targets' features within the same modality or within the same symbolic format. In addition, these claims contradict the findings from related work on perception of objects suggesting that sensory and perceptual features of objects (e.g., color, size, orientation) are not important for identifying objects, thereby giving rise to various object constancies (e.g., size constancy). This section presents the most prominent findings from studies that investigated the specificity of priming and explicit memory by manipulating modality, symbolic format, and visual features of words. Related work on perception of objects, especially work that examined

the influence of orientation and size on identification, priming, and explicit memory is reviewed later in Chapters 3 and 4.

Modality effects. One of the most solidly established findings in the literature is that priming is larger for words studied and tested in the same modality (e.g., visual) than for words studied and tested in different modalities (e.g., auditory at study and visual at test), whereas old/new recognition is typically unaffected by such manipulations (e.g., Blaxton, 1989; Challis & Sidhu, 1993; Clarke & Morton, 1983; Jacoby & Dallas, 1981; Rajaram & Roediger, 1993; Scarborough, Gerard, & Cortese, 1979). To illustrate, Graf et al. (1985) required subjects to study a list of words presented either visually (i.e., written on index cards) or auditorily (i.e., spoken by an experimenter), and tested their memory with an explicit free recall test and an implicit word stem completion test. The results showed that study-to-test changes in the modality of study list presentation reduced priming but not performance on a free recall test. Morton (1979; Jackson & Morton, 1984; see also Bassili, Smith, & MacLeod, 1989) extended these findings from visual tests to auditory implicit tests by showing that auditory presentation at study resulted in a greater priming than visual presentation when subjects were required to identify auditorily presented words embedded in white noise. These findings indicate that, in contrast to explicit memory, priming is specific to targets' modality at study.

Symbolic format effects. Others have investigated the specificity of priming further by manipulating targets' symbolic format (e.g., Durgunoglu & Roediger, 1987; Gerard & Scarborough, 1989; Kirsner, Smith, Lockhart, King, & Jain, 1984;

Scarborough, Gerard, & Cortese, 1984; Warren & Morton, 1982; Weldon & Roediger, 1987; Winnick & Daniel, 1970). They showed that performance on the word identification test is typically higher following study of words rather than pictures (e.g., Warren & Morton, 1982; Winnick & Daniel, 1970), thereby suggesting that priming is specific to targets' study trial symbolic format. Weldon and Roediger (1987) expanded these findings to other implicit memory tests: word fragment completion and picture fragment completion. They required subjects to study a series of pictures and words, and then tested their memory with either word fragment completion tests or with picture fragment naming tests. For the word fragment completion test, subjects were shown words with some letters omitted from each word and were told to complete each word. For the picture fragment naming test, subjects were presented with degraded pictures and were told to name each picture with the first word that came to mind. The results showed that performance on the picture fragment completion test was higher following study of pictures than words, whereas performance on the word fragment completion test was higher following study of words rather than pictures.

Weldon et al. (1995) extended these findings by investigating the effect of study-to-test picture/word changes on both implicit and explicit memory tests. Implicit memory was assessed with a word and a picture fragment completion test, and Weldon et al. replicated the finding that priming is specific to study-to-test overlap in materials' symbolic format. Explicit memory was assessed with several tests: word recognition, word fragment cued recall, and picture fragment cued recall. For word fragment and

picture fragment cued recall tests, subjects were presented with the same cues as for the word and picture fragment completion tests, but they were asked to use the fragments as retrieval cues to recollect previously studied words or pictures. The results from explicit memory tests showed that pictures were remembered better than words on the word recognition test and on the picture fragment cued recall test. In contrast, performance on the word fragment cued recall test was comparable for previously studied words and pictures (see also Weldon, Roediger, & Challis, 1989). In combination, these findings suggest that priming is specific to the study-to-test overlap in word/picture format, whereas explicit memory test performance is sometimes affected and other times unaffected by study-to-test overlap in word/picture format.

Researchers have also examined the specificity of priming by changing the language in which words were presented at study and test. The results showed that, when words are presented alone, study-to-test changes in presentation language (e.g., from Chinese to English) reduce priming on a lexical decision test, a word fragment test, and a word stem completion test (e.g., Durgunoglu & Roediger, 1987; Kirsner et al., 1984; Gerard & Scarborough, 1989; Scarborough et al., 1984). In combination, the findings suggest that priming is often specific to modality, picture/word format, and language.

Visual features effects: Word fragments, typographies, and fonts. An even greater degree of specificity is highlighted by studies that manipulated only the visual features (e.g., typography, font, fragments) of target words between study and test. In a series of seminal studies, Paul Kolers (1975, 1976, 1979) and his colleagues (Kolers & Ostry,

1974) investigated whether when subjects read sentences they process and encode surface features as an integral part of the sentences, or whether surface features are discarded and only the meaning is retained (the pearl-in-the-oyster theory, see Kolers, 1979). Kolers required subjects to read sentences or longer passages that were printed in one of several unfamiliar typographies (e.g., mirror-imaged, inverted, upside-down). He retested his subjects after various delays, ranging from a few minutes to a year. He found that re-reading speed was faster for text presented for testing in the same rather than different typography. These findings suggest that reading transformed text demands processing of visual aspects of read materials, and that reinstating the same visual aspects at testing facilitates implicit memory test performance.

Gardiner (1988, 1989) and Hayman and Tulving (1989) found what they called "hyperspecific" priming on a fragment completion test. When exactly the same word fragments (e.g., _SS_SS_N) were used at study and test, priming was larger than when different fragments were used (e.g., A__A__I_ at study and _SS_SS_N at test). Similarly, Masson (1986) showed that words presented in an unfamiliar typography (e.g., alternating case and with letters mirror reversed) were reread faster when the case of each letter was the same at study and test than when the case of each letter changed between study and test (see also Horton & McKenzie, 1995). Jacoby and Hayman (1987) demonstrated that words tested in a normal lowercase font were more likely to be identified when they were studied in the same lowercase font than when they were studied in highly unusual typeface (10 times larger, 3-D like font). Others have found

reductions in priming effects when the case of words was changed between study and test (e.g., upper case to lower case; Roediger & Blaxton, 1987a; Scarborough et al., 1977), and when words were studied and tested in different typographies (hand-printed vs. typed, focused vs. blurred; Kinoshita & Wayland, 1993; Roediger & Blaxton, 1987b). These and other studies further highlight the specificity of priming.

Graf and Ryan (1990) have pointed out, however, that many studies have failed to find feature-specific priming effects even when the visual appearance of printed words or sentences changed substantially between study and testing (e.g., Carr, Brown, & Charalambous, 1989; Clarke & Morton, 1983; Feustel et al., 1983; Graf & Levy, 1984; Graf & Ryan, 1990; Horton, 1989; Jacoby & Hayman, 1987; Roediger & Blaxton, 1987b; Scarborough et al., 1977; Tardif & Craik, 1989). In an early demonstration, Clarke and Morton (1983) required subjects to study words that were either typed or hand-written in a highly stylized form and they found comparable priming effects on a subsequent identification test when words were tested in typed form.

To gain insight into these conflicting findings and to extend the previous studies that did not include separate tests to assess both implicit and explicit memory, Graf and Ryan (1990) investigated the influence of changing typography between study and testing on both implicit and explicit memory tests. In Experiment 1, they showed that words studied either in normal orientation or rotated by 180 degrees produced comparable priming on an identification test regardless of test words' orientation (e.g., normal or rotated). Recognition test performance showed no advantage for words studied and tested

in the same vs. different orientation. In Experiment 2, Graf and Ryan examined the possibility that various display formats direct processing to different aspects of target words, and thus result in different patterns of memory performance. At study, subjects were presented with words displayed either rotated by 180 degrees (U words) or spelled backward (B words). For testing, subjects were presented with U and B words and their memory was assessed with identification and an old/new recognition test. Graf and Ryan found that priming was reduced by study-to-test changes in the display format, but only for words spelled backwards on the test. In contrast, old/new recognition showed an overall advantage for words spelled backwards at study and, importantly, this advantage was larger for words tested backwards. These results showed that specific priming effects occur with some, but not with other display formats, and that changes in targets' visual features may influence performance on both implicit and explicit memory tests, although they need not contribute equally to implicit and explicit memory performance.

Graf and Ryan (1990) also examined whether specific priming effects would appear when words are presented in different fonts rather than different spatial transformations, and whether specific priming effects can be induced by study tasks that focus subjects' attention on specific visual features of studied words. For this purpose, subjects studied words presented in two uncommon fonts -- either Shadow or Pudgy -- and they were required to either rate the readability of the typefaces or the pleasantness of each word. Graf and Ryan reasoned that the readability study task would focus subjects' attention on the visual appearance of studied words to a greater degree than the

pleasantness rating task, and therefore, format specific effects would be more likely to occur after making readability rather than pleasantness ratings. The results bore out this expectation: Priming was larger when words were studied and tested in the same than in different formats; but this format-specific priming effect was observed only after the readability rating task and not after the pleasantness task. Moreover, format-specific priming was larger for the words displayed in the less familiar Shadow font than in the Pudgy font. Recognition test performance showed an overall advantage after the pleasantness rating task, and an advantage for words studied and tested in the same rather than different formats, but only following the readability study task.

In combination, these findings emphasize that format specific priming effects occur only when subjects focus processing on particular visual features of target materials, and that this kind of processing can be engaged either by specific study instructions or by presenting targets materials in a highly unfamiliar format. They also highlight that implicit and explicit memory can be influenced in similar or different ways by manipulating the visual appearance of target materials.

Summary. Investigations focusing on the specificity of priming to sensory and perceptual features of targets showed that priming effects can be specific to targets' (1) symbolic format (e.g., words/pictures, languages in bilinguals), (2) presentation modality (e.g., visual, auditory), and (3) visual features of words (e.g., typography, font). Priming effects, however, are not always specific to targets' visual features; specific priming effects may occur only under some conditions, namely, when targets' visual features

make targets appear unfamiliar or when processing of such features is demanded by a study task. Finally, previous research also suggests that, contrary to the widely held view, performance on explicit memory tests is influenced by sensory and perceptual attributes, at least under some conditions. Perhaps, the claim that explicit memory is not specific to processing of sensory and perceptual features has arisen because many of the studies investigating specificity on implicit memory tests did not include explicit memory tests.

Summary and conclusions

This review of the existing data allows several generalizations about priming and its relations to explicit memory:

1. Priming is intact in subject groups that have severely impaired explicit memory (e.g., amnesics, children, elderly), thereby implying that implicit memory is mediated, at least in part, by distinct processes or mechanisms from those mediating explicit memory test performance.
2. Priming can be very long-lasting. Priming has been observed even after a period of one year, and thus it cannot be explained as a 'hot tubes' phenomenon.
3. Priming seems to be dependent on different processes for familiar items than for novel items. Priming for familiar items appears to be automatic. In contrast, priming for unfamiliar new items that have not been encountered before, such as pseudowords, novel objects, and new associations, is not automatic but requires unitized processing at study to create new episodic representations.

4. Priming is often, but not always, specific to sensory and perceptual attributes of targets. A more detailed look at the data suggests that specific priming effects may occur only when such attributes make the targets appear unfamiliar or when study trial processing is directed at such attributes, for example, by means of a study task.
5. Contrary to previous claims, performance on explicit memory tests can also be specific to the sensory and perceptual attributes of target items. In fact, most previous experiments on the specificity of priming did not include any explicit memory tests, and therefore, we know very little about the influence of sensory and perceptual attributes on performance of explicit memory tests.

Theoretical explanations

Two kinds of explanations have been offered for similarities and differences between performance on implicit and explicit memory tests: processing and systems perspectives. The processing perspective explains the findings in terms of the cognitive processes that are engaged by different study and test activities. This perspective has been preferred by researchers working with normal healthy subjects. The systems perspective explains the findings in terms of independent memory systems associated with different brain structures. The system perspective is prevalent especially in research with neurocognitively impaired populations such as amnesic patients. What is important is that processing and systems explanations are not mutually incompatible, rather, they serve as complementary metaphors for explaining similarities and differences between

implicit and explicit memory. This section outlines and evaluates various processing and systems views.

Processing views

Processing views are based on the notion that memory is best understood in terms of the processes or cognitive operations that are engaged by different study and test activities (Kolers, 1975, 1976). To illustrate, Kolers proposed that a task such as reading a word or sentence requires a specific set of sensory-perceptual and conceptual analyzing operations and that engaging these operations is like practicing a skill: It increases the fluency and efficiency with which they can be carried out subsequently. In turn, performance on a memory test benefits to the extent that the test engages the same cognitive operations as those engaged by the preceding study task. More generally, it is assumed that remembering is determined by the degree of overlap between study and test processing. This general assumption is known as *transfer appropriate processing* (TAP; cf. Morris, Bransford, & Franks, 1977).

The concept of TAP is not sufficient to explain similarities and differences between performance on implicit and explicit memory tests, however. This is because TAP itself does not specify which cognitive processes are critical for performance on implicit versus explicit memory tests (Graf & Ryan, 1990). Thus, researchers working within this general TAP framework must specify the cognitive processes that mediate implicit versus explicit memory test performance. Several explanations that have specified the processes mediating each form of memory have been proposed: data driven

vs. conceptually driven processes (Roediger & Blaxton, 1987a, 1987b; Roediger & McDermott, 1993; Roediger & Srinivas, 1993; Roediger et al., 1989), environmentally supported vs. subject initiated processes (Craik, 1986), integrative vs. elaborative processes (Graf, 1994; Graf & Mandler, 1984; Graf et al., 1982; Graf & Ryan, 1990; Mandler, 1980, 1988), and interpretive vs. elaborative processes (Masson & MacLeod, 1992; Micco & Masson, 1991). These distinctions are discussed in the following sections.

Data driven vs. conceptually driven processing. Roediger and his colleagues (Roediger & Blaxton, 1987a, 1987b; Roediger & McDermott, 1993; Roediger & Srinivas, 1993; Weldon et al., 1989) have advocated a view based on the distinction between *data* and *conceptually driven* processing. Data driven processing is initiated and guided by the data, for example, by visual features defining words or pictures. In contrast, conceptually driven processing is initiated and guided by concepts, for example, by knowledge and expectations. Roediger assumes, first, that each study task involves a combination of data driven and conceptually driven processing, and different study tasks require different amounts of data driven and conceptually driven processing. A second assumption is that each memory test also engages a combination of data driven and conceptually driven processing. The third assumption is that implicit memory tests (e.g., identification, word stem completion, word fragment completion, picture fragment completion) engage primarily data driven processing, whereas explicit memory tests (e.g., recognition, recall) engage primarily conceptually driven processing (e.g., elaborative processing, mental

imagery). By these assumptions, and consistent with the TAP principle, it follows that performance on implicit memory tests reflects primarily the overlap in data driven processing, whereas performance on explicit memory tests reflects primarily the overlap in conceptually driven processing.

In keeping with this view, Roediger and his colleagues have argued (Roediger, 1990; Roediger et al., 1989; Weldon, Roediger, Beitel, & Johnson, 1995) that manipulations of surface features between study and testing (e.g., modality, symbolic format, typefont) should influence performance on data driven implicit memory tests but should not influence performance on conceptually driven explicit memory tests. In contrast, manipulations of conceptual processing by means of different study tasks (e.g., generating vs. reading words, rating pleasantness vs. counting vowels) should influence performance on conceptually driven explicit memory tests but should not influence performance on implicit memory tests.

The view can explain that priming is reduced by study-to-test changes in presentation modality (e.g., visual vs. auditory), in symbolic format (e.g., words vs. pictures, language in bilinguals), and in surface features, whereas such study-to-test changes do not influence performance on explicit memory tests (e.g., Srinivas & Roediger, 1990). It can also explain that study tasks that promote meaningful processing of to-be-remembered materials improve performance on explicit but not implicit memory tests (see Roediger, 1990; Roediger & Blaxton, 1987b).

Graf and Ryan (1990) have noted, however, that the basic assumption that implicit and explicit memory tests can be distinguished by their requirement for data driven versus conceptually driven processing is not consistent with a number of findings in the literature. First, a number of experiments have demonstrated dissociations between performance on implicit and explicit tests when cues (i.e., data) were exactly the same and only the instructions differed between the tests (Graf & Mandler, 1984; Graf & Schacter, 1987; Roediger et al., 1992; Weldon et al., 1995). Because the cues provided for testing were the same for both kinds of tests, thereby allowing the same amount of data driven processing, no performance dissociations between implicit and explicit memory tests should have been observed. Second, some implicit memory tests require primarily conceptual processing (e.g., free association test, category instance production test), whereas some explicit memory tests require a large degree of data driven processing (e.g., graphemic cued recall test, Blaxton, 1989; word fragment cued recall, Weldon, Roediger, & Challis, 1989). In view of such observations, Roediger et al. (1989; see also Roediger & McDermott, 1993) have recently concluded that *the requirement for data driven versus conceptually driven processing is not necessarily correlated with the implicit versus explicit nature of the memory tests.*

Other drawbacks of this view include the following. First, the distinction between data driven versus conceptually driven processing cannot account for the pattern of findings in amnesic patients who were found to perform as well as normals on both data driven and conceptually driven implicit memory tests and to be impaired on both data

driven and conceptually driven explicit memory tests (Graf et al., 1985). Second, the view does not explain why substantial study-to-test changes in surface features (e.g., hand-written vs. typed words, changes in size) sometimes do, but sometimes do not influence priming (Roediger & McDermott, 1993). Third, the view has difficulty explaining the finding showing that study-to-test changes in surface features (e.g., size) sometimes influence performance on conceptual explicit memory tests such as recognition, while having no influence or parallel influences on data driven implicit memory tests (Roediger & McDermott, 1993).

Environmentally supported vs. subject initiated processing. A closely related view that avoids the problems associated with the distinction between data driven vs. conceptually driven processing was proposed by Craik (1983, 1986). Craik arranged memory tests on a continuum that reflects the degree to which test performance is supported or guided by the environment vs. by the degree to which it is dependent on subject-initiated activities. He argued that implicit memory tests depend primarily on environmentally supported processing whereas explicit memory tests depend primarily on subject initiated processing.

What is considered environmental support? If the environmental support provided by a test includes both the test cues and instructions, Craik's distinction can account for differences between performance on implicit and explicit memory tests better than the distinction between data driven vs. conceptually driven processing (see Graf, 1990; Uttl & Graf, 1993). One drawback of this scheme is, however, that it trades off the

precision afforded by arranging tests only on the basis of cues, as did Roediger and his colleagues. Another drawback of this scheme is that the degree of environmental support provided by particular test cues and by instructions is likely to depend on subjects' preexisting knowledge and on their task relevant skills, including their familiarity with the cues and with the requirements of the task.

Integrative vs. elaborative processing. Graf, Mandler, and their colleagues (Graf & Mandler, 1984; Graf et al., 1982) proposed a view based on the distinction between two memory organizing processes: integrative and elaborative processing. Integration results from processing that bonds features of a target event into a unitized representation; it occurs when subjects either perceive separate components of the event as a coherent whole (e.g., under the guidance of preexisting representations or Gestalt laws like good continuation or proximity) or when they conceive a structure for processing components of an event simultaneously (cf. Graf, 1994; Graf & Schacter, 1989; Mandler, 1980, 1988). Elaboration results from processing that associates a target with other mental contents, thereby embedding its unitized representation in a network of other representations. Elaboration occurs when a target is encoded in relation to the experimental situation -- other targets, situational cues, relevant prior knowledge. To explain the pattern of performance on implicit and explicit memory test, Graf and his colleagues have made two assumptions. First, each study task engages a mixture of integrative and elaborative processing and various tasks require more of one or the other type of processing. Second, implicit tests depend primarily on integrative processing, whereas explicit tests depend

primarily on elaborative processing. By these assumptions and consistent with the TAP principle, performance on implicit memory tests is assumed to reflect primarily study-to-test overlap in integrative processing, whereas performance on explicit memory tests is assumed to reflect primarily study-to-test overlap in elaborative processing.

By this view, normal levels of priming in amnesic patients are explained by assuming that amnesia spares the capacity for integrative processes but impairs the capacity for elaborative processing. Similarly, the stability of priming across the lifespan is explained by assuming that the capacity for integrative processing is fully functional even at an early age and is not affected by aging. The longevity of priming is explained by assuming that any processing unit engaged for perception of a target becomes part of that target's study trial unitized representation, and that perception of the target is influenced by the particular experimental situation, including the experimental room and the cognitive, affective, and motivational state of a subject. Accordingly, when some of this specific context is reestablished (when subjects return for testing), there will be greater overlap between the processing units engaged for perception of targets than when the specific context is changed, and priming can occur in an experiment even though subjects may have encountered the same items many times between study and test (Graf, 1994).

This view also gives insight into the finding that priming for familiar items is mediated differently than priming for new materials that do not have preexisting representations in memory. Priming for familiar items such as words is more or less

automatic because processing of familiar items is guided by preexisting representations. It is assumed that at the time of study or test, preexisting representations of familiar items are re-activated by even minimal amounts of bottom-up, perceptual analysis of targets, and this results in further integration of their representations. Moreover, this type of re-activation is equally likely to occur following semantic and non-semantic study tasks, and consequently, priming effects are comparable across such tasks. In contrast, unitized encoding of new materials such as unrelated word pairs is not automatic because such targets do not have preexisting representations in memory to guide their encoding. Thus, to achieve a unitized encoding of new materials, subjects must engage in processing that creates new representations, for example, by combining two preexisting mental units, by representing a context and a target word into a new unitized mental structure. Priming for novel materials occurs because newly unitized representations can be set up following a single study trial, but the creation of new representations may occur only following study tasks that require processing of new materials as unitized entities.

The specificity of priming is explained by assuming that study trial processing engages a particular set of processing units that become integrated or unitized, and that different subsets of processing units are engaged for processing spoken and written words, for processing of words and pictures, and for processing of words in different languages. Accordingly, when a target's test format matches its format at study, the processing units engaged for perception of the target at test are more likely to overlap those that were engaged for its perception at prior study. A target's study trial

representation is therefore re-activated; it benefits from the increased integration achieved at study.

Interpretive vs. elaborative processing view. Masson and MacLeod (1992; Micco & Masson, 1991) proposed a very similar view focusing on the distinction between interpretive and elaborative processing. Interpretive processes construct a context-sensitive interpretation of the stimulus and they include both data driven and conceptually driven processes. Elaborative processes use the established interpretation of the stimulus and relate it to existing knowledge; elaborative processes may associate any of the features of the interpreted stimuli, including surface and semantic attributes, to the existing knowledge. Masson and MacLeod emphasize that in their view interpretive processing extends beyond the boundaries of the target item as well as beyond the boundaries of a single trial itself. To account for performance dissociations between implicit and explicit memory tests, Masson and MacLeod further assume that implicit memory tests depend only on interpretive processing whereas explicit memory tests depend both on interpretive and elaborative processing. The advantages and disadvantages of this view seem similar to that developed by Graf, Mandler, and their colleagues.

Systems views

Systems views are founded on the notion that memory consists of a number of independent yet interrelated systems, each characterized by different rules of operations, and each served by distinct brain structures (Cohen, 1984; Cohen & Eichenbaum, 1993;

Mishkin, Malamut, & Bechavalier, 1984; Schacter, 1990; Squire, 1986, 1987, 1992; Tulving, 1983, 1985; Tulving & Schacter, 1990; for comprehensive reviews, see Schacter & Tulving, 1994). But what exactly is a memory system? Sherry and Schacter (1987) distinguished between a weak and a strong view of what constitutes a memory system. By the strong view, a memory system is a collection of processing units mediating acquisition, retention, and retrieval that interact only with each other but not with units outside of the system. Thus, different systems do not share any of their component processing units. By the weak view, a memory system is a collection of interacting memory processing units but each of the component units may also interact with other units outside of the system. Sherry and Schacter (1987) further distinguished between systems and subsystems. Subsystems operate according to the common rules of operations but they handle different kinds of information (e.g., pictorial, verbal) and they may be mediated by different brain regions. The aim of system theorists is to identify various memory systems serving various memory functions (e.g., implicit and explicit memory), to elucidate their characteristics, and to find out how they are implemented in the brain.

According to system theorists, similarities and differences between performance on implicit and explicit memory tests occur because different systems mediate performance on the two kinds of tests. One systems view was introduced by Cohen and Squire (1980) who distinguished between *knowing that* and *knowing how*. Knowing how refers to skills such as solving a puzzle or riding a bicycle whereas knowing that refers to

the ability to recollect that an event occurred at a particular time and place. To capture the distinction between these two kinds of knowing, Squire (1986) postulated the existence of two independent memory systems. The declarative memory system mediates conscious recollection of facts and events, whereas the procedural memory system mediates acquisition and performance of skills. The procedural system is assumed to be spared by amnesia, thereby permitting the acquisition of skills, whereas the declarative system is damaged resulting in impaired performance on explicit memory tests.

To accommodate other kinds of non-declarative learning, including priming on implicit memory tests which is spared in amnesic patients, a follow-up proposal by Squire and Zola-Morgan (1988) distinguished between a declarative memory system and a non-declarative memory system, the latter being a heterogeneous collection of systems that mediate memory which is not declarative. Others have distinguished systems like habit versus memory (Mishkin et al., 1984), semantic versus episodic memory (Tulving, 1983, 1984), and perceptual representational versus episodic memory (Tulving & Schacter, 1990; Schacter & Tulving, 1994). This last distinction will be discussed in more detail because it seems the most relevant, detailed, and illustrative example of how systems theorists explain similarities and differences between implicit and explicit memory.

Perceptual representational system. One of the most detailed classification scheme of memory systems has been proposed by Tulving (1983, 1984) who suggested that memory can be divided among three hierarchically organized systems: procedural, semantic, and episodic memory. The procedural system mediates the acquisition and

performance of skills (e.g., typing, riding a bicycle, playing a musical instrument, mirror figure tracing), the semantic system mediates the acquisition of general knowledge and facts (e.g., knowledge about word meaning and about functions of objects), and the episodic system enables remembering of temporally and spatially dated personal events (e.g., remembering what one had for breakfast today). The procedural memory system's domain is behavior, whereas the domain of the semantic and episodic systems is the capacity for representing an external world, the capacity for storage and manipulation of representations of objects, events, and relations among them (Tulving, 1984; Tulving & Schacter, 1990).

To account for performance on implicit memory tests, Tulving and Schacter (1990; see also Schacter, 1990, 1994; Schacter & Tulving, 1994) suggested that priming effects reflect the operations of a new system, called the *perceptual representational system* (PRS), which functions independently of the episodic and semantic systems. The PRS is assumed to be involved in the identification of perceptual objects, including words and sounds. It consists of a class of modular subsystems -- the *visual word form subsystem* (cf. Warrington & Shallice, 1980), the *structural description subsystem* (cf. Riddoch & Humphreys, 1987), and the *auditory word form subsystem* -- that process and represent information about the form and structure but not the semantic and associative properties of words and objects. PRS subsystems all operate at a presemantic level, they mediate priming on implicit memory tests, and they all depend on cortical mechanisms. However, PRS subsystems differ with respect to the information they handle: The visual

word form subsystem represents the visual appearance of words, the structural description subsystem represents the structure of objects in terms of their parts and relations among them (see Biederman, 1987; Sutherland, 1968), and the auditory word form subsystem represents acoustic and phonological information about words.

Although the PRS can be seen as similar to Morton's logogen systems (Morton, 1979), it differs from it in an important respect: Priming effects can be mediated both by preexisting representations and by new representations. Access to information in PRS is assumed to be highly inflexible or hyperspecific; the PRS system is assumed to contain a large number of representations of each word or object, each of which is accessible only through highly specific cues. Finally, Schacter (1990) points out that the PRS is not designed to mediate priming on all kinds of implicit memory tests; it mediates priming only on data driven implicit memory tests including identification, word stem, and word fragment completion tests, whereas priming on other implicit memory tests such as category instance production is mediated by systems other than the PRS, possibly by the semantic memory system.

This systems view accounts well for a number of functional dissociations between implicit and explicit memory tests performance. It is consistent with the findings that priming effects are independent of semantic study trial processing, and it explains why priming is affected by study-to-test changes in modality of presentations, changes in symbolic format (e.g., words/pictures, languages in bilinguals), and changes in other surface features (e.g., font, orientation). It also explains the finding that study-to-test

changes in some visual attributes of objects (e.g., size, mirror-reflection) do not seem to affect priming; this may be because such manipulations do not affect the structural description of objects (see also Biederman, 1987). Finally, the claim that the PRS represents objects as structural descriptions is consistent with the findings that on the object decision task, priming occurs only with structurally possible objects and it never occurs for structurally impossible objects. Impossible objects show no priming because it is difficult to form a representation of their global three-dimensional structure (Schacter, 1992).

The system view is also consistent with a number of findings showing striking dissociations between disorders of access to semantic knowledge of words and objects and disorders of perceptual/structural knowledge about the same objects (e.g., Ellis & Young, 1988; Farah, 1990). To illustrate, patients with visual object agnosia are unable to name objects when presented with their pictures or to answer questions about pictured objects' semantic or associative properties. Yet, these same patients can correctly decide whether or not line-drawings of possible and impossible objects represent real objects (Humphreys & Riddoch, 1987). Schacter (1990) points out that despite their inability to access semantic and associative knowledge about objects, these patients do have access to their structural descriptions since they are capable of deciding whether they are real objects. According to Schacter, this and similar findings are consistent with the view that knowledge of the form and structure of objects is represented in a structural description

system that is distinct from a semantic memory system that represents semantic and associative knowledge about objects.

Conclusions and implications

Processing and systems views provide complementary perspectives on memory. They emphasize different aspects of the mechanisms involved in performance on various memory tests: Processing views emphasize cognitive operations whereas systems views tend to focus on brain structures (Shimamura, 1993). However, both processing and systems views invoke the general idea of transfer appropriate processing and they emphasize that the study/test overlap in different processes or mechanisms is responsible for performance dissociations between implicit and explicit memory tests.

Despite their diversity, all of the featured views are capable of accounting, more or less, for all major findings: development and aging, the longevity of priming, the specificity of priming, and dissociations between implicit and explicit memory caused by neurocognitive trauma. All of the accounts underscore the specificity of priming; they emphasize that *performance on implicit memory test and priming is specific to sensory and perceptual cues whereas performance on explicit memory tests is less dependent on such cues.* They also emphasize that priming is mediated by episodic representations that are specific to particular study trial encounters with to-be-remembered targets.

This emphasis on specificity contrasts with an emphasis on various object constancies in research on object perception. Object perception researchers use the same methods -- identification tests and priming paradigms -- to investigate how we identify

common objects across a variety of orientations and sizes due to variation in observer's viewpoint and distance with respect to an object. In contrast to memory researchers, however, object perception researchers emphasize that variations in sensory and perceptual cues have little or no effect on identifying common objects, thereby demonstrating various object constancies due to shape, size, and color, for example.

My research focused on the link between these two perspectives: memory and object perception research. It investigated the influence of several object attributes -- orientation in the plane of the page, size, and relative size -- on baseline identification, on priming, as well as on performance of explicit memory tests.

The next chapter starts with a brief review of the most recent research on the influence of orientation in the plane of the page on identification, priming, and explicit memory. The core of the chapter is an article reporting the results of three experiments that examined how orientation in the plane of the page influence identification, implicit memory, and explicit memory for color photographs of common objects.

Chapter 3

Orientation information in semantic and episodic Memory

This chapter consists mainly of an article on *Object Orientation In Semantic And Episodic Memory* (Uttl & Graf, in press). Since the preparation of this article, one additional report that is directly relevant to my work has appeared in the literature (i.e., Zimmer, 1995). Zimmer (1995) investigated the influence of objects' orientation in the plane of the page, as well as the influence of size, on implicit and explicit memory. In the experiments on orientation, Zimmer (1995) used 120 line-drawings of objects with an unambiguous principal axis, and he showed them in several orientations: normal, mirror reversed, rotated in the plane of the page by 90°, and reflected on the principal axis of elongation. For the implicit memory test, subjects were presented with an arrow that pointed in a specific direction, followed by an object oriented either in the same direction or in a different direction. Subjects were required to decide as quickly as possible whether the arrow pointed in the same direction as the object. For the explicit memory test, subjects were first asked to make old/new recognition decisions, and then, for the orientation recognition test, they were required to decide for each object whether it appeared in the same orientation as at study. The results showed that old/new recognition was better for objects presented in the same orientation than in a different orientation. The orientation recognition test results showed that subjects were also able to discriminate between objects tested in the same versus different orientation. In contrast, implicit memory test results showed no effect due to study-to-test changes in object

orientation. This latter result is difficult to interpret, however, because Zimmer found no priming in several conditions where objects were presented in the same orientation at study and testing.

In another experiment, Zimmer investigated whether rotating objects by 90° in the plane of the page influences priming on a picture fragment naming test. For this test, fragmented pictures of previously seen and new (non-studied) objects were presented and subjects were required to name them as quickly as possible. The fragments could appear either in the same orientation or in a different orientation at study and test. The results were marginal: There was a non-significant trend towards more accurate and faster performance for objects displayed in the same orientation at study and test. These and other results led Zimmer (1995) to conclude that [his experiments] "clearly show[s] that implicit tests with pictures can be influenced by a variation of the[ir] sensory features [of objects], but size and orientation seem not to belong to them. At least this is valid for those implicit tests that have been used up to now" (p. 271).

These findings and conclusions are puzzling. They are in conflict with the general trend that emerges from the comprehensive review of the previous literature in this chapter. My work also shows that both orientation and size influence identification and priming, at least under some circumstances. Moreover, the findings presented by Zimmer (1995) are weak and, therefore, must be interpreted cautiously. Consequently, Zimmer's conclusions are probably premature and his findings do not have any direct implications for my work.

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Object Orientation Information In Semantic And Episodic Memory

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Abstract

The time required to identify a common object depends on several factors, especially pre-existing knowledge in semantic memory, and episodic representations newly established as a result of a prior study. We report three experiments that investigated the relative contribution of these factors to implicit and explicit memory test performance. In each experiment, subjects were shown color photos of objects, and memory was assessed either with an old/new recognition test or with a test that required them to identify objects that were slowly faded in on a computer monitor. The critical variables were the type of photo -- each showing either an object with a predominant or *cardinal* orientation (e.g., helicopter) or a non-cardinal object (e.g., pencil), and the orientation at which the photos were displayed at study and at test (e.g., on the plane of the page at 0°, 120°, 180°, 240°). For each subject, half of the targets were shown at study and all appeared on the test, with targets displayed either in the same orientation as at study or in a different orientation. For non-studied targets (i.e., in the baseline condition), identification test performance showed a huge effect due to display orientation, but only for cardinal objects. For studied targets, identification test performance showed substantial priming in all conditions, with more priming on cardinal than non-cardinal targets, especially when their display orientation at test was unusual (i.e., 120°, 240°) and the same as at study. We use these findings to discuss the extent to which orientation information is coded in the semantic and episodic memory representations of different kinds of objects.

Object orientation information in semantic and episodic memory

Dissociations between implicit and explicit memory test performance have been investigated by means of many different strategies. The earliest, often cited studies compared amnesic patients and matched control subjects, revealing that the patients can show entirely normal priming or implicit memory test effects for recently studied materials despite profound deficit in their ability to recall or recognize the same materials (for recent reviews see Cohen & Eichenbaum, 1993; Moscovitch et al., 1993; Squire, 1992). Related studies used a developmental strategy and focused on subjects from different age groups; they compared children versus adults or young versus older adults and reported that even though explicit recall and recognition performance change -- showing an increase and then a decrease -- across the life span, implicit memory and priming seem to function normally from early childhood on into late adulthood (e.g., Graf, 1990; Howard, 1988, 1991; La Voie & Light, 1994; Mitchell, 1993; Naito & Komatsu, 1993; Parkin, 1993). By far most often used, however, is an experimental strategy by which one or more variable(s) is manipulated, including materials (e.g., type font, modality of presentation, size, display orientation), how materials are studied (e.g., incidental versus intentional, semantic versus non-semantic processing), or the delay between study and testing (minutes, hours, days, or weeks), and the goal is to assess how these manipulations affect implicit and explicit memory test performance. The findings from studies that used this kind of strategy have been summarized in a number of recent

reviews (e.g., Graf, 1994; Graf & Masson, 1993; Roediger, 1990; Roediger & McDermott, 1993; Schacter, Chiu, & Ochsner, 1993; Shimamura, 1993).

The experiments reported in this article used a materials-manipulation strategy to investigate further the different kinds of memory representations that mediate performance under various implicit and explicit test conditions. We built on several widespread theoretical assumptions about memory. Specifically, we assumed that all memory processing (at study and at test) is interactive, involves both data- and conceptually-driven components (Bartlett, 1932; Norman & Bobrow, 1975), and we considered test performance as in index of the processing overlap between study and testing (Graf, 1994; Graf & Ryan, 1990; Masson & MacLeod, 1992; Morris et al., 1977; Roediger, 1990; Roediger et al., 1989). We postulated that performance on any memory test is determined by several factors, including by the cues and instructions provided by the experimenter -- the test situation (enabling bottom-up processing), the targets' and distractors' pre-existing or semantic representations in memory (enabling top-down processing), and episodic representations that were established during the study trial (also enabling top-down processing) (Craik, 1986; Graf & Birt, in press; Graf & Ryan, 1990; Mori & Graf, in press; Roediger et al., 1989). The latter assumption is crucial because it implies that when test conditions are held constant (i.e., same cues and instructions), a performance difference between studied and unstudied items must be attributed to episodic memory representations, or to an interaction between targets' episodic and semantic memory representations. For another example, it follows that if baseline

performance (i.e., performance on non-studied targets) is different for two kinds of targets, such as high versus low frequency words, this difference is due to their semantic memory representations.

For a concrete illustration of priming and implicit memory test baseline effects that have been attributed to episodic and semantic memory representation differences, respectively, consider investigations that involved the use of familiar versus unfamiliar words, or the use of words versus pseudo-word letter strings. For example, Feustel et al. (1983) presented familiar words or unfamiliar pseudo-words (i.e., pronounceable letter strings like *numdy*) on an oscilloscope, one at a time, with display durations ranging from 24 ms to 68 ms, and the subjects' task was to identify and name each item. The baseline condition results (i.e., performance without prior study) showed that words were identified more accurately than pseudo-words at all display durations. In addition, Feustel et al. reported priming effects (i.e., higher identification test performance in the experimental conditions than in the baseline condition) for both kinds of items, with the size of effects being comparable across items. Similar findings have been reported by others[1] (e.g., Bowers, 1994; Feustel et al., 1983; Haist et al., 1991; Musen & Squire, 1991; Salasoo et al., 1985). By contrast, however, experiments with familiar versus unfamiliar words have shown a positive correlation between word familiarity and implicit memory test baseline performance, but the size of priming effects was negatively correlated with baseline performance (and thus with item familiarity) (e.g., Jacoby & Dallas, 1981; Roediger, Weldon, Stadler, & Riegler, 1992).

To explain these findings, first those from the baseline condition, it is widely held that item familiarity is part and parcel of a target's long-term representation in semantic memory, and that familiarity -- the feeling or cognitive state that is experienced -- is a direct reflection of the strength of these representations. Thus, the baseline data imply that familiar words have stronger semantic memory representations than unfamiliar words, and that they in turn have stronger representations than pseudowords. Elsewhere, we and others speculated further about familiarity and suggested that strength refers to the degree of integration or unitization of a representation (Feustel et al., 1983; Graf & Mandler, 1984; Graf & Schacter, 1989; Graf et al., 1984). The degree of unitization (or the self-coordinating nature) of a target's representation is a positive function of practicing the skills required for identifying that target, and priming is viewed as reflecting an increase in skill (at identifying that target) that was brought about by having recently encountered the target. In other words, a change in skill or in practice at identifying a studied target is interpreted as the episodic memory representation that underlies priming. Therefore, by this general view, the pattern of priming effects shown by previous research implies that even one encounter with a target is sufficient to increase its subsequent skilled identification, and this increase in skill (or in the strength of episodic memory representations) is about the same for unfamiliar words and for pronounceable pseudowords, whereas it is substantially larger for unfamiliar words than for highly familiar words (Graf & Mandler, 1984; Mandler, 1980, 1994).

Like familiarity, a target's orientation in space is also a critical property which might be coded in both long-term representations and in episodic memory representations. Orientation is a property that is essential for many perceptual tasks, including for reading, for grasping, for identifying mis-oriented objects, and it might also be used for remembering a specific recent encounter with an object (e.g., Rock, 1973; Soechting & Flanders, 1993). Pioneering research in memory for orientation information was carried out by Kolers and his colleagues (Kolers, 1973; Kolers & Perkins, 1975) who required subjects to read aloud short paragraphs (~300 words long) that were displayed either in upright, normal orientation or upside down (rotated 180° in the plane of the page). The results showed that in the absence of prior practice, reading rotated displays took over three times longer than reading normally oriented text.

Jolicoeur and his colleagues (Jolicoeur, 1985, 1988; Murray, Jolicoeur, McMullen, & Ingleton, 1993; also Braine, 1965; Maki, 1986) used drawings of natural objects and displayed them in normal orientation (0°) or rotated by 60°, 120°, 180°, 240°, or 300° in the plane of the page. They found that the time required to name the objects showed an M-shaped function, with a sharp linear increase in naming time with departures from 0° to 120° (either in a clock-wise or a counter clock-wise direction), and a pronounced dip for displays at 180° (upside down). To learn about episodic memory coding of information concerning the specific orientation in which a target was displayed at study, Murray et al. (1993) presented each drawing on two different occasions, with the orientation being either the same on both occasions (at 60°, 120°, 240°, or 360°), or

different (e.g., at 60° and 120° on the first and second occasion, respectively). The naming time data showed priming (less time required for naming a previously seen drawing) in all conditions (158 ms), and in addition, there was significantly more priming in the same (236 ms) than different (131 ms) study/test conditions.

This line of research is promising. The baseline data suggest that orientation information is critical to the manner in which objects are represented in long-term or semantic memory, and the pattern of priming effects suggests that orientation information is also coded in the memory representations that are specific to the prior learning episode. In the General Discussion, we will outline a theoretical account for such findings; however, our first objective for this article is to report new experiments on implicit and explicit memory for objects' spatial orientation. We set out to collect more and stronger evidence, and to generalize to new tasks, materials, and test procedures the findings in the literature. To our knowledge, the study by Murray et al. (1993) has not yet been replicated, and thus so far we have at best only minimal data on how orientation information influences priming and baseline performance on implicit memory tests, and on how these are related to explicit memory test performance.

Our experiments had the same basic design as that by Murray et al. (1993). At study and test, pictures of common objects were displayed at 0° or 180° for Experiment 1, or at 0°, 120°, or 240° for Experiment 2 and 3, and the display orientation was either the same at study and test or it was different (e.g., 120° and 240° at study and test, respectively). Implicit memory was assessed with a test that required subjects to identify

objects that were slowly 'faded in' on the computer monitor, and explicit memory was assessed with a recognition test that required making Old/New decisions about studied and non-studied objects. Experiment 1 was mainly exploratory: It was designed (a) to 'pilot' the effectiveness of the specific 'fade-in' procedure we used for identification testing, and (b) to examine whether two different types of pictures -- line drawings versus photographs -- give rise to different patterns of performance. The picture type manipulation was motivated by a concern about external validity, because most previous studies on implicit and explicit memory for pictures only used line drawings (e.g., Biederman & Cooper, 1992; Jolicoeur, 1985, 1988; Murray et al., 1993; Schacter et al., 1990; but see Legault & Standing, 1992) and it is possible that the findings from these materials do not generalize completely to photographs (Brodie, Wallace, & Sharrat, 1991; Legault & Standing, 1992; Price & Humphreys, 1989). Consistent with previous research, we expected that for non-studied targets, identification would be better when they are displayed in upright orientation rather than in any of the rotated orientations, thereby suggesting that orientation information is part and parcel of targets' semantic memory representation. For studied targets, we expected to find priming in all conditions, and consistent with Murray et al. (1993) the size of priming effects was predicted to be larger for targets displayed in the same orientation at study and test than for targets displayed in different orientations.

The target objects used for Experiments 2 and 3 were of two different types which we have identified as *cardinal* and *non-cardinal*, respectively. Cardinal objects are those

that have a typical, normal upright real-life orientation, for example, a helicopter or a kettle; by contrast, non-cardinal objects such as a pacifier or a tennis racquet are orientation independent in the sense that we tend to encounter them in many different orientations. Others have described these kinds of objects as being manipulable or non-manipulable (Konorski, 1967), as having or not having a dominant orientation (Jolicoeur, 1988), as having or not having distinct tops/bottoms (Murray et al., 1993), or as being mono- versus poly-oriented (Verfaillie, 1992). The materials used for the study by Murray et al. (1993) were all of the former type, and we expected to replicate their findings only with our cardinal objects. By contrast, we anticipated that non-cardinal objects would be identified equally easily in all display orientations (under control conditions), thereby suggesting that their semantic memory representations behave as if they are free of orientation information. Consistent with the general memory processing assumptions outlined at the beginning of this article, we also expected that non-cardinal objects would show similar amounts of priming in all study/test display orientation conditions.

Experiment 1

The subjects in Experiment 1 studied a series of pictures (photos and line-drawings) of common objects that were displayed upright or upside-down. At test, the pictures were shown either in the same orientation as at study or in a different orientation. Memory was assessed with an implicit identification test, or with an explicit recall or recognition test. The first test required subjects to identify pictures of objects that were

slowly faded-in on a computer monitor. Consistent with previous research (Murray et al., 1993), we expected more priming for pictures displayed in the same orientation at study and test.

Method

Subjects and design. Forty-eight undergraduate students participated in the experiment for course credit. The design had one between subjects factor -- test type (explicit, implicit), and four within subject factors -- picture type (photographs, line-drawings), history (studied, non-studied), study orientation (upright, upside-down), and test orientation (upright, upside-down). A randomly selected 32 subjects participated in the implicit test condition and 16 in the explicit test condition.

Materials. A set of 168 pictures was prepared: 84 color photos and 84 line-drawings. The photos were of common objects, such as a watch or a bicycle, that were photographed against a light green background by means of a color video camera connected to a TARGA+ video frame grabber. The line-drawings were of common objects included in the clip art library of the Micrographx Charisma 3.0 graphics package (Charisma, 1990), and the vast majority consisted of black drawings on a white background. Each picture was cropped to a rectangle with 240 vertical (100 mm) and 320 horizontal (133 mm) pixels, respectively. Inside each picture, the target object was scaled so as to span approximately 220 pixels on its longest axis. All pictures were displayed on a 14-inch Acer color graphics monitor, driven by a TARGA+ color graphics card operating in 16.7 million color mode at a resolution of 640 by 480.

Four photos and four line-drawings were used for instruction and practice. The remaining 80 pictures of each kind were randomly arranged into 8 subsets with 10 pictures each, and these subsets were used in various conditions of the experiment. As a shorthand, we use the labels P_1 to P_8 to identify the photograph subsets, and D_1 to D_8 to identify the drawing subsets.

Procedure. The study was described as examining perception and memory for pictures of objects. Subjects were tested individually in a session that lasted about 45 minutes. They were seated at a desk about 60 cm from the computer monitor. The session had a study phase and a test phase. The study phase procedure was the same for all subjects. On each trial, a picture was displayed in the center of the computer monitor, either in upright orientation or upside down (i.e., rotated by 180° in the plane of the page), and the subjects' task was to rate its familiarity or commonality. To make the ratings, subjects used a 5-point scale which had *1 - not very common* and *5 - very common* at its endpoints, and they responded by typing their ratings on the computer keyboard. Subjects practiced the rating task on two photos and two drawings; they proceeded at their own pace. Each keypress blanked the screen for 2-3 s (i.e., the time required to load the next picture into memory) and triggered the display of the next picture. Following the practice phase, a randomly arranged list with 80 pictures (4 subsets of photos and 4 subsets of drawings) was presented according to the same procedure. For each subject, two subsets of photos and two subsets of drawings were displayed in upright orientation, and the remaining subsets were shown upside down.

Counterbalancing was used to ensure that across subjects, all picture subsets appeared equally often in each study condition.

The test phase followed immediately after study, but the procedure was different in the two memory test conditions. In the explicit condition, memory was assessed with a free recall test followed by a Yes/No recognition test. For the recall test, subjects received a form with two columns, labeled photographs and line-drawings, respectively. The instructions were to recall the photos and line-drawings from the study list, and to write them on the test form in the appropriate column. The test lasted 5 minutes, and subjects were instructed to draw a line to mark off the number of pictures recalled during each minute.

The recognition test followed immediately after free recall. The test included a random arrangement of 160 pictures -- 80 (4 subsets of photos and 4 subsets of drawings) from the study list plus 80 (4 subsets of photos and 4 subsets of drawings) that had not been studied, with half of them displayed in upright and upside down orientation, respectively. The non-studied pictures served as distractors, and they provided an index of guessing in each orientation test condition. For the studied pictures, the test was constructed so that one subset of each kind (photos, drawings) appeared in the *same orientation* as at study (i.e., upright at study and test, upside-down at study and test), while the others were displayed in a *different orientation* (i.e., upright vs. upside-down at study vs. test, respectively). To illustrate, consider one subject who studied subsets P_1, P_2, D_1, and D_2 in upright orientation, and subsets P_3, P_4, D_3, and D_4 in upside-down

orientation. For this subject, P_1, D_1, P_3, D_3 were tested in their study-phase orientation (i.e., upright for P_1 and D_1, upside-down for P_3 and D_3), whereas the study and test display orientation was different for the remaining subsets (i.e., upright study vs. upside-down test for P_2 and D_2, upside-down study vs. upright test for P_4 and D_4). The same subject viewed the non-studied subsets, P_5, P_6, D_5, and D_6 in upright orientation, and P_7, P_8, D_7, and D_8 in upside-down test orientation. Across subjects, counterbalancing ensured that each picture subset was used equally often in each study and test condition, and that each served equally often as a target or distractor on the recognition test. On each trial of the recognition test, a photo or line-drawing was displayed in the center of the monitor and the task was to decide (Yes/No), as quickly and accurately as possible, whether that picture had appeared in the study list, "regardless of the orientation in which you saw it previously". Subjects used the left- and right-arrow keys, respectively, to record their Yes and No recognition decisions. The test was self-paced, with each keyboard response triggering the display of the next picture. A delay of 2-3 s (i.e., the time required to load the next picture into memory) separated successive test items.

In the implicit condition, the same list of 160 studied and non-studied pictures was presented for an identification test, with materials assigned to conditions and counterbalanced in exactly the same manner as for the recognition test. On each trial, a photograph or line-drawing was displayed by means of a fade-in procedure, and the subjects were told that the objective was to measure their ability to identify objects displayed in different orientations. Specifically, their task was to identify each object as

quickly and accurately as possible, to press the spacebar as soon as they had identified it, and then to record its name by typing it on the keyboard. Pressing the spacebar stopped the fade-in procedure, it cleared the computer screen, and it caused the display of a prompt for recording the name of the displayed object.

The fade-in procedure operated on the pixel map of each picture which was made up of 240 rows and 320 columns. On each pass, the program stepped through all 320 columns, randomly selecting and turning on 1 of the 240 pixels in that column (if a pixel was already turned on, the next one down was selected, etc.). In this way, the picture was slowly faded-in, with all pixels showing after 240 passes or about 20 s. At the end of each pass, the program checked for a keyboard response. If none had occurred, it paused for 72 ms[2] and then continued on to the next pass; if a response had occurred, it recorded the current fade-in level -- the number of passes made prior to the keyboard response. The subjects practiced the identification task with 8 pictures, and then worked through the 160 item test list. A delay of 2-3 s (i.e., the time required to load the next picture into memory) separated successive test items.

Results

On the recall test, the critical dependent measure was the proportion of photographs and drawings that was correctly recollected from each study-phase condition (upright vs. upside-down). For the recognition test, the dependent measures were hits -- the proportion of correctly recognized studied pictures, correct rejections -- the proportion of correctly recognized non-studied pictures, and decision times -- the time (in ms)

required for making hits and correct rejections. Finally, the critical dependent measure for the identification test was the fade-in level required for identification of studied and non-studied photos and drawings in each experimental and control condition. The fade-in level represents the proportion of pixels that had to be turned on for successful identification of an object. The tables and figures, and all statistical analyses that we report, are based on subjects' data, that is, averaged across the different items/pictures that were used in the experiment. Analyses by items showed the same general pattern as analyses by subjects; however, they were less conclusive (i.e., more variable) due to the small number of subjects who received each item in each of the critical conditions of the experiment. The data were screened for outliers -- values that differed by more than three standard deviations from their mean, and to reduce their influence on statistical analyses, outliers were set equal to the value of the largest or smallest non-outlier, respectively. Less than 1.7% of the data points were outliers. The alpha level was set at .05 for all statistical tests. The effects or interactions with p-values between .05 and .10 are noted as 'marginal effects' and they are interpretted in a light of η^2 -- the index of effect size which has properties similar to r^2.

Recall and recognition. Although the original design called for 32 subjects in the explicit memory test condition, we discontinued this part of the experiment after testing 16 subjects and discovering that recognition performance was too high and limited by ceiling effects. Across these 16 subjects, the proportions of hits and correct rejections were .92 and .88, respectively, with between 38% and 75% of the subjects obtaining

perfect scores in various conditions. Thus, the recognition test data were not analyzed further.

On the free recall test, the same 16 subjects recollected a similar proportion of photos (.31) and drawings (.32), and performance was comparable on pictures that had been displayed in upright (.29) or upside-down (.33) orientation during the study phase. An ANOVA with study orientation (upright, upside-down) and picture type (photo, drawing) as within-subjects factors showed no significant main or interaction effects. Our testing procedure required subjects to mark the number of pictures that they recollected each minute, and performance showed a sharp decrease from the first through the fifth minute of the test, $F(4,60) = 29.63$, $MSe = .002$, $\eta^2 = .66$ with means of .13, .08, .05, .04, and .02, respectively. (Note: η^2 is an effect size index with properties similar to r^2.). These means reveal that the 5-min test was long enough to exhaust recollection of the studied targets.

Identification and priming. Preliminary analyses showed that some pictured objects were often identified incorrectly, and others were very difficult to identify. To reduce any confounding effects due to such objects, we excluded the data from those that were correctly identified by less than 80% of all subjects, as well as from those whose identification required more than 80% of their pixels (note: identification that requires all pixels represents a performance floor effect on the test). After screening according to these criteria, the data from 69/80 drawings and 67/80 photos remained available for analysis. Further screening revealed that subjects made incorrect identifications (i.e., they

stopped the fade-in procedure but then could not correctly identify the object) on 2.8% of the test trials. Separate analyses showed that the data from these trials had only a negligible effect, and thus, they were left in the analyses that we report.

Figure 3.1 shows the mean fade-in level that was required for correctly identifying photos and drawings in each experimental and control condition. The means reveal that a higher fade-in level (i.e., more visible pixels) was required for identifying new, non-studied (117.11) versus old, studied (89.61) pictures. The lower fade-in level for identifying studied versus non-studied pictures is an index of priming that reflects the influence due to learning-episode specific memory representations. The figure shows that the overall amount of priming (i.e., the fade-in level advantage of studied over non-studied pictures) was larger for photos (31.83) than for drawings (23.18), and it was larger for pictures tested upside-down (29.67) than upright (25.34). More important, the means show consistently more priming for pictures that were displayed in the same orientation (30.15) rather than in different orientations (24.85) at study and test.

Non-studied, new photos (115.57) and drawings (118.65) were equally easy to identify (cf. Biederman & Ju, 1988), but more pixels were required for identification when they were displayed upside down (124.95) rather than upright (109.28). An ANOVA of performance on non-studied pictures revealed a significant main effect for test orientation, $\underline{F}(1,31) = 37.47$, $\underline{MS}e = 209.85$, $\eta^2 = .547$, with no other effects approaching significance.

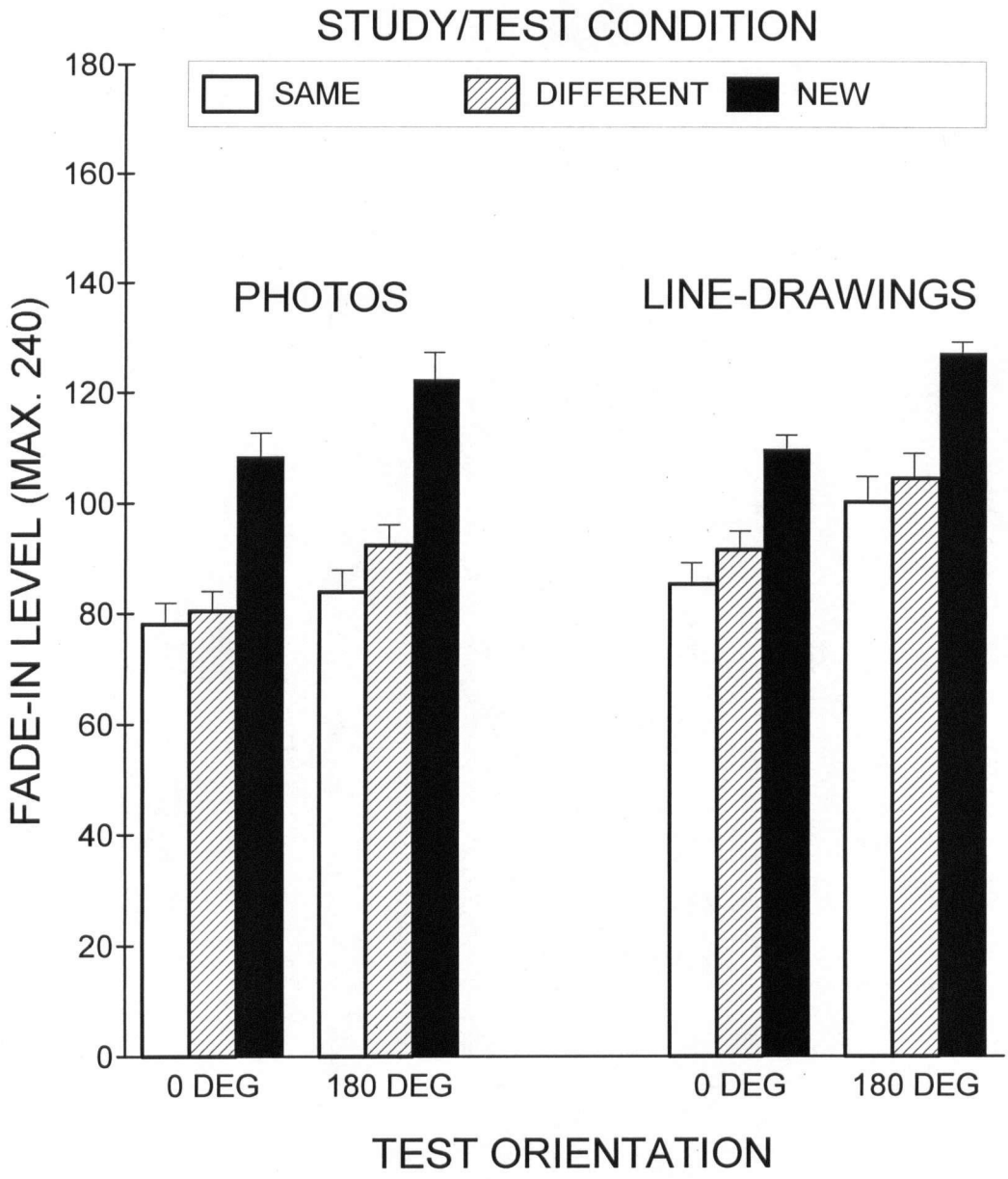

Figure 3.1. The mean fade-in level required for identifying photos and line-drawings, as a function of test orientation (0°, 180°), study/test condition (same/different), and history (studied, non-studied), in Experiment 1. T-bars indicate standard errors of means.

Because of the difference due to test orientation in baserate performance, we computed a priming score for each subject in each experimental condition (i.e., the difference between each subject's performance in each experimental condition minus the corresponding baserate condition mean), and subsequent analyses focused on these scores[3]. An overall ANOVA of priming, with picture type, test orientation, and study/test condition (same, different) as within-subjects factors, confirmed significant main effects for picture type, $F(1,31) = 22.56$, $MSe = 212.12$, $\eta^2 = .42$, and for test orientation, $F(1,31) = 5.32$, $MSe = 226.04$, $\eta^2 = .15$. More importantly, there was also a significant effect for study/test condition, $F(1,31) = 5.85$, $MSe = 308.17$, $\eta^2 = .16$. No other effects approached significance.

Discussion

Experiment 1 revealed several critical findings: substantial priming in all study/test conditions, more priming on photos than line drawings, more priming on upside-down than upright test displays, and consistently larger priming effects when the study and test displays were in the same orientation rather than in different orientations. The overall amount of priming in the present experiment was 27.5 levels (117.11 levels vs. 89.61 levels, for non-studied vs. studied pictures, respectively) or 23.5% [i.e.,(27.5 / 117.11 levels) * 100], which is comparable to the size of priming effects observed with other testing methods. As described in the Method, our test procedure increased screen displays at a rate of about 12 fade-in levels per second, and thus, a 27.5 level priming effect indicates that studied picture were identified over 2 s faster than non-studied

pictures. The finding of a 7.4% [i.e.,{(31.83 - 23.18) / 117.11 levels} * 100] larger priming effect on photos than drawings is novel. The difference is not likely to reflect a difference in subjects' familiarity with photos and drawings because familiarity would have exerted its influence also in the baseline condition, where identification test performance was comparable across materials; moreover, during the study phase subjects rated drawings as less common (3.13) than photos (3.64), $F(1,47) = 100.32$, $MSe = .13$, $\eta^2 = .68$. A more likely possibility is that our photos were more elaborate than the line-drawings (because they included more detail, color, individuating information about each object), which may have caused the encoding of more distinctive episodic representations, and more distinctive representations may provide more avenues for retrieval on the subsequent test. A similar suggestion was offered by Graf and Schacter (1989) who found more priming for concrete than abstract words on a stem completion test.

Most directly relevant to the main hypotheses that motivated the present work were two findings: the fact that non-studied targets were identified more quickly when they were displayed upright rather than upside down, and the finding of an overall 5.3 level or 4.5% [i.e.,(5.3 / 117.11 levels) * 100] larger priming effect when the study and test display were in the same orientation rather than in different orientations. This pattern of findings replicates and extends the work by Murray et al. (1993). The results from the baseline condition point to the orientation specificity of targets' semantic memory representations, and the finding of more priming for targets that were displayed in the

same orientation at study and testing suggests that the episodic representations that mediate priming also contain orientation information.

To gain further insight into how orientation information is coded in memory, we also examined the relation between the size of baseline condition effects produced by the orientation manipulation and the size of priming effects. For each target, we computed an orientation-specificity index, defined as the cost in fade-in levels required for identifying it from an upside down versus upright display, and we found the following correlations between *orientation-specificity and priming* across experimental conditions: for photos, they were -.10, -.09, .43, and .35 in the upright/same, upright/different, upside-down/same, upside-down/different condition, respectively, and for line-drawings, they were -.07, -.16, .39, and .24, in the upright/same, upright/different, upside-down/same, upside-down/different condition, respectively. These findings suggest that those mis-oriented objects that are relatively more difficult to identify (compared to other objects) also tend to yield larger priming effects.

Experiment 2

Experiment 2 was designed to examine more directly the possibility (a) that some *mis-oriented* objects are relatively more difficult to identify than others and (b) that only the former give rise to larger orientation specific priming effects. The experiment followed the same general method as Experiment 1: At study and test, pictures of objects were displayed either in the same orientation or in different orientations, and memory was assessed with explicit and implicit tests either immediately after study, or after a 1-week

delay. In contrast to Experiment 1, however, the pictures were presented in three different display orientations, and all pictures were photos of common objects because they had shown more priming than line drawings. Previous work has revealed that the size of priming effects is larger for materials displayed at 120° and 240° than at 0° (Jolicoeur, 1985, 1988), and thus, all pictures were displayed at 0°, 120°, or 240° and the display orientation was either the same or different at study and test. More relevant to the main hypotheses, the target pictures were of two different kinds: photos of *cardinal* and *non-cardinal* common objects, respectively. Cardinal objects are those that have a typical, normal upright orientation in real life, for example, a helicopter or an elephant; by contrast, non-cardinal objects, such as a pacifier or a pen, are orientation independent in the sense that we tend to encounter them in many different orientations[4]. Consistent with the findings of Experiment 1, we expected that cardinal objects would show larger orientation-specific effects than non-cardinal objects, and that priming effects would be larger for targets tested at 120° and 240° rather than 0°.

Method

Subjects and design. Two hundred and sixteen undergraduate student volunteers participated for course credit. The design had two between-subjects factors -- test condition (explicit, implicit), and study/test delay (immediate, 1-week), as well as four within-subject factors -- object type (non-cardinal, cardinal), history (studied, non-studied), display orientation at study (0°, 120°, 240°), and display orientation at test (0°, 120°, 240°). The subjects were assigned randomly to test conditions, with 96 receiving

the identification test (48 and 48 in the immediate and 1-week delay condition, respectively), and 120 receiving the recall and recognition test (48 and 72 in the immediate and 1-week delay condition, respectively).

Materials. A new set of 186 color photos of common objects was obtained, each cropped to a rectangle with 240 vertical and 320 horizontal pixels, using the same equipment as in Experiment 1. Each object was then 'cut-out' from its background (so as to eliminate any distinguishing context information and shadows) by means of the Aldus PhotoStyler graphics package (PhotoStyler, 1992), superimposed on a circular white background disk which had a 240 pixel (100 mm) diameter, and the resulting picture was saved as a 240-vertical by 320-horizontal pixel rectangle (note: the rectangle space beyond the disk remained blank). Each object was scaled to 220 pixels, as in Experiment 1, and was centered on its background disk. A circular background was used, first, to eliminate the extra orientation information provided by the rectangular displays used in Experiment 1, and second, because the appearance of a circle (as opposed to a rectangle) does not vary across different display orientations (0°, 120°, and 240°).

Of the 186 photos, 120 were critical targets, 60 were fillers, and 6 were used for instructions and practice. The critical targets included 60 photos of cardinal objects[4] (e.g., a helicopter, a car) and 60 non-cardinal objects (e.g., scissors, a flashlight) (see Appendix A). An object was identified as cardinal if three independent raters agreed that it had a typical, upright spatial orientation in real life, whereas an object was categorized as non-cardinal if the raters' consensus was that it did not have a preferred or normal

orientation in space. Each set of critical targets was randomly divided into 12 equal subsets, which are identified with the labels C_1 to C_{12}, for cardinals, and N_1 to N_{12}, for non-cardinals. The photos used for practice and as fillers included objects of either kind (i.e., cardinal and non-cardinal), as well as some whose orientation status was ambiguous.

Procedure. The study and test procedure was identical to that of Experiment 1, with the following exceptions[5]. At study, subjects were shown a random list with 90 pictures, 9 subsets with cardinal objects, and 9 subsets with non-cardinal objects (see Table 3.1). Three subsets of each kind (cardinal, non-cardinal) were displayed in each orientation condition (rotated 0°, 120°, and 240° in the plane of the page) at the time of study. Counterbalancing ensured that across subjects, each picture subset was included equally often in the study list, in each orientation condition. For the recognition and identification test, we presented a random list with 180 pictures, including all 12 subsets of cardinal objects (9 studied and 3 non-studied), all 12 subsets of non-cardinal objects (9 studied and 3 non-studied), and all 60 fillers. As at study, the test pictures were also displayed at 0°, 120°, or 240°.

Table 3.1 illustrates the manner in which materials were assigned to study/test conditions for one subject. Specifically, of the three subsets that appeared in each orientation condition at study, one was displayed in each of the different orientation conditions at testing. Similarly, for the three subsets of non-studied photos of each kind (cardinal, non-cardinal), one subset was tested in each orientation condition. Across subjects, counterbalancing ensured that each subset appeared equally often in each of the

Table 3.1
An example of how cardinal (C) and non-cardinal (N) picture subsets were assigned for one subject in Experiment 2.

Study Display Orientation	Picture subsets	Test Display Orientation 0°	120°	240°
0°	C_1, C_5, C_9	C_1	C_5	C_9
	N_1, N_5, N_9	N_1	N_5	N_9
120°	C_2, C_6, C_{10}	C_2	C_6	C_{10}
	N_2, N_6, N_{10}	N_2	N_6	N_{10}
240°	C_3, C_7, C_{11}	C_3	C_7	C_{11}
	N_3, N_7, N_{11}	N_3	N_7	N_{11}
Non-studied	C_4, C_8, C_{12}	C_4	C_8	C_{12}
	N_4, N_8, N_{12}	N_4	N_8	N_{12}

9 conditions that were obtained by crossing study and test display orientations. Twenty fillers were also presented in each test display orientation; they were included so as to equate the ratio of old (studied) and new (non-studied) items on the test. The fillers were also used in a counterbalanced manner so that across subjects, each appeared equally often in each display orientation condition.

Results

Test performance was scored and summarized as in Experiment 1. A preliminary analysis revealed that 15 pictures (8 cardinals and 7 non-cardinals) were often identified incorrectly (according to the same criteria as used in Experiment 1), and thus, to eliminate their confounding influence, they were excluded from analysis. The data from 105 critical pictures remained available for analysis.

Recall and recognition. On the recall test, subjects recollected a larger proportion of the studied pictures on the immediate test (.30) than on the delayed test (.13), and recall was higher for cardinal (.22) than non-cardinal objects (.17). An ANOVA with test delay (immediate, 1-week) as a between-subjects factor, and with study-phase display orientation (0°, 120°, 240°) and object type (non-cardinal, cardinal) as within-subject factors showed significant main effects for test delay, $F(1,118) = 230.79$, $MSe = .02$, $\eta^2 = .66$, and for object type, $F(1,118) = 79.24$, $MSe = .01$, $\eta^2 = .40$. No other effects approached significance. The delay effect is familiar from previous research. The recall advantage of cardinal over non-cardinal objects could reflect several factors, for example, a difference in the distinctiveness of the depicted objects, or a

difference in their familiarity. The study-phase rating data do not support the latter possibility, however; on average, subjects rated the non-cardinal objects as more familiar (3.62) than the cardinal objects (3.33), $F(1,214)^6 = 159.47$, $MSe = .17$, $\eta^2 = .43$.

As in Experiment 1, performance on the immediate recognition test was extremely high and limited by ceiling effects. Preliminary analyses showed that of all corrected recognition scores (i.e., hits-minus-false alarms) 58% were perfect, and the overall mean of these scores was .87 (with hits and false alarms averaging .93 and .05, respectively). Thus, the immediate recognition test data were not further analyzed. By contrast, performance on the delayed recognition test, shown in Table 3.2, was lower and off the ceiling. Overall, correct recognition of studied targets was comparable for cardinal (.81) and non-cardinal (.82) objects, but subjects made more false alarms on the latter (.20 vs. .28 on cardinal vs. non-cardinal objects, respectively). Because of the difference in false alarms, we computed corrected scores (hits-minus-false alarms), and these were submitted to an ANOVA which had object type, test orientation, and study/test condition (same vs. different) as within-subject factors. The ANOVA showed significant main effects for object type, $F(1,71) = 11.64$, $MSe = .08$, $\eta^2 = .14$, and for study/test condition, $F(1,71) = 4.65$, $MSe = .03$, $\eta^2 = .06$, with no other effects approaching significance. The latter effect reflects a 2% recognition advantage for objects that were displayed in the same (.59) versus different (.57) study/test condition. The recognition advantage of cardinal over non-cardinal objects parallels our free recall test findings and might also reflect the greater distinctiveness of the former objects.

Table 3.2
Recognition test performance accuracy and decision times (means, with standard errors in parentheses) after a 1-week delay for non-cardinal and cardinal objects, as a function of study orientation, test orientation, and history (studied/non-studied), in Experiment 2.

Accuracy (proportion correct)

Study display orientation	Test display orientation 0°	120°	240°
Non-cardinal			
0°	.81 (.026)	.83 (.027)	.80 (.025)
120°	.80 (.024)	.85 (.021)	.84 (.025)
240°	.80 (.025)	.82 (.024)	.83 (.020)
Non-studied	.27 (.026)	.26 (.025)	.32 (.030)
Cardinal			
0°	.81 (.024)	.80 (.025)	.81 (.025)
120°	.77 (.028)	.85 (.021)	.81 (.026)
240°	.76 (.024)	.82 (.024)	.82 (.022)
Non-studied	.18 (.024)	.21 (.024)	.22 (.025)

Decision times (ms)

Study display orientation	Test display orientation 0°	120°	240°
Non-cardinal			
0°	992 (29)	1090 (28)	1070 (31)
120°	1069 (29)	1042 (26)	1027 (24)
240°	1070 (30)	1038 (27)	1030 (27)
Non-studied	1105 (31)	1194 (38)	1144 (32)
Cardinal			
0°	1041 (24)	1167 (34)	1163 (31)
120°	1101 (28)	1059 (24)	1146 (31)
240°	1072 (31)	1113 (31)	1083 (26)
Non-studied	1112 (31)	1242 (36)	1217 (32)

The *recognition decision time data* from the delayed test, also included in Table 3.2, showed that subjects were faster to make correct decisions on old, studied targets (1076 ms) than on new, non-studied (1169 ms) targets. The data from the latter targets (i.e., correct rejections) were examined in an ANOVA with object type and test orientation as within-subject factors; the results showed a significant main effect for test orientation, $F(2,142) = 9.66$, $MSe = 46324$, $\eta^2 = .12$, and a marginal effect for object type, $F(1,71) = 3.67$, $MSe = 53240$, $\eta^2 = .05$, $p = .06$. Because of these effects, the decision time data from all other conditions were used to compute facilitation scores (i.e., the difference in subjects' decision time on hits minus correct rejections). An ANOVA of the facilitation scores, with object type, test orientation, and study/test condition as within-subject factors showed a significant main effect for test orientation, $F(1,142) = 18.99$, $MSe = 24463$, $\eta^2 = .21$, with more facilitation on objects displayed at 120° (133 ms) and 240° (94 ms) than at 0° (51 ms). There was also a significant effect due to study/test condition, $F(1,71) = 18.51$, $MSe = 32503$, $\eta^2 = .21$, with more facilitation for displays that were in the same orientation (128 ms) rather than in different orientations (75 ms) at study and test. No other effects approached significance.

Identification and priming. The identification test data were screened for outliers and summarized as in Experiment 1. Incorrect identifications were made on 3.4% of all test trials; the data from these trials had only a negligible effect and thus were left in the analyses that we report. Figure 3.2 shows the mean fade-in level that was required for identifying photos of objects in each experimental and control condition.

Overall, non-studied objects were identified equally easily in the immediate (134.92) and delayed (131.54) test condition, but they were easier to identify when the test display was at 0° (114.43), rather than at 120° (142.61) or 240° (142.65). An ANOVA of performance on non-studied items, with test delay (immediate, 1-week) as a between-subjects factor, and object type (non-cardinal, cardinal) and test orientation (0°, 120°, 240°) as within subjects factors, showed significant main effects for object type, $F(1,94) = 16.64$, $MSe = 674.02$, $\eta^2 = .15$, and for test orientation, $F(2,188) = 49.69$, $MSe = 1023.80$, $\eta^2 = .35$. The interaction between these factors was also significant, $F(2,188) = 14.44$, $MSe = 1215.71$, $\eta^2 = .13$. No other effects approached significance. Follow-up analyses showed that the test orientation effect was significant for cardinal objects, $F(2,190) = 97.39$, $MSe = 658.21$, $\eta^2 = .51$, but only marginal for non-cardinal objects, $F(2,190) = 2.77$, $MSe = 1563.68$, $\eta^2 = .03$, $p = .07$; moreover, a comparison of means and effect sizes (i.e., η^2) revealed that orientation had a much larger influence on baseline identification of cardinal objects. These results confirm that the selection of cardinal and non-cardinal objects was successfull: Non-cardinal objects were identified equally easily regardless of their orientation in the plane of the page whereas cardinal objects were much more difficult to identify when misoriented from their usual orientation.

The means in Figure 3.2 reveal priming -- a lower level of fade-in or fewer visible pixels required for identifying studied (97.08) than non-studied (133.23) photos -- in all experimental conditions. Because of the differences in performance on non-studied objects, priming scores were used for all statistical analyses. The amount of priming was

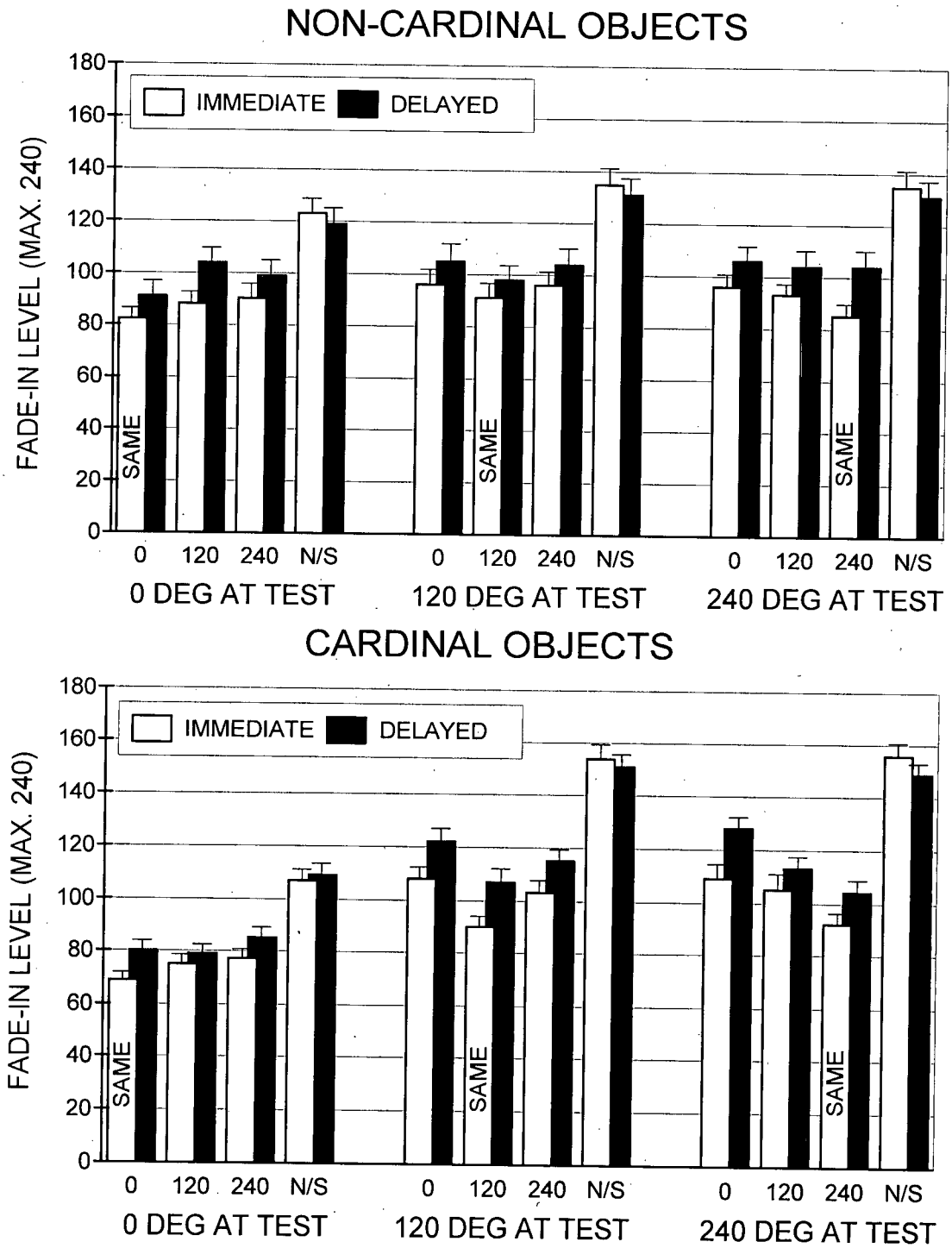

Figure 3.2. The mean fade-in level required for identifying cardinal and non-cardinal objects either on immediate or delayed test, as a function of study orientation (0°, 120°, 240°), test orientation (0°, 120°, 240°), and history (studied, non-studied), in Experiment 2. T-bars indicate standard errors of means.

significant in all experimental conditions; the smallest effect, $t(95) = 9.12$, $p < .001$, was for non-cardinal objects in the different study/test conditions, displayed at 0°. Overall, there was more priming on the immediate (43.49) versus delayed (28.81) test, for cardinal (39.74) versus non-cardinal (32.57) objects, and with test displays at 120° (39.82) or 240° (39.27) versus 0° (29.37). In addition, there was more priming when study and test displays were in the same orientation (42.15) rather than in different orientations (33.16). These observations were confirmed by an ANOVA of the priming scores that had test delay as a between-subjects factor, and object type, test orientation, and study/test condition (same, different) as within-subject factors. The ANOVA showed significant main effects for test delay, $F(1,94) = 24.08$, $MSe = 2667.89$, $\eta^2 = .20$, object type, $F(1,94) = 37.71$, $MSe = 483.93$, $\eta^2 = .29$, test orientation, $F(2,188) = 18.69$, $MSe = 796.68$, $\eta^2 = .17$, and study/test orientation, $F(1,94) = 37.14$, $MSe = 626.73$, $\eta^2 = .28$. The analysis also showed a significant two-way interaction between object type and test orientation, $F(2,188) = 5.72$, $MSe = 634.27$, $\eta^2 = .06$, a marginal two-way interaction between object type and study/test orientation, $F(1,94) = 2.88$, $MSe = 555.11$, $\eta^2 = .03$, $p = .09$, as well as a marginal three-way interaction among object type, test orientation and study/test condition, $F(2,188) = 2.71$, $MSe = 569.05$, $\eta^2 = .03$, $p = .07$. No other effects approached significance.

In part to gain insight into the triple interaction, the priming scores obtained with cardinal and non-cardinal objects were examined separately with planned ANOVAs that had test orientation and study/test condition as within-subjects factors. For non-cardinal

objects, the ANOVA showed a significant main effect for study/test condition, $F(1,94) = 7.53$, $MSe = 840.89$, $\eta^2 = .07$, with no other effects approaching significance. By contrast, for cardinal objects, the ANOVA showed significant main effects for test orientation, $F(2,188) = 35.26$, $MSe = 471.05$, $\eta^2 = .27$, and for study/test condition, $F(1,94) = 54.39$, $MSe = 340.95$, $\eta^2 = .37$, as well as a significant interaction between these factors, $F(2,188) = 3.20$, $MSe = 569.21$, $\eta^2 = .03$. Follow-up tests showed a significant same/different effect for test displays at 120°, $F(1,95) = 13.85$, $MSe = 636.98$, $\eta^2 = .13$, and at 240°, $F(1,95) = 23.20$, $MSe = 536.20$, $\eta^2 = .20$, but only a marginal effect for objects tested at 0°, $F(1,95) = 3.14$, $MSe = 294.67$, $\eta^2 = .03$, $p = .08$.

Discussion

The following findings are most critical: (a) baseline condition performance showed a large orientation effect for cardinal objects and only a marginal effect for non-cardinal objects, (b) orientation specific priming effects occurred with all items but they were much larger with cardinal than non-cardinal objects, and (c) for cardinal objects significant orientation-specific priming effects occurred mainly with test displays in unusual orientations (120° or 240°). These findings tend to support the main hypothesis of Experiment 2 that orientation specific baseline and priming effects would occur only for cardinal objects but not for non-cardinal objects, but we did find small orientation specific effects with non-cardinal objects where none were expected.

To garner more solid support for our main hypothesis, we conducted follow-up analyses that compared performance on that 1/3 of all targets that showed the least

orientation specificity under baseline conditions versus performance on that 1/3 of all targets that showed the most orientation specificity. As in Experiment 1, we defined orientation specificity as the cost in fade-in levels required for identifying a target from a mis-oriented (120° or 240°) versus an upright display. Analyses of baseline performance showed no orientation specific effects for the selected third of the non-cardinal targets, $F(2, 68) = .08$, $MSe = 270.77$, $\eta^2 = .00$, together with a large orientation specific effect for the selected third of the cardinal targets, $F(2, 68) = 172.15$, $MSe = 510.44$, $\eta^2 = .84$, thereby providing even clearer support for our main hypothesis. In all other respects, statistical analyses of performance on the selected targets showed the same results as found in the original analyses.

Experiment 3

Experiment 3 was designed to replicate and extend Experiment 2, as well as to clarify the pattern of orientation specific priming effects found with cardinal and non-cardinal objects. For this purpose, each target item in Experiment 3 was displayed against the background of a full-screen picture that was selected so as either to highlight or to minimize orientation information. To further emphasize the encoding of orientation information, we also used a study task that focused subjects' attention on the orientation in which targets were displayed. In all other respects, the general method was the same as for Experiment 2. On the assumption that priming is mediated by the study/test overlap in processing, we reasoned that orientation specific effects should be larger on objects encountered under conditions that highlight this kind of information.

Method

Subjects and design. One hundred and ninety-two undergraduate student volunteers participated for course credit. The design had two between-subjects factors -- object type (non-cardinal, cardinal), and background type (orientation absent, orientation present), as well as three within-subject factors -- history (studied, non-studied), display orientation at study (0°, 120°, 240°), and display orientation at test (0°, 120°, 240°). The subjects were assigned randomly, with 48 in each of the four conditions defined by combining the two between-subjects factors.

Materials. The set of 186 photos from Experiment 2 was used. The few target objects that were difficult to identify or that tended to elicit identification errors in Experiment 2 were switched with suitable objects from the filler set. In all other respects, the photos were prepared and arranged into subsets as described in Experiment 2.

We also required two sets of 7 background images, identified as orientation-present and orientation-absent, respectively. Of the orientation-present images, five were colorful nature scenes (e.g., a forest meadow) and two were color photos of city scapes (see Figure 3.3). The selection of these backgrounds was guided by two factors: Each picture contained an abundance of vertical and horizontal orientation information, and they were all clearly different from each other.

Each of the orientation-absent images was derived from one of the orientation-present images. To produce an orientation-absent image, we used various image processing tools from Aldus PhotoStyler (PhotoStyler, 1992). We 'cut up' the

Figure 3.3. Examples of the orientation present (top panel) and orientation absent (bottom panel) backgrounds used in Experiment 3. Color versions of such displays were used in the experiment.

orientation-present image into 40 by 40 pixel squares, and randomly rearranged and reoriented (e.g., flipped upside-down) these squares. Then we cut out, rearranged and reoriented various other segments from the resulting collage and used several 2- and 3-D special effect tools (e.g., pinch, whirlpool, blend) to eliminate any remaining horizontal and vertical lines. The goal was to obtain an image that had the same amount and range of colors as the original, but a minimum of orientation information. Each orientation-present and -absent picture was stored as a 480 by 640 pixel image. Five images from each set were used as display backgrounds at study and test, while the remaining two were used for instruction and practice.

Procedure. The study and test procedure was the same as in Experiment 2, with the following exceptions. At study, each subject was shown a randomly arranged list of 45 target pictures (9 subsets with 5 pictures each), with each displayed in the center of either an orientation-absent or -present background image. On each study trial, the software procedure randomly selected and displayed a background image (either orientation-absent or -present, depending on condition), with one of the critical targets superimposed and centered on top of it. The background images were always displayed in the same upright orientation, whereas the targets were displayed at 0°, 120° or 240° (3 subsets in each orientation) as described in Experiment 2 (see Table 3.1). For each subject, each background image was used with 9 randomly selected different targets at study. The subjects' task was to rate "how easy it is to identify the target in the orientation in which it is displayed". They used a 5-point scale for this purpose, with 1

and 5 indicating "very difficult" and "very easy" to identify, respectively. Subjects entered their ratings on the keyboard, and doing so triggered the next study trial.

Immediately after study, memory was assessed with an identification test as described in Experiment 2. The identification test list had 45 studied targets (9 sublists with 5 each), 15 non-studied targets (3 sublists with 5 each), and 30 fillers. Each object was faded-in on top of a background image. On each test trial, the procedure randomly selected an object (target or filler), displayed a background image, and then faded-in the object on top of it as described in Experiment 2. Each studied target was tested in the context of the same background image as in the study phase, with the target's orientation being either the same or different, depending on test condition (as described in Experiment 2). The 15 non-studied targets and 30 fillers were also displayed against background images; a randomly selected 3 of the former and 6 of the latter were paired with each image. The materials were counterbalanced across subjects and they made identification decisions as described in Experiment 2.

Results

Test performance was scored and summarized as in the preceding experiments. Preliminary screening showed that none of the pictures were frequently misidentified (according to the criteria defined in Experiment 1) and thus all were available for analysis. The data were also inspected for outliers and errors; 1.7% of all data points were identified as outliers and these were adjusted as in the preceding experiments.

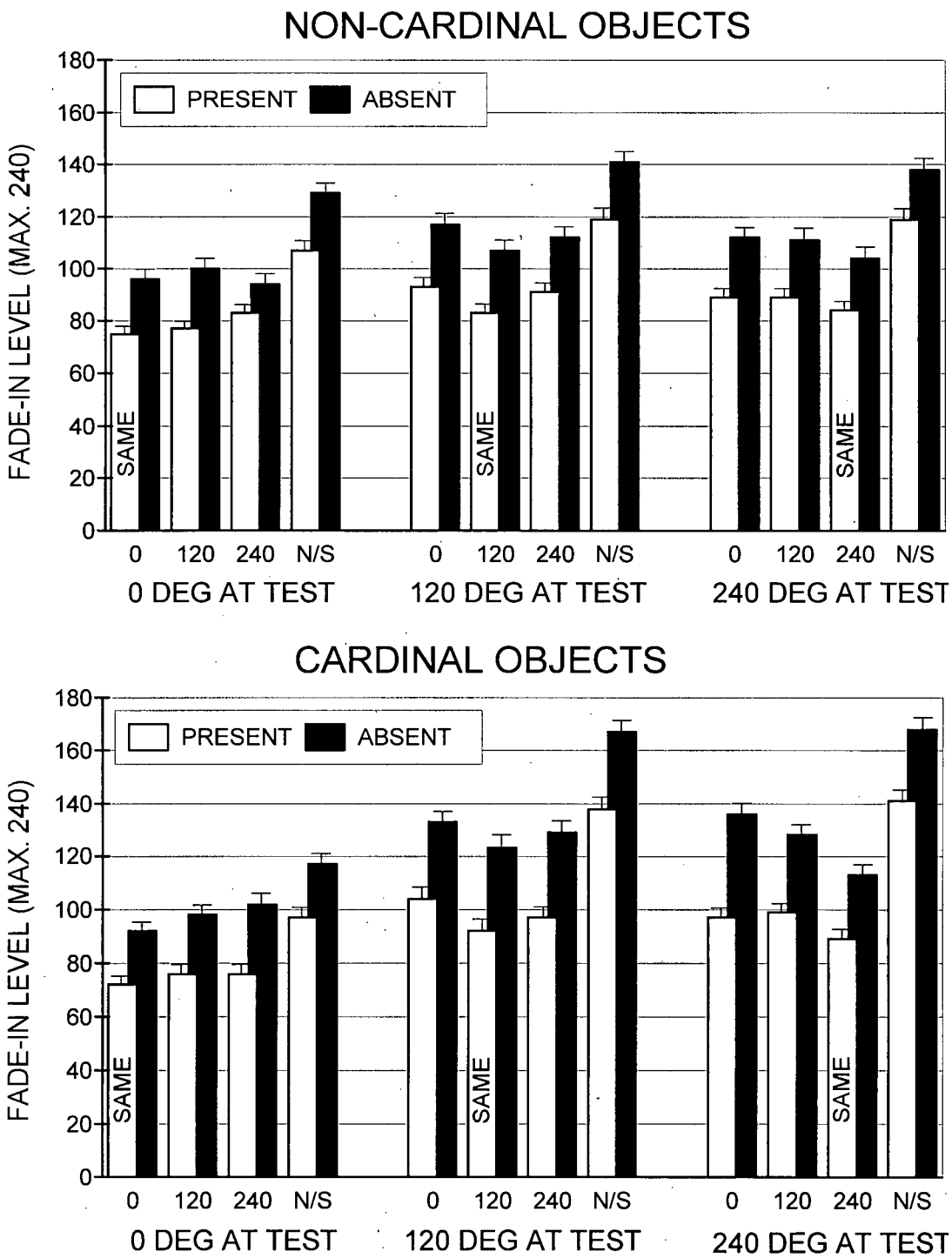

Figure 3.4. The mean fade-in level required for identifying cardinal and non-cardinal objects displayed against orientation-present or -absent background images, as a function of study orientation (0°, 120°, 240°), test orientation (0°, 120°, 240°), and history (studied/non-studied), in Experiment 3. T-bars represent standard errors of means.

Incorrect identifications were made on 2.5% of all test trials; the data from these trials had a negligible effect and thus were left in the analyses that we report.

Figure 3.4 shows the mean fade-in level that was required for identifying photos of objects in each experimental and control condition. Overall, non-studied objects were easier to identify when displayed against an orientation-present (120.13) versus -absent (143.50) background, and if they were non-cardinal (125.28) rather than cardinal (138.36). Non-studied objects were also easier to identify when they were displayed at 0° (112.91) versus 120° (141.17) or 240° (141.37), but test orientation had a larger effect on identification of cardinal than non-cardinal objects. An ANOVA of performance on non-studied objects, with object type (cardinal vs. non-cardinal) and background type (orientation-present vs. -absent) as between-subjects factors, and test orientation as a within-subject factor, showed significant main effects for object type, $F(1,188) = 18.51$, $MSe = 1331.95$, $\eta^2 = .09$, background type, $F(1,188) = 59.05$, $MSe = 1331.95$, $\eta^2 = .24$, and test orientation, $F(2,376) = 116.36$, $MSe = 442.30$, $\eta^2 = .38$. There was also a significant interaction between object type and test orientation, $F(2,376) = 43.11$, $MSe = 442.30$, $\eta^2 = .19$. Separate ANOVAs of performance on each object type revealed a significant effect due to display orientation on cardinal objects, $F(2,190) = 143.04$, $MSe = 465.06$, $\eta^2 = .60$, as well as on non-cardinal objects, $F(2,190) = 9.66$, $MSe = 415.40$, $\eta^2 = .09$. However, as in Experiment 2, a comparison of the means in Figure 3.3 and of effect sizes indicates that test orientation had much a larger influence on baseline identification of cardinal than non-cardinal objects.

The data in Figure 3.4 also show priming -- a lower level of fade-in required for identifying studied (99.24) than non-studied (131.82) photos -- in all experimental conditions. Because of differences in performance on non-studied objects, priming scores were computed as in the preceding experiments and these were used for all statistical analyses. The amount of priming was significant in all experimental conditions; the smallest effect, $t(47) = 6.98$, $p < .001$, was for cardinal objects when tested at 0° orientation, in the different study/test condition. Overall, there was more priming on cardinal (35.25) than non-cardinal (29.91) objects, and on targets that were tested at 120° (34.44) or 240° (37.20) rather than 0° (26.10). More important, there was more priming on targets that were studied and tested in the same orientation (37.64) rather than in different orientations (30.05), but this orientation-specific effect occurred only in some experimental conditions. The priming data were submitted to an ANOVA with object type (cardinal vs. non-cardinal) and background type (orientation-present vs. -absent) as between-subjects factors, and test orientation and study/test condition (same/different) as within-subject factors. The ANOVA showed significant main effects for object type, $F(1,188) = 5.98$, $MSe = 1661.52$, $\eta^2 = .03$, test orientation, $F(2,376) = 49.91$, $MSe = 304.96$, $\eta^2 = .21$, and study/test condition, $F(1,188) = 49.80$, $MSe = 333.39$, $\eta^2 = .21$. There were also significant interaction effects between object type and test orientation, $F(2,376) = 46.96$, $MSe = 304.96$, $\eta^2 = .20$, and between test orientation and study/test condition, $F(2,376) = 3.66$, $MSe = 249.71$, $\eta^2 = .02$. No other effects, including no effects involving background type, approached significance.

To gain insight into the interaction effects, the priming scores obtained with cardinal and non-cardinal objects were examined separately with planned ANOVAs that had background type as a between-subjects factor, and test orientation and study/test condition as within-subject factors. For non-cardinal objects, we found a significant main effect for study/test condition, $F(1,94) = 14.64$, $MSe = 351.50$, $\eta^2 = .14$, with no other effects approaching significance. By contrast, for cardinal objects, the ANOVA showed significant main effects for test orientation, $F(2,188) = 89.12$, $MSe = 330.99$, $\eta^2 = .49$, and study/test condition, $F(1,94) = 38.72$, $MSe = 315.29$, $\eta^2 = .29$, as well as an interaction effect between these factors, $F(2,188) = 3.01$, $MSe = 276.27$, $\eta^2 = .03$. The latter interaction was examined by separate ANOVAs of the priming data from each test orientation which showed a significant study/test condition effect when tested with displays at 240°, $F(1,95) = 30.04$, $MSe = 304.11$, $\eta^2 = .24$, at 120°, $F(1,95) = 9.06$, $MSe = 353.13$, $\eta^2 = .09$, or at 0°, $F(1,95) = 7.09$, $MSe = 217.16$, $\eta^2 = .07$. Figure 3.4 and the effect size results indicate that orientation specific priming was much larger with test-displays at 240° than at 0° or 120°.

Discussion

These findings replicate all critical results from Experiment 2; they show orientation specific effects in baseline identification performance with much more specificity on cardinal than non-cardinal objects, as well as orientation specific priming with larger effects on cardinal than non-cardinal objects. More surprising, we found that the background manipulation and the study task used to focus processing on orientation

information had only one effect: Targets displayed against orientation-present pictures were identified much more quickly than targets displayed against orientation-absent background pictures. An analysis of baseline condition performance showed that target identification in the orientation-present condition was faster -- by about 15 fade-in levels -- than was identification in the immediate condition of Experiment 2, $F(1,188) = 23.59$, $MSe = .19$, $\eta^2 = .11$, which in turn was faster -- by about 9 fade-in levels -- than identification in the orientation-absent condition, $F(1,376) = 8.10$, $MSe = 1309.23$, $\eta^2 = .04$. This pattern of findings suggests that our orientation-present backgrounds had a facilitative influence on identification performance, whereas the orientation-absent backgrounds had a detrimental influence. This kind of outcome is familiar from previous research, for example, by Pomerantz (1981) who found that the orientation of lines was easier to determine when the lines were embedded in meaningful figures than when they were embedded in meaningless or disorganized figures (see also Boyce, Pollatsek, & Rayner, 1989).

The fact that the background manipulation together with a study task that focused processing on orientation had no effect on priming might be explained by a weak experimental manipulation. Arguing against this possibility, however, the data showed that the background image manipulation was clearly strong enough to influence baseline identification performance. Moreover, we found that this manipulation was strong enough also to influence another aspect of performance. Recall that the study task required subjects to rate "how easy it is to identify the target in the orientation [and

background] in which it was displayed." The rating data showed no difference across background conditions, $F(1,188) = 2.14$, $MSe = 1335.28$, $p = .15$, but were sensitive to and showed a strong effect for display orientation, $F(1,188) = 228.90$, $MSe = .09$, $\eta^2 = .55$. More important, we also recorded the time subjects spent on making their study trial ratings, and these data showed that it took less time to make decisions in the orientation-present (2142 ms) than -absent (2549 ms) condition, $F(1,188) = 7.02$, $MSe = 3396445$, $\eta^2 = .036$. These data are additional proof that the background manipulation was effective. However, on this evidence and the fact that priming did show substantial orientation specific effects, it appears that the processing of orientation information must occur automatically, that is, it is not controlled by situation and task constraints, at least not under the limited study and test conditions of the present experiment. Orientation information is critical for many aspects of cognition and thus may be processed automatically, in the same manner as the processing of spatial information or frequency of occurrence information (Hasher & Zacks, 1979; Jorm, 1986).

General Discussion

The main hypothesis that motivated the present investigation was that orientation information is not part and parcel of all long term or semantic memory representations (i.e., the processes that mediate performance on semantic memory tasks or an average computed across the relevant set of episodic representations or episodic instances), it is a property only of cardinal objects' representations, and as a corollary, orientation specific effects were expected to occur in priming only for cardinal but not non-cardinal objects.

The following findings are most relevant. Experiment 1 showed baseline identification was slower for objects displayed upside down rather than upright. Experiments 2 and 3 showed that baseline orientation-specific effects -- the performance cost associated with identifying unstudied objects in an unusual rather than their normal orientation -- are much larger for cardinal than non-cardinal objects. And in all three experiments, priming effects were larger under those conditions and for those targets where baseline identification seemed more difficult. We found a significant correlation between baserate identification performance and amount of priming across all 28 major conditions of Experiments 1, 2 and 3, $r = .68$, $p < .001$. And finally, we found that orientation specific priming effects (more priming when study and test orientation matched rather than mismatched) occurred for both non-cardinal and cardinal objects, but the size of these effects was much larger with cardinal objects.

We interpret the baseline findings as reflecting a property of long-term memory representations, which we call *orientation-fixation*, or how strongly each representation is marked by information about a target's usual orientation. Targets that are highly orientation specific (cardinal objects) must have semantic memory representations with more orientation information; we can say that they are more orientation fixated than are non-cardinal targets. An orientation specific priming effect (defined as the additional priming with matching vs. mismatching display orientations at study and test) also provides an index of orientation fixation but of newly established representations that record a particular recent episode. In our experiments, the overall amount of priming in

each condition can be divided into two portions. More important for now is the orientation specific portion, defined as

$$\frac{\text{amount of priming in same condition - amount of priming in different condition}}{\text{amount of priming in same condition.}}$$

This portion of priming averaged .20 in our experiments. Statistical analyses showed that this specific portion of priming was correlated with baseline performance, $r(24) = .365$, $p < .08$. We also found that this correlation occurred only for the cardinal objects, $r(12) = .535$, $p < .07$, but not for non-cardinal objects, $r(12) = -.085$. Therefore, if this portion of priming reflects the orientation fixation of episodic representations, it appears that the orientation fixations of semantic and episodic memory representations are positively correlated. One possibility is that this correlation reflects the fact that when baseline processing is slowed down for whatever reason (e.g., because a target is shown in an unusual orientation), the system compensates in such a way that identification performance comes to depend more on input from episodic memory representations. Thus, if episodic representations are orientation-specific, and if they suddenly contribute more to target identification, we will find bigger orientation-specific priming effects.

Our findings show many similarities with those from prior research with familiar and unfamiliar words and pseudo-words, and how they seem to be represented in semantic and episodic memory. On word identification tasks, baseline identification occurs faster for familiar than unfamiliar words or pseudo-words; our findings show that depending on orientation-fixation, cardinal objects are identified more quickly in their

usual rather than a new, unfamilar orientation. We assume that both of these effects reflect the strength of one aspect of episodic and semantic memory representations.

Previous experiments with words have shown that overall word familiarity is inversely related to the size of priming effects, and by contrast, our findings show that targets' cardinality (orientation fixation) is positively correlated with amount of priming. Therefore, if we consider priming to reflect the strength of episodic memory representations, it appears that the correlation between strength of semantic and episodic memory representations is different for familiarity and for cardinality: positive for one and negative for the other. Why the difference? Consider that only cardinal objects have a usual or fixed orientation in real life, and thus, only cardinal objects can be displayed in an unusual, unfamiliar orientation (at 120° or 180°); non-cardinal objects have no *usual* orientation and thus appear equally familiar in all orientations. Therefore, if we translate the orientation manipulation from our experiments in term of familiarity, our findings tell us that cardinal targets displayed in their more familiar orientation were more easily and quickly identified and were associated with smaller priming effect, compared to when the same targets were displayed in an unfamiliar orientation. Thus, for both familiarity and cardinality (translated in terms of familiarity), we now have a negative correlation between semantic and episodic memory representation strength.

How do we conceptualize semantic and episodic memory representations and the manner in which they influence baseline condition and priming effects? At the most fundamental level, we share the notion that memory test performance reflects the degree

of processing overlap between study and test (Graf, 1994; Graf & Ryan, 1990; Masson & MacLeod, 1992; Morris et al., 1977; Roediger et al., 1989). We postulate semantic memory representations that consist of collections of neurons that have become functionally integrated or unitized as a pattern or map, and we assume that familiar, as opposed to unfamiliar, items have *more strongly unitized* representations, meaning that their representations can be re-activated more easily or quickly (Feustel et al., 1983; Graf & Mandler, 1984; Graf & Schacter, 1989; Graf et al., 1984). By these assumptions, we interpret the baseline findings from previous word-familiarity experiments -- higher identification test speed or accuracy for familiar words -- as reflecting their stronger unitized semantic memory representations. If one grants that only cardinal objects can be displayed in familiar or unfamiliar orientations, the baseline findings from our experiments show the same relation and thus have the same interpretation. Object identification performance in our experiments was positively correlated with identifying cardinal targets shown in familiar orientations, and we interpreted this finding as reflecting strength differences in the orientation fixation of semantic memory representations.

Biederman's (1987) geon model for object recognition offers a concrete way to illustrate how this might occur. According to Biederman, objects are represented in terms of geons, basic geometric object building blocks, and special terms that define relations between or among them (e.g., geon A is *above* geon B, geon C is *to-the-right-of* geon B). Therefore, if a cardinal object is displayed in an unfamiliar, novel orientation, the

relational terms will mismatch and identification requires that the system carries out special computations to resolve this conflict; for this reason, performance will be slower or less accurate. Non-cardinal objects may have semantic memory representations that require no or only minimal relational terms to organize their geons, or they may be stored by means of multiple representations that together encompass many different display orientations (Edelman & Bulthoff, 1992; Jolicoeur, 1992; Tarr, 1992; Tarr & Pinker, 1989). In either case, such objects will be identified equally easily in any orientation.

We postulate that semantic memory representations' degree of unitization is the cumulative product of experience. Each time a target is perceived, a particular set of neurons is simultaneously engaged (activated/inhibited), and this pattern of activity or map, and what happens to it over time, represents the target in terms of how it was processed on one particular occasion (Mandler, 1980; McClelland & Rumelhart, 1985). In this way, a new episodic memory representation (a new instance) is established. A familiar target has been encountered many times and thus will have many more such maps. By these assumptions, the strength of unitization of semantic memory representations can be conceptualized as an average or generalization computed across such maps that becomes more stable or reliable (in a statistical sense) with each additional map that is established.

We interpret the size of priming effects to reflect the unitization of the newly established episodic memory representation, and we explain priming differences in terms of how the system processes information at study and at test. At study, identifying a

highly familiar target seems to require only minimal sensory/perceptual input to reactivate the target's semantic memory representation (which is strongly unitized), and thus the episodic trace that is established at study will contain only minimal information to distinguish each particular occurrence. By contrast, an unfamiliar target with a less unitized semantic memory representation would require more extensive data and situation specific (bottom-up) processing to activate its pre-existing representation, and therefore, the new representation established during the study trial would have more information to distinguish each instance of the target's occurrence. At the time of testing, the system seems to optimize its performance by relying on both semantic and episodic representations, in proportion to their degree of unitization, and thus, it follows that targets with strongly unitized semantic memory representations are associated with weaker episodic memory representations. Our results show this negative correlation between baseline and priming effects in the domain of memory for orientation information, thereby extending previous work that showed the same association in relation to word familiarity.

Chapter 4

Object size and relative size in semantic and episodic memory

The experiments reported in this chapter examined the influence of two attributes of objects -- size and relative size -- on different types of memory tests. This research is a direct extension of the experiments reported in the previous chapter. There, the goal was to investigate the extent to which the spatial orientation of objects influences baseline performance (i.e., identification without prior study), how it affects priming, and how it influences performance on explicit memory tests. The goal of the research reported in this chapter is to investigate whether objects' size and objects' relative size are attributes that behave the same way as objects' spatial orientation, or whether they show a different pattern of influences on baseline identification, on priming, as well as on performance of explicit memory tests.

In the preceding chapter, the finding that orientation influenced baseline performance was interpreted as reflecting the fact that orientation information is part and parcel of the semantic memory representation (i.e., the processes that mediate performance on semantic memory tasks, an aggregate of the relevant set of episodic representations) of objects. This chapter addresses a similar issue: whether objects' size and objects' relative size are also included in their semantic memory representations. The orientation effects on priming in the previous chapter were used to argue that orientation information is also included in objects' episodic memory representations. Similarly, the

question for this chapter is whether size and relative size are also encoded in the episodic representations that mediate priming.

The size of objects, like their orientation, may have no influence on identification and priming under a variety of conditions. This expectation follows directly from perception research which has emphasized that objects tend to be perceived as the same size despite changes in their retinal size that are brought about by changes in viewing distance (e.g., Coren, Ward, & Enns, 1994; Gregory, 1978; Rock, 1983). Similarly, the size of familiar objects in photos does not look odd despite the variety of sizes in which the objects can be displayed. These and similar observations indicate that size changes may have no effect on object identification, and they suggest that size information may not be encoded in the episodic representations that mediate priming.

The size of objects seems important for object identification under at least some conditions, however. For example, objects that are far away are more difficult to identify than objects that are nearby, especially when the observer is in less than optimal viewing conditions, such as in fog or in haze. And some objects can be identified only on the basis of their relative size. A violin, for example, differs from a viola by size only (in terms of visual properties).

The last example also underscores the importance of objects' context or setting on identification. In everyday life, we use various cues for identification, especially the setting in which an object occurs. If the size of an object, with respect to its setting (i.e., an object's relative size), violates our pre-existing knowledge, the object may appear odd,

either too large or too small. It seems reasonable to assume that for objects that violate our size expectations, their relative size may be encoded and become part of episodic memory representations, and in turn, we might expect an influence on priming due to relative size manipulations. The following paragraphs briefly review previous research on the influence of size and relative size on object identification, on priming, as well as on explicit memory for objects.

Effects of size on identification and priming. Biederman and Cooper (1992) have recently shown that the size of line-drawings of common objects influences naming speed of non-studied items (i.e., baseline performance). On each trial, they presented a line-drawing of an object for 100 ms, followed by a mask, on a computer monitor in one of six sizes: 2°, 3°, 4°, 5°, 6°, and 7° of visual angle. Subjects were required to name each object as quickly as possible. Biederman and Cooper found a parabolic relationship between object size and naming times; intermediate sized (5°) objects were named in only 711 ms, whereas both the smallest objects (2°) and the largest objects (7°) were named more slowly, in 789 ms and in 758 ms, respectively.

Biederman and Cooper (1992) also examined the influence of object size on priming on a picture naming test. For this experiment, they briefly presented line-drawings of objects on a computer monitor; each object was displayed either in a small size (3.5°) or a large size (6.2°). These sizes were selected to keep identification performance comparable in a baseline condition (i.e., without prior study of objects). Subjects were asked to name line drawings for two blocks (a study block and a test block)

of 32 trials, as quickly as possible. On the test block, the line drawings were shown either in the same size as at study or in a different size (small at study and large at test; large at study and small at test). The pattern of performance on the study block (baseline condition) showed that the size of objects did not influence naming speed. More important, performance on the test block showed that study-to-test changes in size did not influence the magnitude of priming. Cave and Squire (1992), and Cooper et al. (1992) also found that study-to-test changes in the size of objects have only a minimal effect or no effect on the amount of priming. Collectively, these findings suggest that size is not part of, or is at best only minimally implicated, in the episodic representations that mediate priming of objects (e.g., Biederman & Cooper, 1992; Cave & Squire, 1992; Cooper et al., 1992). This outcome may not be surprising, however, when -- as in the existing research -- the size manipulation is too small even to influence baseline object identification. There can be no question that a sufficiently large size manipulation will influence baseline performance and the critical question is: What is the influence of size in this situation?

Effects of size on explicit memory. The effect of size on explicit memory test performance has been investigated using two different tests: old/new recognition which requires subjects to decide whether each target did or did not appear in a prior study list (regardless of its size), and size recognition which requires subjects to decide whether targets appeared previously in the same size. Kolers, Duchnicky, and Sundstroem (1985) examined the influence of size with both of these test types. They presented subjects with

a long series of randomly arranged words and photos of faces, and each word or face was displayed in 1 of 5 sizes: 2°, 4°, 6°, 8°, or 10° of visual angle. Each target appeared twice in the series, and on the second presentation, it appeared either in the same size or in a different size. Subjects were required to decide for each target whether it had appeared previously in the series (old/new recognition), and whether or not targets recognized-as-old had been shown previously in exactly the same size (size recognition). Kolers et al. found that size played an important role in old/new recognition of faces but not in recognition of words. Subjects were more accurate in making old/new decisions on large as opposed to small faces, and more importantly, they were more accurate on faces studied and tested in exactly the same size rather than in different sizes. By contrast, subjects were equally good at recognizing words regardless of their size and regardless of whether their size changed between their first and second presentation.

On the size recognition test, Kolers et al. (1985) found that both words and faces showed similar effects due to changes in size: Size recognition was high when items were either presented in the same size on the first and second encounter on a continuous recognition test, or when display sizes were most discrepant on the two encounters (2° on the first, 10° on the second). Size recognition was low when the sizes on the two encounters were changed some but not too much by only a few degrees of visual angle (e.g., 2° on the first, 4° on the second) (see also Robinson & Standing, 1992; Zimmer, 1995).

In a series of studies, Jolicoeur and his colleagues (Jolicoeur, 1987; Milliken & Jolicoeur, 1992) extended these findings by showing that study-to-test changes in object size have a detrimental impact on old/new recognition memory for other kinds of materials: abstract line-drawings (e.g., drawings of blobs, polygons, stick figures) and line-drawings of common objects. Jolicoeur (1987) found that study-to-test changes in size have a larger effect on recognition of abstract shapes, such as blobs and stick figures, than on recognition of line drawings of common objects. He also demonstrated that old/new recognition test performance was affected more by larger study-to-test changes in targets' size (i.e., 1:4 size ratio) than by smaller changes in size (i.e., 1:2 size ratio), at least when targets were abstract shapes. Milliken and Jolicoeur (1992) dissociated the effects due to the perceived size of objects from effects due to their retinal size, and they found that old/new recognition performance was affected primarily by perceived size.

Relative size. The influence of relative size on baseline identification, priming, and explicit memory has not been investigated extensively. In one relevant line of research, however, Biederman, Mezzanotte, and Rabinowitz (1982) examined the effect of violating familiar relations between objects and their settings (e.g., by manipulating the size of a sofa set in a living room) on subjects' ability to decide whether or not a target object is present in a given location. On each trial, subjects were presented with a target name, followed by a cue pointing either to the location where the target would appear or to another location. Immediately after the presentation of the cue, a target or a distractor object, embedded in a line-drawn scene, appeared for 150 ms and was followed by a

mask. Subjects' task was to decide as quickly and accurately as possible whether the object shown in the cued location was the target. The results showed that when objects were presented in unfamiliar sizes, either too large or too small relative to their setting, subjects made more errors and they took longer to make their decisions.

To the extent that the test task used by Biederman et al. (1982) is valid, these findings indicate that relative size manipulation can influence the speed of identifying objects. But it is also possible that the size manipulation that was used changed the whole scene by masking parts of the scene that were or were not visible when objects were shown in different sizes. For this reason, it is difficult to separate any influences due to the relative size manipulation from influences due to other changes in scene composition.

The combined results of previous research suggest that objects' size has little or no effect on the magnitude of priming, at least for cases where size also has no influence on baseline performance. In contrast, previous findings suggest that size is an important property for deciding whether targets are recognized as old, and for deciding whether targets were studied in the same size as shown on the test. The findings also highlight that the effect of size manipulation on old/new recognition varies with the kind of to-be-remembered materials and with the magnitude of the size change between study and testing.

By contrast to these relatively clear findings, however, far less is known about the influence of object size on baseline identification. Biederman and Cooper (1992) found a

significant influence due to size on baseline identification of objects, but other investigators either did not report performance in the baseline condition or they reported no effects or only minimal effects due to size on baseline performance. Still less is known about the influence of relative size of objects on baseline identification performance, on priming, and on performance on explicit memory tests.

Present experiments. The two experiments reported in this article were designed to investigate the influence of relative size on priming and explicit memory, and to collect more and stronger evidence on how size influences baseline identification, priming, and explicit memory. These experiments were also intended to generalize previous findings to new tasks, materials, and test procedures.

Experiment 1 examined the influence of object size on identification performance and on old/new recognition performance, for both studied and non-studied items. The goal was to extend previous research in a number of different ways. Experiment 1 employed a broader range of object sizes: 4°, 8°, and 16° of visual angle. It used a different method for presenting targets at testing (a fade-in procedure) where target-relevant information accumulates over time. The fade-in procedure mimics the perceptual experience of driving towards a target in less than ideal weather conditions, such as fog or rain. The same method was used to present targets for both implicit and explicit memory tests, except that different instructions were given to subjects. Experiment 1 also included two delays between study and test: a few minutes versus one week. Finally, Experiment 1 investigated the effect of size with new materials, color

photographs of common objects, that may be more familiar to subjects and that may include more information (e.g., color, shading) than the line drawings and abstract shapes that have been used for previous investigations.

The second experiment in this series was designed to examine the influence of relative size of objects on implicit and explicit memory test performance, for both studied and non-studied targets. Although the size of objects displayed alone may exercise only a minimal or no influence on perception and priming, the size of objects displayed in an unusual relative size, for example, a baby-stroller that looks much larger than a space-shuttle, violates our knowledge of how things ought to be. In turn, this may influence how quickly we can identify such objects, and it may also affect priming and explicit memory test performance.

Experiment 1

The subjects in Experiment 1 studied a series of color photos of real-life objects displayed in one of three sizes: small (4° of visual angle), medium (8° of visual angle), and large (16° of visual angle). At test, the photos were displayed either in the same size as at study or in a different size. Memory was assessed either with an identification test or with an old/new recognition test. On both tests subjects were shown photos of objects that were slowly faded-in. The identification test required subjects to identify each object as quickly as possible, whereas the old/new recognition test required them to decide for each object whether it had been seen at study. Subjects were tested twice: a few minutes after study and again after a 7-day delay.

Method

Subjects and design. Ninety-six undergraduate student volunteers participated for course credit or for $10.00. The design had one between-subjects and four within-subjects factors. The between subjects factor was test type (identification, old/new recognition). The within subjects factors were the size of objects at study (small, medium, large), the size of objects at test (small, medium, large), history (studied, non-studied), and test delay (a few minutes, 7 days). Forty-eight subjects were assigned randomly into each of the two conditions defined by the between-subjects factor.

Materials. A set of 188 color photos of real-life objects, such as a camera or a bus, were obtained and digitized using an HP ScanJet III color scanner so that the resulting images were 1280 pixels wide and 960 pixels high. Each object was 'cut out' from its background by means of the Aldus PhotoStyler graphics software (PhotoStyler, 1992), and then scaled to fill three rectangles of different sizes: 640 by 480 pixels, 320 by 240 pixels, and 160 by 120 pixels. The scaled objects were then superimposed and centered on 640 by 480 pixels white background. Of the 188 photos, 120 were critical targets (sets denoted by C), 60 were fillers (sets denoted by F), and 8 were used for instruction and practice. The 120 critical items were randomly divided into two equal sets (set C_A and set C_B) of 60 items. For purposes of counterbalancing items across subjects and conditions, each of these sets was then further subdivided into 12 equal subsets of 5 items (subsets C_{A1} to C_{A12} and subsets C_{B1} to C_{B12}). Similarly, the 60 fillers were randomly divided into 2 sets (set F_A and set F_B), and each of these sets of fillers was

divided into three equal subsets (subsets F_{A1} to F_{A3} and subsets F_{B1} to F_{B3}). All pictures were displayed on a 14-inch Acer color graphics monitor, driven by a TARGA+ color graphics card operating in 16.7 million color mode at a resolution of 640 by 480.

Procedure. The study was described as examining perception and memory for pictures of objects. Subjects were tested individually in two sessions, separated by 7 days, each lasting from 30 to 50 minutes. The first session had a study phase and a test phase. The study phase was the same for all subjects. On each trial, a photo of an object was displayed in the center of the computer monitor, in one of the three sizes (small, medium, or large), and the subjects' task was to rate its familiarity. To make the ratings, subjects used a 5-point scale which had 1 -- not very common and 5 -- very common at its endpoints, and they responded by entering their ratings on the computer keyboard. Subjects practiced the rating task on three photos, proceeding at their own pace. Each key-press blanked the screen for about 2 s and triggered the display of the next photo. Following the practice phase, a randomly arranged list with 90 photos was presented according to the same procedure. Each subject was shown 30 photos in small size, 30 photos in medium size, and 30 photos in large size.

Table 4.1 illustrates the manner in which photos were assigned to study/test conditions for one subject. It shows that the study list included 9 subsets from each set of critical items (e.g., C_{A1} to C_{A3}, C_{A5} to C_{A7}, C_{A9} to C_{A11}, and C_{B1} to C_{B3}, C_{B5} to C_{B7}, C_{B9} to C_{B11}). From this list, six subsets were displayed in small size (e.g., C_{A1}, C_{A5}, C_{A9}, and

Table 4.1
An example of how photo subsets were assigned for one subject, in Experiment 1.

		First Test Display Size			Second Test Display Size		
	Photo subsets	**Small**	**Medium**	**Large**	**Small**	**Medium**	**Large**
Study Display Size	**Small**						
	C_{A1}, C_{A5}, C_{A9}	C_{A1}	C_{A5}	C_{A9}			
	C_{B1}, C_{B5}, C_{B9}				C_{B1}	C_{B5}	C_{B9}
	Medium						
	C_{A2}, C_{A6}, C_{A10}	C_{A2}	C_{A6}	C_{A10}			
	C_{B2}, C_{B6}, C_{B10}				C_{B2}	C_{B6}	C_{B10}
	Large						
	C_{A3}, C_{A7}, C_{A11}	C_{A3}	C_{A7}	C_{A11}			
	C_{B3}, C_{B7}, C_{B11}				C_{B3}	C_{B7}	C_{B11}
	Non-studied						
	C_{A4}, C_{A8}, C_{A12}	C_{A4}	C_{A8}	C_{A12}			
	C_{B4}, C_{B8}, C_{B12}				C_{B4}	C_{B8}	C_{B12}

C_{B1}, C_{B5}, C_{B9}), six subsets in medium size (e.g., C_{A2}, C_{A6}, C_{A10}, and C_{B2}, C_{B6}, C_{B10}), and six subsets in large size (e.g., C_{A3}, C_{A7}, C_{A11}, C_{B3}, C_{B7}, C_{B11}). Each test list also included a random arrangement of 90 photos, including all 12 subsets of critical items from one of the critical sets (9 studied and 3 non-studied), and all 30 fillers from one of the filler sets (i.e., F_A or F_B). Table 4.1 shows that of the three subsets that appeared in each size condition at study, one was displayed in each of the different size conditions at test. Similarly, for the three subsets of non-studied photos, one subset was tested in each size condition. Across subjects, counterbalancing ensured that each subset appeared equallyoften in each of the 12 conditions that were obtained by crossing study and test display sizes.

A test list also included thirty fillers that were included so as to equate the ratio of old (studied) and new (non-studied) items on the test. Ten fillers were shown in small size, 10 fillers were shown in medium size, and 10 fillers were shown in large size. The fillers were also used in a counterbalanced manner so that across subjects, each appeared equally often in each display condition.

The first test phase followed immediately after study. Depending on the test condition, subjects were given an identification test or an old/new recognition test. On each trial of the identification test, a photo was slowly faded-in on the center of the monitor screen. Subjects were told that the goal was to measure their ability to identify objects displayed in different sizes. They were instructed to identify each object as quickly and accurately as possible, to press the left mouse button as soon as they had

identified it, and to say out loud the name of the object. Pressing the left mouse button stopped the fade-in procedure, it cleared the computer screen, and it caused the display of a prompt to enter the name of the object on the second monitor screen, visible only to the experimenter. The experimenter responded by typing the object name on the keyboard.

The fade-in procedure operated on the 640 by 480 pixel map of each picture, which was arranged into 320 columns and 240 rows of four-pixel-square elements. On each pass, the program stepped through all 320 columns, randomly selecting and turning on 1 of the 240 pixel-squares in that column (if a four-pixel-square was already turned on, the next one down was selected, etc.). In this way, the picture was slowly faded-in with all four-pixel-squares showing after 240 passes or about 15 s. At the end of each pass, the program checked for a keyboard response. If none had occurred, it paused for about 40 ms and then continued on to the next pass; if a response had occurred, it recorded the current fade-in level -- the number of passes made prior to the keyboard response.

The old/new recognition test employed exactly the same test display procedure as the identification test. One each trial, a photo of each object was slowly faded-in on the computer screen. However, the subjects were told that their task was to decide, as quickly and accurately as possible, whether each object had appeared in the study list, regardless of the size in which they saw it previously. Subjects pressed the left and right mouse buttons to record their old and new recognition decisions, respectively.

The second test phase followed the first after a delay of seven days. Subjects were given the same type of test as the one they completed in the first phase, either the old/new

recognition test or the identification test. However, subjects who were tested using C_A and F_A item sets in the first phase were tested using C_B and F_B item sets in the second phase and vice versa (see Table 4.1). This procedure ensured that, prior to testing, subjects had seen each studied photo only once. Counterbalancing ensured that each item was presented equally often in each of the experimental conditions.

Results

The critical dependent measure for the identification test was the fade-in level required for identification of studied and non-studied photos in each experimental condition. The fade-in level represents the proportion of pixels that had to be turned on for successful identification of an object. For the old/new recognition test, the dependent measures were hits (the proportion of correctly recognized studied pictures), correct rejections (the proportion of correctly recognized non-studied pictures), and the fade-in level required for making hits and correct rejections. The tables and figures, and all statistical analyses are based on subjects' data. The data were screened for outliers defined as values that differed by more than 2.5 standard deviations from their mean. To reduce their influence on statistical analyses, outliers (less then 2% of data) were set equal to the value of the largest or smallest non-outlier, respectively. The alpha level was set at .05 for all statistical tests. The effects or interactions with p-values between .05 and .10 are noted as 'marginal effects' and they are interpretted in a light of η^2 -- the index of effect size which has properties similar to r^2.

Identification and priming. Figure 4.1 shows the mean fade-in level that was required for correctly identifying photos in each experimental and control condition. Overall, new, non-studied objects were identified equally easily in the immediate (109.17) and delayed (108.43) condition and identification was easier for objects in large displays (81.69), harder for objects in medium size displays (106.86), and hardest for objects in the smallest size displays (137.85). An ANOVA of performance on non-studied items, with test delay (minutes, 1-week) and test display size (small, medium, large) as within subjects factors, showed a significant main effect for test display size, $F(2,94) = 122.11$, $MSe = 622.11$, $\eta^2 = .72$. No other effects approached significance.

Because of the differences in performance on non-studied objects, all subsequent statistical analyses were conducted on priming scores -- a lower level of fade-in required for identifying studied than non-studied photos. The amount of priming was significant in all experimental conditions, with the smallest effect, $t(47) = 3.75$, when targets were small at study and large at test, in the delayed test condition. Overall there was more priming on the immediate test (27.02) than on the delayed test (16.85). An ANOVA of the priming scores, with test delay (minutes, 1-week), test display size (small, medium, large), and study/test condition (same, different) as within-subjects factors showed a significant main effect for test delay, $F(1,47) = 41.49$, $MSe = 375.36$, $\eta^2 = .47$, a significant interaction between test delay and test display size, $F(2,94) = 3.31$, $MSe = 288.94$, $\eta^2 = .07$, and a marginal effect of study/test condition, $F(1,47) = 3.14$, $MSe = 258.26$, $\eta^2 = .06$, $p = .083$. No other effects approached significance.

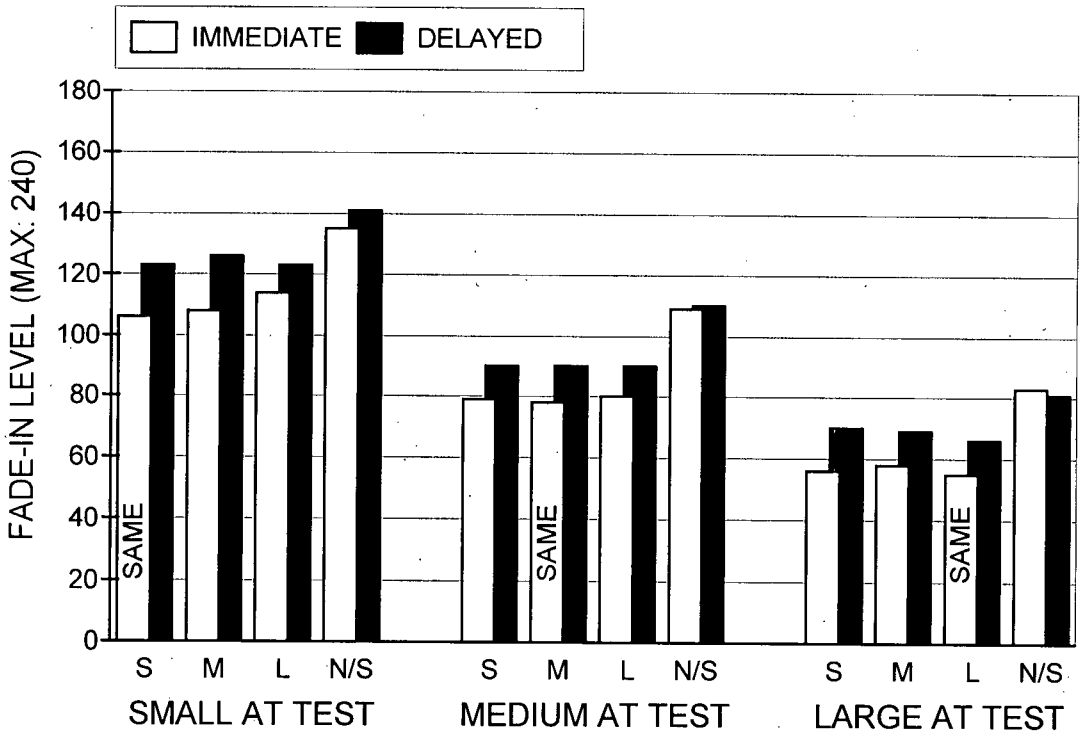

Figure 4.1. The mean fade-in level required to identify photos of objects either on immediate or delayed test, as a function of study size (small, medium, large), test size (small, medium, large), and history (studied, non-studied), in Experiment 1.

The marginal effect of study/test condition indicates that the critical study/test size manipulation had only a weak effect on priming. Overall priming for objects displayed in the same size at study and test was higher (23.51) than for objects displayed in different sizes at study and test (21.14). A series of exploratory t-tests showed that the effect of study/test size manipulation did not reach significance for any of the test size conditions, neither on immediate test nor on delayed test (largest $t(47) = 1.31$ for large objects on the immediate test). If all objects that were medium size at study and test are excluded from the analyses, t-tests showed that the effects of study/test size manipulation approached significance only for objects tested in small size on the immediate test, $t(47) = 1.87$, $\eta^2 = .07$, $p = .069$ (all other ts < 1).

The significant test delay × test display size interaction was followed by separate ANOVAs of the immediate and the delayed test priming data, with test display size (small, medium, large) as a within-subjects factor. The ANOVA of the immediate test data showed no effect of test display size, $F(2,94) = .21$, $MSe = 120.50$, $\eta^2 = 0$. In contrast, the ANOVA of the delayed test data showed a significant effect of test display size, $F(2,94) = 6.38$, $MSe = 118.00$, $\eta^2 = .12$. Follow-up t-tests showed that priming was comparable for objects displayed in small and in medium size at test, $t(47) = .74$, but that priming was smaller for objects displayed in large size than for objects displayed in medium or in small size, $t(47) = 2.67$ and $t(47) = 3.73$.

Recognition. Performance on the immediate recognition test was extremely high, and limited by ceiling effects. Preliminary analyses showed that 58% of all corrected

recognition scores (hits-minus-false alarms) were perfect, and the overall mean of these scores was .87 (with hits and false alarms .92 and .05, respectively). Thus, the immediate recognition test data were not further analyzed. In contrast, performance on the delayed recognition test, shown in Figure 4.2, was lower and off the ceiling. On the delayed test, performance on non-studied items showed that subjects made more false alarms on small size displays (.16) versus medium or large size displays (.13 and .12).

Because of the differences in false alarms, corrected scores were computed by subtracting false alarms from hits, and these were submitted to an ANOVA which had test display size (small, medium, large) and study/test condition (same, different) as within-subjects factors. The analysis showed a significant main effect for test display size, $F(2,94) = 5.55$, $MSe = .03$, $\eta^2 = .11$, and a significant interaction between test display size and study/test condition, $F(2,94) = 4.08$, $MSe = .03$, $\eta^2 = .08$. Follow-up tests showed a significant study/test condition effect for objects displayed as large at test, $F(1,47) = 10.65$, $MSe = .02$, $\eta^2 = .19$, but not for objects displayed as small or medium, $F(1,47) = 0$, and $F(1,47) = .68$, respectively. Objects displayed as large were recognized more accurately when they were previously studied as large versus medium or small.

The *recognition decision fade-in levels* data from the delayed test are presented in Figure 4.3; they showed that subjects were faster (i.e., required fewer fade-in levels) to make correct decisions on old, studied targets (84.31) than on new, non-studied targets (100.58). The baseline condition data (non-studied targets) revealed that subjects correctly rejected as old large targets (76.24) faster than medium targets (97.43), and

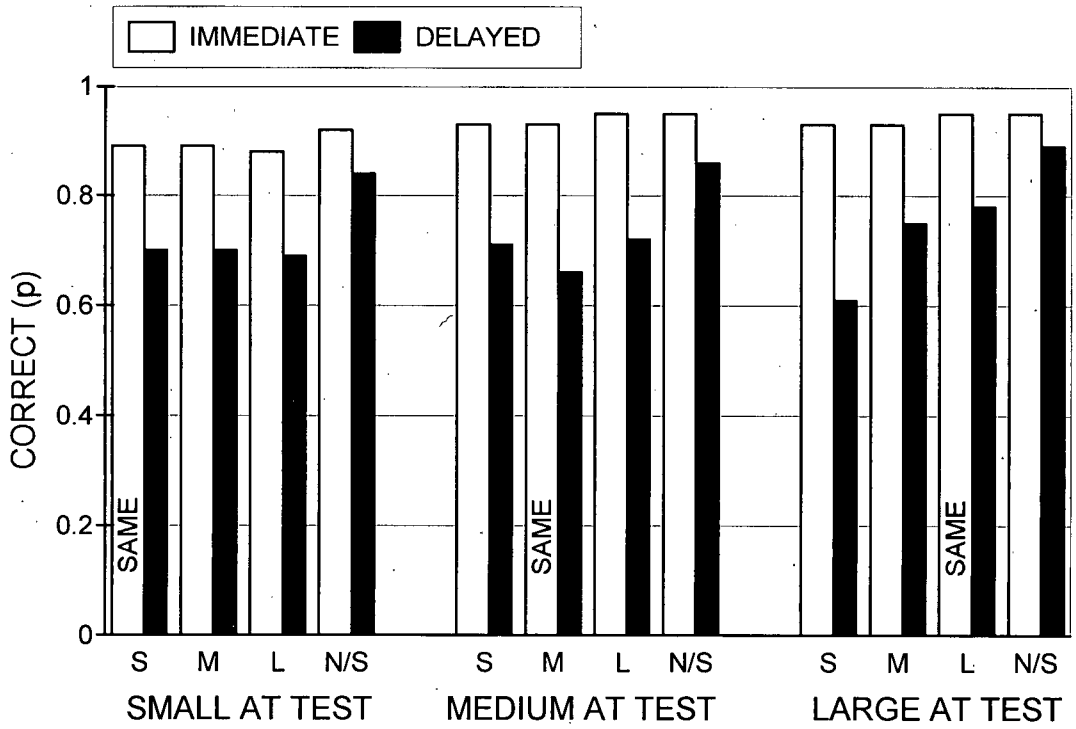

Figure 4.2. The proportion of correct decisions on the old/new recognition test for photos of objects either on immediate or delayed test, as a function of study size (small, medium, large), test size (small, medium, large), and history (studied, non-studied), in Experiment 1.

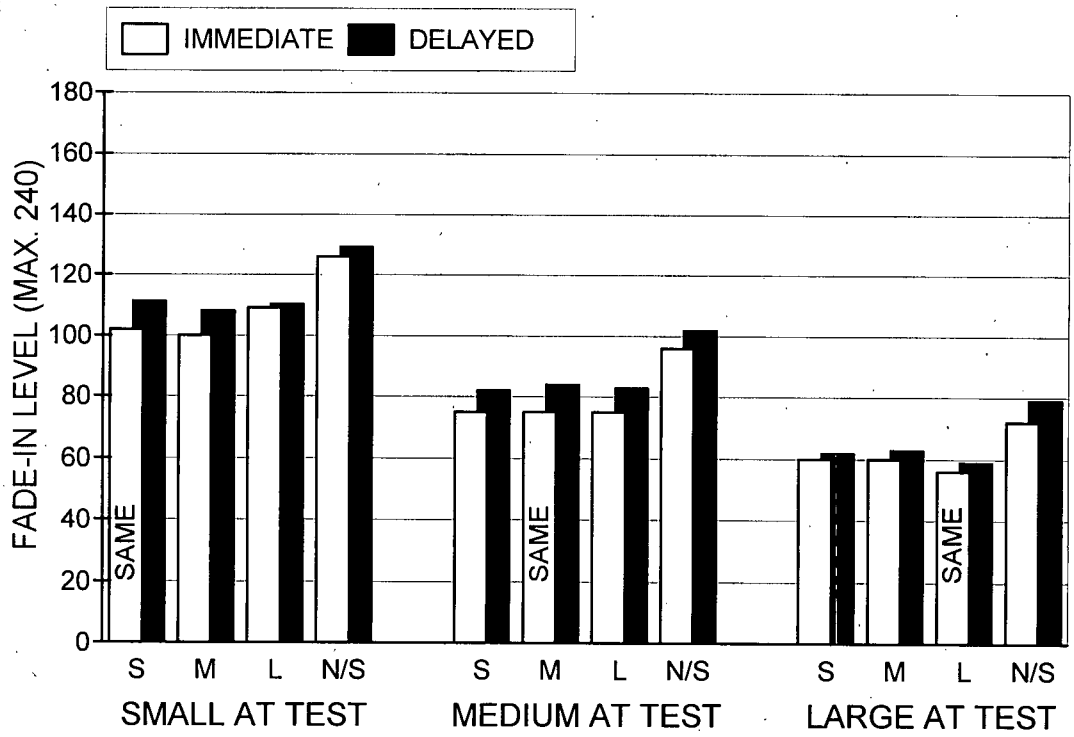

Figure 4.3. The mean recognition decision fade-in level required for making correct decisions on the old/new recognition test either on the immediate or the delayed test, as a function of study size (small, medium, large), test size (small, medium, large), and history (studied, non-studied), in Experiment 1.

medium targets were rejected as old faster than small targets (128.08). An ANOVA on the fade-in data for correct rejections showed a signification main effect for test display size, $F(2,94) = 79.38$, $MSe = 410.80$, $\eta^2 = .63$. Because of this effect, the fade-in level data from all other conditions were used to compute facilitation scores (i.e., fade-in level for correct rejections minus fade-in level for hits). An ANOVA of facilitation scores, with test display size (small, medium, large) and study/test condition (same size, different size) as within-subjects factors showed no significant effects or interactions, all Fs < 1.

Discussion

Experiment 1 revealed several critical findings. First, the size manipulation had a large effect on identification of non-studied objects: Small objects were more difficult to identify than medium size objects, and in turn, medium size objects were more difficult to identify than large objects. Second, study-to-test changes in object size resulted in only a weak, marginal effect on the magnitude of priming despite the fact that there was substantial priming in all study/test conditions. Third, study-to-test changes in object size impaired performance on an old/new recognition test but only for objects tested as large.

Experiment 1 showed a strong influence of the size manipulation on baseline identification. In the previous chapter, the influence due to object orientation on baseline identification was interpreted as reflecting the extent to which orientation information is part and parcel of their pre-existing semantic memory representations. However, this type of inference does not seem warranted in the present situation, for at least two

reasons. First, it is possible that larger objects are more easily identified than smaller objects because the process of scaling an object down to a smaller size involves a loss of information: By the fade-in procedure that was used, the area covered by 16 pixels in a large object is represented by only 4 pixels in a medium object, and by only 1 pixel in a small object. Thus, a large versus small object may contain more critical features to aid its identification. Rather than arguing that size information is part of objects' semantic memory representations, the size effect on baseline performance may be entirely due to the size-scaling method used for the present investigation. An alternative account of the baseline data focuses on the resolving power of the visual/perceptual system. Individual features of objects which are required for identification may be more readily resolved in large than small objects.

The critical manipulation, study-to-test changes in size, had only a weak, marginal effect on the magnitude of priming. This finding is surprising in view of the fact that the size manipulation had a large effect on baseline performance. By the widespread notion that priming reflects the overlap between study and test processing of the sensory and perceptual features of targets, there should be an influence due to size on the magnitude of priming especially across situations that differ in baseline performance.

The weak finding due to the size manipulation cannot be attributed to insufficient experimental power. The power of Experiment 1 to detect, for example, a 20% or 30% *reduction in the amount of priming* due to a study-to-test change in size was .93 and 1.0, respectively.

The weak finding may indicate that, in contrast to orientation information, size information is not coded or is coded only minimally in the episodic memory representations that mediate priming of familiar objects.

However, the findings leave open the possibility that object size may be coded in episodic representations of objects that are less familiar, and this possibility may explain the weak, inconsistent effect on priming that occurred due to the study/test size manipulation. Jolicoeur (1987) investigated the influence due to study-to-test size changes on old/new recognition test performance, and he found that the magnitude of size effects in both decision accuracy and decision speed was larger for unfamiliar targets than for familiar targets. Thus, if the objects used in Experiment 1 differed in terms of familiarity, size information may have been coded for some of them and this subset of objects might have contributed to the weak size-specific effects on priming.

The finding that study-to-test changes in object size reduced performance on the old/new recognition test, but only for large objects, can be explained by making the following assumptions, illustrated by Figure 4.4. First, performance on the recognition test was largely dependent on the data provided by the fade-in procedure, and the displays of large versus small objects differ in terms of the overall amount of information that they provide. To illustrate, in Figure 4.4, it is assumed that large test displays include 100 features (or bits of information) whereas small test displays include only 50 features. Second, it is assumed that making old/new decisions requires subjects to compare data from the test displays with data in their stored representations of previously studied

objects. If the stored representations of large objects are more detailed (they include as many as 100 features) than those of small objects (they include at most 50 features), subjects are most likely to find a match between information provided by large displays and information included in study trial representations of large but not small objects. Thus, one might predict an influence due to size for large test displays. By contrast, information provided by small test displays is most likely included in the representations of small, medium, or large objects, and therefore, old/new recognition decisions are unaffected by study-to-test size manipulations. By this way of thinking about the findings, the influence due to study-to-test size manipulation on old/new recognition test performance for large but not for small objects occurs because subjects are more superficial in processing small than large object displays. The superficial processing of small objects may be due to the data limited nature of small displays. Some evidence consistent with this notion comes from the baseline data showing that subjects make more false alarms on small objects than on medium or large objects, thereby suggesting that subjects consult fewer features before they make old/new decisions.

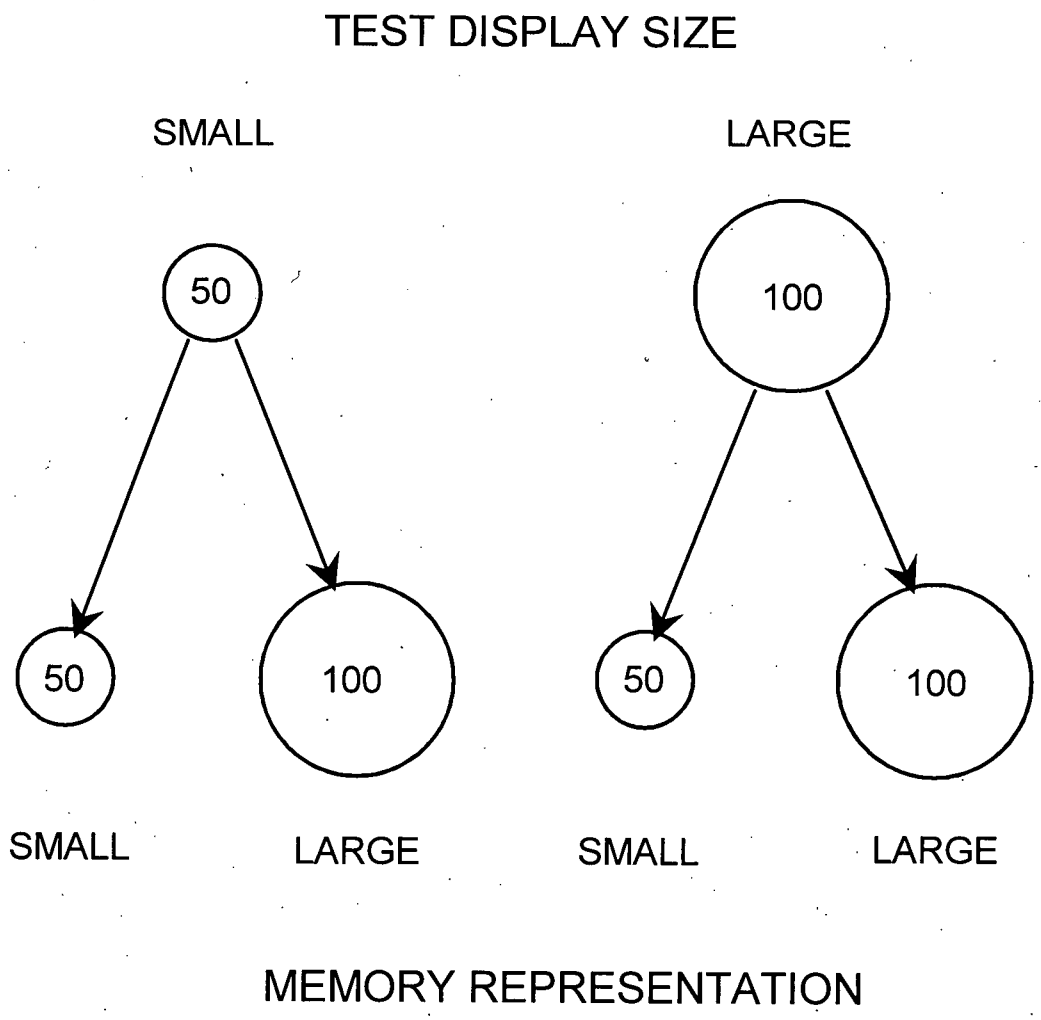

Figure 4.4. A model of old/new recognition decision test performance, in Experiment 1. It is assumed that large object displays include 100 features (or bits of information) whereas small object displays include only 50 features, and that memory representations of objects studied as large are more detailed than those studied as small. If subjects compare data from test displays with data in memory representations of studied objects, study-to-test changes in objects' size will influence performance on objects displayed as large but not on those displayed as small at test.

Experiment 2

Experiment 2 was designed to examine whether the relative size of objects influences priming and explicit memory. In the previous experiment, study-to-test changes in object size had only minimal effects on priming. But this outcome may only mean that the size manipulation was simply too small. After all, in many everyday situations, human beings learn and use their knowledge of relative size of familiar objects; for example, they learn to distinguish between a viola and a violin by their relative size. When objects are presented in unfamiliar relative sizes, they tend to look odd, they attract our attention, and we notice such deviations readily. We notice immediately that something is wrong when children appear much smaller than plants as in the movie *Honey, I shrunk the kids!* By this line of reasoning, one would expect that when objects' relative size violates our pre-existing knowledge, it becomes part of episodic memory representations, and thereby can influence the magnitude of priming.

In this experiment, subjects were presented with photos of two common objects on each trial; one object was designated the context and the other object was designed the target. Contexts and targets were selected so as to be approximately the same size in real life. To manipulate the object's relative size, the displays showed the target either in an appropriate relative size (i.e., the same size as the context) or in an inappropriate relative size (i.e., much smaller or much larger than the context). To emphasize encoding of relative size information, the study task focused subjects' attention on size by requiring them to rate the appropriateness of the targets' relative size. Implicit memory was tested

by means of the same rating task; implicit memory or priming was indexed by facilitation in rating speed on studied versus non-studied targets. The implicit memory test was disguised as a continuation of the study phase. Explicit memory was tested by means of an old/new recognition test. On each trial, the context object was displayed on the screen first, and subjects were required to classify it as either man-made or natural. This classification task was used to ensure that subjects' paid attention to the context object. Next, the target object was slowly faded-in until subjects made either a relative size rating or an old/new decision according to test instructions. Each target was displayed in either the same relative size at study and test or in different relative sizes at study and test.

A secondary question addressed by Experiment 2 was whether the influence due to a relative size manipulation on priming and explicit memory is mediated by specific associations between the objects that formed each context-target pair. For this purpose, each target appeared either with the same context object at study and test or with a different context at study and test. Previous research has shown that context-target associations influence both the magnitude of priming on word stem completion tests and performance on verbal explicit memory tests such as cued recall tests (e.g., Graf & Schacter, 1985, 1987, 1989; Schacter & Graf, 1989). For example, Graf and Schacter (1989) found associative priming, larger effects for targets presented with the same versus different contexts, on the word stem completion test, and they showed that performance on a cued recall test is higher when targets appear with the same contexts versus different contexts at study and test. Consistent with these findings, one might expect associative

effects on priming and on old/new recognition test performance in the present experiment.

Method

Subjects and design. Eighty undergraduate student volunteers participated for course credit. The design had five within subjects factors: history (studied, non-studied), target relative size at test (too small, too large), study/test context (intact, re-arranged), study/test relative size (same relative size vs. different relative size), and test type (implicit, explicit). To highlight targets' size, I have deliberately confounded the relative size manipulation with the size manipulation.

Table 4.2 illustrates the design, and shows how the materials were assigned to the various experimental conditions. In Table 4.2, the name of the context item always appears first (the left-hand member of the pair) and the name of the target item always appears second (the right-hand member of the pair). Lower case printing signifies objects that were displayed as small whereas upper case printing denotes objects that were displayed as large. Thus, 'LAMP--hammer' in Table 4.2 means that the context was a lamp displayed in large size and the target was a hammer displayed in small size.

Materials. A set of 348 color photos of common objects was obtained and digitized using an HP ScanJet III color scanner so that the resulting images were 1280 pixels wide and 960 pixels high. All photos were scanned in 24 bit per pixels color mode. Each object was 'cut out' from its background by means of the Aldus Photostyler graphics software, superimposed and centered on a 1280 by 960 pixels white background,

Table 4.2
Examples of study list pairs and test list pairs in various experimental conditions, in Experiment 2.

Study Study pair type	**Test** Target relative size/Study-test relative size condition
Intact study pairs	
	Small/Same size
LAMP--hammer	LAMP--hammer
BOOK-helmet	BOOK-helmet
	Small/Different size
REVOLVER--TISSUE	REVOLVER--tissue
GRATER--SCREWDRIVER	GRATER--screwdriver
	Large/Same size
dustpan--COFFEE-MAKER	dustpan--COFFEE-MAKER
watermelon--RABBIT	watermelon--RABBIT
	Large/Different size
leaf--nutcracker	leaf--NUTCRACKER
toothbrush--remote-control	toothbrush--REMOTE-CONTROL
Re-arranged study pairs	
	Small/Same size
BOOK-hammer	LAMP--hammer
LAMP-helmet	BOOK-helmet
	Small/Different size
GRATER --TISSUE	REVOLVER--tissue
REVOLVER --SCREWDRIVER	GRATER--screwdriver
	Large/Same size
watermelon--COFFEE-MAKER	dustpan--COFFEE-MAKER
dustpan--RABBIT	watermelon--RABBIT
	Large/Different size
toothbrush--nutcracker	leaf--NUTCRACKER
leaf--remote-control	toothbrush--REMOTE-CONTROL
	Small/Non-studied
	GARBAGE-CAN--guitar
	PIG--newspaper vending machine
	Large/Non-studied
	artichoke--SIEVE
	brush--SUNGLASSES

Note. For each pair listed in the table, the name of the context always appears first (the left-hand member of the pair) whereas the name of the target always appears second (the right-hand member of the pair). Lower case printing signifies objects that were displayed as small whereas upper case printing denotes objects that were displayed as large. Thus, 'LAMP--hammer' means that the context was a lamp displayed in large size and the target was a hammer displayed in small size.

and then scaled to fill two different rectangles, either 512 by 384 pixels for large displays or 160 by 120 pixels for small displays.

One hundred and twenty critical pairs (i.e., context-target pairs) were created by repeating the following procedure. First, four photos of objects were selected randomly, without replacement, from the pool of available photos with the constraint that they had to be all about equally large in real-life. Second, two of these four objects were randomly chosen as targets, leaving the other two objects as contexts. Third, the targets were randomly combined with the contexts to create two *intact pairs*. *Re-arranged pairs* were formed by switching the context objects between the two intact pairs. Finally, all pairs were checked, and if any pair -- either intact or re-arranged -- had two strongly associated objects (e.g., a tea cup and a tea spoon), the subset of objects was returned to the pool of items. This procedure was followed until all pairs met the criteria.

For purposes of counterbalancing items across subjects and conditions, critical context-target pairs were randomly divided into 20 sets (Cr_1 to Cr_{20}). Each target from any of these sets was associated with two contexts in the same set. Each target was associated with one context in the intact pair and with another context in the re-arranged pair. This materials arrangement ensured that both of the contexts associated with each target also occurred in the same set of context-target pairs.

Experiment 2 also required 7 practice pairs (Pr) and 24 filler pairs (Fi) that were formed exactly the same way as critical pairs. The former pairs were used for instructions and practice and the latter pairs were used to equate a number of studied and non-studied

pairs on the old/new recognition test. Finally, sixteen *catch pairs* (Ca) were formed by combining two unrelated objects in such a way that, in real life, each target object was either much smaller or much larger than its assigned context object. The catch pairs were employed to ensure and to verify that subjects followed the relative size rating instructions and were not simply responding based on target object display size.

All picture-pairs were displayed on a 17-inch Sony Multiscan CPD-1730 color monitor, driven by an ATI Mach64 Turbo Graphics Pro video card operating in 16.7 million color mode at a resolution of 1024 by 768 pixels.

Procedure. Subjects were tested individually in a single session lasting about 45 minutes. The experiment was described as examining perception and memory for pictures of objects. It had three phases. The first and second phase required subjects to rate the relative size of target objects (relative size rating test). The purpose of the first phase was for subjects to study for an implicit memory test. In the second phase, the implicit memory test was given and subjects studied new materials for an explicit memory test. The third phase was for explicit memory testing and it required subjects to make old/new recognition decisions for each target object.

At the beginning of the first phase, subjects were presented with 3 Pr and 2 Ca context-target pairs that were color printed on 11×14 inch sized paper cards. Using these cards, an experimenter explained the relative size rating task. For each context-target pair, subjects were required to decide whether the target was displayed as too small in

relative size, as appropriate in relative size, or as too large in relative size. They practiced this task until the experimenter was satisfied that they understood the task.

Following instructions and practice, subjects did the relative size rating test that was administered by means of a computer. On each trial, a photo of a context object was displayed either on the left or on the right of the center of the computer monitor, and subjects were required to say aloud whether the displayed context object was man-made or natural. This classification task was used to ensure that subjects attended to each context object. A second and a half later the target photo was slowly faded-in next to the context. Subjects were told that the goal was to measure their ability to rate the relative size of target objects. They were instructed to rate the appropriateness of targets' relative size as quickly as possible and to maintain a high level of accuracy; subjects pressed the left mouse button for targets shown as too small, the middle button for targets displayed in an appropriate relative size, and the right mouse button for targets shown as too large. The mouse button press caused the target to pop into full view immediately. The target remained on the screen for 1 s, and then both the context and target were erased, and the next trial began in about 2 s.

The fade-in procedure operated on the 1024 × 768 pixel map of each picture. The program stepped through this map, in a pseudo-random sequence, and on each step, it turned on 1 of the not-yet-turned-on pixels. The generator of pseudo-random sequence was designed in such a way that no pixel in the map was visited twice (i.e., sampling without replacement), and the relations between the number of pixels turned on and time

elapsed from the start of fade-in was linear. The mouse button press generated a software interrupt, stopped the fade-in procedure, and caused the current fade-in level to be recorded in terms of pixels turned on prior to the mouse button press.

Following the practice trials, a list of 96 Phase 1 pairs was presented according to the same procedure. Table 4.3 illustrates the manner in which context-target pairs were presented in Phase 1 as well as in Phase 2 and Phase 3 for one subject. In this table, intact pairs are preceded by 'I' and re-arranged pairs by 'R'. Contexts are denoted by 'C' and targets are denoted by 'T'. Contexts or targets displayed in small size have subscript 'S' and contexts and targets displayed in large size have subscripts 'L'. The letters in parentheses indicate whether the target was displayed in the same size condition (S), in a different size condition (D), or whether it was not studied previously (N). Critical sets of context-target pairs are identified by Cr, sets of fillers are identified by Fi, and sets of catch pairs are identified by Ca.

Table 4.3 shows that Phase 1 pairs (the study list for the implicit memory test) included 4 sets of intact pairs (Cr_1, Cr_2, Cr_6, Cr_7) and 4 sets of re-arranged pairs (Cr_{11}, Cr_{12}, Cr_{16}, Cr_{17}). Two of the intact pair sets were presented with large contexts (Cr_1, Cr_2), and two of them were presented with small contexts (Cr_{11}, Cr_{12}). Similarly, two of the re-arranged pair sets were presented with large contexts (Cr_6, Cr_7), and two of them were presented with small contexts (Cr_{16}, Cr_{17}). For each of these two-set-groups defined by intact/re-arranged pair and small/large context conditions, one set appeared with targets displayed in inappropriate relative size (either too small or too large: Cr_1, Cr_6, Cr_{11}, Cr_{16})

Table 4.3

An example of how sets of context-target pairs were assigned for one subjects, in Experiment 2. Phase 1 pairs were a study list for the implicit memory test, Phase 2 pairs were a test list for the implicit memory test *and* a study list for the old/new recognition memory test, and Phase 3 were a test list for the old/new recognition memory test.

Phase 1 (56 pairs)	Phase 2 (112 pairs)	Phase 3 (96 pairs)	Pair set	# of pairs
I-C_LT_S →→	I-C_LT_S (S)		Cr_1	6
I-C_LT_L →→	I-C_LT_S (D)		Cr_2	6
	I-C_LT_S (N) →→	I-C_LT_S (S)	Cr_3	6
	I-C_LT_L (N) →→	I-C_LT_S (D)	Cr_4	6
		I-C_LT_S (N)	Cr_5	6
I-C_ST_L →→	I-C_ST_L (S)		Cr_6	6
I-C_ST_S →→	I-C_ST_L (D)		Cr_7	6
	I-C_ST_L (N) →→	I-C_ST_L (S)	Cr_8	6
	I-C_ST_S (N) →→	I-C_ST_L (D)	Cr_9	6
		I-C_ST_L (N)	Cr_{10}	6
R-C_LT_S →→	I-C_LT_S (S)		Cr_{11}	6
R-C_LT_L →→	I-C_LT_S (D)		Cr_{12}	6
	R-C_LT_S (N) →→	I-C_LT_S (S)	Cr_{13}	6
	R-C_LT_L (N) →→	I-C_LT_S (D)	Cr_{14}	6
		I-C_LT_S (N)	Cr_{15}	6
R-C_ST_L →→	I-C_ST_L (S)		Cr_{16}	6
R-C_ST_S →→	I-C_ST_L (D)		Cr_{17}	6
	R-C_ST_L (N) →→	I-C_ST_L (S)	Cr_{18}	6
	R-C_ST_S (N) →→	I-C_ST_L (D)	Cr_{19}	6
		I-C_ST_L (N)	Cr_{20}	6
		I-C_LT_S (N)	Fi_1♣	12
		I-C_ST_L (N)	Fi_2♣	12
R-C_LT_S →→	I-C_LT_S (S)		Ca_1♣	2
R-C_LT_L →→	I-C_LT_L (S)		Ca_2♣	2
R-C_ST_L →→	I-C_ST_L (S)		Ca_3♣	2
R-C_ST_S →→	I-C_ST_S (S)		Ca_4♣	2
	I-C_LT_S (N)		Ca_5♣	2
	I-C_LT_L (N)		Ca_6♣	2
	I-C_ST_L (N)		Ca_7♣	2
	I-C_ST_S (N)		Ca_8♣	2

Note. In this table, intact pairs are preceded by 'I' and re-arranged pairs by 'R'. Contexts are denoted by 'C' and targets are denoted by 'T'. Contexts or targets displayed in small size have subscript 'S' and contexts and targets displayed in large size have subscripts 'L'. The letters in parentheses indicate whether the target was displayed in the same size condition (S), in a different size condition (D), or whether it was not studied previously (N). Critical sets of context-target pairs are identified by Cr, sets of fillers by Fi, and sets of catch pairs by Ca.

♣ Context-target pairs were fixed across subjects -- they were not counterbalanced.

and the other set appeared with targets displayed in an appropriate size (Cr_2, Cr_7, Cr_{12}, Cr_{17}). Phase 1 pairs also included 4 sets of catch pairs (all re-arranged: Ca_1 to Ca_4), one set in each of the context-target size combinations, to ensure that subjects followed the relative size rating instructions.

Phase 2 was the implicit memory test and the study for Phase 3 -- the old/new recognition test. Phase 2 was disguised as a continuation of Phase 1, and as far as subjects were concerned, there was only one phase that included both Phase 2 and Phase 1 pairs and that required subjects to make relative size ratings of each target. Table 4.3 shows that Phase 2 pairs (the implicit memory test list and the study list for Phase 3 old/new recognition test) included 8 sets of pairs that were previously studied (all intact pair sets: Cr_1, Cr_2, Cr_6, Cr_7, Cr_{11}, Cr_{12}, Cr_{16}, Cr_{17}) and 8 sets of items that were not studied (4 intact pair sets: Cr_3, Cr_4, Cr_8, Cr_9, and 4 re-arranged pair sets: Cr_{13}, Cr_{14}, Cr_{18}, Cr_{19}). The previously studied targets appeared either in the same relative size (Cr_1, Cr_6, Cr_{11}, Cr_{16}) or in different relative sizes (Cr_2, Cr_7, Cr_{12}, Cr_{17}) and they appeared either with the same context objects (Cr_1, Cr_2, Cr_6, Cr_7) or with different context objects (Cr_{11}, Cr_{12}, Cr_{16}, Cr_{17}). Four of the non-studied item sets were used to measure the baseline performance (Cr_3, Cr_8, Cr_{13}, Cr_{18}) and all 8 non-studied item sets served as the study list for Phase 3 -- the old/new recognition memory test. Phase 2 pairs also included 8 sets of catch pairs, 4 of them previously studied (intact pair sets Ca_1 to Ca_4) and 4 of them non-studied (intact pair sets Ca_5 to Ca_8). The catch pairs were included to ensure that subjects continued to follow the relative size rating instructions.

Phase 3 was for the old/new recognition test. On each trial of the old/new recognition test, the context and target were presented in exactly the same way as they were presented during the relative size rating test (i.e., during Phase 1 and Phase 2). When the context appeared, subjects were again required to classify it as man-made or natural, but then the task was to decide, as quickly and accurately as possible, whether each target had appeared previously during the relative size rating test, regardless of the size and regardless of the context in which it had appeared. Subjects pressed the left mouse button when they thought the target was old (previously studied), they pressed the middle button when they were uncertain whether the target was old or new, and they pressed the right mouse button when they thought the target was new. The mouse button press caused the target to pop into full view immediately. The target remained on the screen for 1 s, and then both the context and target were erased, and the next trial begun in about 2 s.

Phase 3 pairs, also shown in Table 4.3, included 8 sets of pairs that were previously studied in Phase 2 (intact pair sets: Cr_3, Cr_4, Cr_8, Cr_9, Cr_{13}, Cr_{14}, Cr_{18}, Cr_{19}) and 4 sets of pairs that were not studied previously (intact pair sets: Cr_5, Cr_{10}, Cr_{15}, Cr_{20}). Previously studied targets appeared either in the same relative size as in Phase 2 (S_3, S_8, S_{13}, S_{18}) or in different relative sizes (S_4, S_9, S_{14}, S_{19}) and they appeared either with the same context objects as in Phase 2 (Cr_3, Cr_4, Cr_8, Cr_9) or with different context objects (Cr_{13}, Cr_{14}, Cr_{18}, Cr_{19}). The non-studied items were used to measure the false alarm rate for each test target relative size condition. Phase 3 pairs also included 2 sets of non-

studied filler pairs (24 pairs in total) that were used to equate a number of studied vs. non-studied pairs.

Across subjects, counterbalancing ensured that each set of critical pairs (Cr_1 to Cr_{20}) appeared equally often in each of the 20 experimental conditions shown in Table 4.3. In addition, targets were presented on the left side of the screen for the first 10 sets in Table 4.3 and they were presented on the right side of the screen for the other 10 sets. The counterbalancing ensured that, across the subjects, each target appeared equally often on each side of the screen, in each of the 20 experimental conditions. Finally, context-target pairs were presented in random order in each phase, and the order was randomized for each subject.

Results

The critical dependent measure on the relative size rating test was the fade-in level required for rating studied and non-studied targets in each experimental condition. The fade-in level represents the proportion of randomly selected pixels that had to be turned on for successful identification of a target object. For the old/new recognition test, the dependent measures were hits (the proportion of correctly recognized studied targets), correct rejections (the proportion of correctly recognized non-studied targets), and the fade-in levels required for making hits and correct rejections. The tables, figures, and all statistical analyses are based on subjects' data. The data were screened for outliers defined as values that differed by more than 2.5 standard deviations from their mean. To reduce their influence on statistical analyses, outliers (less then 2% of data) were set equal

to the value of the largest or smallest non-outlier, respectively. The alpha was set at .05 for all statistical tests. The effects or interactions with p-values between .05 and .10 are noted as 'marginal effects' and they are interpretted in a light of η^2 -- the index of effect size which has properties similar to r^2.

Relative size rating and priming. Figure 4.5 shows the mean fade-in level that was required for making relative size ratings in each experimental and control condition. Overall, non-studied objects were rated faster when their displayed size was relatively large (80.56) than when they were shown as relatively small (107.10). An ANOVA of performance on non-studied targets with size (small, large) as a within subject factor showed a significant main effect, $F(1,79) = 55.06$, $MSe = 511.80$, $\eta^2 = .51$.

Because of the differences in baseline performance (i.e., performance on non-studied targets), priming scores were used for all further statistical analyses. The means in Figure 4.5 reveals priming: Lower levels of fade-in or fewer visible pixels were required for rating studied (72.76) than non-studied (93.79) targets in all experimental conditions (smallest $t(79) = 4.76$ in the re-arranged pairs/large relative size at test/same size condition). The figure also seems to show more priming for objects tested relatively small (23.98) than for those tested relatively large (18.15).

The main analysis focused on two questions: Is priming influenced by the study-to-test changes in relative size, and is the influence (if any) due to study-to-test changes in relative size mediated by specific associations between the target and context objects? Relevant to the first question, Figure 4.5 shows that study-to-test changes in relative size

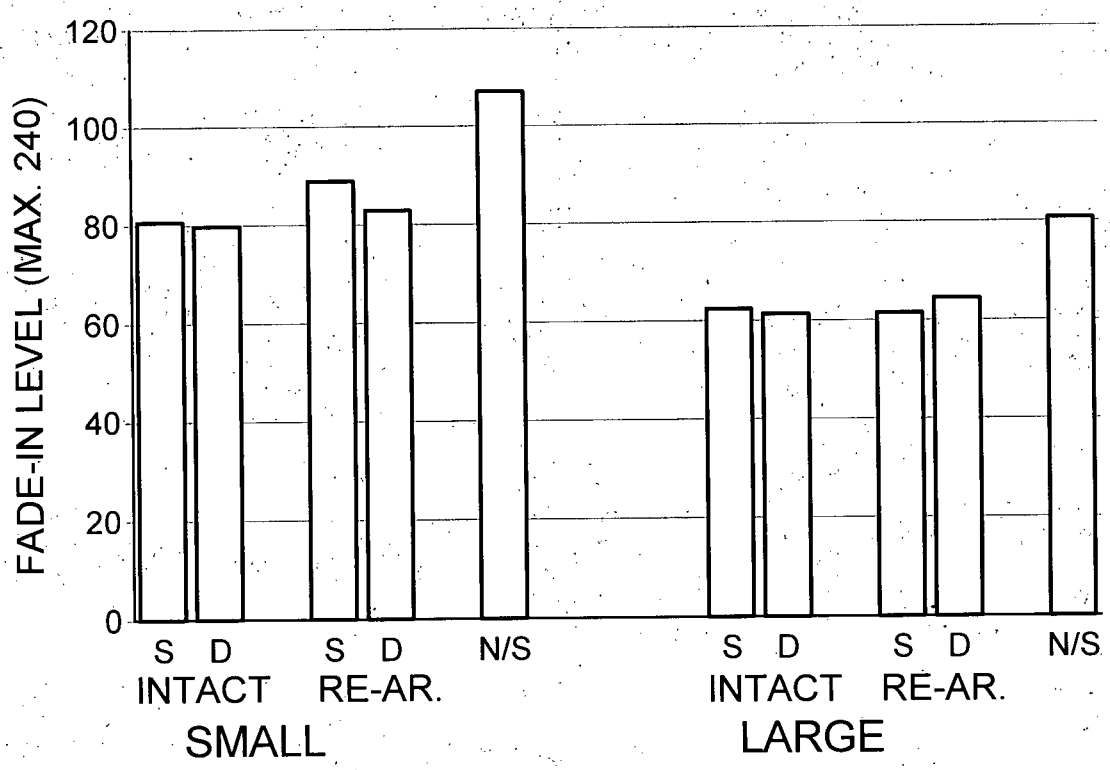

Figure 4.5. The mean fade-in level required for making ratings on the relative size rating test for photos of objects, as a function of targets' relative size at test (small, large), study/test context (intact, re-arranged), study/test relative size condition (same, different), and history (studied, non-studied), in Experiment 2.

had no effect (or only a minimal effect) on the magnitude of priming; priming was comparable for objects studied and tested in the same (20.43) versus different (21.70) relative size.

With respect to the second question, Figure 4.5 indicates that there was a small overall influence due to study-to-test changes in contexts (i.e., intact/re-arranged pair manipulation); priming was slightly larger for targets studied and tested with the same context (22.79) versus with different contexts (19.33). These observations were confirmed by ANOVA of the priming scores that had target relative size at test (small, large), study/test relative size condition (same, different), and study/test context (intact, re-arranged) as within subject factors. The analysis showed significant main effects for target relative size at test, $F(1,79) = 13.00$, $MSe = 382.85$, $\eta^2 = .14$, and for study/test context, $F(1,79) = 5.10$, $MSe = 376.33$, $\eta^2 = .06$. The analysis also showed a marginally significant interaction between study/test context, $F(1,79) = 3.78$, $MSe = 221.21$, $\eta^2 = .05$, and a significant tree-way interaction between target relative size at test, study/test relative size, and study/test context, $F(1,79) = 4.35$, $MSe = 190.95$, $\eta = .05$. No other effects or interactions approached significance.

To gain insight into the triple interaction, the priming scores were analyzed separately for targets displayed relatively small at test and for targets displayed relatively large at test. For targets tested relatively small, the ANOVA showed a significant main effect of study/test context (intact vs. re-arranged), $F(1,79) = 7.59$, $MSe = 348.36$, $\eta^2 = .09$, with no other effects approaching significance. For targets tested relatively large, the

ANOVA showed no significant effects. These results indicate that study-to-test changes in context influenced priming only when targets were displayed relatively small at test.

Recognition. Figure 4.6 shows performance on the old/new recognition test in terms of correct decisions. On non-studied targets, subjects were less accurate when targets were displayed relatively small versus relatively large at test (.82 vs. .90). Because of this difference in baseline performance, corrected scores were computed by subtracting false alarms (incorrect decisions on non-studied targets) from hits (correct decisions on studied targets), and these were submitted to an ANOVA which had target relative size at test (small, large), study/test context (intact, re-arranged), and study/test relative size (same, different) as within subject factors. The ANOVA showed a significant main effect for target relative size at test, $F(1,79) = 97.30$, $MSe = .02$, $\eta^2 = .55$, with no other effects nor interactions approaching significance. Targets displayed in large relative size were recognized more accurately than those displayed in small relative size (77.5 vs. 66.5).

The *recognition decision fade-in levels* data, *for correct decisions* are shown in Figure 4.7. The figure indicates that, for non-studied targets, subjects were faster (i.e., they required fewer fade-in levels) to make correct decisions on targets displayed relatively large (52.82) than on targets displayed relatively small (84.71), $t(79) = 9.83$.

Because of this effect, the fade-in level data from all other conditions were used to compute facilitation scores (i.e., fade-in level for correct rejections minus fade-in level for hits). Figure 4.7 shows that facilitation scores were larger for targets displayed relatively small (22.69) than for those displayed relatively large (12.80). More importantly,

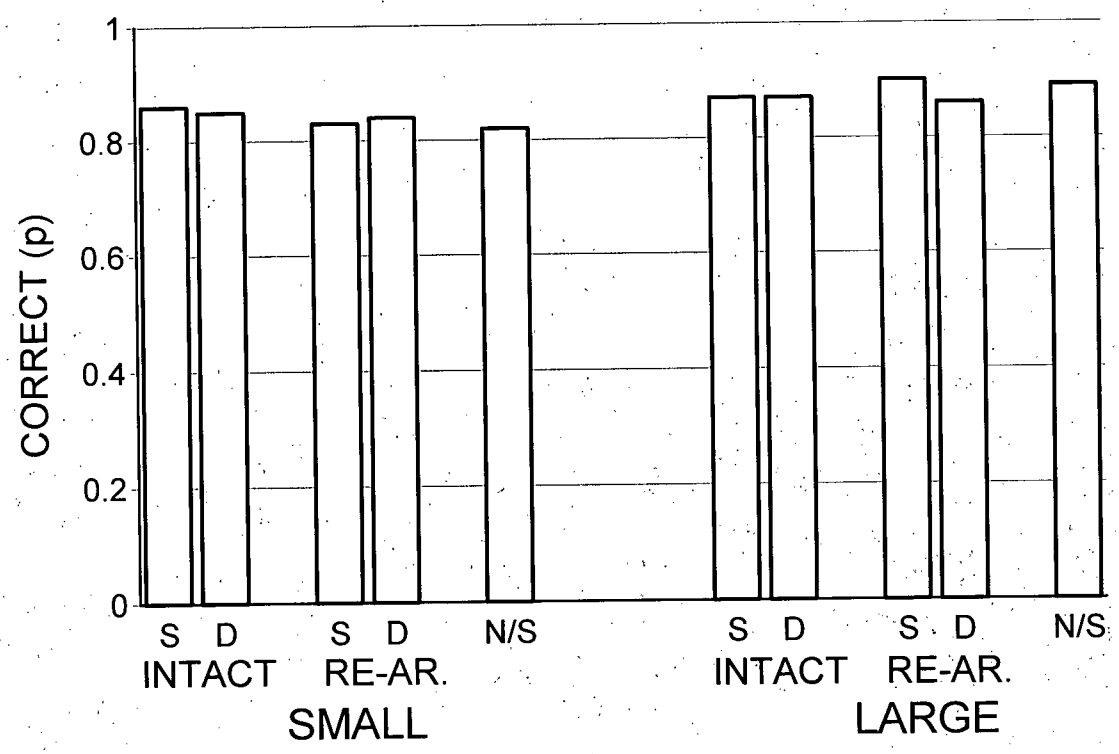

Figure 4.6. The proportion of correct decisions on old/new recognition test for photos of objects, as a function of targets' relative size at test (small, large), study/test context (intact, re-arranged), study/test relative size condition (same, different), and history (studied, non-studied), in Experiment 2.

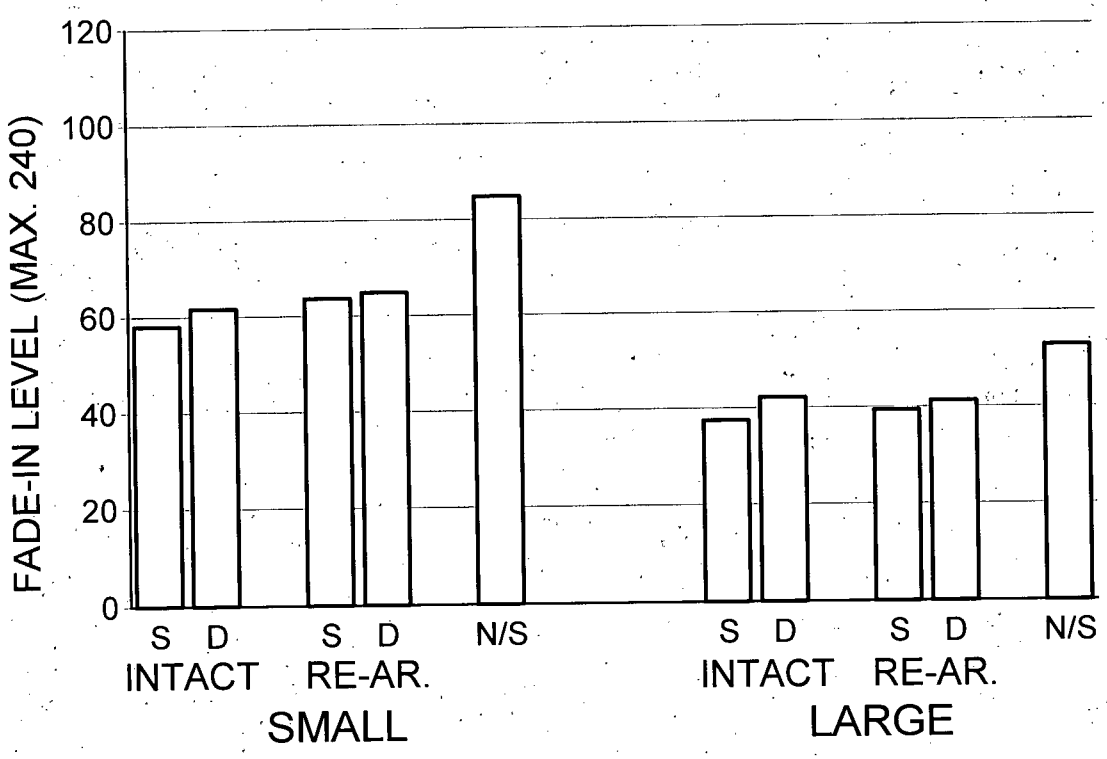

Figure 4.7. The mean recognition decision fade-in level required for making *correct decisions* on old/new recognition test, as a function of targets' relative size at test (small, large), study/test context (intact, re-arranged), study/test relative size condition (same, different), and history (studied, non-studied), in Experiment 2.

facilitation scores were larger for targets studied and tested in the same (19.15) versus different (16.34) relative size, and they were larger for targets studied and tested with the same (18.97) versus different (16.52) context. An ANOVA of facilitation scores, with target relative size (small, large), study/test relative size (same, different), and study/test context (intact, re-arranged) as within subject factors showed main effects of target relative size, $F(1,79) = 31.05$, $MSe = 496.12$, $\eta^2 = .28$, study/test relative size, $F(1,79) = 7.75$, $MSe = 151.10$, $\eta^2 = .09$, and study/test context, $F(1,79) = 6.17$, $MSe = 166.38$, $\eta^2 = .07$. There was also a marginal two way interaction between target relative size and study/test context, $F(1,79) = 3.57$, $MSe = 161.04$, $\eta^2 = .04$. Follow-up simple effect analyses showed a significant effect of study/test context for targets displayed relatively small, $F(1,79) = 6.91$, $MSe = 227.10$, $\eta^2 = .08$, but no effect due to study/test context for targets displayed relatively large, $F(1,79) = .22$, $MSe = 100.04$, $\eta^2 = .00$. Thus, the facilitation scores on the old/new recognition test follow the pattern of priming scores on the implicit memory test with one exception: They show a clear advantage for targets studied and tested in the same versus different relative size.

Figure 4.8 shows *recognition decisions fade-in level* data for *incorrect decisions*. The incorrect decisions were relatively rare -- only 14% of all old/new decisions fell into this group, and thus the incorrect decision fade-in level data preclude any statistical analyses. Nevertheless, the figure highlights a clear difference due to the relative size manipulation, especially when targets were displayed relatively small at test. Overall,

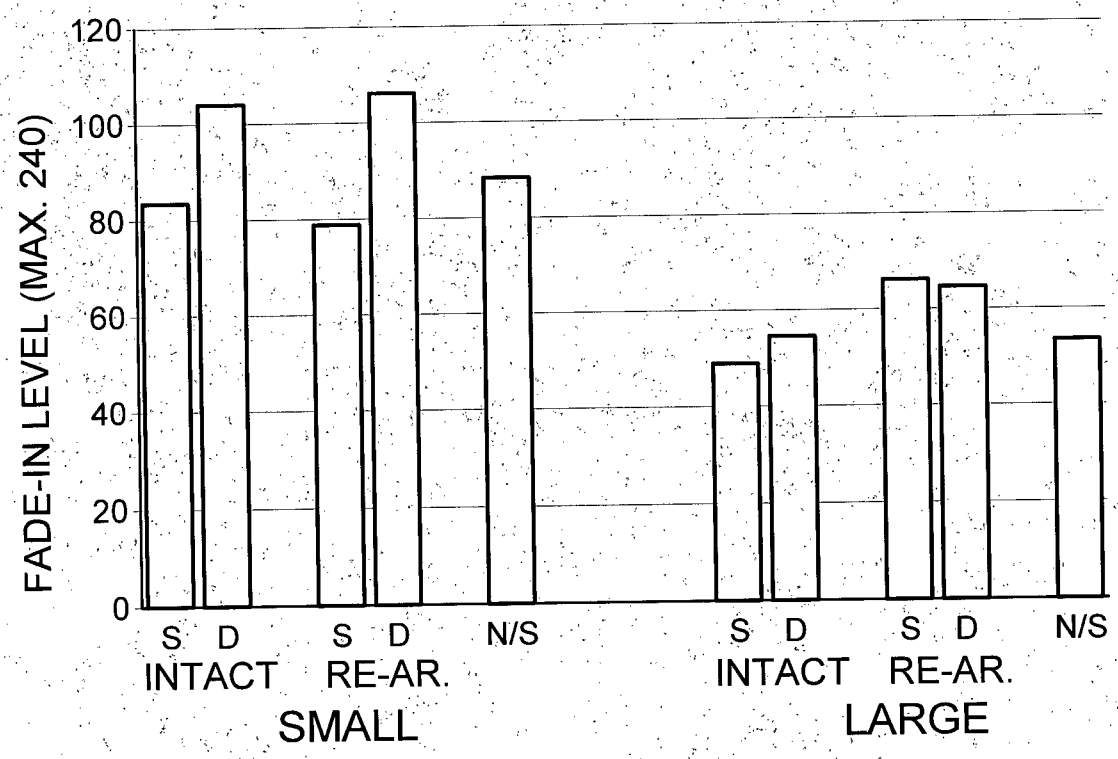

Figure 4.8. The mean recognition decision fade-in level required for making *incorrect decisions* on old/new recognition test, as a function of targets' relative size at test (small, large), study/test context (intact, re-arranged), study/test relative size condition (same, different), and history (studied, non-studied), in Experiment 2.

subjects were faster for targets displayed in the same (69.36) versus different (82.29) relative size.

Discussion

The main goal of Experiment 2 was to examine the expectation that relative size is encoded in the episodic memory representations that mediate priming. The experiment yielded some evidence in support of this expectation: There was priming in all experimental conditions, and the recognition fade-in level data showed a clear influence due to the relative size manipulation.

In addition to these positive findings, however, Experiment 2 also failed to produce a number of expected effects. The data from the relative size rating test show no influence due to the relative size manipulation and they show only a minimal effector a weak effect due to the context (intact/re-arranged pairs) manipulation. The recognition data also show no influence due to the relative size or the context manipulation.

The failure to find any effects due to relative size and due to the context manipulation on recognition test performance is surprising, but perhaps less so in light of the fact that performance was generally too high, and therefore possibly limited by ceiling effects. An alternative possibility is that test was too weak or not suited for picking up relative size and context effects (cf. Smith, 1988, 1994; Smith & Vela, 1986; see also Mori & Graf, in press).

The second surprising non-finding is that the relative size manipulation had no effect on priming. The most likely reason for this outcome is that the task -- the relative

size rating test -- was too complicated, or too confusing, at least for some subjects. It is possible that the most important determinant of performance on this test is the processing involved in decision making rather than in identifying objects. Thus, if priming is primarily perceptual phenomenon (e.g., Jacoby, 1983; Roediger & Srinivas, 1993), this test may not be suitable to measure influences due to perceptual processing that seems to be a hallmark of priming.

This excuse is supported by an analysis of performance on catch trials. Catch trials were included in the relative size rating test to monitor whether subjects followed instructions when required to rate the relative size of objects. The catch trial results showed that subjects succeeded on only 78% of catch trials (by guessing alone they would have succeeded on 67% of catch trials). It is possible that some subjects made their relative size ratings on the display sizes rather than on real life size. Whatever the reason for poor catch trial performance, these observations suggest that the relative size rating task may not be a valid and reliable instrument for assessing influences due to relative size manipulations.

The questionable validity and reliability, or suitability of the relative size rating test may also explain the failure to find context specific (intact vs. re-arranged pairs) effects on priming. Alternatively, context effects might not have emerged because the relative rating task did not engage the semantic/elaborative processing that appears to be necessary for the occurrence of such effects. In a series of experiments, Graf and Schacter (1985, 1987, 1989; Schacter & Graf, 1986, 1989) found that context-specific

priming effects on a word stem completion test occurred only following study tasks that required subjects to set up a unitized representation for each context-target pair by means of semantic/elaborative processing. Although, the relative size rating task was designed to engage such processing, it is possible that subjects approached the task in a different manner. Subjects could have assigned a size number to each object depending on its real-life size, and they could have made their ratings simply by comparing the objects by their size-number. It is also possible that instead of following the instructions, subjects resorted to rating objects in terms of their displayed size, and if so, they did not need to process paired-objects in relation to each other.

The main goal of Experiment 2 was to examine the expectation that relative size is encoded in the episodic memory representations that mediate priming. The strongest evidence for this claim comes from the recognition fade-in level data which showed that subjects were faster at making old/new decisions for objects displayed in the same relative size versus different relative sizes at study and test. It has been argued elsewhere (e.g., Bentin & Moscovitch, 1990; Mandler, 1980; Mandler, Goodman, & Wilkes-Gibbs, 1982) that the decision speed on old/new recognition tests reflects, at least in part, the fluency of perceptual re-processing of studied targets, that the speed facilitation in recognition decisions reflects the newly established episodic memory representations that also mediate priming on implicit memory tests. By this reasoning, the recognition decision fade-in level data indicate that relative size was included in episodic memory representations that mediate priming.

Overall, Experiment 2 seems to have raised more questions than were answered. On-going work addresses some of these, in part, to garner more direct evidence on how relative size influences priming. One current experiment employs a simpler relative size rating task, one that is more likely to ensure semantic/elaborative processing of each context-target pair. This follow-up experiment permits a direct assessment of the reliability of subjects' relative size ratings. Finally, the experiment explores a broader range of relative size manipulations.

Chapter 5

General Discussion

The question of how we identify everyday objects has interested both memory and object perception researchers for a long time. In object perception research, the goal has been to investigate the influence of various circumstances such as orientation changes, size changes, and illumination changes on object identification. The findings emphasized object constancies, such as shape constancy (constancy of perceived shape of an object despite changes in its orientation) and size constancy (constancy of perceived size of an object despite changes in its objective distance and retinal image size); they underscored the fact that variations in sensory data due to changes in a range of factors (e.g., orientation) often have no effect on identification of familiar objects. By contrast, memory researchers interested in object identification have been concerned primarily with how a single encounter with a target influences its subsequent identification. Their results have suggested that object identification performance is best when the precise circumstances that accompany study and test are the same, that a change in orientation, for example, reduces object identification. For this reason, memory research has highlighted the fact that identification performance effects are (i.e., priming is) specific to the sensory and perceptual processing that occurs at study and test.

This combination of findings and theorizing seems odd. If, as argued by object perception work, object identification is uninfluenced by changes in such factors as orientation, size, and illumination, at least under some circumstances, these same factors

should not produce the highly specific priming effects due to study-to-test manipulations reported by memory research. My dissertation work was designed to investigate this conflict in findings and theorizing; it focused on the circumstances that give rise to object constancies, and it inquired about the conditions that produce memory effects which are specific to object attribute manipulations. I made the following assumptions: First, that identification of objects that have not been studied reflects influences due to their pre-existing or semantic memory representations. Second, I assumed that priming -- faster identification of studied versus non-studied objects -- is due to episodic memory representations or due to interactions between episodic and semantic memory representations. In my dissertation, the term *episodic* memory representation refers to an episode-specific representation established at prior study. In contrast, the term *semantic memory representation* is used as an abbreviation for the processes that mediate performance on semantic memory tasks. Semantic memory representations are a function, an aggregate of the relevant set of episodic representations.

Overall, the goal of my dissertation research was to investigate whether objects' orientation, size, and relative size are part of the semantic and episodic memory representations that mediate identification of non-studied and studied objects. To learn more about episodic memory representations, my experiments also included explicit memory tests.

I conducted two series of experiments. The first focused on orientation in the plane of the page and it asked about the influence of object orientation on baseline

identification (i.e., identification of non-studied targets), on priming (i.e., identification of studied targets), and on performance of explicit memory tests. The experiments also inquired whether orientation is equally important for identifying and remembering of all objects, or whether some objects are more affected by mis-orienting them in the plane of the page. It seemed likely that objects which are usually encountered in only one orientation, called *cardinal objects* (e.g., helicopter), would be more affected by mis-orienting them in the plane of the page whereas objects that are frequently encountered in many different orientations, called *non-cardinal objects* (e.g., scissors), would be less influenced by mis-orienting them in the plane of the page. This distinction between cardinal and non-cardinal objects is not strictly categorical: Cardinal and non-cardinal objects represent end points on an orientation-cardinality continuum[4]. The second series of experiments focused on objects' size and on relative size; it asked about the influence of objects' size on baseline identification, on priming, and on performance of explicit memory tests, as well as about the influence of objects' relative size on priming and on performance of explicit memory tests.

Summary of major findings

For non-studied objects, the results from my work showed that both orientation and size influenced object identification, and as expected, the effects due to orientation were different for cardinal and non-cardinal objects. Cardinal objects were much more difficult to identify when they were displayed in an unusual orientation rather than in their normal orientation, whereas non-cardinal objects were identified more or less equally

easily regardless of their orientation in the plane of the page. Identification of non-studied objects was also influenced by the size manipulation in my experiments, with objects displayed as large being easier to identify than objects displayed as small.

For studied objects, the identification test results revealed different patterns of effects due to orientation for cardinal and non-cardinal objects. Study-to-test changes in orientation influenced priming for both non-cardinal and cardinal objects, but orientation specific priming effects (larger priming when study and test orientations matched rather than mismatched) were much larger with cardinal than non-cardinal objects. A correlational analysis revealed a moderate correlation ($r = .54$) between baseline identification and the orientation specific portion of priming, but only for cardinal objects. Study-to-test changes in the size of objects' displays had only a weak effect on priming despite the fact that objects' display size had a large effect on baseline identification. The results due to relative size manipulation were surprising; they showed that study-to-test changes in objects' relative size did not influence the magnitude of priming on the relative size rating test. However, the relative size manipulation had a large influence on the fade-in level required for making old/new recognition decisions: Subjects were much faster (i.e., required fewer visible pixels) at making old/new decisions for objects displayed in the same relative size at study and test than for objects displayed in different relative sizes.

The experiments also revealed several influences due to the orientation, size, and relative size manipulations on performance of explicit memory tests. First, performance

on old/new recognition tests showed a minimal 2% advantage for objects studied and tested in the same versus different orientations. The second finding was that old/new recognition test performance was influenced by study-to-test changes in object's display size but only when objects were displayed as large at test. The third and surprising finding was that the relative size manipulation did not influence performance on the old/new recognition test.

Representational implications

These findings are clues about the semantic and episodic memory representation of various object attributes such as orientation, size, and relative size. By the assumption that identification of non-studied targets is mediated by pre-existing semantic memory representations, the size of the influence due to an attribute manipulation on baseline identification performance indicates how strongly an object's semantic representation is defined by information specific to that attribute. The pattern of effects on baseline identification performance therefore suggests that orientation information is coded and is critical for semantic memory representations, but only for some objects. Cardinal objects which are usually encountered in only one orientation seem to have semantic memory representations that are more *orientation-fixed* than non-cardinal objects which are encountered in many different orientations and thus seem to have *orientation-free* semantic memory representations.

The large effects on baseline identification performance due to the size manipulation could also be interpreted as showing that objects' size is critical for

semantic memory representations. However, this inference about the semantic memory representation of size information is not required because, as argued in Chapter 4, it is more likely that the size effects on baseline identification occurred because of the data limited nature of small versus large object displays.

Consistent with the assumption that priming effects measure influences due to episodic memory representations, or due to interaction between episodic and semantic memory representations, the pattern of priming in my work suggests the following: The finding of orientation specific priming effects (larger priming for objects studied and tested in the same versus different orientation) indicates that orientation information is represented in the newly established episodic representations that mediate priming. In addition, the size of orientation effects suggest that the episodic representations of cardinal objects are more orientation specific than the representations of non-cardinal objects.

The weak and inconsistent size-specific priming effects in my work are more difficult to interpret. It is possible that weak and inconsistent size effects occurred because size is coded in the episodic memory representations of only some objects (e.g., subjects might notice an unusual size exaggeration for some objects).

My findings on relative size effects are more clear cut, and they suggest that the relative size of objects is included in the episodic memory representations that mediate priming. The strongest evidence for this claim comes from the recognition decision fade-

in level data; they showed a clear advantage for objects studied and tested in the same relative size versus in different relative sizes.

My findings also showed that the object orientation and object size manipulations influenced performance on explicit memory tests, thereby indicating that both orientation and size are represented in objects' episodic memory representations that mediate performance on explicit memory tests. Moreover, the findings showing that the size manipulations had a large influence on old/new recognition test performance while having only minimal effect on identification test performance suggest that the influence due to this attribute manipulations depends on how the system is queried: whether it is queried by using an implicit memory test or whether it is queried by using explicit memory tests.

Broader theoretical implications

Baseline identification data. The baseline identification data from my work are directly relevant to theoretical accounts of object perception and identification. For example, according to one widespread theoretical view, Biederman's (1987) recognition-by-components model, object identification is achieved by activating a single viewpoint-invariant structural description of an object class. A structural description consists of geons, basic geometric building blocks (e.g., a cylinder, a cone), and specific terms that define viewer-centered relations between or among geons (e.g., geon A *is above* geon B, geon C *is to-the-right-of* geon B). This view predicts that mis-orienting objects in the plane of the page slows identification or makes it less accurate because the manipulation

changes the relations among geons. For example, when an object is rotated by 180°, the relation 'geon A is above geon B' changes to the relation 'geon A is below geon B'. Consequently, a mis-oriented object will not match its semantic memory representation and extra computation is required to identify it. My findings require a qualification (a rider) of this claim, however; they showed that orientation influences identification speed for cardinal but not for non-cardinal objects. Non-cardinal objects were identified equally easily in any orientation, and thus they fit Biederman's model only if we assume that their semantic memory representations include no relational terms or only minimal relational terms.

Other models of object perception and identification postulate that all objects are stored by means of multiple representations (instances), and for non-cardinal objects these representations include many different display orientations (Edelman & Bulthoff, 1992; Jolicoeur, 1992; Tarr, 1992; Tarr & Pinker, 1989; Tarr, 1995). Tarr and Pinker's (1989; Tarr, 1995) multiple-views-plus-transformation theory is an example of such a view that fits my findings. Tarr and Pinker suggested that objects are represented as a collection of viewpoint-specific representations. Critically, they argued that whether a particular view of an object is represented in semantic memory depends on how frequently the object is seen from that particular viewpoint. By their model, identification is achieved by matching an object's percept against perceptual representations in semantic memory; and if the current percept does not match any of the pre-existing views, the percept is transformed to the best matching view in semantic memory. It

follows that an object seen from a familiar view is identified very easily, by a direct match with a semantic memory representation, whereas when seen from a less familiar or unfamiliar view, the object is identified more slowly because extra computation (e.g., transformations) is required for matching it against semantic memory representations. By definition, only cardinal objects can appear unfamiliar as a result of mis-orienting them in the plane of the page, thereby explaining why my orientation manipulations (see Chapter 3) influenced identification performance only for cardinal but not for non-cardinal objects.

Tarr and Pinker's (1989) model emphasizes that encoding of specific object views is frequency dependent, and in this sense, their view is closely related to several instance theories of memory (Feustel et al., 1983; Hintzman, 1986; Logan, 1988; Mandler, 1980). Instance theories assume, first, that each encounter with a target that results in a unitized percept establishes a new memory representation (a new instance). Thus, familiar views of an object are represented in memory by many more instances than unfamiliar views because the former have been encountered more frequently. A second assumption of instance views is that each remembered instance contributes to target identification on subsequent encounters with it. Instance models thus predict that familiar views of objects are identified more quickly than unfamiliar views, and that a new instance of a familiar view would have less influence on subsequent object identification than would a new instance of an unfamiliar view.

In the instance views, semantic memory representations are computed as a function of the relevant set of episode-specific representations. Thus, cardinal objects' semantic memory representations behave as orientation-fixed because all instances of cardinal objects are in only one cardinal orientation. In contrast, semantic memory representations of non-cardinal objects behave as orientation-free because non-cardinal objects have instances in many different orientations.

Priming data. The pattern of priming effects in my work also has implications for many of the contemporary views of priming. One widespread account of priming was proposed by Roediger and his colleagues (Roediger, 1990; Roediger, Weldon et al., 1989; Weldon, Roediger, Beitel, & Johnson, 1995). An initial version of their account focused on the differences between data versus conceptually driven processing, maintaining that implicit memory test performance depends primarily on data driven processing whereas explicit memory test performance depends primarily on conceptually driven processing. They explained priming by arguing that it is due to the increased fluency in data driven processing which in turn is a direct consequence of a previous encounter with to-be-remembered (TBR) targets. By this view, study-to-test changes in the data of TBR targets (i.e., sensory features) should influence performance on data driven tests, such as word or picture identification. By contrast to this prediction, however, my findings showed that at least some substantial data manipulations (e.g., orientation for non-cardinal objects) had no influence on priming.

More recently, in a revised model, Roediger and his colleagues (Roediger & Srinivas, 1993; Roediger & McDermott, 1993) emphasized that what is critical for performance on implicit memory tests is a transfer in perceptual processing as opposed to a transfer in data driven processing. This modified view can easily accommodate my findings. However, this modified view does not specify which attribute manipulations will influence perceptual processing.

An equally widespread but different account of priming has been advocated by Squire and his colleagues (Squire, 1986, 1987; Cohen & Squire, 1980). They posited two memory systems, a procedural system that mediates priming effects and a declarative system that mediates explicit memory effects. Priming effects are attributed to the increased efficiency of processing within the procedural system. Unfortunately, like Roediger's revised model, this view also does not illuminate the conditions under which factors like orientation, size, and relative size will or will not influence priming.

Schacter and his colleagues (Tulving & Schacter, 1990, Schacter, 1992, 1994) have proposed a more elaborate systems account of priming and explicit memory performance. They proposed that priming is mediated by its own special system, the perceptual representational system (PRS), which functions independently of the episodic and semantic memory systems. The PRS is assumed to be involved in identification of various kinds of stimuli, including words and objects. The PRS is a class of modular subsystems, and one of them -- the structural description subsystem -- is said to be responsible for objects' identification. The structural description subsystem (SDS)

computes a structural description of each TBR object (a structural description is an organized arrangement of information about the form but not the semantic or associative properties of an object).

Based on evidence from brain-lesioned monkeys and single-cell recordings, Schacter (1992, 1994) argued that the SDS may be located in the inferior temporal cortex, and he pointed out that the responding of cells in the inferior temporal cortex is usually not affected by changes in the retinal size of an object. Schacter relied on this fact to explain that the SDS does not represent size information, and therefore, that study-to-test changes in objects' size do not influence the magnitude of priming. Unfortunately, Schacter's view does not explain the computational mechanism that mediates the transfer of information across objects displayed in different sizes. In addition, this view also does not illuminate the pattern of orientation effects in my work.

The pattern of priming effects in my work can be accommodated by a number of instance views of priming (Feustel et al., 1983; Graf, 1994; Graf & Schacter, 1986, 1987; Graf & Ryan, 1990; Mandler, 1980; Logan, 1988; Masson & MacLeod, 1992). These views assume, first, that each encounter with a target results in a new episodic memory representation, and second, that all representations of a target (e.g., new and pre-existing) are re-activated on any subsequent encounters to facilitate target perception. Of course, the influence due to a specific new episodic memory representation will be reduced to the extent to which a target already has many pre-existing representations in memory.

Instance views can explain my findings on orientation effects in the same manner as they explain frequency effects in priming. A target that has been previously encountered in many different orientations already has many representations in each of these orientations, and thus, a single new representation contributes relatively little to identification performance in any orientation. Instance views can also explain my finding of weak and inconsistent effects due to the object size manipulations. We are all highly familiar with photos of objects displayed in many different sizes, and thus, unless an object is displayed in an unusual size, it can be processed primarily in terms of its pre-existing memory representations, and as a consequence, priming effects are not size specific.

General systems implications

My findings can also be used to illustrate more general aspects of the functioning of the human information processing system. For one thing, by demonstrating that study-to-test changes in the attributes of objects do not always result in specific priming effects, my research indicates that the episodic memory encoding of attributes is not obligatory, that the system does not treat all attributes alike (it shows different effects for orientation and size). This prompts the obvious question: What factors determine whether an attribute of an object is encoded in its episodic memory representation?

In an attempt to answer this question, I assume that processing is always interactive, involving both top-down and bottom-up components (Bartlett, 1932; Norman & Bobrow, 1975), and that the content of a percept (and therefore of a new episodic

memory representation) is determined in part by the system factors (e.g., pre-existing representations, skills) and in part by the availability of the data (e.g., target cues, context) required for processing (see Graf & Birt, in press). By these assumptions, my findings on size effects suggest that the system either already has pre-existing memory representations in all sizes, or that it has a capacity to compute these in all sizes equally easily. By contrast, my findings on orientation effects suggest that the system does not have pre-existing representations of all objects in all orientations. When the system has no representations of an object in a particular orientation, it needs to compute one, and therefore, a new episodic memory representation includes orientation information. In other words, it appears that the system needs to compute new representations only when presented with something new, when confronted with novelty.

Apart from responding to novelty, the system is also responsive to instructions or study task manipulations (Graf & Ryan, 1990). Although the system may have default ways of processing stimuli, depending on both pre-existing memory representations and available data, the system's tendencies to engage in such a habitual processing can be controlled or overcome by means of a study task that focuses processing on a particular attribute. This may help explain the relative size effects in my last experiment. The study task in this experiment required subjects to rate target objects' relative size; it focused processing on relative size and thereby ensured the episodic memory encoding of this type of information.

Methodological contributions

My research also makes a critical methodological contribution: It provides a new method for assessing memory performance. Even though we already have a large number of tests for assessing implicit and explicit memory (see Chapter 2), none of the existing tests have the same characteristics as my fade-in method. First, the fade-in method allows the use of color photos of real objects as target materials whereas previous methods were used mainly with words and only sometimes with pictures -- line-drawings. By enabling the use of color photos, the fade-in method allows manipulation of TBR materials along a broader range of dimensions such as color, illumination, and shading, and therefore, it provides new opportunities for investigating some of the basic processes that mediate perception and memory.

A second advantage of my fade-in method is that it is a much more precise measuring instrument than related methods used in previous research. For example, a picture fragment method (see Chapter 2) relies on showing a subject a series (between 4 to 8) of fragmented versions of a target picture, starting with the most incomplete version and proceeding to more and more complete versions until a subject makes a response or until the most complete version, a full picture, is presented. Because the picture fragment method uses only between 4 to 8 fragment levels, it provides only a very rough index of subjects' performance. In contrast, the fading procedure randomly turns on individual pixels forming each image, and it records a number of pixels already turned on at the time

a subject makes a response, thereby using many more intervals to measure subjects' performance.

A third advantage of my method is that the fade-in speed is adjustable. As a result, the fade-in method is suited for investigating, for example, whether priming is mediated by the increased fluency of perceptual processing or whether it is due to an increase in information available in memory.

Finally, a fourth advantage of the fade-in method is that by utilizing high fidelity color photos rather than black-and-white line-drawings and by using the fade-in procedure rather than a series of discrete fragments, my identification tests seem to be more similar to real-life tasks such as identifying targets under less than optimal weather conditions (fog, rain), thereby being more ecologically valid.

Limitations

My work also has a few limitations. Two of these concern my last experiment on relative size. First, in that experiment, performance on the old/new recognition test was quite high, although off the ceiling. It is possible that effects due to relative size might have emerged if the test had been more difficult. This possibility is made more likely by my other findings showing that the size manipulation (Experiment 1 in Chapter 4) influenced old/new recognition performance when testing occurred 7 days after study.

A second and more serious limitation is the failure to observe priming effects due to the relative size manipulation (Experiment 2 in Chapter 4), and the failure to find clear and consistent associative priming effects. As discussed in Chapter 4, one reason for

these null or minimal effects may have been due to the fact that at least some subjects found it too difficult to make relative size ratings. A second possibility is that associative priming effects are more difficult to observe when both the context and the target are pictures. Previous studies investigated associative priming only with words, and thus, my experiment was the first such attempt to observe associative priming using photos of common objects as materials.

In addition to these limitations, my work also has more general limitations that concern generalizability of my findings across tests, across materials, and across subjects. Previous research has shown that priming effects can differ across different implicit memory tests. For example, modality effects (larger priming when the modality of TBR targets is the same versus different at study and test, for example, visual/visual versus auditory/visual) occur only on some implicit memory tests, such as word identification (Jacoby, 1983), but not on other implicit tests such as category instance production (Graf, Shimamura, & Squire, 1985). In view of such evidence, further research is required to find out whether my findings would generalize to other kinds of implicit memory tests (e.g., picture fragment completion tests, picture naming tests).

My work involved use of a wide range of common objects and the study lists were substantially larger than those used in many of the previous studies. But all my materials were photos of objects, and thus it is not clear whether the findings would generalize to other kinds of materials, such as abstract shapes. Jolicoeur (1987) has found that old/new

recognition test effects due to size manipulation varied depending on whether the materials were abstract shapes versus line-drawings of common objects.

All of my experiments were conducted with university undergraduate students as subjects, and thus caution must be exercised in generalizing to other populations such as children or older adults. Previous work has shown that adults' performance declines with advancing age, and it may be that older adults also become less sensitive to attribute manipulations of the type used in my work.

An additional potential limitation of my work (also embedded in any other studies of implicit and explicit memory) has to do with whether performance on an implicit memory test was free of explicit memory strategies, and whether performance on explicit memory test was unaffected by implicit memory recollection. This potential confound has been discussed extensively in the recent literature (see Chapter 2). While important to rule out, this confound (if it occurred) cannot explain several aspects of my findings. For example, it cannot explain why orientation did not influence performance on explicit memory tests but did influence performance on implicit memory tests, and it cannot explain why size manipulation influenced performance on the explicit memory test but not on the implicit memory test.

Conclusions

This line of research is an initial promising step towards answering the questions that motivated my experiments. It illuminates some of the factors that influence perception and memory for attributes of objects, it suggests that familiar objects may be

identified in terms of instances, and it highlights the close relation between perception and memory. The findings are interesting and they have clear implications for theories of perception and memory, and my work also contributes a new, sensitive and flexible method for investigating the processes involved in perception and memory.

My research also raises new questions: Will my findings generalize to other kinds of tests, to other materials, to other populations? How do other attributes of objects influence perception and memory? Does the influence due to various attribute manipulations change with development and age? Does memory for attributes follow different life-span trajectories in Alzheimer's patients versus normal older individuals? Some of these questions are being addressed by ongoing studies.

Footnotes

[1] The size of priming effects for words and pseudo-words varies across experiments, depending on whether baseline performance was equated for the two kinds of materials and whether it was expressed as a difference or proportion score.

[2] By adjusting the pause or idle period that follows each pass of the pixel-painting procedure, a target can be faded-in in as little as 2.7 s (on an IBM compatible computer with 486-2DX66 CPU) or as slowly as desired.

[3] Because of the difference in baserate performance on pictures that were displayed upright versus upside down, we also computed proportional scores by dividing each priming score into the corresponding baseline mean. Analysis of these proportional scores showed the same pattern of significant effects as found with the original priming scores.

[4] Although *cardinal* and *non-cardinal objects* are treated as members of two different object categories in my research, cardinality is a continuum, which at one end is defined by objects that are always encountered in the same orientation (e.g., house), and at the other end is defined by objects that are frequently encountered in many different orientations (e.g., a beach ball).

[5] The fade-in procedure used for the identification test was modified by reducing the idle period that followed each pass of the pixel-painting procedure from 72 ms to 50.5 ms (see Footnote 2).

[6] Degrees of freedom are different for this analysis because one subject's data file was unreadable.

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Appendix A

List of Cardinal and Non-cardinal Objects

Cardinal objects	Non-cardinal objects
airplane	audio tape
alarm clock	banana
baby stroller	battery
backpack	binoculars
balloon	broccoli
barbecue	brush
bear	bun
bed	butterfly
bench	can opener
bulldozer	candy
bus	carrot
candle	clothes peg
chair	coins
chest of drawers	corn
coffee maker	dart
computer	dustpan
desk	film
duck	flash light
exercise bike	flipper
fan	fork
film projector	glove
fire extinguisher	goggles
fox	grapes
garbage bin	hairbrush
gas pump	hammer
globe	hole puncher
goat	knife
helicopter	leaf
hydrant	light bulb
jug	lighter
kettle	lipstick
Kleenex	lock
lamp	measure
mailbox	mushrooms
man	orange
motorcycle	pacifier

newspaper box
pencil sharpener
perfume
phone
phone booth
picnic table
pig
plant
police car
printer
ship
slide projector
slide tray
sofa
soldier
stapler
stove
tank
toaster
tricycle
tripod
truck
typewriter
vase

paint brush
peanut
peeler
pencil
pepper
pine cone
pins
razor
scissors
screwdriver
seastar
sieve
spatula
spoon
screws
stapler remover
tape
tennis ball
tennis racquet
toilet paper
tomato
toothpaste
toothbrush
watch