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Transfer-appropriate processing (TAP) and repetition priming

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Transfer-appropriate processing (TAP), as applied to implicit memory, has tended to emphasize general forms of processing (e.g., *perceptual* or *conceptual* processing). In the present studies, the TAP principle was employed in a more specific manner in order to more precisely assess the relations between the processing engaged during first exposure and that engaged during second exposure to items. Thirteen experiments used a two-phase, cross-task design in which participants engaged in different combinations of seven specific intentional tasks between Phase 1 and Phase 2. Maximum repetition priming was found when tasks were the same in Phases 1 and 2. When Phase 1 and Phase 2 tasks differed, there were lesser, or no, repetition priming effects, depending on the particular combination of tasks. The results demonstrate the importance of the specific intentional processes engaged during repetition priming and the potential heuristic value of TAP, as a principle and methodology, for exploring the organization of memory and related process models.

People are generally faster or more efficient in performing a task on a stimulus when there has been previous experience in performing the same task on the same stimulus. The transfer-appropriate processing (TAP) principle was developed as an expression of this general relation, specifically applied to memory (Bransford, Franks, Morris, & Stein, 1979; Morris, Bransford, & Franks, 1977). Initial research demonstrated that explicit memory was facilitated by the degree of overlap between processes engaged during a first study exposure and those engaged during a second test exposure. More recently, the TAP framework has been extended to implicit memory phenomena (e.g., Blaxton, 1989; Graf & Ryan, 1990; Roediger & Blaxton, 1987; Roediger, Weldon, & Challis, 1989; Srinivas, 1996).

The applications of TAP within the implicit memory domain have generally been oriented toward considerations of the differences and dissociations between implicit and explicit memory. For example, Roediger and his colleagues (e.g., Roediger & McDermott, 1993) have developed TAP explanations couched in terms of distinguishing perceptual and conceptual processes. They related these process differences to performance differences between implicit and explicit memory, although acknowledging that a complete account involves more than a simple one-to-one correspondence between these processes and memory performance differences. Graf and his associates (e.g., Graf & Ryan, 1990; Graf & Schacter, 1985, 1987; Schacter & Graf, 1986) have developed an alternative TAP framework that suggests that integrative and elaborative processes are correlated with enhancements of implicit and explicit memory, respectively.

Reflecting on these and related developments of the TAP framework, Gorfein and Bubka (1997) note that although transfer-appropriate processing "seems to be the best model of repetition priming at this time," it "lacks specificity" and "fails to specify in advance when appropriate processing will be engaged, rendering it almost untestable" (p. 236). These remarks are quite appropriate if one considers TAP to be a model of repetition priming or any other memory phenomena. However, TAP was originally proposed as a principle, not as a model, of memory-processing relations (Bransford et al., 1979; Morris et al., 1977). As such, TAP can be seen as a complement to the encoding specificity principle (ESP), which is a principle related to encoded representations of properties of stimuli and their context (Tulving, 1972, 1979; Tulving & Thomson, 1973). From this perspective, TAP and ESP are construed to be methodological and conceptual rules for reasoning about and investigating memory phenomena, not as specific process or structural models for particular memory phenomena. The TAP principle states that memory performance will tend to be maximized when the participants in an experiment engage in the same intentional act during initial exposure to the items and during later opportunities for showing memory for the items. In a complementary fashion, the ESP principle states that memory performance will tend to be maximized when participants are presented with the same stimulus situation during initial exposure and during later memory opportunities for items. Together, TAP and ESP can be construed to claim that the coded memory of an event represents the unique interaction of a particular intentional act engaged with a particular stimulus situation.

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In general, it is obvious that, in any experiment, participants are always engaged in some specific, intentional act when they are dealing with stimulus events. However, theoretical discussions of implicit memory have tended to focus on sensory-perceptual information or higher level conceptual properties of the stimulus event. The particular intentional tasks in which participants engage have tended to be relegated to methodological details. Even approaches that have adopted the TAP perspective have tended to focus on perceptual and conceptual properties related to the stimulus event, rather than on the specific intentional acts that led to those perceptual and conceptual encodings. For example, differential performance on perceptual implicit tasks has been found when crossing the typography of stimuli between Phase 1 and Phase 2 of the experiments (typed vs. handwritten, Roediger & Blaxton, 1987; or uppercase vs. lowercase, Blaxton, 1989), the format of stimuli (backward vs. normal; Graf & Ryan, 1990), and the form of the stimuli (picture vs. word; Roediger & Blaxton, 1987).

In most of the implicit memory studies related to TAP, the specific tasks that participants perform during acquisition and test differ. The acquisition tasks vary, depending on the nature of the encoded properties that are of interest in the experiment. The test tasks usually involve some form of item identification (e.g., word stem or fragment completion, word naming, or perceptual identification). In most cases, these Phase 2 identification tasks are distinct tasks differing in intentional focus from the acquisition tasks. This can be problematic from the TAP/ESP perspective as previously expressed, which argues that the coded memory is a unique interactive combination of the intentional act and the stimulus situation.

Potential limitations can be illustrated in the context of the first experiment reported by Roediger, Weldon, Stadler, and Riegler (1992). The experiment was complex, but a subset of the conditions are sufficient for the present discussion. Consider two acquisition conditions and one test condition from this experiment. One acquisition condition involved a letter-counting task. A second acquisition condition involved a pleasantness judgment task. At the time of test, participants in both conditions performed a fragment completion test as a measure of implicit memory. Following both acquisition conditions, performance for old items was better than that for new items, with the two conditions showing equivalent degrees of priming. Loosely speaking, from a TAP perspective one might say that the two conditions result in equal degrees of processing overlap between the acquisition and the test. More precisely, the interaction of intentional letter counting on words and intentional fragment completion of partial words is presumably due to some processing similarities. Likewise, the interaction of intentional pleasantness ratings of words and intentional fragment completion of partial words is presumably due to some processing similarities. Are these similarities or overlapping processes the same? The comparison between the two conditions simply does not provide information pertinent to this question. To answer such questions, one must directly compare the letter-counting and the pleasantness-rating conditions.

Consider the following variation on the Roediger et al. (1992) design. This alternative design involves two acquisition conditions, such as letter counting and pleasantness judgments, crossed with the same two test conditions—namely, letter counting and pleasantness judgments. During test, both old words (i.e., words that occurred in acquisition) and new words (i.e., words that did not occur in acquisition) are presented. The dependent variable in all cases is response time. The present TAP perspective would predict that the conditions involving the same intentional acts on the same words between acquisition and test (*same-task conditions*) should result in greater priming than do the conditions involving different intentional acts on the same words (*cross-task conditions*). Greater than zero priming in the cross-task conditions would indicate the degree of overlap in processes between the letter-counting and the pleasantness judgment tasks interacting with the same objects. Additional information about overlapping processes is provided by the relation between the cross-task conditions, in particular whether priming is the same regardless of the direction of transfer (letter counting to pleasantness vs. pleasantness to letter counting). Finally, comparisons of the cross-task conditions with the same-task conditions can provide more precise information about the relative degree of processing overlap. The size of the priming effect in the same-task conditions can be used to scale the degree of processing overlap in the cross-task conditions.

In fact, Roediger et al. (1992) were experimentally comparing the word fragment and word stem completion tasks as tests of implicit memory. The comparisons were made in part by manipulating learning tasks that were different from these two completion tasks. Note that as an alternate methodology, they could have used both fragment completion and stem completion as learning tasks and then factorially crossed these same tasks at test and measured reaction time (RT) as an indicant of processing overlap. One implication of this perspective is that no particular task (or set of tasks) necessarily has any privileged status as *the* test(s) of implicit memory.

The focus on transfer as a function of processing similarity is not a novel concern. For example, Kolers and his colleagues (e.g., Kolers & Perkins, 1975; Kolers & Roediger, 1984) were interested in comparing transfer involving same and different procedures. This interest shares important features with the TAP perspective but also differs in important ways. Kolers and his colleagues seemed to be primarily concerned with the potential transfer effects across tasks (e.g., transfer between reading of upright text and reading of inverted text) that were independent of the particular material being read (e.g., sentences). (It may be just as accurate to say that Kolers was interested in the same task [i.e., reading, in all cases] and what was varied was the stimulus materials [e.g., normal, inverted, or other text]; either way, the approach differs from the

TAP approach, as described below.) Generally, practice with a particular reading task on one set of sentences was followed by either the same or a different reading task on a second set of sentences. Comparisons provided information about transfer based on the similarities of tasks that were independent of the particular stimuli being processed. The emphasis was on task similarities per se and not on similarities between task by stimulus interactions.

The TAP approach, in contrast, focuses on particular episodic events—that is, the interaction of particular mental acts with particular stimulus situations. This approach, as we have implemented it, is distinguished methodologically from alternative approaches by factorially combining the acquisition task, the test task, and stimulus repetition (i.e., old vs. new items at the time of test). As will be described in more detail below, in the present experiments we examined repetition priming (in particular, the difference in response time to old and new items) as a function of whether the same tasks or different tasks were performed at acquisition and at test (e.g., lexical decision followed by lexical decision, lexical decision followed by animacy judgment, etc.). Our goal was to understand how implicit memory, as assessed by repetition priming, would be affected by the intentional acts engaged at acquisition and at test, where these intentional acts were defined by tasks, such as lexical decision, size judgments, animacy judgments, and so on.

The difference between this approach and other approaches can perhaps be best explained by presenting a contrasting example. If we had crossed acquisition task and test task but used different stimuli in the two phases, our method would be similar to the one used by Kolers (e.g., Kolers & Perkins, 1975). In such an experiment, the relative overall speeds of responding in Phase 2 would be of principal interest. On the basis of Kolers's findings, one would expect, for instance, lexical decision in Phase 2 to be faster if subjects performed a lexical decision in Phase 1 rather than an animacy judgment in Phase 1. Our methodology pushes one step further by including repeated and unrepeated stimuli (old vs. new at test) and examining the relative speeds of responding to these classes of items. Note that, in principle, transfer effects at the level of tasks are independent of transfer effects at the level of tasks combined with particular stimuli. Continuing the example, overall responding might be faster for lexical decisions preceded by lexical decisions than for lexical decisions preceded by animacy judgment, and yet repetition priming might be identical for these sequences of tasks. At the risk of belaboring the issue, we add that the designs and comparisons used in the present experiments statistically controlled for task-to-task transfer effects on the items. Task-to-task transfer effects apply equally to old and to new items and thus cannot affect repetition priming. It is worth noting that in one project, Kolers (1975) did include a limited manipulation of old versus new items, and the results of the experiment were congruent with the TAP perspective. Participants read sentences in either normal or inverted type and then later reread the same sentences all in inverted type. Participants read the repeated sentences faster if the sentences had been originally presented in the inverted form—that is, priming was greatest when the acquisition and test conditions were the same.

Several studies of repetition priming have used designs in which the tasks at acquisition and at test were manipulated systematically (Gorfein & Bubka, 1997; Thompson-Schill & Gabrieli, 1999; Vriezen, Moscovitch, & Bellos, 1995). However, even in these cases, the discussion has tended to focus on the stages of processing (Vriezen et al., 1995) or the encoded qualities of the stimulus event (e.g., visual vs. functional semantic memory structures; Thompson-Schill & Gabrieli, 1999), rather than highlighting the pertinence of the specific intentional acts per se.

The present work is explicitly concerned with a TAP approach involving specific intentional tasks interacting with specific stimulus situations. The primary purpose of the present work is to demonstrate the potential efficacy of this approach by demonstrating general patterns of results that must be accounted for by any theory of implicit memory. Seven different tasks were chosen to represent various combinations of perceptual and conceptual judgments. Vowel counting, E-check, and lexical decision tasks were chosen as tasks that tapped different aspects of surface, perceptual information of presented items. Animacy, pleasantness, hardness, and bigness judgments were chosen to tap various aspects of the conceptual referents of the presented items.

The 13 reported experiments produce complex patterns of data. The presentation of methods and results will be described in an order that will facilitate subsequent discussion. The experiments were actually conducted in the following order: Experiments 1, 8, 10, 5, 2, 9, 11, 12, 13, 7, 3, 4, and 6. Since most of the experiments to be described involved similar designs and methods, the commonalities will be described in a General Method section. Details that differ between experiments will be described in the appropriate specific Method sections.

GENERAL METHOD

Participants

The participants were undergraduates in introductory psychology courses at Vanderbilt University, who participated in the research for course credit.[1] They had normal or corrected-to-near-normal vision.

Design

Most of the experiments employed the same (2 × 2 × 2) mixed design, in which type of acquisition task (Task 1 or Task 2) was manipulated between subjects and type of test task (same or different) and item type (old or new) were manipulated within subjects. These experiments involved two phases. During acquisition, the participants performed one of two tasks (Task 1 or Task 2). During test, the participants performed two tasks; one was the same as the acquisition task (e.g., Task 1, if acquisition involved Task 1), and the other was different from the acquisition task (e.g., Task 2, if acquisition involved Task 1). The order of same versus different tasks was counterbalanced across participants. Three experiments (Experiments 3, 4, and 6) involved a 2 × 2 design in which a single acquisition task and two different test tasks were manipulated within subjects. Altogether, seven different tasks were used across the 13 experiments: lexical decision, animacy, liking, bigness, hardness, E-check, and vowel-count judgment tasks.

In the lexical decision task, the participants had to decide whether the string of letters was a word (e.g., SHIP) or a nonword (e.g., KILE). Animacy judgments involved judging whether the word referred to an object that was animate/having life (e.g., HAWK) or inanimate/not having life (e.g., ROCK). Liking judgments required participants to determine whether the word referred to a likeable (e.g., DOVE) or dislikable (e.g., GERM) object. Hardness judgments involved ascertaining whether the word referred to a hard (e.g., ROCK) or soft (e.g., QUILT) object. Bigness judgments involved deciding whether the word referred to an object that was bigger (e.g., PIANO) or smaller (e.g., BERRY) than a shoebox. The E-check task involved deciding whether or not the word contained an E. The vowel-count task involved deciding whether or not the word contained more than two vowels.

Materials

Words and nonwords used in the experiments were typed in uppercase. Nonwords were formed by rearranging the letters of words to form nonpronounceable nonwords for Experiment 1 and pronounceable nonwords for the other experiments involving lexical decisions. Different sets of words were used in the different experiments, with the words in each experiment being chosen to be clear examples of the categories appropriate to the different types of tasks used in that experiment. For example, when the animacy task was crossed with the liking task, one fourth of the words were animate and likeable (e.g., DOVE), one fourth were inanimate and likeable (e.g., BIKE), one fourth were animate and dislikable (e.g., GERM), and one fourth were inanimate and dislikable (e.g., BOMB). The words presented during acquisition formed the set of old words. Half of the old words were subsequently presented during each of the two tasks during test. New words were words not shown during acquisition. In Experiments 1, 5, 8, and 10, old and new words were randomly assigned to the test lists. In the remaining experiments, assignment of words was counterbalanced across participants—so that each word occurred approximately equally often as (1) an old versus new test item, (2) a Task 1 versus Task 2 test item, and (3) a same test task item versus a different test task item[2]—and the test orders were counterbalanced. The presentation order of words within acquisition and test lists was randomized for each participant.

Procedure

All the participants signed a form of consent to participate in the study. Instructions and materials were presented on 8088 personal computers in 80-column uppercase font. The participants were tested individually. They read the task instructions. The instructions in each case described the particular judgments that participants were to make (e.g., animate vs. inanimate, etc.), included examples of the judgments, and asked the participants to make their judgment responses as fast as possible but to try not to make mistakes. At no point in the instructions, or at any other point in the experimental session, were the participants told anything about possible repetitions of words. The participants were then presented words one at a time for judgment on a computer screen. The participants were asked to respond as quickly and as accurately as possible by pressing the "z" or "/?" key on the keyboard to indicate different judgments (e.g., "z" for animate and "/?" for inanimate judgments). Each trial began with a ready signal (*) that appeared in the middle of the screen for 500 msec. It was followed by the presentation of a word/letter string centered on the screen, which remained visible until the participant responded. A 500-msec interval between the response and the next trial was used in the first four experiments conducted (reported as Experiments 1, 5, 8, and 10 in the manuscript). A 1,500-msec interval was used in the remaining experiments. This change was simply a modification introduced to put less speed stress on the participants and was not done for any particular theoretical interest. As can be seen in Table 1, the effect of the difference in interval seemed to be an overall slowing of judgment RTs with the 1,500-msec interval, but no change in the pattern of results. Note the generally faster RTs and higher error rates for Experiments 1, 8, and 10 with a 500-msec interval, as compared with their replications in Experiments 2, 9, and 11 with a 1,500-msec interval. In both acquisition and test tasks, the participants were given initial practice trials, followed by a set of experimental trials. The participants were given brief breaks after the acquisition task and after the first task during test. Type of response and RTs were recorded for each item.

Analyses

For all the experiments, the participants' RTs were converted to $\log_{10}$ transformed values, and the means of these log values were analyzed by mixed factor analyses of variance. The RTs are based on all the responses made by the participants.[3] The results of the 13 experiments are summarized in Table 1. The means and standard deviations (*SD*s) for all the conditions are presented in the table. For reading convenience, the means of the log-transformed data have been converted back into RTs in milliseconds. The *SD*s are not readily converted back to milliseconds and, thus, are reported as log values. The repetition priming effects are the differences between the RTs to new items and to old items in the various conditions. To provide a basis for comparing, across experiments, the degree of transfer under different combinations of conditions, the effect size scores (i.e., new item mean − old item mean/*SD*) are reported. The mean square errors (*MSE*s) terms for the three-way interactions (acquisition task × test task × old/new item) in the different experiments were used as estimates of the *SD*s, in experiments involving the complete crossing of acquisition and test conditions. In the three cases involving incomplete crossings (Experiments 3, 4, and 6), the *MSE* for the two-way interaction was used. The priming effects are also represented as t scores with associated p values ($p < .01$, $p < .05$, or $p < .10$, two-tailed). The t scores were calculated using the *MSE* terms for the three-way interactions (or the two-way interaction in Experiments 3, 4, and 6). The effect size values and the t scores are, of course, correlated but give somewhat different perspectives on the patterns of results. Table 1 also presents error rates. Although there were some differences among error rates across conditions, the three-way interactions (or two-way interactions in Experiments 3, 4, and 6) were not significant in any of the experiments, so the patterns of errors do not equivocate the RT results to be reported.

EXPERIMENT 1

The purpose of Experiment 1 was to assess same-task and cross-task transfer between lexical decision, a task oriented to surface properties of the item presented, and animacy judgment, a task oriented to properties of the conceptual referent of the item presented.

Method

The two tasks in Experiment 1 were the lexical decision task and the animacy judgment task. There were 22 participants in the lexical decision acquisition group and 19 participants in the animacy judgment acquisition group. The materials consisted of four-letter words (M_{freq} = 22.1, SD = 39.3)[4] and nonwords. Nonwords were

Table 1
Results of the 13 Experiments

Experiment	Task			New Items			Old Items			RP Effect	Effect Size	*t* Score
	1	2	*N*	*M*	*SD*	Error	*M*	*SD*	Error	(msec)		
1	X	X	22	516	.071	.034	507	.068	.028	9	0.32	1.07
	A	X	19	535	.048	.082	525	.046	.095	10	0.34	1.06
	A	A	19	713	.078	.108	636	.059	.056	77	2.04	6.28‡
	X	A	22	684	.078	.085	676	.076	.074	8	0.20	0.68
2	X	X	13	675	.100	.004	611	.106	.000	64	1.25	3.19‡
	A	X	16	689	.076	.001	652	.089	.000	37	0.69	1.95*
	A	A	16	894	.101	.113	765	.094	.081	129	1.97	5.56‡
	X	A	13	917	.058	.077	916	.081	.065	1	0.01	0.04
3	X	X	19	609	.077	.040	567	.075	.018	42	1.04	3.21‡
	X	A	19	795	.069	.087	815	.079	.090	−20	−0.36	−1.11
4	X	X	18	661	.093	.019	593	.077	.031	68	1.13	3.39‡
	X	B	18	934	.104	.086	897	.098	.081	37	0.43	1.28
5	X	X	20	650	.066	.097	600	.066	.031	50	1.04	3.30‡
	E	X	20	624	.051	.093	589	.056	.044	35	0.76	2.39†
	E	E	20	502	.044	.056	479	.085	.063	23	0.62	1.95*
	X	E	20	555	.073	.036	547	.066	.031	8	0.19	0.60
6	X	X	19	611	.079	.040	576	.064	.013	35	0.82	2.52†
	X	V	19	1,196	.065	.084	1,204	.072	.071	−8	−0.08	−0.25
7	A	A	32	868	.107	.050	794	.101	.047	74	0.99	3.97‡
	B	A	32	908	.074	.067	876	.084	.048	32	0.40	1.61
	B	B	32	924	.090	.039	840	.085	.038	84	1.06	4.26‡
	A	B	32	904	.111	.014	855	.097	.014	49	0.63	2.53†
8	X	X	20	599	.065	.138	560	.053	.059	39	1.12	3.53‡
	L	X	20	634	.076	.091	609	.075	.066	25	0.66	2.08†
	L	L	20	783	.106	.228	710	.098	.228	73	1.62	5.13‡
	X	L	20	771	.089	.197	738	.091	.197	33	0.71	2.25†
9	X	X	14	741	.128	.007	650	.113	.004	91	1.90	5.03‡
	L	X	15	697	.077	.007	623	.067	.000	74	1.62	4.44‡
	L	L	15	905	.106	.043	764	.085	.037	141	2.45	6.72‡
	X	L	14	1,019	.120	.025	944	.118	.025	75	1.10	2.91‡
10	A	A	20	755	.133	.193	670	.119	.184	85	1.57	4.96‡
	L	A	20	782	.089	.169	747	.103	.197	35	0.59	1.85*
	L	L	20	777	.101	.201	708	.095	.260	69	1.22	3.84‡
	A	L	20	807	.133	.216	744	.117	.206	63	1.06	3.35‡
11	A	A	32	890	.075	.061	771	.085	.039	119	1.62	6.47‡
	L	A	32	964	.105	.053	923	.102	.059	41	0.48	1.93*
	L	L	32	1,047	.141	.086	844	.098	.083	203	2.42	9.68‡
	A	L	32	939	.090	.089	877	.092	.056	62	0.78	3.10‡
12	H	H	32	978	.112	.091	834	.118	.061	144	1.85	7.41‡
	L	H	32	901	.081	.038	865	.086	.047	36	0.47	1.87*
	L	L	32	830	.093	.031	753	.087	.031	77	1.13	4.53‡
	H	L	32	1,002	.146	.080	933	.110	.092	69	0.83	3.32‡
13	B	B	32	890	.094	.014	792	.084	.011	98	1.53	6.10‡
	H	B	32	1,011	.112	.031	962	.102	.023	49	0.65	2.61†
	H	H	32	943	.125	.017	828	.097	.001	115	1.70	6.81‡
	B	H	32	855	.090	.014	814	.080	.011	41	0.65	2.61†

Note—Tasks 1 and 2 denote task instructions during Phases 1 and 2, respectively, and refer to the following judgments: X, lexical decision; A, animacy; B, bigness; E, E-check; V, vowel count; L, likeability; H, hardness. Standard deviations are in $\log_{10}$ units. RP, repetition priming. $^{*}p < .10$. $^{\dagger}p < .05$. $^{\ddagger}p < .01$.

presented only when the participants were engaged in the lexical decision tasks at acquisition or at test; no nonwords were presented during animacy judgments. The nonwords were generally nonpronounceable strings. The acquisition and test tasks each began with 10 practice trials, which, during acquisition, were followed by 48 experimental trials in the animacy judgment condition and 96 experimental trials in the lexical decision condition, which included 48 nonword experimental trials. Test tasks consisted of 32 experimental trials[5] in the animacy judgment condition and 64 experimental trials in the lexical judgment condition, which included 32 nonword trials. In this first experiment, the order of test tasks was not counterbalanced. The order of test tasks was counterbalanced in the other experiments. The interval between a participant's response and the next ready signal was 500 msec.

Results

Animacy acquisition followed by animacy test showed significant priming, whereas the other three combinations did not show priming. Lexical acquisition to lexical test did not show the priming that would be expected from the TAP perspective. The lack of repetition priming in this experiment was probably due to the use of nonpronounceable nonwords, which likely made the word versus nonword discrimination too easy, with resulting ceiling effects. This interpretation is supported by the fact that lexical to lexical priming effects were found in seven of the other experiments. The cross-task transfer from animacy acquisition to lexical test was not significant, but this might have been due to the same ceiling effect. There is a suggestion of cross-task transfer in this condition in Experiment 2. No cross-task transfer was found in the lexical to animacy transfer condition.

EXPERIMENT 2

The purpose of the second experiment was to replicate the general design of Experiment 1 but to alter the nature of the nonwords in order to alleviate the potential ceiling effects in that experiment. Pronounceable nonwords were used in Experiment 2.

Method

The two tasks in this experiment were the lexical decision task and the animacy judgment task. There were 13 participants in the lexical decision acquisition group and 16 participants in the animacy judgment acquisition group. The materials consisted of four- to eight-letter words (M_{freq} = 27.1, SD = 39.9) and nonwords. Nonwords were presented only when the participants were engaged in the lexical decision tasks at acquisition or at test; no nonwords were presented during animacy judgments. The nonwords were pronounceable strings. The acquisition and test tasks each began with 8 practice trials, which were followed by 40 experimental trials in the animacy judgment condition and 60 experimental trials in the lexical decision condition, which included 20 nonword experimental trials. The interval between a participant's response and the next ready signal was 1,500 msec.

Results

The same-task conditions (animacy acquisition to animacy test and lexical acquisition to lexical test) both showed priming effects and greater degrees of transfer than did their comparable cross-task test conditions. The cross-task transfer conditions showed an asymmetrical pattern of priming: The cross-task transfer from animacy acquisition to lexical test showed some evidence of priming, but no cross-task transfer was found in the lexical to animacy transfer condition.

EXPERIMENT 3

The purpose of Experiment 3 was to verify the lack of transfer in the lexical to animacy cross-task transfer condition.

Method

Experiment 3 involved only two within-subjects conditions: lexical to lexical and lexical to animacy. There were 19 participants. In other respects, the method was the same as that in Experiment 2, including a 1,500-msec between-trials interval.

Results

The same-task condition (lexical to lexical) showed substantial priming effects. The different-tasks, lexical to animacy, condition showed no cross-task transfer priming.

EXPERIMENT 4

The purpose of Experiment 4 was to assess whether the results of Experiment 3 would be replicated when the animacy task was replaced with another task that also emphasized perceptual/conceptual properties of the objects referred to by the words.

Method

Experiment 4 involved two within-subjects conditions: lexical to lexical and lexical to bigness. There were 18 participants. Word frequency statistics were M_{freq} = 17.4, with SD = 33.6. In other respects, the method was the same as that in Experiment 2, including a 1,500-msec between-trials interval.

Results

The same-task condition (lexical to lexical) showed substantial priming effects. The different-task condition (lexical to bigness) showed no reliable cross-task transfer priming.

EXPERIMENT 5

The purpose of Experiment 5 was to assess same-task and cross-task transfer between two tasks that tend to emphasize the surface features of the presented items, the lexical decision task and the E-check task.

Method

The two tasks in this experiment were the lexical decision task and the E-check judgment task. There were 20 participants in the lexical decision acquisition group and 20 participants in the E-check acquisition group. The materials consisted of four-letter words (M_{freq} = 23.7, SD = 43.9) and nonwords. Nonwords were presented only when the participants were engaged in the lexical decision tasks at acquisition or at test; no nonwords were presented during E-check judgments. The nonwords were generally pronounceable strings. The acquisition and test tasks each began with 10 practice trials, which, during acquisition, were followed by 48 experimental trials in the E-check judgment condition and 96 experimental trials in the lexical decision condition, which included 48 nonword experimental trials. Test tasks consisted of 32 experimental trials in the E-check judgment condition and 64 experimental trials in the lexical decision condition, which included 32 nonword trials. The interval between a participant's response and the next ready signal was 500 msec.

Results

The same-task conditions (lexical to lexical and E-check to E-check) both showed priming effects and greater degrees of transfer than did the comparable cross-task test conditions. The cross-task transfer conditions showed an asymmetrical pattern of priming. The cross-task transfer from E-check acquisition to lexical test showed some evidence of priming. No cross-task transfer was found in the lexical to E-check transfer condition.

EXPERIMENT 6

The purpose of this experiment was to assess whether the lack of transfer in the lexical to E-check condition in

Experiment 5 would be replicated when the E-check task was replaced with another task that also emphasized letter/ graphemic properties of the presented words.

Method

Experiment 6 involved two within-subjects conditions: lexical to lexical and lexical to vowel count. There were 19 participants. Word frequency statistics were $M_{freq} = 123$, with $SD = 238$. In other respects, the method was the same as that in Experiment 2, including a 1,500-msec between-trials interval.

Results

The same-task condition (lexical to lexical) showed substantial priming effects. The different-task condition (lexical to vowel count) showed no cross-task transfer priming.

EXPERIMENT 7

The purpose of Experiment 7 was to assess same-task and cross-task transfer between two tasks that were both oriented to properties of the conceptual referent of the presented items—that is, animacy and bigness judgments.

Method

Experiment 7 involved animacy and bigness judgment tasks. Both tasks involved judgments of the perceptual/conceptual properties of the objects referred to by the words. There were 32 participants in the animacy judgment acquisition group and 32 participants in the bigness judgment acquisition group. The materials consisted of four- to eight-letter words ($M_{freq} = 28.2$, $SD = 43.3$). The acquisition and test tasks each began with 8 practice trials, which were followed by 40 experimental trials. The interval between a participant's response and the next ready signal was 1,500 msec.

Results

The same-task conditions (animacy to animacy and bigness to bigness) both showed priming effects and greater degrees of transfer than did their comparable cross-task test conditions. The cross-task transfer conditions showed an asymmetrical pattern of priming. The cross-task transfer from animacy acquisition to bigness test showed evidence of priming. The cross-task transfer priming from bigness acquisition to animacy test was not significant.

EXPERIMENT 8

The purpose of Experiment 8 was to assess same-task and cross-task transfer between lexical decision, a task oriented to surface properties of the item presented, and like/dislike judgment, a task oriented to properties of the conceptual referent of the item presented.

Method

The two tasks in Experiment 8 were the lexical decision task and the like/dislike judgment task. There were 20 participants in the lexical decision acquisition group and 20 participants in the like/dislike judgment acquisition group. The materials consisted of four-letter words ($M_{freq} = 25.6$, $SD = 48.0$) and nonwords. Nonwords were presented only when the participants were engaged in the lexical decision tasks at acquisition or at test; no nonwords were presented during like/dislike judgments. The nonwords were pronounceable strings. The acquisition and test tasks each began with 10 practice trials, which, during acquisition, were followed by 48 experimental trials in the like/dislike judgment condition and 96 experimental trials in the lexical decision condition, which included 48 nonword experimental trials. The test tasks consisted of 32 experimental trials in the like/dislike judgment condition and 64 experimental trials in the lexical decision condition, which included 32 nonword trials. The interval between a participant's response and the next ready signal was 500 msec.

Results

The same-task conditions (like/dislike to like/dislike and lexical to lexical) both showed priming effects and greater degrees of transfer than did their comparable cross-task test conditions. The cross-task transfer conditions showed a relatively symmetrical pattern of priming, with significant priming in both cases.

EXPERIMENT 9

The purpose of Experiment 9 was essentially to replicate Experiment 8.

Method

The two tasks in this experiment were the lexical decision task and the like/dislike judgment task. There were 14 participants in the lexical decision acquisition group and 15 participants in the like/dislike judgment acquisition group. The materials consisted of four- to eight-letter words ($M_{freq} = 34.0$, $SD = 72.8$) and nonwords. Nonwords were presented only when the participants were engaged in the lexical decision tasks at acquisition or at test; no nonwords were presented during like/dislike judgments. The nonwords were pronounceable strings. The acquisition and test tasks each began with 8 practice trials, which were followed by 40 experimental trials in the like/dislike judgment condition and 60 experimental trials in the lexical decision condition, which included 20 nonword experimental trials. The interval between a participant's response and the next ready signal was 1,500 msec.

Results

The results replicated the findings of Experiment 8. The same-task conditions (like/dislike to like/dislike and lexical to lexical) both showed priming effects and greater degrees of transfer than did their comparable cross-task test conditions. The cross-task transfer conditions showed a relatively symmetrical pattern of priming, with significant priming in both cases.

EXPERIMENT 10

The purpose of Experiment 10 was to assess same-task and cross-task transfer between two tasks that were both oriented to properties of the conceptual referent of the presented items—that is, animacy and like/dislike judgments.

Method

The two tasks in Experiment 10 were the animacy judgment task and the like/dislike judgment task. There were 20 participants in the animacy judgment acquisition group and 20 participants in the like/dislike judgment acquisition group. The materials consisted of four-letter words (M_{freq} = 22.1, SD = 39.3). The acquisition and test tasks each began with 10 practice trials, which, during acquisition, were followed by 48 experimental trials. Test tasks consisted of 32 experimental trials. The interval between a participant's response and the next ready signal was 500 msec.

Results

The same-task conditions (like/dislike to like/dislike and animacy to animacy) both showed priming effects and greater degrees of transfer than did their comparable cross-task test conditions. Both cross-task transfer conditions showed significant priming, but in an asymmetrical pattern, with the animacy to like/dislike condition showing a relatively greater degree of cross-task transfer, as compared with the like/dislike to animacy condition.

EXPERIMENT 11

The purpose of Experiment 11 was essentially to replicate Experiment 10.

Method

The two tasks in Experiment 11 were the animacy judgment task and the like/dislike judgment task. There were 32 participants in the animacy judgment acquisition group and 32 participants in the like/dislike judgment acquisition group. The materials consisted of four- to eight-letter words (M_{freq} = 21.3, SD = 40.5). The acquisition and test tasks each began with 8 practice trials, which were followed by 40 experimental trials. The interval between a participant's response and the next ready signal was 1,500 msec.

Results

The results replicated the findings of Experiment 10. The same-task conditions (like/dislike to like/dislike and animacy to animacy) both showed priming effects and greater degrees of transfer than did their comparable cross-task test conditions. Both cross-task transfer conditions showed significant priming, but in an asymmetrical pattern, with the animacy to like/dislike condition showing a relatively greater degree of cross-task transfer, as compared with the like/dislike to animacy condition.

EXPERIMENT 12

The purpose of Experiment 12 was to assess same-task and cross-task transfer between two tasks that were both oriented to properties of the conceptual referent of the presented items—that is, hard/soft and like/dislike judgments.

Method

The two tasks in Experiment 12 were the hard/soft judgment task and the like/dislike judgment task. There were 32 participants in the hard/soft judgments acquisition group and 32 participants in the like/dislike judgment acquisition group. The materials consisted of four- to eight-letter words (M_{freq} = 33.6, SD = 75.7). The acquisition and test tasks each began with 8 practice trials, which were followed by 40 experimental trials. The interval between a participant's response and the next ready signal was 1,500 msec.

Results

The same-task conditions (like/dislike to like/dislike and hard/soft to hard/soft) both showed priming effects and greater degrees of transfer than did their comparable cross-task test conditions. Both cross-task transfer conditions showed significant priming, but in an asymmetrical pattern, with the hard/soft to like/dislike condition showing a relatively greater degree of cross-task transfer, as compared with the like/dislike to hard/soft condition.

EXPERIMENT 13

The purpose of Experiment 13 was to assess same-task and cross-task transfer between two tasks that were both oriented to properties of the conceptual referent of the presented items—that is, hard/soft and bigness judgments.

Method

The two tasks in this experiment were the hard/soft judgment task and the bigness judgment task. There were 32 participants in the hard/soft judgment acquisition group and 32 participants in the bigness judgment acquisition group. The materials consisted of four- to eight-letter words (M_{freq} = 15.8, SD = 29.7). The acquisition and test tasks each began with 8 practice trials, which were followed by 40 experimental trials. The interval between a participant's response and the next ready signal was 1,500 msec.

Results

The same-task conditions (bigness to bigness and hard/soft to hard/soft) both showed priming effects and greater degrees of transfer than did their comparable cross-task test conditions. Both cross-task transfer conditions showed significant and comparable (i.e., a relatively symmetrical pattern of) priming.

GENERAL DISCUSSION

The overall patterns of results can be summarized as follows: (1) Across the 13 experiments, the same-task conditions show the greatest transfer; (2) in Experiments 1–7, the patterns of different-task cross transfer are asymmetrical, with one of the conditions showing nonsignificant transfer; (3) in Experiments 8–13, the patterns of cross-task transfer are either asymmetrical or symmetrical, with all the conditions showing at least minimal transfer. These patterns suggest some general conclusions concerning the utility of the TAP perspective and the mechanisms that underlie implicit priming.

As would be expected from a TAP perspective that emphasized specific intentional tasks, in general, same-task conditions show greater priming than do different-task conditions. These findings emphasize the importance of specific task transfer. Although the point may seem obvious, the results indicate that whatever explanation one devises for the results, the accounts must attribute a major proportion of the effect to specific interactions of specific intentional tasks with specific intended objects. We will briefly reconsider the importance of same-task condition results following discussion of the cross-task transfer findings.

The asymmetric patterns of cross-task transfer in the different-task conditions in Experiments 1–7 put further constraints on viable theoretical accounts. TAP accounts that are based on overlap of general perceptual or conceptual processes are too coarse-grained to capture the patterns of data. Given general ideas of *perceptual* or *conceptual* processing, there must be some overlap in processing among the tasks in all seven experiments. In some sense, people must perceive letters to decide the lexical validity of letter strings, but this does not guarantee transfer based on this shared processing. Likewise, in some sense, one must know that a string is a word in order to judge that string for perceptual or conceptual properties of the objects referred to by those words, but this does not guarantee that such overlap in processing will mediate repetition priming from lexical decision to animacy or bigness judgments. Instances of no cross-task transfer priming in Experiments 1–7 require explanation beyond the positing of a shared general process.

Vriezen et al. (1995), facing similar asymmetric patterns of cross-task transfer, suggested that accounts based on shared processes might be supplemented with a distinction between different stages or levels of processing. For example, if the acquisition task focuses on a higher conceptual level of processing, such as animacy judgments, one might find transfer to a lower perceptual or lexical level of processing, such as lexical decision. In contrast, lower levels of acquisition processing, such as lexical decision, may not result in transfer to higher levels of conceptual processing, such as animacy judgments. Although this stages-of-processing explanation might reflect some aspects of the mechanisms underlying priming, other aspects of the data suggest limitations in this type of account. For example, E-check, a task involving a lower level of letter processing, primes lexical decision, a task that could be construed as involving a higher level of processing, whereas lexical decision does not prime lower level judgments, such as E-check and vowel counting. Tasks that might intuitively involve different levels of processing, such as lexical decision versus pleasantness judgment, do result in cross-task priming. A hierarchy of stages or levels does not seem to capture important aspects of the data pattern.

Consider the following sketch of an alternate conceptual account that might supplement, or possibly supplant, explanations based on stages of processing. The proposed conceptualization is admittedly post hoc to the data and, as such, is offered as a view that organizes most of the findings and provides hypotheses for further investigation. The data patterns suggest that priming may be the result of two distinct types of processing overlap between acquisition events and test events. Please note that the following description will be couched in terms of overlap or similarity in tasks, but we are assuming throughout that the process overlap actually pertains to the similarity between intentional task $\times$ intended object interactions that occur during encoding at acquisition and later test. Since the intended objects (i.e., old words) are held constant across acquisition and test, the description is simplified by simply referring to the between-tasks relations. But as was previously noted, we assume that any priming that occurs in the present designs must be due to encodings of specific tasks interacting with specific words; any general task-to-task transfer effects generalized across words are statistically controlled in the present designs.

We propose that one type of process overlap that can result in priming is based on intentional processes that have been automatized and are automatically elicited by the presented words during acquisition. We suggest that these automatized encodings can occur independent of, and parallel to, the intentional processes that are associated with the experimentally assigned tasks. Furthermore, we suggest that these automatized processes mediate priming only when the task actually engaged during test matches with processes automatically activated during acquisition. Priming involving the lexical decision and the pleasantness judgment tasks appear to involve such automatized processes. A second type of mechanism that can result in priming involves sets of processes that are elicited by the specific tasks during acquisition and test and that involve actual overlap in processing between the specific tasks that are engaged in the two phases of the experiment. The patterns of results involving animacy, hardness, bigness, and pleasantness judgments appear to involve such overlapping processes. This type of priming occurs only when the tasks during the two phases actually share some processes. In summary, priming will occur when automatic responses at acquisition match intentional processes at test or when intentional processes at acquisition match intentional processes at test.

Consider the case for priming based on automatized processes, first for lexical decisions, then for pleasantness judgments. Adult readers have extensive experience identifying words. These processes are automatized (witness, e.g., that we cannot look at a correctly spelled word in our native language without understanding it) and occur spontaneously when single words are presented in an experiment, even when the experimentally assigned task involves other types of judgments, such as animacy. We suggest that such spontaneous acts of word identification result in priming when the test task involves word identification processes, such as lexical decision. This could account for the cases of cross-task transfer priming of lexical decision following a variety of different acquisition tasks, including animacy, E-check, and pleasantness judgments. Vriezen et al. (1995) report a similar pattern of cross-task transfer that is open to the same interpretation. In their third experiment, lexical decisions crossed with "man-made" judgments between acquisition and test showed cross-task transfer in the man-made to lexical condition. Also, in their fifth experiment, which crossed a lexical decision task with a naming task (both of which intuitively involve some type of word identification processes), priming was evidenced in both cross-task transfer conditions. The fact that lexical decisions show priming following such a wide variety of tasks suggests that the acquisition processing that mediates this priming is probably processing that is occurring independently of the specific task that is imposed by experimental instructions. However, as contrary evidence, it should be noted that when Gorfein and Bubka (1997) crossed lexical decisions with ambiguity judgments, they found little evidence for priming in either cross-task transfer condition. The lack of priming in the ambiguity to lexical condition suggests there are additional constraints on the effects related to such automatized word identification processes.

The pleasantness judgment results also indicate the operation of automatized processes spontaneously elicited by the stimuli. Previous research suggests that basic attitude reactions, such as like versus dislike reactions, are automatically elicited simply by exposure to individual words. For example, Bargh, Chaiken, Raymond, and Hymes (1996) and Franks, Roskos-Ewoldsen, Bilbrey, and Roskos-Ewoldsen (1998) showed that the relative positive versus negative normative valence of words affects speed of pronunciation and lexical decisions, respectively. Thus, in the present experiments, it is probable that the words presented during acquisition automatically elicited like or dislike reactions, even though the required acquisition task was different, such as animacy judgment or lexical decision. If so, this spontaneous reaction could lead to the priming that occurs for like/dislike test tasks following a variety of different acquisition tasks, including lexical, animacy, and hardness judgments.

It is important to be clear that this account assumes that the priming based on such automatized reactions will occur only when the reactions that spontaneously occur during acquisition match the processes elicited by the actual intentional task that is required at time of test. Without this latter assumption, very different patterns of data would have been obtained. If priming were simply based on the overlap of spontaneously elicited automatized reactions between acquisition and test, priming should have been found in all of the cross-task transfer conditions. In all of the experiments, the individual words during acquisition would elicit word identification and basic attitude reactions, and likewise, the individual words during test would also automatically elicit these same reactions. If this was all that was required for priming, priming should have been ubiquitous across the experiments. Because priming was not ubiquitous, we suppose that neither automatized reactions nor explicitly required reactions during acquisition will mediate transfer simply by matching spontaneous automatized reactions during test. Rather, priming that is due to automatized reactions will occur only when the automatized reactions during acquisition match the processes invoked by the intentional act at test.

We speculate that there is a second type of process overlap that mediates priming that involves different intentional tasks' sharing a process in common. This notion is closely related to the idea of sharing stages or levels in common, but we suggest that the shared processes that can mediate priming are quite specific in nature and that these shared specific processes are not obligatory stages with set serial orderings. As was previously discussed, such general or obligatory processes would result in ubiquitous priming across all conditions, contrary to the obtained data.

The results that seem pertinent to this second type of process overlap involve the animacy, hardness, bigness, and pleasantness judgments. Intuitively (and that is all our claim is at this point, a speculation based on intuition), all of these tasks seem to engage perceptual–conceptual processes that involve the referent of the word, in contrast to the properties of the word per se. We hypothesize that the patterns of cross-task transfer among these tasks are based on the tendency to strategically engage in imaging of the referent as part of the judgment processes. If these imaging processes are engaged during both acquisition and test, the shared imaging processes could be a source of the priming that is found between these tasks. This hypothesis could be investigated directly in TAP designs that crossed timed imagery judgments with these different intentional tasks or indirectly with designs that manipulated the ease of imaging the words or included secondary tasks that interfered with imaging.

The cross-task transfer between bigness and hardness judgments showed a symmetric pattern of priming, suggesting that these tasks shared a common set of processes, with additional processes being unique to the particular tasks, as is shown by the even higher levels of same-task transfer. Interestingly, the cross-task transfer between pleasantness judgments and animacy and hardness judgments was asymmetrical. Animacy and hardness judgments led to greater degrees of pleasant judgment priming than occurred in the converse cases. This asymmetric pattern suggests that both of the proposed types of process overlap may be operative in these cases. That is, when the acquisition task is either the animacy or the hardness judgment, subsequent facilitation of pleasantness judgments could be due to both the automatized like/dislike responses and to the referent imaging processes. The corollary is that pleasantness judgments lead to enhanced animacy and hardness judgments via shared referent imaging processes but that automatic processes related to animacy and to hardness do not occur spontaneously during acquisition.

In Experiment 7, the transfer from animacy to bigness was significant, but the bigness to animacy judgments condition was not, although the difference approached one-tail significance in the expected direction. Given the above discussion, one might have expected that bigness judgments would elicit imaging processes and, thus, priming from bigness to animacy judgments would have occurred. Further research can indicate whether this asymmetry is evidence contrary to the speculative hypothesis or a case of Type II error.

A general area for future research is the empirical mapping of the space of overlapping processes among the wide variety of possible implicit tasks. If the present interpretation of the data is accurate, this endeavor will involve separating effects that are due to the two different types of processes—that is, stimulus-elicited automatized processes and task-elicited shared processes. It is the latter effects that would be particularly pertinent to understanding the structure of implicit memory. The present experiments were not designed to make the precise quantitative measurements of particular same-task or different-task cross-mapping differences that would be necessary for such modeling processes. However, in anticipation of such modeling efforts, we would like to emphasize the importance of conditions involving the same intentional tasks at acquisition and test. Performance in such same-task conditions will provide reference points for scaling the sizes of priming effects as a part of such modeling efforts. Cross-task transfer priming effects can be scaled as the proportions of the interval between zero priming and same-task priming, rather than simply as differential differences from zero priming. Such measures could have advantages—for example, in comparing tasks with intrinsically different speeds of baseline performance.

The TAP perspective was originally proposed as a constraint on the explanatory power of the levels-of-processing account of explicit memory effects (Bransford et al., 1979; Morris et al., 1977). Investigations of implicit memory have also explored the levels-of-processing manipulations, usually in the context of contrasting explicit and implicit processes. With some equivocation, the general conclusion seems to be that levels-of-processing manipulations affect explicit memory but not implicit memory (see Roediger & McDermott, 1993). Much of the work used implicit memory measures, such as word fragment or word stem completion. If one considers other implicit memory measures, this general conclusion regarding levels-of-processing effects is questionable. The present results illustrate this point. For example, consider the conditions comparing lexical decision and animacy judgments. Animacy judgments during acquisition would be considered a higher level of processing than lexical decision. When the test involved animacy judgments, animacy acquisition resulted in greater priming, an effect that could be seen as evidence of a levels-of-processing effect on implicit memory. But, of course, if the test involved lexical decision, lexical decision during acquisition resulted in greater priming, a finding in direct contrast to a levels-of-processing view. Examination of results across the experiments suggests similar equivocation in cases in which the two acquisition tasks are tested by a different implicit memory task. The point is that whether or not one finds levels-of-processing effects in implicit memory depends on the relation between acquisition and test tasks, a state of affairs that strongly raises questions as to whether a levels-of-processing account is an appropriate way of looking at the findings.

Finally, it should be noted that previous work investigating implicit memory from the TAP perspective was largely concerned with the relations between implicit and explicit memory processes. In contrast, the present work is focused on relations among implicit memory processes and is not concerned with explicit memory. Nevertheless, it is possible to create designs that are analogues of the present designs that would involve reaction time measures of same- and cross-task performance when one of the tasks is an explicit memory task, such as item recognition. It remains to be seen whether the patterns of priming obtained with such an approach will help to elucidate the relations between implicit and explicit memory processes.

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NOTES

1. Across experiments, 3 participants' data were eliminated from analyses because they did not attempt to perform the tasks but merely held down a single response key throughout the experimental session.

2. The number of participants tested in the different experiments was not always an exact multiple of 8, so counterbalancing was not complete in all cases. The counterbalance scheme within acquisition conditions was such that the old/new variable would be balanced for multiples of 2 participants, items in task conditions would be balanced for multiples of 4 participants, and items in the same- versus different-test conditions would be balanced for multiples of 8 participants. Given the replication of conditions across experiments and the general nature of the effects sought, the degree of counterbalancing actually obtained was deemed sufficient.

3. We report RTs summed across both *correct* and *error* responses because we were reluctant to introduce potential biases in the data that were due to selective elimination of different numbers of responses from different conditions by different participants (cf. Ulrich & Miller, 1994). We did conduct analyses on *correct* responses only, and the patterns of results remain as reported in the paper.

4. Word frequency data are based on Kučera and Francis (1967).

5. In Experiments 1, 5, 8, and 10, only 32 of the possible 48 acquisition items were used as old items, 16 each in Phase 1 and Phase 2. The remaining 16 acquisition items were used in a third testing phase that is not discussed in the present paper.

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