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Separating Implicit from Explicit Retrieval Processes in Perceptual Identification

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Light and Kennison (this issue) proposed that bias effects in the forced-choice perceptual identification of words result from a strategy engaged in by subjects to retrieve explicit information about the words. This article enumerates several problems with this proposal and presents new experimental data against it. It is concluded that subjects do not ordinarily employ an explicit retrieval strategy. The data are discussed in the context of the general problem of separating implicit from explicit influences on performance. © 1996 Academic Press

Over the past decade, interest in the phenomena of implicit memory has exploded, fueled by the potential for new insights into the world of the unconscious. A large number of new results from both old and new tasks and from previously untapped subject populations has accumulated in a relatively short period of time. Not surprisingly, the pace at which data have been collected has not been matched by the construction of models to explain them. Recently, however, Ratcliff and McKoon (1997) have proposed a detailed information processing model, the counter model, to explain how prior experience implicitly affects performance in the perceptual identification of words. This model rests on the assumption that the effect of prior experience has an implicit source. Light and Kennison (this issue) question this assumption. They claim that, in some circumstances, the influence on performance of prior study of the words in a perceptual identification test is due to subjects engaging in a deliberate and conscious strategy to recall words that were studied. In this article, we enumerate problems with their claim, show that it should be rejected, and discuss how the retrieval of explicit information about prior episodes can best be separated from implicit retrieval.

The influence of prior experience on perceptual identification is illustrated by the data in Table 1, from a forced-choice experiment by Ratcliff and McKoon (1997). The experiment consisted of two phases, study of a list of words and tests of those words mixed with new words. Each test word was flashed briefly and following a test word, subjects were given two alternatives and asked to decide which of the alternatives matched the flashed test word. When the two alternatives were similar to each other, prior study gave both costs and benefits. For example, suppose the flashed target word was *died.* If it had been previously studied, there was a benefit to performance, an increase in the probability of correctly choosing *died* from the

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TABLE 1
Probability Correct in Forced Choice

Study condition	Similarity of forced-choice alternatives: Similar (died vs. lied)	Dissimilar (died vs. sofa)
Proportion of similar alternatives = .8		
Target studied	.85	.83
Distractor studied	.66	.88
Neither studied	.75	.87
Proportion of similar alternatives = .2		
Target studied	.74	.78
Distractor studied	.51	.77
Neither studied	.62	.79
Instructed subjects (Experiment 1)		
Target studied	.85	.83
Distractor studied	.38	.49
Neither studied	.59	.65

Note. The data with the proportion of similar alternatives are .8 and .2 are from Ratcliff and McKoon (1997). The instructed subjects are from Experiment 1 reported in this article.

similar alternatives *died* and *lied.* But when *lied* had been studied, there was a cost to performance, a decrease in the probability of correctly choosing *died* from the alternatives *died* and *lied.* However, when the two alternatives were not similar to each other (e.g., *died* and *sofa*), there was no significant effect of prior study. There was no benefit from prior study of *died* and no cost from prior study of the alternative *sofa.* In sum, with similar alternatives, but not with dissimilar alternatives, subjects showed a bias to respond with a previously studied word. This pattern was the same whether the proportion of tests with similar forced-choice alternatives was high, .8 (top panel in Table 1), or low, .2 (middle panel). Bias is also found in a range of other tasks (Ratcliff & McKoon, 1996; Ratcliff, Allbritton, & McKoon, 1997).

The data in Table 1 show bias in perceptual identification with a forced-choice procedure. Bias can also be obtained with a naming procedure. Instead of subjects being given two alternatives from which to choose the flashed target, they are asked to name the target word aloud. Prior study of the target typically benefits performance, increasing the probability of producing a correct response. But prior study of a word similar to the target increases the probability of reporting that word in error, a cost (Ratcliff & McKoon, 1997).

The counter model is an update of Morton's (1969) logogen model. It explains bias as the effect of prior experience on decision processes. The counter for a previously studied word attracts evidence away from the counters for competing words, making itself more likely to be chosen as the response. Counters can attract only from nearby counters for similar words, not from far away counters for dissimilar words, so bias appears only with similar alternatives. The counter model provides accurate quantitative as well as qualitative predictions for the effects of prior study on several different perceptual identification tasks, including the forced-choice and naming tasks.

In the counter model, bias is simply a by-product of normal word identification processes. The counter model locates bias—the implicit memory effect—in the heart of normal identification processes. Thus, Light and Kennison's (this issue) proposal that bias is due to strategic recall of studied words presents a direct challenge. Light and Kennison limit their proposal to forced choice; they say that the naming procedure does not involved deliberate recall. Their proposal is also limited to forced choice with similar alternatives, although they give no reason for this limitation. That is, they give no explanation for why subjects deliberately recall studied words when the forced-choice alternatives are similar but not when they are dissimilar. For forced choice with similar alternatives, they offer three lines of support for their claim of deliberate recall: First, they attempt to show that variables known to affect the explicit retrieval of episodic information also affect bias. Second, they attempt to show that the only subjects who show bias are those who say they ''guessed'' using a strategy based on recall of words from the study list. Third, they attempt to show that the performance of self-reported guessers is equivalent to the performance of subjects who are explicitly instructed to guess.

In this article, we demonstrate that the arguments presented by Light and Kennison (this issue) are not convincing. The main issue is how to distinguish the implicit influences of prior episodes on performance from the retrieval of explicit information about those episodes. We suggest that within-task manipulations of retrieval processes are required. We first list problems with other methods of distinguishing implicit from explicit retrieval and then illustrate a within-task retrieval manipulation.

HOW NOT TO SEPARATE IMPLICIT INFLUENCES ON MEMORY FROM EXPLICIT RETRIEVAL

Functional Dependence

''Functional independence'' is one criterion for separation that has frequently been proposed (Schacter & Tulving, 1994). According to this criterion, the separation of implicit from explicit retrieval is revealed when some experimental variable affects performance on an implicit task but does not affect performance on a task that requires the explicit retrieval of episodic information or when a variable does not affect performance on an implicit task but does affect performance on the explicit task. Light and Kennison turn this independence criterion into a dependence criterion: when a variable that affects performance on a task that requires explicit retrieval also affects performance on an implicit task, then it can be concluded that performance on the two tasks depends on the same source of information in memory, not on separate sources of information. The fallibility of the functional independence criterion has been pointed out frequently (e.g., Hintzman, 1990; McKoon, Ratcliff, & Dell, 1986; Ratcliff & McKoon, 1986; Roediger, 1990), and functional dependence is no less problematic.

Light and Kennison attempted to show functional dependence between bias in forced-choice perceptual identification and explicit retrieval by manipulating three variables: They predicted that a very long study list should decrease bias because it decreases the retrieval of episodic information relative to a shorter list; that counting the vowels in study words should decrease bias because it decreases episodic retrieval relative to rating the words for their pleasantness; and that older adults should show decreased bias because they show decreased episodic retrieval relative to young adults.

What is wrong with this reasoning is that functional dependence between two tasks does not demonstrate that performance in the tasks depends on the same source of information in memory. A single variable can affect retrieval of information from multiple sources in memory. For example, more words from a categorized list can be stored in short-term memory than from a random list, and more words from a categorized list can be retrieved from long-term memory. The fact that the categorization variable affects retrieval from both short- and long-term memory in the same way does not necessitate giving up the short-term versus long-term distinction. Likewise with perceptual identification and explicit retrieval tasks: at some level, bias in perceptual identification must depend on retrieval of information based on the prior encoding episode just as explicit retrieval must. But retrieval in the two cases could depend on exactly the same encoded information or on different sources of information, and a single variable might affect retrieval from multiple sources in the same or different ways.

Although the most important critique of Light and Kennison's empirical manipulations concerns the fallibility of functional dependence reasoning, we point out in passing that none of the three variables they used yielded findings that were consistently in accord with their predictions. A very long study list eliminated bias for many of their subjects even though cued recall was well above chance (Experiment 3, but see Ratcliff & McKoon, 1997, Experiment 7, where a long study list did not eliminate bias in forced choice); some subjects who showed bias did not show more bias when study words were rated for pleasantness than when vowels were counted (''standard guessers,'' Experiment 5), and older adults did not generally show less bias than young adults.

Subject Questionnaires

In addition to the empirical manipulations just described, Light and Kennison attempted to support their claim of explicit retrieval in perceptual identification with subjects' reports of their own strategies. On a questionnaire given after the perceptual identification test, those subjects who reported a conscious strategy of guessing the one of the forced-choice alternatives that had been on the study list were the subjects who showed bias. Subjects who reported that they did not use this strategy did not show bias.

A subject's report that he or she responded by guessing cannot be taken as conclusive, or even persuasive, evidence that responses actually were determined by guessing. Suppose on the contrary, that the bias shown in a subject's responses was due to the unconscious influences of implicit information. The subject might well notice that his or her responses tended to be the alternatives that had been on the study list. To what is the subject to attribute this tendency? Likely, to guessing; for the typical subject, no other explanation would come to mind. In fact, Light and Kennison's

TABLE 2
Probability Correct: Idealized Data for Examining Subject–Item Interactions

	Group 1		Group 2		
Study condition	Subject 1	Subject 2	Subject 3	Subject 4	Means
Target studied	.60 item a	.60 item a	.80 item a	.80 item a	.70
Neither target nor distractor studied	.70 item b	.70 item b	.50 item b	.50 item b	.60

subjects did not even have to think up the guessing explanation themselves; it was explicitly offered to them as a possibility on the questionnaire.

Selecting Subjects out of Counterbalanced Designs

The uninformative nature of subjects' attributions is one problem with Light and Kennison's use of the questionnaire data. Another is a possible subject selection artifact. Light and Kennison used a counterbalanced Latin square design. This design ensures that every subject and every test item appear in every experimental condition equally often, but at the expense of confounding subjects and items. Selecting out subjects who were guessers from subjects who were nonguessers runs the risk of selecting unequally from different levels of the counterbalancing factor. Unequal selection destroys the design because items are no longer equally represented across conditions. Consider the idealized data shown in Table 2, where items a and b represent two different groups of items. Subjects in Group 1 received item a in the study target condition and item b in the no study condition; subjects in Group 2 had the reverse assignment. Overall, there is a 10% benefit of having studied the target word. Eliminating subject 4 reduces the benefit to almost nothing (3%).

The problem is that when subject 4 is eliminated, a high baseline item (b) is eliminated from the study condition and a low baseline item (a) is eliminated from the no study condition. The reduction in benefit from 10 to 3% cannot be inextricably assigned to the elimination of subject 4; it could equally well be due to the elimination of item b from the study condition and/or the elimination of item a from the no study condition.

Furthermore, consider the situation from the point of view of subject 4, who has a large benefit (30%). The subject might notice that he or she was frequently choosing the alternative from the study list and might attribute this to guessing. However, there is another likely explanation, that item b in the study condition was very easy and item a in the no study condition was very difficult.

To select subjects from counterbalanced Latin square designs, they should be selected equally from each counterbalancing group. This is especially true for Light and Kennison's experiments where the measure was percentage correct and there were only 10 items in each experimental condition, so that eliminating even one or two extra subjects from one group versus another could change the pattern of results. Light and Kennison did not report that they selected equally, so whether their analyses of data from guessers versus nonguessers are subject to selection problems is unclear.

Separating Subjects by Effect Size

Light and Kennison (last page of Discussion) also point out that some of their subjects showed large bias effects while others showed no bias effect at all. With a counterbalanced Latin square design, this difference among subjects (as stated) is not meaningful. Subject 2 in Table 2, for example, does not appear to show a benefit of prior study. But this may be only because of the particular assignment of items for subject 2—the difficult item a in the study condition and the easy item b in the no study condition.

Differences among subjects in the amount of bias observed in their data are also to be expected when variability is taken into account. Suppose that the true benefit for a subject is .1, with the true probability correct in the no study condition .7 and the true probability correct in the study condition .8. Assuming binomial variability and using the normal approximation to the binomial, then the probability that the subject's data actually show any benefit is only .7 ($z = .53$); there is a .30 probability that the difference between the study and no study conditions will be 0 or less. In addition to this within-subject variability, there is also between-subjects variability. The size of the true benefit will be different for different subjects. Taking these sources of variability in the amount of bias into account, the proportions of subjects showing facilitation and inhibition effects in Light and Kennison's experiments are close to what might be expected (see Ratcliff & McKoon, 1997, Fig. 2, for another sample of individual subject data).

WITHIN-TASK MANIPULATION OF RETRIEVAL PROCESSES: EXPERIMENT 1

The issue that Light and Kennison (this issue) have raised is an important one. To understand how implicit memories influence performance, it is essential to separate them from explicit memories about previous episodes. Where we disagree with Light and Kennison is in the method of separation. Light and Kennison look outside of an implicit memory task for external means of separation, looking at subjects' questionnaire responses, performance on other, possibly functionally dependent, tasks, or performance by different subject populations. These methods may be more or less convincing depending on the circumstances in which they are applied, but we suggest that separation can best be achieved by finding a diagnostic within the implicit task itself.

For perceptual identification, an obvious manipulation presents itself, namely, the use of similar versus dissimilar forced-choice alternatives. If the bias shown in perceptual identification depends, at least in part, on subjects' conscious retrieval of explicit information about words from the study phase of the experiment, then bias should appear both when the forced-choice alternatives are similar to each other and when they are dissimilar. If a subject chooses *died* because he or she consciously remembers it being on the study list, then it should not matter whether the other choice was *lied* or *sofa*. Usually, however, bias does not appear with both similar and dissimilar alternatives; it appears only with similar alternatives (as shown in Table 1). This was one of the main reasons we originally rejected retrieval of explicit information as playing a role in bias in perceptual identification (Ratcliff & McKoon, 1997; Ratcliff et al., 1989). Light and Kennison (this issue) do not explain why subjects would decide to consciously recall studied words with similar alternatives but not with dissimilar ones.

To reinforce the point that guessing based on conscious retrieval of explicit information should give bias for both similar and dissimilar alternatives, we conducted a new experiment in which we instructed subjects to guess words from the study list when they were otherwise unsure of which alternative to choose (using the same instruction as Light and Kennison had used for their instructed subjects). We expected that with this instruction, bias would obtain with dissimilar as well as similar alternatives.

The data in Table 1 which show an effect of prior study for similar but not dissimilar alternatives come from Experiment 1 in Ratcliff and McKoon (1997). For the experiment described here, we used the same materials and procedure as the earlier experiment except that we added the instruction to guess words from the study list when unsure.

Method

Materials. The same 80 triples of words were used as for Experiment 1 in Ratcliff and McKoon (1997). All three words of a triple had the same number of letters, always between 4 and 7. Two of the words of a triple had the same visual shape, and the third was as different in shape as possible, where shape refers to the outline of the letters. For example, *d* would be similar to *l* but dissimilar to *s*, and *p* would be similar to *g* but dissimilar to *h.* The two words *data* and *date* have about the same shape but the third word of their triple, *club,* has a different shape. The words were chosen from the Brown University corpus (Kucera & Francis, 1967). The two similar words of a triple differed by one similarly shaped letter or (in 34 cases) by two or more similarly shaped letters (these were words of five or more letters).

Equipment. Stimulus materials were displayed on an oscilloscope with a fast phosphor that was programmed to present words with stimulus presentation times ranging from 1 ms up, in increments of 1 ms. Words were written on the scope in letters produced by a character generator in the oscilloscope hardware. The oscilloscope was controlled by a PC and responses were collected on the PC keyboard.

Procedure. The experiment began with a block of test items used for calibration, to set individually for each subject the amount of time for which target words would be flashed. The flash time for the first four test words was set very long, 100 ms, to orient the subject to the sequence of test events. The remainder of the block consisted of a series of 50 trials. Two flash times, 15 and 30 ms, were used. Accuracy for each flash time was printed on the PC screen at the end of the trials and the experimenter used these accuracy values to choose a flash time for the subject for the remainder of the experiment, aiming at about 70% correct performance.

The calibration block was followed by four study–test blocks. The study list in each block contained 12 words and the test list in each block contained 20 test words. Words presented for study were displayed one at a time, for 1 s per word. Subjects were instructed to learn the words for a later (unspecified) test. A study list was separated from its immediately following test list by a warning signal (a row of asterisks) displayed for 2.5 s.

The sequence of events for each test item in both the calibration and the study–test blocks was as follows: a row of minus signs was presented for 400 ms as a warning signal; this was followed by a blank screen for 300 ms; then the test word was flashed; then a mask was displayed for 300 ms, covering where the test word had been; then two words were displayed side by side, and subjects had to choose which of them had been flashed. Subjects responded by pressing the *Z* key on the keyboard to indicate that the left-hand word was the flashed test word or the *?/* key to indicate the right-hand word. After the response, the warning signal began the sequence over again for the next item. The mask was a row of @ characters that were displayed in a larger font than the letters of the flashed test words so that the mask completely covered all the space that the letters had covered. Which of the two alternatives, the right or the left one, was the correct choice was decided randomly. Subjects were instructed that the flashed target words were difficult to see and that they should try hard to do figure out what they were; however, if they could not figure out what a word was, they were instructed to guess a word from the study list.

In Experiment 1 of Ratcliff and McKoon (1997), the probability of similar versus dissimilar alternatives was varied (as shown in Table 1). In the experiment reported here, the probabilities of similar versus dissimilar alternatives were held constant at .5 each. For each of the 80 triples, one of the two similar words was designated the target to be flashed in the test phase. There were two test conditions: For half of the triples, the two words that were presented for forced choice were the target and the similar word from its triple and for the other half of the triples, the words presented for forced choice were the target and the dissimilar word from its triple. There were also three study conditions. In the first study condition, the target had been presented in the study list. In the second study condition, the other of the forced-choice alternatives had been studied. In the third study condition, neither the target nor the forced-choice alternative had been in the study list. For example, for the target *died* with similar forced-choice alternatives (*died lied*), either *died* or *lied* or neither was studied. For the target *died* with dissimilar alternatives (*died sofa*), either *died* or *sofa* or neither was studied. The two test conditions and the three study conditions were crossed to form the six conditions shown in Table 1. In each of the four study–test blocks, there were three test items from each of the conditions in which one of the words of a triple had been studied and four test items from each of the no study conditions. The order of items in the study and test lists, the choices of which of the similar words in triples were designated the flashed targets, and the assignment of triples to conditions were all decided randomly, with the randomization changed after every second subject.

Subjects. The 10 subjects were recruited from the Northwestern University community and paid $8.00 each for their participation in the experiment. The flash times assigned to individual subjects varied from 12 to 15 ms (except for one subject at 32 ms), with a mean of 15.6 ms (or 13.8 ms without the subject with the long flash time).

Results

The bottom panel of Table 1 shows the mean probability correct for each condition. The main finding is that costs and benefits of prior study were obtained for dissimilar as well as similar alternatives. Instructing subjects to use a strategy of guessing the alternative that had appeared on the study list changed the usual finding that bias appears only with similar alternatives (top panels of Table 1) to bias for both similar and dissimilar alternatives.

Analysis of variance showed a significant effect of study condition, $F(2, 18) = 33.1$, that did not interact with whether the alternatives were similar or dissimilar, $F(2, 18) = 1.3$. Accuracy with dissimilar alternatives was slightly higher overall (about 5%) than with similar alternatives, as was the case for data from Ratcliff and McKoon (1997, Experiment 4), but this difference was not significant ($F(1, 9) = 2.4$). The standard error of the mean probabilities correct was .020.

Not only did instructing subjects lead to bias for dissimilar alternatives, it also increased the overall amount of bias. If the size of the bias effect is calculated as the difference in probability correct between the study target and study similar word conditions, then there is about a 2.5 times larger bias effect for instructed subjects than uninstructed subjects with similar alternatives, and the size of this difference with dissimilar alternatives is almost as large. A large increase in the amount of bias would be expected because the study lists were relatively short (12 words) and explicit memory for words from them should be good.

DISCUSSION

Light and Kennison's (1994) claim was that subjects in forced-choice perceptual identification experiments make their responses by using a deliberate strategy to recall previously studied words. If this claim were correct, then subjects who are actually instructed to recall should produce the same patterns of data as uninstructed subjects. But they do not. In the experiment reported in this article, instructed subjects showed a large bias effect with dissimilar alternatives but uninstructed subjects show no bias with dissimilar alternatives (Ratcliff & McKoon, 1997). Instructed subjects also showed a larger bias effect overall than uninstructed subjects. And, in Light and Kennison's data (Experiment 5), instructed subjects showed a larger ($3\frac{1}{2}$ times larger) bias effect with a pleasantness rating study task than a vowel counting task, but uninstructed subjects did not. In each case, the difference between the data for instructed and uninstructed subjects was the difference that would be expected if the instructed subjects were engaged in the intentional retrieval of explicit information about the presence of words on the previously studied list.

Because instructed subjects show what would be expected of intentional retrieval of explicit information and uninstructed subjects do not show this pattern of data, it might be tempting to conclude that uninstructed subjects' responses must have been based only on the retrieval of implicit information, uncontaminated by the use of explicit memory for the studied words. This would not be a valid conclusion. Uninstructed subjects might have made use of explicit memory unintentionally and they might have made use of it in different ways (affected differently by experimental manipulations) than instructed subjects, just as subjects given a recall task are assumed to engage in different uses of explicit memory than subjects given a recognition task. Instructed subjects might, for example, follow implicit retrieval with attempts at recall of explicit information about studied words. Uninstructed subjects might follow implicit retrieval with (perhaps unintentional) explicit recognition of whether a word had appeared on the study list.

The possibility that explicit retrieval processes can affect performance in an implicit task even for uninstructed subjects rules out the ‘‘retrieval intentionally criterion’’ (Richardson-Klavehn, Lee, Joubran, & Bjork, 1994; Schacter, Bowers, & Booker, 1990) as a method of separating explicit from implicit retrieval. According to this criterion, a tasks taps implicit but not explicit information if there is some explicit task for which the test stimuli are exactly the same as in the implicit task, there exist manipulations that affect performance on the explicit task but not the implicit task, and there exist manipulations that affect performance on the implicit task but not the explicit task. Ratcliff and McKoon (1995) demonstrated empirically that this criterion fails. Object decision is a task for which subjects are asked to decide whether a line drawing depicts a ‘‘possible’’ object, one that could exist in the real world, or an ‘‘impossible’’ object. Schacter, Cooper, and Delaney (1990) described the task as tapping implicit memory. It fills the retrieval intentionality criterion because recognition, a task requiring retrieval of explicit information about previously studied objects, can use exactly the same stimuli as object decision and there exist manipulations that affect performance on recognition but not object decision and vice versa. Despite filling the criterion, the object decision task does not provide a ‘‘process-pure’’ measure of implicit retrieval. Instead, performance is determined by a mixture of implicit and explicit memories. The typical result in object decision is that only decisions about possible objects, not decisions about impossible objects, are affected by prior exposure to those objects. However, the typical result can be changed by the imposition at test of a response time deadline or a memory load (Ratcliff & McKoon, 1995). Under these conditions, decisions about both possible and impossible objects are affected by prior exposure. This change in the pattern of data can be ascribed to the elimination of explicit retrieval processes because both deadline and memory load are generally thought to be manipulations that make retrieval of explicit information difficult. Thus, performance in the regular object decision task, without deadline or memory load, appears to reflect a mixture of explicit and implicit processes even though the task meets the retrieval intentionality criterion.

The retrieval intentionally criterion fails on logical grounds and the object decision task demonstrates it can also be shown to fail on empirical grounds. Thus, the criterion offers no way to decide whether the task at issue in this article, perceptual identification, is free from explicit contamination, even though it would probably pass the criterion. It could probably be shown, for example, that pleasantness rating versus vowel counting for the words in the study list would affect recognition but not perceptual identification of flashed target words, and modality of presentation of the study words would affect perceptual identification but not recognition.

Another popular technique for separating recollection of explicit information about prior episodes away from another component of processing that involves implicit or familiarity information is Jacoby’s process dissociation method (1991). Subjects perform a task under two conditions, one (‘‘inclusion’’) for which conscious recollection facilitates a class of responses and the other (‘‘exclusion’’) for which conscious recollection facilitates the suppression of that class of responses. In the suppression case, subjects might, for example, be instructed to respond negatively if they explicitly remember that a test item had appeared on the study list. There are two possible problems in using this method to separate explicit from implicit influences on memory. First, Richardson et al. (1994) point out that the information required for conscious suppression might become available as the result of implicit retrieval. In other words, the influence of implicit retrieval might be hidden by use of the very information it makes available. Second, the process dissociation method must always divide processing into two components. If performance was determined by only a single process, the same process in both the inclusion and exclusion conditions, the process dissociation method would still produce estimates of the influences of two separate processes (see Ratcliff, Van Zandt, & McKoon, 1995). Because the method offers no way to know a priori whether the number of processes underlying performance is exactly two or some other number, there is no way to know whether the method is being applied appropriately.

How then can it be decided, for any task in general and perceptual identification in particular, that performance reflects only implicit retrieval, not a mixture of implicit and explicit retrieval? We believe that examination of within-task retrieval processing has the best chance for success. First, there are a number of standard techniques available for analytic examination of retrieval processes, including full versus divided attention manipulations (Jacoby, Woloshyn, & Kelley, 1989), response time deadlines, and memory loads (McKoon & Ratcliff, 1986; Ratcliff & McKoon, 1995). By restricting either processing time or processing resources, these manipulations eliminate (or sharply curtail) the slower processes that are associated with the retrieval of explicit information. Second, there may be some finding within the data from a task itself that offers an index of explicit retrieval. For perceptual identification, the difference in forced choice data for similar versus dissimilar alternatives offers such an index. We argued that instructions to choose words from the study list removed the difference between similar and dissimilar data because the instructions induced explicit retrieval, suggesting that explicit retrieval ordinarily plays little or no part in perceptual identification. For object decision, explicit retrieval was indexed by examination of slow versus fast responses (Ratcliff & McKoon, 1997). The slower responses, those that would be expected to be associated with explicit retrieval, were affected differently by experimental variables than faster responses.

Investigation of within-task retrieval processing leads, almost inevitably, to the realization that a processing model is required. The multiple and interacting effects of different variables and different performance indices on different empirical measures are not easily combined into an understandable picture of performance without a detailed model. The approach exemplified by the retrieval intentionality criterion, for example, offers no help with untangling the changing patterns of response speed versus response accuracy when the time or resources available for processing are restricted. The counter model (Ratcliff & McKoon, 1997) is one attempt to explain multiple empirical results. It offers an explanation of how word identification processes produce performance for naming, forced choice, and single item identification procedures. It also provides a theoretical structure for simultaneously understanding the effects of experimental variables such as the amount of time for which a test word is flashed, the delay between study of words in a list and perceptual identification tests of the words, types of forced-choice alternatives, word frequency, and the number of words in the lexicon that are similar to a test word (''neighborhood size''). The development of competing models and tests of the models' differential predictions will be one important impetus for further advances in research in this domain.

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