Recollection, Fluency, and the Explicit/Implicit Distinction in Artificial Grammar Learning

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In this article, the authors propose that both implicit memory and implicit learning phenomena can be explained by a common set of principles, in particular via participants’ strategic use of recollective and fluency heuristics. In a series of experiments, it was demonstrated that manipulating processing fluency had an impact on classification decisions in an artificial grammar learning task (Experiments 1, 2, 4, and 7), showing that participants were using a fluency heuristic. Under identical conditions, however, this manipulation had no effect on recognition decisions (Experiments 3 and 5), consistent with a greater default reliance on recollection. Most significant, the authors also showed that a fluency effect can be induced in recognition (Experiments 4–6) and can be eliminated in classification (Experiment 7).

A curious paradox about current research in learning and memory is that despite the similarity of their names, research on implicit learning and on implicit memory has been largely unconnected. Implicit learning, a term coined by Reber (1967), refers to people’s ability to learn about a structured domain without being able to fully verbalize their knowledge. In contrast, implicit memory describes the influence of prior experiences on current behavior in the absence of explicit recollection of those experiences. Compared with the huge number of studies that have been published in each of the two areas, only very few studies have attempted to theoretically connect them (e.g., Berry & Dienes, 1991; Higham, Vokey, & Pritchard, 2000; Whittlesea & Dorken, 1993). In this article, we bring these areas together within a single conceptual framework. Specifically, we develop and explore a unified approach relating implicit learning in artificial grammar learning on the one hand and memory processes, namely recollection and fluency, on the other.

Recollection and Fluency

People have an impressive capacity for storing and retrieving information about particular events. This recollective memory allows people to recall specific experiences and their contexts, such as what they did on their last holiday, and has been extensively investigated psychologically (Tulving, 2002), computationally (Raaijmakers & Shiffrin, 1992), and at the neural level (O’Reilly & Rudy, 2001). However, people also have the ability to make memory decisions in the absence of recollection. In these circumstances, such decisions appear to be made on the basis of a more automatic influence of memory that researchers call fluency (Kelley & Jacoby, 2000). Jacoby and Dallas (1981) suggested that when processing an item fluently, people may attribute the fluency to the item’s repetition and hence tend to experience it as a familiar item. This is a useful heuristic because a prior encounter with a stimulus will often lead to enhanced processing of this stimulus on a later occasion. It is in accord with the fluency account that artificial enhancement of the fluency with which a novel test item is processed—via perceptual or conceptual manipulations—can induce illusions of remembering. For example, Whittlesea, Jacoby, and Girard (1990) showed that participants were more likely erroneously to judge a new word to be old if it was masked by light versus heavy visual noise. Words masked by light noise were read more fluently than ones masked by heavy noise, and Whittlesea et al. proposed that participants in turn (mis)attributed this fluent processing to a source in the past (i.e., repetition). Further support comes from a variety of studies in which illusions of familiarity were created by variations in perceptual priming (e.g., Jacoby & Whitehouse, 1989) or semantic context (e.g., Whittlesea, 1993). Whittlesea and Williams (2000, 2001a, 2001b) extended the fluency hypothesis by stating that it is not fluency per se that makes stimuli appear familiar but surprising fluency. According to their discrepancy-attribution hypothesis, which is a central element in Whittlesea’s (1997) selective construction and preservation of experience (SCAPE) account of memory, people have an expectation of how fluently they will process a stimulus, and if that expectation is violated, the extra fluency is attributed to a source in the past. Furthermore, it has been shown that people do not...
An artificial grammar. Letter strings are generated by following the arrows from the input state until the structure is left at an output state. Each time an arrow is chosen, the associated letter is appended to the string. Letter strings that are generated this way are called grammatical, whereas all other letter strings are called ungrammatical.

**Figure 1.** An artificial grammar.
A number of findings can be marshaled in support of this perspective. For instance, participants in a study by Johnstone and Shanks (2001) saw grammatical strings in the training phase under the cover of a short-term memory task. At test, novel grammatical and ungrammatical strings were presented for classification, and orthogonally, half the items were constructed from familiar letter pairs (bigrams) and triplets (trigrams) and half from unfamiliar ones. Responding was found to be controlled entirely by the familiarity of the bigrams and trigrams from which the test strings were constructed. Indeed, by pitting familiarity against grammaticality, Johnstone and Shanks were able to demonstrate a reverse grammaticality effect whereby ungrammatical strings composed of relatively familiar chunks were more likely to be called grammatical than were grammatical strings made up of relatively unfamiliar parts. On the basis of these results, Johnstone and Shanks argued that implicit knowledge in AGL experiments can be fully explained in terms of the familiarity of bigrams and trigrams and hence in terms of perceptual (and perhaps conceptual) fluency (Whittlesea, 1993; Whittlesea & Dorken, 1993). Other studies also support the idea that the familiarity of string fragments rather than knowledge of the grammar underlies successful performance in AGL experiments (e.g., Kinder, 2000; Kinder & Assmann, 2000).

Also consistent with the fluency account is the finding that if participants are asked to make affective decisions about stimuli (i.e., to judge the extent to which each test string is liked or disliked) rather than grammaticality decisions, an effect of grammatical status is again observed in that grammatical strings tend to be preferred to ungrammatical ones (Gordon & Holyoak, 1983; Newell & Bright, 2001). Similar to the basic grammaticality effect, the fluency account explains this result by proposing that grammatical strings are processed slightly more fluently than ungrammatical ones. Participants are then assumed to attribute this enhanced fluency to the dimension that has been made salient in the experimental instructions, namely, the likeability of the strings.

If grammatical test items are processed more fluently than ungrammatical ones, then one might also expect them to be identified more rapidly. Buchner (1994, Experiment 1) evaluated this prediction by presenting test strings in a perceptual clarification procedure in which each string was initially masked by visual noise that dissipated steadily over a period of a few seconds. Participants pressed a button as soon as they could identify the string. Buchner found a reliable priming effect consistent with the fluency theory: Grammatical strings were identified about 200 ms faster than ungrammatical ones. In a second experiment, Buchner asked the further question of whether the speed (i.e., fluency) at which participants perceived test strings covaried with the likelihood of their calling them grammatical, but he was unable to find such an effect. At first sight, this experiment appears to challenge the fluency theory, but as we shall show in several of the experiments we report here, such a covariation can be observed under certain conditions.

Although this research has, we believe, clarified researchers’ understanding of implicit knowledge and has yielded stronger conclusions than are supported by earlier work, two major questions remain. First, we have no direct, independent evidence that variations in processing fluency affect responding to test strings under implicit learning conditions. Even in Buchner’s (1994) studies, fluency was itself not directly manipulated; rather, natural variations in fluency across test strings were found to covary with grammatical status. Second, it is not clear what the relationship is between implicit memory (and particularly the central role of fluency) and explicit memory. Is there any evidence that performance on explicit memory tasks is less dependent on experienced variations in processing fluency than it is on implicit memory or implicit learning tasks? If so, how might we explain this finding?

Explicit Recognition Versus Implicit Classification

If explicit (e.g., classification) and implicit (e.g., recognition) performance in a structured domain are subserved to different degrees by recollection and fluency, then one might expect to observe behavioral differences in participant performance on AGL tasks when only the test demands are varied. Contrary to this prediction, Kinder and Shanks (2001) found some evidence that classification and recognition are functionally indistinguishable in the AGL task, and their results suggest that explicit and implicit memory are influenced by fluency in a rather similar manner. In two studies, participants were initially asked to memorize strings from a finite-state grammar and were then presented with some combination of novel grammatical strings, novel ungrammatical strings, and the original study items. In one case, standard classification instructions were given, whereas in another, recognition instructions were used: That is, participants were asked to judge whether test strings were old or new. Despite the difference in the normatively correct responses in the studies (in the recognition task, only the original study items should be called “old,” whereas in the classification task, both the original study items and the novel grammatical items should be called “grammatical”), there was little evidence that participants responded differently in the two cases, suggesting that the major influence on performance in both cases was the fluency of processing the different test items, differentially enhanced by prior experience of the same or similar strings. A similar set of findings was reported by Whittlesea and Dorken (1993).

In the experiments reported here, in contrast, we show that participants respond in qualitatively different ways in classification and recognition tasks. We interpret this result in terms of a shift in the degree of reliance on recollection and fluency.

Buchner (1994), in the study described above, also examined the role of processing fluency in explicit memory. Recall that participants perceived grammatical strings more rapidly in a perceptual clarification procedure than ungrammatical ones. Although this variation in processing fluency did not covary with grammaticality judgments, it did covary with recognition decisions. Thus, Buchner’s findings suggest that implicit and explicit memory may differ in their decisional bases: Under otherwise identical circumstances, processing fluency may affect one type of decision but not the other.

In the present research, we sought to ask, first of all, whether the fluency account is viable as an explanation of judgments about the general properties of a structured domain. To do this, we experimentally manipulated processing fluency. Letter strings in the test phase of an AGL task clarified over a period of a few seconds. Participants pressed a key as soon as they could read the string, the string disappeared, and they then judged whether that string was grammatical or not. The critical novel aspect of our procedure is
that some test strings clarified slightly faster than others, and we speculated that this would induce an artificial feeling of familiarity that would influence grammaticality decisions. As a result, faster clarifying strings should be endorsed more frequently than slower clarifying strings. Participants’ response times are also interesting in principle, because they allow us to determine whether grammatical strings are perceived more rapidly than ungrammatical strings, as the priming theory predicts. However, in our procedure, response time turned out not to be a pure measure of perception. In analyses of the response times, which we do not report in this article, we found instead evidence that they were influenced by both perception and decision processes. This is probably because participants knew in advance whether they had to judge the grammaticality of each test item or whether it was old or new. Thus, rather than pressing the key when they had fully identified the string, participants could press it as soon as they had sampled sufficient information to make their judgment. In Buchner’s (1994) procedure, in contrast, participants did not know what response was required until after the string had disappeared, as they had to give both recognition and classification judgments within the same experiment. As a result, in Buchner’s experiment, response times were purely dependent on perception.

Despite the fact that the speed (i.e., fluency) of processing did not covary with the likelihood of calling strings grammatical in Buchner’s (1994) experiment, several of the studies reported here do show such an effect (Experiments 1, 2, 4, and 7). We then proceeded to ask whether a similar influence of fluency is found for explicit decisions about specific instances of a structured domain, that is, in recognition decisions. In Experiment 3, we show that under conditions in which fluency does affect classification decisions, it fails to affect recognition decisions. However, the major results we report are that the influence of fluency on classification can be made to disappear (Experiment 7) and that a fluency effect in recognition can be made to appear (Experiments 4, 5, and 6). Thus, the default is different in the two types of decision and, most important, one cannot predict the processes that will be engaged in a memory decision by knowing whether it is nominally implicit or explicit. Instead, people can be induced to use information retrieved from memory in different ways, depending on an interaction between the type of decision, the types of items presented, and the task demands at test.

**Experiment 1**

In this experiment, we sought direct, independent evidence for the fluency account of grammaticality decisions in AGL. We manipulated fluency by presenting test items under a perceptual clarification procedure in which some strings clarified fractionally more rapidly than others on the display. We predicted that more rapid clarification would lead to an increased feeling of fluency, which would, in turn, increase the proportion of “grammatical” responses to test strings.

**Method**

**Participants**

Sixty-nine students (51 women and 18 men) from University College London participated in Experiment 1 to fulfill part of their course requirement. They were from 18 to 30 years old and had a mean age of 19.4 years. Participants were randomly assigned to two experimental groups, 33 to the 5/6 group and 36 to the 3/6 group (for explanations of the groups, see below).

**Stimuli**

There was one set of training stimuli as well as three sets of test strings, each of which contained 16 items. The training set was identical to Training List 1 used in Experiment 1 of Vokey and Brooks (1992) and was generated from the grammar shown in Figure 1. Test stimuli were old grammatical strings (i.e., the training strings), new grammatical strings, and new ungrammatical strings. The new grammatical and new ungrammatical strings were the List 2 items used by Vokey and Brooks. Each of these test stimuli differed from its most similar training item in at least two positions. Training and test strings were presented on the display in letters 22 mm high. Strings were from three to seven letters in length.

**Procedure**

**Training stage.** Participants were told that they were taking part in a simple short-term memory experiment. Each training stimulus was presented on the computer screen for 3 s. When it disappeared, participants had to type the string on the keyboard. If they were correct, a new string appeared. If they made a mistake, the old string was presented again. After any third incorrect entry, participants were reminded to try not to make mistakes and a new string was presented on the next trial. The training strings were presented in a randomized order. Each string was presented four times during training.

**Test stage.** Participants were told that the strings they had just seen had been generated using a complex set of rules that constrained letter order. They were instructed to classify each test string according to whether or not you have just seen follow the rules? Please press ‘Y’ for yes or ‘N’ for no” appeared. For counterbalancing assignment of items to conditions (fast vs. slow clarification) within participants, we presented the whole set of test items twice. Items randomly chosen to clarify slowly in the first half of the test stage clarified quickly in the second half, and vice versa.

**Results and Discussion**

The level of significance was set to .05 in all statistical tests. Figures 2A and 2B show the endorsement rates for the three types of test items, computed separately for both groups and for fast- and slow-clarifying items. It is evident that participants discriminated
between grammatical and ungrammatical items: Grammatical items (both new and old) were judged grammatical more often than ungrammatical items in both groups. Furthermore, old grammatical items were endorsed more often than new grammatical items. Accordingly, an analysis of variance (ANOVA) including the factors group, clarification speed, and item type revealed a significant main effect of item type, \( F(2, 134) = 397.88, MSE = 171.33, \eta^2_p = .86 \).

In accordance with our hypothesis, fast-clarifying items were endorsed more often than slow-clarifying items, \( F(1, 67) = 9.41, MSE = 91.25, \eta^2_p = .12 \). This result is consistent with the notion of attributional use of fluency in grammaticality decisions. Although the fluency effect was somewhat larger in the 3/6 group than in the 5/6 group, there was no significant Group \( \times \) Clarification Speed interaction, \( F < 1 \). However, the 95% within-subjects confidence intervals (computed according to the method of Loftus & Masson, 1994) show that there was a fluency effect for old and new grammatical items in the 3/6 group but only a marginally significant effect for old grammatical items in the 5/6 group. No other main effect or interaction in the ANOVA was statistically significant (all \( F \)s \(<1.7\), all \( p \)s \(>.19\)).

Because we counterbalanced assignment of items to conditions (fast vs. slow clarification) within participants, we had to present the entire set of test items twice. Although presenting test items twice is quite common in AGL experiments (e.g., Reber, 1967), it may have the effect that participants adopt a stricter response criterion in the second half of the test to avoid endorsing ungrammatical test items they saw in the first half. This might prevent them from relying on fluency in their judgments as much as they would if test items were not repeated. To test this hypothesis, we computed \( B^{''} \) (see Snodgrass & Corwin, 1988) as a measure of the participants’ bias in the first and second half of the test. \( B^{''} \) was

\[ \text{Figure 2. Mean endorsement rates (in percentages) for fast- and slow-clarifying items in Experiments (Exp.) 1–3. A: The 3/6 group in Experiment 1 (classification instructions). Clarification rates were either 1 pixel per 3 ms (fast) or 1 pixel per 6 ms (slow). B: The 5/6 group in Experiment 1 (classification instructions). Clarification rates were either 1 pixel per 5 ms (fast) or 1 pixel per 6 ms (slow). C: Experiment 2, classification instructions. D: Experiment 3, recognition instructions. The error bars indicate the 95% within-subjects confidence interval (computed according to the method of Loftus & Masson, 1994). old gram. = old grammatical strings; new gram. = new grammatical strings; new ungram. = new ungrammatical strings.} \]
.105 in the first half of the test and .066 in the second half—thus, the bias was actually slightly more liberal in the second half—and the two values did not differ significantly from each other, t(68) = 1.78, p = .08. Also, the false-alarm rate did not increase significantly from the first half (M = 0.289) to the second half (M = 0.313) of the test, t(68) = −1.38, p > .17, indicating that participants did not have difficulty rejecting ungrammatical items they had seen in the first half of the test.

**Experiment 2**

This experiment was designed to replicate Experiment 1 under slightly altered conditions. The results of Experiment 1 seem to suggest that the fluency effect increases as the difference between the two clarification rates rises. Therefore, in Experiment 2, we chose average rates of 1 pixel per 2 ms and 1 pixel per 6 ms, thus creating a difference between the two speeds that was even larger than the difference in the 3/6 group of Experiment 1.

**Method**

**Participants**

Thirty-two students (19 women and 13 men) from University College London took part in Experiment 2, receiving financial compensation (£5, approximately U.S.$8) for participating. They were from 19 to 50 years old and had a mean age of 27.3 years.

**Stimuli and Procedure**

Stimuli and procedure were identical to those in Experiment 1, except for the rate of the fast-clarifying test strings. These items clarified at an average rate of 1 pixel per 2 ms.

**Results and Discussion**

Figure 2C shows the endorsement rates for the three types of items, computed separately for fast-clarifying and slow-clarifying trials. As in Experiment 1, participants discriminated between grammatical and ungrammatical items: Grammatical items (new and old) were judged grammatical more often than ungrammatical items. Also, old grammatical items were endorsed more often than new grammatical items. Accordingly, an ANOVA including the factors clarification speed and item type revealed a significant main effect of item type, F(2, 62) = 136.63, MSE = 226.16, \( \eta^2_p = .82 \).

On average, fast-clarifying items were endorsed more often than slow-clarifying ones, F(1, 31) = 6.90, MSE = 76.81, \( \eta^2_p = .18 \). Although the ANOVA revealed no significant Item Type × Clarification Speed interaction, F(2, 62) = 2.66, MSE = 77.18, p = .08, the 95% confidence intervals show that only new grammatical items yielded a statistically significant fluency effect. The nonsignificant fluency effect for old grammatical items might be due to a lack of power when testing that effect for only a single item type (the mean power of single comparisons was .29; for details on power estimation, see Experiment 6). The absence of a fluency effect for ungrammatical items is a replication of the finding in Experiment 1 and thus requires some consideration. One tentative hypothesis to explain this result is based on the fact that whether ungrammatical items clarify quickly or slowly, they are processed less fluently than the other two item types because they comprise fewer familiar chunks. This might prevent participants from having an experience of surprising fluency even if items are presented at a fast speed of clarification.

We again compared bias in the two halves of the test. B1 was .078 in the first half and −.013 in the second half—thus, bias was actually slightly more liberal in the second half—but the difference between the two values was only marginally significant, t(31) = 1.97, p = .06. The false-alarm rate did not increase significantly from the first half (M = 0.316) to the second half (M = 0.357) of the test, t(31) = −1.45, p > .15, indicating that participants did not have difficulty rejecting ungrammatical items they had already seen during testing.

**Experiment 3**

In Experiment 3, we tested whether fluency also affects judgments in a recognition task. This experiment was identical to Experiment 2 except for the instructions given at test. Participants were told to decide for each test item whether it had been in the training list.

**Method**

**Participants**

Sixteen students (11 women and 5 men) from University College London took part in Experiment 3. They were from 19 to 34 years old and had a mean age of 23.6 years. They were recruited from the same population as participants in Experiment 2, were tested during the same period of time, and were paid the same amount of money (£5, approximately U.S.$8).

**Stimuli and Procedure**

The stimuli and procedure were the same as those in Experiment 2. As noted above, only the testing instructions were altered: Participants were now instructed to distinguish between old strings they had already seen during training and novel strings. They did this by pressing the Y key (to respond “yes, the string is old”) or the N key (to respond “no, the string is new”) on the keyboard. To avoid extreme response biases, we told the participants that 50% of the strings were old and 50% were new.

**Results and Discussion**

Figure 2D shows the proportions of “old” responses for the three types of items, computed separately for fast-clarifying and slow-clarifying trials. As in Experiments 1 and 2, participants discriminated between grammatical and ungrammatical items: Grammatical items (new and old) were called “old” more often than ungrammatical items were. Also, old grammatical items were endorsed more often than new grammatical items. Accordingly, an ANOVA including the factors clarification speed and item type revealed a significant main effect of item type, F(2, 30) = 155.31, MSE = 138.46, \( \eta^2_p = .91 \).

In this experiment, proportions of “old” responses for fast-clarifying items were almost identical to those for slow-clarifying items, F < 1. The Item Type × Clarification Speed interaction was
not statistically significant either, \( F < 1 \). Thus, there was no fluency effect in Experiment 3.

Once again, we tested whether participants adopted a stricter response criterion in the second half of the test to avoid endorsing new grammatical and ungrammatical items they had seen in the first half. \( B' \) was \(-.29\) in the first half and \(-.26\) (and thus slightly more conservative) in the second half, but the difference was not statistically significant, \( t(15) = -0.31, p > .75 \). The false-alarm rate increased significantly from the first half \( (M = 0.37) \) to the second half \( (M = 0.44) \) of the test, \( t(15) = -2.54, p < .05 \), indicating that in the second half of the test, participants called "old" some of the new grammatical and ungrammatical items they had seen in the first half of the test. However, the analysis of bias showed that participants did not try to avoid producing false alarms by adopting a stricter response criterion. Thus, the absence of a fluency effect cannot be attributed to a stricter response criterion in the second half of the test.

Combined Analysis of Experiments 2 and 3

Perceptual fluency was shown to have an impact on classification responses (Experiment 2) but to have no effect on recognition responses (Experiment 3). To find out whether this difference between the two patterns of results is statistically significant, we computed an ANOVA on the data of both experiments, with type of instructions as a between-subject variable. (As the participants from the two experiments came from the same population and were tested during the same period of time, a comparison across experiments is warranted.) The ANOVA revealed a significant main effect of item type, \( F(2, 92) = 247.01, MSE = 197.56, \eta^2_p = .84 \), indicating that endorsement rates differed significantly among the three classes of items. There was no significant Type of Instructions \( \times \) Item Type interaction, \( F(2, 92) = 2.39, MSE = 197.56, p = .10 \). Although there had been a significant main effect of clarification speed in Experiment 2, this effect was no longer significant when the data of Experiments 2 and 3 were pooled, \( F(1, 46) = 1.18, MSE = 70.16 \). However, there was a significant Type of Instructions \( \times \) Clarification Speed interaction, \( F(1, 46) = 4.34, MSE = 70.16, \eta^2_p = .09 \), indicating that the fluency effect was larger in Experiment 2 than in Experiment 3 (consistent with the significant fluency effect in Experiment 2 and the absence of a fluency effect in Experiment 3).

Strategic Use of Recollection and Fluency

The results of Experiment 3 are strikingly at variance with those of Experiments 1 and 2 in that there was no hint of a fluency effect. Whereas participants appear to use a fluency heuristic when making implicit (classification) decisions, an artificial manipulation of fluency has no detectable impact on explicit (recognition) decisions (we replicate these results in experiments reported below). As previewed in the introduction to this article, the results of Experiments 1–3 are consistent with the view that decisions about the general properties of a structured domain (e.g., classification) and about specific exemplars of that domain (e.g., recognition) may be differentially based on distinct memory processes. This account—developed by Brooks (1978), Jacoby (1991), Jacoby and Brooks (1984), Whittlesea, Brooks, and Westcott (1994), Yonelinas (2001), and others—proposes that a fluency-attribution heuristic is only one of a pair of strategies that participants can adopt for using past experience in the service of current decisions. This heuristic, which is precisely the one we have used to explain classification, is nonanalytic in the sense that participants are assumed to process a stimulus as a whole rather than analyzing it by its parts. If the whole item engenders a feeling of fluent or coherent processing, then an attribution process is recruited to find a suitable dimension in the environment (e.g., grammatical status) to which that fluent processing can be attributed. The other strategy, recollection, is analytic and involves examining a stimulus’s parts to see whether any of them acts as a cue for recalling details of the context in which the item was previously encountered. The assumption is that participants may shift the balance between these strategies and that in so doing, the influence of processing fluency on current decisions may change. Finally, we propose that the default strategy adopted by our participants under the present procedures is to rely mainly on fluency in the case of classification but on recollection in the case of recognition. In the remaining experiments reported in this article, we attempt to provide supporting evidence for this account and, in particular, we attempt to demonstrate that participants’ strategies may be shifted both in classification and in recognition.

What reason is there to believe that participants may shift their decision strategies in implicit and explicit memory tests? Whittlesea and Price (2001) provide one compelling example within the context of the mere-exposure effect. Participants saw a continuous stream of pictures, each presented for about 40 ms, and in a subsequent test stage were shown pairs of pictures, one being from the study set (i.e., old) and the other being novel. The mere-exposure effect refers to the fact that participants under such circumstances tend to pick the old item when asked to make a preference rating, and Whittlesea and Price replicated this result. They also found that when asked instead explicitly to select the old stimulus (i.e., when they received recognition instructions), participants were unable to do so, hence replicating the classic dissociation result of Kunst-Wilson and Zajonc (1980). Whittlesea and Price proposed that participants normally adopted a nonanalytic strategy under preference conditions but an analytic strategy under recognition conditions. In the former, participants would choose the item at test that evoked the greatest global feeling of goodness, and this would tend to be the true old item whose processing would be subtly more fluent than that of the novel item. In contrast, under recognition conditions, participants would study each test item, analytically attempting to find some discriminating detail diagnostic of a prior encounter with that item. As the study items were presented so briefly and the test items were so similar to one another, this analytic strategy was doomed to failure.

To test this account, Whittlesea and Price (2001) attempted to reverse the mere-exposure effect and, as a result, observed a tendency for old stimuli now to be selected under recognition but not preference conditions. They achieved this by implementing two changes. First, in the preference condition, participants were required to justify their decisions, which Whittlesea and Price assumed would evoke an analytic or recollective strategy. Second, in the recognition condition, participants were asked to select the stimulus that was globally most similar to a study item, a change of instructions that Whittlesea and Price assumed would encourage
nonanalytic processing (i.e., reliance on fluency). The results were consistent with these assumptions: Old stimuli were now selected with higher probability under recognition conditions than under preference conditions.

Verfaellie and Cermak (1999) have also provided evidence consistent with a change in strategy use in making memory decisions. Their study focused solely on explicit recognition judgments. Participants, who either had amnesia or were not memory impaired, made recognition memory judgments on words, but all the words were, in fact, new. Verfaellie and Cermak found that judgments were influenced by processing fluency: The probability of an “old” response was higher for both the participants with amnesia and the control participants for words that they perceived more rapidly in a perceptual clarification procedure, and this is consistent with the use of a nonanalytic fluency heuristic. In another experiment, however, the fluency effect disappeared in control participants but not in the participants with amnesia, and the change in procedure that elicited this contrasting pattern of results was minimal: Now, the test included old as well as new words. This suggests that for non-memory-impaired participants, the presence of old words is sufficient to cause a shift from reliance on fluency to reliance on recollection, presumably because participants find that they can recall occasional details of episodic context and this encourages more reliance on recall.

A series of experiments reported by Johnston, Hawley, and Elliott (1991), in which a perceptual clarification procedure similar to ours was used, provided results consistent with those of Verfaellie and Cermak (1999). In one experiment in which no old items were presented during test, the authors found a large effect of perceptual fluency on recognition decisions. This effect was considerably smaller in a further experiment in which old items were included in the test list. It almost vanished in a third experiment in which the training task involved a deeper level of processing, thus increasing test performance. Johnston et al. interpreted this result to be evidence for fluency being used as a cue for recognition decisions, particularly when there is little or no explicit memory (i.e., when the episodic context cannot be recalled).

Our strategy was similar to that of Whittlesea and Price (2001), in that we asked whether the fluency effect we had observed in classification could be made to disappear and whether a fluency effect in recognition could be made to appear. Our first step in this direction was to focus on recognition. An immediate implication of Verfaellie and Cermak’s (1999) and Johnston et al.’s (1991) results is that we might expect to obtain a fluency effect in recognition judgments if the old items that were included in the test phase of Experiment 3 are eliminated, thus encouraging greater reliance on fluency. This prediction served as the basis for our next experiment.

Experiment 4

In Experiment 4, we removed old grammatical (and new ungrammatical) items from the test list, thus encouraging nonanalytic processing. One group of participants was tested under classification instructions and the other was tested under recognition instructions.

Method

Participants

Twenty-five students (12 women and 13 men) from University College London took part in Experiment 4, receiving a financial compensation (£5, approximately U.S.$8) for participating. They were from 19 to 66 years old and had a mean age of 26 years. Fifteen participants were tested under recognition instructions and 10 under classification instructions.

Stimuli and Procedure

The stimuli and procedure were the same as in Experiments 2 and 3, with the exception that there were only new grammatical test stimuli. Classification instructions were adopted from Experiment 2 and recognition instructions were adopted from Experiment 3.

Results and Discussion

Figure 3 shows the endorsement rates on fast-clarifying and slow-clarifying trials in the two experimental groups. In both groups, a fluency effect was obtained in that fast-clarifying items were more likely to be endorsed than slow-clarifying ones, $F(1, 23) = 15.05, MSE = 81.38, \eta^2_p = .40$. There was no significant Clarification Speed × Type of Instructions interaction, $F < 1$, indicating that the fluency effect was of similar magnitude in the two groups. Furthermore, the within-subjects confidence intervals indicate that the fluency effect was significant in both groups. Participants performing classification were more likely to endorse test strings than were participants performing recognition, $F(1, 23) = 4.97, MSE = 198.45, \eta^2_p = .18$. This is similar to a result obtained by Kinder and Shanks (2001), who found that participants’ response bias was lower in classification than recognition: Participants are more willing to call an item “grammatical” than they are to call it “old.”

Participants in the classification group showed similar endorsement rates for new grammatical items in the first half ($M = 0.57$) and second half ($M = 0.58$) of the test; this difference was not statistically significant, $t(9) = -0.19, p > .85$. In the recognition group, the probability of endorsing new grammatical items in—
increased in the second half of the test (first half: $M = 0.44$; second half: $M = 0.53$), but this difference was not statistically significant either, $t(14) = -1.94, p = .07$.

Experiment 5

It appears that the presence of old items at test (Experiment 3) eliminates a fluency effect on recognition judgments that would otherwise be present (Experiment 4). The aim of the present experiment was to replicate the results of Experiment 3 and the recognition group of Experiment 4 in a single study, thus allowing us to directly compare the fluency effect that occurs when all three item types are included with the fluency effect that occurs when only new grammatical items are presented. All of the participants in this experiment were given recognition instructions. One group of participants was presented with all three types of test items, whereas the other group was presented only with new grammatical items.

Method

Participants

Forty students (30 women and 10 men) from University College London took part in Experiment 5. They received a financial compensation (£5, approximately U.S.$8) for participating. They were from 18 to 43 years old and had a mean age of 25.3 years. Twenty participants were tested on all three item types, and 20 were tested on only new grammatical items.

Stimuli and Procedure

The stimuli, procedure, and instructions were the same as in Experiment 3 for the participants tested on all three item types and the same as for the recognition group of Experiment 4 for the participants tested on only new grammatical items.

Results and Discussion

Figure 4 shows the proportions of “old” responses for fast- and slow-clarifying strings in the two groups. When only new grammatical items were presented, endorsement rates were about 9% higher for fast-clarifying than for slow-clarifying items, $t(19) = 2.72, \eta^2 = .28$, which is a replication of the results of Experiment 4. When all three item types were presented, in contrast, the proportions of “old” responses for fast-clarifying and slow-clarifying items were very similar to each other and, accordingly, did not differ significantly, $F(1, 19) = 2.17, MSE = 48.59$. Only with new ungrammatical items was the average endorsement rate slightly higher for fast-clarifying items than for slow-clarifying items. There was no significant Item Type \times Clarification Speed interaction, $F(2, 38) = 1.24, MSE = 92.33$.

It is particularly interesting to ask whether participants relied on fluency to a larger extent when they were presented only with new grammatical items than they did when receiving all three item types. A $2 \times 2$ ANOVA on the data from the new grammatical items including the factors group and clarification speed indeed revealed a significant Group \times Clarification Speed interaction, $F(1, 38) = 4.53, MSE = 103.49, \eta^2 = .11$. There was a significant fluency effect for new grammatical items when only these were presented, as already reported, but no effect for new grammatical items when all three item types were presented, $t(19) = -0.29, p > .77$. To summarize, in Experiment 5, we showed in a single study that when only new grammatical items are in the test list, perceptual fluency affects recognition judgments, whereas there is no fluency effect when all three item types are presented.

In this experiment, we again compared responding in the first and second half of the test. Participants who were presented with all three item types had a slightly more liberal bias in the second half ($B'' = -0.23$) than in the first half of the test ($B'' = -0.17$), but the difference was not statistically significant, $t(19) = 0.95, p > .35$. The false-alarm rate did not increase significantly from the first half ($M = 0.35$) to the second half ($M = 0.37$) of the test, $t(19) = -0.83, p > .42$. Participants who were presented only with new grammatical items called these items “old” slightly more often in the second half ($M = 0.47$) than in the first half ($M = 0.44$) of the test, but this difference was also not statistically significant, $t(19) = -0.77, p > .44$.

Figure 4. Mean endorsement rates (in percentages) for fast- and slow-clarifying items in Experiment 5 (recognition instructions). A: Only new grammatical items. B: All item types. The error bars indicate the 95% within-subjects confidence interval (computed according to the method of Loftus & Masson, 1994). new gram. = new grammatical strings; old gram. = old grammatical strings; new ungram. = new ungrammatical strings.
Experiment 6

In Experiment 6, we intended to replicate Experiment 5 (recognition instructions) under slightly altered conditions. One group (the NG group) of participants was tested as before on only new grammatical items. The other group (the OG + NG group) was tested on old grammatical and new grammatical items, omitting the new ungrammatical items. Our intention was to verify that the old grammatical items (and not the new ungrammatical items) are the ones that are crucial for determining which strategy is adopted by the participants.

Method

Participants

Forty-six students (40 women and 5 men; the gender of 1 participant was not recorded) from University College London took part in Experiment 6 to fulfill part of a course requirement. They were from 18 to 22 years old and had a mean age of 19.3 years. Twenty-two participants were in the NG group and 24 were in the OG + NG group.

Stimuli and Procedure

The test stimuli were the 16 old and 16 new grammatical strings from Experiments 1–3. Fast and slow clarification rates were the same as in Experiments 2–5. Instructions were identical to the ones in Experiment 3, the recognition group of Experiment 4, and Experiment 5; that is, participants received recognition instructions. The NG group was presented only with new grammatical items and the OG + NG group was presented with both old and new grammatical items. The whole set of test items (either old and new grammatical items or only new grammatical items) was presented twice. Items that clarified slowly in the first half of each part clarified quickly in the second half, and vice versa. In each group, one half of the participants received one test sequence, and the other half received a different one. Both test sequences contained the same items in the same order, but strings that clarified slowly in one sequence clarified quickly in the other, and vice versa. In all other respects, the procedure was identical to that of Experiment 5.

Results and Discussion

Figure 5 shows the proportions of “old” responses for fast- and slow-clarifying strings in both groups. In accord with our hypothesis and the results of Experiments 3–5, the clarification speed effect on new grammatical items was considerably larger in the NG group than in the OG group. The effect on new grammatical items was considerably larger in the NG group than in the OG group, t(23) = 2.67, one-tailed, p = .042. Further t tests revealed that in the OG group, the fluency effect was reliably different from 0, t(23) = 5.22, one-tailed, p < .005. In the NG group, t(23) = 1.52, one-tailed. Thus, including old grammatical items in the test phase is sufficient to induce a recollective mode in recognition.

To evaluate the null results in this experiment and in Experiments 3 and 5, it is useful to have estimates of the statistical power of the ANOVA to reveal a fluency effect if, contrary to our assumption, participants were relying on fluency information during recognition when old items were included in the test. The fluency factor in the ANOVA yields the same result as a t test comparing fast- and slow-clarifying strings when all three item types are collapsed. We computed the power of that t test, according to the suggestions of Cohen (1977), using GPOWER (Faul & Erdfelder, 1992). For estimating power in Experiment 3 and in the group presented with all three item types of Experiment 5, we used the difference between endorsement rates of fast- and slow-clarifying items in Experiment 2 as an estimate of the effect that we could expect if participants were relying on fluency information. The analysis revealed that the power of the t test (and thus the power of the fluency main effect in the ANOVA) was .53 in Experiment 3 and .67 in Experiment 5. To obtain an estimate of power in Experiment 6, we used only the old and new grammatical items of Experiment 2, because only these were presented to the NG + NG group. The analysis yielded a power value of .75. On the basis of the power values in Experiments 3, 5, and 6, we can compute the probability of not finding a true fluency effect in all three experiments (i.e., the combined beta error in these experiments). This probability is less than .05, and we therefore regard the evidence that people do not use a fluency strategy when old items are included in the recognition test as quite strong.

Once again, we investigated the effects of the repeated presentation of test items. Participants in the NG + OG group had a slightly more conservative bias in the second half (B” = –0.042) than in the first half (B” = –0.120) of the test, but the difference was not statistically significant, t(23) = –1.69, p > .10. The false-alarm rate did not increase significantly from the first half (M = 0.39) to the second half (M = 0.41) of the test, t(23) = –0.56, p > .58. Participants in the NG group called these items “old” equally often in the first and second halves of the test (M = 0.51).

Figure 5. Mean endorsement rates (in percentages) for fast- and slow-clarifying items in Experiment 6 (recognition instructions). A: Results of the NG group (tested on only new grammatical items). B: Results of the OG + NG group (tested on old and new grammatical items). The error bars indicate the 95% within-subjects confidence interval (computed according to the method of Loftus & Masson, 1994). new gram. = new grammatical strings; old gram. = old grammatical strings.
Experiment 7

Our aim in Experiment 7 was to investigate whether the use of fluency information during classification can be subject to the strategic control of the participants. Specifically, we asked whether participants are able to ignore fluency information in making classification decisions if they are instructed to do so. In Experiment 7, we told one group of participants that items would clarify at different speeds and that they should ignore this when making their judgments. A second group received standard instructions.

Researchers in previous studies that investigated whether reliance on fluency information can be eliminated used recognition or cued-recall tasks. Jacoby and Whitehouse (1989) performed a recognition test in which test items were preceded by a context item that either matched or did not match the test item. Presenting a matching context item was meant to enhance processing fluency of test items. Presentation duration was varied between experimental groups, such that in one group, participants were unaware of the presentation of the context item, whereas in the other group, they were aware of it. Jacoby and Whitehouse found that only participants in the unaware group showed a higher probability of endorsing test items that were preceded by the same item. Thus, being aware of the experimental manipulation enhancing processing fluency prevented participants from attributing the extra fluency to a source in the past. Similarly, Whittlesea et al. (1990) showed that informing participants about the fluency manipulation in a recognition task could eliminate its effect. Lindsay and Kelley (1996) used a cued-recall task in which they asked participants to complete easy and difficult word fragments with previously studied items. In accordance with the fluency account, they found that easy word fragments yielded higher endorsement rates than difficult ones. When informing participants of the fluency manipulation in a cued-recall task, the fluency effect disappeared in studied items but remained the same in new items. This result suggests that feelings of familiarity might be more difficult to avoid in cued-recall tasks than in recognition tasks.

Method

Participants

Thirty-two students (17 women and 15 men) from University College London took part in Experiment 7, receiving a financial compensation (£5, approximately U.S.$8) for participating. They were from 16 to 37 years old and had a mean age of 20.2 years. Sixteen participants were tested in each of the two groups.

Stimuli and Procedure

The stimuli and procedure were the same as in Experiment 4. That is, participants were tested only on new grammatical items.

Instructions

Participants in one group received standard classification instructions. Participants in the other group additionally read the following sentences:

As explained above, the following letter strings will clarify gradually. Please ignore the fact that some strings clarify more quickly than others (it is relevant for participants in a different experiment). Try not to let the clarification speed influence your yes/no decisions.

Results and Discussion

Figure 6 shows the endorsement rates for fast- and slow-clarifying strings under both standard and ignore instructions. Under standard classification instructions, the results of the other experiments were replicated, showing that fast-clarifying strings were judged grammatical more often than were slow-clarifying ones. Under ignore instructions, by contrast, no such difference occurred—there seemed to be even a small difference in the opposite direction. A $2 \times 2$ ANOVA including the factors type of instructions and clarification speed revealed no effect of clarification speed, $F < 1$, but a significant Type of Instructions $\times$ Clarification Speed interaction, $F(1, 30) = 5.40$, $MSE = 146.48$, $\eta^2_p = .15$. We computed $t$ tests that showed that there was a significant difference between fast- and slow-clarifying items under standard classification instructions, $t(15) = 2.78$, $\eta^2_p = .34$, but not under ignore instructions, $t(15) = -.93$. This result shows that participants who were instructed to ignore fluency indeed did not use this kind of information for classification. Thus, participants do not necessarily attribute fluency to the grammatical status of the items when making classification decisions.

There are two possible explanations of our finding. First, participants might have discounted the familiarity of the fast-clarifying test items as being produced by the speed of clarification rather than their presentation during training. However, in that case, we would expect endorsement rates for fast-clarifying items to be significantly lower than those for slow-clarifying ones (see Jacoby & Whitehouse, 1989, who obtained such a result). The second explanation, which is the one we prefer, is that participants abandoned reliance on fluency information and adopted a recollective mode in making their judgments. The present results do not allow us to decide between these two possibilities, because we found that endorsement rates for fast-clarifying strings were numerically but not significantly lower than the rates for slow-clarifying strings. However, both explanations are in accord with the idea that people are able to control fluency attribution in...
classification decisions, although according to the first explanation, this control is confined to the experimental manipulation of fluency.

There was no effect of repeating test items in the second half of the test. In the first half of the test, participants endorsed 54.9% of all new grammatical items and, in the second half, 56.4% (these numbers include the responses of both groups combined). The difference was not statistically significant, $t(31) = -0.71, p > .48$. To summarize, in none of the experiments, either under classification instructions or under recognition instructions, did the repeated presentation of test items make participants adopt a stricter criterion in the second half of the test to avoid falsely endorsing test items that they had seen in the first half. Only in one case (Experiment 3, with recognition instructions) did participants produce significantly more false alarms in the second half of the test, thus indicating that they erroneously called items “old” because they had seen them in the first half. We conclude that presenting test items twice did not generate large effects in our experiments; presumably, simply viewing a string once in the test was insufficient to affect memory and thus did not influence performance when that string reappeared later in the test. In the training stage, by contrast, participants had to type each item on the keyboard after it had been presented on the screen. Furthermore, the whole training list was presented four times during training, thus giving participants a much better chance to memorize items than they had during testing.

Experiment 8

This experiment was designed to exclude a possible alternative explanation of the fluency effect observed in our other experiments. As a result of participants responding more quickly to fast-clarifying items, the total presentation time (i.e., the time from presentation onset to response) was shorter for fast-clarifying than for slow-clarifying items. These shorter presentation times could be the true reason for fast-clarifying items being endorsed more frequently: Perhaps endorsement becomes less likely the longer the decision is delayed and has nothing to do with variations in fluency. In this experiment, we sought to isolate the possible influence of the presentation time by presenting half of the test items with a longer duration than the other half but without any clarification (cf. the control experiment by Luo, 1993). Each string appeared immediately and did not clarify gradually. If different presentation durations were the true reason for the fluency effect found in our experiments, endorsement rates should be higher when items are presented with a shorter duration.

Method

Participants

Sixteen students (7 women and 9 men) from University College London took part in Experiment 8, receiving a financial compensation (£5, approximately U.S.$8) for participating. They were from 19 to 50 years old and had a mean age of 21.4 years.

Stimuli and Procedure

The stimuli were the same as in Experiments 4 and 7, that is, participants were tested only on new grammatical items. Participants were instructed the same way as were those in Experiment 1 (classification instructions), except they were not told that the stimuli would appear gradually on the screen. They were also not informed about the different presentation durations. The short presentation time was 3,500 ms and the long one was 6,500 ms (these were the approximate response times in the classification group of Experiment 4). Participants could not respond until after the stimulus disappeared, as in previous experiments. Each item was presented twice, in the first half of the test stage at one duration and in the second half at the other duration. Trials with different presentation durations were administered in a random sequence.

Results and Discussion

The mean endorsement rate for short presentations was 57.81% ($SE = 2.45$), and the mean endorsement rate for long presentations was 55.86% ($SE = 1.93$), $t(15) = 1.16$. Thus, we can conclude that the fluency effect found in our experiments cannot be attributed to different presentation durations for fast- and slow-clarifying items.

General Discussion

The key idea in this article is that participants in AGL experiments may adopt different decision strategies that determine the precise way in which prior experiences combine to influence current performance. We proposed that by default, people adopt a strategy whereby processing fluency is used to make grammaticality decisions. We provided the first direct experimental demonstration of the fundamental role of processing fluency in such decisions. In Experiments 1–3, we found that an experimental manipulation of perceptual fluency had an impact on categorization decisions (Experiments 1 and 2) but did not affect recognition judgments (Experiment 3). Taken in isolation, this result seems to support the notion of distinct memory processes contributing to the two different types of decisions. Instead, we explored an account according to which fluency attribution is a strategy that participants can adopt in both categorization and recognition. We assumed that participants instructed to recognize strings would, by default, use an analytic strategy relying on recollection of the episodic context rather than a nonanalytic strategy relying on perceptual fluency. Further, we hypothesized that participants would switch to a nonanalytic strategy if the testing task was impossible to solve by means of an analytic strategy (i.e., recollection). We tested this account by giving recognition instructions and presenting new grammatical items but not old grammatical items (Experiments 4–6). The results supported our hypothesis: When no old strings were included in the testing set and thus an analytic strategy was doomed to fail, we found a fluency effect under recognition instructions. In Experiment 7, we investigated whether fluency attribution during implicit categorization is also subject to the strategic control of the participants. One group of participants was instructed that some strings would clarify more quickly than others and that they should ignore this fact. As hypothesized, those participants did not use fluency information in their classification decisions, whereas another group of participants that had received standard classification instructions did.

Our results corroborate the notion of analytic and nonanalytic processing being strategies that can be used in both implicit and explicit tasks and, in turn, lend weight to our proposal that implicit learning and implicit memory are different manifestations of the
same thing: prototypical implicit memory phenomena (e.g., effects of prior exposure on perceptual judgments; Whittlesea et al., 1990) that arise from the use of processing fluency as an attributional basis for judgments in just the same way that fluency influences grammaticality decisions in AGL. Moreover, as it is generally assumed that the attribution of processing fluency is a largely unconscious process, this fits with the ubiquitous finding that participants in AGL experiments have great difficulty verbally justifying their grammaticality decisions.

Much has been made of the finding that verbal reports are often impoverished (see Shanks & St. John, 1994) and tend not to accurately measure the full extent of a person’s knowledge. Yet interpretation of this finding has been a constant source of controversy. Reber (1989) has assumed that the gap between what participants know and what they can report in AGL experiments is an index of unconscious knowledge and has proposed that a powerful unconscious learning system exists. The present results suggest an alternative view, however (see also Tunney & Shanks, in press). We propose that people become sensitive to the familiarity of the bigrams, trigrams, and larger fragments constituting the study strings and then judge a test string to be grammatical if it is composed of familiar parts and hence is processed fluently. This account does not require the postulation of an unconscious learning system: It proposes that verbally describing the rules of the grammar is difficult because those rules are not learned in any sense (consciously or unconsciously). To put things another way, we are proposing the following causal chain: (a) The grammar specifies the bigram and trigram construction of study strings and hence determines which string fragments will benefit most from enhancements in fluency, then (b) variations in fluency determine the participants’ likelihood of endorsing different test items. The proximal explanation for grammaticality decisions is variation in processing fluency. In this account, there is no reason why participants should be able to report the distal reasons why some strings are processed more fluently than others are.

Although a good deal of evidence supports this account (see Johnstone & Shanks, 2001; Shanks, Johnstone, & Kinder, 2002), many authors have claimed instead that grammar learning involves the acquisition of implicit knowledge of the rules of a grammar (e.g., Marcus et al., 1999; Reber, 1989). Although such an account offers no obvious explanation for the influence of manipulations of processing fluency on grammaticality judgments, proponents of this view can refer to one particular piece of evidence in support of their position: Grammaticality judgments can be made at levels well above chance when test items are created using a vocabulary entirely different from that of the study items (e.g., Altmann, Dienes, & Goode, 1995; Manza & Reber, 1997). Transfer to a novel domain seems to require a rather abstract representation such as would be provided by a specification of the rules of the grammar. This claim has proved controversial for a number of reasons (Tunney & Altmann, 1999, 2001), and it would therefore be interesting to ask whether fluency affects performance in transfer just as it does with same-vocabulary test items. If a fluency effect was observed, this would be problematic for abstraction accounts. Such a result would suggest, at the very least, that the processes controlling classification decisions are similar in same-domain and in transfer (changed-vocabulary) conditions.

Last, what can we say about the precise mechanisms involved in the uses of recollection and fluency for implicit and explicit judgments? Are these entirely distinct processes functionally and neurally? Some researchers have argued that recollection and fluency are controlled by separate mechanisms. Consider evidence from studies on the use of “remember” and “know” responses. Tulving (1985) suggested that true recollective experience in a memory test can be indexed by autonoetic consciousness in which the episodic aspects of the study event are consciously reexperienced. Tulving also proposed that fluency or familiarity is associated with noetic consciousness (i.e., knowing), whereby the person knows that an item was studied but does not reexperience any specific information about the study event. Consistent with the separate-mechanisms view, numerous variables have been found to affect the proportions of “remember” and “know” responses independently (e.g., Rajaram, 1993). However, it has also been questioned whether the data require separate mechanisms. For example, signal detection models according to which there is a single underlying memory variable have been quite successful at reproducing the main findings from experiments using the remember–know procedure (Xu & Bellezza, 2001) by assuming that participants have a stricter response criterion for saying “remember” than for saying “know.” Admittedly, the qualitative differences we have observed between otherwise identical tasks would appear difficult to reconcile with a simple signal detection model.

Similarly, there is evidence that fluency and recollection are controlled by distinct brain systems and are differentially affected by brain lesions (Norman & O’Reilly, 2001), but evidence from AGL is somewhat unpersuasive on this issue: Kinder and Shanks (2001) argued that the performance of people with amnesia and normal individuals on matched explicit and implicit versions of AGL tasks could be understood without assuming separate neural systems. These important issues must await further research. What the current studies show is that a common set of principles can be useful in understanding both implicit memory and implicit learning.

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**Call for Nominations: Rehabilitation Psychology**

The APA Publications and Communications (P&C) Board has opened nominations for the editorship of *Rehabilitation Psychology* for the years 2006–2011. Bruce Caplan, PhD, is the incumbent editor.

Candidates should be members of APA and should be available to start receiving manuscripts in early 2005 to prepare for issues published in 2006. Please note that the P&C Board encourages participation by members of underrepresented groups in the publication process and would particularly welcome such nominees. Self-nominations are also encouraged.

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Gary R. VandenBos, PhD, and Mark Appelbaum, PhD, have been appointed as cochairs for this search.

To nominate candidates, prepare a statement of one page or less in support of each candidate. Address all nominations to

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The first review of nominations will begin December 8, 2003. The deadline for accepting nominations is December 15, 2003.