Distinctiveness and Repetition in Item Recognition

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Abstract

We scrutinized the hypothesis that the distinctiveness of unrepeated stimuli yields a surprising enhancement of their memorability, when contrasted with repeated stimuli. In three experiments, unrepeated stimuli were respectively rendered either (a) distinctive by intermixing them with repeated stimuli from a different category, or (b) nondistinctive, by having both unrepeated and repeated items originate in the same categories. The recognition-memory strength difference between unrepeated and repeated stimuli, in terms of a signal-detection measure of sensitivity, was less when the unrepeated stimuli were distinctive than when they were not distinctive. The results are considered with reference to contemporary theoretical interpretations.
Distinctiveness and Repetition in Item Recognition

The construct of memory strength is central to research concerning people's memory performance. In psychological investigations, stimulus strength has been operationalized in terms of frequency of presentation (Shiffrin, Huber, & Marinelli, 1995; Stretch & Wixted, 1998), duration of study (Bruno, Higham, & Perfect, 2009; Hockley & Caron, 2007), list length (Ohrt & Gronlund, 1999), short versus long interval before testing (Singer & Wixted, 2006), and other variables. The impact of these manipulations on different measures of memory facilitates the evaluation of competing memory theories.

Likewise pervasive in memory analysis is the concept of distinctiveness. Distinctive stimuli are those that are conspicuous with reference to their accompanying materials or their context of presentation by virtue of being unique, different, incongruent, or bizarre. The latter characteristics frequently result in superior memory performance for distinctive stimuli than for their nondistinctive counterparts. Intuition suggests that this results from the enhanced processing attracted by the distinctive experience, although such insights belie the complexity of these issues (Hunt, 2006). Regardless of the contributing mechanisms, distinctiveness is viewed as a construct central to the definition of memory strength (e.g., Dobbins & Kroll, 2005; Jacoby & Craik, 1979). However, this approach demands care to provide an explanation of enhanced performance rather than a description of observed effects (Hunt, 2006, p. 3).

This study pursued recent findings of Singer (2009a, 2009b) concerning the impact of presentation frequency on response criterion shifts. Those investigations used a paradigm involving people's study of one semantic category whose items were presented once, intermixed with another category whose items were repeated. They focused on the impact of memory strength (viz. repetition) on people's recognition-decision criterion (Brown, Lewis, & Monk, 1977; Hirshman, 1995). However, the results also suggested that people's memory for unrepeated stimuli, although inferior to their repeated counterparts, was surprisingly good. Prior studies have exhibited comparable trends, as quantified by the sensitivity measure $d'$ of signal detection theory.
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(Morrell, Gaitan, & Wixted, 2002); leading to questions about relatively robust memory for ostensibly weak stimuli (J. T. Wixted, personal communication, April 29, 2010).

The present experiments pursued the hypothesis that unrepeated items from one category (e.g., birds) are distinctive when intermixed with repeated items from another category (e.g., body parts). Distinctiveness in this arrangement would reflect the relative infrequency of encountering the bird names. The resulting prediction is that people's recognition of the unrepeated items will be better using the latter presentation scheme than in suitable control contexts. The design constitutes a variant of the classic isolation paradigm, in which an "isolate" appears in the context of background items from a different category (von Restorff, 1933).

**Distinctiveness and Isolation**

These assumptions warrant a brief consideration of contemporary analyses of the isolation paradigm. Von Restorff (1933) reported that when people encounter a single word (the isolate) that differs from all other list items on a dimension such as colour, recall of the isolate is far superior to that of the other stimuli. Hunt (2006) documented that von Restorff's data and analysis emphasized impaired memory for the background items rather than enhanced memory of the isolate. The popular contrasting analysis that the isolate, by virtue of its salience, receives enhanced processing (e.g., Jenkins & Postman, 1948) is not consistent with von Restorff's placement of the isolate at the beginning of the list; a position at which it would not be salient. Dunlosky, Hunt, and Clark (2000) presented multimeasure evidence bearing on this point. Their subjects made judgment-of-learning ratings for isolates that appeared in different portions of their lists. Relative to suitable controls, the ratings were elevated for isolates in the middle but not at the beginning of their lists. This indicated that people experience only later isolates as particularly salient. However, recall of the isolate did not vary with list position. That is, isolate status always benefitted the stimulus.

These findings converge with accumulating evidence that distinctiveness exerts its impact appreciably at retrieval. In this regard, the subjects of McDaniel, Dornburg, and Guynn (2005)
encountered common and bizarre sentences. In a counterbalanced design, the sentence types were either intermixed or segregated at study; and likewise intermixed or segregated in a recall test. The bizarre sentences were recalled better than common ones when the two were intermixed during test but not when testing was segregated. In contrast, the arrangement of the sentence types during study exerted no effect. This implicated retrieval mechanisms as underlying the impact of the distinctiveness of bizarre sentences. Also highlighting retrieval processes is the distinctiveness heuristic (Dodson & Schacter, 2002), an analysis that will be addressed in the Discussion.

Notwithstanding these retrieval mechanisms, it is well-documented that memory performance is enhanced by complementing study with (a) semantic or "deep" judgments (Gallo, Meadow, Johnson, & Foster, 2008; Hunt, 2003), (b) generation tasks (McCabe & Smith, 2006), and (c) supplementary stimuli such as pictures (Dodson & Schacter, 2002). All of these manipulations are proposed to increase the distinctiveness of the learned stimuli.

Hunt (2006) framed a reconciliation between the encoding and retrieval facets of distinctiveness with emphasis on the isolation paradigm. Two principles were particularly central to his analysis. First, he proposed that exceptional recall of the isolate reflects encoding of stimulus differences in a context of similarity--that is, the subject detects the difference between the isolate and the common feature of all background items. In this regard, in a homogeneous control list in which the isolate appears among members of its own category, only encoding of stimulus similarity (i.e., membership in the uniform category) is afforded. In a heterogeneous control list in which the isolate appears among random words, in contrast, there is simply no readily-available dimension of similarity against which to process differences.

Hunt's (2006) analysis received support from Hunt and Lamb's (2001) evidence that encouraging the subjects to focus on the differences among the background items abolishes the memory advantage of the isolate. Hunt and Lamb reasoned that the background items routinely foster similarity processes. Therefore, a difference-processing orientation enhances the encoding
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The second central principle of Hunt's (2006) analysis was that the isolation effect requires that the context in which differences have putatively been encoded be reinstated at test. In one relevant study (Hunt & Smith, 1996), people encountered lists of five words from common categories. They were alternatively instructed to identify one similarity or one difference between the first item of a list and the remaining four items. The difference instruction was viewed as providing an encoding advantage, because lists of five cocategory members spontaneously afford similarity processing. In subsequent memory testing for the first items of the lists, the subjects' own difference responses from study cued recall better than did their similarity responses. In addition, the subject's own difference cue produced better recall than did another person's difference cue. Thus, recall was best when the cue reinstated the context of the subject's own encoding of difference.

Summary

These issues are relevant here because of similarities between the paradigm of this study and the isolation paradigm. As discussed earlier, the present paradigm involves a presentation of multiple items from one category in the context of repeated presentations of items from another category. The net impact, from the subject's perspective, is the infrequent appearance of items from the unrepeated category. Although this is appreciably different from the isolation paradigm arrangement of just a single bird name appearing among numerous (unrepeated) body part names, our intuition and suggestive evidence (Morrell et al., 2002; Singer, 2009b) predicted surprisingly strong memory for the unrepeated items. Three interrelated experiments pursued this possibility.

Experiments 1a, 1b, and 1c

Experiment 1 evaluated the hypothesis that unrepeated items are surprisingly memorable compared with repeated ones and that this is because the former are distinctive in the context of their lists. To achieve this goal, it was necessary to compare, in some manner, distinctive versus
nondistinctive unrepeated items. Here, manipulations were designed to render unrepeated items as distinctive in Experiment 1a but not in Experiments 1b and 1c. In Experiment 1a, unrepeated items were distinctive by virtue of stemming from a different category than that of the repeated items. Unrepeated items in Experiment 1b were not distinctive because they were drawn from the same category as the repeated items. For reasons that will be elaborated later, Experiment 1c presented pairs of categories both of which bore repeated and unrepeated items, again rendering the unrepeated ones not distinctive.

Method

Subjects. The subjects were native-English speaking male and female students recruited from the introductory psychology subject pool at the University of Manitoba. Students participated for partial credit toward a course requirement. There were 88, 62, and 48 different participants in Experiments 1a, 1b, and 1c, respectively.

Materials. In all three experiments, the stimuli were derived from sets of 40 words from each of the following categories: American states, birds, body parts, chemical elements, cities, countries, diseases, male names, mammals, occupations. The geographical locations consisted of the 40 most populous locations according to internet population lists in 2002. Words for the remaining categories represented the 40 highest-frequency items from the production norms of Battig and Montague (1969). However, the population and category lists excluded multiple-word (e.g., blue jay) and category-naming (e.g., blueBIRD) stimuli. Population size and production frequency will jointly will be referred to as "normative," throughout. Names of people and locations were capitalized. The stimuli appeared in the Appendix of Singer and Wixted (2006).

Experiment 1a. The materials comprised four counterbalanced lists. To construct the study materials of list 1, the ten categories were randomly assigned to five pairs which in turn were assigned random positions in the list. Within each pair, the categories were randomly assigned to the repeated condition (three presentations) or the unrepeated condition (one presentation). Finally, from each of the 20 pairs of items within each category, ranging from the highest to the
lowest normative ranking, one item was randomly designated as a target and the other as a lure.

However, only 10 of the 20 available repeated-category targets were randomly chosen to appear in the study list. Likewise, only 10 unrepeated-category targets were retained for study, and those were the ones that corresponded in normative ranking to the excluded repeated items. Each resulting study block comprised 10 repeated targets, presented three times each; and 10 unrepeated targets. Thus, each of five study blocks presented 40 stimuli, for a total of 200 stimuli across the five pairs.

For recognition testing, five blocks of probes preserved the category pairing and order of the study blocks. Each block presented the 10 targets and 10 corresponding lures of each of the two categories. This yielded a total of 40 probes per block and 200 for the experiment.

The materials also included the practice category, "colour." It comprised six repeated items (3 repetitions), six unrepeated items (1 repetition), and 24 test probes (12 targets, 12 lures).

Lists 2 to 4 served the purpose of counterbalancing. Across those lists, the items of list 1 were cycled across the conditions defined by crossing (a) repetition and (b) target-lure.

**Experiment 1b.** The materials of Experiment 1b were four counterbalanced lists derived from those of Experiment 1a. For example, one study block of list 1 in Experiment 1a presented repeated cities and unrepeated birds. In Experiment 1b, those unrepeated bird names were replaced by the 10 city targets that were randomly excluded in Experiment 1a. As a result, one study block in Experiment 1b presented only city targets: 10 repeated and 10 unrepeated. In the corresponding recognition block for cities, the subject encountered 10 repeated targets, 10 unrepeated targets, and 20 lures. Lists 2 to 4 were constructed in a similar manner.

There was a practice category of six repeated and six unrepeated colour-name targets, and a corresponding recognition block of 12 targets and 12 lures. In all other respects, including the total number of studied and tested items, the materials of Experiment 1b were identical to those of Experiment 1a.

**Experiment 1c.** The materials again comprised four counterbalanced lists. Following the
principles of Experiment 1a, the categories were randomly assigned to five pairs, half the items per category were designated as targets, and the pairs were assigned a list position. Like in Experiment 1a, 10 of the available targets of each category were presented for study. Unlike Experiment 1a, however, each category bore both repeated and unrepeated stimuli: specifically, five of each.

For the recognition blocks, the crucial innovation was to randomly select one of the study categories of each pair to have only its repeated items tested \( (n = 5) \) and the other category to have only its unrepeated items tested \( (n = 5) \). Lures in equal numbers came from the same ordered pairs as the targets. This yielded a total of 20 probes per recognition block. In contrast with Experiment 1a, the unrepeated targets should not have appeared to be distinctive, because repeated targets from their category were presented during study.

Lists 2 to 4 were constructed by cycling the items across the four conditions defined by crossing (a) repetition and (b) which category was to have its repeated items tested. The practice materials were identical to those of the other experiments.

**Procedure.** The sessions were conducted with groups of one to four subjects who were tested in spacious cubicles in a large room. Each station included a personal computer, keyboard, and monitor. The "." and "x" keys respectively served as Yes and No buttons. All experimental events were regulated by the MEL experimental software package.

The procedure was the same in each experiment. Written instructions previewed the structure of the experiment for the subject. During study, each block was preceded by messages identifying the upcoming categories, reminding subjects to remember each item as well as possible, and encouraging them to rest if so desired. Study was initiated with a press of the space bar, which was following by a 1.5-s interval. Each target was displayed for 1100 ms (cf. Singer, 2009b, Experiment 3) and was followed by a 250-ms interstimulus interval (ISI). The stimulus words were left-adjusted to row 10, column 1 of the screen and appeared in random order. Study blocks ended with a 3-s interval.
After study, a screen message directed the subject to work for 5 minutes on an arithmetic puzzle printed on a yellow sheet which was placed face-down by the computer. The end of this intervening task was signalled by a tone.

Recognition testing proceeded in blocks. Each block presented randomly intermixed test probes from single categories (Experiment 1b) or pairs of categories representing the earlier study blocks. The recognition blocks also appeared in same order as during study. Each block began with messages identifying the upcoming category(s) and instructing the subjects to place their index fingers on the Yes and No keys. The subject pressed the space bar to initiate the block. On each trial, a fixation "X" appeared for 500 ms at row 10, column 1 of the screen. Then, the probe was left-adjusted to that position and remained visible until a response was registered. There was a 500-ms interval after response. Each recognition block presented its 20 probes in random order and was followed by a 3-s interval. At the end of testing, a screen message thanked the subject for participating.

Study and testing were each preceded by the one-category block of colour words that provided the subjects with practice at the tasks.

Results

The raw data comprised the frequency of "yes" replies for the conditions of the different experiments. "Yes" replies are hits for targets and false alarms for lures. For consistency with the antecedent studies of this project (e.g., Singer & Wixted, 2006), we evaluated recognition performance as the signal-detection strength $d'$, derived from the raw data. Likewise, we quantify the decision criterion (here labelled $z_c$) as its position on the strength-of-evidence continuum (see Dobbins & Kroll, 2005; Singer & Wixted, 2006, p. 128; Treisman & Williams, 1984; cf. Hockley & Caron, 2007 and Morrell et al., 2002, who simply reported the false alarm rates). The choice of criterion measures helps to expose certain between-list criterion shifts that complement the main $d'$ patterns.

The experiments were designed to demonstrate that recognition performance is superior for
distinctive than nondistinctive unrepeated items. More specifically, Experiments 1b and 1c were designed as different controls for Experiment 1a. The unrepeated study items of Experiment 1b were nondistinctive because they originated in the same category as the repeated items, in contrast to different categories in Experiment 1a. Note, however, that there was only one hypothetical lure distribution in the recognition testing of Experiment 1b (e.g., cities) in contrast with the two of Experiment 1a (e.g., cities and birds). That confound, though arguably minor, was addressed by Experiment 1c, in which the unrepeated targets were rendered nondistinctive in a different fashion but which offered two hypothetical lure distributions in testing.

By virtue of this design, the main focus was on analysis of variance (ANOVA) which was first applied separately to the $d'$ values of the three experiments. Then, two additional ANOVAs respectively combined Experiment 1a with the other experiments. These composite ANOVAs were predicted to reveal significant Repetition × Experiment interactions. Such interactions would diagnose a smaller $d'$ difference between the repeated and unrepeated conditions in Experiment 1a (distinctive unrepeated items) than in the other experiments. It is noteworthy that the main hypothesis cannot be evaluated by a direct comparison of the $d'$ values of the unrepeated conditions of the different experiments, because they stem from study and test conditions that are not comparable.

Experiments 1a, 1b, and 1c. The raw data and signal-detection statistics, shown in Table 1, were analyzed using ANOVA. In all cases, repetition was a within-subjects variable and list was a between-subjects variable. As a counterbalancing variable, list held no theoretical interest, so its effects are not reported. The results for hits and false alarms will be reported only if they are incongruent with the $d'$ and $z$, results, respectively. A significance level of $\alpha = .05$ is used unless otherwise indicated.

Insert Table 1 about here

The data of two subjects of Experiment 1a and one subject of Experiment 1c who did not discriminate targets from lures were excluded. ANOVA applied to the $d'$ scores of the remaining
subjects yielded significant effects of repetition in each of Experiment 1a, $F(1, 84) = 102.12$, $MSE = 0.12$; Experiment 1b, $F(1, 58) = 297.89$, $MSE = 0.06$; and Experiment 1c, $F(1, 43) = 54.41$, $MSE = 0.23$.

Table 1 shows $z$ scores and the false alarm rates from which they were derived only for Experiment 1a. ANOVA of the $z$ values revealed that the effect of repetition was not significant, $F < 1$. In Experiment 1b, all lures originated from the same category and so did not distinguish the repeated and unrepeated conditions. The mean false alarm rate in Experiment 1b was .203 ($S.E. = 0.01$). In Experiment 1c, the lures did stem from categories that had either their repeated or unrepeated targets tested. However, because all categories presented both repeated and unrepeated stimuli during study, there was no reason to expect an impact of type of target that was tested. Indeed, the false alarm rate for categories with unrepeated tested-targets was 0.175 ($S.E. = .02$) versus 0.165 ($S.E. = .02$) for categories with repeated tested-targets, $F < 1$.

**Comparison of the experiments.** It was explained earlier that the advantage of distinctive versus nondistinctive unrepeated items would be reflected by Repetition × Experiment interactions in the comparison of the $d'$ scores of Experiment 1a with those of the other experiments. In each of these analyses, repetition was a within-subjects variable and experiment and list were between-subjects variables. In the comparison of Experiments 1a and 1b, the Repetition × Experiment interaction was significant, $F(1, 142) = 9.78$, $MSE = 0.10$. This reflected a smaller $d'$ difference between the repeated and unrepeated conditions when the unrepeated items were distinctive (Experiment 1a) than when they were not distinctive (Experiment 1b). There was also a main effect of repetition, $F(1, 142) = 312.61$, $MSE = 0.10$. The Experiment main effect was not significant.

Likewise, comparison of Experiments 1a and 1c yielded a significant Repetition × Experiment interaction, $F(1, 126) = 3.95$, $MSE = 0.16$. The repetition main effect was significant, $F(1, 126) = 153.01$, $MSE = 0.16$. It should be noted that, as reported earlier, the repetition effect was significant in all three experiments. What the Repetition × Experiment interactions reveal is
that the *magnitudes* of the repetition effect differed among the experiments. Finally, there was a significant main effect of Experiment, $F(1, 126) = 7.75, MSE = 0.79$, reflecting mean $d'$ scores of 1.64 and 1.96 in Experiments 1a and 1c, respectively.

**Discussion**

Prior evidence and experimenter intuition have suggested that unrepeated study items from one category are surprisingly memorable when intermixed with repeated items from a different category (Morrell et al., 2002; Singer, 2009b; J. T. Wixted, personal communication, April 29, 2010). Here, it was postulated that such a phenomenon might reflect the distinctiveness of the unrepeated items. To evaluate the hypothesis, Experiments 1b and 1c were designed to control, in different ways, the proposed distinctiveness of the unrepeated items of Experiment 1a. In Experiment 1b, subjects studied repeated and unrepeated items from the same category, so the unrepeated items were not distinctive. In Experiment 1c, the subjects studied category pairs like in Experiment 1a, but the unrepeated items were not distinctive because both categories presented repeated and unrepeated items. The Experiment 1c recognition test for each category pair, furthermore, presented only the repeated items of one category and the unrepeated items of the other category. As a result, the recognition blocks of Experiment 1c had a structure identical to those of Experiment 1a.

The statistical comparisons of the experiment pairs clearly supported the main hypothesis. In both cases, the Repetition $\times$ Experiment interaction was significant, reflecting a smaller $d'$ difference between the unrepeated and repeated conditions when the unrepeated condition was distinctive (Experiment 1a) than when it was nondistinctive (Experiments 1b and 1c).

As noted in the Results, the present hypotheses were evaluated in terms of the Repetition $\times$ Experiment interactions rather than by direct comparisons among the unrepeated-condition $d'$ values of the different experiments. This is because the unique set of study and test conditions associated with the three experiments likely impacted overall representational strength, as reflected by the Experiment main effect in the comparison of the $d'$s of Experiments 1a and 1c.
Such effects would render comparisons between corresponding $d'$ values between the experiments to be questionable in importance. We speculate that the latter Experiment main effect stemmed in part from the presentation in Experiment 1c of only 20 probes per recognition block, versus 40 in Experiment 1a.

Only in Experiment 1a did the lures distinguish the repeated from the unrepeated condition. The similar rate of false alarms in those conditions, and the corresponding $z_c$ values, indicate that the subjects applied similar decision criteria to repeated and unrepeated probes. This is consistent with prior findings that stimulus strength does promote a criterion shift under conditions of rote study (Morrell et al., 2002; Singer, 2009a; Stretch & Wixted, 1998; Verde & Rotello, 2007).

**Theoretical analyses of these effects.** As considered in the introduction, the theoretical domain of distinctiveness is somewhat perilous. Analyses emphasizing the enhanced processing of distinctive stimuli as a result of their salience are intuitively appealing but have been appreciably qualified. It is also necessary to avoid the circularity of simply defining memory strength and distinctiveness in terms of one another (Hunt, 2006). Rather, as discussed earlier, one analysis that seems applicable to the present findings is Hunt's proposal that the processing of differences in a context of similarity enhances memory performance. One advance of the present study concerns the configuration of stimuli to which Hunt's analysis might apply. In von Restorff's (1933) isolation paradigm, only a single stimulus represented a unique feature, and each background item appeared just once. In Experiments 1a-1c, in contrast, the minority category was represented by numerous exemplars and each background item was repeated. In spite of these differences from the original paradigm, the present results were consistent with the proposal that the minority category items were distinctive.

A competing treatment of these effects might stem from the distinctiveness heuristic analysis (Dodson & Schacter, 2002; Schacter, Israel, & Racine, 1999). It holds that people metacognitively adopt more stringent retrieval criteria for memory test-probes for which they
expect to retrieve vivid, distinctive details than for less distinctive probes. The distinctiveness heuristic is predicted to modulate recall and recognition and more generally to reduce false alarms for the class of probes perceived to be distinctive. This hypothesis has been supported by reduced false alarms to stimulus categories made distinctive by (a) complementing stimulus words with pictures (Dodson & Schacter, 2002) and (b) equipping subjects with a deep rather than a shallow orienting task during study (Gallo et al., 2008). McCabe and Smith (2006) documented comparable outcomes in the DRM critical-lure paradigm. In that paradigm, people frequently incorrectly remember a critical lure (e.g., *sleep*) with reference to a study list of words associated with the critical lure (e.g., *bed, rest, wake, etc.*; Deese, 1959; Roediger & McDermott, 1995). McCabe and Smith documented reduced false memories in recall and recognition when subjects had to generate their study words using a decoding procedure, a manipulation posited to enhance distinctiveness; compared to when they simply read the study words.

However, the present data did not exhibit the depressed false alarms central to the distinctiveness heuristic. To the contrary, false alarms were strikingly similar in the unrepeated versus repeated conditions of Experiment 1a (see Table 1), the only experiment in which the lures were inherently associated with a repeated or an unrepeated category. The processing of differences in a context of similarity (Hunt, 2006) therefore offers a more convincing account of these findings than does the distinctiveness heuristic.

A major corollary of these findings is that distinctiveness partly overcomes infrequency or lack of repetition. It was discussed at the outset that repetition is one of the most studied and robust of factors affecting memory strength (e.g., Shiffrin et al., 1995; Stretch & Wixted, 1998). It is intriguing, therefore, that the distinctiveness a stimulus may be inversely related to its degree of repetition. As a result, distinctiveness might overcome or oppose repetition as a memory variable.

In conclusion, we explored the possibility that the relative weakness of unrepeated stimuli may be partly offset by distinctiveness that accrues from the very infrequency of the unrepeated
class. Experiments 1a-1c demonstrated a smaller representational-strength difference between
unrepeated and repeated stimuli when the former are rendered distinctive than when they are not.
This outcome constitutes important progress toward characterizing the distinctiveness of
unrepeated *stimuli*. 
References


Footnotes

1. The latter choices were dictated by the need to equate the number of stimuli with those of Experiment 1b. In Experiment 1b, both repeated and unrepeated items came from the same category. Given the total of 40 stimuli per category and the need for 20 foils, there could be only 10 repeated targets and 10 unrepeated targets. Furthermore, the unrepeated targets of Experiment 1b had to be different in their ordinal ranking from the repeated ones, so the corresponding items were selected in Experiment 1a.
Table 1

Experiment 1 Results as a Function of Repetition (Standard Errors in Parentheses)

<table>
<thead>
<tr>
<th>Experiment</th>
<th>Repetition</th>
<th>Hits</th>
<th>False Alarms</th>
<th>$d'$</th>
<th>$z_c$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Exp. 1a (two categories, distinctive)</td>
<td>Repeated</td>
<td>.823 (.01)</td>
<td>.249 (.02)</td>
<td>1.91 (.08)</td>
<td>0.83 (.05)</td>
</tr>
<tr>
<td></td>
<td>Unrepeated</td>
<td>.682 (.02)</td>
<td>.241 (.02)</td>
<td>1.38 (.06)</td>
<td>0.84 (.05)</td>
</tr>
<tr>
<td>Exp. 1b (one category, nondistinctive)</td>
<td>Repeated</td>
<td>.845 (.01)</td>
<td></td>
<td>2.04 (.08)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Unrepeated</td>
<td>.637 (.02)</td>
<td></td>
<td>1.28 (.06)</td>
<td></td>
</tr>
<tr>
<td>Exp. 1c (two categories, nondistinctive)</td>
<td>Repeated</td>
<td>.855 (.01)</td>
<td></td>
<td>2.30 (.12)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Unrepeated</td>
<td>.667 (.02)</td>
<td></td>
<td>1.57 (.10)</td>
<td></td>
</tr>
</tbody>
</table>

Note: The false alarm rate and $z_c$ scores did not distinguish the repeated and unrepeated conditions in Experiments 1b and 1c (see text).