

Repetition Blindness and Illusory Conjunctions: Errors in Binding Visual Types With Visual Tokens

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Repetition blindness (Kanwisher, 1986, 1987) has been defined as the failure to detect or recall repetitions of words presented in rapid serial visual presentation (RSVP). The experiments presented here suggest that repetition blindness (RB) is a more general visual phenomenon, and examine its relationship to feature integration theory (Treisman & Gelade, 1980). Experiment 1 shows RB for letters distributed through space, time, or both. Experiment 2 demonstrates RB for repeated colors in RSVP lists. In Experiments 3 and 4, RB was found for repeated letters and colors in spatial arrays. Experiment 5 provides evidence that the mental representations of discrete objects (called "visual tokens" here) that are necessary to detect visual repetitions (Kanwisher, 1987) are the same as the "object files" (Kahneman & Treisman, 1984) in which visual features are conjoined. In Experiment 6, repetition blindness for the second occurrence of a repeated letter resulted only when the first occurrence was attended to. The overall results suggest that a general dissociation between types and tokens in visual information processing can account for both repetition blindness and illusory conjunctions.

The fundamental problem of vision is generally taken to be *recognition*, that is, how we identify things by looking at them. Recognition can be described as a process that takes the raw visual image as input and delivers as output a list of the categories of objects present in the scene. But there must be more to vision than recognition, so defined. After all, when we look at the world we perceive not an unstructured soup of visual categories, but rather a spatiotemporally organized array in which those categories are associated with particular objects and events.

The visual system must therefore have a way of linking recognized visual categories (which will be called "types") with distinct, spatiotemporally defined visual objects ("tokens"). I will argue here that not only is the recognition of visual types *logically* separable from the individuation of visual tokens, but that the two are in fact functionally dissociable processes in human vision. To make this case, I will marshal evidence from two seemingly unrelated visual phenomena—repetition blindness (Kanwisher, 1987) and illusory conjunctions (Treisman & Schmidt, 1982). I will argue that the same type-token distinction is fundamental to both phenomena.

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Repetition Blindness

Repetition blindness (Kanwisher, 1987; Kanwisher & Potter, 1989, 1990) refers to the fact that subjects have difficulty detecting repetitions of words presented in rapid serial visual presentation (RSVP). The phenomenon occurs at rapid presentation rates even when the two occurrences of the repeated word are separated by several intervening words, and even when the appearance of the two occurrences differs (upper vs. lower case). Repetition blindness (RB) has been demonstrated most clearly in a recall task for RSVP sentences; subjects selectively omit second occurrences of repeated words. For example, in response to the sentence "It was work time so work had to get done," most subjects said something like "It was work time so had to get done." Thus, even sentence grammar and meaning are insufficient to save a repeated word from oblivion.

Kanwisher and Potter (1989) argued that repetition blindness is likely to be a specifically visual phenomenon, because there is no "repetition deafness" for repeated words in rapid, auditorily presented sentences (using compressed speech). They showed further that RB cannot simply be explained by the lack of spatial cues differentiating the two occurrences: The effect is undiminished when the RSVP stream progresses rightwards across the display so that each word appears in a different spatial location.

Repetition blindness has been interpreted (Kanwisher, 1987) in terms of the distinction between type recognition and token individuation. In recognition, a word is identified as a type (e.g., the word *work*). In individuation, a word is characterized as a particular token of a given type (e.g., as the second instance of the word *work*). Repetition blindness arises because even though the second occurrence of a repeated word is recognized as a type (e.g., its lexical entry becomes activated the second time around), it is not individuated as a distinct event, or token. Instead, the second occurrence (called

"C2")¹ becomes assimilated to the first ("C1"), and only one token of the word type is registered consciously.

Although there is now a good deal of evidence for this characterization of repetition blindness, a deep theoretical account of why the phenomenon occurs is not yet available. One possibility is that repetition blindness may function as a visual heuristic that prevents a single object from being tallied more than once (whenever intermittences in the stimulus cause the same stimulus object to be recognized several times). Another possibility (Kanwisher, 1990) is that RB reflects a processing limitation in the visual system's solution to the "binding problem" (the problem of how, given a display with more than one object, the visual features of a single object are bound together).

An alternative characterization of repetition blindness has already been ruled out. Repetition blindness cannot be explained as the result of a recognition node for a given word becoming refractory after registering the first occurrence of that word. This hypothesis can be rejected because threshold recognition of the last word in an RSVP list is helped, not hindered, by a prior occurrence of that word earlier in the same list (Kanwisher, 1987, Experiment 3). In this task, subjects did not have to establish token representations for the preceding words in each list, so the target word did not constitute a "second token," and was therefore not subject to repetition blindness. (Indeed, although subjects showed a significant benefit from such repetitions, when asked about them after the experiment all subjects denied having noticed them.) Thus, recognition (type activation) does not seem to be impaired by repetition. Instead, repetition blindness happens when subjects recognize both occurrences but do not encode them as two distinct events.

Illusory Conjunctions

Illusory conjunctions (Treisman & Schmidt, 1982) are incorrect recombinations of visual features that result when attention is diverted. For example, after viewing a briefly presented array containing a pink *O* and a green *T*, subjects sometimes confidently report having seen a pink *T*. Illusory conjunctions have traditionally been explained in the context of feature integration theory (Treisman & Gelade, 1980). According to this theory, primitive visual features (e.g., "red") can be detected in parallel over the whole visual field, but conjunctions of features (e.g., both "red" and "X-shaped") can only be correctly detected after they are conjoined through a serial act of attention directed to the location of the target item. When attention is diverted, illusory conjunctions of features may result. Though feature integration theory has recently come under some criticism (e.g., Egeth, Virzi, & Garbart, 1984; Johnston & Pashler, 1989; Wolfe, Cave, & Franzel, 1989), the core insight remains: features and their conjunctions are processed in fundamentally different ways.

Feature integration theory can also be cast in terms of types and tokens (Kahneman & Treisman, 1984; Kanwisher, 1987). Searching for a single feature in an array can be thought of as a pure type task, because activation of the feature type is

sufficient to verify the existence of a target and array tokens need not be individuated at all. However, when searching for a target defined by a conjunction of features such as "red *X*," it is insufficient to simply verify that the component features are present. After all, both "red" and "*X*" are present even when the array contains only red *O*s and green *X*s, but no red *X*. Rather, what is needed in conjunction search is precisely the information that the redness and the *X*-ness are associated with the same "object file" (Kahneman & Treisman, 1984), or token (Kanwisher, 1987).² Thus, although feature search can be done on the basis of (feature) type activations alone, conjunction search requires knowledge about how feature types are linked to particular object tokens.

To sum up, both repetition blindness and illusory conjunctions have been explained in terms of the type-token distinction. Repetition blindness can be thought of as a failure to link one type to two tokens, whereas an illusory conjunction can be thought of as an error in linking one token to two types. In the present study, I explore whether this parallel actually reflects shared underlying processing mechanisms. I address this question by investigating (a) whether repetition blindness and illusory conjunctions occur for the same kinds of visual stimuli, and (b) whether the tokens that are necessary to detect repetitions are the same mental entities as the tokens necessary to conjoin visual features.

It should be acknowledged, however, that there is some tension in what counts as a visual "type" in the two cases. In past work on repetition blindness, the relevant "types" have generally been words, whereas in the work on illusory conjunctions, the "types" have most often been primitive visual features. Despite the obvious differences between primitive visual features and complex visual types like words, there is some justification for combining the two categories into one inclusive concept of "types." In many situations, all of these visual types behave similarly. For example, illusory conjunctions have been reported not only in spatial arrays for combinations of primitive features, but also in temporal lists for combinations of word identity and letter case (Lawrence, 1971), letter identity and color (McLean, Broadbent, & Broadbent, 1982), and photographs of objects and the frames surrounding them (Intraub, 1985, 1989). This study will explore whether RB generalizes in the opposite direction, from temporal word lists to spatial arrays of primitive features. The first experiment investigates whether repetition blindness occurs for items distributed across space as well as time.

¹ Throughout this article, C1 and C2 will refer to the first and second critical items, whether they are the same, as in the repeated condition, or different, as in the control unrepeated condition. C1 and C2 have the same meaning as R1 and R2 (respectively) in my earlier articles. The terminology has been changed because the "R" implied repetition, but was in fact used for both repeated and unrepeated conditions.

² Of course, if "red *X*" itself was a preexisting visual type, one might be able to encode a red *X* by simply activating that type, but the evidence indicates that such conjunctive visual types cannot be directly accessed in visual search. This issue is discussed in more detail in the General Discussion section.

Experiment 1

Most of the earlier research on repetition blindness has used the standard RSVP technique, in which words are displayed sequentially in the same place on a computer screen. One account of repetition blindness, then, might be that the visual system assumes that any repetitions of the same thing appearing in the same place result from the same single object. A great deal of evidence points to spatial location as the main "tag" used to "index" or keep track of items (e.g., see Ullman, 1984). Thus, if location tags are the only way to individuate like items, such individuation would fail in normal RSVP.

Evidence against this account of repetition blindness was presented by Kanwisher and Potter (1989), who showed that repetition blindness is undiminished when each successive word in the RSVP stream is staggered two character spaces to the right of the previous word. However, one could argue that in this kind of display the whole RSVP string is taken as a single object that moves and changes, neutralizing the token benefits of different spatial positions. To rule out this account, the two occurrences of the repeated item must be presented simultaneously in two different locations. Because in the real world one object cannot be in two different locations at once, the two simultaneous stimulus items cannot reasonably be interpreted as two different views of the same thing. Indeed, Mozer (1989) reported that subjects more severely underestimate the number of letters in a briefly presented spatial array when it contains letter repetitions than when all letters are unique. It seems reasonable to take this result as evidence for RB in spatial arrays. On the other hand, because Mozer used a different task, which produced much smaller effects than those typically found in repetition blindness paradigms, a further investigation was called for.

Experiment 1 compared repetition blindness for items distributed across time, space, and both space and time. Because one cannot see a whole sentence in a brief flash, letters were used in this experiment rather than words. Following the methodology of Kanwisher and Potter (1990, Experiment 8), subjects were asked to report the whole letter string. Because random letter strings produced overall low recall performance in pilot tests, strings making up words and pronounceable nonwords were used.

Critical words were chosen such that removal of the second occurrence of a repeated letter (C2) would yield a new word. These items will be labeled "repeated-letter" words. For example, removal of the second *a* in *manager* yields *manger*. The ideal unrepeated control for a word like this would be a word identical to *manager* except with the first *a* changed to a different letter and the remaining *a* removable to yield a new word. This criterion was rarely met precisely. An additional problem was that word frequency, which is correlated with length, might bias the subject toward the C2-omitted version of the word. To circumvent these problems, pronounceable nonwords were also used as stimuli, making exact unrepeated controls possible. For example, *conotle* was changed to *canotle* in the unrepeated condition. Repetition blindness was assessed by comparing subjects' tendency to omit C2 in reporting repeated and unrepeated strings.

The experiment was run in three different formats to compare the extent of repetition blindness when the letters were distributed across time, space, and both. In the "stationary RSVP" format (Kanwisher & Potter, 1990), items differed only in the time at which they were displayed.³ In the "moving RSVP" format, items were spread across both space and time. Finally, in the "simultaneous" condition, items appeared simultaneously at different locations.

Method

Subjects. Seventy-two subjects from the Massachusetts Institute of Technology (MIT) subject pool participated in this experiment, 24 in each of the three versions (see Footnote 3). Subjects were native speakers of American English and were under 30 years of age. They were paid for their participation.

Materials. We used 12 repeated-letter words, 12 repeated-letter nonwords, and their unrepeated controls. With one exception, repeated-letter items had only one letter repeated. There were one to three letters intervening between the two occurrences of the repeated letter (C1 and C2). Items varied in length from five to seven letters. For each word, an unrepeated control word in which C2 was also "removable" was chosen to be as close as possible in length and CV structure (e.g., *plan[t]* was the control for *star[t]*). Each nonword was generated by following the consonant-vowel structure and C1-C2 serial position patterns of one of the word items, but changing the letters (e.g., *po[p]lar/ma[p]les* was changed to *ro[r]tal/so[r]tal*). Stimulus items are given in Appendix A.

Design and procedure. Each subject saw 12 words and 12 nonwords in random order, counterbalanced for repeatedness across two versions of the materials.

Each trial began when the subject pushed the space bar on a computer keyboard. An asterisk (or row of eight asterisks in the simultaneous condition) appeared for 750 ms in the location of the subsequent stimulus item. Then the display appeared, as described below. Subjects were asked to wait until the display ended, and then to write down the letter string as accurately as they could on a sheet of paper. They were told that some items would spell out real words and some nonsense words, but that their task was simply to write down as much of the word or nonword as they could see. There were 10 practice trials.

In the stationary RSVP version (republished from Experiment 8 of Kanwisher & Potter, 1990; see Footnote 1), each letter was displayed one at a time for 133 ms in the same position. Immediately after the final letter, a percent sign appeared for 133 ms as a mask.

In the moving RSVP version, each letter was displayed one at a time for 83 ms, one character space to the right of the previous letter. An ampersand appeared immediately to the left of each letter, to mask the preceding letter.

In the simultaneous version, the string of letters appeared all at once as a word. The word (or nonword) was displayed for 33 ms, followed by a row of eight percent signs displayed for 100 ms as a mask.

Apparatus. Stimuli were presented on a CRT screen with a rapid fade phosphor, controlled by an IBM AT. Each whole word subtended about two degrees of visual angle. The experiment was carried out in normal room illumination.

³ The stationary RSVP condition is republished from Kanwisher and Potter (1990) to provide the appropriate comparison for the other two conditions. These experiments were identical in design, were run at about the same time, and differed only in presentation format.

Table 1
Percentage of Responses That Include Both C1 and C2: Experiment 1

Format	Condition					
	Words		Nonwords		Both	
	Repeated	Unrepeated	Repeated	Unrepeated	Repeated	Unrepeated
Stationary RSVP	40	72	27	62	34	67
Moving RSVP	35	65	22	47	28	56
Simultaneous	45	51	17	30	31	41

Results

We analyzed the data by comparing how often the subject included both C1 and C2 in his or her response, as a function of whether the item was repeated or unrepeated. The results of this analysis are shown in Table 1.

Analysis of variance by subjects revealed significant main effects of repeatedness, $F(1, 69) = 245.1, p < .001$; word-nonword, $F(1, 69) = 68.5, p < .001$; and format, $F(2, 69) = 3.3, p < .05$. There was a significant Format \times Word-Nonword interaction, $F(2, 69) = 4.0, p < .05$, and a Repetition \times Format interaction, $F(2, 69) = 22.3, p < .001$. The latter interaction results from the smaller repetition effect in the simultaneous condition than in the other two formats. For example, in a separate analysis of only the simultaneous and stationary RSVP formats, there was a highly significant Repetition \times Format interaction, $F(1, 46) = 27.8, p < .001$. No other main effects or interactions were significant (all $F_s < 1$). In separate analyses of each format alone, both main effects of repeatedness and word-nonword were significant for each of the three formats, all six $F_s(1, 23) > 10.0$; all six $p_s < .005$. The interaction of these two variables was not significant for any of the formats, with all three $F_s < 1.0$.

Discussion

These findings demonstrate, first, that repetition blindness occurs for letters distributed across time, space, and both. Thus, repetition blindness is more general than previously demonstrated; it occurs for tokens defined by any kind of spatiotemporal boundaries. Second, although having C1 and C2 appear in different spatial locations did not affect repetition blindness if the letters appeared one at a time (replicating Kanwisher & Potter's [1989] finding for words), it did reduce RB when the letters were presented simultaneously. Why was RB less severe for the simultaneous version than the other two versions?

One possibility is that (as discussed above) the simultaneous condition is the only presentation format in which C1 and C2 cannot be interpreted as arising from two different sightings of the same object. Another possibility is that in the simultaneous presentation format, the relevant level of processing is the word, not the letter. The much briefer presentation time required to produce a significant error rate is consistent with this speculation.⁴ Moreover, Kanwisher and Potter (1990) showed that when words are the task-relevant units there is no repetition blindness for letters that are shared by

two different words in a list (e.g., the *t* in *fault* and *heart*). Thus, in this experiment the difference between temporally distributed and simultaneous formats may be confounded with a change in the level of analysis of the stimulus items. The fact that there was no interaction between the repeatedness and word-nonword variables does not necessarily argue against this account, because orthographically regular nonwords would be expected to be processed at the word level when mixed together with actual words in the same experiment. (Also, although the triple interaction was insignificant, there appears to be a trend that RB for word stimuli dropped more in the simultaneous condition than did RB for nonword stimuli, consistent with the idea that the drop was due to a change in the level of analysis from letters to words.)

I pursued this issue further in Experiment 3, in which I investigated repetition blindness for simultaneously presented arrays of simple visual features under conditions that do not invite a change of the level of processing. First, I felt it was important to establish whether repetition blindness occurs for simple visual features at all, under the usual conditions of rapid serial visual presentation. In earlier studies of temporal numerosity, subjects consistently underestimated the number of light flashes (White, 1963) or tones (Harvey & Treisman, 1973) in a rapid sequence of identical stimuli. Although this might constitute evidence of RB for simple perceptual features, these studies included no unrepeated control, so the underestimates could have resulted from some more general problem in encoding rapidly presented items, repeated or not.⁵ I chose colors as the best features to use in Experiment 2, because they are strong candidates to be primitive features in visual information processing.

Experiment 2

If repetition blindness and feature integration theory have any deep connection, then one would certainly expect to find repetition blindness for repetitions of primitive visual features

⁴ The presentation rates used in the three conditions were chosen to produce similar error rates in the unrepeated condition. Although the rates were not perfectly matched, slowing the presentation time for simultaneous words did not increase RB, but diminished it.

⁵ Indeed, pilot work by Kanwisher and Treisman found no evidence that the number of tones in a rapid sequence was more severely underestimated when the sequence contained repeated tones than when it did not.

like colors. In this experiment, subjects viewed RSVP sequences of four colored symbols, and they were asked for an ordered report of the colors in each sequence. The key question was whether subjects would perform worse in reporting the colors from lists containing a repeated color than from lists not containing a repeated color.

Symbols were used rather than simple color patches because pilot results indicated that spatially overlapping color patches are hard to process individually at the rates characteristic of repetition blindness, whereas the colors of the mostly nonoverlapping symbols were easy to see. To diminish the distraction and possible Stroop naming interference from the symbols themselves, I used American Standard Code for Information Interchange (ASCII) symbols that were difficult to name. They were presented in the same order on each trial.

Two positions in each list were chosen as C1 and C2. In the repeated condition these two were the same color, and in the unrepeated condition they were different colors. Thus, performance could be compared in the two conditions by simply scoring how often the subject reported both C1 and C2 colors correctly in each condition.

One possible problem was that any difference in performance between the repeated and unrepeated lists might be due to an unwillingness to report repeated colors. The instructions stressed the existence of repetitions, and the importance of reporting repetitions if they were seen. Even so, subjects may have been more likely to guess unrepeated colors than repeated colors, spuriously elevating the unrepeated score. To control for this possibility, I included a condition in which repetition blindness was expected to be weak. On half the trials in this condition C1 was the first item in the list, and C2 was the last. With both primacy and recency effects, and an increased stimulus onset asynchrony (SOA) pitted against repetition blindness, I predicted that the RB effect would be weak in this control condition. In contrast, in the test condition, C1 was always the second and C2 was always the fourth item in each list. Because both test and control conditions were mixed together in the same experiment, it would be unlikely that any differential effect of repeatedness on the two groups could be due to response bias.

Method

Subjects. Twenty-four subjects participated in this experiment. Half of them participated in exchange for course credit and half in exchange for payment. All subjects in this experiment, and in the rest of the experiments reported in this article, were students at the University of California, Berkeley, from 18 to 35 years old.

Materials and design. The two independent variables in this experiment were repeated-unrepeated and test-control. The critical items C1 and C2 were either the same color (repeated) or different colors (unrepeated). The other two colors in each trial were always different from C1 and C2, and different from each other. In the second variable manipulated, C1 and C2 were either in Serial Positions 2 and 4 (test) or in Serial Positions 1 and 4 (control). There were 48 test items, 12 in each of the four subconditions created by crossing repeated-unrepeated with test-control. Six different colors were used in this experiment: red, blue, green, white, brown, and purple. The design unconfounded the variable repeatedness from any

main effects of the discriminability of particular colors, particular serial positions, or any interactions of the two.

I included 20 filler trials in the experiment, to match the design of Experiment 3. Half of the fillers contained repeated colors and half did not; they were not distinguishable to the subjects from test trials.

Procedure. Each trial began when the subject pressed the space bar on the computer keyboard. A character-sized rectangle of gray points appeared in the middle of the screen for 750 ms. Then the sequence of colored symbols appeared one at a time in the same place for 117 ms each, followed by the gray rectangle of points for 117 ms as a mask. In every trial, the four symbols were \diamond , $\#$, $@$, and \square (always appearing in that order). Each symbol subtended about 0.5 degrees.

Subjects were instructed to ignore the symbols but to pay attention to the colors and report them in order as soon as the sequence ended. They were also told, twice and with emphasis, that some of the sequences contained two different symbols of the same color. Subjects were instructed to be sure and say the color name twice if they saw it twice. They were told to say "blank" in a given position if they didn't have any idea what color was in that position.

All subjects were run first on a series of training trials, which they were told were just like the test trials, only slower. The colored symbols in these training trials were presented for 1,200 ms each. On the first two trials, the experimenter named the colors for the subject as the colors went by. On the next six trials, the subject named the colors. In this training sequence, three of the trials contained repeated colors, and because of the slow presentation rate, the subject could not avoid naming the colors twice on these trials. This training sequence served to guarantee that all subjects knew the appropriate color names and understood that there could be repeated colors in the experiment. After the slow training sequence, subjects went through a sequence of six practice trials at the usual rate before beginning the experimental trials.

Apparatus. This experiment and the rest of the experiments in this study were run on an AST AT computer with an NEC Multisync II screen. The software used for creating and running the experiments was Micro Experimental Laboratory (MEL), sold by Psychology Software Tools, Inc. (Schneider, 1988). In the MEL code, the numbers of the colors were: white, 15; blue, 9; red, 4; brown, 6; green, 2; purple, 13. The experiment was carried out in normal room illumination.

Results

A trial was counted as correct if the subject included both the critical colors (C1 and C2) in his or her report (regardless of report order). These data are shown in Table 2. Analysis of variance yielded significant main effects of repeated-unrepeated, $F(1, 23) = 27.5$, $p < .001$, and test-control, $F(1, 23) = 11.5$, $p < .005$, and a significant interaction of the two, $F(1, 23) = 4.9$, $p < .05$.

Table 2
Percentage of Responses That Include Both C1 and C2:
Experiment 2

C1/C2	Serial position	Condition	
		Repeated	Unrepeated
Test	2, 4	14	38
Control	1, 4	32	44

Discussion

Repetition blindness was demonstrated in this experiment by the fact that for test trials, subjects included both C1 and C2 in their reports less often in the repeated condition (14%) than in the unrepeated condition (38%). Because the effect of repetition was significantly greater for test trials than for control trials (and these trials were intermixed in the experiment), response bias or guessing strategies cannot account for the full effect seen in the test trials. Repetition blindness is evidently robust for colors in serially presented lists.

In a follow-up study, an additional 16 subjects were run in this experiment with a different set of instructions, to see if RB for colors would generalize to a different task. In this version of the experiment, subjects were simply asked whether each list contained a repeated color. (They were told that half the sequences did.) After correcting for guessing,⁶ subjects detected the repetition in 51% of control lists and 25% of test lists, $F(1, 15) = 5.5, p < .05$. This control:test condition performance ratio resembles that found for the recall task on repeated trials (32% control and 14% test), but differs from that found for unrepeated trials (44% control and 38% test). Thus, the same pattern of difficulty in encoding repeated items is evident in both recall and repetition detection tasks.

Once I had demonstrated repetition blindness for simple visual stimuli (i.e., colors and letters) in RSVP, I was in a position to use these stimuli to explore repetition blindness in spatial arrays. Although the stationary condition in Experiment 1 showed some RB for simultaneously presented letters, the items in that experiment were probably processed as words. In the next experiment, I asked whether repetition blindness would occur for simultaneously presented arrays of simple visual features under conditions that do not have the same ambiguity of processing levels seen in Experiment 1.

In Experiment 3a, six configurally different letters were chosen for the stimulus set both because they were highly nameable (thus minimizing difficulties in encoding and retrieval) and because they were likely to differ in terms of primitive shape features. Experiment 3b was identical, except that patches of six different colors were substituted for the six letters of Experiment 3a.

Experiment 3

Subjects viewed briefly presented arrays of four letters (Experiment 3a) or four color patches (Experiment 3b), chosen from a set of six.⁷ The subjects' task was simply to report the two array items indicated by surrounding cue boxes. The question was whether, after correctly reporting the first item (C1), subjects would be less likely to report the second item (C2) correctly if it was the same (the repeated condition) than if it was different (the unrepeated condition). Two items were cued rather than one because RB is an inability to individuate two tokens of the same type, rather than an inability to simply individuate one of two identical stimulus tokens. The cues appeared either just before the array (the precue condition) or immediately after a mask that followed the array (the postcue condition).

To diminish the possibility of a response bias favoring report of unrepeated items, the instructions strongly emphasized the existence of trials containing two identical targets. To avoid floor or ceiling effects, I used a staircase procedure to adjust the stimulus duration periodically to keep performance at about 50% correct on unrepeated trials. ("Correct" means that both items were reported correctly in the correct order.) Separate staircases were used for precue and postcue conditions. In each case, the adjustments were made on the basis of performance on unrepeated trials only, but repeated and unrepeated trial durations were yoked together.

Method

Subjects. Twenty-one University of California, Berkeley, students participated in both the letter version (Experiment 3a) and the color version (Experiment 3b). Five subjects were excluded because their performance on unrepeated trials averaged 15% or less correct in either the letter or the color experiment, indicating that they were not far above chance even for the longest allowable stimulus duration. (Even for these subjects, scores in the repeated condition averaged lower than scores in the unrepeated condition.)

Materials and design. In Experiment 3a, the array items were capital letters drawn from the set E, X, T, O, S, and W. These letters were selected to be as likely as possible to differ in terms of primitive shape features. In Experiment 3b, the array items were small color patches (composed of two adjacent # signs) drawn from the set of red (MEL code #4), purple (#5), green (#16), white (#63), yellow (#54), and blue (#9). Each stimulus array consisted of four items from the set displayed in the center of the quadrants of an outline square with a fixation point in the center. The cue display that specified which two of the four array items were to be reported consisted of the larger outline square and fixation point, with two smaller boxes inside surrounding two of the quadrants. (The two outermost sides of each cue box were shared with the outline square.) For the letter experiment, the mask consisted of four-point array rectangles covering the positions of the four array letters (these were actually ASCII symbols the size of letters but composed of four vertical dotted lines). For the color experiment, the mask was four rectangular color Mondrians that covered the four color patch positions.

Viewing distance was not fixed, but most subjects viewed the display from a distance of about 15". This resulted in the following angular sizes of the display components (in degrees): the outline square was about 4.8×4.8 , the array items were 1.5 degrees from the fixation point, the color patches (Experiment 3b) were 1 (horizontal) \times 0.7 (vertical), the letters (Experiment 3a) were 0.5×1 , and the cue boxes were 2×2.4 . Each of the four masking rectangles in the letter experiment exactly overlapped the letter area (0.5×1 degrees); the four multicolored masking rectangles for the color experiment were 1.5×1.2 degrees.

Experiments 3a and 3b were isomorphic (that is, Experiment 3a was translated into Experiment 3b by converting each particular letter into a patch of a particular color). In each experiment, the 96 test trials were broken down in 24 trials in each of the four conditions

⁶ The number of "real yeses" was calculated for each condition with the following formula: # real yes = $[\text{Obs} - 12(\text{FTR})]/(1 - \text{FTR})$, where Obs is the number of Observed yeses in that condition and FTR is the false target rate, or the percentage of unrepeated trials in which subjects wrongly said "yes."

⁷ This experiment was reported in Kanwisher (1990).

created by crossing repeatedness by cue (pre- vs. post-). Six different cue displays (all possible combinations of two square quadrants) were used; repeatedness and pre-postcue were counterbalanced within each of the six cue arrangements. In addition to the test items, there were 48 filler trials that included repeated colors or letters that were not both probed as target items. There were two versions each of Experiments 3a and 3b. The design counterbalanced for any effects of particular letters or colors, target locations, or any interaction of these factors.

Procedure. Each trial began when the subject hit the return key on the computer keyboard. A fixation point appeared for 750 ms in the center of an the outline square defining the border of the array. Then, in the precue condition, the two small cue boxes appeared inside the outline square for 150 ms, surrounding the location where two of the stimulus items would next appear. Next, the stimulus array composed of four letters or color patches flashed on briefly (one in the center of each quadrant of the large square). The stimulus array was displayed for a variable interval determined by the staircase manipulation. Finally, a mask composed of four rectangular white point arrays (Experiment 3a) or four rectangular color Mondrians (Experiment 3b) flashed on for 200 ms, covering the locations of the four array items. The postcue condition was identical, except that the cues appeared after the mask, not before the stimulus array.

Subjects were instructed to fixate on the point, look at the array and the cue boxes, and report the two items appearing in the location surrounded by the cue boxes. The subject typed the response into the computer keyboard. This response was either the two letters they thought they saw (Experiment 3a) or the first letters of the names of the two colors they thought they saw (Experiment 3b). If they had no idea what color or letter was presented in either or both positions, they typed corresponding question marks. Subjects were told to report the two items in a prespecified order (top before bottom; if both were on the same row, then left before right).

Subjects were told three times during the instructions that the experiment contained trials with repeated letters (or colors), and that if they thought both target items were the same they should type the corresponding letter in twice. Before the experimental session, subjects learned the stimulus set by going through eight trials with feedback that were just like the experimental trials (including both repeated and unrepeated trials) except that the stimulus array was displayed for a full second. This served to train them on the color names (and letter set) and to make sure they understood how to respond correctly to repeated trials. Then they did 24 faster practice trials (without feedback) before the experiment began.

We adjusted stimulus durations periodically throughout the practice test and experimental trials, using a staircase adjustment procedure, to keep unrepeated performance at about 50% correct for both precue and postcue unrepeated conditions. There were separate staircases for precue and postcue trials. Although staircase adjustments were made on the basis of unrepeated trials only, both unrepeated and repeated trials in a particular cue condition were affected the

same way. For each staircase, the adjustment was made after each two test unrepeated trials had been carried out in that cue condition. If both trials were correctly responded to (that is, both target items were correctly reported in the correct order), then the duration was decreased by one refresh (17 ms); if both were incorrect, then the duration was increased by one refresh; if one was correct, the duration stayed the same. For precue trials, the display duration started at 83 ms and was not allowed to go below 33 ms or above 100 ms; postcue trials began at 133 ms and were kept in the 50–150 ms range.

Results

The results are shown in Table 3. Individual target items were only scored as correct if they were reported in the correct location (indicated by report order). The data were then scored in terms of the conditional probability of getting the second item correct, given that the first item was reported correctly. Performance on the first item alone was fairly constant across conditions—averaging 77% for the letter experiment and 68% for the color experiment—so this technique does not differ much from simply reporting the percentage of trials in which subjects got both target items correct.

A guessing correction was used to discount each subject's raw unrepeated score by the expected number of correct unrepeated responses due to guessing, based on an analysis of that subject's errors. We made corrections for unrepeated precue and unrepeated postcue conditions separately. For each, we tallied the number (N) of incorrect responses in which the subject got the first item correct but reported a different item from the array in the place of the second item. Each raw unrepeated score (i.e., the number in which both the first and second target were correctly reported in the correct order) was discounted by $N/2$ before dividing by the total number of trials in which the first item was correctly reported. This procedure corrects for both location guesses and outright identity guesses (see Appendix B). Conservatively, no correction was made for the repeated condition data.

Analysis of variance by subjects on the (corrected) probability of getting the second item correct, given correct report of the first, showed a significant main effect of repetition, $F(1, 15) = 10.0, p < .01$, and significant interactions of Cue \times Repetition, $F(1, 15) = 16.4, p < .01$, and Cue \times Color-Letter, $F(1, 15) = 8.3, p < .05$. There was no interaction of Repetition \times Color-Letter, $F = 0.1$. No other main effects or interactions reached significance.

Table 3
Probability of Getting the Second Item Correct (Given Correct Report of the First) as a Function of Cue and Whether the Two Items Are the Same (Repeated) or Different (Unrepeated): Experiment 3

Stimulus	Experiment	Precues			Postcues		
		Repeated	Unrepeated	Duration	Repeated	Unrepeated	Duration
Letters	3a	.41	.60	62	.44	.48	120
Colors	3b	.24	.49	88	.45	.46	129

Note. The duration column gives the average stimulus duration for that condition (in ms). Unrepeated scores have been corrected (downward) for guessing.

Discussion

These data show substantial repetition blindness for both colors and letters, but only in the precue condition. That is, in the precue condition, given correct report of the first target item, subjects were significantly less likely to get the second item correct when it was the same as the first (e.g., "XX" or "red red"), compared to when it was different (e.g., "XT" or "red blue"). Thus, repetition blindness—and the type-token problem it exemplifies—generalize to spatial displays of simple visual features.

One important aspect of Experiment 3 is that the repetition blindness found in the precue condition cannot be explained in terms of an overall lack of location information. In the current experiment, items were only counted as correct when they were reported in the correct location, so both repeated and unrepeated trials required subjects to bind item identities to their locations. Thus, a general binding problem or an overall lack of positional information (as implicated by Keren & Boer, 1985; Mozer, 1989) cannot explain the observed difference in performance in the repeated and unrepeated conditions. (If anything, a general binding problem would favor the repeated condition, because switches between the two target locations would not be detected, whereas they would be counted as incorrect in the unrepeated condition.) Instead, there seems to be a particular difficulty in binding one type to two different tokens, above and beyond the difficulty of binding two different types to two different tokens (or, it would seem, in binding two different types to one token, as in feature conjunction). The early experiments on RSVP word lists demonstrate the analogous situation for temporal tokens: Temporal RB was robust even though the serial order of unrepeated words was reported quite accurately.

Why does RB occur primarily in the precue condition? At first glance, one might have predicted the opposite—that is, that directing attention to the target locations ahead of time (as in the precue condition) might have diminished RB. This did not happen; RB is evidently robust even when subjects know the target locations before the stimulus appears. But why was there no RB in the postcue condition?

The most plausible hypothesis is that there was no RB in the postcue condition simply because the stimulus durations were longer in that condition. (Recall that separate stimulus duration staircases were used for precue and postcue trials, and postcues had to be displayed for a substantially longer time to attain similar performance in the unrepeated condition.) Indeed, if serial attention is the key bottleneck in token individuation, then one would expect longer stimulus durations to reduce or eliminate repetition blindness (see also Mozer, 1989, Experiment 5). The next experiment tested the hypothesis that when stimulus durations were held equal in the precue and postcue conditions, the same amount of RB would result in each condition.

Experiment 4

I predicted that the same amount of repetition blindness would be found for the precue and postcue conditions if the

stimulus presentation durations in the two conditions were yoked together in one staircase procedure. Notice that it is not obvious in advance that this can be done without putting the precue trials at ceiling or postcue trials at floor. One other change was made to reduce the difficulty of the postcue condition, and to conform to a more standard postcuing paradigm (as used by Bjork & Murray, 1977, and others): Postcues were presented simultaneously with the mask, rather than after it.

Method

Subjects. Twenty-six University of California, Berkeley, students participated both in the letter version (Experiment 4a) and the color version (Experiment 4b). Two subjects were excluded because their performance on unrepeated trials averaged 15% or less correct in either the letter or the color experiment, indicating that they were not far above chance even for the longest allowable stimulus duration. (Even for these subjects, repeated scores averaged lower than unrepeated scores.)

Materials and design. The materials and design were identical to Experiment 3.

Procedure. The procedure was identical to that of Experiment 3, except that (a) in the postcue condition, the cues appeared simultaneously with the mask (immediately after the offset of the stimulus array), (b) the mask in the letter version was changed to three adjacent white # symbols, and (c) only one staircase procedure was used for all the conditions. As in Experiment 3, only performance on test unrepeated trials was used to adjust the staircase, but unlike Experiment 3, durations of trials in all conditions were determined by the same single staircase. The staircase was adjusted every four test unrepeated trials. If more than two of the last four trials had been answered correctly, the stimulus duration was decreased by one refresh (17 ms); if less than two had been answered correctly, it was increased by one refresh. Minimum durations were set at 33 ms, and maximums were set at 117 ms for the color version and 100 ms for the letter version.

Results

The results are shown in Table 4. Individual target items were only scored as correct if they were reported in the correct location (indicated by report order). The data were then scored in terms of the conditional probability of getting the second item correct, given that the first item was reported correctly. The same technique as that of Experiment 3 was used to correct for guessing in the unrepeated condition; no repeated condition guessing correction was used.

Analysis of variance by subjects revealed significant main effects of repetition, $F(1, 23) = 12.7, p < .005$, and pre-postcue, $F(1, 23) = 34, p < .001$. There was a significant interaction of color-letter and pre-postcue, $F(1, 23) = 12.6, p < .005$. There was no interaction of repetition and pre-postcue, $F(1, 23) < .05$. No other main effects or interactions reached significance (all $F_s < 1$).

Discussion

Evidently, when stimulus exposure durations are equalized in the two conditions, there is significant repetition blindness for both precued and postcued target item pairs. Thus, the

Table 4
Probability of Getting the Second Item Correct (Given Correct Report of the First) as a Function of Cue and Whether the Two Items Are the Same (Repeated) or Different (Unrepeated): Experiment 4

Stimulus	Experiment	Precues		Postcues		Duration
		Repeated	Unrepeated	Repeated	Unrepeated	
Letters	4a	.61	.76	.39	.49	80
Colors	4b	.52	.66	.39	.54	91

Note. The duration column gives the average stimulus duration for that version (in ms). Unrepeated scores have been corrected (downward) for guessing.

lack of RB in the postcue condition of Experiment 3 is probably due to the longer exposure durations used in that condition.⁸ The fact that brief exposure durations are necessary to obtain repetition blindness is consistent with the idea that it is the inability to serially attend to each array item that causes repetition blindness in briefly presented displays. A similar argument has been made by Mozer (1989), who showed that subjects made more severe underestimates of the number of letters in a spatial array if it contained repeated letters, but only when the array was presented under brief ("attention-limited") conditions, not under longer masked ("data-limited") conditions.

The results of Experiments 1–4 indicate that repetition blindness is indicative of a very general dissociation in the processing of visual types and visual tokens. It spans the gamut of visual stimuli from serial lists of words, letters, colors, and pictures (M. Potter, personal communication, 1987), to spatial arrays of colors and letters.

What are the implications for the relationship between repetition blindness and feature integration theory? The present results leave open the possibility that feature conjunctions and their errors (i.e., illusory conjunctions) happen in the same visual representations as repetition perception and its failure (i.e., repetition blindness). Both processes require type-to-token mappings; the question is whether it is the same kinds of tokens that are involved in encoding conjunctions and repetitions. This issue was addressed in Experiment 5.

Experiment 5

In Experiment 5, I investigated what would happen if the second occurrence of a repeated letter in an RSVP string appeared in red, with all the other letters being white.⁹ Intuitively, one might suspect that the salience of the red letter would reduce repetition blindness for that letter.¹⁰ However, if the present view is correct, one would have to predict that repetition blindness would be just as severe when C2 is displayed in red.

Suppose that the tokens that are necessary to conjoin features are the same mental entities as the tokens that are missing in repetition blindness. In that case, feature conjunction of C2's color and shape would only be possible after repetition blindness has already been overcome—that is, after C2 has been individuated as a new token. But if feature conjunction can only happen after the critical stage for repetition blindness, then a particular conjunction of features in

the stimulus should not be able to affect repetition blindness. In particular, there would be no way for C2's redness to boost individuation of C2 more than an adjacent letter.

This issue can also be addressed by asking subjects to report which letter they thought was red. If indeed feature conjunction requires the same tokens as repetition detection, then any items lost in repetition blindness should not have conjoined features. One consequence of this would be that when the second occurrence of a red repeated letter is omitted in recall, its redness should not tend to be attributed to the first occurrence of that same letter. The next experiment tested both this prediction and the prediction that making C2 red should not diminish repetition blindness.

Method

Subjects. Twenty-four subjects from the University of California, Berkeley, community participated in this experiment. They were paid for their participation.

Materials. The stimulus items were the same as those used in Experiment 1: 12 repeated words (and their unrepeated controls) and 12 repeated nonwords (and their unrepeated controls). For all items, C2 was "removable" in that if it was omitted, a word turned into a new word, or a pronounceable nonword turned into a new pronounceable nonword.

The other variable (besides repetition) was which letter was red. In the test condition, C2 was red. In the control condition, an adjacent letter was red. Usually, this was the letter after C2, but when C2 was the last letter in the string, it was the letter before C2.

I included 12 filler items in the experiment to provide more variety in the serial position of the red letter. Six of these were words, six were nonwords, and for each one either the first or the second letter was displayed in red.

Design and procedure. Each subject saw six test items in each of the four conditions created by crossing repetition with red location (test vs. control). These items appeared in random order (mixed with the 12 fillers), counterbalanced over four versions of the experiment.

Each trial began when the subject pushed the space bar on the computer keyboard. First, a number sign (#) appeared in the middle

⁸ Recent pilot data from my laboratory suggests that RB for colors may also depend on other factors, such as visual angle.

⁹ This idea was first suggested to me by Molly Potter.

¹⁰ An anonymous reviewer has reminded me that Jonides and Yantis's (1988) work shows that in spatial arrays, a distinctive color will not automatically pull attention (as sudden onsets do). However, even if a distinctive color does not automatically pull attention, it can clearly be used to direct attention when the task requires it, as the present task did.

of the screen for 500 ms. Then the letters were displayed one at a time in the same place for 133 ms each. Immediately after the last letter, a number sign appeared for 133 ms as a mask.

Subjects were instructed to read the string of letters as carefully as they could and to write them down on a sheet of paper as soon as the string ended. They were told that they might find the task easier if they "sounded out" the string as it appeared. They were also told that some of the items were real words and some were not, but that their task was simply to write down as much of the word or nonword as they could see. After they wrote down the string of letters, they were asked to circle the letter they thought appeared in red, guessing if necessary. Finally, they were told that if they knew where in the list the red letter was, but they didn't know what letter it was, they could indicate the letter with a dash and circle it. Ten practice trials preceded the experimental trials.

Results

I addressed two main questions in this experiment: (a) is there less repetition blindness when C2 appears in red, and (b) when a repeated red C2 is omitted, does its redness migrate to C1? To answer the first question, we counted the percentage of trials in each of the four conditions in which subjects correctly reported both C1 and C2. To answer the second question, we tabulated the number of times that the subject circled C1 as a percentage of all trials in which C2 was included in recall versus omitted in recall and as a function of repeated versus unrepeated condition.

The data relevant to the first question are shown in Table 5. Analyses of variance showed that performance was significantly higher in the unrepeated condition than in the repeated condition, $F(11, 23) = 66.7, p < .001$, and significantly higher when C2 was red (test) than when it was not (control), $F(1, 23) = 8.9, p < .01$. The interaction of these two variables was also significant, $F(1, 23) = 4.5, p < .05$. The interaction was a result of the fact that redness boosted performance only for unrepeated trials, $t(23) = 3.2, p < .005$, but not for repeated trials, $t(23) = .2, p > .5$. On the other hand, this finding does not indicate that RB, which is a function of the ratio (not the difference) of repeated to unrepeated scores, was more severe in the test than in the control condition: When the extent of repetition blindness, measured by the "repetition blindness index" (the repeated score divided by the sum of the repetition and unrepeated scores), was calculated separately for the test (.29) and control (.27) conditions for each subject, there was no statistically significant difference, $F(1, 23) < 1, p > .5$.

The second key question was where the redness was seen when C2 was presented in red in the repeated condition but was omitted in recall. In particular, in what percentage of these responses do subjects circle C1? Unfortunately, the data

on redness migration was marred by several problems: (a) some subjects failed to follow the instructions to circle a letter in each trial; (b) when only one of the critical letters was reported, it could not be determined whether this was C1 or C2 on the 20 test repeated trials (across all subjects) in which none of the intervening letters were included in report; and (c) subjects circled C1 so rarely overall that there was not enough data for statistical comparisons.

Nevertheless, by looking at the responses that did not suffer from the first two problems mentioned above we can get some indication of what happens to C2's redness when C2 itself is suppressed by repetition blindness. In particular, of all the unambiguous responses in the test repeated condition (summed across all subjects) in which C2 was omitted from recall, C1 was circled only 10% of the time, whereas a letter adjacent to C2 was circled 41% of the time. This was similar to the pattern of responses seen when C2 was omitted in the test unrepeated condition: C1 was circled on 11% of these trials, and a letter adjacent to C2 was circled on 40% of the trials. Thus, when no C2 token is available, an adjacent-letter token often assumes its role, whether the C2 token was preempted by repetition blindness, or simply forgotten (as in the unrepeated condition).

When red C2s were included in recall, however, subjects generally correctly attributed the redness to C2, whether or not it was a repetition. Specifically, of the unambiguous trials in which C2 was included in recall in the test repeated condition, C2 was circled 61% of the time, a letter adjacent to C2 was circled 17% of the time, and C1 was circled on 5% of trials. Unrepeated trials showed a similar pattern: 61% circlings of C2, 18% circlings of a letter adjacent to C2, and 6% circlings of C1.

Discussion

Evidently, making the second occurrence of a repeated letter red does not save it from repetition blindness. This surprising finding is consistent with the hypothesis that feature conjunction (e.g., of C2's redness with letter identity) requires token individuation of C2. Thus, particular conjunctions of features in the stimulus cannot exert a specific effect on repetition blindness because features cannot be conjoined until after repetition blindness has already been overcome. (However, although redness cannot boost individuation of red letters, it can facilitate report of already individuated letters: This is seen in the fact that performance in the unrepeated condition was higher when C2, rather than an adjacent letter, was red.) Second, when a repeated red letter suffers repetition blindness, its redness does not seem to migrate to the earlier occurrence of that same letter. Instead, it tends to be attributed to a letter adjacent to C2. In a sense, then, RB can be thought of as causing illusory conjunctions: Because establishment of the correct letter token is prevented, only incorrect letter tokens are available for (illusory) conjunction.

Both findings support the hypothesis that feature conjunction requires token individuation. This hypothesis implies that when token individuation fails (as in repetition blindness), feature conjunction will also fail. The two main findings

Table 5
*Percentage of Responses That Include Both C1 and C2:
Experiment 5*

Red location	Condition	
	Repeated	Unrepeated
Test (C2 red)	25	69
Control	23	51

of Experiment 5 follow from this: (a) making a repeated C2 red does not facilitate individuation of C2, and (b) the redness from C2 does not selectively migrate to C1 when C2 is omitted, because the redness was never conjoined with C2 identity in the first place.

In other words, feature conjunction apparently requires token individuation; the tokens involved in repetition detection and the tokens involved in feature conjunction seem to be the same thing. Thus, both feature conjunction and the perception of repetitions—and their respective failures in the cases of illusory conjunction and repetition blindness—may occur at the same stage of visual processing, in which visual types become linked to visual tokens.

If visual attention is necessary to bind types to tokens, then unattended items should not get tokenized. This allows a prediction to be made. The standard account of repetition blindness (Kanwisher, 1987) holds that RB results from an inability to token individuate two tokens of the same type within a short period, rather than an inability to activate the type twice during that period. Thus, if the first occurrence is recognized but not token individuated, there will be no blindness for the second occurrence. This prediction was confirmed, for example, in Experiment 3 of Kanwisher's (1987) study, in which report of only the last item in a list was helped, not hindered, by an earlier occurrence of the same word in that list. Therefore, if unattended items do not get tokenized, they should not cause blindness for later occurrences of the same type. This hypothesis was tested in Experiment 6.

Experiment 6

Experiment 6 provided a further test of the hypothesis that only attended (and not unattended) items would cause repetition blindness for later occurrences of the same type.¹¹ RSVP lists of letters were presented to subjects, who were asked to report only the letters that appeared in color. In each list of 10, three letters appeared in color (red, green, blue, yellow, or magenta); the rest were displayed in white. If only attended items are tokenized, then only C1s that appear in color should interfere with report of later occurrences of the same letter; unattended (i.e., white) C1s should not cause repetition blindness.

A secondary question addressed in this experiment was whether there would be repetition blindness for repeated colors, even though the particular color of each item was irrelevant to the task. Thus, repeated letters were presented in three ways in this experiment: C2 was always in color, but C1 could either be in the same color, a different color, or no color at all (i.e., white). When C1 appeared in white, an adjacent letter became colored.

Method

Subjects. Thirty subjects from the University of California, Berkeley, community participated in this experiment in exchange for course credit.

Materials. Each of the 36 stimulus lists was made by randomly choosing 10 capital letters, without replacement. The three colored letters were either in Serial Positions 4, 7, and 10; 5, 8, and 10; or 5,

7, and 9. Items that fell in these serial positions were randomly assigned to three different colors (from the set including blue, green, red, yellow, and magenta). Then various changes were made in these lists to create the repeated condition and to create the three C1 color conditions (same color, different color, and no color).

C1 was always the first colored item in the list, C2 was the second, and the third was not a critical item. The lists, as described in the previous paragraph, were in the unrepeated different-color condition. To make the other five conditions, only C1 changed. To make the repeated condition, C1 simply became the same letter as C2, keeping its color the same as the original unrepeated C1. To create the same-color condition (whether repeated or unrepeated), C1 was changed to match the color of C2. To make the no-color condition (whether repeated or unrepeated), the old (different-color) C1 was changed to white and an adjacent letter became the new C1, assuming the color that the old C1 had had. For lists that had C1 and C2 in Serial Positions 5 and 7, it was the letter preceding the old (different-color condition) C1 that became the new (no-color condition) C1; for the other two serial position pairs, the new C1 was the letter following the old C1. Notice that because "C1" always refers to the first (to-be-recalled) colored letter in the sequence, in the repeated no-color condition C1 and the first occurrence of the repeated letter are not the same.

Design and procedure. Each subject saw six lists in each of the six conditions created by crossing repetition (repeated vs. unrepeated) by C1-color (same color, different color, and no color). Each item appeared once in each condition, counterbalanced across six versions of the experiment.

To begin each trial, the subject pushed the space bar on the computer screen. A number sign (#) appeared in the middle of the screen for 500 ms. Then the sequence of letters appeared one at a time in the same place for 133 ms each. Immediately after the last letter, a number sign flashed on in the same place for 150 ms.

Subjects were told to report the names of the colored letters in each list, in the order they had appeared if possible. They were told that sometimes a letter would appear twice in the same list, and if they saw a letter twice, they should say it twice in their response. They were also told that sometimes a color would appear twice in a single list, and if so, both of its letters should be reported. Subjects were instructed that each list would contain three colored items, so they should give three responses if possible. However, if they had no idea what a given letter was, they were allowed to say "blank" in its position. Four practice trials preceded the experiment.

Results

Trials in which the subject included both C1 and C2 in their response were scored as correct, even when they were not reported in the correct order. The data are shown in Table 6. Analysis of variance revealed significant main effects of repetition, $F(1, 29) = 24.0, p < .001$, and C1-color, $F(2, 58) = 6.0, p < .005$, and a significant interaction of the two, $F(2, 58) = 6.8, p < .005$. When the same analysis was run on only the different-color and same-color trials (with no-color trials excluded), the interaction disappeared, $F(1, 29) < 1$. Thus, the original interaction of Repetition \times C1-Color is due entirely to the differential effect of repetition in the no-color condition versus the other two conditions.

¹¹ The idea for this experiment was suggested to me by Anne Treisman.

Table 6
*Percentage of Responses That Include Both C1 and C2:
 Experiment 6*

C1 color	Condition	
	Repeated	Unrepeated
Different	14	34
Same	20	39
None	36	36

Most of the errors subjects made in this task were illusory conjunctions (i.e., reports of letters that were in the list but that were not colored). Averaging across all subjects, all three responses on each trial, and all conditions, 55% of response items were correct, 30% were illusory conjunctions, 9% were blanks, and 5% were intrusions of letters not in the list.

Discussion

Repetition blindness occurred (to a similar extent) whether C1 and C2 were the same or a different color. However, there was no blindness at all when C1 did not appear in color. This result is in sharp contrast to that of Experiment 5, in which both the white letters and the colored letter had to be attended. Thus, as predicted, only attended letters cause repetition blindness for later occurrences of themselves. This is consistent with the idea that attention is required for token individuation. Notice, however, that 133 ms ought to be plenty of time to recognize a letter—even when it is not required for report. Thus, it is likely that even when C1 was not attended (or token individuated) it was still recognized.

On the other hand, an alternative account of Experiment 6 is possible. Perhaps repetition blindness only happens when C1 itself must be reported—not simply when it must be individuated and remembered. Although this response-level view of RB is consistent with the results of Experiment 6, it cannot account for a variety of other experiments demonstrating RB under conditions that require individuation and memory of C1, but do not require the subject to actually report C1.

First, subjects perform badly and with low confidence on repetition detection tasks, in which they must simply indicate either (a) whether or not a list contains a repetition (Kanwisher, 1986; see also Experiment 2 in the present study), or (b) which item appeared twice in a list (Kanwisher, 1987). Second, subjects more often rate RSVP sentences as ungrammatical when they contain grammatically necessary repeated words (e.g., “When she spilled the ink there was ink all over”) than when they do not (e.g., “When she spilled the liquid there was ink all over”; Kanwisher, 1986). Third, Bavelier and Segui (1990) showed that subjects are slower to verify that two (adjacent) words appeared in a sentence if one of them is the second occurrence of a repeated word. For example, subjects would be slower to verify “ink all” in the first (repeated-condition) sentence above than in the second (unrepeated-condition) sentence. These examples demonstrate that explicit report of C1 is not a necessary condition for RB.

Notice that in Experiment 6 there was no repetition blindness for repeated colors—that is, on unrepeated trials subjects performed no worse when C1 and C2 were the same color than when they were different colors. At first glance, this result might seem to conflict with the RB for colors seen in Experiment 2. However, there was no reason for subjects to encode specific colors in this experiment; all that mattered for the task was whether an item was colored at all (as opposed to white). Thus, RB does not appear to happen for repeated types that are irrelevant to the task at hand.

General Discussion

Both illusory conjunctions and repetition blindness have been explained in terms of distinctions between the processing of visual types and tokens (Kahneman & Treisman, 1984; Kanwisher, 1987; Treisman & Schmidt, 1982); whereas illusory conjunctions can be thought of as errors in assigning two different types to a single token, repetition blindness can be seen as a failure to assign a single type to two different tokens. In these studies, I asked whether this parallel actually reflects shared underlying processing mechanisms. To address this question, I investigated (a) whether RB occurs for the same kinds of stimuli that produce illusory conjunctions, and (b) whether the tokens that are necessary to detect repetitions are the same mental entities as the tokens necessary to conjoin visual features.

Experiments 1–4 provide evidence that RB does occur for the kinds of stimuli that produce illusory conjunctions. In Experiments 1, 3, and 4, RB was demonstrated for items displayed in spatial arrays as well as temporal lists, and Experiments 2, 3, and 4 demonstrate RB for simple visual features like color and shape. Earlier work on illusory conjunctions has shown the converse: Illusory conjunctions occur not only for spatially distributed arrays of primitive features, but also for temporally distributed lists of more complex visual types like word identity and letter case (Lawrence, 1971), letter identity and color (McLean, Broadbent, & Broadbent, 1982), and photographs of objects and the frames surrounding them (Intraub, 1985, 1989). Thus, illusory conjunctions and repetition blindness appear to affect the same broad range of visual stimuli.

Experiment 5 provides evidence that the visual tokens necessary to detect repetitions are the same as the tokens necessary to conjoin features. Repetition blindness for letters was undiminished when the second occurrence of the repeated letter was the only item that appeared in red (and all the other letters in the list appeared in white). This surprising result can be explained if we assume that the second occurrence (C2) was never conjoined with the redness feature, because feature conjunction requires a link to the same visual token that is precluded by repetition blindness. Furthermore, when subjects in this experiment were asked to say which letter appeared in red, they rarely indicated the first occurrence of the repeated letter. Both findings suggest that repeated items that are lost in repetition blindness do not have conjoined features. Thus, the tokens necessary to conjoin features and the tokens necessary to perceive repetitions appear to be one and the same.

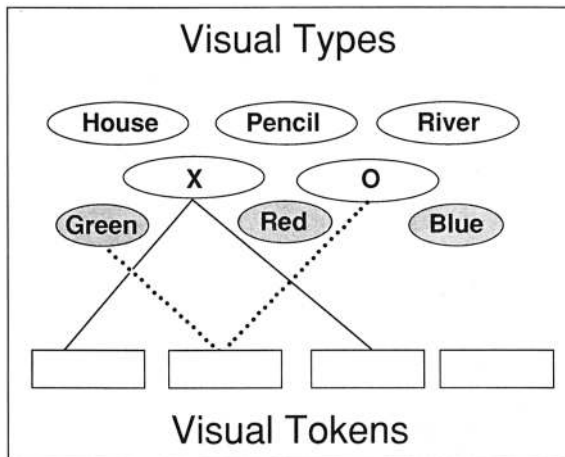


Figure 1. Linking types to tokens to encode (a) a repetition, as shown in the solid lines (two Xs), and (b) a conjunction, as shown in the dotted line (a green O). (Errors in the binding process result in repetition blindness and illusory conjunctions, respectively.)

The original RB model (Kanwisher, 1987) can thus be extended to accommodate illusory conjunctions as well as repetition blindness (see Figure 1). As in the earlier model, types are activated representations in memory, which are originally free-floating, but which can become linked to particular object or event tokens in a continually updated episodic "map." The present findings generalize the model to include spatial as well as temporal tokens, and to include simple visual features as types. In the extended model, conjunctions are encoded by linking multiple types to the same object token, and repetitions are encoded by linking multiple tokens to the same type (see Figure 1). Failure of either process results from errors in the mechanism that binds visual types to visual tokens.¹² The convergence of accounts of repetition blindness and illusory conjunctions raises a number of theoretical issues and problems.

Spatial Uncertainty

According to the present view, token individuation ought to require visual attention. Thus, conditions of limited attention will produce errors on tasks that require the individuation of visual tokens, namely, conjunctive search, feature-absent search¹³ (Treisman & Gormican, 1988), and repetition detection. But, although repetition blindness and illusory conjunctions evidently originate in the same mechanism, the conditions that bring them about are somewhat different. Illusory conjunctions result from an overall difficulty in binding types to tokens (repeated or not), whereas repetition blindness results from an additional difficulty in binding one type to two tokens.¹⁴ This difference can be seen more clearly by examining the role of "spatial uncertainty" (the lack of location information) in the two phenomena.

Spatial uncertainty has been invoked to account for illusory conjunctions (Prinzmetal & Keyzar, 1989),¹⁵ and for earlier reported cases of difficulty with repeated letters (Keren & Boer, 1985; Mozer, 1989), which are similar or identical to

repetition blindness. For example, Mozer (1989) has argued that the difficulty in detecting repetitions of an object in a spatial array is a direct consequence of the overall lack of location information. Given identity information without location information, he argues, there is no way to encode dual occurrences. But this account cannot explain the spatial RB seen in Experiment 3; the difficulty in encoding dual occurrences could not have resulted from an overall lack of location information distinguishing C1 and C2 because such location information was just as necessary for unrepeated pairs (items were only counted as correct if reported in the correct location). Similarly, an overall temporal uncertainty cannot explain temporal RB, because RB occurs under conditions in which the order of unrepeated items is quite discernable. Instead, RB results from a particular difficulty in binding one type to two tokens, over and above the problem of binding two different types to their respective different tokens.

Keren and Boer (1985) have also argued that spatial uncertainty is a necessary condition for the repeated-letter inferiority effect (RLIE), in which subjects perform worse at identifying a briefly presented letter when it is flanked by an identical letter than when it is flanked by a different letter. They find that the effect obtains only when the target is cued after the array, not when it is cued before. But the RB for precued items seen in the present Experiments 3 and 4 suggests that it is not postcues or spatial uncertainty per se, but rather the requirement to individuate two different tokens of the same type that generates the difficulty with repeated items. Keren and Boer's result can be explained simply by the fact that with precues only one item must be individuated, but with postcues subjects must individuate both tokens because they do not know which one will be cued.

A Paradox

The new RB model (depicted in Figure 1) captures an important generality by bringing feature conjunction and

¹² Kanwisher (1990) has argued that the tendency for people to make both kinds of errors under attention-limited conditions is reminiscent of the behavior of connectionist networks, which have both a "binding problem" (Hinton, McClelland, & Rumelhart, 1986) and a "type-token problem" (Norman, 1986).

¹³ Feature-absent search, for example searching for an O among Qs, is also a token task in that one needs to know both that there is a token, and that it is not linked to the relevant type.

¹⁴ It may also be particularly difficult to bind two types to one token when the two types are from the same dimension: Wolfe, Stewart, Shorter, Friedman-Hill, and Cave (1990) argued that visual search for within-dimension conjunctions (of two different colors or two different orientations) is always slow and serial (but see also Grabowecky & Khurana, 1990).

¹⁵ Prinzmetal and Keyzar (1989) argued that illusory conjunctions result from the fact that spatial resolution is lower for colors than for luminance. But this attempt to explain illusory conjunctions in terms of the properties of early vision is implausible because it would predict color mixing rather than color migration. It seems more likely that in experiments on illusory conjunctions, the colors are easily resolved but become recombined with form features at a later categorical stage in which they must become linked to particular tokens.

repetition blindness together under a single theoretical framework. Furthermore, it accommodates the fact that it is not only primitive visual features that can float free, unanchored to particular locations in space and time. More complex stimulus categories like words sometimes behave in a similar fashion. Specifically, words can be illusorily conjoined with other visual types (like letter case) and they can be recognized without achieving a full-fledged conscious representation as a particular object or event (for example, see Cheesman & Merikle, 1986; Marcel, 1983).¹⁶

On the other hand, there remain important differences between primitive visual types, like colors, and complex visual types, like words. For example, there seem to be a number of special-purpose spatially parallel modules to process primitive features (Treisman, Cavanagh, Fischer, Ramachandran, & von der Heyt, 1990), along with specialized mechanisms to direct attention to particular locations in these "feature maps" (Treisman, 1988). It is unlikely that such mechanisms exist for word recognition. This presents a puzzle.

The model put forth here holds that a visual stimulus defined by a conjunction of features can only be reliably identified after those features become bound together through links to the same object token. Evidence that words are conjunctive stimuli of this kind comes from the fact that when several words are presented too briefly to allow focused attention to each one in turn, illusory recombinations of letters are sometimes seen (McClelland & Mozer, 1986; Treisman & Souther, 1986). But if words are defined by conjunctions of features, and conjunctive stimuli can only be recognized after token individuation, then how can the second occurrence of a repeated word be recognized without already being individuated as a new token? There is a paradox here, because repetition blindness seems to imply recognition without individuation (Kanwisher, 1987).

Tokens and Object Files

This paradox can be sharpened by examining the two different theoretical perspectives it emerges from, in particular, by comparing the present RB model with Kahneman and Treisman's ideas about "object files" (Kahneman & Henik, 1981; Kahneman & Treisman, 1984; Treisman, 1988; see also Duncan, 1984). Though originally derived from completely different sets of findings, the two approaches have converged to produce very similar ideas about the organization of visual information.

Kahneman and Treisman define an object file as "the representation that maintains the identity and continuity of an object perceived in a particular episode" (Kahneman & Treisman, 1984, p. 54). The current suggestion that feature conjunctions are encoded by binding two different visual types to one visual token is essentially the same as Treisman's suggestion that activated features must be collected in one object file (through an act of attention to the object's location) to encode a conjunction. And, in general, the RB model's separation of type recognition from token individuation is an echo of Treisman's requirement to "separate the recognition network from the temporary object files" (Treisman, 1988, p. 222).

However, although both approaches highlight the dissociation of types and tokens in vision, they seem to disagree about which is primary. Kahneman, Treisman, and Gibbs (1983) argue that "perception appears to give primacy to objects, defining them more by spatio-temporal constraints of continuity than by their attributed labels or properties." Similarly, Treisman (1988) suggests that it is only after features are collected into object files that they can be matched to stored representations. To paraphrase loosely, this view holds that tokens must come before types in vision.

In contrast, repetition blindness has been explained as a case in which an item is recognized without ever becoming linked to a new token. According to this view, tokens play a different—although important—role in processing. They allow the end products of recognition to become stabilized into a conscious representation of objects and events, unclouded by any partial activations left over from the recognition process itself. This perspective is similar to Marcel's (1983) description of "recovery"—a process in which an unconscious perceptual hypothesis (type) can result in a conscious representation by becoming linked to its spatiotemporally defined sensory source (token).

Thus, the RB model and the object file model disagree on whether tokens are always primary in visual information processing. Furthermore, this disagreement is impossible to resolve with the available data. If tokens must always be individuated before conjunctive types can be recognized (as Treisman and her collaborators claim), then how could word-specific repetition blindness occur? And why should illusory conjunctions of a word with its letter case or color be so frequent? On the other hand, if any preexisting visual types can be recognized before token individuation (as in the RB model), then why does accurate identification of conjunctive types sometimes require attention? Clearly, neither model can explain the full range of phenomena.

Although a definitive resolution of this issue must await further research, some speculation is possible. There must be some situations in which conjunctive visual types can be recognized without first becoming individuated and some situations in which features must be packaged into object files before accurate identification is possible. One possibility is that these two situations correspond to two different routes to word (and perhaps also object) recognition: a serial, attention-demanding "tokens-first" route and a fast but fallible "types-first" route. The tokens-first route might be necessary for correct recognition in an environment of conjoinable features (as when LANE must be recognized when displayed near SAND without being confused with LAND). On the other hand, environments with few conjoinable features might al-

¹⁶ Type recognition without token individuation happens not only for repeated words in the case of repetition blindness, but also for unrepeated words when presentation rate is very fast. For example, when subjects are asked to report the words from a short RSVP list presented at 10 or more words per second, they often show fairly good performance with very low confidence. Subjects tend to be very reluctant to say anything, and are surprised to learn that they are correct. The difficulty may be that untokenized type activations feel subjectively more like thoughts than percepts.

low fairly accurate recognition without prior token individuation. In Treisman's terminology, the types-first route would amount to allowing direct access from feature maps to stored object descriptions in memory—an alternative to the object-file (tokens-first) route.

This dual-route model could resolve the paradox of how repetition blindness can be a case of recognition without token individuation, even though words are conjunctions and feature conjunction requires token individuation. Specifically, if the words in rapid RSVP sentences can generally be recognized through the types-first route, then repetition blindness can result when the second occurrence of a repeated word is recognized but not individuated as a new token. On the other hand, under conditions in which conjoinable features are present in high numbers, word recognition will become error-prone if attention is limited and token individuation fails (McClelland & Mozer, 1986; Treisman & Souther, 1986). In this kind of situation, most correctly recognized words will already have been tokenized. Thus, in an environment of highly conjoinable features, one might not find reliable recognition without token individuation.

If this account is correct, it may help explain why object and scene recognition is so fast (Potter, 1975) even though most complex objects are conjunctions of features, and recognizing them should require serial attention (Dehaene, 1989). In particular, one might speculate that objects, like words, may often be recognizable through the fast but fallible types-first route. Thus, the serial (token-first) processes that go on in conjunctive visual search (Treisman & Gelade, 1980) may not be typical of most real-world object recognition. However, more evidence is needed before this idea can be accepted.

Two Levels of Tokens

One problem with the notion of the types-first processing route should be addressed at this point. In particular, there are several reasons to think that the visual system *cannot* activate types first, but must begin with some kind of tokens. First, most researchers agree that one of the first tasks in vision is to segment the visual array into candidate objects, and that this segmentation is a necessary precondition for recognition (Duncan, 1984; Fox, 1977; Marr, 1982; Pylyshyn, 1988; Treisman, 1988; Ullman, 1984). Furthermore, the examples of apparent motion and (to some extent) stereopsis demonstrate that early vision is involved in the matching of as-yet-unrecognized blobs. These correspondence problems are solved largely on the basis of spatiotemporal information; they involve the matching of tokens, not types (Kolers, 1972; but see also Green, 1989). Finally, it would seem that some kind of primitive tokens would have to be created by preattentive vision to provide destinations to which visual attention can be directed (Duncan, 1984; Kahneman & Henik, 1981; Pylyshyn, 1989). But if early vision must begin with tokens, then what is meant by the types-first processing route?

The hypothesis offered here is that the preattentive blobs early vision must use to compute apparent motion, stereopsis, and scene segmentation simply cannot be the same entities as the tokens involved in the conscious representation of objects

and events. This point is illustrated in Experiment 1b, in which a string of letters moved continuously from left to right across the screen. In this experiment, repetition blindness occurred without creating an apparent discontinuity in the rightward motion of the letter string. (Presumably, if C2 was simply omitted from the string, leaving a momentarily blank screen, such a discontinuity or "jump" would have been noticed.) Thus, even though C2 did not register as a conscious independent event, it was apparently counted as a blob by the apparent motion process. Although this evidence is merely anecdotal, it illustrates the distinction between the preattentive blobs of early vision and the higher level tokens that constitute the conscious representations of objects and events.

"Where" and "When"

One aspect of the current theoretical perspective is that it gives space and time similar roles in the organization of visual information. In particular, a consideration of the various ways that visual types can be mapped onto visual tokens reveals that spatially and temporally defined tokens are subject to similar binding errors. Just as one can sometimes know the identities but not the locations of objects in an array (Treisman & Gelade, 1980; but see also Johnston & Pashler, 1989), one can also know the identities but not the serial order of items presented in a temporal sequence (Scarborough & Sternberg, 1967). And, as argued above, similar errors occur for spatially and temporally distributed items when two types must be bound to one token (i.e., temporal and spatial illusory conjunctions) and when one type must be bound to two tokens (i.e., temporal and spatial repetition blindness).

So, although much attention has recently been paid to the dissociation between "what" and "where" in vision (Rueckl, Cave, & Kosslyn, 1989; Sagi & Julesz, 1985; Ungerleider & Mishkin, 1982) an analogous dissociation may characterize temporally distributed stimuli (i.e., "what" vs. "when"). But these two dissociations may in fact not be distinct. The identities of objects and events can be established and maintained as those objects follow trajectories through both space and time (e.g., see Pylyshyn, 1989). Treisman gives the example of a distant airplane that "retains its continuity as a single perceptual object, even when we see it flap its wings and alight on a nearby tree, thus forcing us to change the label we initially assigned" (Treisman, 1988, p. 219). Thus, the visual system must be able to deal with both space and time at once, to analyze object *trajectories* (perhaps the true dichotomy is "what" vs. "whither").

But must the actual *processing* of temporally distributed stimuli not differ greatly from the processing of spatial arrays, because temporal stimuli arrive over time, but spatial stimuli are all available at once? Not necessarily. If serial attention is required to individuate spatial tokens, then the processing of spatial stimuli may be spread over time in much the same way that it must be for temporally distributed stimuli (although the rates may differ). Thus, the subjectively salient differences between temporal lists and spatial arrays may not be mirrored by such great differences in the way those stimuli are processed.

The picture that emerges from this study is one in which our conscious experience of the visual world is organized into a representation of individual objects and events (tokens). The identities and properties of these objects are encoded through links to preexisting visual categories (types). This discrete and categorical form of representation may characterize not only the way primitive features are organized and combined in early visual information processing, but also the way these features give rise to and become associated with more complex visual categories like the identities of words, objects, and scenes.

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Appendix A

Stimulus Materials, Experiments 1 and 4

A. Words

1. report (*deport*)
2. breathe (*clothe*)
3. closest (*beast*)
4. linens (*ravens*)
5. manager (*bloats*)
6. poplar (*maples*)
7. deduce (*medals*)
8. bible (*table*)
9. diverse (*morale*)
10. titles (*vitals*)
11. start (*plant*)
12. dense (*lunge*)

B. Nonwords

- levolt (*mevolt*)
 bleaske (*bloaske*)
 coprars (*copfars*)
 rafels (*ramels*)
 conotle (*canotle*)
 mamber (*gamber*)
 rortal (*sortal*)
 nental (*sental*)
 waberne (*waborne*)
 fufle (*gufle*)
 choth (*croth*)
 ferge (*farge*)

Appendix B

Rationale for the Guessing Correction Used in Experiments 3 and 4

A given unrepeated array contains four items: C1 ("A"), C2 ("B"), and two nontarget items ("C" and "D"). Each array excludes two items from the set ("E" and "F"). Thus, in this notation, the correct response to any unrepeated array is "AB." The point of the guessing correction is to estimate the expected number of correct "AB" responses due to guessing, so that this number can be subtracted from the raw unrepeated score.

Two assumptions simplify the task. First, I assumed that correct responses of "A" for C1 are not due to guessing. (Performance on C1 is similar across conditions—and quite high overall—so guessing of C1 is unlikely to play a major role.) Thus, I was concerned only with strategies of guessing C2. Second, I assumed that C2 guesses can be of two kinds: location-guesses (in which the subject guesses an item from a pool of identified but not located items), and identity-guesses

(in which the subject guesses an item outright without having identified or located it).

With regard to C2 Identity Guesses, the expected number (I) of trials in which the subject (sees C1 correctly and) guesses C2 outright and which result in a correct response can be estimated by counting the total number of trials in which the subject gets C1 correct but guesses one of the nonarray items, for example E, for C2. Thus, $I = \#(AE)$. Because there are actually two nonarray items, it is better to use them both for a less noisy estimate:

$$I = 1/2[\#(AE) + \#(AF)].$$

With regard to C2 Location Guesses, a first approximation of the number L of C2 Location Guesses that result in the correct response

comes from counting the number of responses in which the subject gets C1 correct but gives one of the nontarget items for C2. Thus,

$$L \approx 1/2[\#(AC) + \#(AD)].$$

But this number overestimates L because in some responses "AC," C was an outright guess, not a location guess. How many "lucky guesses" of C are there? We have just estimated the amount: it is I. The same argument holds for "AD" responses. So the best estimate for L is

$$L = 1/2[\#(AC) + \#(AD) - 2I].$$

The overall guessing correction ("GC") for Unrepeated trials, then, requires subtracting both I (the expected number of correct responses

due to identity-guessing C2) and L (the expected number of correct responses due to location-guessing C2) from the raw Unrepeated score (that is, the total number of correct unrepeated responses). That is, $GC = L + I$. Substituting in, we see that I cancels:

$$GC = 1/2[AC + AD - 2I] + I$$

and

$$GC = 1/2[AC + AD].$$

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Butcher, Geen, Hulse, and Salthouse Appointed New Editors, 1992-1997

The Publications and Communications Board of the American Psychological Association announces the appointments of James N. Butcher, University of Minnesota; Russell G. Geen, University of Missouri; Stewart H. Hulse, Johns Hopkins University; and Timothy Salthouse, Georgia Institute of Technology as editors of *Psychological Assessment: A Journal of Consulting and Clinical Psychology*, the Personality Processes and Individual Differences section of the *Journal of Personality and Social Psychology*, the *Journal of Experimental Psychology: Animal Behavior Processes*, and *Psychology and Aging*, respectively. As of January 1, 1991, manuscripts should be directed as follows:

- For *Psychological Assessment* send manuscripts to James N. Butcher, Department of Psychology, Elliott Hall, University of Minnesota, 75 East River Road, Minneapolis, Minnesota 55455.
- For *JPSP: Personality* send manuscripts to Russell G. Geen, Department of Psychology, University of Missouri, Columbia, Missouri 65211.
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