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Cross-Modal Feature and Conjunction Errors in Recognition Memory

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Four experiments were conducted to investigate whether a modality-specific familiarity contributes to feature and conjunction errors and, hence, to recognition memory. In each experiment, the presentation modality of compound words was manipulated at study (auditory or visual), and in Experiment 2 the presentation modality for the test also was manipulated. In Experiment 3, participants were pushed to respond quickly in order to create a reliance on familiarity rather than recollection. In Experiment 4, a direct manipulation of response deadline was employed. Across experiments, auditory and visual tests did not produce different hit rates or feature and conjunction error rates, and shifts in study-to-test modality did not affect hit rates or feature and conjunction error rates. The response deadline manipulation of Experiment 4 affected old/new discrimination but not feature and conjunction effects (feature/new and conjunction/new discrimination), producing a dissociation. Unlike implicit perceptual memory, modality information does not appear to contribute significantly to the familiarity underlying feature and conjunction errors. The familiarity underlying feature and conjunction errors, and thus in recognition memory, is different from the familiarity underlying perceptual implicit memory.

Key Words: recognition memory; false recognition; feature errors; conjunction errors; modality effect; recollection; familiarity; dual-process theory.

A renewed interest in false memory phenomena has attracted much attention and has spawned several articles—enough to warrant a special issue of *Journal of Memory and Language* (1996) on illusory memories. The experiments in this article add to research on a particular false recognition phenomenon. In this research, items presented during a study phase serve as primes for lures on a later recognition test. For example, the presentation of study word primes such as *gemstone, heartburn,* and *drumbeat* bias participants to falsely judge test lures such as *headstone* (part old, part new; called a feature lure) or *heartbeat* (both parts old but rearranged; called a conjunction lure) as having been presented in the earlier study phase (Underwood, Kapelak, & Malmi, 1976; Reinitz, Lammers, & Cochran, 1992). Typically, participants correctly identify old words at a greater rate than they misidentify conjunction, feature, or new words. Conjunction errors are produced at a higher probability than feature errors, which in turn, are produced at a higher probability than new words (old > conjunction > feature > new). Conjunction errors have been obtained with a variety of materials, including words (e.g., conjunction of syllables; Underwood &

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Zimmerman, 1973; Reinitz et al., 1992), compound words (Ghatala, Levin, Bell, Truman, & Lodico, 1978; Underwood et al., 1976), word phrases (Underwood et al., 1976), face drawings (Reinitz et al., 1992; Reinitz, Morrisey, & Demb, 1994), photographs (Bartlett & Searcy, 1998), and abstract pictures (Kroll, Knight, Metcalfe, Wolf, & Tulving, 1996). In addition, conjunction errors have been obtained for different subject groups (e.g., normal young adults and amnesics, Kroll et al., 1996; Reinitz, Verfaellie, & Milberg, 1996; young and older adults; Kroll et al., 1996; Rubin, Van Petten, Glisky, & Newberg, 1999).

Two theoretical approaches have been used to explain conjunction errors. One view follows a representational approach (primarily), and the other view follows a procedural account. The representational viewpoint proposes the existence of specific types of memorial *representations* (e.g., Kroll et al., 1996; Reinitz et al., 1992, 1994, 1996). In contrast, the procedural approach focuses on the *processes* involved in a given memory task (e.g., Jones & Jacoby, 1997; Rubin et al., 1999) without making any claims about representations. In some sense the two frameworks are similar. Both approaches use two factors to explain conjunction errors. What differs between the two views is how those two factors are used to account for the errors. For example, the current state of the representational viewpoint explains conjunction errors but neglects feature errors. Also, the representational approach does not address potential differences in retrieval dynamics for the different types of representations. In contrast, a dual-process account explains both feature and conjunction errors and has different retrieval dynamics for the different processes. Although we point out the differences between the two approaches, the general distinctions are not far apart in our view.

The representational approach proposes two types of representations, features and configurations (e.g., Kroll et al., 1996; Reinitz et al., 1992, 1996). Features are lower level representations and may be bound together to form more global or configurative representations. For example, the word *gemstone* would be encoded as two separate features, *gem* and *stone.* In addition, these two features could be bound to form a configurative representation, *gemstone.* Recognition of old words is suggested to be based on both features and configurations. False recognition of conjunction lures is thought to be based on either features in the absence of configurations (e.g., Kroll et al., 1996; Reinitz et al., 1992, 1996) or inaccurate configurations formed during encoding (Kroll et al., 1996). The rate of old word recognition is typically greater than the rate of conjunction errors because recognition of old words benefits from configurative representations in addition to features.

One hypothesis on conjunction errors promotes the idea that the errors are produced because of a failure to bind features together during encoding or because a configuration has been forgotten (e.g., Kroll et al., 1996; Reinitz et al., 1992, 1994, 1996). In other words, this hypothesis promotes the idea that conjunction errors occur because a configurative representation is unavailable. For example, for the study words *blackmail* and *jailbird,* the features *black* and *bird* (and, presumably, *mail* and *jail*) would be available (and accessible) but the configurations, *blackmail* and *jailbird,* would not be available.[1] In the absence of a configuration, the two features, *black* and *bird,* could be conjoined during retrieval, creating an illusion that the word *blackbird* was presented earlier in the study phase. Feature errors have not been considered to be different from chance (Reinitz et al., 1992, 1996). Thus the theory has not been developed to account for these errors.

An additional, but less developed, hypothesis on conjunction error production was suggested by Kroll et al. (1996). In their proposal, conjunction errors may occur, in part, because the components of study primes are inappropriately bound during the study phase. For example, for the study words *blackmail* and *jailbird* the elements *black* and *bird* could be bound inappropriately during encoding *to form an inaccurate configurative representation, blackbird.* This

[1] The terms “available” and “accessible” are used here in the same spirit as in Tulving and Pearlstone’s (1966) article.

binding error is supposed to occur when the two study episodes occur close together in timer. (Because a feature lure contains an element on the test that was not presented in the study phase, this errant binding hypothesis does not account for feature errors.) Two theoretical mechanisms in the hippocampus were proposed to be involved in the binding process, a binding mechanism and a binding mechanism inhibitor. Essentially, the binding mechanism allows one to bind elements of stimuli together, correctly or incorrectly, while the binding inhibitor protects one from inappropriately binding elements that do not belong together. The notion is that conjunction errors occur due to a failure of the binding inhibitor to hold the binding mechanism in check—a disinhibition of binding. This hypothesis was suggested in order to account for some data from individuals with hippocampal damage, but the same disinhibition of binding was suggested to occur in normals, albeit at a lower rate.

At this point a critical problem with the two hypotheses developed under the representational view is that no explanation is offered to account for the production of feature errors (Rubin et al., 1999). Research conducted under a procedural approach, however, can account for both feature and conjunction errors. Rubin and colleagues argued that feature and conjunction errors are based on familiarity (though see Reinitz et al., 1992).

We support the idea that familiarity contributes to feature and conjunction errors, but we prefer a dual-process approach. Dual-process theory (e.g., Atkinson & Juola, 1974; Jacoby, 1991; Jacoby, Yonelinas, & Jennings, 1997; Mandler, 1980, 1991; Yonelinas, 1997) proposes that two processes, recollection and familiarity, provide independent bases for responding on a memory task. Recollection is considered to be relatively slow and controlled, whereas familiarity is thought to be relatively fast and automatic (Atkinson & Juola, 1974; Dosher, 1984; Hintzman & Curran, 1994; Yonelinas & Jacoby, 1994).

In a dual-process account, old words may be recognized based on either familiarity or recollection (or both). Thus, for old word recognition familiarity and recollection act in concert. Feature and conjunction errors are considered to be based on *familiarity in the absence of recollection.* For feature and conjunction lures, familiarity with the component(s) presented in the study phase pushes one to respond “old,” but recollection of the prior study word primes can allow one to avoid an “old” response. Therefore, familiarity and recollection are placed in opposition. Feature and conjunction lures instate a greater level of familiarity than new words, resulting in a false alarm rate for feature and conjunction words that is above baseline. (We refer to feature and conjunction error rates above baseline as feature and conjunction effects.) Because conjunction lures comprise two old components but feature lures comprise one old component and one new component, conjunction lures engender greater familiarity than feature lures. Finally, feature and conjunction lures (e.g., *blackbird*) may be poor cues for the retrieval of study word primes (e.g., *blackmail* or *jailbird*). For instance, feature and conjunction lures may not spur retrieval of study prime words spontaneously. More impressively, despite conditions that should encourage recollection (e.g., being informed of the feature and conjunction lures), participants have committed feature and conjunction errors (e.g., Kroll et al., 1996; Reinitz et al., 1992). Thus, conscious attempts to retrieve study primes often may be fruitless.

Both behavioral and neuropsychological data support the recollection/familiarity distinction. Typically, dissociations have been produced by affecting recollection, which is observed in the hit rate, without influencing familiarity, which is characterized by a null effect on feature and conjunction error rates. For example, dividing attention at encoding (Reinitz et al., 1992) has decreased recognition of old words compared to a full attention condition but influenced feature and conjunction errors relatively little, if at all. Similarly, a level of processing manipulation during encoding has affected old word recognition but not feature and conjunction error rates (Reinitz et al., 1996). Older adults and amnesics have produced lower old/new discrimination than young adults or controls. In contrast, feature and conjunction error rates for older adults and amnesics have been equal to or greater than those for young adults or normal controls (Kroll et al., 1996; Reinitz et al., 1996; Rubin et al., 1999). Finally, event-related potentials for false alarms to feature and conjunction lures are distinguishable from those for hits to old words (Rubin et al., 1999). In Rubin and colleagues' work, measures of frontal lobe and medial temporal/diencephalic function were related to false alarm rates but not the hit rate.

A concept often associated with or equated to familiarity is processing fluency (e.g., Carrol & Kirsner, 1982; Jacoby & Dallas, 1981; Johnston, Dark, & Jacoby, 1985; Wagner & Gabrieli, 1998; Whittlesea, 1993). Under a dual-process approach, perceptual fluency or familiarity is thought to be the basis for perceptual priming on implicit memory tests (for reviews on implicit memory, see Richadson-Klavehn & Bjork, 1988; Roediger & McDermott, 1993; Schacter, 1987). Fluency also is thought to influence recognition judgments. Although recognition fluency may be perceptual or conceptual (Luo, 1993; Whittlesea, 1993), the fluency/familiarity that provides a basis for responding in recognition may be "functionally and anatomically distinct" from the fluency/familiarity underlying perceptual priming (Wagner & Gabrieli, 1998, p. 211). Finally, feature and conjunction errors have been proposed to be based on a "retrieval-based processing fluency" (Rubin et al., 1999, p. 3).

The primary source(s) of familiarity or fluency underlying feature and conjunction memory errors has remained unspecified. Underwood and his colleagues (1976) attributed the production of conjunction errors to nonsemantic factors (e.g., nonconceptual factors). Direct evidence for this idea was obtained by Ghatala et al. (1978), though they found that semantic conditions could slightly influence the probability of conjunction errors. Certainly, Underwood and colleagues' concept of "nonsemantic" factors was broad, being defined as including "the more or less raw perceptual responses involving visual-phonetic-articulatory responses" (p. 299). Surprisingly, though, no research has been designed to pinpoint the specific nonsemantic factors involved in the production of feature and conjunction errors.

The familiarity underlying feature and conjunction memory errors of faces and other nonverbal stimuli is presumably perceptual. (This perceptual form of memory should not be confused with conjunction errors from perception experiments; e.g., Prinzmetal, Presti, & Posner, 1986; Treisman & Schmidt, 1982; Treisman & Souther, 1986). However, the familiarity underlying feature and conjunction errors of compound word stimuli could be perceptual, lexical, conceptual, or any combination of the three. On the conceptual front, recognition usually is affected more by conceptual than perceptual factors (Wagner & Gabrieli, 1998), and semantic relateness can increase the likelihood of conjunction errors (Ghatala et al., 1978).

On the perceptual front, which is the focus of the present work, perceptual fluency can exert some influence on recognition judgements (Johnston, Hawley, & Elliot, 1991; Johnston, Dark, & Jacoby, 1985; Rajaram, 1993; Whittlesea, 1993), and a substantial conjunction error rate on recognition has been reported for conditions where semantic relatedness between study and test words was not a factor (Ghatala et al., 1978). Johnston and colleagues' concluded that perceptual fluency may be most pronounced when explicit memory (e.g., recollection) is minimal. Given our position that feature and conjunction errors are based on familiarity in the absence of recollection, Johnston and colleagues' conclusion seems to fit well with the idea that perceptual factors might be responsible for feature and conjunction errors. Although perceptual priming for conjunction words has has been obtained inconsistently (Reinitz & Alexander, 1996; Reinitz & Demb, 1994; Reinitz et al., 1996), this inconsistency may simply reflect a lack of unitization of the conjunction words during the study phase (cf. Weldon, 1991). Thus, perceptual or lexical fluency (or both) probably contribute to feature and conjunction errors for compound words.

Recognition hit rates have been found to be affected by perceptual manipulations (e.g., Kirsner, 1973, 1974; Jacoby & Dallas, 1981; Rajaram, 1993) and are thought to comprise perceptual and conceptual (elaborative) processes (e.g., Gardiner, 1988; Gardiner & Parkin, 1990; Gregg & Gardiner, 1994; Jacoby & Dallas, 1981; Mandler, 1980; Rajaram, 1993). However, changes in study-to-test modality (e.g., aural study and visual test) do not always affect recognition memory relative to a situation in which the study and test modality match (e.g., visual study and visual test). Some researchers have found an advantage on the hit rate for words presented in the same study and test modalities (Gregg & Gardiner, 1994; Jacoby & Dallas, 1981; Kirsner, 1974), but others have not (Challis et al., 1993; Gregg & Gardiner, 1994; Rajaram, 1993).[2] Gregg and Gardiner (1994) used the Remember/Know procedure (e.g., Gardiner, 1988; Tulving, 1985) and found a significantly higher proportion of Know judgments, which are thought to provide an index of familiarity, for a modality-consistent condition when the study stimuli were presented relatively briefly and the orientation task was perceptual in nature. On the other hand, Rajaram (1993) found no differences between Remember or Know judgments on modality-consistent and modality-inconsistent conditions.

One problem with previous recognition studies is that any effect on perceptual familiarity may have been obscured by recollection because the focus was on the hit rate. However, if recollection and familiarity operate in concert for the identification of old words, then any influence on perceptual familiarity might be clouded by a similar effect on recollection. What is needed is a way to tease apart the separate influences of the two processes.

The method of opposition (e.g., Jacoby, 1991) is a procedure that allows one to gain insight to the separate influences of recollection and familiarity. As already noted, feature and conjunction lures fulfill a condition of opposition. Familiarity pushes one to commit feature and conjunction errors, whereas recollection can be used to avoid such errors. Therefore, feature and conjunction errors offer a unique avenue to investigate whether a modality-specific familiarity contributes to recognition memory.

The experiments described below were designed to test whether a modality-specific familiarity contributes to feature and conjunction errors and recognition memory, in general. Two assertions were tested. First, the experiments tested whether recognition familiarity is different from the perceptual fluency underlying perceptual priming (e.g., Wagner & Gabrieli, 1998). Second, the experiments tested an idea on false recognition and the modality of study presentation (Smith & Hunt, 1998).

Wagner and Gabrieli (1998) compared findings for implicit perceptual memory, conceptual implicit memory, and familiarity-based recognition on a number of dimensions. One manipulation not addressed in their article was aural/visual modality shifts from study to test. Changing the modality from aural study to a (visual) perceptual implicit test has been shown to produce about half as much priming compared to modality-consistent conditions (e.g., visual study and visual test; perceptual identification, Jacoby & Dallas, 1981; Kirsner, Milech, & Standen, 1983; Rajaram & Roediger, 1993; word stem completion, Graf, Shimamura, & Squire, 1985; Jacoby, 1996; and word fragment completion, Blaxton, 1989; Challis et al., 1993; Rajaram & Roediger, 1993; Roediger & Blaxton, 1987). In contrast, as noted above, aural/visual modality effects have been small and inconsistent on recognition tests. On the other hand, none of those studies used a method of opposition to separate the influences of recollection and familiarity. Thus, presentation modality, a perceptual manipulation, might have an effect on recognition familiarity similar to that of implicit perceptual familiarity. The question was: Would a change in study-to-test modality reduce feature and conjunction error rates?

Finally, there has been little work on false recognition of words that includes a modality manipulation. One recent study that used the Deese/Roediger and McDermott (1959/1995) false memory paradigm obtained differences in false recognition of critical lures for auditory and visual study lists (Smith & Hunt, 1998). A

[2] The difference in Jacoby's study came in the form of a Modality × Word frequency interaction. A modality effect occurred for low- but not high-frequency words.

TABLE 1

Sample Design for Experiment 1

Item type	Visual study	Auditory study	Test (visual)	No. of test items
Old, auditory		"lumberyard"	lumberyard	10
Old, visual	playmate		playmate	10
Conjunction, auditory primes		"buckwheat" "slapshot"	buckshot	10
Conjunction, visual primes	checklist needlepoint		checkpoint	10
Conjunction, mixed primes	drawbridge	"shoestring"	drawstring	20
New	—	—	eyelash	20

Note. For the conjunction conditions, the order within each pair of study word primes was counterbalanced across participants. Quotes indicate that a word was heard (but not seen).

much lower false alarm rate for critical lures occurred when the study list was presented visually. Smith and Hunt suggested that "visual processing of the study items provides a better basis for differentiation [between study items and critical lures] than does auditory presentation" (p. 714). This modality differentiation hypothesis was tested in our experiments. If the hypothesis extends to feature and conjunction errors, then one would expect participants to show a lower false alarm rate after a visually presented study list than an auditorially presented study list.

EXPERIMENT 1

Three questions were addressed in the experiment. First, does changing the presentation modality from study to test affect recognition of old words? Second, is the familiarity underlying conjunction errors sensitive to shifts in study to test modality? Finally, does mixing the presentation modality of the study word primes (one prime visual and one prime auditory) reduce the probability of conjunction errors.

In the design of the experiment, presentation modality of the study words was manipulated (aural or visual), but the presentation modality of the test was always visual. There were four basic conditions: old, conjunction where the study prime words were presented in the same modality (both visual or both auditory), conjunction where the study prime words were presented in different modalities (one visual and one auditory), and new. The old words were divided so that one half of the old words was studied visually and the other half was studied aurally. The conjunction condition with both study primes presented in the same modality was divided so that one half of the study primes was presented visually (both visual) and the other half was presented aurally (both auditory). Thus, the six item types were old-aural study, old-visual study, conjunction-aural study primes, conjunction-visual study primes, conjunction-one aural, one visual study prime, and new words (see Table 1 for a sample of the design).

If conjunction errors are sensitive to study-to-test shifts in modality, then aurally studied words should produce a lower conjunction error rate than visually studied words (e.g., transfer appropriate processing; cf. Roediger & Blaxton, 1987; Roediger, Weldon, & Challis, 1989). The mixed-modality conjunction condition presented an interesting situation. For that condition, one component of a conjunction test word would occur in the same modality as during the study phase (visual), but one component would occur in the other modality (aural). Again, if the amount of study-to-test overlap in modality is important in the production of conjunction errors, then one would predict that the mixed modality condition would produce fewer errors than the visual study/visual test condition but more errors than the auditory study/visual test condition. In contrast, if conjunction errors occur due to a binding process during encoding (Kroll et al., 1996) then mixing the modality of the study word primes might lead to fewer conjunction errors than when the two study primes occur in the same modality (visual or aural). Finally, if participants are better able to discriminate between old words and critical lures for visual study conditions than for auditory study conditions, then the false alarm rate should be lower for the feature and conjunction lures in a visual study condition (presuming no difference in hit rates; e.g., Smith & Hunt, 1998).

Method

Participants. The participants in all four experiments were New York University undergraduates who received credit toward an introductory psychology course as compensation. Each participant took part in only one of the four experiments. Thirty-two students participated in Experiment 1.

Materials and equipment. The same materials and equipment were used in all four experiments. Two hundred fifty-eight compound words were used as stimuli. Each word was recorded in an alto female voice and stored in an individual sound file (approximately 1.4 s in length) on a Pentium computer equipped with a VGA monitor and a soundcard. The entire set of compound words was composed of 80 sets of compound word triplets (240 words) and 18 buffer/practice test words. Each triplet included a target word (e.g., *blackbird*), which ranged from 6 to 11 letters long, and study word primes (e.g., *blackmail* and *jailbird*), each of which overlapped the target word with regard to one of its lexical components. The experiment was run by a program composed with Micro Experimental Laboratory software (MEL; Schneider, 1990).

Design and procedure. A repeated-measures design was used for the experiment. The 80 triplets were divided into 8 lists of 10 triplets, 2 lists for each condition (old, conjunction with study primes presented in the same modality, conjunction with study primes presented in different modalities, and new). For the old words, one list was presented visually, while the other list was presented auditorially. For the conjunction condition in which the two study primes were presented in the same modality, one list was presented visually, but the other list was presented auditorially. Thus, there were six item types: old-visual study, old-aural study, conjunction with both study word primes presented visually, conjunction with both study word primes presented aurally, conjunction with study word primes presented aurally and visually (one aural and one visual), and new. The two nontarget words of each triplet served as study word primes for the conjunction conditions and were used during the study phase only. The 8 lists were rotated through each of the four conditions so that each list occurred in each condition equally often across subjects. In addition, the modality of the study words and the order of the study prime words were balanced across subjects. For example, for the conjunction condition where the study primes were presented in different modalities, the design was balanced for whether each study prime word was presented aurally or visually and for the presentation order of the two study primes. The study primes were separated by an average of three intervening items, with a minimum of one and maximum of five intervening items.

A single study order based on item type (100 study trials: 20 target words for the old conditions and 80 study primes for the conjunction conditions) was constructed, and for each participant the program called the words from the appropriate lists for that counterbalancing assignment and placed them in the appropriate item type “slots” of the study list. Thirty-two study list variations were used to balance the assignment of lists across the conditions. In the study list, no more than four trials within a given modality and no more than two words within a given item type occurred consecutively. Six primacy and six recency buffers (half visual and half auditory) preceded or followed the study words.

In the study phase, participants were told to read and listen to the words silently in preparation for a later memory test. The participants were not told about the various types of items that they would encounter on the test. The study words were presented at a 3-s rate. This study rate was used by Underwood and colleagues in their conjunction error experiments (Underwood et al., 1976; Underwood & Zimmerman, 1973). Words presented visually were seen in white lettering on a black background for 1.5 s, and words presented aurally were heard in an alto female voice for a duration of about 1.4 s. To equate the timing for auditory and visual trials, the computer program allowed 1.5 s for each sound file to be played. After the presentation of each study word, visual or aural, there was a 1.5-s intertrial interval.

After the study phase, participants received instructions for an 80-item, visual yes/no recognition test without mention of the conjunction lures. After the test instructions but before the test trials, a 16-item practice test was given. The old words on the practice test were buffers from the study list, and no conjunction lures were on the practice test. This same practice test also was used in Experiments 2–4. For the actual test the order of the stimuli was the same for each participant. (Note that a given stimulus represented different item types across subjects because of the counterbalancing employed in the study phase.) Each test trial lasted 3.75 s, and the intertrial interval was 1 s. A test trial was about the same duration as that in Underwood and colleagues studies (Underwood et al., 1976; Underwood & Zimmerman, 1973). A test word was presented for 1.5 s, during which time participants were to think about whether the word was presented in the study phase. After 1.5 s, the test word was replaced by a response signal (******). At the onset of the response signal, participants were given 1.5 s to enter their recognition judgment by pressing one of two keys (labeled "Y" for "Yes, the word is old" or "N" for "No, the word is not old"). Failure to respond in the allotted time was not a large concern because mean response times in a similar experiment by Kroll and colleagues (1996) ranged from 1.47 to 1.74 s. Nevertheless, if a response was not entered within 1.5 s, the program provided visual feedback—a late signal ("TOO SLOW")—for 750 ms. Otherwise, the screen was blank for 750 ms after a response was entered.

The use of the 1.5-s poststimulus delay period (in a sense, a long deadline procedure) deserves some comment. Initially, a series of experiments was planned in which auditory test conditions and response deadline manipulations were to be employed. (In fact, the order in which the experiments were conducted is different than the order in which we present them.) The 1.5-s delay period in this experiment was used to achieve consistency across the series. For example, because of programming limitations, responses could not be collected while a sound file was being played (approximately 1.4 s in duration). Thus, for auditory test conditions in other experiments, a minimum of a 1.42-s delay was needed.

Results and Discussion

The mean proportions of "old" responses for the various item types can be seen in Fig. 1. These proportions were based on all trials, regardless of whether the trial timed out. (The time-out rate was extremely low, .01.) As expected, old words were judged "old" at a rate higher than that for conjunction lures, which in turn, were identified "old" at a rate higher than that for new words. The modality of the study presentation, however, had little effect on recognition for old or conjunction words. A repeated-measures ANOVA on the six item types was conducted on the mean proportions of "old" responses. The outcome of the ANOVA indicated that at least two of the means differed significantly [$F(5,155) = 37.19$, $MS_e = .02$], and a Tukey test demonstrated that the differences between each of the general conditions—old, conjunction, and new—was significant: old word (aural = .68 and visual = .68), conjunction word (auditory study primes = .48, visual study primes = .43, and mixed study primes = .45), and new word (.25) (α was set at .05 for all analyses throughout the experiments).

The modality manipulations failed to produce any significant differences. There was no difference between the auditory and visual old word item types, and the visual and auditory conjunction word primes produced conjunction errors at similar rates (.43 vs .48, respectively). Mixing the modality of the study primes within the study phase (one visual and one auditory) did

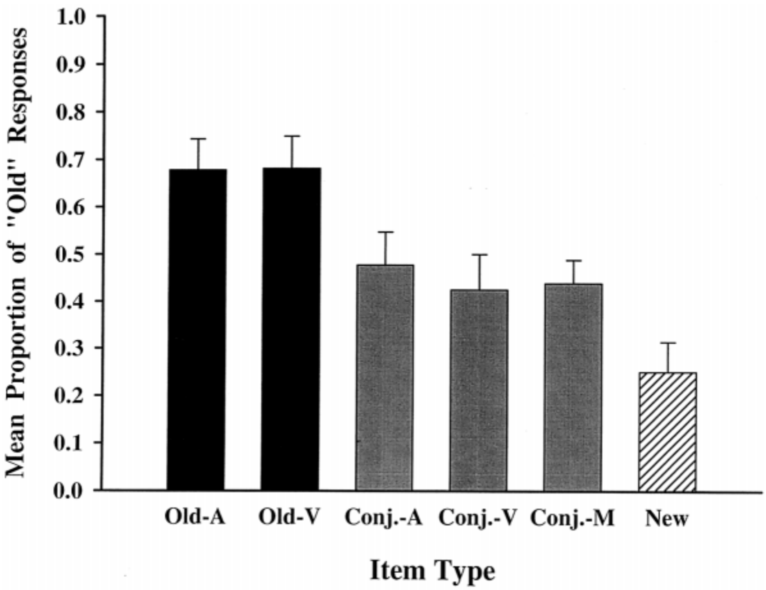

FIG. 1. Experiment 1: Mean proportion of "old" responses across item types. Error bars represent 95% confidence intervals. Conj., Conjunction; A, Auditory study presentation; V, Visual study presentation; M, Mixed study presentation (one auditory and one visual).

not reduce the conjunction error rate relative to when the study primes were presented in the same modality (.44 vs .45, respectively). Thus, no evidence was obtained for a perceptually specific component of familiarity in old word recognition or conjunction error production. In addition, if conjunction errors occur due to an inappropriate binding of the features during encoding (Kroll et al., 1996), then mixing the study modality of those components does not seem to influence the probability of those binding errors. Finally, the visual modality discrimination advantage hypothesized by Smith and Hunt (1998) gained little, if any, support. In that hypothesis, fewer errors should have been committed in the visual conjunction primes condition relative to the auditory conjunction primes condition. While the trend was in the direction predicted by that hypothesis the difference was not significant [$t(31) = 1.24$, $SE = .04$].

EXPERIMENT 2

No modality effects were obtained in Experiment 1. Experiment 2 included feature lures and an auditory test condition to further pursue the questions regarding perceptual familiarity. (The conjunction condition with mixed study primes—one auditory and one visual—from Experiment 1 was dropped.) For the feature condition, test lures contained one component of a prior study word and a new component. We expected that the pattern of results for feature errors would resemble those for conjunction errors. However, because there is less overlap between the study and test components in a feature condition relative to a conjunction condition, we expected the feature error rate to be less than the conjunction error rate (cf. Kroll et al., 1996; Reinitz et al., 1992, 1996; Rubin et al., 1999). Again, proponents of representation theories that use a feature/configurative distinction have not attempted to explain the occurrence of feature errors. However, procedural theories do account for feature effects.

Almost all conjunction studies have used a visual study and a visual test phase. One exception is a report of three experiments that used auditory presentations at study and test (Ghatala et al., 1978).[3] None of those experiments, however, manipulated presentation modality at study or test. Therefore, we manipulated the presentation modality of words during the study and test phases to allow for comparisons between a visual and an auditory recognition test. This experiment provided a stronger test of whether fewer errors would be made for modality-inconsistent conditions (different modality at study and test). Lower error rates for the modality-inconsistent conditions would indicate that the familiarity underlying recognition is the same as that underlying perceptual priming. The experiment provided another test of the visual discrimination advantage hypothesis (Smith & Hunt, 1998). Under that hypothesis, fewer errors would be expected for feature and conjunction lures whose components were presented visually during the study phase.

Method

Participants. Sixty-four undergraduates participated.

Design and procedure. A 2 (Study modality: aural vs visual) × 2 (Test modality: aural vs visual) × 4 (Item type: old, conjunction, feature, and new word) mixed design was used. Study modality and Item type were manipulated within participants, but Test modality was manipulated between participants. Thus the study-to-test conditions for the visual test group were visual-to-visual and auditory-to-visual, and the study-to-test conditions for the auditory test group were auditory-to-auditory and visual-to-auditory. Thirty-two participants were randomly assigned to each test group.

Two lists of 10 critical words (20 items per list) were assigned to each of the four conditions (old, conjunction, feature, and new). For the old, conjunction, and feature conditions, half of the corresponding study words were presented visually, while half of the words were presented aurally (one list of 10 words for each item type). A single study order based on item type (80 trials: 20 target words, 20 feature word primes, and 40 conjunction word primes) was constructed so that no more than 2 trials of a given item type and no more than 4 trials of a given modality occurred in a row. The computer program called words from the appropriate lists (by participant) into the appropriate "slots" of the study list. For counterbalancing purposes, the two study word primes for the feature word condition were presented equally often in the study phase (across participants) and for the conjunction condition the presentation order of the two study primes was balanced (across participants). Also, the study primes for feature and conjunction words were balanced for the presentation modality at study and test. These counterbalances were accomplished across participants, resulting in 32 study list variations. Spacing between the study word primes for the conjunction condition ranged from 1 to 5 intervening words with a mean of 3 words. The same primacy and recency buffers from Experiment 1 were used before and after the critical study trials.

The study and test instructions, practice test, and critical recognition test for the visual test group were the same as those in Experiment 1. For the auditory test group, the timing parameters were the same as the visual test group, but the test words were presented aurally rather than visually.

Results and Discussion

The time-out rate was negligible (.01), and the mean proportions of "old" responses for the study word conditions are presented in Table 2. As expected, the rate of responding "old" decreased across item types in the following order: old word (.69), conjunction lure (.48), feature lure (.36), and new word (.26). However, the study and test modality manipulations appeared to have little influence on the probability of "old" judgments. More importantly, the study and test modality factors did not appear to produce an interaction. A 2 (Test group) × 4 (Item type) mixed ANOVA was conducted to establish the differences between item types. Only the effect of Item type was significant [$F(3,186) = 158.99$, $MS_e = .01$], and a follow-up Tukey test showed that each of the item type means was significantly different from the

[3] Two participants with alexia (no agraphia) in Kroll et al.'s (1996) Experiment 1 were given visual word presentations accompanied by aural presentations.

TABLE 2

Mean Proportion of "Old" Responses by Experiment, Test Procedure, Study-Test Modality, and Item Type

Exp.	Deadline procedure	Study-test modality	Item type: Old	Conj.	Feature	New
2	Long	A-A	.70	.45	.32	.24
2	Long	V-A	.65	.47	.34	.24
2	Long	V-V	.72	.48	.38	.28
2	Long	A-V	.70	.50	.40	.28
3	Short	V-V	.65	.50	.42	.36
3	Short	A-V	.59	.51	.44	.36
4	Long	V-V	.69	.40	.33	.27
4	Long	A-V	.70	.48	.38	.31
4	Short	V-V	.61	.49	.38	.30
4	Short	A-V	.72	.57	.47	.40

Note. Exp., Experiment; Conj., Conjunction; A, Auditory; V, Visual.

others. There was no significant difference between the false alarm rates to new items on the two tests [auditory vs visual; $t(62) = .92$, $SE = .04$]. Therefore, to test the main effect of Study modality and the Study × Test modality interaction, a 2 (Study modality) × 2 (Test modality) × 3 (Item type: old, conjunction, and feature) mixed ANOVA was conducted on the hit and false alarm rates without any correction for baseline false alarms. (Analyses conducted on the corrected hit and error rates gave the same outcome.) None of the analyses involving study or test modality was significant (all Fs $<$ 1.59). Feature and conjunction errors occurred for both auditory and visual tests, with no modality difference in error rates between the two tests, and cross-modal error rates were equivalent to modality-consistent error rates. Finally, although modality-consistent old words were better recognized than modality-inconsistent old words, the differences were small, and the Modality × Item type interaction was not significant.

The absence of modality-shift effects supports the idea that the familiarity underlying recognition is different than the familiarity underlying perceptual priming. However, the lack of a modality effect on feature and conjunction errors lends no support to the visual discrimination advantage hypothesis. Also, the lack of a modality effect suggests that, if conjunction errors are due to a either a disinhibition of binding or a faulty retrieval process (e.g., conjoining features during retrieval), modality does not play a role in either explanation. Neither the disinhibition of binding nor the availability hypothesis accounts for the feature effects in the experiment, whereas the familiarity/recollection distinction does account for these effects.

EXPERIMENT 3

We have proposed that feature and conjunction errors are due to the influence of familiarity in the absence of recollection but that correct recognition of old words is based on the independent contributions of familiarity and recollection working in concert. There were no modality effects on feature and conjunction errors in Experiments 1 and 2, demonstrating that, at least with those procedures, the familiarity underlying feature and conjunction errors is not sensitive to shifts in modality. Neither experiment, though, produced changes in familiarity or recollection.

There is the possibility that the influences of both recollection and familiarity are greater in modality-consistent conditions but that the two processes offset each other. That is, greater familiarity in the modality-consistent condition could increase the probability of feature and conjunction errors, but greater recollection in the modality consistent conditions could decrease the probability of feature and conjunction errors, resulting in a null effect. The consequence of offsetting effects from recollection and familiarity is that one may fail to detect increases in recollection and familiarity (Jacoby, Jones, & Dolan, 1998). However, this problem can be overcome by manipulating a variable known to reduce recollection but not familiarity. Therefore, we conducted an experiment designed to attenuate the influence of recollection but leave the influence of familiarity unchanged.

Within the recollection/familiarity framework, several experiments have used a response deadline manipulation to decrease the amount of time that participants have to rely on recollection, the slower of the two processes (e.g., Atkinson & Juola, 1974; Jacoby, 1999; Jacoby et al., 1998; McElree, Dolan, & Jacoby, 1999). Recently, this type of procedure has allowed the identification of separate, and sometimes offsetting, effects on recollection and familiarity (Jacoby, 1999; Jacoby et al., 1998). For example, in series of experiments by Jacoby and colleagues (1998) study repetition increased both familiarity and recollection. In one experiment the separate effects of study repetition on recollection and familiarity offset each other, resulting in a null result. In a following experiment, an increase in familiarity was observed by reducing recollection through the use of a response deadline. Pushing participants to respond quickly to recognition test probes (within 750 ms) revealed an increase in familiarity for study words presented three times compared to one time. Participants made more exclusion errors to the repeated study words. In contrast, when participants waited to respond to recognition probes (wait 1250 ms with a 750-ms response window) fewer exclusion errors were made to repeated than to nonrepeated stimuli. In the wait condition, the increase in familiarity for repeated stimuli, evidenced by the increase in exclusion errors for the deadline condition, was countered successfully by an increase in recollection.

In the present experiment, we used the same study conditions and procedure of the visual test group in Experiment 2 but decreased the amount of time allotted to respond during the test. Relative to the results for the visual test group in Experiment 2, the hit rate (or old/new discrimination) for the deadline group was expected to be reduced. However, the critical interest centered on the feature and conjunction error rates. If both recollection and familiarity are greater for modality-consistent conditions but the two processes are in opposition, a decrease in recollection could reveal a modality shift effect on familiarity.

Method

Participants. Thirty-two students participated.

Design and procedure. The experiment employed a 2 (Study modality: visual vs aural) × 4 (Item type: old, feature, conjunction, and new word) repeated-measures design.

The study phase was the same as that in Experiment 2. After the study phase participants received instructions for a speeded yes/no, visual recognition test. Once again, there was no mention of the feature and conjunction lures. As a part of each recognition trial, a "get ready" signal (+++++++) was presented for 500 ms, followed by a test word for 850 ms in the center of the computer screen. Participants were told to try hard to respond within the 850 ms by pressing one of two keys (labeled "Y" or "N"). If a response was not made within 850 ms a late signal was displayed, and no response was collected. The late signal/postresponse delay (750 ms) and intertrial interval (1000 ms) were the same as those in the prior experiments, and the same 18-word practice recognition test was administered before the critical recognition test.

Results and Discussion

The proportion of "old" responses for each of the item types, broken down by study modality, may be seen in Table 2. Time-outs accounted for only a small proportion of trials (.04) and did not vary much across study modality. There was only a slight difference in the time-out rate across the item types (.03 for old words, .04 for conjunction words, .05 for feature words, and .05 for new words). As expected, old words were correctly identified (.62) at a higher rate than those for which conjunction, feature, and new words were misidentified (.50, .43, and .36, respectively). Thus, despite the short response deadline, old/new discrimination was well above chance. In addition, both conjunction and feature words were judged “old” at rates greater than that for new words. A repeated-measures ANOVA on the four item types showed that at least two of the item type means were significantly different [$F(3,93) = 28.$, $MS_e = .01$], and a follow-up Tukey test revealed that each of the pairwise comparisons was significant.

Of greater interest were the data based on modality. Overall, study modality did not influence the proportion of “old” responses given for the three studied conditions (old, conjunction, and feature words). There was a small difference in the hit rates for the two study modality conditions, with visually presented words receiving a higher proportion of “old” responses than auditorially presented words. A 2 (Study Modality) $\times$ 3 (Item type: old, conjunction, or feature) repeated-measures ANOVA was conducted to evaluate whether the study modality influenced the proportion of “old” responses across item types, but neither the main effect of Modality nor the Modality $\times$ Item type interaction was significant [$F(1,31) = .29$, $MS_e = .03$; and $F(2,62) = 1.12$, $MS_e = .03$, respectively].

As in Experiment 2, changing the presentation modality from study to test did not affect feature and conjunction error rates. (Although changing the study-test modality decreased the hit rate, the difference was not significant, $t(31) = 1.50$, $SE = .04$, $p = .07$, one-tailed.) The hit rate was lower and false alarm rate for new words was higher than in Experiment 2. Thus, recollection was reduced. Despite lower old/new discrimination a shift in modality did not produce any reliable effects across the different study conditions. This result does not support the idea that modality-consistent errors are based on both greater familiarity and recollection than modality-inconsistent conditions.

EXPERIMENT 4

To this point we have maintained that recollection and familiarity oppose each other in feature and conjunction conditions. However, another possibility is that both familiarity and recollection may facilitate feature and conjunction errors. In that point of view feature and conjunction conditions represent “in concert” conditions rather than opposition conditions.

While a response deadline was used in Experiment 3, response deadline was not manipulated directly. Thus, no conclusion from Experiment 3 can be drawn on whether recollection plays a facilitative role in feature and conjunction errors. In this experiment, response deadline was manipulated to test this idea. The response deadline manipulation also provided a stronger test of the disinhibition of binding hypothesis. If conjunction lures share the same representational attributes as old words (e.g., both based on feature representations and a configurative representation), then manipulations that influence old word recognition should influence conjunction errors in the same manner. We predicted that a short response deadline would decrease the hit rate. Therefore, the disinhibition of binding hypothesis predicts that a response deadline also should decrease the conjunction error rate.

The design of Experiment 4 was similar to that of Experiments 2 or 3, but response deadline was manipulated directly as a within-participant variable. On half of the test trials participants were pushed to respond quickly (short deadline condition). On the other half of the test trials participants were forced to wait a short period of time before responding (long deadline condition). In order to accomplish this within-participant design, a change was made to the test procedure. A response signal was used for all trials and the onset of that signal was varied. For the short deadline trials the stimulus onset asynchrony was 400 ms. For long deadline trials the stimulus onset asynchrony was 1800 ms. This amount of time was slightly greater than in previous experiments (1500 ms) in order to provide more time for recollection. The response signal was on the screen for 450 ms and, if necessary, a late signal appeared for 400 ms after the response signal. A response was allowed during the late signal to lower the timeout rate from Experiment 3.

A final difference in procedure for this experiment changed the modality manipulation to a between-participants manipulation. This change allowed more responses in the deadline conditions to be collected for each participant. Perhaps more important, the change conformed to the design of Smith and Hunt's (1998) false memory experiment. In their experiment, study words were presented in only one modality. In our earlier three experiments, the study phase included both visual and auditory presentations. To provide a better test of the visual discrimination advantage hypothesis, we wanted to test the hypothesis under conditions that resembled their design.

Method

Participants. Sixty-four undergraduates participated.

Design and procedure. The experiment used a 2 (Study modality: auditory vs visual) × 2 (Response deadline) × 4 (Item type: old, conjunction, feature, and new) design. Study modality was a between-participants factor, but Response deadline and Item type were within-participant factors. Thirty-two students were assigned randomly to two groups (auditory study or visual study).

The same eight lists from Experiments 2 and 3 were used. Four lists were used for the short deadline condition, and the other four lists were used for the long deadline condition. Each of the 4 lists in each response condition was assigned to one of the four item types: old, feature, conjunction, or new. Each list served in each condition as each item type equally often across participants. As in the previous experiments, the "old" component of feature lures (e.g., first or second feature) and the order of the conjunction study word primes were balanced across participants.

The procedure for the study phase was the same as in Experiments 2 and 3. For all test trials, participants were to respond at the appropriate signal (*****). For the short deadline trials, a test probe was seen for 400 ms before the onset of the response signal. The response signal duration was 450 ms, and participants were instructed to try to respond while the response signal was present. After 850 ms from the onset of a test word, the response signal was replaced by a late signal (!!!!!). The participants were told that if the late signal appeared, they had waited too long to respond. If the late signal appeared, a response was required immediately so that a response could be collected. The late signal was displayed for 400 ms. This change in procedure was designed to reduce very fast responding (e.g., below 500 ms) and decrease the number of time-outs (by allowing a response during the late signal). If a participant did not enter a response while either the response signal or the late signal was present (a time-out), the program continued to the next test trial. For the long deadline trials, the delay period was increased from 1500 ms to 1800 ms to allow more time for participants to use recollection as a basis for responding. The durations of the response and late signals were the same in the short and long deadline trials.

TABLE 3

Mean Corrected Scores for Experiment 4 by Test Procedure, Study-Test Modality, and Item Type

Deadline procedure	Study-test modality	Old	Conj.	Feature
Long	V-V	.42	.13	.06
Long	A-V	.39	.17	.07
Short	V-V	.31	.19	.08
Short	A-V	.32	.17	.07

Note. Conj., Conjunction; A, Auditory; V, Visual.

Results and Discussion

The mean proportion of "old" responses for each condition are displayed in Table 2, and the mean proportions of "old" responses for the "studied" conditions are shown in Table 3, corrected for baseline differences. (The mean time-out rate was negligible, .01). As in the previous experiments, the same pattern of "old" response rates emerged: old > conjunction > feature > new. This pattern was consistent across deadline and modality conditions. The only apparent effect of modality was an increased likelihood of responding "old" for the auditory study group, particularly under the short deadline condition. The response deadline appeared to affect old/new discrimination but not conjunction/new or feature/new discrimination. A 2 (Study modality: auditory or visual) × 2 (Response deadline: deadline vs wait) × 4 (Item type: old, conjunction, feature, or new) mixed ANOVA produced significant effects of Group [$F(1,62) = 7.78$, $MS_e = .08$], Deadline [$F(1,62) = 11.33$, $MS_e = .03$], and Item type [$F(3,186) = 156.50$, $MS_e = .02$] and a significant Deadline × Item type interaction [$F(3,186) = 5.15$, $MS_e = .02$]. A follow-up Tukey test demonstrated that the differences between each of the item types were significant. A 2 (Modality) × 2 (Response deadline) mixed ANOVA on the baseline rates showed that the auditory study group had a higher baseline false alarm rate than the visual study group [$F(1,62) = 4.17$, $MS_e = .03$] and that the short-deadline condition produced a higher baseline false alarm rate than the long-deadline condition [$F(1,62) = 6.16$, $MS_e = .02$]. The interaction was not significant ($F = 1.07$).

To accommodate the different base rates, the "old" response rates for the old, conjunction, and feature item types were corrected by subtracting out the corresponding base rate. These data appear in Table 3 and show more obvious patterns that the uncorrected data. First, study modality did not influence the likelihood of an "old" response. Second, the deadline manipulation affected recognition of old words but not false recognition of conjunction and feature lures. A 2 (Modality) × 2 (Response deadline) × 3 (Item type) mixed ANOVA on the corrected scores (see Table 3) produced a significant effect of Item type and a significant Deadline × Item type interaction [$F(2,124) = 7.64$, $MS_e = .02$]. The interaction was predicted to occur because of differences in the hit rates but not false alarms rates. The interaction was pursued further with two ANOVAs, a repeated-measures ANOVA on the corrected hit rates (collapsed across study modality) and a 2 (Response deadline) × 2 (Item type: Conjunction vs Feature) repeated-measures ANOVA on the corrected false alarm rates. The effect of Response deadline was significant for the analysis on the hit rates [$F(1,63) = 9.80$, $MS_e = .03$] but not the false alarm rates [$F = .29$]. Thus, the response deadline manipulation affected recollection, demonstrated by a change in old/new discrimination, but not familiarity, characterized by no change in conjunction/new or feature/new discrimination. This outcome supports the hypothesis that recollection facilitates recognition of old words but not false recognition of feature and conjunction lures. As such, the results are contrary to the idea that both familiarity and recollection facilitate feature and conjunction errors.

These findings are not consistent with a finding that obtained Remember/Know judgments (e.g., Gardiner, 1988; Gardiner & Java, 1990; Gardiner & Parkin, 1990; Tulving, 1985) on feature and conjunction errors for drawings of faces (Reinitz et al., 1994). Reinitz and his colleagues found that endorsements of feature and conjunction stimuli as "old" were accompanied by Know judgments as well as Remember judgments. If one accepts that Remember judgments in their experiments provided a measure of recollection, then one would conclude that feature and conjunction errors are facilitated by recollection and familiarity. However, in our experiment a variable known to affect recollection (response deadline) did not influence feature and conjunction error effects, which indicates that recollection did not facilitate feature and conjunction errors.

There are a couple of explanations for the discrepancy between our findings and the Remember/Know findings of Reinitz and colleagues (1994). An intriguing possibility is that familiarity can lead one to erroneously experience a subjective awareness consistent with Remember judgments. This type of familiarity-based phenomenological experience would suggest that the relationship between that subjective experience and the underlying processes of recollection and familiarity is one of independence. Currently, there is no work supporting this view. A second, and perhaps more plausible, possibility is that participants in Reinitz and colleagues' experiment reported a Remember response based on a particular facial feature rather than on all of the features and the relational information connecting those features (cf. Mantyla, 1997). Such responses would highlight the need to define recollection, as well as Remember judgements, carefully. Recollection, as we have considered it, is an all-or-none retrieval success at the item level. In this view, one may Know that some aspect of an event was experienced but falsely attribute that aspect to a different episode. A final possibility for the discrepancy between our results and the results of Reinitz and colleagues may be the differences in materials. Reinitz et al. used face drawings, whereas we used compound words. Although feature and conjunction errors occur for both word and face stimuli, the processes involved for the two types of stimuli may be fundamentally different.

The results of this experiment also provide no support for the disinhibition of binding hypothesis. Under that hypothesis, old words and conjunction lures share the same characteristics (e.g., both are based on feature representations and a configurative representation). The difference, of course, is that configurative representations for the old words are accurate, whereas the configurative representations for the conjunction lures are inaccurate. A manipulation that influences recognition of old words should affect false recognition of conjunction lures in the same manner. The prediction of the disinhibition of binding hypothesis is that a response deadline should decrease the rates of both hits and conjunction errors. However, the response deadline manipulation decreased old/new discrimination without affecting conjunction/new discrimination.

Finally, the results highlight a shortcoming of the availability hypothesis proposed by Reinitz and colleagues (1992, 1996). In their proposal, conjunction errors occur because "subjects selectively forget, or fail to encode, global structures [configurative representations] of the stimuli that were originally studied" (p. 287; Reinitz et al., 1996). Old word recognition is based on features and configurations, but false recognition of conjunction lures is based on features only. In our experiment, a retrieval manipulation affected old/new discrimination but not conjunction effects (or feature effects). An explanation of this outcome requires one to comment on the accessibility of configurative representations. For the short deadline condition, configurative representations were presumably available but not accessible in the short amount of time that was given. This demonstration suggests that configurative representations for the conjunction study primes might be available but not accessible. As we already have indicated, recollection for the study word primes may be low because the feature and conjunction lures are poor retrieval cues for the prime words presented in the earlier study phase. Therefore, while configurative representations may not have been encoded or selectively forgotten, configurative representations also may have been formed and retained but not be inaccessible under the given retrieval conditions.

GENERAL DISCUSSION

The conclusions to be drawn from the experiments are straightforward. Across four experiments, the pattern of results for "old" responses (old > conjunction > feature > new) was consistent with previous findings on feature and conjunction errors (Kroll et al., 1996; Reinitz et al., 1992, 1994, 1996; Rubin et al., 1999). More important, though, were the following findings. First, feature and conjunction errors occurred for auditory as well as visual tests, and the error rates on the two types of tests did not differ. Second, at least under our conditions, feature and conjunction error rates were not affected by shifts in presentation modality from study to test. In addition, mixing the modality of the study word primes did not affect conjunction error rates relative to when the study primes were presented in the same modality. Finally, a response deadline manipulation decreased old/new discrimination but not feature and conjunction effects. The response deadline manipulation also did not affect recognition of modality-consistent and modality-inconsistent old words differentially.

Across the first three experiments, three groups showed a nonsignificant old word recognition advantage for a modality-consistent over a modality-inconsistent condition, and one group showed no difference. In Experiment 4, the modality consistent group (visual study group) held a slight old word recognition advantage over the modality-inconsistent group (auditory study group) for the long deadline condition but a slight disadvantage for the short-deadline condition. In an effort to gain more power, the data for old word conditions, as well as those for overlapping conjunction conditions, were combined from the four experiments. These scores were adjusted for baseline differences (i.e., the base rate was subtracted). Collapsing across experiments there was a very small advantage for modality-consistent old words (.39) over modality-inconsistent old words (.37). This trend was reversed for the conjunction condition. Slightly more errors were committed under modality-inconsistent (.19) than modality-consistent conditions (.18). The feature condition (Experiments 2–4) showed the same trend as the conjunction condition (modality consistent: .08 and modality inconsistent: .09). Formal analyses on those data failed to produce any significant modality effects. Thus, overall the differences between modality-consistent and modality-inconsistent conditions were tiny and not significant.

We have proposed that feature and conjunction errors are based on familiarity in the absence of recollection. Importantly, recollection involved in old word recognition is different than recollection involved in the feature and conjunction conditions. For the old word condition, the test cue is the exact word that was presented earlier in the experiment—a recognition task. In contrast, for the feature and conjunction conditions the study word prime(s) is the focus of recollection (e.g., *blackmail* and *jailbird*), and the feature and conjunction test lures (e.g., *blackbird*) or their separate lexical components (e.g., *black* and *bird*) can serve as retrieval cues for a study prime. Thus recollection on the feature and conjunction trials is in the form of cued recall. However, the retrieval cues are relatively ineffective. If recollection for the study prime words were strong, then a short response deadline, which reduces the ability to use recollection, should have produced an increase in feature and conjunction errors. No such increase in error rates occurred. Because the retrieval cues in the feature and conjunction conditions are relatively ineffective, recollection for the study word primes is near zero. Therefore, the familiarity evoked by the components of the feature and conjunction lures is virtually unopposed by recollection. (However, there may be conditions where feature and conjunction lures are avoided successfully through the use of recollection.)

The retrieval cues may be weak for two reasons. First, there is little semantic overlap between many of the study primes (e.g., *buckwheat* and *slapshot*) and their corresponding feature or conjunction lure (e.g., *buckshot*). Second, the usefulness of the separate lexical components (e.g., *black* and *bird*) as retrieval cues may be hindered by appearing in a different context. In other words, it may be difficult to separate the lexical components (*black* and *bird*) from a unitized concept (*blackbird*) for use as retrieval cues. Finally, in our experiments participants were not informed of the feature and conjunction lures. Thus, the feature and conjunction lures might have to initiate retrieval of the study word primes spontaneously. On the other hand, in the debriefing many participants indicated awareness that some test words were similar but different from study words, and some participants were adamant that they had not fallen for the lures. (No tallies were taken. However, some of these individuals who claimed that they did not fall for the lures were the worst offenders.) Thus, anecdotal evidence suggests that despite attempts to avoid committing feature and conjunction errors, participants were largely unable to do so (see also Kroll et al., 1996; Reinitz et al., 1992).

In the disinhibition of binding hypothesis, conjunction errors are based on inaccurate representations formed during encoding (Kroll et al., 1996).[4] In Experiment 1, mixing the study modality of the conjunction word primes did not influence the likelihood of conjunction errors relative to when the study modality was

[4] To be fair to Kroll and colleagues (1996), their idea was that a disinhibition of binding contributes to conjunction errors but is not the main factor. In their view, the absence of configurative representation is the major factor (e.g., the availability hypothesis).

held constant for the word primes. Therefore, an inappropriate binding of conjunction components during the study phase does not seem to be affected by changes in the modality. However, if conjunction errors are due to an inappropriate binding of stimulus elements during encoding, then the representations of inappropriately bound components (conjunctions) should have the same attributes as representations for properly bound components (old words). Therefore, if a retrieval manipulation affects the probability of recognizing old words (e.g., old/new discrimination), then that same manipulation should affect the probability of conjunction errors in the same manner (e.g., conjunction/new discrimination).

Experiment 4 provided a test of the disinhibition of binding hypothesis. The retrieval manipulation attenuated old/new word discrimination but did not influence the conjunction effect. If conjunction errors are due to a disinhibition of binding, then one would have expected a parallel decrease in the conjunction error rate. Thus, the experiment failed to provide any evidence in support of the disinhibition of binding hypothesis, at least with the range of study prime lags (one to five intervening words) that were used. Instead, the response deadline results supported a dual-process explanation of feature and conjunction error production. However, the disinhibition of binding hypothesis may still account for conjunction error differences between individuals with hippocampal damage and normal participants, and disinhibition of binding may occur in normals when the lag between study primes is very short (e.g., zero).

The results of Experiment 4 also suggest that a modification to the availability hypothesis of conjunction error production is needed. Instead of being unavailable, configurative representations may be available but not accessible (e.g., Tulving & Pearlstone, 1966). Of course, this modification would not solve the problem of feature errors. The feature/configurative approach would still need to be modified to account for the occurrence of feature error above chance.

The four experiments tested the modality discrimination advantage that was suggested by Smith and Hunt (1998) to account for modality differences in the false recognition of critical lures in the Deese/Roediger and McDermott (1959/1995) paradigm. They found very large differences in false recognition based on the study modality. Visual study led to a much lower false recognition rate than auditory study. However, in the present experiments no such advantage occurred for the feature and conjunction lures. Thus, at least under our conditions, the modality discrimination explanation does not extend to false recognition of feature and conjunction lures. Of course, in the Deese/Roediger and McDermott (1959/1995) paradigm no physical part of the critical stimuli is presented during the study phase, and the critical lures are strong conceptual associates of the study words. In feature and conjunction error experiments, parts of the critical lures are presented during the study phase and the lures are not necessarily strong conceptual associates of the study primes.

Turning to the recognition of old words, main effects of modality on recognition memory for old words often are not obtained, and several recent experiments have failed to yield significant differences (Challis et al., 1993; Gregg & Gardiner, 1994; Rajaram, 1993; though see Gregg & Gardiner's data on Know judgments). Where differences between study/test modality-consistent and modality-inconsistent conditions have been obtained, the differences have been very small, though usually in favor of the modality-consistent condition. For example, Kirsner (1974) obtained modality effects on recognition memory (despite ceiling effects) but had a great deal of power to detect a significant difference. The differences in accuracy that Kirsner obtained were small ($M = .02$), but there were 90 (item) $\times$ 20 (subject) observations (1800 total) for each of four study/test modality conditions (aural–aural, aural–visual, visual–aural, and visual–visual). Our results add to this growing list but offer a novel null result. In our experiments, familiarity was isolated for the feature and conjunction test trials. If a perceptually specific familiarity contributes to false recognition of feature and conjunction lures, then one would expect that this same familiarity would contribute to the recognition of old words. However, no evidence was gained in support of a perceptually specific familiarity using our modality manipulations.

Some researchers have drawn parallels between the familiarity underlying recognition and perceptual priming (e.g., Gardiner & Parkin, 1990; Jacoby & Dallas, 1981; Rajaram, 1993), but others have concluded that recognition memory is independent of priming in perceptual implicit tests (Challis et al., 1993; Hayman & Tulving, 1989; Squire, Shimamura, & Graf, 1985; Tulving, Schacter, & Stark, 1982; Wagner, Gabrieli, & Verfaellie, 1996). More specifically, Wagner and colleagues (1996) obtained dissociations between familiarity from recognition and familiarity from perceptual priming. Finally, neuropsychological and neuroimaging data have implicated the role of different brain structures for explicit and implicit tests (Gabrieli, Fleischman, Keane, Reminger, & Morrell, 1995; Reinitz et al., 1996; Squire, 1992; Squire et al., 1985; Tulving & Schacter, 1990). For example, Gabrieli and colleagues reported that the right occipital region of the brain is critical to perceptual priming for degraded stimuli. In contrast, other areas of the brain (e.g., medial temporal or diencephalic regions) are thought to be important for explicit memory tasks such as recall, cued recall, and recognition.

The present results failed to obtain effects from perceptual manipulations on feature and conjunction errors (or hits). Thus, our experiments failed to obtain modality effects on the familiarity underlying feature and conjunction errors, which are arguably relatively pure measures of familiarity. The lack of modality effects on these recognition errors contrasts sharply with results from perceptual implicit tests in which robust modality effects have been obtained (e.g., Blaxton, 1989; Challis et al., 1993; Graf et al., 1985; Jacoby & Dallas, 1981; Kirsner, Milech, & Standen, 1983; Roediger & Blaxton, 1987; also see Jacoby, 1996). The data from the present experiments support the argument that the familiarity underlying recognition memory is different from that which underlies priming on perceptual implicit tests. However, there may be instances, unrealized at this time, in which familiarity underlying recognition may be shared with perceptual implicit tests. For example, encoding conditions that emphasize the perceptual features of stimuli might show a similar effect for a recognition test and a perceptual implicit test.

In conclusion, although our experiments did not identify any specific nonsemantic factor involved in the production of feature and conjunction errors, the results have narrowed the search. At the risk of accepting the null hypothesis, modality information simply does not appear to contribute significantly to feature and conjunction errors with compound words. (This is not to say that modality information is not accessible and might not be important on some other task.) The question remains: What specific nonsemantic familiarity processes underlie feature and conjunction errors? For verbal stimuli, feature and conjunction errors are produced by varying the arrangements of lexical or syllabic components. Perhaps a portion of the familiarity underlying conjunction errors on compound words may be based on lexical processing (e.g., Kirsner, Dunn, & Standen, 1989; Weldon, 1991, 1993) that may be both nonsemantic and free of modality information. Certainly, this notion is speculation. However, more research needs to be conducted to identify the factors underlying feature and conjunction errors, and this speculation may provide some direction for that research.

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