A Single-System Signal-Detection Theory of Repetition Priming and Recognition Memory

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Declaration

I declare that the work presented in this thesis is the result of my own work.

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Abstract

A dominant view in current memory research is that there are distinct implicit (unconscious) and explicit (conscious) memory systems. The present thesis proposes an alternative, single-system signal-detection model of priming (a traditional implicit memory phenomenon) and recognition (a traditional explicit memory phenomenon). The model has two core assumptions: 1) priming and recognition are driven by the same memory strength signal, and 2) this signal is subjected to independent sources of random noise for priming and recognition tasks (the variance of which is typically greater for priming tasks). The model is shown to account for numerous results: 1) the sensitivity of priming tasks does not typically exceed that of recognition tasks, and priming therefore does not occur when recognition is at chance (Experiments 1–8); 2) the magnitude of the effect produced by manipulations of attention at encoding is greater on recognition than priming (Experiments 5–8), and this can give rise to single dissociations (Appendix 1); 3) priming and recognition can be very weakly correlated, even though they are driven by the same memory signal; 4) priming can occur for items that are not recognised (Experiment 9; Simulation Study 2); 5) the relationship between the identification latencies to misses and false alarms can change as a function of overall memory strength (Simulation Study 3); 6) priming and fluency are relatively intact in amnesics, despite severe impairments in recognition (Simulation Study 4). Thus, contrary to previous interpretation, (2)–(6) are not inconsistent with a single-system view; (1) suggests that the contents of the memory driving priming are accessible to consciousness. Finally, the predictions of the model were tested in a novel paradigm, the CID-2AFC task (Experiments 10–12; Simulation Study 5). Limitations of the model (and a dual-system version) were revealed, suggesting directions for future research.

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Chapter 1: Introduction

Memory can manifest itself in a variety of ways. In a seminal review article, Schacter (1987) drew attention to the distinction between explicit and implicit memory. Explicit memory is generally described as conscious recollection of previous experiences, whereas implicit memory is unconscious, and is revealed when previous experience facilitates or alters performance in a task, but in the absence of any conscious recollection of these experiences. Determining the relationship between these types of memory is important for our overall understanding of memory, and in recent years a lot of research has been devoted to this aim. Explicit memory is typically measured with traditional memory tasks such as recognition and free recall. Numerous phenomena have been proposed to reveal the influence of implicit memory (see Schacter, 1987), but perhaps the most intensively researched of all of these is repetition priming.

Repetition priming (henceforth, priming) refers to a change in identification, detection or production of an item (e.g., a word) as a result of prior exposure to the same or a similar item. Many different tasks are used to measure priming (see Roediger & McDermott, 1993). For example, in a perceptual identification task items are presented extremely briefly, making them difficult to identify. Priming is shown in this task if a greater proportion of old (studied) words are identified than new (non-studied) words (e.g., Jacoby & Dallas, 1981; Neisser, 1954). In a perceptual clarification task, old and new words gradually clarify into view and priming is shown if identification reaction times are shorter for old items than new items (e.g., Feustel, Shiffrin, & Salasoo, 1983; Johnston, Dark, & Jacoby, 1985; Stark & McClelland, 2000). Other widely used tasks are word-stem completion (Graf, Squire, & Mandler, 1984) and word-fragment completion (Tulving, Schacter, & Stark, 1982).

Priming is frequently compared to recognition, which refers to the capacity to judge whether an item has been previously presented in a particular context. Recognition tasks lend themselves for comparisons with priming because, like many priming tasks, a single item may be presented on each trial. In a typical recognition experiment, a participant studies a list of items and then later, in a test phase, is presented with old and new items and instructed to judge whether each item was presented in the study phase. Thus, the instructions of recognition tasks refer to the study episode whereas the instructions of priming tasks typically do not. As such, recognition tasks are often classified as direct tests of memory and priming tasks as indirect tests (Johnson & Hasher, 1987; Richardson-Klavehn & Bjork, 1988).

1.1 Multiple-Systems Theory

An influential and largely dominant view is that priming is mediated by an implicit memory system whereas recognition is mediated by a distinct explicit memory memory system (Gabrieli, 1998, 1999; Gabrieli, Fleichman, Keane, Reminger, & Morrell, 1995; Schacter, 1987; Schacter & Tulving, 1994; Tulving & Schacter, 1990; Tulving et al., 1982; Wagner & Gabrieli, 1998). The terms non-declarative and declarative have been proposed instead of implicit and explicit by Squire and colleagues (Squire, 1994, 2004; Squire & Knowlton, 2000; Squire, Knowlton, & Musen, 1993). The systems are thought to be distinct in the sense that they operate according to different principles and within different regions of the brain.

At this point it is important to distinguish between at least three different uses of the terms ‘implicit’ and ‘explicit’. The first refers to the task instructions (e.g., as used by Roediger & McDermott, 1993). A task is sometimes described as explicit when the instructions orient the participant to the study episode whereas a task is described as implicit when the instructions make no reference to the study episode. In this case, the terms implicit and explicit are synonymous with the terms ‘indirect’ and ‘direct’, respectively. The second usage refers to whether conscious re-experiencing occurs during retrieval of an item. When this occurs, retrieval is said to be explicit and when it does not, retrieval is said to be implicit (e.g., Graf & Schacter, 1985; Schacter, 1987). The final use refers to the hypothetical memory source or store. In this sense, implicit memory is a source of memory that is not accessible to awareness whereas the contents of explicit memory are available to awareness (e.g., Squire, 1994, 2004). There is much overlap in the use of these terms; the present thesis uses the terms implicit and explicit to refer to hypothetical memory sources.

Many studies have shown that priming and recognition can be dissociated (for reviews see Richardson-Klavehn & Bjork, 1988; Roediger & McDermott, 1993) and these dissociations have been taken to support a multiple-systems view. For example, priming and recognition can be dissociated in normal adults and also in amnesics, priming can occur in the absence of recognition/awareness, and performance in priming and recognition tasks have been shown to be stochastically independent. Furthermore, neuroimaging evidence has shown that priming and recognition are associated with activity in different brain regions. I now review this evidence.

1.1.1 Functional Dissociations

Studies with normal individuals have shown that certain independent variables affect recognition but have smaller, often unreliable effects on priming. This supports a multiple-systems view because it suggests that the memory system supporting recognition can be selectively influenced or is at least affected differently to the memory system supporting priming. For example, recognition is much greater for items that are processed semantically at encoding (e.g., by answering questions about an item's meaning) than those processed non-semantically at encoding (e.g., by deciding whether the item contains a particular letter). This type of 'levels of processing' (Craik & Lockhart, 1972) manipulation is typically regarded as having no reliable benefit on priming (e.g., Jacoby & Dallas, 1981; see also the meta-analysis of four experiments in Richardson-Klavehn, Clarke, & Gardiner, 1999). Although other studies (e.g., McBride, Dosher, & Gage, 2001) and meta-analyses (Brown & Mitchell 1994; Challis & Brodbeck, 1992) have indicated that priming is affected by this type of manipulation, it remains the case that the effect is clearly greater on recognition.

Manipulations of attention at encoding have also been shown to dissociate priming and recognition in a similar manner. If participants perform a concurrent task while encoding study items (e.g., monitoring an auditorily presented string of digits for a target sequence), recognition is reduced but priming is unaffected or only slightly reduced (see Mulligan & Brown, 2003, for a review). This type of dissociation has been taken as evidence for the differential nature of implicit and explicit memory (e.g., Parkin, Reid, & Russo, 1990). Studies investigating the effects of attention at encoding are considered in greater detail in Chapter 4.

A similar dissociation is produced by the administration of benzodiazepines (e.g., midazolam) prior to the study episode (see Gohneim, 2004, for a review). This substantially impairs recognition and some studies have reported that priming is only sometimes affected (e.g., Polster, McCarthy, Sullivan, Gray, & Park, 1993), while other demonstrations of effects are more concrete (Hirshman, Passannante, & Henzler, 1999). For example, Hirshman et al. (1999) found that priming in perceptual identification and word-fragment completion tasks was reduced by almost one half following administration of midazolam compared to a control condition. Again, the size of the effect appears to be larger on recognition and as such, these dissociations are usually interpreted in terms of the differential effects of the drug on implicit and explicit forms of memory (e.g., Hirshman et al., 1999; Polster et al., 1993).

Variables have also been identified that produce the reverse dissociation: they produce greater effects on priming than recognition. These variables typically involve changing the physical form of an item between study and test. For example, priming is greatly reduced for items which are presented at test in a different modality to study, but recognition is less affected (e.g., Craik, Moscovitch, & McDowd, 1994; Jacoby & Dallas, 1981; Kirsner, Milech, & Standen, 1983). This has been taken as evidence of the highly specific nature of the (implicit) memory that supports priming, compared to recognition (Tulving & Schacter, 1990).

Finally, certain variables have been shown to have opposite effects on performance in each task (e.g., Jacoby, 1983; Richardson-Klavehn et al., 1999). For example, Jacoby (1983) presented words at test in a perceptual identification task and a recognition task. Priming was greater when the word was read at study compared to when it was generated (e.g., from its antonym), whereas recognition was greater for the generated words than read words. A similar pattern occurs when pictures are studied at encoding and are presented at test as words: There is little priming for words studied as pictures compared to priming for words studied as words (e.g., Weldon, 1991; Winnick & Daniel, 1970), but recognition of words studied as pictures is greater than recognition of words studied as words (Madigan, 1983).

Thus, priming and recognition can be functionally dissociated in normal adults in various ways. Variables have been identified which have 1) greater effects on recognition than priming, 2) greater effects on priming than recognition, and 3) opposite effects on priming and recognition performance. From the multiple-systems perspective, this supports the notion that the implicit and explicit memory systems supporting priming and recognition operate according to different principles and are therefore functionally distinct (e.g., Gabrieli, 1998; Wagner & Gabrieli, 1998).

1.1.2 Population Dissociations

Some of the most compelling evidence that priming and recognition are mediated by distinct memory systems comes from studies with amnesic individuals. The onset of amnesia in these individuals is typically associated with neurological damage to the hippocampus and medial temporal lobe regions. Despite impaired levels of recognition, amnesics show similar levels of repetition priming to controls (e.g., Cermak, Talbot, Chandler, & Wolborst, 1985; Graf et al., 1984; Warrington & Weiskrantz, 1970, 1974). There is also an extremely amnesic individual, E. P., who performs no better than chance in tests of recognition and yet shows relatively intact priming (e.g., Conroy, Hopkins, & Squire, 2005; Hamman & Squire, 1997a; Stark &

Squire, 2000; Stefanacci, Buffalo, Schmolock, & Squire, 2000). The dissociation in amnesia is often considered to represent the strongest evidence for the multiple-systems view because it suggests that the memory system supporting recognition can be selectively impaired, while also suggesting that the neural basis of the system is distinct to the system supporting priming. More specifically, it suggests that the hippocampal/medial temporal lobe regions are crucial for recognition but not priming.

Priming, on the other hand, is thought to depend upon regions of the neocortex (Squire, 1994, 2004; Gabrieli, 1998). Consistent with this view, visual word priming is impaired in certain individuals with damage to the right occipital lobes whereas recognition is relatively intact (Gabrieli et al., 1995; Keane, Gabrieli, Mapstone, Johnson, & Corkin, 1995). When considered together with the dissociation in amnesia, this constitutes a double dissociation and has been taken as evidence for the existence of a visual implicit memory system in the right occipital lobe which is crucial for priming but not recognition (Gabrieli et al., 1995).

1.1.3 Stochastic Dissociations

Stochastic independence between priming and recognition has traditionally been taken to support the multiple-systems view. This essentially refers to the finding that priming and recognition performance are often not correlated (Hayman & Tulving, 1989; Jacoby & Witherspoon, 1982; Schacter, Cooper, & Delaney, 1990; Stark & McClelland, 2000; Tulving et al., 1982; Tulving & Schacter, 1990; Tulving & Hayman, 1993). Stochastic independence has been considered evidence for the multiple-systems view because if priming and recognition depended upon a common memory representation, one might expect performance to be associated rather than independent: an item that can be recognized should be more likely also to show primed responding.

The use of this type of dissociation as evidence for multiple systems has been criticized on methodological and theoretical grounds (Hintzman, 1990; Hintzman & Hartry, 1990; Kinder & Shanks, 2003; Ostergaard 1992; Roediger, Buckner, & McDermott, 1999; see Poldrack, 1996 for a review). For example, the method relies on the demonstration of a null effect and Poldrack (1996) has shown that the statistical power needed to reasonably conclude that two measures are stochastically independent is not achieved by many studies purporting to demonstrate stochastic independence. Furthermore, Ostergaard (1992) has suggested that only a small proportion of the variance in the performance of priming tasks is due to the influence of memory, and that priming tasks may be affected by many more non-memorial influences than recognition, which is regarded to be a relatively pure measure of memory. As a result, low or near-zero correlations are to be expected even though the same memorial representation may drive priming and recognition. The issue is further complicated by demonstrations of stochastic independence between performance in word-fragment and perceptual identification priming tasks which are commonly assumed to be driven by the implicit memory system (Witherspoon & Moscovitch, 1989). Poldrack (1996) concluded that the use of stochastic dissociations as evidence for distinct systems is fraught with problems. It is therefore not so surprising that this dissociation is no longer as widely used to support the multiple-systems view (but see Tulving, 1999; Stark & McClelland, 2000). However, given its widespread usage in the past, a convincing theory of priming and recognition should still be able to account for such a finding (Raaijmakers, 2005). The issue of stochastic independence is revisited in Chapters 4 and 5.

1.1.4 Priming in the Absence of Recognition

Demonstrations of priming in the absence of recognition in normal adults constitute evidence for what is arguably one of the defining characteristics of implicit memory, that its contents are not accessible to awareness (Roediger & McDermott, 1993; Schacter, Bowers, & Booker, 1989; Squire, 1994; Stadler & Roediger, 1998). A number of studies have shown that priming can occur when recognition is not reliably above chance (e.g., Eich, 1984; Kunst-Wilson, & Zajonc, 1980; Merikle & Reingold, 1991). If the same (consciously accessible) source of memory drives priming and recognition, and if one assumes that recognition tasks are more sensitive to this source (because the instructions require participants to consciously refer back to the study episode), priming should not occur when recognition is also at chance (e.g., Shanks & St. John, 1994). Such a finding therefore strongly suggests that priming depends upon a different memory source to recognition. Key evidence for priming in the absence of recognition is examined more closely in Chapter 2.

Other studies have used tasks which measure priming and recognition concurrently (e.g., by interspersing recognition trials with perceptual clarification trials) and have shown that priming occurs even for items not overtly recognized (Stark & McClelland, 2000). This finding has also been taken to indicate the involvement of distinct sources of memory in priming and recognition. This finding is considered, together with other findings from this type of paradigm, in Chapters 5 and 6.

1.1.5 Neuroimaging

Priming and recognition are associated with different patterns of neural activity, consistent with the multiple-systems view that the implicit and explicit systems are neurally distinct (Tulving & Schacter, 1990). (For reviews of neuroimaging and priming see Henson, 2003; Schacter & Buckner, 1998.) Priming is associated with reduced haemodynamic responses in occipital, temporal, and pre-frontal regions (Schacter, Alpert, Savage, Rauch, & Albert, 1996; Henson, 2003; Schott, et al., 2005). Recognition and explicit memory, on the other hand, are associated with haemodynamic response increases in prefrontal, parietal, and medial temporal regions (Schacter et al., 1996; Henson, Rugg, Shallice, Josephs, & Dolan, 1999; Eldridge, Knowlton, Furmanski, Bookheimer, & Engel, 2000; Schott et al., 2005). ERP studies also indicate that priming and recognition related activity at retrieval is associated with distinct time-courses and typographies (Paller, Hutson, Miller, & Boehm, 2003; Rugg et al., 1998). These differences also extend to the study phase: Subsequent priming and explicit memory are associated with different patterns of haemodynamic responses at encoding (Schott et al., 2006) and also different electrophysiological responses (Schott, Richardson-Klavehn, Heinze, & Duzel, 2002).

1.1.6 Associations

Although a great amount of emphasis is often placed on the differential effects of variables on priming and recognition, there are some relatively clear cases of associations. For example, repeating items multiple times at encoding increases priming and recognition (Ostergaard, 1998). Recognition and priming are also greater for low frequency words than high frequency words (Bowers, 2000; Jacoby & Dallas, 1981;

Ostergaard, 1998). Both priming and recognition decrease over long retention intervals (e.g., Moscovitch & Bentin, 1993), although there is some dispute as to whether the rates of forgetting in priming and explicit memory tasks are different (McBride & Dosher, 1997; McBride, Dosher, & Gage, 2001; Schacter, 1987; Tulving et al., 1982). (For other associations see Richardson-Klavehn & Bjork, 1988, and also the Information Availability Model section below.)

Associations between priming and recognition might be taken to indicate that a common memory system mediates priming and recognition, but advocates of the multiple memory systems approach have suggested that associations merely show that the different memory systems share some similar properties, and not necessarily that the same system drives performance (e.g., Gabrieli, 1999; Tulving & Schacter, 1990).

Similarly, when a variable is found to affect priming and recognition (such as levels of processing manipulations), this seems to weigh against the notion that the explicit memory system is *selectively* influenced by the variable. However, when effects are also found on priming, some have argued that this is due to contamination of the measure of priming with explicit memory (e.g., Hamman & Squire, 1996).

1.2 Alternative Theoretical Frameworks

Alternative accounts of much of the dissociation evidence have been proposed. First, the use of dissociation evidence to support notions of distinct systems or processes has been criticised by many (e.g., Buchner & Wippich, 2000; Dunn, 2003; Dunn & Kirsner, 1988, 2003; Hintzman, 1990; Juloa & Plunkett, 2000; Kinder & Shanks, 2001, 2003; Merikle & Reingold, 1991; Ostergaard, 1992; Perruchet & Gallego, 1993; Plaut, 1995; Poldrack, 1996; Poldrack, Selco, Field, & Cohen, 1999; Shanks & Perruchet, 2002; Shanks & St. John, 1994; Shanks, 1997, 2005; Van Orden, Pennington, & Stone, 2001). For example, single dissociations (cases where one measure is affected and the other is apparently not) could arise because of differences in the sensitivity of the tasks and not necessarily because a system is selectively influenced (Shallice, 1988). If priming and recognition depended upon the same memory system, but priming was less sensitive to the effects of a variable than recognition, effects would be less likely to be detected. Consistent with this notion, Buchner and Wippich (2000) have shown that the reliability of many priming tasks (as measured by split-half correlations) is reliably less than that of comparable recognition tasks (see also Buchner & Brandt, 2003; Meier & Perrig, 2000). One important exception to this was the perceptual identification task, which was found to be as reliable as recognition (but see ahead to Chapter 4). Therefore, on purely statistical grounds, priming in many tasks is less likely to be affected by independent variables than recognition, which could give rise to single dissociations.

Furthermore, to assert that a variable has no effect on a particular measure requires accepting the null hypothesis, and this can be difficult to justify given that the size of a true effect could be extremely small (Dunn, 2003). This same criticism applies when two (opposite) single dissociations are used together to constitute a double dissociation (e.g., the double dissociation between amnesics and right occipital lobe damaged patients, Gabrieli et al., 1995) where again it could be argued that the failure to detect an effect on one measure does not mean that the effect does not exist. It should be noted, however, that there are studies which have reported that variables have no effect on priming even though the power to detect very small effects is high (e.g., levels of processing, see meta-analysis in Richardson-Klavehn et al., 1999). Ways to circumvent the problem of accepting the null hypothesis have been proposed. For example, Dunn and Kirsner (1988) proposed that the logic of reversed association—demonstrating a variable has opposite effects on two measures which are associated under different conditions—could be used to provide evidence of distinct processes, but this logic is not commonly adopted. Exceptions are studies by Richardson-Klavehn, Lee, Joubran, and Bjork, (1994) and Richardson-Klavehn et al. (1999) which used the logic of reversed association to demonstrate differences in the retrieval intentionality processes between recognition and priming.

1.2.1 Transfer-Appropriate Processing

The theory of transfer-appropriate processing (TAP) predicts dissociations between tasks based on the extent to which each task engages conceptual or perceptual processes, without postulating distinct implicit and explicit memory systems (Blaxton, 1989; Roediger & Blaxton, 1987; Roediger & McDermott, 1993; Roediger, Weldon, & Challis, 1989). The term ‘perceptual processes’ refers to the analysis of perceptual or surface level features, whereas ‘conceptual processes’ refers to the analysis of meaning or semantic information. According to the TAP theory, performance on memory tests benefits to the extent to which the processes involved at retrieval match those engaged at encoding. Most repetition priming tasks are thought to draw primarily upon perceptual processes whereas recognition is thought to draw primarily upon conceptual processes (Roediger & Blaxton, 1987). The type of processing a task engages can be determined by looking at the effects of a number of critical variables on performance. For example, if generating an item at encoding leads to better performance than reading, the test is classified as conceptual, while if the reverse is true, the task is classified as perceptual (Roediger et al., 1989).

TAP can account for a wide range of dissociations between priming and recognition, including the effects of levels of processing, modality, read-generate manipulations and the picture-superiority effect (Roediger & McDermott, 1993). For example, the effects of levels of processing are explained in the following way: semantic processing at encoding is primarily conceptual in nature, and this will therefore benefit recognition which relies on conceptual processing. However, priming will be relatively unaffected because priming tasks rely on perceptual processing.

Although TAP can account for many of the dissociations between priming and recognition, it does not address the issue of awareness which is a central feature of the multiple-systems approach. For example, TAP does not specify whether the contents of the information supporting priming are accessible to awareness or not. Other limitations of the TAP account are evident when the theory is applied to findings from amnesia. Amnesia tends to affect performance on direct memory tests while leaving performance on indirect memory tests relatively unaffected, regardless of whether the task relies on perceptual or conceptual processing (e.g., Levy, Stark, & Squire, 2004). In other words, the dissociations shown by amnesics tend to follow the indirect/direct distinction rather than the perceptual/conceptual distinction (see also Roediger et al., 1999).

1.2.2 Information Availability Model

Another model which accounts for priming and recognition without postulating a distinction between implicit and explicit memory is the information availability model (IAM) (Ostergaard, 1998). In the IAM, priming is assumed to be driven by a single, episodic source of memory and there is a strong emphasis on the different ways that tasks interact with this representation. The model assumes that there is typically a greater influence of non-memorial factors on performance in priming tasks than recognition. As support for this, Ostergaard (1992, 1998) points out that overall levels of performance in priming tasks are typically high. According to the IAM, these non-memorial factors effectively constrain priming effects, giving rise to dissociations.

The IAM predicts that when the influence of these non-memorial factors is reduced, effects of variables on priming should emerge (because the relative influence of the study episode will be greater). Consistent with this, Ostergaard (1998) showed that when the amount of perceptual information directly available from a stimulus (a non-memorial factor in IAM) in a perceptual clarification task was reduced (by using a long clarification duration), variables such as word-frequency, number of repetitions and delay had clear effects on priming (and recognition). However, in a condition in which the amount of perceptual information directly available from the stimulus was greater (by using a short clarification duration), a dissociation was evident such that these variables had effects on recognition but not priming. This was taken to support the IAM and the notion that there is a greater influence of non-memorial factors on performance in priming tasks than recognition, which can give rise to dissociations.

A similar logic has also been used to question the notion that priming in amnesics is equivalent to that of controls (and is therefore selectively spared in amnesia) (e.g., Ostergaard & Jernigan, 1996, vs. Hamann, Squire, & Schacter, 1995). Ostergaard has claimed that, when carefully assessed, priming is impaired in amnesia (e.g., Jernigan & Ostergaard, 1993; Ostergaard & Jernigan, 1993, 1996). Priming effects are often proportional to baseline levels of performance in the priming tasks (in controls, Ostergaard, 1998, and amnesics, Ostergaard, 1994). Baselines are often slower in amnesics than controls (e.g., identification response times are longer in perceptual identification and perceptual clarification tasks) which could lead to elevated levels of priming, effectively masking any priming deficit in these individuals. Indeed, when differences in baselines are equated between amnesics and controls, amnesics actually show lower levels of priming than controls (Ostergaard, 1994). Furthermore, under conditions in which constraints on priming are reduced (e.g., by using a long clarification duration), amnesics show clear impairments in priming relative to controls (Ostergaard, 1999), consistent with the prediction of the IAM model. This evidence appears to undermine the view that the memory system crucial for priming is selectively spared in amnesia and suggests that amnesia damages a single system which is crucial for both priming and recognition.

1.2.3 Simple Recurrent Network Model

The simple recurrent network (SRN), a single-system connectionist model of priming and recognition, has been shown to explain a number of the dissociations widely regarded as evidence for multiple systems (Kinder & Shanks, 2001, 2003). Kinder and Shanks (2003) used the SRN to reproduce the double dissociation shown by amnesics and individuals with damage to the right occipital lobe (Gabrieli et al., 1995). To do this they first assumed that amnesia is associated with a generalized (rather than specific) learning deficit, whereas they assumed that occipital lobe damage is associated with a visual processing deficit. Next, to simulate the brief masked presentation of an item in the perceptual identification task, each item was input into the SRN in degraded form, whereas each item was input in non-degraded form for the recognition task. A double dissociation emerged from the SRN solely because of the way that the assumed nature of the deficits and differences in task procedures interacted with the underlying memory representation. Also, despite being driven by the same memory representation, priming and recognition performance of the SRN were not correlated.

Furthermore, Kinder and Shanks (2001) demonstrated that the SRN can simulate the striking dissociation shown by the extremely dense amnesic individual E.P. (Hamann & Squire, 1997a, 1997b). Again, by assuming that the deficit in amnesia is a generalized learning deficit, the model simulated normal amounts of priming despite levels of recognition that were, for all practical purposes, no greater than chance levels (recognition percent correct was 1% above chance). Kinder and Shanks (2003) also suggested that the effects of changes in modality between study and test could be accounted for by the including additional layers in the SRN which represent the input from different modalities. If it is assumed that priming depends primarily on modality-specific representations and recognition primarily on modality unspecific representations, shifts of modality could affect priming but not recognition. Other connectionist models have been used to successfully explain double dissociations in other fields without the need to postulate distinct systems (e.g., Juloa & Plunkett, 2000; Plaut, 1995). In sum, the results of the simulation studies with the SRN show that the dissociations shown by patients with amnesia and right occipital lobe damage, findings of stochastic independence, and also priming in the absence of recognition in amnesics are not inconsistent with a single system account.

1.4 Other Accounts

1.4.1 Activation

An early view is that exposure to an item temporarily activates a pre-existing lexical representation of the item which is more likely to be activated when it is presented again (e.g., the logogen model*,* Morton, 1969, 1979). Activation occurs independently of the processing required for recognition, which is assumed to be driven by episodic memory traces which depend upon elaborative processing (i.e., the formation of new associations) (Graf & Mandler, 1984). This theory of priming has fallen out of favour, largely because priming has been shown to exist for novel stimuli for which preexisting representations do not exist (e.g., Keane, Gabrieli, Noland, & McNealy, 1995; Stark & McClelland, 2000). Furthermore, priming can be detected even after long retention intervals (Cave, 1997; Mitchell, 2006; Tulving et al., 1982), which is inconsistent with the notion that activation is temporary.

1.4.2 Counter Model

Ratcliff and McKoon (1996) have proposed that priming does not arise because of modifications to the learning systems associated with implicit memory (assumed by some versions of the multiple-systems theory, e.g., Schacter, 1990; Squire, 1994), but rather that it reflects a bias to identify a stimulus as one seen before. Evidence for this view comes from performance in a modified perceptual identification task (Ratcliff, McKoon, & Verwoerd, 1989). In this task an item is briefly presented (the target) and then two alternatives are presented. The participant is asked to decide which item was briefly presented. If one of the alternatives had been presented at study, it is likely to be the item chosen, regardless of whether it was actually the target or not. This finding suggests that the influence of the study exposure is to bias the response towards the old item. This notion that priming is a biasing effect is embodied in the counter model (Ratcliff & McKoon, 1997) which was developed to account for this and other findings with the two-alternative forced-choice (2AFC) identification task.

The counter model is cast in a traditional information processing approach and it is assumed that variables can affect different levels of the system. Ratcliff and McKoon (1997) suggest that dissociations can arise between priming and recognition because recognition depends primarily upon a different level of the system to priming. For example, stochastic dissociations are assumed to arise because the features of a word that make it easily identifiable in priming tasks are independent from the features of the word that make it easily recognizable. Thus, this model does not propose distinct systems to explain priming and recognition.

1.4.3 REMI

REMI (Retrieving Effectively from Memory: Implicit) (Schooler, Shiffrin, & Raaijmakers, 2001) is a model of priming in the perceptual identification task that is based upon the REM model of recognition (Shiffrin & Steyvers, 1997). It has primarily been applied to findings in the 2AFC identification paradigm (Schooler et al., 2001; Wagenmakers, Zeelenberg, Huber, Raaijmakers, Shiffrin, Schooler, 2003). Explicit memory is assumed to rely on an episodic trace whereas implicit memory is assumed to rely on a lexical trace. Thus, unlike the counter model, priming and recognition are assumed to rely on different systems. Although the counter model and REMI are viable models of priming, they have largely been used to account for various findings in the

2AFC task and have not been directly applied to the dissociation evidence for multiple-systems.

1.4.4 Summary

In sum, much of the evidence that has been taken to support the notion that priming and recognition are mediated by distinct implicit and explicit memory systems has been reinterpreted by TAP theory, the IAM model, and the SRN model, which do not propose distinct implicit and explicit systems. Other models of implicit memory have been proposed such as the counter model and REMI, but these have not been used to directly address the dissociation evidence above. An advantage of the use of models in theory construction is that they can provide formal quantitative accounts of the phenomena of interest rather than descriptive verbal theories which are notoriously imprecise by nature and susceptible to alternative interpretation.

Of the IAM and SRN models, the SRN seems to provide a more complete account of priming and recognition. The IAM is an abstract model of the memorial and non-memorial influences on performance in priming tasks and does not explicitly include recognition in the model. On the other hand, the SRN models both priming and recognition and produces quantitative estimates of each. Although the SRN has been shown to successfully account for important dissociations from a single-system perspective, it remains to be seen whether it is a plausible model of declarative memory/recognition. Reber (2002) has criticized the SRN on the grounds that many learning epochs were required during training in order for the model to simulate particular dissociations. He argues that recognition, in contrast, can occur after a single exposure.

1.5 Signal-Detection Theory

One theory that is widely regarded as a plausible account of recognition is signal-detection theory (Green & Swets, 1966; Macmillan & Creelman, 2005). In standard signal-detection models of recognition old and new items are represented as overlapping Gaussian distributions on a single 'strength of evidence' continuum. Because of the influence of the study phase, the mean strength of the old item distribution is assumed to be greater than that of new items. Typically, a participant is assumed to decide whether an item is old or new by assessing its value of strength relative to a decision criterion located at some point along this continuum. If the strength exceeds the criterion then the participant will judge the item as old (i.e., they will make a positive response), otherwise they will judge the item as new (i.e., they will make a negative response).

Signal-detection models containing a single memory strength variable can explain many recognition phenomena (see Wixted, 2007, for a review) (although there is much debate as to whether a model which contains an additional recollection component should be preferred, see e.g., Parks & Yonelinas, 2007). If priming and recognition depend upon the same memory variable, a logical step would be to extend a signal-detection theory of recognition to also account for priming. This has already been done in the field of implicit learning with informative results.

Shanks and colleagues (Shanks, 2005; Shanks & Perruchet, 2002; Shanks, Wilkinson, & Shannon, 2003) have proposed a single-system signal-detection model which is conceptually very similar to signal-detection theory of recognition judgments and their latencies (e.g., Pike, 1973; Ratcliff & Murcock, 1976; Stretch & Wixted, 1998). This model has been used to account for dissociations between priming and recognition in the serial reaction time task (Neissen & Bullemer, 1987), a task used widely by researchers of implicit learning. Priming in this task is shown by faster response execution of repeated motor sequences than new sequences. In an experiment by Shanks and Perruchet (2002) priming and recognition for repeated sequences were above chance overall, but a dissociation was also present such that some participants who performed no better than chance at recognizing the repeated sequences (as measured with 6-point recognition confidence judgments) nevertheless showed a priming effect. Shanks and Perruchet (2002) showed that a signal-detection model in which the same memory strength signal drove priming and recognition, but was subjected to independent sources of noise for each task, predicted this dissociation. According to the model, this dissociation was merely an artifact, a consequence of random sampling and measurement error (as represented by noise in the model). Noise was crucial because when it was not included, the dissociation was not predicted (Shanks et al., 2003). Thus, this dissociation, which could have been taken to indicate the independence of priming and recognition, and used as evidence for a multiple-systems view, was in fact consistent with a single-system account.

Although a single-system signal-detection theory has been proposed to understand dissociations in the field of implicit learning, this type of theory has not yet been used to understand dissociations in the field of implicit memory. The present thesis attempts to do this and it is proposed that many dissociations between priming and recognition that appear to be indicative of the involvement of multiple memory systems are in fact more parsimoniously explained by a single-system signal-detection model.

The model is the first to deal with priming, recognition (and fluency) simultaneously. It may serve as a useful benchmark for evaluating multiple- versus single-system theories, and, via its testable predictions, may present a falsification challenge to researchers.

1.6 Overview of Thesis

The aim of the present thesis is to re-evaluate some of the behavioural evidence that has been used to support the notion that priming and recognition are driven by distinct implicit and explicit memory systems. Experiments 1 to 4 in Chapter 2 present numerous attempts to replicate some key evidence for the existence of implicit memory. No evidence is found for a form of memory, the contents of which are not accessible to awareness. Chapter 3 presents a single-system signal-detection computational model of priming and recognition which contains only a single source of memorial evidence. In Chapter 4 this model is applied to Experiments 5 to 8 which were conducted to examine the effects of manipulations of attention at encoding on recognition and priming in the perceptual identification task. The model simulated greater effects of attention on recognition than priming, correlations of performance between the tasks, and also reliability data. In Chapter 5, the model is extended to account for fluency effects—the tendency for faster identified items to be judged old—in a perceptual clarification paradigm. The model is applied to findings from this paradigm which have been taken as evidence for distinct sources of memory in priming and recognition. In Chapter 6 the model is used to account for some recent evidence from amnesics, who show relatively preserved priming and fluency effects despite impaired recognition. Chapter 7 presents a modified version of the gradual clarification paradigm and tests the predictions of the model in this paradigm in Experiments 10 to 12. Results with this new paradigm reveal limitations of the single-system model, and also a dual-system version of the model.

Chapter 2: Priming and Awareness

According to some multiple-systems accounts (e.g., Squire, 1994), a defining characteristic of implicit memory is that its contents are not accessible to awareness. How might evidence for an unconscious form of memory be demonstrated? Schacter et al. (1989) suggest that unconscious memory is demonstrated when reliable priming is obtained despite performance on a direct test, such as recognition, being at chance. From this type of dissociation, Schacter et al. (1989) argue that one can infer that the memory driving priming is not available to awareness because if it was, then it would have been used in the direct test in which the motivation to do so was stronger (because of the nature of the instructions). The patient E. P. has been shown to produce this very pattern (e.g., Hamann & Squire, 1997a, 1997b). However, this is a unique case, and amnesics typically perform above chance in recognition tests, making it difficult to discount the possibility that the priming effects are driven by a form of memory that is accessible to awareness (see also Kinder & Shanks, 2001, 2003). Furthermore, in normal adults, some have concluded that there is scarcely any evidence that priming can occur when recognition is at chance (see Butler & D. C. Berry, 2001, 2004).

Even if this dissociation can be demonstrated, whether it can be considered an unequivocal demonstration of unconscious memory is questionable. For example, to claim that null awareness (as indicated by chance recognition) has been obtained, one must assume that the purported direct test of memory exhaustively indexes all of the memory available to awareness, a difficult, if not impossible criterion to justify (Merikle & Reingold, 1990; Schacter et al., 1989; Shanks & St. John, 1994). Another problem is that comparisons are frequently made between direct and indirect tasks with different characteristics: Tasks may differ in such things as the retrieval cues presented at test, the response metric on which performance is measured, how reliable they are, or the extent to which performance is affected by response bias. If any of these differences exist, then it could be argued that the observed dissociation is caused by the various characteristics of the tasks rather than differences in the forms of memory that they are purported to measure (Buchner & Wippich, 2000; Kinder & Shanks, 2001, 2003; Merikle & Reingold, 1991; Reingold, 2003).

In an attempt to circumvent many of these issues, Merikle and Reingold (1991) proposed that the logic of the relative sensitivity approach (Reingold & Merikle, 1988) could be used to provide unequivocal demonstrations of unconscious memory. In this approach, direct and indirect tasks are made as comparable as possible by matching them on all characteristics except task instructions. Merikle and Reingold argued that given a minimal a priori assumption—that "the sensitivity of a direct discrimination is assumed to be greater than or equal to the sensitivity of a comparable indirect discrimination to conscious, task relevant information" (Reingold & Merikle, 1988, p. 566)—unconscious influences are necessarily implicated whenever the sensitivity of an indirect task exceeds that of a comparable direct task, even if performance on the latter is above chance.

According to Merikle and Reingold (1991), a number of studies meet the requirements of this approach and therefore qualify as demonstrations of unconscious memory. For example, in an early study by Eich (1984), participants shadowed prose presented to one ear in a dichotic listening task. To the non-shadowed ear, pairs of words were presented, consisting of a homophone and a context word, such as TAXI-

FARE, which was intended to bias the meaning of the homophone to its less common meaning. At test, participants were presented with old or new homophones and were asked to make a recognition judgment (direct task) or to spell the word (indirect task). Recognition was at chance for the non-shadowed homophones, but participants were more likely to spell them in their less common form (i.e., FARE rather than FAIR) than would be expected by chance. According to Merikle and Reingold (1991), the greater sensitivity of the indirect task than the comparable direct task is evidence for unconscious memory in Eich's (1984) study. However, contrary to the conclusion drawn by Merikle and Reingold (1991), the spelling and recognition tasks in Eich's (1984) procedure are not completely comparable. The tasks differed in terms of the response made: In the indirect task, participants spell a word, and in the recognition task they make an old-new judgment. Thus, it is not clear that this study actually meets the criteria of the relative sensitivity approach. Furthermore, many aspects of Eich's (1984) study have since been criticized (Wood & Cowan, 1995; Wood, Stadler, & Cowan, 1997), and the generality of the results are questionable because the homophone spelling task is not a commonly used indirect task.

Mere exposure effect studies in which exposure to stimuli can increase liking judgments in the absence of recognition memory for those stimuli also qualify, according to the logic of the relative sensitivity approach, as demonstrations of unconscious memory (e.g., Kunst-Wilson & Zajonc, 1980). However, this pattern has been replicated in some studies (e.g., Bonanno & Stillings, 1986; Seamon, Marsh, & Brody, 1984) but not in others (Fox & Burns, 1993; Newell & Shanks, 2007). Similarly, demonstrations of this pattern with non-affect judgments are equivocal. For example, early demonstrations of this effect with brightness and darkness judgments (Mandler, Nakamura, & Van Zandt, 1987) have not since been replicated (Seamon, McKenna, & Binder, 1998, but see Whittlesea & Price, 2001). Thus, it seems fair to say that in studies investigating the mere exposure effect, convincing, reliable evidence for unconscious memory remains elusive.

Merikle and Reingold (1991) used the logic of the relative sensitivity approach in their experiments to provide a compelling demonstration of unconscious memory. Merikle and Reingold (1991) presented a pair of words, one above the other, for 500 ms on each study trial and required participants to read aloud the word that was cued with arrows. At test, a single word was presented on each trial against a mottled background mask that degraded the appearance of the word. Participants in the direct task judged whether the word had been presented in the study phase (old–new recognition judgments), and participants in the indirect task judged whether the contrast between the word and the background was high or low. Trials at test were arranged into three blocks consisting of an equal number of old and new words (either cued and new words in their Experiment 1 or uncued and new words in their Experiment 2). The key finding was that when uncued and new words were presented at test, the sensitivity of the indirect task in Blocks 1 and 2 was significantly greater than that of the direct task, which was at chance in these blocks. Given their a priori assumption, Merikle and Reingold interpreted this result as an "unequivocal demonstration of unconscious memory" (p. 231).

Merikle and Reingold's (1991) study has been cited over 100 times in the literature and has even been referred to as an "existence proof" that the contents of the representation supporting priming are not accessible to awareness (e.g., Roediger &

McDermott, 1993; Stadler & Roediger, 1998; Wippich, 1995). However, a search of the literature indicates that this evidence has never been replicated. Merikle and Reingold's (1991) study is also influential because, for uncued words, there was a trend for the sensitivity of the direct task to increase across test blocks, and for the sensitivity of the indirect task to decrease. Erdelyi (2004) has argued that these block effects may have implications for the transitory nature of direct and indirect performance over time. Plainly, given the impact and potential implications of Merikle and Reingold's findings, their results demand replication. Thus, the primary aim of Experiments 1–4 was to replicate this evidence for unconscious memory.

It is important to emphasize from the outset that the validity of Reingold and Merikle's relative sensitivity approach is not being questioned here: If performance on indirect tests reflects, at least in part, memory not available to awareness, then it should be possible to show greater sensitivity to the influence of previously exposed items in the indirect test than in the direct test, as Merikle & Reingold did. If, however, the contents of the memory driving greater-than-chance performance on the indirect task are accessible to awareness then one would not expect the sensitivity of the indirect task to exceed that of a comparable direct test (see, e.g., Shanks & St. John, 1994).

2.1 Experiment 1

The aim of Experiment 1 was to follow Merikle and Reingold's method as closely as possible and replicate their evidence for unconscious memory: greater sensitivity of the contrast (indirect) task to the influence of uncued items in Test Blocks 1 and 2 than a comparable recognition (direct) task (Merikle & Reingold, 1991, Experiment 2A). The contrast task can be shown to be a sensitive test of memory if judgments of visual contrast between a word and a background mask are affected by whether a word has been previously exposed. Jacoby, Allan, Collins, and Larwill (1988) showed that the intensity of background white noise is judged as lower when an old, rather than a new, item accompanies it; this is commonly regarded as reflecting the greater fluency that comes from reprocessing recently presented items (see also Gooding, Mayes, & Meudell, 1999). Applying this logic to the contrast task, an old word is assumed to be perceived more easily (or stand out more) than a new word against the background mask. Sensitivity to familiarity (prior exposure) can be shown in the contrast task if a greater proportion of old words are judged as being presented in high-contrast conditions than are new words.

2.1.1 Method

2.1.1.1 Participants

99 UCL psychology undergraduates participated as part of a 1st year laboratory class. Their ages ranged from 18 to 40 years, with a mean of 19.5 years.

2.1.1.2 Design

A 2 x 3 mixed design was used in which participants were allocated at random to either the contrast (n = 53) or the recognition (n = 46) tasks, and sensitivity was measured over three test blocks.

2.1.1.3 Materials

Participants were run individually in sound dampened cubicles in all experiments. The program was written in Visual Basic 6.0 and used ExacTicks v1.1

(Ryle Design, 1997) to achieve millisecond accuracy. All stimuli were presented on a monitor with the screen resolution set to 1024 x 768 pixels.

A word pool consisting of 384 five-letter nouns and 384 six-letter nouns with a Kucera and Francis (1967) frequency of 2–15 per million was formed. For each participant, words were randomly chosen from this pool to compile three 48-word lists containing an equal number of five- and six-letter words. Each list acted as either the cued, uncued or new word set. The cued and uncued word lists were presented at study, and the uncued and new word lists were presented at test. All study and test words were presented in white 26 point lowercase Arial font against a black background.

Study word pairs were constructed from the cued and uncued word lists. Selection of words from each list was randomized with the constraint that members of each pair had the same number of letters. The presentation order of these pairs was randomized for each participant. An additional 12 pairs of words consisting of an equal number of five- and six-letter pairs were randomly selected from the word pool to act as primacy and recency filler trials: Half of these pairs acted as the first six study trials and the other half acted as the last six study trials. None of the filler stimuli were presented at test. Thus, the study phase consisted of 60 trials in total. On each trial a word was presented 7 mm (0.50º)[1] above and 7 mm (0.50º) below a fixation dot, which measured approximately 4 mm (0.31º) in diameter. Each word was approximately 5–8 mm (0.38º–0.61º) high; five-letter words were approximately 30 mm (2.29º) in length and six-letter words were approximately 36 mm (2.75º) in length. An arrow measuring

[1] The visual angle subtended by each stimulus dimension at a viewing distance of approximately 75 cm is provided within parentheses following each measurement.

5 mm (0.38º) in length was located approximately 7 mm (0.50º) from each end of the cued word. Thus, the entire stimulus display measured approximately 34 mm (0.50º) vertically and 60 mm (4.57º) horizontally on the screen. The cued word appeared an equal number of times above and below the fixation dot across study trials.

A single word was presented on each contrast and recognition trial against a static rectangular mask. Trials were arranged into six 16-trial blocks with the constraint that each block contained an equal number of uncued, new, five-letter and six-letter trials. Thus, there were 96 test trials in total; 48 uncued and 48 new word trials. Two types of masks were used: In one, 50% of the pixels were white (high-contrast-condition mask) and in another, 55% of the pixels were white (low-contrast-condition mask).[2] Each mask measured approximately 45 mm (3.43º) horizontally and 10 mm (0.76º) vertically. For each participant, four high-contrast and four low-contrast masks were randomly generated. Each of the eight masks was used with one old and one new word in each block of 16 trials. Uncued and new words were presented equally as often in high- and low-contrast conditions.

2.1.1.4 Procedure

A white fixation dot was presented at the centre of a black background at the start of the study phase. Participants were told to initiate each trial when they were

[2] The low-contrast-condition mask density value differed slightly from that used by Merikle and Reingold (60%). The reason for this change was that the required contrast discrimination was deemed too easy with Merikle and Reingold's original mask densities. It is unlikely that any failure to replicate their results would be due to this change because the key findings held even when a constant mask density was used across trials in Experiments 2–4. Merikle and Reingold's pattern of results also held even when they used a constant mask density (Merikle & Reingold, 1991; Experiment 2B).

looking at the fixation dot by pressing the *Enter* key. After a trial was initiated, the fixation dot was replaced by a 200 ms unfilled interval. The target display was presented for 500 ms and consisted of a pair of words, one presented above the fixation location and one below. A white arrow was presented at each end of the cued word. Participants were required to read aloud the cued word; both accuracy and speed were emphasised in the study phase instructions. A 2000 ms unfilled interval followed the offset of the target display, after which the fixation dot reappeared to indicate to the participant that they could initiate the next trial. No indication of the impending memory test was given.

On each trial of the test phase an uncued or new word was presented. Recognition participants were instructed to read aloud each word on every trial and then press the "O" or "N" key to indicate whether they thought the word was "old" or "new". They were told that half of the words had been presented in the first stage but were words that they did not have to read aloud, and that these words were therefore old; they were also told that the other half of the words had not been presented before in the experiment and were therefore new. Before the target trials commenced, participants completed eight practice trials to familiarise themselves with the response buttons. In these trials the words "old" and "new" were presented four times each in an equal number of high- and low- contrast conditions.

Participants in the contrast discrimination task were told that they would see a single word on each trial that would be presented against a background of visual noise. They were told that half of the presentation conditions were high-contrast and half were low-contrast; their task was to read the word aloud and then decide whether the contrast was "high" or "low". If a word appeared to "stand-out" from the background then the contrast was high and they must press the "H" key. On the other hand, if they thought the word appeared to "blend" into the background then the contrast was low and they must press the "L" key. On eight practice trials the word "word" was presented in an equal number of high- and low-contrast conditions.

Participants in both tests were told that the word would remain on the screen until they had pressed a key, and that the required discrimination could be difficult to make. The instructions encouraged participants to do their best in making their judgments. No indication of test block transition was given and no feedback was given as to the correctness of their responding. Instructions between the two tasks were designed to be as similar as possible except for the required discrimination and references made to the study phase. The entire testing procedure took approximately 25 minutes. Participants were debriefed upon completion of the test phase.

2.1.2 Results and Discussion

An alpha level of .05 was used for all statistical tests. The assumption of sphericity was tested with Mauchly's W statistic. Huynh–Feldt's correction (Huynh & Feldt, 1976) was applied to the degrees of freedom when the assumption of sphericity was violated.

2.1.2.1 Sensitivity of the Contrast and Recognition Tasks to Familiarity

The results were analyzed in a fashion similar to that used by Merikle and Reingold (1991), first dividing each participant's 96 test trials into three blocks of 32 trials and then computing the sensitivity (A'; see J. G. Snodgrass & Corwin, 1988) of each task to familiarity (prior exposure) at each block.[3] For the recognition task, a hit was defined as responding "old" to an uncued word, and a false alarm was defined as responding "old" to a new word. For the contrast task, a hit was defined as responding "high" to an uncued word, and a false alarm was defined as responding "high" to a new word. Thus, in this first analysis, the data were collapsed across the contrast variable. The mean hit and false alarm rates to uncued words in both tasks are displayed in Table 2-1.

Figure 2-1 shows the mean sensitivity (A') of each task to uncued words at each test block and indicates that, in contrast to the findings of Merikle and Reingold (1991), the recognition task was more sensitive to familiarity than was the contrast task. A 2 x 3 mixed analysis of variance (ANOVA) with Task (recognition, contrast) and Block (Blocks 1, 2, 3) as factors revealed a significant main effect of Task, $F(1, 97) = 19.95$, $p < .001$, indicating that sensitivity to familiarity was indeed greater in the recognition task than in the contrast task, thus failing to replicate Merikle and Reingold's key evidence for unconscious influences of memory. In addition, performance did not reliably vary across Blocks, $F(2, 194) = 1.48$, $p = .23$, nor did the Task interact with the Test Block, $F(2, 194) = 1.09$, $p = .34$.

Further analysis confirmed that, for uncued words, the recognition task was sensitive, but the contrast task was not. Recognition performance was significantly

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[3] Where H = hits and FA = false alarms: For H ≥ FA, $A' = 0.5 + [(H - FA)(1 + H - FA)]/[4H(1 - FA)]$. For FA ≥ H, $A' = 0.5 - [(FA - H)(1 + FA - H)]/[4FA(1 - H)]$. The hit and false-alarm rates were adjusted as suggested by J. G. Snodgrass and Corwin (1988) in order to avoid undefined values of the sensitivity measures. The data were also analyzed with *Pr* (= H - FA) and d' as alternative measures of discriminability, and the same qualitative pattern of results was found.

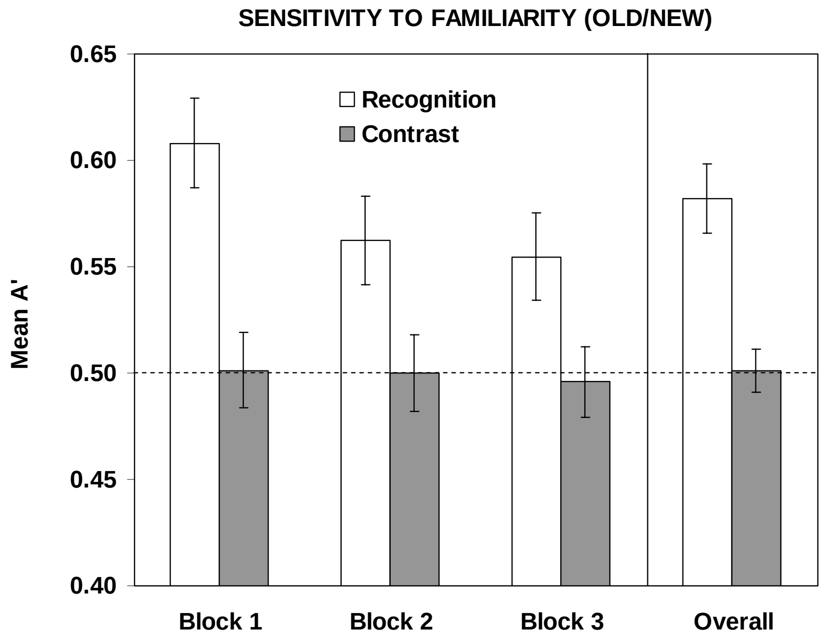

Figure 2-1. Mean sensitivity (A') of the contrast and recognition tasks to familiarity at each test block and overall in Experiment 1. (Bars indicate standard errors.)

greater than that expected by chance ($A' = .5$) at all three test blocks—Block 1, $M = .61$, $SEM = .02$, $t(45) = 5.15$, $p < .001$; Block 2, $M = .56$, $SEM = .02$, $t(45) = 2.98$, $p < .005$; Block 3, $M = .55$, $SEM = .02$, $t(45) = 2.68$, $p < .01$—and also when collapsed across test blocks—overall $M = .58$, $SEM = .02$, $t(45) = 5.08$, $p < .001$. Sensitivity in the contrast task, however, did not significantly differ from chance at any block or overall: Block 1, $M = .50$, $SEM = .02$, $t(52) = 0.07$, $p = .94$; Block 2, $M = .50$, $SEM = .02$, $t(52) = 0.002$, $p = .99$; Block 3, $M = .50$, $SEM = .02$, $t(52) = -0.25$, $p = .81$; overall, $M = .50$, $SEM = .01$, $t(52) = 0.09$, $p = .93$. Thus, whereas Merikle and Reingold (1991) found that, for uncued words, the contrast discrimination test was sensitive to familiarity, these results indicate that the contrast task was not sensitive to familiarity.

Table 2-1. *Mean hit and false alarm rates for the recognition and contrast tasks in Experiments 1–4*

	Block 1		Block 2		Block 3		Overall	
Experiment and Condition	Hits	FA	Hits	FA	Hits	FA	Hits	FA
Experiment 1								
Recognition								
M	.500	.367	.465	.389	.445	.379	.469	.373
SE	.021	.021	.024	.023	.027	.024	.019	.019
Contrast								
M	.490	.489	.464	.459	.474	.477	.475	.474
SE	.023	.023	.025	.027	.022	.026	.020	.020
Experiment 2								
Contrast task –cued								
M	.506	.418	.521	.450	.521	.518	.516	.460
SE	.040	.038	.034	.024	.032	.037	.026	.022
Contrast task –uncued								
M	.532	.532	.524	.476	.471	.544	.509	.518
SE	.039	.043	.039	.031	.036	.042	.031	.028
Experiment 3								
Recognition								
M	.505	.431	.417	.422	.466	.407	.461	.417
SE	.048	.051	.050	.041	.058	.052	.029	.031
Contrast								
M	.529	.520	.510	.534	.505	.559	.515	.539
SE	.049	.046	.031	.042	.054	.046	.040	.041
Experiment 4								
100 ms study exposure								
Recognition								
M	.402	.402	.350	.395	.428	.337	.389	.373
SE	.036	.037	.033	.033	.043	.035	.032	.030
Contrast								
M	.484	.480	.451	.480	.454	.444	.461	.467
SE	.035	.040	.031	.039	.039	.037	.024	.029
500 ms study exposure								
Recognition								
M	.431	.353	.363	.363	.373	.270	.384	.321
SE	.064	.058	.062	.057	.065	.059	.060	.053
Contrast								
M	.402	.426	.495	.471	.480	.559	.457	.485
SE	.052	.050	.046	.042	.036	.023	.017	.018

Note. FA = false alarms; *SE* = standard error.

2.1.2.2 Sensitivity of the Contrast and Recognition Tasks to Contrast

Another way of analyzing the same set of data is to compute the sensitivity of each task to contrast (high or low contrast level). In this analysis, for the contrast task, a hit was defined as responding "high" to a high contrast level, and a false alarm was defined as responding "high" to a low contrast level. For the recognition task, a hit was defined as responding "old" to a word presented at high contrast, and a false alarm was defined as responding "old" to a word presented at low contrast. Thus, in this analysis, the data are collapsed across the uncued–new manipulation.

Figure 2-2 shows the mean sensitivity (A') of each task to contrast and indicates that sensitivity to this dimension was much greater in the contrast than in the recognition task. In other words, when participants were instructed to judge the contrast level, they were able to do so, and their sensitivity to contrast was greater than that of participants responding indirectly to this dimension (i.e., in recognition). This was confirmed by a 2 x 3 mixed ANOVA with Task (recognition, contrast) and Block (Blocks 1, 2, 3) as factors, which revealed a main effect of Task, $F(1, 97) = 71.55$, $p < .001$. A significant Block effect was also obtained, $F(1.93, 187.00) = 3.14$, $p < .05$, indicating that performance changed over blocks; however, these two factors did not interact significantly, $F(1.93, 187.00) = 2.27$, $p = .11$. The sensitivity of the contrast task to contrast significantly exceeded the chance level of performance at each test block and also overall: Block 1, $M = .70$, $SEM = .02$, $t(52) = 8.56$, $p < .001$; Block 2, $M = .66$, $SEM = .02$, $t(52) = 6.64$, $p < .001$; Block 3, $M = .70$, $SEM = .02$, $t(52) = 9.74$, $p < .001$; overall, $M = .70$, $SEM = .02$, $t(52) = 10.15$, $p < .001$. For the recognition task, sensitivity to contrast was significantly above chance in Test Block 3, $M = .54$, $SEM = .02$,

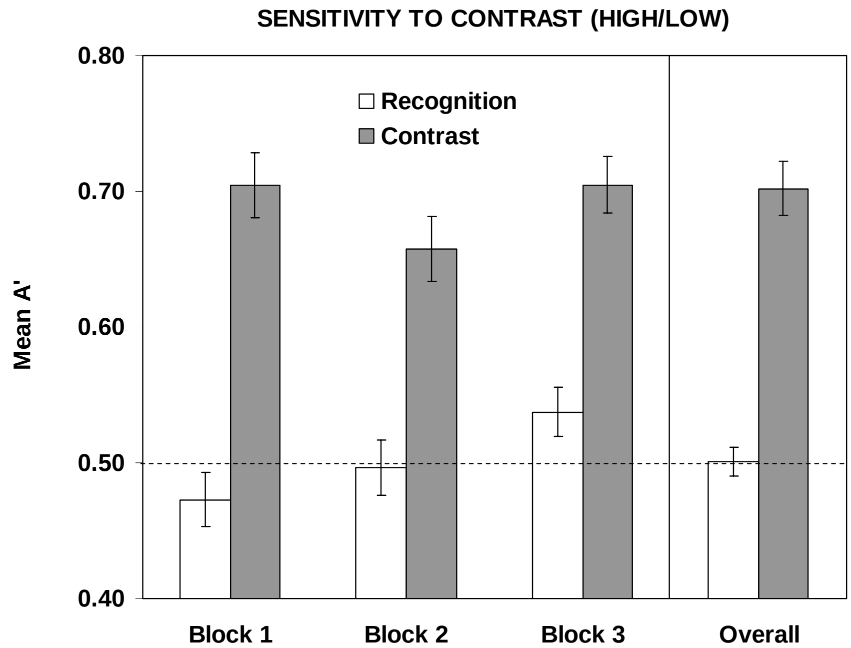

Figure 2-2. Mean sensitivity (*A*') of the contrast and recognition tasks to contrast at each test block and overall in Experiment 1. (Bars indicate standard errors.)

$t(45) = 2.08$, $p < .05$, suggesting that responding in this block was influenced by the ease with which a word was read against the background, independently of whether it had been previously exposed (cf. Whittlesea, 1993; see also Goldinger, Kleider, & Shelley, 1999); however, it was not significantly above chance in Block 1, Block 2, or overall: Block 1, $M = .47$, $SEM = .02$, $t(45) = -1.38$, $p = .18$; Block 2, $M = .50$, $SEM = .02$, $t(45) = -0.19$, $p = .85$; overall, $M = .50$, $SEM = 0.01$, $t(45) = 0.06$, $p = .95$. Together, these results suggest that responding in the contrast task was influenced by the contrast between a word and the background and hardly at all by whether it was familiar (i.e., repeated).

The key result from this experiment was that the sensitivity of the direct task to familiarity was significantly greater than the sensitivity of the indirect task. Contrary to the findings of Merikle and Reingold (1991), the sensitivity of the direct task was greater than chance in Blocks 1 and 2, whereas the sensitivity of the indirect task was at chance in these blocks. The results therefore represent a complete failure to replicate their demonstration of unconscious memory. What was the reason for the null priming effect in the contrast task? One possibility, as indicated from the previous analysis of each task's sensitivity to contrast, is that the difference in contrast levels may have interfered with any effect of prior word exposure on high or low responding. It is also possible that the contrast task used here is not a sensitive indirect test of memory. These two possibilities were addressed in the next experiment.

2.2 Experiment 2

In Experiment 2, priming in the contrast task for cued and uncued study words was investigated. Unlike Experiment 1, every word in Experiment 2 was presented against a mask of the same density at test. If differences in the contrast levels prevent priming, then a priming effect may emerge for uncued words in Experiment 2. However, if contrast differences do not interfere with priming, and instead the representation of uncued words formed at study is not sufficient to support priming in this task, then one would expect to replicate the null effect from Experiment 1. A cued word condition (cf. Merikle & Reingold, 1991; Experiment 1) was included to determine whether the contrast task is sensitive to the familiarity of these words, and should be considered a sensitive indirect test of memory. Priming is deemed more likely for these words than for uncued words because more attention is paid to them at study (e.g., Crabb & Dark, 1999).

2.2.1 Method

There were three major differences from the general method of Experiment 1: (a) At test, participants made judgments about cued and new words (cued condition) or uncued and new words (uncued condition); (b) a fixed mask density (50%) was used on every test trial and was randomly generated for each trial; and (c) sensitivity was only measured in a contrast task.

The study phase procedure was the same as that used in Experiment 1 except that participants in all experiments were positioned approximately 100 cm from the screen at the start of the experiment, and naming responses were recorded so they could be later checked for accuracy. All other aspects of the method were the same as those used in Experiment 1.

Forty participants were randomly allocated to the uncued (n = 20) or cued (n = 20) word conditions. Their ages ranged from 18 to 27 years, with a mean of 20.1 years. Each participant in this and subsequent experiments in this chapter was recruited from a University College London psychology department subject database, was tested in individual testing sessions, was paid £4 for participation, and was told that the experiment concerned word perception. All reported normal or corrected-to-normal vision and reported English as their first language.

2.2.2 Results and Discussion

The results were analyzed in a fashion similar to that used in Experiment 1.[4] For every participant in the cued and uncued conditions, sensitivity (*A′*) to familiarity was calculated at each test block of 32 trials, the means of which are displayed in Figure 2-3. A 2 x 3 mixed ANOVA with Cuing (uncued, cued) and Block (Blocks 1, 2, 3) as factors revealed a significant main effect of Cuing, $F(1, 38) = 5.04$, $p < .05$, indicating that sensitivity to familiarity was greater in the cued than in the uncued condition. Similar effects of manipulations of selective attention at encoding on repetition priming have been documented in the attention and repetition priming literature (Crabb & Dark, 1999; MacDonald & MacLeod, 1998; Mulligan, 2002; Phaf, Mul, & Wolters, 1994). There was also a significant main effect of Block, $F(2, 76) = 3.47$, $p < .036$, indicating that sensitivity changed across blocks; however, Block did not interact with Cuing, $F(2, 76) < 1$.

In the uncued condition, the sensitivity of the contrast task was not significantly greater than chance in Blocks 1 or 2 or overall: Block 1, $M = .50$, $SEM = .03$, $t(19) = -0.07$, $p = .95$; Block 2, $M = .54$, $SEM = .03$, $t(19) = 1.32$, $p = .20$; and overall, $M = .49$, $SEM = .02$, $t(19) = -0.53$, $p = .53$. It was marginally significantly subchance in Block 3, $M = .44$, $SEM = .03$, $t(19) = -2.09$, $p = .051$. In the cued condition, however, sensitivity was significantly greater than chance overall, $M = .55$, $SEM = .02$, $t(19) = 2.90$, $p < .01$, but not at each block, Block 1, $M = .56$, $SEM = .03$, $t(19) = 1.94$, $p = .07$;

[4] Study phase responses were checked after the experiment; because practically no errors (e.g., incorrectly naming the uncued word) were made in this and subsequent experiments in this chapter, no further analysis was conducted upon the errors.

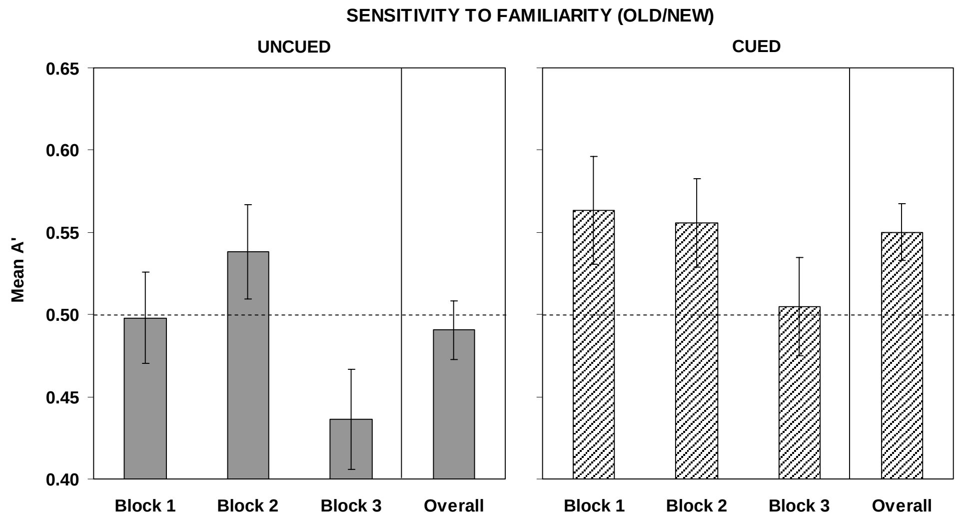

Figure 2-3. Mean sensitivity (*A*') of the contrast task to familiarity at each test block and overall in Experiment 2. Left panel: data for words that were uncued at study. Right panel: data for words that were cued at study. (Bars indicate standard errors.)

Block 2, $M = .56$, $SEM = .03$, $t(19) = 2.06$, $p = .054$; and Block 3, $M = .50$, $SEM = .03$, $t(19) = 0.16$, $p = .87$.

The null uncued priming effect in this experiment was obtained despite a constant mask density being used on every test trial. This suggests that the null effect in Experiment 1 was also not due to interference from differences in the contrast level. The results also suggest that, as found by Merikle and Reingold, the contrast discrimination task is a sensitive indirect task: Indeed, the magnitude of cued word priming obtained overall ($M = .55$) was comparable to that obtained by Merikle and Reingold (approx. $M = .55$ overall). This result supports the notion that prior exposure to words can influence judgments of perceptual contrast between a word and the background mask. However, more important, the results support the finding from Experiment 1 that, for uncued words, even when a constant mask density is used on each trial, the sensitivity of the contrast task does not significantly differ from chance and would therefore not exceed that of the comparable direct task.

The finding that recognition for uncued words was greater than chance overall in Experiment 1 but at chance in Blocks 1 and 2 of Merikle and Reingold's experiments raises the possibility that the participants used here may have been more motivated than Merikle and Reingold's in the recognition task. It could be argued that for true comparability with Merikle and Reingold, the sensitivity of the contrast task needs to be shown to be no greater than the sensitivity of the recognition task when recognition task sensitivity is at or closer to chance.

2.3 Experiment 3

The aim of Experiment 3 was to reduce the sensitivity of the recognition task to chance for uncued words and then observe the magnitude of priming. In line with the view that attention is required for modification of long-term memory (see Cowan, 1995, for a review), reducing the study exposure duration should decrease the amount of attention paid to uncued words at encoding and therefore have a detrimental effect on recognition memory. Thus, in this experiment, cued and uncued words were presented for a shorter study exposure duration than the words were in Experiments 1 and 2, and performance was measured in both recognition and contrast tasks.

2.3.1 Method

All aspects of the design and method were the same as those for Experiment 1 with the following exceptions: (a) The exposure duration of the word pairs at study was reduced from 500 ms to 150 ms; (b) a fixed mask density (45%) was used on every test trial and was randomly generated for each trial; and (c) similar to Merikle and Reingold (1991, Experiment 2B), the frequency of the cued, uncued, and new word stimuli was set at 1 per million (Kucera & Francis, 1967). 24 participants were recruited (12 contrast, 12 recognition). Their ages ranged from 18 to 22 years, with a mean of 22.0 years.

2.3.2 Results and Discussion

The mean sensitivity of each task to familiarity is displayed in Figure 2-4. The data from Experiment 3 were analyzed in a manner similar to that used in Experiment 1. A 2 x 3 mixed ANOVA with Task and Block as factors revealed a significant main effect of Task, $F(1, 22) = 5.85$, $p < .025$, indicating that for uncued words, the sensitivity of the direct task was significantly greater than that of the indirect task. No significant effect of Block, $F(2, 44) < 1$, or interaction between the factors, $F(2, 44) < 1$, was obtained.

Sensitivity to familiarity was significantly greater than chance in the recognition task overall, $M = .54$, $SEM = .01$, $t(11) = 2.82$, $p < .02$, but not when considered at each block: Block 1, $M = .56$, $SEM = .04$, $t(11) = 1.62$, $p = .13$; Block 2, $M = .49$, $SEM = .03$, $t(11) = -0.17$, $p = .87$; Block 3, $M = .55$, $SEM = .03$, $t(11) = 1.55$, $p = .15$. It is worth noting that decreasing the study exposure duration was effective in reducing recognition performance compared with the recognition group in Experiment 1. Recognition was significantly lower in this experiment, $t(39) = 1.85$, $p < .05$ (one-tailed, correcting for unequal variances). Again, the contrast task was not found to be sensitive to familiarity either overall or at each block: overall, $M = .48$, $SEM = .02$, $t(11) = -0.97$, $p = .37$;

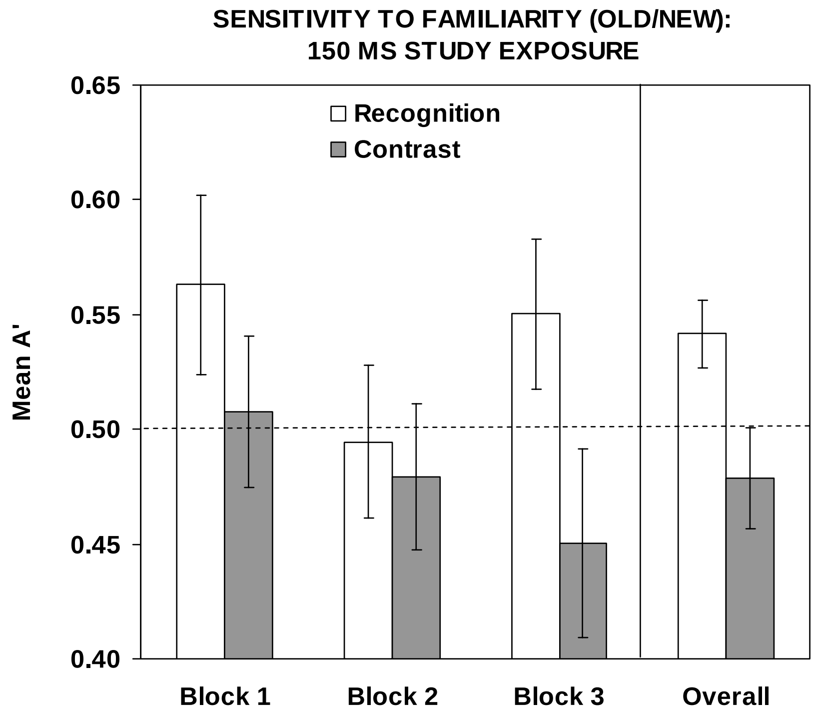

Figure 2-4. Mean sensitivity (*A*') of the contrast and recognition tasks to familiarity at each test block and overall in Experiment 3. (Bars indicate standard errors.)

Block 1, $M = .51$, $SEM = .03$, $t(11) = 0.24$, $p = .82$; Block 2, $M = .48$, $SEM = .03$, $t(11) = -0.65$, $p = .53$; and Block 3, $M = .45$, $SEM = .04$, $t(11) = -1.21$, $p = .25$. The greater variability in sensitivity across blocks in this experiment and in Experiment 2, compared with that in Experiment 1, although not reliable, can probably be attributed to the smaller number of participants in each experimental condition.

The results from Experiment 3 indicate that for uncued words, even when the study exposure is reduced to 150 ms, the overall sensitivity of the recognition task is still significantly greater than chance and significantly greater than that of the indirect task, which was at chance, thus replicating the results of Experiments 1 and 2.

2.4 Experiment 4

Despite the study manipulation used in Experiment 3, recognition memory for uncued words remained above chance; thus, a more severe manipulation of attention at encoding was used in this experiment to minimize the likelihood of processing the uncued word to a sufficient depth necessary to support recognition performance. First, a red-lined box and red arrows cued the location of the cued word; second, this location was precued; third, the study exposure duration was reduced to 100 ms; and fourth, immediately after the presentation of the target display at study, the location of the uncued word was backward-masked. In addition, given that the pattern of the results thus far contradicts those of Merikle and Reingold, the main findings from Experiments 1 to 3 were replicated again using the original study exposure duration (500 ms).

2.4.1 Method

In the 100 ms study exposure condition, the new sequence of events on each study trial was as follows: (a) A fixation dot was on screen until participants initiated the trial by pressing *Enter*; (b) the fixation dot was replaced by a blank field for 200 ms; (c) a red-lined box and two red arrows precued the location of the cued word for 150 ms; (d) the target display was presented for 100 ms, the cued word appearing within the red-lined box and arrows; and (e) on the offset of the target display and cue, the location of the uncued word was backward-masked for 500 ms with hash marks (########). All other aspects of the design and method of this condition were the same as those of Experiment 3. A second condition was added, which was identical to that used in Experiment 3, except that the study exposure duration was 500 ms.

There were 36 participants (18 contrast, 18 recognition) in the 100 ms study exposure duration condition and 24 participants (12 contrast, 12 recognition) in the 500 ms study exposure duration condition.

2.4.2 Results and Discussion

2.4.2.1 Study Exposure Duration of 100 ms

The sensitivity of each task to familiarity is shown in Figure 2-5. A 2(contrast, recognition) x 3(Block 1, 2, 3) mixed ANOVA revealed no main effect of Task, $F(1, 34) < 1$; Block, $F(2, 68) = 2.50$, $p = .09$; or interaction, $F(2, 68) = 1.35$, $p = .27$, indicating that sensitivity did not significantly differ in the recognition or contrast tasks overall, nor did sensitivity in each task vary reliably across blocks.

Sensitivity to familiarity in the contrast task did not significantly differ from chance at each block or overall: Block 1, $M = .50$, $SEM = .03$, $t(17) = 0.12$, $p = .90$; Block 2, $M = .49$, $SEM = .03$), $t(17) = -0.43$, $p = .67$; Block 3, $M = .51$, $SEM = .03$, $t(17) = 0.25$, $p = .81$; and overall, $M = .50$, $SEM = .02$, $t(16) = 0.01$, $p = .99$. Similarly, sensitivity in the recognition task was not significantly greater than chance in Block 1, Block 2, or overall: Block 1, $M = .49$, $SEM = .03$, $t(17) = -0.19$, $p = .85$; Block 2, $M = .46$, $SEM = .03$, $t(17) = -1.27$, $p = .22$; and overall, $M = .51$, $SEM = .02$, $t(17) = 0.67$, $p = .52$, but was significantly greater than chance in Block 3, $M = .58$, $SEM = .03$; $t(17) = 2.67$, $p = .016$. This apparent hypermnesia effect from Block 2 to Block 3 was confirmed by a significant paired-sample *t*-test, $t(17) = 2.24$, $p < .05$, and replicates Merikle and Reingold's finding of hypermnesia across these test blocks. This finding

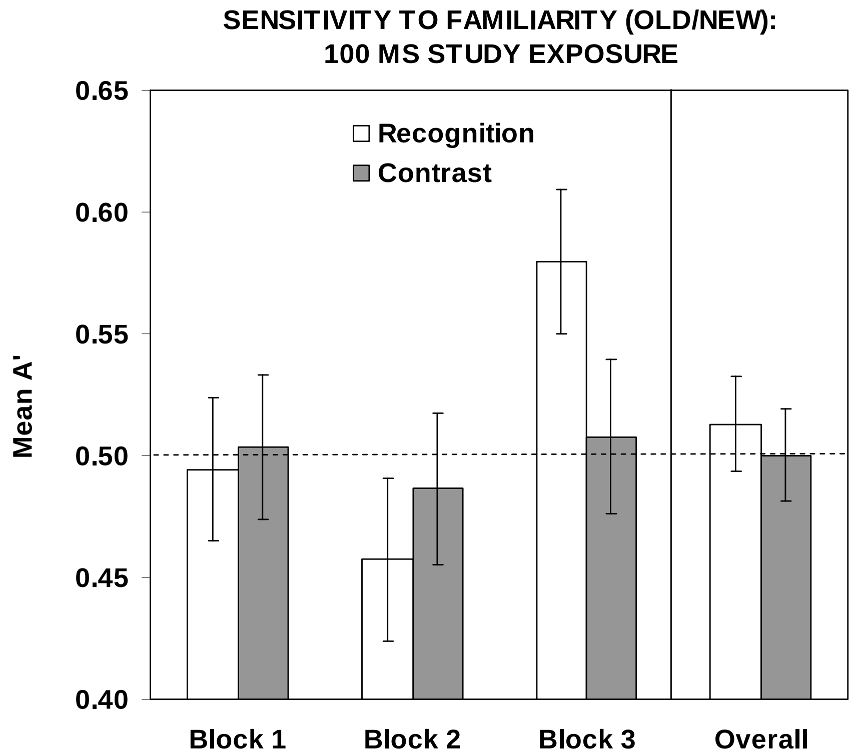

Figure 2-5. Mean sensitivity (*A*') of the contrast and recognition tasks to familiarity at each test block and overall in Experiment 4 (100 ms study exposure). (Bars indicate standard errors.)

provides some support for Merikle and Reingold's suggestion that participants may have changed their strategy as trials progressed in the recognition phase.

The key finding, however, was that the stronger manipulation of attention used at encoding was successful in decreasing recognition memory to chance in Block 1, Block 2, and overall, thus achieving the desired comparability to Merikle and Reingold's recognition performance for uncued words. Despite this, no repetition priming was observed. In conclusion, when direct sensitivity is at chance, indirect sensitivity is similarly at chance.

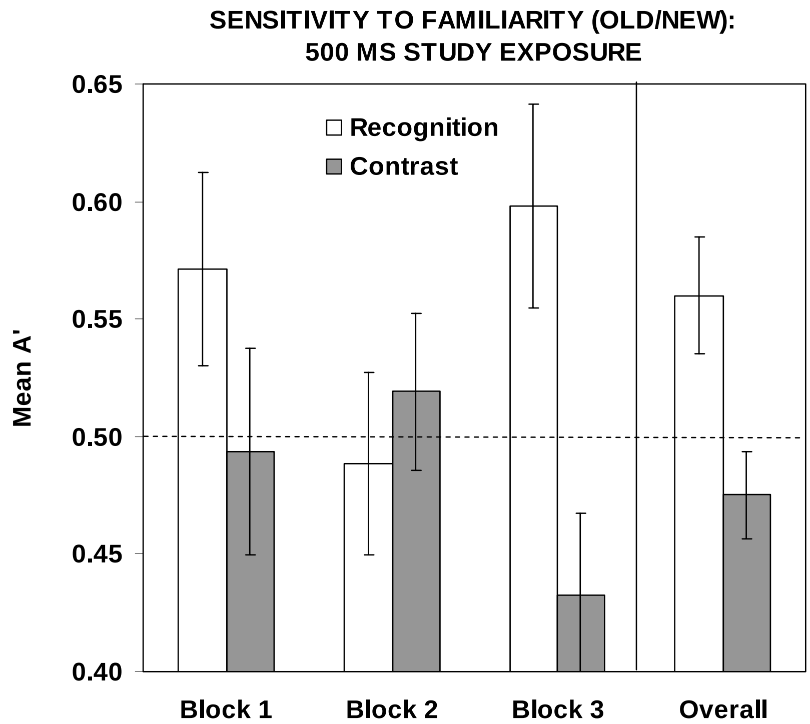

Figure 2-6. Mean sensitivity (*A*') of the contrast and recognition tasks to familiarity at each test block and overall in Experiment 4 (500 ms study exposure). (Bars indicate standard errors.)

2.4.2.2 Study Exposure Duration of 500 ms

The mean sensitivity of each task to uncued words is displayed in Figure 2-6. A 2 (contrast, recognition) x 3 (Blocks 1, 2, 3) mixed ANOVA revealed a significant effect of Task, $F(1, 22) = 6.95$, $p = .02$, indicating that the sensitivity of the direct task was greater than the sensitivity of the indirect task. No main effect of Block, $F(2, 44) < 1$, was found, nor did Task significantly interact with Block, $F(2, 44) = 2.69$, $p = .08$.

Sensitivity to familiarity in the recognition task was above chance overall, $M = .56$, $SEM = .02$, $t(11) = 2.44$, $p < .05$, and in Block 3, $M = .60$, $SEM = .04$, $t(11) = 2.25$, $p < .05$, but not in Blocks 1 or 2: Block 1, $M = .57$, $SEM = .04$, $t(11) = 1.73$, $p = .11$; and

Block 2, $M = .49$, $SEM = .04$, $t(11) = -0.30$, $p = .77$. Sensitivity to familiarity in the contrast task did not differ from chance at each block or overall: Block 1, $M = .49$, $SEM = .04$, $t(11) = -0.14$, $p = .89$; Block 2, $M = .52$, $SEM = .03$, $t(11) = 0.57$, $p = .58$; Block 3, $M = .43$, $SEM = .03$, $t(11) = -1.95$, $p = .08$; and overall, $M = .48$, $SEM = .02$, $t(11) = -1.34$, $p = .21$. The results of this condition are in concordance with those of Experiments 1–3: For uncued words, the sensitivity of the recognition task was significantly greater than the sensitivity of the contrast task, which was at chance.

2.5 Power of Experiments 1–4 to Obtain Priming of Uncued Words

Merikle and Reingold found that the priming effect for uncued words was largest in Block 1. Collapsing across Experiments 1–4, priming was at chance in this block, $M = .50$, $SEM = .01$, $t(114) = 0.11$, $p = .91$. With the mean sensitivity ($A' = .54$) and standard deviation (.13) of Merikle and Reingold's contrast task in Block 1 (from Experiments 2A and 2B combined) as an estimate of the maximum priming effect for uncued words, the power of the contrast task in this study to detect this effect, collapsed across all experiments, was .91 (one-tailed).

2.6 Discussion of Experiments 1–4

The primary aim of Experiments 1 to 4 was to replicate key evidence for the existence of unconscious memory (Merikle & Reingold, 1991). According to the logic of the relative sensitivity approach (Reingold & Merikle, 1988), evidence for unconscious memory is revealed whenever the sensitivity of an indirect task exceeds that of a comparable direct task. Despite adopting the same paradigm and procedures as Merikle and Reingold (1991), no evidence for unconscious influences of memory was found. Crucially, across four experiments, the sensitivity of the indirect task to uncued words was never greater than the sensitivity of the direct task. In contrast to Merikle and Reingold's findings, and despite ample statistical power, contrast judgments were not sensitive to the influence of uncued study words when recognition was greater than chance in Experiments 1, 3, and 4 or even when recognition was reduced to chance in Experiment 4. The results of Experiment 2 showed that the contrast discrimination task is a sensitive indirect test of memory in that it was sensitive to the influence of cued study words (cf. Merikle & Reingold, 1991, Experiment 1).

Chance recognition is a difficult outcome to obtain, as indicated by these and other researchers' findings. For example, not only was recognition greater than chance in Experiments 1 and 4 (500 ms exposure duration), but it remained so in Experiment 3 with exposure durations vastly shorter than those used in the Merikle and Reingold procedure. It was only in Experiment 4 (100 ms exposure duration), with extra procedures for ensuring that attention was withdrawn from the uncued words, that recognition was reduced to chance (in Blocks 1 and 2, at least). Similarly, Crabb and Dark (2003) showed that recognition memory persisted despite high perceptual loads and short exposure durations at encoding. For example, when four words were presented on every study trial (and participants had to make a response if one of the words was a target word), recognition memory for these words remained reliably greater than chance, even when the exposure duration was reduced from 600 ms to 200 ms. It is important to note that without the chance-level recognition performance in Merikle and Reingold's study, their data would not show a priming effect that was greater than the recognition memory effect and, hence, would not provide evidence for unconscious memory.

Numerous other studies support the priming results of Experiments 1 to 4 and have shown that when recognition is at or approaching chance, repetition priming effects similarly diminish or at least do not exceed recognition performance (e.g., Hawley & Johnson, 1991, Experiment 2; MacDonald & MacLeod, 1998; Moscovitch & Bentin, 1993; Mulligan, 2002). For example, in a similar encoding paradigm to the current experiments, MacDonald and MacLeod (1998, Experiment 3) cued one of two words on each study trial by presenting it in a specific color. For uncued words, they managed to obtain chance recognition memory but found no repetition priming in a rapid reading task. Indeed, as others have noted (e.g., Butler & D. C. Berry, 2001), few studies have demonstrated repetition priming in the absence of recognition memory, and as mentioned in the introduction to the chapter, many demonstrations have proven difficult to replicate.

An important question is why the results of Experiments 1–4 differ so markedly from those of Merikle and Reingold. A study by Whittlesea and Price (2001) suggests that performance on direct and indirect tests of memory depends largely on the extent to which the instructions differentially hinder or facilitate the adoption of strategies that differ in the extent to which they allow probing of specific memory representations. By this alternative interpretation, it is the difference in strategy (i.e., an analytic or nonanalytic strategy) elicited by each task that mediates direct and indirect task performance (Whittlesea & Price, 2001; see also M. Snodgrass, 2004 for a discussion). It is possible that minor procedural differences between the Merikle and Reingold study and this one caused participants to adopt different strategies in Merikle and Reingold's, leading to the difference in findings. For example, if the instructions in the present experiments happened to encourage an analytic strategy at test (because, e.g., participants were encouraged to do their best in making their judgments), then this, as Whittlesea and Price showed, could have eliminated evidence for the influence of prior study in the indirect test, although it is less clear how a more analytic strategy would explain better recognition performance in the direct test relative to that of Merikle and Reingold.

Other procedural differences may have contributed to the differences in findings: The visual angles of the words in the study phase were slightly larger here than in Merikle and Reingold's experiments, particularly in Experiment 1, which means that uncued words may have been more visible in Experiments 1–4, even when participants were fixating on the cued words. Unlike Merikle and Reingold, participants were not required to use a chin rest in the study phase of the experiments, so it is possible that participants moved in the study phase and there was greater variability in the visual angle. Although these criticisms are valid, unless these differences varied systematically between task conditions, it is difficult to see how they could have produced the observed differences.

In conclusion, the results of Experiments 1–4 question a key pillar of evidence in support of unconscious memory. It is important to note that the results do not challenge or undermine the logic of the relative sensitivity approach in demonstrating unconscious memory and also that a failure to demonstrate unconscious memory does not constitute evidence against its existence. However, since no-one doubts that memories can be conscious, unless convincing, reliable evidence for the existence of unconscious memory can be provided, then the principle of parsimony suggests that we should assume that there are only conscious forms of memory. Thus, in the absence of such evidence, the results of Experiments 1–4 are consistent with the more parsimonious notion that the content of the memory supporting performance on direct and indirect tasks is accessible to awareness (Kinder & Shanks, 2001, 2003; Perruchet & Vinter, 2002; Shanks & St. John, 1994).

Chapter 3: A Single-System Signal-Detection Model of Priming and Recognition

In this chapter a model is presented in which priming and recognition are driven by a single memory source. The model is conceptually very similar to standard signal-detection models of recognition judgments and their latencies (Pike, 1973; Ratcliff & Murdock, 1976; Stretch & Wixted, 1998) and extends previous work with this type of model (Shanks, 2005; Shanks & Perruchet, 2002; Shanks et al., 2003). It is important to note from the outset that the model is not one of the priming and recognition tasks themselves and the mechanisms involved in them; instead the model is specified at a more abstract level, and is principally one of the *influence of memory* on performance. The model starts with the assumption that, at test, both old and new items are associated with a memory strength variable called familiarity f. f is a normally distributed random variable:

$$f \sim N(\mu, \sigma_f) \qquad (3\text{-}1)$$

which, because of prior exposure, is assumed to have a greater mean value for old items (μ_{old}) than for new items (μ_{new}). For a given item, the same value of f contributes to both recognition and priming tasks (which is what makes it a single-system model). Importantly, the value of f is subjected to independent sources of random noise for each task. The judgment made during a recognition task depends on the variable J_r:

$$J_r = f + e_r \qquad e_r \sim N(0, \sigma_r) \qquad (3\text{-}2)$$

where e_r is a normally-distributed random variable with mean of zero and standard deviation of σ_r.

To simulate accuracy in recognition, J_r is compared against a criterion value, C. If the value of J_r exceeds the criterion for a given item, then it will be judged "old", otherwise it will be judged "new". In principle C is free to vary, however, for the sake of simplicity, C is set to the midpoint between the means of the old and new familiarity distributions, i.e., $(\mu_{\text{new}} + \mu_{\text{old}})/2$. Thus, the greater an item's value of f, the greater the likelihood that it will be judged old in a recognition task. The addition of e_r to f in Equation (3-2) is formally equivalent to adding e_r to the decision criterion C. The σ_r parameter can therefore be taken to represent variability in the placement of the decision criterion from item-to-item (Wickelgren, 1968).

The response made during a priming task depends on the variable J_p which, like J_r, is also driven by f:

$$J_p = f + e_p \qquad e_p \sim N(0, \sigma_p) \qquad (3\text{-}3)$$

where e_p is another source of random noise which is independent of e_r and has a mean of zero and standard deviation σ_p. e_p represents measurement error, or more generally, the influence of non-memorial factors on performance in priming tasks (e.g., Ostergaard, 1992).

J_p can be transformed to simulate responses for different types of priming tasks. In Chapter 4, J_p is used to simulate correct/incorrect identification responses in a perceptual identification task, and in Chapters 5 to 7, a linear transformation is applied to simulate identification RTs in a perceptual clarification task. Regardless of the transformation, it is assumed that greater values of f lead to higher levels of performance in the priming task (e.g., a greater likelihood of correctly identifying a given item in a perceptual identification task, or a greater likelihood that an item will have a short identification RT in a perceptual clarification task). The model simulates priming effects because μ_{old} is generally greater than μ_{new}, and performance will therefore tend to be better for old items.

An important feature of this model is that the variance of the noise associated with priming tasks is typically greater than that associated with recognition tasks, i.e., $\sigma_p > \sigma_r$. This is because performance in priming tasks is, in general, believed to be influenced by a larger range of non-memory-related factors than is recognition (Kinder & Shanks, 2001, 2003; Ostergaard, 1992, 1998). In support of this, the reliability coefficients associated with performance in priming tasks are often found to be lower than those of recognition tasks (Buchner & Brandt, 2003; Buchner & Wippich, 2000; Meier & Perrig, 2000). As the simulations in Chapters 4 to 7 will show, this assumption is important for the model to reproduce a wide range of results (particularly in Chapter 4). For example, one straightforward consequence of the larger noise variance for priming is that the model predicts that the recognition task will be more sensitive to f than the priming measure. In other words, priming will not occur in the absence of recognition. This is precisely the trend that was observed in Experiments 1–4 in the previous chapter.

Chapter 4: Attention at Encoding

Studies that have used manipulations of attention at encoding have produced a variety of dissociations between priming and recognition that have been taken as evidence for a multiple systems view and may therefore be challenging for the single-system model to account for. This evidence is now considered.

Although it is fairly clear that attentional manipulations at encoding impair recognition, the evidence regarding the influence that these manipulations have on priming is mixed. Some studies have obtained dissociations, finding that attentional manipulations affect recognition but not priming (Jacoby, Woloshyn, & Kelley, 1989; Kellogg, Newcombe, Kammer, & Schmitt, 1996; Mulligan & Hartman, 1996; Parkin, et al, 1990; Parkin & Russo, 1990; Russo & Parkin, 1993; Schmitter-Edgecombe, 1996a, 1996b; Szymanski & MacLeod, 1996; Wolters & Prinsen, 1997). These studies have typically manipulated attention by requiring participants to perform some concurrent task while encoding items (e.g., tone-monitoring, digit-monitoring, performing addition sums, or maintaining a string of digits in working memory; dual-task manipulations). For example, Parkin et al. (1990) found that recognition was impaired in a condition in which participants monitored a series of tones during encoding relative to a non-divided attention condition where participants simply read the items. However, priming in a word-fragment completion task was unaffected by the dual-task manipulation. Parkin et al. (1990) took this as evidence that implicit memory does not depend on attention at encoding, but explicit memory does. Similar conclusions from dissociations such as this have been drawn by others (e.g., Kellogg et al., 1996; Parkin & Russo, 1990; Wolters & Prinsen, 1997). This type of dissociation has been demonstrated with a variety of priming tasks which include: word-fragment completion (Mulligan, 1998; Mulligan & Hartman, 1996), fame-judgments (Jacoby et al., 1989), picture-fragment completion (Parkin & Russo, 1990), word-stem completion (Wolters & Prinsen, 1997), lexical decision (Kellogg et al., 1996), and perceptual identification (Mulligan, 2003, Experiment 1; Schmitter-Edgecombe, 1996a, 1996b; but see Mulligan, 2003, Experiments 2–4).

A similar dissociation was also found by Szymanski and MacLeod (1996) who used a Stroop manipulation at study. Szymanski and MacLeod (1996) found that priming in a condition in which words were named at study (full attention condition) did not reliably differ from priming in a condition in which the colour of the text was named at study (reduced-attention condition). Recognition, however, was impaired in the reduced attention condition. Szymanski and MacLeod (1996) took this result to support the distinction between implicit and explicit memory. Other studies, however, have failed to replicate this result (Mulligan & Hornstein, 2000; Rajaram, Srinivas, & Travers, 2001; Stone, Ladd, & Gabrieli, 2000; Stone, Ladd, Vaidya, & Gabrieli, 1998). For example, Stone et al. (1998) found that priming in a perceptual identification task was severely reduced in a colour naming condition relative to a word-naming condition. Effects from studies using the Stroop task are therefore mixed.

Studies that have found effects on priming (Bentin, Moscovitch, & Nirhod, 1998; Crabb & Dark, 1999, 2003; Eich, 1984; Hawley & Johnston, 1991; Johnston & Dark, 1985; MacDonald & MacLeod, 1998; Mulligan, 2002, 2003; Phaf, Mul, & Wolters, 1994; see also Experiment 2 in Chapter 2) often use selective attention manipulations at study, in which attention is diverted from the target stimulus to distractor stimuli that are presented synchronously at study. For example, Phaf et al. (1994) presented a pair of words, one to the left and one to the right of fixation, on each study trial. After a 200 ms delay, one of the words was cued with an arrow and participants were instructed to read this word aloud. The offset of the stimulus display was triggered by the onset of the vocal response. In subsequent perceptual identification and word-stem completion tasks, significant priming for cued and uncued (non-arrowed) words was obtained, but priming for uncued words was less than that of cued words. Similarly, other studies using selective attention manipulations have reported decreases in priming in tasks such as lexical decision (Bentin et al., 1998), perceptual identification (Crabb & Dark, 1999, 2003; Mulligan & Hornstein, 2000, Experiment 4; Mulligan, 2002, Experiment 2), the homophone spelling task (Eich, 1984), perceptual clarification (Johnston & Dark, 1985), word-stem completion (Crabb & Dark, 1999), and naming (MacDonald & MacLeod, 1998).

Thus, with regard to functional dissociations, the evidence suggests that selective attention manipulations are more likely to have effects on priming, whereas dual-task manipulations are more likely to produce dissociations. I return to this possible difference between selective and dual-task manipulations in the Discussion of this chapter. Despite this inconsistency, it is fairly clear that attentional effects are weaker on priming than on recognition.

Also challenging for the single-system model are reports of priming for less-attended items occurring in the absence of recognition in normal adults. Studies that have obtained this dissociation have typically employed selective-attention manipulations of attention at study (e.g., Bentin et al., 1998; Eich, 1984; Johnston &

Dark, 1985; Merikle & Reingold, 1991; Vuilleumier, Schwartz, Duhoux, Dolan, & Driver, 2005). For example, Bentin et al. (1998) presented pairs of words on each trial but cued one of the pair members by presenting it in a certain colour. Priming for uncued words was obtained in a lexical decision task when recognition memory was close to chance.

Finally, some studies using attentional manipulations at encoding have also reported stochastic independence between priming and recognition (Eich, 1984; Parkin & Russo, 1990). As reviewed in Chapter 1, this type of evidence has been interpreted by some to be indicative of distinct systems.

In sum, manipulations of attention have produced three types of results that may be challenging for the single-system model: (a) effects of attentional manipulations on recognition but not priming (or at least greater effects on recognition than priming), (b) priming in the absence of recognition, and (c) lack of correlations between performance on priming and recognition tasks. Experiments 5–8 in this chapter aimed to test the model by using a manipulation of attention at encoding. After these experiments are presented, the model is then applied to the data.

4.1 Simulating the Effects of Attention

The effects of attention at study can be simulated in the model by varying the familiarity of old items according to the amount of attention they are assumed to receive, i.e., the value of μ_{old} is greater for attended than unattended items. What are the qualitative predictions of the model? Given that priming depends on the same familiarity value as recognition, effects of attention on priming are predicted, albeit not necessarily of the same magnitude (because of the differences in variance of the noise distributions associated with each task). The greater noise variance for priming tasks also means that it is unlikely (given a finite number of trials) that one will observe priming in the absence of recognition, for example, for less-attended items. Finally, the model predicts a correlation between priming and recognition, though this correlation can be weak, depending on the variances of the task-specific noise (σ_r and σ_p) relative to the variance and difference in means of the familiarity distributions (σ_f and μ_{new} vs. μ_{old}) (see Simulation Study 1 below). Robust evidence contradicting these predictions would falsify the model.

4.2 Experiments 5–8

In Experiments 5–8, a selective attention manipulation was used which was very similar to the one used in Experiments 1–4. In each of the following experiments, pairs of words were presented for 500 ms, one above the other, on each study trial. Arrows cued one of the pair, which was to be read aloud. At test, cued or uncued (non-arrowed) words from the study phase were presented together with new words in either an old-new recognition task or a perceptual identification task. This priming task was used to measure priming because: (i) it has been evaluated favourably as a perceptual priming task (Roediger & McDermott, 1993), (ii) it has been reported to have a reliability that is higher than many other implicit memory tasks (Buchner & Wippich, 2000), (iii) the task is frequently used to compare priming with recognition performance, and (iv), as detailed in the Methods section, perceptual identification performance can be measured using the same response metric as recognition. Thus, although the priming and recognition tasks differ in terms of the manner of presentation of stimuli in each task (i.e., in degraded form vs. not degraded), and also in the type of response (i.e., production of a word vs. old/new judgment), for the above reasons, dissociations between recognition and priming in the perceptual identification task might be considered to be particularly compelling evidence for multiple memory systems.

Priming and recognition for cued and uncued words was tested between participants in Experiment 5 and within-participants in Experiment 6. In Experiment 7, priming and recognition were also tested for uncued words that were presented four times at study (uncued-4 words). In Experiment 8, priming and recognition were tested within-participants for uncued words only.

4.1.1 General Methods

First, the general method of Experiments 5–8 is described. Details of the differences between each experiment are given later.

4.1.1.1 Participants

The participants in the following experiments were recruited from a psychology subject database, reported having normal or corrected-to-normal vision, reported English as their first language, and were paid for their participation. There were 45 participants in Experiment 5 (*n* priming = 23, *n* recognition = 22), 26 in Experiment 6 (*n* priming/recognition = 12, *n* recognition/priming = 14), 46 in Experiment 7 (*n* priming/recognition = 23, *n* recognition/priming = 23), and 24 in Experiment 8 (*n* priming/recognition = 12, *n* recognition/ priming = 12).

4.1.1.2 Materials and Design

Each experiment was run on a computer in a sound-dampened cubicle. The experimental software was written in Visual Basic 6.0 and used ExacTicks v1.1 (Ryle Design, 1997) to achieve millisecond accuracy.

The stimuli in this and subsequent experiments were low frequency six-letter nouns (with a frequency of occurrence of one per million in Experiment 5, 1–5 per million in Experiment 6, and 1–8 per million in Experiments 7 and 8; Kucera & Francis, 1967). All word stimuli were presented in white 26 pt Arial font against a black background. The stimuli were arranged into lists for each experiment: one list for each stimulus type (cued, uncued, and new in Experiments 5, 6 and 8; cued, uncued, new and uncued-4 in Experiment 7) in each test phase. The assignment of lists to each type of stimuli was counterbalanced across subjects according to a Latin square.

On each study trial, pairs of words were presented. One member of the word pair was cued and the other was not. There were 48 target trials (trials that contained stimuli that would later appear at test) in Experiment 5, 72 in Experiment 6, 240 in Experiment 7, and 108 in Experiment 8.

The trials of each test phase were arranged into three blocks. In Experiments 5 and 6, each block contained an equal number of cued, uncued and new item trials. In Experiment 7, an equal number of cued, uncued and uncued-4 words were presented in each block, but because of the extra type of old stimuli (uncued-4), there were twice as many new word trials in a block as there were a given type of old stimuli trials. In Experiment 8 there were an equal number of uncued and new words in each block. No indication of block transitions was given to participants. The selection of the stimuli to be presented in each block of each experiment and the order in which items were presented was randomly determined.

Thus, in Experiment 5, there were 48 trials in total for each type of stimulus; in Experiment 6, there were 36 trials for each type of stimulus; in Experiment 7 there were 24 trials for each type of study item (cued, uncued, and uncued-4) and there were 48 trials for new items; and in Experiment 8 there were 54 trials in total for each type of stimulus.

4.1.1.3 Procedure

Each participant was seated approximately 100 cm from the monitor at the beginning of every experiment. At the start of the study phase, a white fixation dot (measuring 0.4 cm in diameter and subtending approximately 0.23° of visual angle) was displayed at the centre of a black background. Participants were told to initiate each trial when they were looking at the fixation dot by pressing the ENTER key. After a trial was initiated, the fixation dot was replaced by a 200 ms blank field. The target display consisting of a pair of words was then presented for 500 ms. One word was presented 0.6 cm (0.34°) above the fixation point and one 0.6 cm (0.34°) below. Each word pair consisted of a cued and uncued word chosen randomly from the appropriate list. The cued word appeared an equal number of times above and below the fixation point and this position was randomly determined for each study trial.

Each six-letter word was approximately 3.4 cm long (1.9°) and 0.6 cm high (0.34°). The entire stimulus display measured approximately 3.2 cm (1.83°) vertically and 6 cm (3.43°) horizontally on the screen. One word of the pair was cued by a pair of arrows, and each arrow measured 0.5 cm (0.28°) in length and was located approximately 0.8 cm (0.46°) from the end of the cued word. Participants were required to read out the cued word; both accuracy and speed were emphasised in the study instructions. Study phase responses were audio-recorded to be later checked for accuracy.

A 2000 ms unfilled interval followed the target display after which the fixation dot reappeared to indicate to the participant that they could initiate the next trial. In Experiments 5, 6 and 8, trials were self-initiated in this way; in Experiment 7, however, study trials were automatically initiated by the computer: after the fixation dot had been presented for 500 ms, the sequence of events for the next trial was automatically initiated. This procedure was adopted in Experiment 7 because of the larger number of study trials relative to the other experiments and an automated study phase constrained the total study phase completion time. The first and last trials of the study phase (four trials in Experiment 5, and eight trials in Experiments 6–8) acted as primacy and recency filler trials, and none of these filler stimuli later appeared in the test phase.

On each trial of the recognition test a study-phase or new word was presented. Beneath this word the question ‘‘Is this word OLD or NEW? Press O or N’’ was displayed in blue 14 pt MS Sans Serif font. Participants were told in the instructions that an ‘‘old’’ word could be a cued or an uncued word from the first phase, and they were also informed of the relative proportions of old and new words. Participants who performed the recognition task after the priming task were told that none of the words they were about to make decisions for were presented in the priming stage and were also reminded as to the nature of the first stage. When the O or N key was pressed the display was replaced with a 1200 ms blank field and then the next word was displayed.

The task used to measure priming was a perceptual identification task. On each trial, an uncued or new word was presented very briefly. A single trial consisted of (a) the words ‘‘Get Ready’’ presented centrally in blue 12 pt MS Sans Serif font for 1000 ms, (b) a blank field for 2000 ms, (c) a white ‘+’ fixation sign for 500 ms, (d) a blank field for 500 ms, (e) the target word at the same location as the fixation sign for 33 ms, and (f) a mask consisting of a row of ‘########’s for 500 ms. The participant’s task was to then identify each target word by typing it on the keyboard.

4.1.2 General Analysis

All study responses were later checked for study errors (incorrectly naming the uncued word instead of the cued word on a single trial). The error rate in each experiment was practically zero and no further analysis of the study responses was conducted.

4.1.2.1 Comparing Recognition and Priming Using the Same Metric

In order to compare the priming and recognition tasks with the same metric, performance was measured as the hit rate minus the false alarm rate (henceforth Hits - FAs). This metric of sensitivity was chosen because it is simple and makes few assumptions (Snodgrass & Corwin, 1988); scoring the data instead using equal-variance signal-detection theory did not affect the qualitative pattern of results.[1,2] In the

[1] The choice to measure priming and recognition with Hits-FAs did not affect the conclusions of any of the experiments in this chapter, and the results for d' were also calculated. When sensitivity was analysed with d', the qualitative patterns of results in Experiments 5–8 were the same, except for the following: In analysis of Experiment 6, the task was found to significantly interact with the task order (priming/recognition, recognition/priming), $F(1, 22) = 4.61$, $p = .043$. However, the interaction between the two factors was still significant for each task-order group (Fs > 26.57, ps < .001). A

recognition task, the hit rate and false alarm rate were calculated for each stimulus type (i.e., cued/uncued) for every participant. In the priming task, identification attempts to cued, uncued and new words at the first exposure duration (33 ms) were classified as either a ‘‘hit’’ (a positive response, in the form of a correct identification of an old word), or a ‘‘false alarm’’ (a correct identification of a new word), and the hit and false alarm rates were calculated accordingly.

Given that some participants found the priming task more difficult than others, only participants who made at least 5 correct identifications at 33 ms were included in the subsequent analysis. By this criterion, in Experiment 5, one participant was excluded from the analysis and hence the total *n* for the priming group was 22. The majority of participants were well above this criterion (median = 65 out of 144 possible correct identifications at 33 ms, range: 17–114). In Experiment 6, two participants were excluded from the analysis (both from the priming/recognition task-order group) and hence the total *N* was 24 (median = 59.5 out of 108, range: 14–92). Four participants were excluded from Experiment 7 (all four identified zero words correctly) and hence

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further difference in the analysis of Experiment 6 was that the correlation between priming and recognition performance for cued items approached significance, $r(23) = .39$, $p = .057$, consistent with the prediction of the model. Furthermore, calculation of sensitivity by d' allowed calculation of each subject's criterion, C. As assumed in the model, the mean value of C (calculated from the cued hit rate and false alarm rate for each task in Experiments 5–7, and from the uncued hit rate and false alarm rate for each task in Experiment 8) did not significantly differ between tasks for any experiment (Experiment 5–7, ts < 1; Experiment 8, $t(23) = 1.49$, $p = .15$).

[2] Note that, when sensitivity is measured by d', there is an analytic solution for the sensitivities predicted by the model for the recognition task. Because it is assumed that f is a normally distributed variable and that σ_f (old) and σ_f (new) are equal, d' for the recognition task is equal to $\mu/\mathrm{sqrt}(\sigma_f^{\ 2} + \sigma_r^{\ 2})$. However, Hits - FAs was chosen to analyze sensitivity because the same assumptions were not made in the analysis of the data (plus an analytic solution for the identification task is not so tractable, see Simulation Study 1).

the total N was 42 (median = 71.5 out of 120, range: 32–115). No participants were excluded in Experiment 8 according to this criterion (median = 44.5 out of 108 in the test phase, range: 8–93).

4.1.2.2 Split-Half Correlations

Split-half correlations were used as reliability estimates of performance in the recognition and priming tasks (e.g., Buchner & Wippich, 2000). For every participant, two halves of the task were created by assigning odd number trials to the first half and even numbered trials to the second half. Following Buchner and Wippich (2000), Hits - FAs could then be calculated for each type of study word for both halves of the task. The split-half correlations were estimated as the Pearson correlations between these summary scores.

An α level of 0.05 was used for statistical tests, and t-tests were two-tailed. Tests involving repeated-measure factors with more than two levels were corrected for non-sphericity using the Greenhouse-Geisser correction.

4.2 Experiment 5

This experiment was run using a standard version of the perceptual identification task, in which the exposure duration of each test word was gradually increased until the participant identified it correctly. More specifically, the initial duration of test words was 33 ms (two screen refreshes at 60 Hz), and if the participant did not type the test word correctly, it was repeated with durations incremented by 17 ms until the participant identified it correctly (after which the next trial began). Performance in this task was scored as the proportion correct at the shortest duration (33 ms). The perceptual identification (priming) and recognition tasks were run on different groups of participants, to minimise interference between tasks. The data from this first experiment were used to set many of the free parameters of the model (see Simulation Study 1 section below).

4.2.1 Results

Inspection of the upper-left panel of Figure 4-1 indicates that the attentional manipulation had a large effect on Hits - FAs for recognition but a much smaller effect for priming. This was confirmed by a 2 (recognition, priming) x 2 (cued, uncued) mixed ANOVA, which yielded a significant interaction between these factors, $F(1, 42) = 113.29$, $p < .001$. Simple effects analyses revealed that there were significant effects of the attentional manipulation on both recognition, $t(21) = 15.90$, $p < .001$, and priming, $t(21) = 6.84$, $p < .001$. Simple effects analyses also confirmed that, for cued words, recognition was greater than priming, $t(42) = 11.91$, $p < .001$, whereas for uncued words, priming and recognition task sensitivity did not significantly differ, $t(42) = 0.28$, $p = .78$. Further analysis revealed that both priming and recognition of cued words exceeded the chance level of performance (Hits - FAs = 0; priming, $t(21) = 8.48$, $p < .001$; recognition, $t(21) = 20.16$, $p < .001$) as did that of uncued words (priming, $t(21) = 2.64$, $p < .05$; recognition, $t(21) = 3.12$, $p < .005$). The hit and false alarm rates are shown separately in Figure 4-2.[3]

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[3] Priming was also compared to recognition when analysed at the 50 ms exposure duration rather than the 33 ms duration. The reason for this was to check that the qualitative pattern of results did not differ when identification responses were scored at a different exposure duration. A 2 (cued, uncued) x 2 (priming, recognition) ANOVA revealed significant main effects of cuing, $F(1, 42) = 193.27$, $p < .001$, task, $F(1, 42) = 128.30$, $p < .001$, and also a significant interaction, $F(1, 42) = 84.58$, $p < .001$. As was

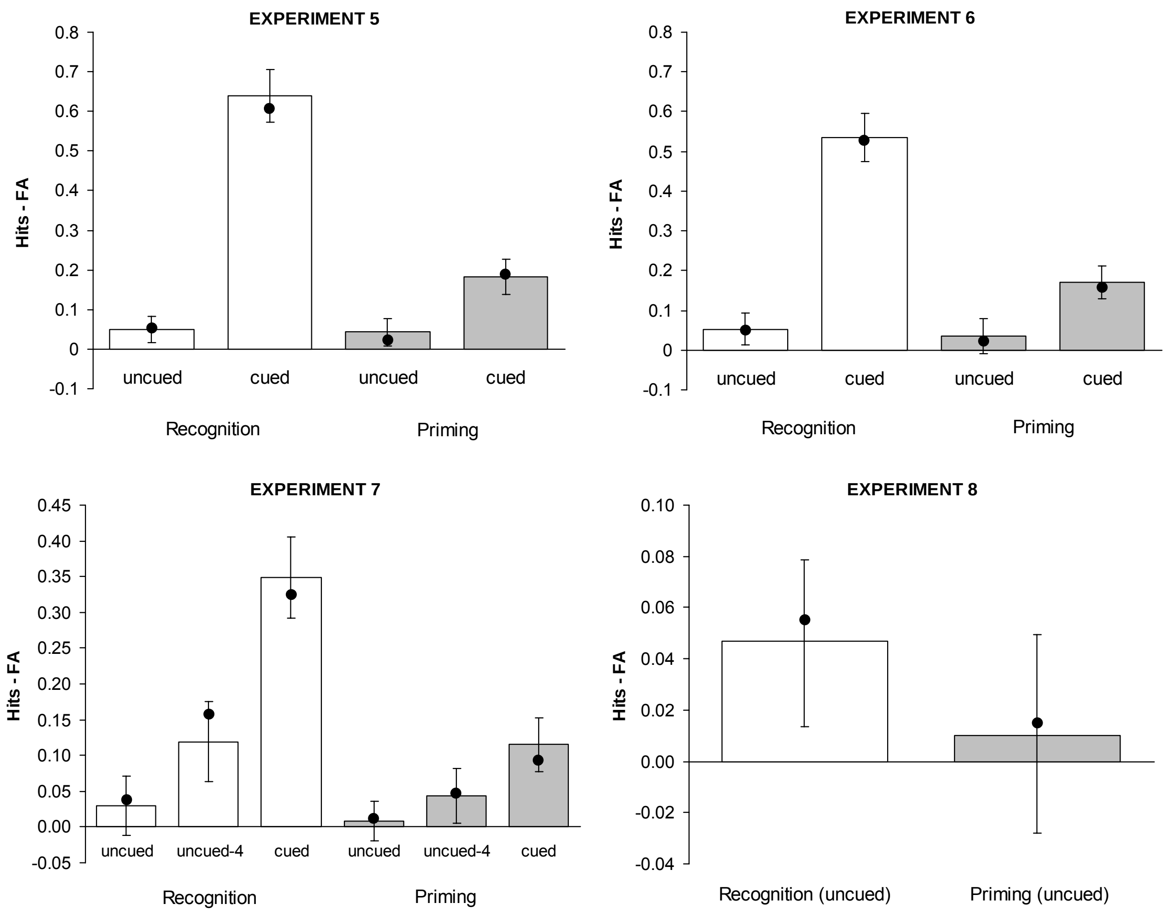

Figure 4-1. Mean sensitivity (Hits - FAs) of the priming and recognition tasks as a function of cuing in Experiments 5–8. Bars indicate experimental data (error bars indicate 95% confidence intervals); closed-circles indicate model results.

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observed at the 33 ms exposure duration, priming for cued words (M = 0.12, SE = 0.03) was significantly greater than priming for uncued words (M = -0.004, SE = 0.02), $t(21)$ = 3.42, p = .003. Priming was significantly greater than chance for cued items, $t(21)$ = 3.70, p = .001, but, unlike the analysis conducted at the 33 ms duration, there was no significant priming for uncued items, $t(21)$ = 0.22, p = .83. Split-half correlations were also calculated for identification performance scored at the 50 ms exposure duration. The split-half correlation for cued words was not reliably greater than chance for cued, $r(21)$ = -.09, p = .69, or uncued words, $r(21)$ = -.22, p = .32. This pattern of results, with the exception of the null priming effect for uncued items, is in accord with the analysis conducted at the 33 ms exposure duration. The results are also in agreement with the results for priming when collapsed across Experiments 5–8 (see Summary of Experiments 5–8 section).

4.2.1.1 Conventional Analysis of Priming in Terms of Test Word Duration

To check that the effect of attention on priming was reproduced when the perceptual identification task was analysed in a more conventional manner, the final exposure duration before the word was correctly identified was also analysed. The % priming for cued words and uncued words was calculated as 100 × (mean final new word exposure duration - mean final old word exposure duration)/mean final new word exposure duration. A paired *t*-test on the % priming scores for cued and uncued words revealed that there was a significantly larger amount of priming for cued words (M = 14.9%, SE = 1.4) than uncued words (M = 2.6%, SE = 1.4), $t(21) = 8.71$, $p < .001$. Additional analysis indicated that the amount of priming was significantly greater than chance (0%) for cued words, $t(21) = 11.10$, $p < .001$, and approached significance for uncued study words, $t(21) = 1.91$, $p = .07$. Thus, the pattern of results from the more conventional analysis of the perceptual identification task agreed with those from the sensitivity (Hits - FAs) analysis at a single duration (33 ms).

4.2.1.2 Split-Half Correlations of Recognition and Priming

The split-half correlation for cued words in the recognition task was greater than chance (0), $r(21) = .72$, $p < .001$, indicating that recognition performance for these words was reliable. In contrast, the split-half correlation for uncued words did not exceed that expected by chance, $r(21) = -.08$, $p = .73$. This suggests that performance was more reliable when sensitivity was greater. However, the split-half correlation in the priming task was not reliably greater than chance for either cued, $r(21) = .12$, $p = .60$, or uncued, $r(21) = -.10$, $p = .65$, words. This indicates that performance was generally not reliable in the priming task, even when priming was greater than chance.

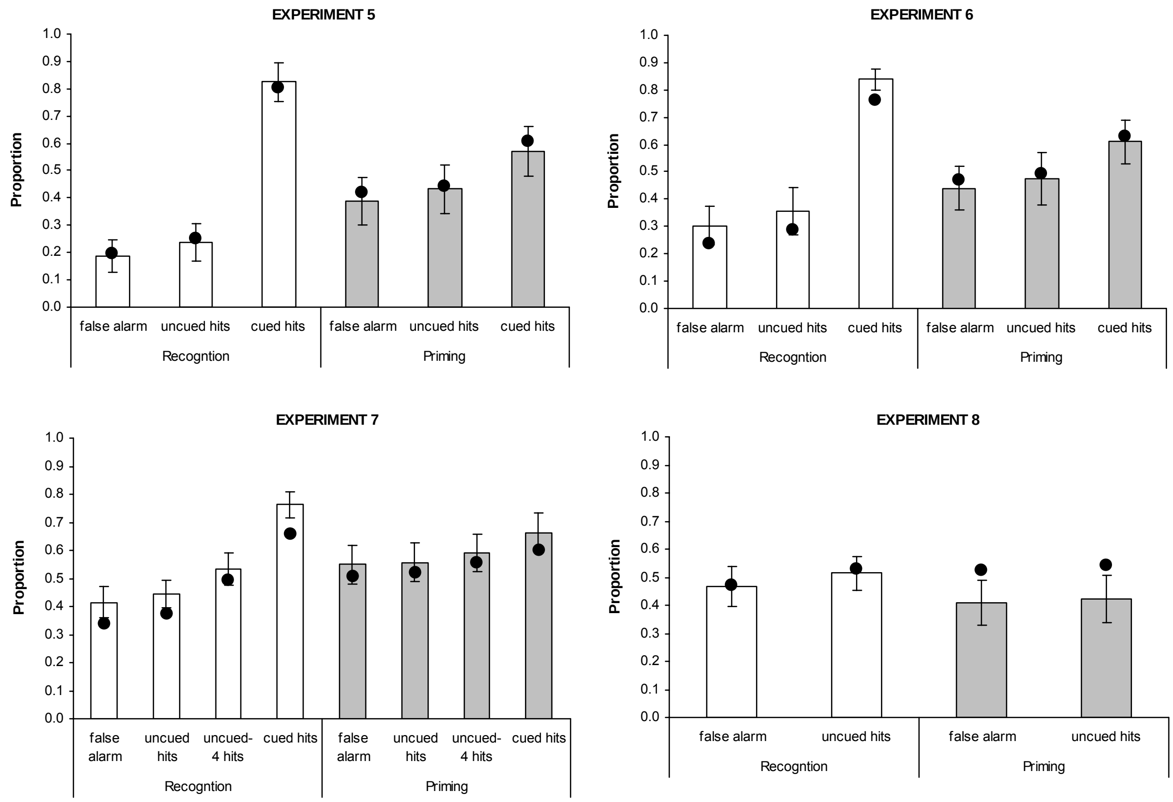


Figure 4-2. Mean proportion of old responses (Recognition task) and mean proportion of correct identifications (Priming task) as a function of test item type in Experiments 5–8. Bars indicate experimental data (error bars indicate 95% confidence intervals), closed circles indicate model results.

4.3 Experiment 6

Given that the sensitivity analysis for the priming task in Experiment 5 agreed with the more conventional analysis of test word duration, the remaining experiments measured sensitivity of priming using only a single, fixed duration of test words (33 ms). This also had the advantage of increasing the comparability of the priming and recognition tasks because each test item was only presented once. Experiment 6 was a replication of Experiment 5, but with the Test factor run within- rather than between-participants. This also permitted an analysis of the correlations across participants between performance on the priming and recognition tasks. To avoid repetition effects at test, different words were used in the priming and recognition tasks, i.e., one half of the studied words were tested in the priming task, while the other half were tested in the recognition task. To counterbalance any task order effects, half of the participants performed the priming task first, while the other half performed the recognition task first.

4.3.1 Results

The upper right panel of Figure 4-1 shows the sensitivity in each task and indicates a very similar pattern of results to those of Experiment 5, namely a large effect of the manipulation of selective attention on recognition and a much smaller one on repetition priming. A 2 (recognition, priming) x 2 (cued, uncued) x 2 (test order: priming/recognition, recognition/priming) ANOVA revealed that firstly, neither the main effect of trial order ($F < 1.00$) nor any of its interactions were significant (Fs < 2.27, ps > .15), indicating that whether priming or recognition was the first or second test phase did not result in any significant difference in performance. Secondly, like Experiment 5, a significant Cuing x Task interaction, $F(1, 22) = 57.69$, $p < .001$, was obtained. Furthermore, simple effects analyses indicated that cuing had effects on both recognition, $t(23) = 12.58$, $p < .001$, and priming, $t(23) = 5.03$, $p < .001$. Simple effects analyses also indicated that for cued words, sensitivity was greater in the recognition task than the priming task, $t(23) = 11.34$, $p < .001$, whereas for uncued words, sensitivity did not differ in the recognition and priming tasks, $t(23) = -0.58$, $p = .57$. Recognition of uncued words was significantly greater than that expected by chance (Hits - FAs = 0), *t*(23) = 2.80, *p* = .01, but, unlike Experiment 5, priming of uncued words was not, *t*(23) = 1.70, *p* = .10.

4.3.1.1 Split-Half Correlations of Recognition and Priming

Like in Experiment 5, the split-half correlation for cued words in the recognition task was greater than chance, *r*(23) = .50, *p* = .012, but the correlations for uncued words in the recognition task, *r*(23) = -.17, *p* = .43, and for cued and uncued words in the priming task (*r*(23) = -.04, *p* = .86 and *r*(23) = -.18, *p* = .40, respectively), did not exceed chance (though in this case, it should be remembered that overall sensitivity for uncued words in the priming task was not reliably greater than zero).

4.3.1.2 Correlations Between Recognition and Priming

Given that priming and recognition tasks were performed for each subject, the correlations between the two tasks could now be examined. A Pearson correlation was performed on overall performance scores for cued and uncued words in each task. The correlation did not exceed chance for either cued, *r*(23) = .21, *p* = .32, or uncued, *r*(23) = -.05, *p* = .83, words.

4.4 Experiment 7

Given that priming performance was so low for uncued words in Experiment 6, a further condition was added in Experiment 7 in which some uncued words were repeated multiple times during study. Repetition of items presented for study can increase the magnitude of priming and recognition (e.g., Ostergaard, 1998). However, it is not clear what the effect will be when the repeated items are not in the focus of attention, and whether the effects upon priming and recognition will be parallel.

Therefore an ‘‘uncued-4’’ condition was added, corresponding to words presented four times as uncued words at various intervals across the course of the study phase (each time with a different cued word). At least two study trials intervened before an uncued-4 word was repeated. This inclusion of uncued-4 words also required extra filler words to act as cued words on the trials in which uncued-4 items were presented. None of these filler items appeared in the test phases.

4.4.1 Results

Sensitivity performance is shown in the bottom left panel of Figure 4-1. Four presentations improved performance for uncued words in both priming and recognition tasks, and more so for the recognition task. Performance was analysed with a 2 (recognition, priming) x 3 (cued, uncued, uncued-4) x 2 (test order: priming/recognition, recognition/priming) mixed ANOVA. Unlike Experiment 6, there was a significant interaction of task order and type of task, $F(1, 40) = 6.29$, $p < .05$. This interaction reflected greater sensitivity of the recognition task when it was performed first than when it was performed second, $F(1, 40) = 5.27$, $p < .05$ (the sensitivity of the priming task did not significantly differ with test order, $F < 1$). Moreover, the basic cuing x task interaction remained in the priming/recognition group, $F(2, 40) = 9.49$, $p < .001$, and also in the recognition/priming group, $F(2, 40) = 11.05$, $p < .001$. Given that this interaction was not obtained in any other experiment and that it does not change the overall pattern of results, it was not explored further. No other effect involving the task order factor was significant, *F*s < 1.8, *p*s > .19.

Like Experiments 5 and 6, a significant interaction was found between Cuing and Task, $F(2, 80) = 20.52$, $p < .001$. Simple effects analysis showed that there was an effect of Cuing on priming, $F(2, 82) = 18.01$, $p < .001$, and on recognition performance, $F(2, 82) = 84.05$, $p < .001$. Sensitivity was significantly greater in the recognition task than in the priming task for cued words, $t(41) = 6.90$, $p < .001$, and uncued-4 words, $t(41) = 2.20$, $p = .03$, but not uncued words, $t(41) = 0.84$, $p = .41$.

Recognition performance was superior for uncued-4 words than uncued words (presented once), $t(41) = 3.91$, $p < .001$, but recognition performance for uncued-4 words, however, was significantly worse than for cued words, $t(41) = 7.65$, $p < .001$. Similarly, priming was (marginally) significantly greater for uncued-4 words than uncued words, $t(41) = 1.88$, $p = .07$, but priming for uncued-4 words was significantly less than that of cued words, $t(41) = 3.72$, $p < .001$. Although the effect of repetition of uncued words was numerically greater for recognition than priming, a 2 (priming, recognition) x 2 (uncued, uncued-4) ANOVA indicated a non-significant interaction between these factors, $F(1, 41) = 2.58$, $p = .12$. Thus, repeating uncued words four times had the effect of increasing the magnitude of both priming and recognition.

Further analysis revealed that priming and recognition performance for uncued words did not exceed chance (priming, $t(41) = 0.59$, $p = .56$; recognition, $t(41) = 1.44$, $p = .16$). Performance for uncued-4 words, however, was significantly greater than chance in both tasks (priming, $t(41) = 2.30$, $p = .03$; recognition, $t(41) = 4.30$, $p < .001$), as was performance for cued words (priming, $t(41) = 6.15$, $p < .001$; recognition, $t(41) = 12.42$, $p < .001$).

4.4.1.2 Split-Half Correlations of Recognition and Priming

Like in Experiments 5 and 6, the split-half correlation for cued words in the recognition task was greater than chance, $r(41) = .54$, $p < .001$, as it was also for uncued-4 words, $r(41) = .50$, $p < .001$. The split-half correlations for cued and uncued-4 words were not significant in the priming task however, $r(41) = .11$, $p = .50$, and $r(41) = .01$, $p = .94$. Finally, the split-half correlations for uncued words were not reliable in either the recognition, $r(41) = .21$, $p < .19$, or priming, $r(41) = -.11$, $p = .49$, tasks (though again, this is in the context of an overall sensitivity that was not reliably greater than zero in either case).

4.4.1.3 Correlations Between Recognition and Priming

Similar to Experiment 6, overall performance for priming and recognition was not significantly correlated for cued words, $r(41) = -.01$, $p = .95$, or uncued words, $r(41) = -.10$, $p = .54$, and was also not significant for uncued-4 words, $r(41) = -.06$, $p = .73$.

4.5 Experiment 8

An inconsistency across experiments thus far is that priming for uncued words was obtained in Experiment 5 but not in Experiments 6 or 7. In Experiment 8 this was investigated further by only presenting uncued words at test. One speculative explanation for the difference in results is that the presence of cued (attended) words at test might influence the strategies used by participants in the priming task. Even though participants in the priming task were not told that study items were being presented, it is possible that once a participant realised that some of the items are old in the priming task (which is more likely when cued items are presented at test) they then tried to perform the task by attempting to remember items from the study phase. This action could result in interference and possibly reduce the sensitivity of the priming task. Thus, this experiment tested whether priming for uncued words could be obtained when there was no interference possible from cued words.

4.5.1 Results

Sensitivity performance is shown in the bottom right panel of Figure 4-1. Like Experiment 2, recognition of uncued words was greater than priming. A 2 (recognition, priming) x 2 (task order: priming/recognition, recognition/priming) mixed ANOVA revealed no effects of task-order (both *F*s < 1). There was a trend for the sensitivity of the recognition task to be greater than that of the priming task, but the main effect of task did not reach significance, $F(1, 22) = 2.66$, $p = .12$. Further analysis revealed that performance in the recognition task was significantly greater than chance, $t(23) = 2.90$, $p < .01$, while priming was not, $t(23) = .60$, $p = .55$.

4.5.1.1 Split-Half Correlations of Recognition and Priming

Like Experiments 5–7, the split-half correlations for uncued words in the priming and recognition tasks were not greater than chance, $r(23) = -.31$, $p = .14$, and $r(23) = -.04$, $p = .87$, respectively.

4.5.1.2 Correlations Between Recognition and Priming

In contrast to Experiments 5–7, priming and recognition performance for uncued words was found to be significantly correlated in Experiment 8, $r(23) = .44$, $p < .05$.

4.6 Summary of Experiments 5–8

In relation to the three hypotheses in the introduction to the chapter: (1) all four experiments showed a reliable effect of attention on priming, i.e., greater priming for cued than uncued words, (2) no experiment showed reliable priming for uncued words when recognition for these words was at chance (i.e., no experiment showed greater performance in the priming than recognition task), (3) there were no significant correlations between priming and recognition for cued or uncued words in Experiments 5–7, although priming and recognition for uncued words was significantly correlated in Experiment 8.

Furthermore, (4) all experiments showed a greater effect of attention on recognition than priming, (5) split-half correlation estimates of performance in the priming task did not exceed chance for any type of study word in Experiments 5–8, even when overall performance was greater than chance, and (6) split-half correlation estimates of performance in the recognition task did exceed chance for cued words in Experiments 5–7 (and uncued-4 words in Experiment 7), but never for uncued words.

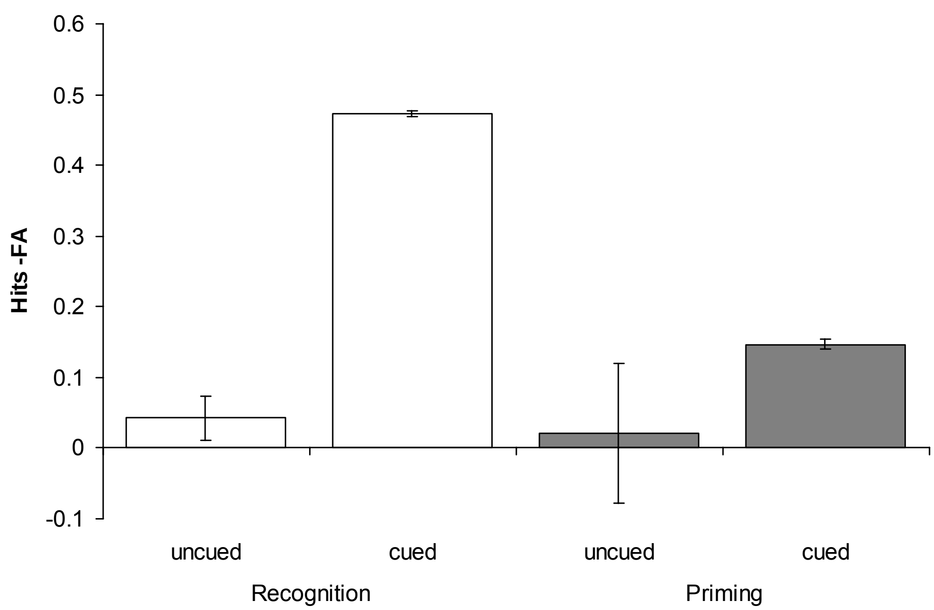


Figure 4-3. Mean sensitivity (Hits - FAs) of the priming and recognition task as a function of cuing, collapsed across Experiments 5–8 (error bars indicate 95% confidence intervals).

The combined Hits - FAs data for cued (Experiments 5–7) and uncued-once stimuli (Experiments 5–8) are presented in Figure 4-3. Similarly, combined reliability measures were calculated, and the collapsed data confirmed the pattern of reliability observed in each experiment: the split-half correlation for cued words in the recognition task was greater than chance, $r(87) = .71$, $p < .001$, but the correlations for uncued words in the recognition task, $r(111) = .004$, $p = .96$, and cued or uncued words in the priming task, $r(87) = .13$, $p = .27$, and $r(111) = -.10$, $p = .32$, respectively, did not exceed chance. Also similar to the general pattern of findings across experiments, the correlation between priming and recognition performance collapsed across experiments was not significant for cued words, $r(87) = .15$, $p = .23$, or for uncued words, $r(87) = -.06$, $p = .61$.

4.8 Simulation Study 1

The model in Chapter 3 was applied to the results of Experiments 5–8. First, the model needed to be extended in order to simulate performance in the perceptual identification task. Performance was simulated in a slightly different way to that of the recognition task to reflect the fact that there are numerous items a participant could output for a response during identification (whereas in recognition there are only two possible responses). First, a constant T was added to the value of J_p (Equation 3-3) of the item presented at test, where T represents a boost in familiarity resulting from the presentation of the item in degraded form at test.

Boosting the familiarity of the test item, regardless of its old/new status, is similar to the manner in which the effects of perceptual identification exposures are simulated in other models such as REMI (Schooler et al., 2001) and the counter model

(Ratcliff & McKoon, 1997). T is temporary in the sense that, after the test trial has been simulated, J_p returns to its previous value. To determine the participant's response, the presented item's value of J_p is compared to the values of J_p of all of the other N items in the test phase of the experiment being simulated, plus an extra N items representing non-test items (which have J_p values that are derived in an identical manner to new items). The extra N items represent the other words in a participant's vocabulary (albeit crudely). In other words, a competition takes place in parallel between all items in the participant's vocabulary, and the item with the greatest value of J_p is output for response (Nosofsky, 1985; Thurstone, 1927).[4]

Despite their different mechanisms, identification trials, like recognition trials, can still be classified using signal-detection terms: In identification, the subject's goal is to accurately identify each item. Sensitivity to the influence of memory can be shown in the task if the proportion of correct identifications to old items is greater than new items. Thus, if the item being presented is old and is also chosen as output for a response, then the item is classified as a hit (because a positive response, in the form of a correct identification, is made to an old item). If the item being presented is new and is also chosen to be output as a response, then the item is classified as a false alarm (because a positive response is made to a new item).

Unlike the absolute criterion that is employed in the simulation of the recognition data, the decision process in identification uses a relative criterion (because

[4] In principle, T could also be added to the value of J_r for each item presented during a recognition test trial, increasing the comparability of the ways in which priming and recognition performance is simulated. However this is not necessary: it would have no effect on simulated recognition performance because both the old and new distributions would be shifted by a constant amount.

whether an item is output for a response depends on the J_p values of the other items that are being compared). Other models of identification also use a relative criterion. For example, there are some similarities between the present model of identification and the counter model designed by Ratcliff & McKoon (1997). The counter model simulates the identification process using counters to represent words. When a word is flashed for perceptual identification, counts are accumulated in the counters according to the perceptual evidence from the flash and also from random noise. The counter which surpasses the maximum of the others by a criterion number of counts is output for response.

The counter model, and also other models of identification, for example, REMI, designed by Schooler et al. (2001), give detailed accounts of the mechanisms involved in identification, and can account for a range of priming results. For example, both the counter model and REMI can take into account the effects of visual similarities between items, a factor which affects identification performance (e.g., Ratcliff & McKoon, 1997). The model presented here, however, is not intended as a detailed mechanistic account of the processes involved in perceptual identification: By simulating the influence of memory upon task performance, the model is intended to serve primarily as an avenue through which the common f variable can be mapped onto both a recognition and an identification response.

Note that the calculation of J in Equations (3-2) and (3-3) has been framed in terms of sequential drawing from two distributions (first from the old/new familiarity distribution, then from the task-specific noise distribution) to illustrate the conceptual distinctions in the model. The model as presented in Chapter 3 applies when the old items that appear in priming and recognition tasks are identical: the only difference in the calculation of J for each task is the addition of task-specific noise to an item's value of f. However, when the items that appear in each task are different (i.e., as is the case in Experiments 5–8 in which there is no repeat presentation of items across test phases), the values of f in each task can be considered independent of one another. In other words, the values of J can be simulated by drawing from a single normal distribution for priming, and a single normal distribution for recognition:

$$J_{r,old/new} \sim N(\mu_{old/new}, \text{sqrt}(\sigma_f^2 + \sigma_r^2)) \quad (4\text{-}1)$$

$$J_{p,old/new} \sim N(\mu_{old/new}, \text{sqrt}(\sigma_f^2 + \sigma_p^2)) \quad (4\text{-}2)$$

Importantly, though this formulation of the model may look like a dual-system instantiation, this interpretation would be a mistake; there is no scope in this model for experimental manipulations to affect the distributions of familiarity independently for each task (i.e., the mean of the old item familiarity distributions are the same for each task in this model). Put differently, this description of the model leaves open the possibility that an experimental manipulation could affect μ_{old} differently in Equations (4-1) and (4-2), yet this is precisely what the single-system model precludes. The reference to the model here as a single-system model reflects a conceptual framework for memory—that is that the same memory representation mediates performance in priming and recognition tasks.

For present purposes, the model can be simplified by setting $\mu_{\text{new}} = 0$ and $\mu_{\text{old}} = \mu$, with no loss of generality (i.e., μ represents the difference in the means of the old and new distributions). However, an extension is necessary for the model to account for individual differences between participants, in order to simulate correlations across participants within and between the recognition and priming tasks. This was done simply by drawing a value of μ randomly from a normal distribution for each participant, i:

$$\mu_i \sim N(\mu, \sigma_\mu) \quad (4\text{-}3)$$

where σ_μ is the standard deviation of the mean familiarity across participants.

The values of μ_i for cued and uncued old items also needed to be separated. This was achieved by assuming that increases in familiarity owing to attention at study scale the mean familiarity of cued, $f_{i,c}$, relative to uncued, $f_{i,u}$, items such that:

$$f_{i,u} \sim N(\mu_i, \sigma_f) \quad (4\text{-}4)$$

$$f_{i,c} \sim N(\beta_c \mu_i, \sigma_f) \quad (4\text{-}5)$$

where β_c is a new parameter.

The other parameters of the model (see Equations (3-1)–(3-3)) are the standard deviation of the distribution of familiarity values across items, σ_f, the standard deviation of the noise associated with the recognition task, σ_r, the standard deviation of the noise associated with the priming task, σ_p, and also the temporary increase in strength associated with the presentation of an item in degraded form in the perceptual identification task, T. To reduce the degrees of freedom in the model, the values of σ_f and σ_r were constrained to be equal, given that the important factor is the size of σ_p relative to σ_f and σ_r (Eqs. (4-1) and (4-2)). There were also a priori constraints that $\mu_i > 0$, i.e., that one presentation at study increases familiarity, even when uncued, and that $\beta_c > 1$, that is, that cuing during study (selective spatial attention) increases familiarity. Finally, the criterion, C, for recognition was fixed (for a given participant) as the midpoint of the weakest and strongest distributions of familiarity in a given experiment.

In other words, C_i was fixed as $C_i = \beta_c \mu_i /2$ in Experiments 5 to 7, and as $C_i = \mu_i /2$ in Experiment 8. This was because the principle concern was to reproduce the basic pattern of sensitivity results (Hits - FAs), rather than fitting the hit and false alarm rates as closely as possible, for which allowing C to vary across participants (and/or conditions and tasks) would have helped.

For Experiment 5, this left 6 free parameters: μ, σ_μ, β_c, σ_f ($= \sigma_r$), T, and σ_p. There were 10 degrees of freedom in the data (hit rate for cued words, hit rate for uncued words and false alarm rate for new words, for each of the priming and recognition tasks, plus split-half reliability measures for cued and uncued words for each of the recognition and priming tasks). The values of the parameters are shown in Table 4-1.

The data were simulated using the same number of trials as in the test phases of the experiments (48 trials per stimuli type in Experiment 5, 36 in Experiment 6, 24 trials for cued, uncued and uncued-4 items and 48 trials for new items in Experiment 7, and 54 in Experiment 8), and using 10,000 simulated subjects. The large number of participants means that the error bars on the simulation results are negligible. The simulation results are shown for hit and false alarm rates in Figure 4-2 (and also for the derived measure, Hits - FAs, in Figure 4-1) and for the correlations in Figure 4-4. The error bars on the experimental data are 95% confidence intervals. It can be seen that the model results lie within these intervals for all cases in Experiment 5.

The model was then applied to Experiment 6. The same parameter values were kept from Experiment 5, except for μ which was decreased from 0.065 to 0.055 (which decreased the mean familiarity of both uncued and cued items, given that they are

Table 4-1. *Parameters of the model in Simulation Study 1*

Symbol	Meaning	Value			
		Exp. 1	Exp. 2	Exp. 3	Exp. 4
σ_f	Standard deviation of familiarity distributions (new/cued/uncued)	**0.2**	0.2	0.2	0.2
σ_r	Standard deviation of noise associated with recognition (constrained to equal σ_f)	0.2	0.2	0.2	0.2
σ_p	Standard deviation of noise associated with priming	**1.0**	1.0	1.0	1.0
μ	Mean familiarity of uncued items	**0.065**	**0.055**	**0.033**	**0.04**
σ_μ	Standard deviation of mean of uncued items across subjects	**0.03**	0.03	0.03	0.03
β_c	Proportional increase in mean of cued relative to uncued items	**8.33**	8.33	8.33	-
β_{u4}	Proportional increase in mean of uncued-4 relative to uncued items	-	-	4	-
T	Increase to target item strength within an identification trial	**2.8**	2.8	2.8	2.8

Note. Exp. = Experiment. **BOLD** indicates that the parameter was varied to fit the data; a dash indicates that this condition was absent from the experiment.

related by the scaling factor β_c). This change could be justified by the longer study and test lists (i.e., longer retention interval for a given word) in Experiment 6, and possibly the different participants. With the exception of the hit rate for cued items in the recognition task, the model reproduced all of the hit and false alarm rates, and split-half measures of reliability, which were within the empirical range, providing further support for the robustness of the model. Furthermore, the model also reproduced the low correlations between priming and recognition tasks when tested across participants

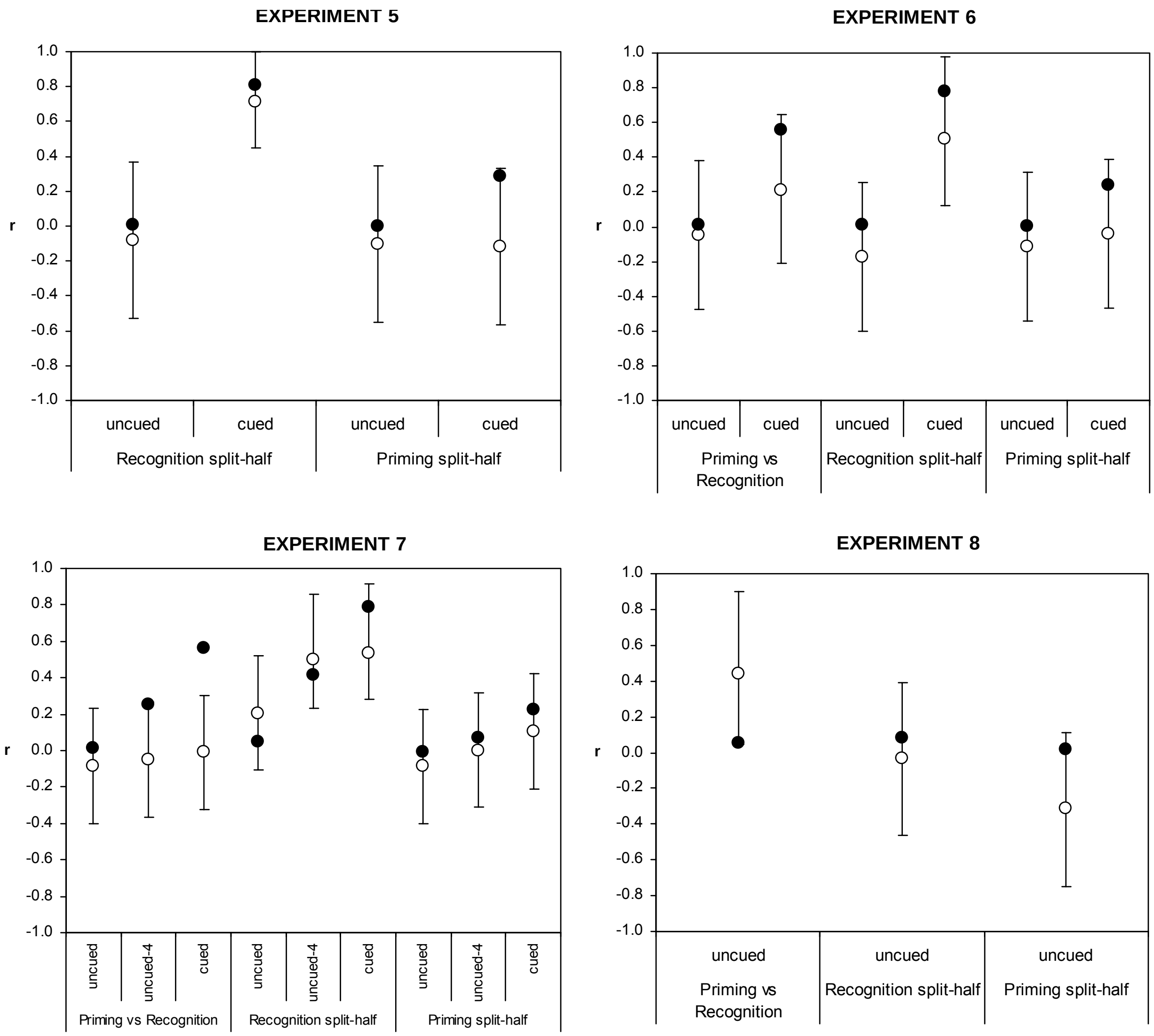


Figure 4-4. Inter-task (priming vs. recognition) correlations (*r*) and split-half reliability estimates of priming and recognition tasks for Experiments 5–8. Open-circles indicate experimental data (error bars indicate 95% confidence intervals); closed-circles indicate model results.

(see Figure 4-4), which were numerically greater for cued than uncued items, even though their confidence intervals overlapped zero in both cases (see Summary of Simulations section for further discussion).

The introduction of uncued-4 items in Experiment 7 required the addition of a parameter, β_{u4}, which reflected the increase in mean familiarity of uncued items when they were presented four times relative to once, i.e., $f_{i,u4} \sim N(\beta_{u4}\mu_i, \sigma_f)$. However, to minimise degrees of freedom in the model, this parameter was fixed a priori as 4. There was a need to reduce μ from 0.055 to 0.033, which again could be justified by the much longer study and test lists in Experiment 7 than in Experiment 6 (240 study trials versus 72). Most importantly, the model reproduced the effect of attention and of repetition on Hits - FAs in both tasks (see Figure 4-1). For the recognition task the model provided a fit to the hit rate for uncued-4 items, but the fits to the cued hit, uncued hit and false alarm rates were not as accurate as they were in Experiments 5 and 6 (see Figure 4-2). It is evident that the fits would benefit from a more liberal value for the criterion *C* (which was constrained here), in order to increase both the hit and false alarm rates. Nonetheless, the model reproduced all of the hit and false alarm rates in the priming task, and also all of the split-half measures of reliability for both tasks. It also reproduced the correlations between priming and recognition, with the exception of the correlation between priming and recognition for cued items (see Figure 4-4).

Finally, apart from the need to increase μ to 0.04 (which could again be justified in terms of the shorter study list length in Experiment 8 than Experiment 7 (i.e., 108 study trials vs. 240), the same parameter values provided sufficient fits to all conditions in Experiment 8, except for the hit and false alarm rate in the priming task, which fell just outside of the empirical range.

4.8.1 Summary of Simulations

The model fits the data according to the three hypotheses in the introduction to the chapter: (1) Given that study exposure (whether cued or uncued) increases familiarity, the model necessarily predicts that attention modulates priming, (2) assuming that priming and recognition rely on the same familiarity measure, priming can never be greater than chance when recognition is (truly) at chance; indeed, if the noise associated with priming tasks is greater than that associated with recognition tasks, priming can never exceed recognition, (3) given that, relative to recognition, only a small proportion of the variance in priming task performance is due to familiarity, the correlation between priming and recognition will be low; however, the model necessarily predicts a positive correlation between recognition and priming tasks because they depend on the same underlying familiarity signal.[5]

Furthermore, if the noise associated with priming tasks is greater than that associated with recognition tasks, the model predicts a greater effect of attention on sensitivity measures (e.g., Hits - FAs) for recognition than for priming, as was consistently found in Experiments 5–8. This is because sensitivity measures are a function of both the (i) difference in means of the old and new distributions and (ii) the spread of those distributions. This means that even though the difference in mean familiarity for cued/uncued and new items is equivalent in the single-system model, the spread of the final distributions used to make a decision (i.e., J in Equations (4-1)-(4-2)) is greater in the priming task when the noise is greater.

Of particular note is that the model produces values for the split-half measures of reliability in the priming task (Figure 4-4) that are low enough to fit the data. The model always predicts a reliability greater than zero because a non-zero value for the difference

[5] Taking into account the stochastic nature of the model, these statements are of course based on asymptotic performance (i.e., large numbers of trials). With a small number of trials, there is always the possibility that random fluctuations can cause an empirical result contrary to one or more of these statements. According to the model, such a finding would however not hold in the long run (i.e., would not be reproducible with large numbers of trials).

in mean familiarity of old and new items (μ) always implies similar Hits - FAs for odd and even trials. However, the relatively large contribution of random noise from trial to trial in the priming task, σ_p, means that the reliability can be small. Nonetheless, one would predict that, with a greater number of trials (i.e., more powerful measure of split-half reliability), the reliability of both priming and recognition tasks would be significantly greater than zero for all types of item. The smaller noise in the recognition task explains the larger (and in many cases significant) reliability values for this task.

A similar argument applies to the non-significance of the between-task correlations in the present study: the model can predict correlations that are low enough to be difficult to detect given a statistical power comparable to that in the present study. Nonetheless, the significant, positive correlation that was found in one of the three experiments (Experiments 6–8) provides some support for the model's assumption that recognition and priming share a common distribution of familiarity.

4.9 Discussion

Three predictions of the model (as stated in the Introduction to the chapter) were tested across Experiments 5–8 using a manipulation of selective attention at study. Firstly, as predicted by the model, effects of attention were observed on both priming and recognition performance: Priming and recognition were greater for cued words than uncued words. This result is in line with a number of other studies that found effects of selective attention at study on priming and recognition (Bentin et al., 1998; Crabb & Dark, 1999, 2003; Eich, 1984; MacDonald & MacLeod, 1998; Mulligan, 2002; Phaf et al., 1994; see also Chapter 2, Experiment 2) and is inconsistent with claims that manipulations of attention do not affect priming (e.g., Parkin & Russo, 1990).

Secondly, the model predicts that recognition is more sensitive to the underlying strength variable than is priming. It is therefore unlikely that priming will be found in the absence of recognition in an experiment (or that the magnitude of priming will be greater than that of recognition, when compared with the same response metric). In line with this prediction, priming for uncued, uncued-4 or cued words was never greater than recognition for such words in any of the four experiments. Indeed, recognition memory for uncued words occurred in the absence of reliable priming for these words (in Experiments 6, 7 and 8). Priming in the absence of recognition has been found in some studies using manipulations of attention at encoding (e.g., Eich, 1984). Experiments 5−8 do not directly address these findings; however, the results of all four experiments suggest that selective attention manipulations are unlikely to produce a pattern of priming greater than recognition, even when recognition is reduced (or is very close) to chance (e.g., for uncued words in Experiment 7).

Thirdly, the model predicted that priming and recognition performance will be correlated. In support of this prediction, a significant correlation for uncued items was observed in Experiment 8. However, in Experiments 6 and 7 (in which calculation of the correlation was also possible), performance between tasks for cued, uncued or uncued-4 words was not significantly correlated. In the account presented here, because only a small proportion of the variance in priming measures is due to memory, correlations between performance in both tasks, when obtained, will be weak (Ostergaard, 1992). A true correlation of zero would be evidence against the model.

The experimental and simulation results, together with the results of a number of recent studies, converge on the notion that recognition tasks are generally more reliable measures of memory than are priming tasks (Buchner & Brandt, 2003; Buchner & Wippich, 2000; Meier & Perrig, 2000). The perceptual identification task, however, has been found by Buchner and Wippich (2000) to be reliable and to have a reliability that is comparable to recognition. In Experiments 5−8, this pattern was not obtained: the perceptual identification task did not reliably measure memory for any stimulus type, even when the split-half reliability estimates were calculated for data collapsed across experiments. It is possible that this discrepancy in results is due to the greater power of Buchner and Wippich's (2000) study to detect reliability of the priming task, or it could be due to minor procedural differences between the perceptual identification tasks used (e.g., the presentation duration for all items in Buchner and Wippich's test was a pre-determined threshold for each participant).

A trend discernable from the reliability analysis is that in both the recognition and priming tasks, the split-half correlations are generally greater when sensitivity is higher (e.g., greater reliability estimates for cued items than uncued items). One could therefore propose that differences in task reliability could be solely explained by differences in task sensitivity. However, the significant split-half correlation of uncued-4 words for recognition in Experiment 7 counts against this proposal. The sensitivity (Hits - FAs) of recognition to the influence of uncued-4 stimuli is similar in magnitude to (if not less than) that of cued items in priming in Experiments 5–8. Despite this similarity, the split-half correlation for uncued-4 items in recognition was significant, while those of cued items in the priming task of Experiments 5–8 are not. This supports the account proposed here: that the lower reliability of the priming task reflects greater noise variance associated with the priming task response than the recognition decision (without any difference in the underlying memory signals, or in the means of the familiarity distributions). In other words, some idiosyncratic difference of the priming task per se causes its reliability to be lower than that of recognition.

The account presented here could help to shed light on the different patterns of results from studies using dual-task versus selective attention manipulations at study. As described in the introduction to the chapter, dual-task manipulations have been reported to affect recognition but not priming, whereas selective attention manipulations have been found to affect both. The model predicts that there will be effects of attention on both tasks and that they will be smaller on priming than recognition. If the effects of dual-task manipulations are weaker than selective attention manipulations then it will be harder to detect effects of dual-task manipulations on priming than selective attention manipulations. A study by Mulligan (2003) suggests that this may actually be the case. Mulligan (2003) found that effects of dual-task manipulations on priming emerge when the difficulty of the secondary task is increased. In his Experiment 1, a digit monitoring secondary task (detecting sequences of 3 odd digits in a row) produced effects on recognition but not priming (in a perceptual identification task), reproducing the typical dissociation. However, when the difficulty of the secondary task was increased, either by making the presentation of distractors synchronous with the presentation of the target, or by increasing the frequency of responding at test (rather than only requiring responses when a target sequence was detected), effects on priming were found.

Mulligan concluded that the failure to detect an effect of attention on priming in his Experiment 1 was not due to a lack of power. He calculated that the power to detect an effect on priming which was half the size of the effect on recognition was .95. If it is true that there is no effect on priming, then this would be evidence against the model. In Appendix 1, the data from Mulligan's (2003) Experiment 1 were simulated with the model. Given close fits to the recognition performance, the model prediction for the difference in Hits - FAs between full- and divided-attention conditions in the priming task was revealed as .06. The power of Mulligan's experiment to detect this effect is approximately .44, which is substantially lower than the power value he reported. Thus, the failure to detect an effect of the dual-task manipulation on priming in Mulligan's Experiment 1 could be due to a lack of power. The single dissociation reported by Mulligan is therefore not necessarily inconsistent with the model.

This explanation of single dissociation evidence could also be applied to studies that have found that depth of processing manipulations at study can affect recognition but have much smaller effects on recognition, which are sometimes not detected (Brown & Mitchell, 1994; Jacoby & Dallas, 1981). The model could account for this finding quite readily: If it is assumed that deeper levels of processing of items lead to greater mean levels of f, then as f increases, recognition performance will increase at a greater rate than priming. The final point being made here—that dissociations may arise because of the failure to reject the null hypothesis—is not new (e.g., Dunn, 2003); however, the model goes further and allows one to produce a quantitative prediction of the size of the expected effect on priming, given the effect on recognition.

Chapter 5: Fluency, Priming, and Recognition

According to some dual process theories of recognition, priming and recognition are related by means of fluency: It has been proposed that the facilitation in processing, or fluency, from priming can give rise to a feeling of familiarity which can serve as a basis for recognition (Mandler, 1980; Jacoby & Dallas, 1981; Yonelinas, Regehr, & Jacoby, 1995; for a review of studies concerned with attributions of fluency see Kelley & Rhodes, 2002). Indeed, recognition judgments can be influenced by manipulations designed to enhance fluency or speed of processing. For example, Whittlesea, Jacoby, and Girard (1990) showed that an item presented in a low masking condition at test was more likely to be judged old than an item presented in a high masking condition. Whittlesea et al. (1990) argued that the items presented in low levels of masking were easier to read and were processed more fluently than those presented in high masking. This enhanced fluency was misattributed to prior exposure of the word, thereby serving as a basis for responding in recognition.

Studies that look at the contribution of fluency, or priming, to recognition often use perceptual clarification procedures to present each item (e.g., by presenting a stimulus for successively longer durations, Stark & McClelland, 2000; or by slowly unmasking a stimulus, Conroy et al., 2005; Johnston, Dark, & Jacoby, 1985; Johnston, Hawley, & Elliot, 1991; Verfaellie & Cermak, 1999). Typically, the participant's task is to identify each item and then to make a recognition judgment to the item. Priming is shown by faster identification reaction times (RTs) to old than new items. The "fluency effect" is the term usually used to describe the shorter RTs to items *judged* old than items *judged* new, independent of actual old/new status (Conroy et al., 2005; Johnston et al., 1985). Thus, the task permits concurrent assessment of recognition, priming, and fluency, providing a measure of each for every item. Indeed, RTs can be compared across all four possible outcomes; that is, hits (old items judged old), misses (old items judged new), false alarms (new items judged old), and correct rejections (new items judged new).

Two studies have used this paradigm with normal adults and have produced results that have been interpreted as evidence for distinct memorial bases of priming and recognition (the next chapter considers a study with amnesics). Briefly, they are that a) priming can occur for items not overtly recognized (Stark & McClelland, 2000), and b) RTs to false alarms are faster than RTs to misses when recognition is poor, and vice versa when recognition is relatively good (Johnston et al., 1985). The aim in this chapter is to consider each of these findings and to determine whether they are incompatible with the model.

The evidence from Stark and McClelland (2000) is considered first. Stark and McClelland (2000) used the CID-R (continuous identification with recognition) task (Feustel et al., 1983) to investigate the relationship between priming and recognition. On a study trial of this task, an item is presented for a brief duration (e.g., 17 msec) and is then followed by a mask (####) for the remainder of a presentation block (e.g., 233 msec). The item is then re-presented, at a slightly longer duration (e.g., 34 msec) and is again replaced with the mask for the remainder of the presentation block (i.e., 216 msec). Presentation continues in this way, with the item being presented for longer and longer durations until it is identified (or until the presentation duration of the word equals the duration of a presentation block). The same procedure is used in a second test phase (in which some items are repeated from the study phase), except that participants make a recognition judgment following each identification.

A particularly striking finding was reported by Stark and McClelland (2000): identification RTs for misses (old items judged new) were faster than those for correct rejections (new items judged new). In other words, even though certain items were not remembered, a priming effect still occurred for these items. Stark and McClelland (2000) argued that this result supports the notion that the sources of recognition memory and priming are independent, since if priming and recognition depend upon the same memory source then priming should not occur when recognition is absent. Furthermore, they found that performance in the two tasks was not significantly correlated, bolstering the case for independence.

5.1 Experiment 9

Experiment 9 was conducted to replicate Stark and McClelland's (2000) finding of priming for items judged new. After this experiment is presented, the model is applied to the results. An experimental replication was conducted rather than simply fitting the model to Stark and McClelland's (2000) data because, as discussed in Chapter 2, reliable demonstrations of priming in the absence of awareness remain elusive and independent replications of such effects are therefore important.[1] Words were presented at study using the CID procedure; participants pressed a button when they could identify

[1] Stark and McClelland's (2000) other main results of 1) repetition priming for nonwords and 2) differences in the magnitude of priming for words, non-words, and pseudowords, were not of interest for present purposes because they were not used to argue for independent memorial bases of priming and recognition.

the word and then named the word aloud. At test, old and new words were presented to participants using the CID-R procedure.

5.1.1 Method

5.1.1.1 Participants

24 individuals were recruited through a UCL participant database. Their ages ranged from 19 to 38 with a mean of 23.3 years. All participants reported normal or corrected-to-normal vision, reported English as their first language, were tested individually in sound-dampened cubicles, and were paid £4 in return for taking part. The experiment was fully automated and the experimenter was not present in the room during the course of the experiment.

5.1.1.2 Materials

120 words were selected with similar constraints to Stark and McClelland (2000): All words had 4 letters, had a frequency of occurrence of 10 to 200 per million (Kucera & Francis, 1967) and a maximum score of 500 on imagability and concreteness scales in the MRC Psycholinguistic database (Coltheart, 1981). All words were presented in white 20 pt Courier font. Two 50-word lists were constructed. Each word list acted as the old or new stimuli, counterbalanced across participants.

5.1.1.3 Procedure

At study, a single word was presented on each CID trial. At the start of each trial a mask (a row of hash marks ####) was presented for 500 msec to orient the participant. Next, a word was presented in lowercase 20 pt Courier font for 17 msec. The mask was then presented in 26 pt Courier font for 233 msec, forming a 250 msec presentation block. The word was then immediately presented again, but this time the exposure duration was increased by 17 msec and the mask followed for the remainder of the 250 msec presentation block. Presentation continued in this way, with the total stimulus plus mask time remaining constant in each block, until the mask duration was zero msec (15 blocks). When a response was made (by clicking the left mouse button) the mask was immediately presented for 2000 msec. Participants then clicked a button which was presented below the stimulus presentation area to advance to the next trial.

There were 70 study trials in total. The first and last 10 trials were considered primacy and recency filler trials and none of the words from these trials appeared at test. The RTs from these filler trials were not included in any subsequent analysis. The remaining 50 trials contained the stimuli which would later appear at test. For the study phase, participants were told that a word would flash on the screen for longer and longer durations, and that this would make it appear clearer over time. They were told that they must click the left mouse button when they knew the identity of the word and then read it aloud. On each trial, the time from the onset of the stimuli to the onset of the button press was recorded. The importance of speed was emphasized, however, errors were discouraged: Participants were told that they should click the mouse button only when they were confident that they could identify the word correctly. If the word had not been identified by the end of the trial then a message appeared to the participants asking them to try to be faster on the next trial. RTs longer than 3750 msec (the time that had elapsed by the end of the last stimulus presentation block) were not recorded. No indication of the upcoming recognition test was given.

After the study phase, the instructions for the test phase were presented. A single word was presented using the CID procedure on each test trial. Participants were again instructed to press the mouse button when they could identify the word and then read the word aloud; they were additionally told that after identifying each word they would have to make a judgment about the word. After each identification, the probe “old or new?” appeared on the screen and two buttons labeled old and new were also presented on the screen below this probe. Participants were instructed to click the button labeled ‘old’ if they thought that the word they had just identified was one from the study phase. They were told to click the ‘new’ button if they thought that the word had not been presented in the study phase. There were 100 test trials in total (50 old and 50 new trials). The selection of a word for each trial was randomly determined. Misidentification trials at study or test were excluded from all subsequent analysis. Responses were recorded on a tape recorder and later checked for accuracy.

An alpha level of .05 was used for statistical tests, and *t*-tests were two-tailed. Tests involving repeated-measure factors with more than two levels were corrected for nonsphericity using the Greenhouse-Geisser correction.

5.1.2 Results

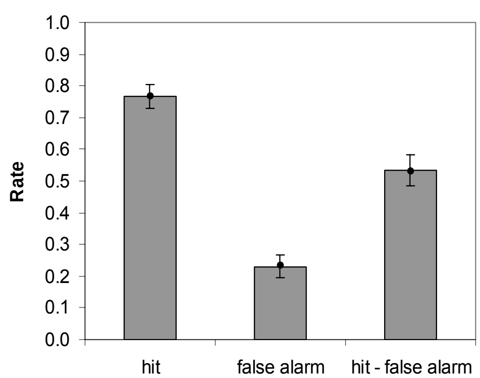
5.1.2.1 Study Phase

The number of errors made was very low (M < 1% errors across participants) and as a result no further analysis of the errors was conducted. The mean identification RT to the study words was 1441 msec (standard error of the mean (*SEM*) = 41 msec).

5.1.2.2 Test Phase

Recognition accuracy, as measured by *d′*, was significantly greater than chance overall, $t(23) = 18.38$, $p < .001$, ($M\ d' = 1.53$, $SEM = 0.08$; *M* hit rate = .77, $SEM = .02$; *M* false alarm rate = .23, $SEM = .02$).

For every participant, priming was calculated as the mean RT for new items minus the mean RT for old items. Priming was significantly greater than zero overall, $t(23) = 5.70$, $p < .001$ ($M = 85$ msec, $SEM = 15$), indicating that old items were identified more quickly at test than new items (new items $M = 1397$ msec, $SEM = 46$; old items $M = 1312$ msec, $SEM = 40$). Consistent with Stark and McClelland (2000), no significant correlation was found between this priming effect and recognition accuracy (*d′*), $r(23) = -.25$, $p = .24$. As has been observed in previous studies, a significant fluency effect was also obtained (judged old *M* RT = 1315 msec vs. judged new *M* RT = 1392 msec), $t(23) = 7.77$, $SEM = 10$, $p < .001$.



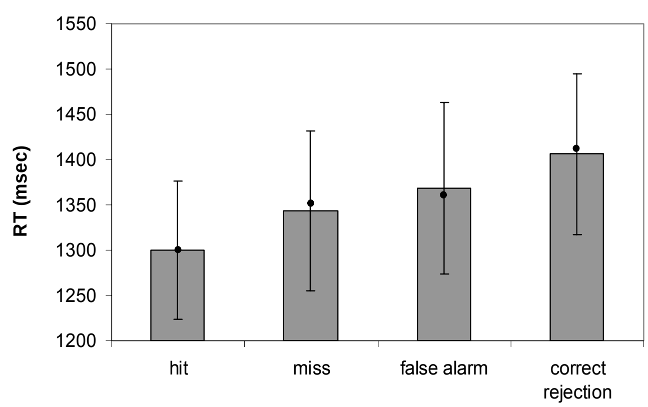


Figure 5-1. Simulated and empirical data of Experiment 9. Left panel: hit and false alarm rates for the recognition task. Right panel: CID-R identification RTs classified according to recognition outcome. Grey bars indicate data; error bars indicate 95% confidence intervals of the data. Circles indicate model results. Each simulated data point is based upon 50 observations for each old/new stimulus type and for 10,000 participants.

The RTs to correct rejections, misses, false alarms and hits are shown in Figure 5-1. A repeated measures ANOVA, comparing the RTs for these recognition outcomes revealed a significant difference between the RTs in the four categories, $F(3, 69) = 11.25$, $p < .001$. Of primary interest for this experiment, RTs to misses were significantly faster than RTs to correct rejections, $t(23) = 3.55$, $p = .002$ (*M* priming for items judged new = 63 msec, *SEM* = 18), replicating Stark and McClelland (2000). Furthermore, additional comparisons (*t*-tests, Bonferroni corrected) revealed that the RTs to hits were faster than those to correct rejections, $t(23) = 7.34$, $p < .05$, and false alarms, $t(23) = 2.97$, $p < .05$. No other significant comparisons were found.

In summary, the key result from this experiment was that a priming effect for items judged new was obtained, replicating Stark and McClelland (2000). Priming and recognition were also found to be at levels significantly greater than that expected by chance, and not significantly correlated.

5.2 Simulation Study 2: Experiment 9

To simulate recognition in the CID-R task, Equation (3-2) was again used, and as before, each item's value of J_r was then compared to C (which was fixed as the midpoint of the old and new distributions of familiarity, i.e., $C = \mu/2$) to determine the recognition response. However, Equation (3-3) needed to be transformed to simulate identification RTs in the CID-R task. This was done by assuming that the RT is a decreasing function of f, while still keeping the independent noise parameter, e_p:

$$\mathrm{RT} = b - sf + e_p \qquad e_p \sim N(0, \sigma_p) \tag{5-1}$$

where the parameters b and s are merely scaling parameters which represent the RT intercept and slope (rate of change of RT with f), respectively. This method of simulating the influence of memory on RTs is similar to previous applications of the model (Shanks & Perruchet, 2002; Shanks et al., 2003). Thus, an item with a relatively large value of f in the CID-R task is likely to be judged old and also to receive a short identification RT.

As in Simulation Study 1, the model in Chapter 3 was simplified by setting $\mu_{\text{new}} = 0$ and $\mu_{\text{old}} = \mu$, with no loss of generality (i.e., μ represents the difference in the means of the old and new distributions). The other parameters of the model (see Equations (3-1)-(3-2), (5-1)) are the standard deviation of the distribution of familiarity values across items, σ_f, the standard deviation of the noise associated with the recognition task, σ_r, the standard deviation of the noise associated with identification, σ_p, the RT intercept, b, and the familiarity slope, s, associated with the generation of the RTs.

There were some a priori constraints imposed on the parameter values in order to be consistent with Simulation Study 1, namely that $\sigma_r = \sigma_f = 0.2$. This left four free parameters: μ, σ_p, s, and b. There were 8 degrees of freedom in the data (RT to hits, misses, false alarms, and correct rejections; the hit and false alarm rates for the recognition task; the mean item RT variance; and the Pearson correlation between priming and recognition[2]). The values of the parameters (in this and the next simulation)

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[2] Correlations were simulated in Simulation Study 1 by drawing a value of μ randomly from a normal distribution for each participant (Equation (4-3)). The standard deviation of the mean familiarity across participants, σ_μ, was not included in this simulation study in order to improve the ratio of free parameters to degrees of freedom in the data and, moreover, only one correlation is being simulated here rather than several (as was the case in Simulation Study 1). It should be noted though that when the simulation was repeated with μ_i included (and σ_μ was set at 0.028 as in Simulation Study 1), the predicted correlation was very similar to the one obtained in this simulation and was still within the 95% confidence interval ($r = .10$ with μ_i included vs. $r = .07$ without).

are shown in Table 5-1. They were chosen by a hand-search process: first μ was varied to fit the recognition data, next the free parameters relating to the priming task (in Equation (5-1)) were varied to fit the RT data.

For each old and new item, a single value of f was randomly sampled on each trial from the relevant distribution. The value of f was then scaled and combined with one source of noise (Equation (5-1)) to derive an RT which formed the basis of the fluency and priming data. The same value of f was then combined with another source of noise (Equation (3-2)) to determine the recognition response. To reiterate, the same memory signal (familiarity) contributed to priming, recognition and fluency.

The data were simulated using the same number of trials as in Experiment 9 (50 old, 50 new trials) and using 10,000 simulated participants. The large number of participants means that the error bars on the simulation results are negligible. The simulation results for the RTs to hits, misses, false alarms, and correct rejections are shown in the right panel of Figure 5-1, and for recognition performance in the left panel of Figure 5-1. The error bars on the experimental data are 95% confidence intervals. It can be seen from Figure 5-1 that the model results are within these intervals for all cases.

In order to determine whether the variability in RTs predicted by the model is realistic, the variance of each subject's RTs to items at test was calculated. This variance was fit by the model: *M* item *SD* of RTs (within-subject) in Experiment 9 = 277 msec, 95% + CI = 302 msec, 95% - CI = 252 msec; *M* item *SD* of RTs (within-subject) predicted by the model = 296 msec). The primary reason for fitting this variance was to

Table 5-1. *Parameters of the model in Simulation Studies 2 and 3*

Symbol	Meaning	Exp. 9	Johnston et al. (1985)	
			Exp.1	Exp. 2
		Figure 5-1	Figure 5-2	
σ_f	Standard deviation of familiarity distributions(new/old)	0.20	0.20	0.20
σ_r	Standard deviation of recognition noise	0.20	0.20	0.20
σ_p	Standard deviation of priming noise	**290**	290	290
μ	Mean familiarity of old items	**0.42**	**0.54**	**0.12**
b	RT intercept	**1400**	**4150**	**4950**
s	RT-Familiarity slope	**210**	**550**	550

Note. Exp. = Experiment. **BOLD** indicates that the parameter was varied to fit the data.

ensure that RTs are generated within the appropriate intervals and to constrain the σ_p free parameter.

Lastly, the model predicts a near zero correlation between priming and recognition, r = .07, which is also within the 95% confidence interval of the correlation observed in Experiment 9 (r = -.25, 95% + = .17, 95% - = -.68).

5.2.1 Discussion

The single-system model predicted that priming would occur for items judged "new", suggesting that this result is not necessarily indicative of independent memorial bases of priming and recognition. In fact, the prediction of RT(misses) < RT(correct rejections) falls quite naturally out of the model, which assumes a signal-detection-like process in recognition: because an old item's value of J_r must exceed C for an old judgment to occur (see Equation (3-2)), values of f will tend to be greater for misses than correct rejections. RTs for misses will tend to be faster than correct rejections simply because RTs are a decreasing function of f (see Equation (5-1)). In fact, the model would even predict this qualitative pattern if no task-specific sources of noise were included (i.e., $e_r = e_p = 0$ in Equations (3-2) and (5-1)). Without these sources of noise f(hits) > f(false alarms) > f(misses) > f(correct rejections) (as predicted by signal-detection theory) and RTs will inversely correspond to f such that RT(correct rejections) > RT(misses) > RT(false alarms) > RT(hits).

Yet in the simulation of Experiment 9, the inclusion of the noise parameters results in the pattern of RTs being RT(correct rejections) > RT(false alarms) > RT(misses) > RT(hits). Consistent with this, there was a numerical trend for RT(false alarm) > RT(miss) in the experimental data, but this difference was not significant. It should be noted though that this pattern has been found in some studies: Johnston et al. (1985; Experiment 1) found that RTs to false alarms were indeed significantly slower than RTs to misses (see ahead to Simulation Study 3). Stark and McClelland (2000) also observed numerical trends for RT(false alarms) > RT(misses) for word stimuli (but reported no statistical comparisons).

Why does the model predict, correctly, that (in some cases) RT(false alarms) > RT(misses)? Consider a new item which has an "average" f and is combined with a high value of e_r (to generate J_r in Equation (3-2)); this item will still have the same average value of f when its RT is generated (in Equation 5-1) and it will be unlikely to again be combined with a large value of e_p. Thus, a new item with a J_r value that exceeds C can be classified as a false alarm, but it will not neccessarily have a comparably short RT, because it is unlikely that e_r and e_p will both be large for the same item. The reverse scenario occurs for misses: an old item with an average f that is combined with a large negative value of e_r lowering its value of J_r below C will be classified as a miss. When that same value of f is used to predict the item's RT, it will be unlikely to again be combined with a large negative value of e_p meaning that misses will not necessarily result in comparably long RTs and the RTs can be shorter than those of false alarms. (This property is related the phenomenon of regression to the mean.) Thus, the inclusion of noise parameters in the model (in particular, e_r) is important for it to explain this result.

In the next simulation study, the model is applied to another set of results that have been obtained with a similar task and have been taken as evidence for distinct bases of priming and recognition. In this case, the relationship between RT(misses) and RT(false alarms) is shown to vary with μ, and again, it is the inclusion of the noise parameters that is critical in increasing the model's explanatory power.

5.4 Simulation Study 3: Johnston et al. (1985)

In a classic study by Johnston et al. (1985), participants read items at study and identified ones which gradually clarified from a mask at test. In their Experiment 1 the stimuli were words and recognition and priming were at levels greater than chance. The order of RTs was RT(correct rejections) > RT(false alarms) > RT(misses) > RT(hits). In Experiment 2 the stimuli were non-words, recognition and repetition priming were at levels lower than those of Experiment 1, and the order of RTs changed to RT(correct rejections) > RT(misses) > RT(false alarms) > RT(hits). Thus, the pattern of RTs in Experiment 2 resembled that predicted by a model of priming and recognition similar to the one presented here but with no noise parameters (as discussed above). The results of Experiment 2 were interpreted as evidence that recognition relied primarily upon a single memory signal when recognition performance was poor. However, since the pattern of RTs in Experiment 1 did not conform to the predictions of such a model, Johnston et al. (1985, 1991) interpreted the RT(false alarm) > RT(miss) pattern as evidence that an additional memory factor contributed to recognition when overall recognition performance was higher. This paper has been cited as evidence that priming and recognition are mediated by different memory bases.

As shown in the simulation of Experiment 9, inclusion of decision noise allows the model to predict the RT(false alarm) > RT(miss) pattern. However, what is not clear is whether the model predicts RT(false alarm) < RT(miss) when recognition is poor. To investigate this, the model was applied to Experiments 1 and 2 of Johnston et al. (1985). The values of σ_r, σ_f, and σ_p were kept from the previous simulation, leaving μ, s, and b as the three free parameters for Experiment 1. The parameter s was held constant across experiments, but it was necessary to change b (as well as μ) between experiments, resulting in two free parameters for Experiment 2. The change in b can be justified by the generally slower RTs in Experiment 2 than Experiment 1. There were 6 degrees of freedom in the data for each experiment (hit rate, false alarm rate, RTs to hits, misses, false alarms, and correct rejections). The parameter values are shown in Table 5-1 and the model results for RTs are shown in Figure 5-2.

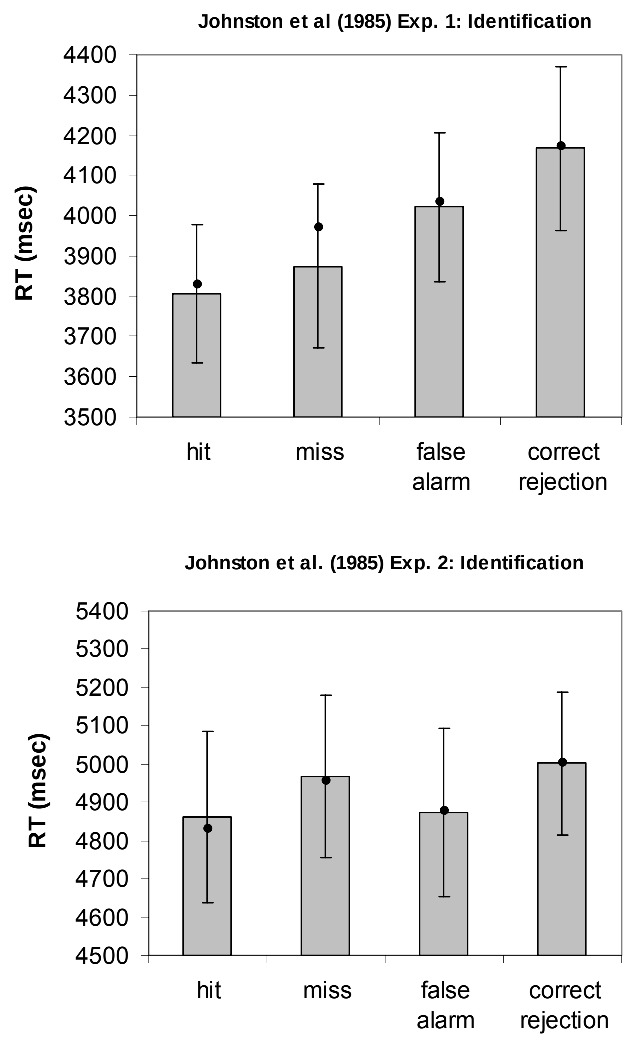

Figure 5-2. Simulated and empirical data of Johnston et al. (1985) Experiment 1 and 2. Error bars indicate 95% confidence intervals (estimated from Johnston et al., 1985). Top panel: identification RTs for Experiment 1 (top) and Experiment 2 (bottom). Each simulated data point is based upon 88 observations for each old/new stimulus type and for 10,000 participants.

It can be seen that all of the model results for the RTs to hits, misses, false alarms and correct rejections were within the empirical range. For the recognition task, the model results for the hit and false alarm rates in Experiment 1 were .83 and .17, respectively (Johnston et al., 1985, hit rate = .70, 95% CI = 0.04; false alarm rate = .21, 95% CI = 0.04). In Experiment 2, the model hit and false alarm rates were .58 and .42, respectively (Johnston et al., 1985, hit rate = .57, 95% CI = 0.04; false alarm rate = .29, 95% CI = 0.04). Although the fits for two of the four hit and false alarm rates were outside of the empirical range, the crucial aspect of these results is that the model predicted RT(false alarms) > RT(miss) when recognition was high (in Experiment 1), and RT(false alarm) < RT(miss) when recognition was low (in Experiment 2).

Furthermore, Johnston et al. (1985) also reported that the fluency effect (i.e., shorter RTs for hits and false alarms vs. misses and correct rejections) was attenuated by recognition performance: In Experiment 1 the effect was 310 msec, but in Experiment 2, the effect was lower at 122 msec. The model also predicted this trend: in Experiment 1 the fluency effect was 275 msec, whereas in Experiment 2 it was 133 msec.

Why does the model predict a reversal in the RTs to misses and false alarms at different levels of recognition performance? To answer this question it is important to distinguish between two ways in which a new item can have a value of J_r that exceeds C and be classified as a false alarm: First, an item can have a relatively high value of f that exceeds C even after being combined with e_r (to form J_r in Equation (3-2)). When a false alarm occurs in this way, an item's RT will be tend to be comparably short because its value of f will still be relatively high when its RT is generated (by combining it with e_p in Equation (5-1)). Second, an item's value of f may not exceed C initially, but it does exceed C when it is combined with a high value of e_r. As explained in the previous Discussion section, when false alarms occur in this way an item's RT will not necessarily be comparably short because when the same (relatively average or low) f is used to generate its RT, it is unlikely to again be combined with another large value of noise (e_p).

When μ is low, the distributions of f for old and new items are closer together and there will be many more items with values of f that initially exceed C (because the mean of the new item distribution is closer to C). So when μ is low, the first cause of false alarms (above) (i.e., high f) will dominate, and false alarms will have comparably short RTs. However, when μ is high, the distributions of f are further apart and there will be a much lower number of items that have values of f that initially exceed C (because the mean of the new item distribution is further from C). Thus, when μ is high, false alarms will mainly arise through the second cause (above) (i.e., high e_r) and the RTs to false alarms will not be comparably short. The reverse process occurs for old items: when μ is low, misses will have comparably long RTs because many more old items will have values of f that are initially below C. When μ is high, misses will not tend to have comparably long RTs because there will be relatively fewer old items that have values of f that are initially below C. Thus, when μ is low RTs to false alarms can be shorter than RTs to misses and vice versa when μ is high. As was the case in the previous simulation study, the inclusion of the noise parameters in the model is crucial for its capacity to account for this pattern of results.

In sum, this simulation study showed that the model predicts that the difference between RTs to misses and false alarms changes as a function of recognition strength, as was observed by Johnston et al. (1985). Thus, this finding also does not seem to compel an account in which priming and recognition are mediated by distinct memorial bases.

5.5 Discussion of Chapter 5

The main aim of these studies was to present an alternative interpretation of two results that have been taken as evidence for distinct memory bases for priming and recognition. First, in Experiment 9, Stark and McClelland's (2000) observation of priming for old items that were not overtly recognised was replicated, and then showed to be predicted by the single-system signal-detection model in which one memory source drives priming and recognition. Second, the model was applied to the results of Johnston et al. (1985). As found by Johnston et al. (1985), the model predicted slower RTs to false alarms than misses when recognition performance was relatively good, but predicted that RTs to false alarms will be quicker than RTs to misses when recognition was lower. The results of these simulations show that these two findings are consistent with the single-system model. Thus, these findings are not necessarily indicative of the involvement of multiple sources of memory in priming and recognition.

A limitation of the model concerns the use of fluency as a heuristic in recognition: For example, Johnston et al. (1991) found that when identification and recognition trials were blocked, rather than interleaved, fluency effects did not occur. This was taken as evidence that interleaving the identification and recognition trials encouraged a reliance on speed of identification as a heuristic. The reason the model does not predict a difference between blocked and interleaved versions of the task is because the same value of f is used to determine an item's RT *and* its recognition judgment, in other words, there is a relationship between an item's RT and its likelihood of being judged old (see Equations 3-2 and 5-1). There is much evidence to suggest that other manipulations can affect the probability with which fluency is used as a cue for recognition (e.g., Jacoby & Whitehouse, 1989; Kinder, Shanks, Cock, & Tunney, 2003; Whittlesea, et al., 1990), though the model in its current state, does not speak to the use of fluency as a heuristic. It has been suggested (e.g., by Levy, Stark, & Squire, 2005) that because manipulations designed to enhance fluency tend to increase the proportion of old judgments to old and new items, such manipulations merely influence decision bias, and not recognition accuracy. If this were the case then the model would be able to account for this quite readily by allowing the decision criterion, *C*, to vary.

Chapter 6: Amnesia, Priming, Recognition, and Fluency

The simulation studies reported in the previous chapter were concerned with priming, recognition, and fluency in normal adults. In this chapter, some recent and seemingly compelling evidence for distinct memorial bases of priming and recognition in amnesia is considered. Conroy et al. (2005) used a perceptual clarification paradigm to investigate the contribution of fluency to recognition judgments in two groups of amnesic patients: one had medial temporal lobe lesions (MTL group, n = 2), the other had just hippocampal lesions (H group, n = 3). In the study phase of Conroy et al. (2005, Experiment 1), participants were told that words would be presented, but too briefly for conscious perception. In fact, no words were presented. At test, words clarified from a mask and a recognition judgment was made after every identification. A fluency effect was found for the MTL and H groups (i.e., RT(items judged old) < RT(items judged new)), which was comparable in size to a control group (CON group, n = 8). In addition, as an alternative method of measuring fluency, Conroy et al. (2005) took a median split of the RTs and looked at the percentage of old judgments in each half. More old judgments were made to words identified in the quick half than in the slow half, and this effect did not differ across groups. These results suggest that recognition judgments in amnesics and the controls were influenced by fluency of identification.

The study phase of Experiment 2 was genuine: words were presented and participants read them aloud. The test phase of Experiment 2 was the same as Experiment 1 except that the stimuli were old or new. Relative to the control group, the H group was impaired at recognition and performance in the MTL group was very close to chance.

Despite this impairment in recognition, amnesic patients showed levels of fluency and priming (faster RTs for old than new words) which were comparable to the CON group.

To the extent that fluency can give rise to a feeling of familiarity and act as a basis of recognition (as suggested by e.g., Mandler, 1980; Jacoby & Dallas, 1981), Conroy et al. (2005) reasoned that fluency from priming should contribute to recognition in amnesia, yet clearly this was not the case. In a further test of this hypothesis, Conroy et al. (2005) derived an estimate of recognition for each group, given the observed magnitude of fluency and priming. They found that the estimate of recognition was much lower than was actually observed, suggesting that fluency from priming did not contribute to recognition (see also Poldrack & Logan, 1997, for a related finding). This, coupled with the dissociation between fluency and priming on the one hand and recognition on the other in amnesia, led Conroy et al. (2005) to argue for the independence of the memorial bases of priming and recognition (see also Stark & Squire, 2000).

Is it necessary to interpret the results from Conroy et al. (2005) using a multiple-systems view, or can they be explained by a single-system account? Accordingly, the aim in this chapter was to simulate their findings with the single-system model. Furthermore, one of the MTL patients in Conroy et al. (2005), E. P., showed priming in the absence of recognition (see also Hamann & Squire, 1997a), presenting an additional challenge for the model.

6.1 Simulation Study 4: Conroy et al. (2005)

How can the effects of amnesia be simulated with the model? The performance of amnesic patients in Conroy et al.'s (2005) Experiments 1 and 2 was simulated by assuming that, relative to controls, there is a larger amount of noise in the encoded memory signal and also in the assessment of that signal. More specifically, the values of σ_f and σ_r were greater for the simulations of the amnesic groups than the control group; the values of these parameters were also associated with the severity of the amnesia such that they were greater in the more severe MTL group than the H group. In more psychological terms, the greater value of σ_f in amnesics represents greater variability in the degree to which an item resonates with an underlying memory representation at test. The psychological meaning of changing σ_r can be described as follows: The addition of e_r to f in Equation (3-2) is, in fact, formally equivalent to adding e_r to the decision criterion C.[1] Signal-detection modelers have added variability to the decision criterion as far back as Wickelgren (1968) and more recently, to simulate the receiver-operating characteristic (ROC) slope of remember-know judgments (Wixted & Stretch, 2004). In this sense, a greater value of σ_r in amnesia represents a greater amount of variability in the placement of the decision criterion from item-to-item, relative to controls.[2]

Others have also modeled amnesic performance by introducing greater amounts of noise into their simulations: For example, using the REM modeling framework, Malmberg, Zeelenberg, and Shiffrin (2004) simulated the effects of Midazolam-induced amnesia on recognition by assuming that the storage of memory traces is noisier in a Midazolam group than a control group. By varying the parameter c (the probability that an item gets stored accurately) between groups they were able to reproduce patterns of results that had previously been taken as evidence for a dual-process account of recognition memory.

[1] I'm grateful to John Dunn for pointing this out.

[2] The assumption could have also been made that σ_p is greater in amnesics relative to controls. However, increasing σ_p would have resulted in little quantitative change in the simulation results and, the qualitative pattern of results would not have been crucially altered. σ_p is therefore is kept constant across simulations of the amnesic and control groups.

The following simulations were carried out in a similar manner to Simulation Studies 2–3 except that for Experiment 2, *f* depended on whether an item was old or new, while in Experiment 1 all item values came from a single (new item) distribution. The CON group data from Experiment 2 was chosen as a point of departure for these simulations because these results bear the most resemblance to the previous simulation studies (i.e., normal adult participants who performed at levels greater than chance in priming and recognition). There were four free parameters for the simulation of the CON group data: σ_p, *s*, μ, *b*. The values of the parameters are shown in Table 6-1. Changes in σ_p and *s* from Simulation Studies 2–3 were necessary in order to characterize the ensuing effects of Conroy et al.'s (2005) different clarification task on RTs; that is, there is bound to be greater variability in RTs as a result of the longer clarification duration used (11 sec) (requiring an increase in σ_p), and priming/fluency effects in general were larger than in the previous simulated studies (requiring an increase in *s*). Changes in the *b* parameter across groups can be justified simply by the different baseline levels of responding in each of the groups (e.g., the H group produced the fastest RTs overall and therefore *b* is lowest here). The parameter values of σ_p, *s*, and μ were then set for the simulation of the MTL and H groups of this experiment. To then simulate the increased noise associated with the encoding and assessment of the memory signal in amnesia relative to the CON group, σ_f and σ_r were varied according to the severity of amnesia (greater for the MTL group). There were 8 degrees of freedom in the data for each condition (RTs to judged old and new items, RTs to actual old and new items, percent correct in recognition, d' for recognition, and the % of items judged old in the fast and slow identification medians).

Table 6-1. Parameters of the model in Simulation Study 4

Symbol	Meaning	Value					
		Conroy et al. (2005)					
		Exp. 2 CON	Exp. 2 H	Exp. 2 MTL	Exp. 1 CON	Exp. 1 H	Exp. 1 MTL
		Figures 6-1–6-2			Figure 6-3		
σ_f	Standard deviation of familiarity distributions (new/old)	0.20	**0.32**	**0.70**	0.20	0.32	0.70
σ_r	Standard deviation of recognition noise	0.20	**0.50**	**1.80**	0.20	0.50	1.80
σ_p	Standard deviation of priming noise	**1000**	1000	1000	1000	1000	1000
μ	Mean familiarity of old items	**0.37**	0.37	0.37	0	0	0
b	RT intercept	**8770**	**7650**	**9500**	8770	7650	9500
s	RT-Familiarity slope	**1450**	1450	1450	1450	1450	1450

Note. Exp. = Experiment. **BOLD** indicates that the parameter was varied to fit the data.

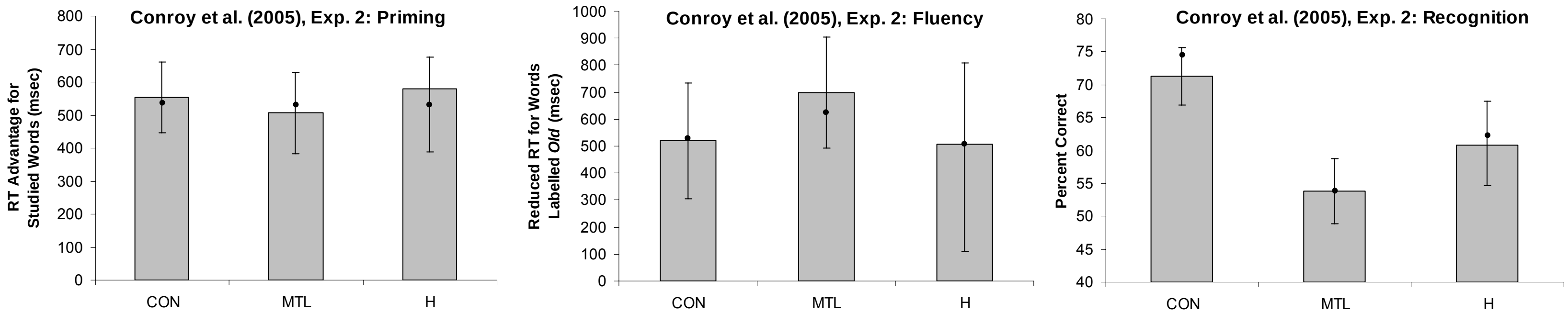

Figure 6-1. Priming, fluency, and recognition in Conroy et al. (2005) Experiment 2. Left Panel: Priming effect (identification time for new words minus identification time for old words). Centre Panel: Fluency effect (identification time for words judged new minus the identification time for words judged old). Right Panel: Percentage of correct responses (hits plus correct rejections) in the recognition task. Brackets show 95% confidence intervals of the mean for controls (CON), and range of scores (estimated from Conroy et al., 2005) for patients with medial temporal lobe lesions (MTL) or damage limited to the hippocampal regions (H). Circles indicate model results. Each simulated data point is based upon 40 observations for each old/new stimulus type and for 10,000 participants.

The data were simulated using the same number of trials as in the experiments (40 trials per old/new stimulus type in Experiments 1 and 2) and using 10,000 simulated participants. The simulation results for priming, fluency, and recognition for Experiment 2 are shown in Figure 6-1. The error bars on the CON data are 95% confidence intervals (estimated from Conroy et al., 2005) and those on the MTL and H data are the range of data from the patients in those groups (estimated from Conroy et al., 2005) (the range was used because of the limited amount of participant data in the MTL and H groups). It can be seen from Figure 6-1 that the model results lie within these intervals for all data points in Experiment 2 and that the model gives very close fits to the (mean) RTs for actual old/new and judged old/new items as shown in Figure 6-2 (where error bars are unknown).

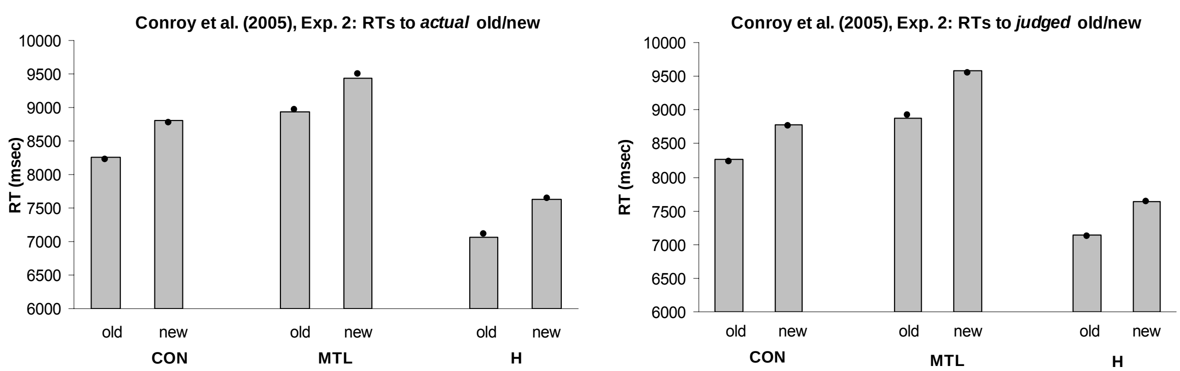

Figure 6-2. RTs to actual old/new words (left panel) and judged old/new (right panel) words in Conroy et al. (2005), Experiment 2. Grey bars indicate Conroy et al. (2005) data and circles indicate model results. Each simulated data point is based upon 40 observations for each old/new stimulus type and 10,000 simulated participants.

Conroy et al. (2005) also analyzed their recognition data with *d'*. These results and those of the simulations are presented in Table 6-2. It can seen that there is a very close correspondence between the data and simulation results.

Table 6-2. *Recognition performance (d') in Conroy et al. (2005) Experiment 2*

	Recognition accuracy (*d′*)	
	Conroy et al. (2005) Exp. 2	Simulation Exp. 2
CON	1.31	1.31
H	0.59	0.62
MTL	0.19	0.19

In Experiment 2, Conroy et al. (2005) also looked at whether a fluency effect was present within the subset of items that were new (i.e., whether RT(false alarm) < RT (correct rejection)). The CON group did not show this fluency effect for new items (false alarms: 8,839 msec, correct rejections: 8,831 msec), but the MTL group did (false alarms: 9,180 msec, correct rejections: 9,706 msec). Conroy et al. (2005) took this finding to mean that recognition was primarily based upon declarative memory in healthy adults (i.e., that responding was not based upon fluency), but that declarative memory did not affect fluency based responding in the MTL group (because the declarative memory necessary for recognition was lacking). (Conroy et al., 2005, did not report these results for the H group.) Consistent with Conroy et al.'s (2005) data, the model also predicted that there would be a large fluency effect for new items in the MTL group (false alarms: 9,182 msec, correct rejections: 9,767 msec), but contrary to Conroy et al. (2005), the model predicted that there would be a fluency effect—albeit a smaller one—for new items in the CON group (false alarms: 8,505 msec, correct rejections: 8,861 msec). I return to this difference between the prediction of the model and the result of Conroy et al. (2005) in the Discussion.

As found by Conroy et al. (2005), the model predicted that a greater percentage of items identified quickly (items with RTs less than the median) would be judged old than items identified slowly (items with RTs greater than the median). These results are presented in Table 6-3, and again it can be seen that the simulation results are comparable to those of Conroy et al. (2005).

Table 6-3. *Percentage of items judged old that were identified "quickly" (RT < median) and "slowly" (RT > median) in Conroy et al. (2005)*

	Percentage of items judged old			
	Identified Quickly		Identified Slowly	
	Conroy et al. (2005) Exp. 2	Simulation	Conroy et al. (2005) Exp. 2	Simulation
CON	57.8	59.9	38.7	40.0
H	62.5	58.9	49.0	40.9
MTL	66.3	58.6	46.2	41.5

To estimate the role of fluency in recognition judgments, Conroy et al. (2005) asked what recognition performance would have been had all of the items from the quick median half been judged old and all of those in the slow median half been judged new. These results are presented in Table 6-4. The low estimates of percent correct and d' were taken as evidence that even if judgments were entirely based upon RTs, the RTs could not have been a strong cue for recognition accuracy. A striking finding is that when the same analysis was performed on the simulated data, the estimates of recognition were also comparably low (see Table 6-4).

Conroy et al. (2005) then conducted an analysis which was intended to give an estimate of the contribution of fluency from priming to recognition. This involved calculating an estimate of percent correct based on the magnitude of the priming and fluency effects for old and new words (see Conroy et al., 2005, p. 19 for details). The recognition percent correct estimates for each group are presented in Table 6-5. These low estimates were taken to indicate that priming and fluency do not significantly contribute to recognition, and that recognition must therefore be based upon some other memorial source. Again, the same analysis was performed on the simulated data, the

Table 6-4. *Estimates of recognition performance if all items identified quickly were judged old and items identified slowly were judged new in Conroy et al. (2005), Experiment 2*

	Percent Correct		*d'*	
	Conroy et al. (2005) Exp. 2	Simulation	Conroy et al. (2005) Exp. 2	Simulation
CON	61.6	60.0	0.59	0.51
H	55.8	59.3	0.29	0.47
MTL	61.3	57.2	0.57	0.37

results of which are also presented in Table 6-5. The percent correct estimates were also very low. Thus, the low levels of recognition accuracy estimated from the fluency effects calculated by Conroy et al. (2005) are not inconsistent with a model in which a single memory strength variable mediates priming and recognition. Because this variable is subjected to independent sources of noise for each task, it can appear as if there is a lack of relationship between priming and recognition, even though they are driven by the same memory strength signal.

Although the primary concern here was to simulate the results of Experiment 2, further support for the robustness of the model was sought by applying it to Experiment 1. For all groups, the same parameter values were retained from simulation of

Table 6-5. *Estimates of recognition performance given the observed magnitude of the fluency effect within each group for old and new words in Conroy et al. (2005), Experiment 2*

	Percent Correct Estimate	
	Conroy et al. (2005), Exp 2	Simulation
CON	52.2	52.0
H	50.8	51.7
MTL	52.6	51.3

Experiment 2, except for μ which was decreased from 0.37 to zero (because there was no study phase and therefore no influence of memory at test in Experiment 1). The model results for the fluency effects in each group, and also for the RT values from which they were derived, are presented with Conroy et al.'s (2005) data in Figure 6-5. With the exception of the fluency effect for the MTL group, all the model results lie within the range of results observed by Conroy et al. (2005).

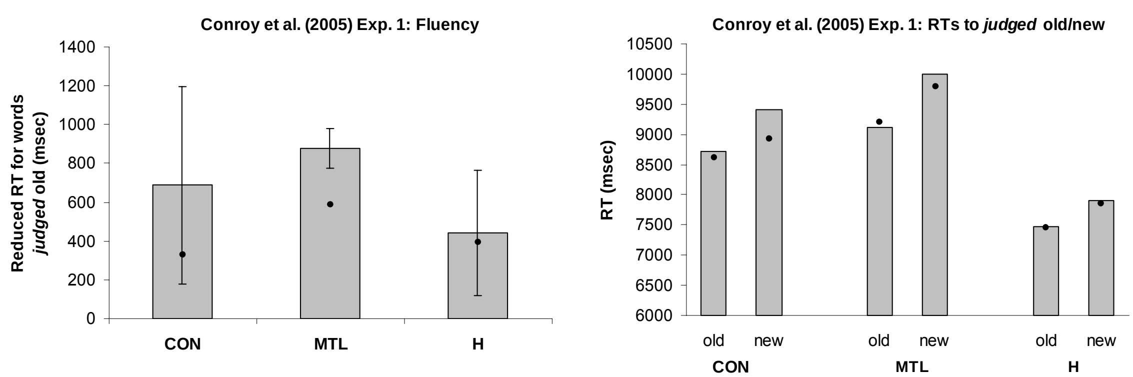

Figure 6-3. Fluency effects (left panel) and RTs to judged old and judged new words in Conroy et al. (2005), Experiment 1. Brackets show 95% confidence intervals of the mean for the CON group, and range of individual scores (estimated from Conroy et al., 2005) for the H and MTL groups. Circles indicate simulations based upon 40 observations per stimulus type for 10,000 participants.

In Experiment 1, Conroy et al. (2005) also found that more old judgments were made to words identified in the quick half than in the slow half, and this effect did not differ significantly across groups. These results are presented with the simulation results in Table 6-6. This shows that the model predicts fluency effects for all groups even when all items at test are new.

Table 6-6. *Percentage of items judged old that were identified "quickly" (RT < median) and "slowly" (RT > median) in Conroy et al. (2005), Experiment 1*

	Percentage of items judged old			
	Identified "quickly"		Identified "slowly"	
	Conroy et al. (2005) Exp. 1	Simulation	Conroy et al. (2005) Exp. 1	Simulation
CON	64.4	56.3	48.4	43.8
H	59.2	57.0	50.0	42.7
MTL	61.3	58.4	35.0	41.8

6.2 Discussion

The model reproduces the dissociations between priming, recognition, and fluency in amnesia reported by Conroy et al. (2005). By assuming that there is a larger degree of noise in the encoding and assessment of the memory signal in amnesics than controls, the model predicted fluency and priming effects for the amnesic groups that were comparable to controls, despite impaired recognition in the H group relative to the CON group, and near chance recognition in the MTL group. Furthermore, as calculated by Conroy et al. (2005), the model also predicted that, even if judgments were based solely on speed of identification, recognition would be low. The same parameter values were then applied to Conroy et al.'s (2005) Experiment 1 where there was no influence of memory and the model still predicted fluency effects in all three groups of participants.

Conroy et al. (2005) found that the RTs for false alarms were shorter than correct rejections for the MTL group in both experiments indicating a relationship between fluency and recognition judgments, but this was only the case for the CON group in Experiment 1, suggesting that the presence of declarative memory interferes with fluency-based responding (in Experiment 2). The model predicted an effect for the CON group in both experiments (likewise for the MTL group). The evidence regarding this discrepancy in the CON data from other studies which have used comparable study/test conditions to Conroy et al. (2005) is mixed: Verfaellie and Cermak (1999) also found no difference in RTs for false alarms and correct rejections, whereas other studies have reported a numerical trend for false alarms to be faster than correct rejections (see e.g., Johnston et al. 1985; 1991; Stark & McClelland, 2000). Indeed, in Experiment 9 of Chapter 5 there was a numerical trend for RTs to false alarms to be shorter than those of correct rejections (1368 msec vs. 1406 msec) when recognition memory was good.

In any case, the difference between the RTs to correct rejections and false alarms in Conroy et al. (2005) was smaller in the CON group than the MTL group (-8 msec vs. 526 msec). In line with this trend, the model also predicts that this difference is smaller in the CON group than the MTL group (356 msec vs. 585 msec).

The model predicted very slightly lower priming effects in the MTL and H groups than the CON group. This is necessarily the case since priming and recognition depend upon the same memory source in the model, and variables will therefore tend to have similar effects upon performance in each task. This prediction therefore conflicts with the notion that priming is intact in amnesia (e.g., Hamman & Squire, 1997a), an issue which has proven controversial (see e.g., Ostergaard, 1999). It is relevant to note, however, that Verfaellie and Cermak (1999, Experiment 2), whose study has much in common with Conroy et al. (2005), did find that priming was impaired in amnesics relative to controls.

Despite the fact that priming and recognition are mediated by a common memory source in the model, it predicted a priming effect (of 530 msec) for the MTL group when recognition was, for all practical purposes, no different from chance (53.9% of the items were recognised correctly, $d' = 0.19$; see Figure 6-3). The severely amnesic individual E. P. is reported to perform normally in tests of priming despite chance performance in tests of recognition and this pattern is typically regarded as compelling evidence for priming and recognition being mediated by multiple memory systems (e.g., Hamann & Squire, 1997a; Stark & Squire, 2000). However, as the above simulation results show, this pattern is compatible with the single-system model (see also Kinder & Shanks, 2001): It would be difficult to achieve sufficient statistical power to detect an effect of the magnitude predicted (3.9% greater than chance). Even if it can be convincingly shown that E. P.'s recognition memory has been completely eliminated by his amnesia and yet his priming performance is untouched (as some have argued), the view here is that it is worth exercising caution in drawing strong conclusions from individual cases; ideally one would like to see replications of this pattern in other patients.

The results also speak to dual-process accounts of recognition which propose that fluency from priming can contribute to recognition (e.g., Jacoby & Dallas, 1981;

Johnston et al., 1991; Mandler, 1980). As shown by Conroy et al. (2005), it is difficult to obtain direct evidence for this notion when one estimates the magnitude of recognition based on identification RTs to old and new items. A similar conclusion was reached by Poldrack and Logan (1997): Participants in their study made lexical decision judgments to old or new items and gave a recognition judgment after each decision. Discriminability between old and new items in the lexical decision task was measured with dRT, the distance between the standardized RT distributions for old and new items. Values of dRT were significantly less than the observed values of d' for the recognition task, and could account for only a small proportion of recognition discrimination suggesting that fluency (response speed) could not have been the sole factor in recognition judgments. Consistent with these findings, when recognition was estimated in the model in a similar manner to Conroy et al. (2005), the estimated contribution of fluency was also minimal. From the perspective of the model, the fluency from priming does not make a direct contribution to recognition, but rather, a common memory source supports above-chance performance in each task (i.e., there is a common cause rather than a causal chain). Thus, although low estimates of the contribution of fluency to recognition seem to suggest a lack of a relationship between priming and recognition, this result is predicted by a single-system account.

In conclusion, contrary to the interpretation provided by Conroy et al. (2005), the simulations in this chapter suggest that the findings from their study are not inconsistent with a single-system view of priming, recognition, and fluency.

Chapter 7: Predicting 2AFC Recognition From Identification RTs: Implications for the Single-System Model

The single-system model predicts that the greater the value of f of an item, the shorter its identification RT will be and the greater the likelihood it will be judged old. Chapters 5 and 6 showed that this prediction was generally supported in the CID-R task. Items judged old tended to have shorter RTs, regardless of whether the item was old or new. Experiments 10 to 12 in this chapter explore this prediction further by applying the model to a modified CID task in which the recognition judgments are two-alternative forced choice (2AFC), rather than old-new. On a typical 2AFC trial an old and new item are presented and the participant must judge which of the two items they think was presented previously (Shepard, 1967). Signal-detection models of 2AFC typically assume that the item which has the greater strength is then judged old (Wickelgren, 1968; Macmillan & Creelman, 2005). If identification also depends upon the same strength variable, then the identification RT of the item judged old will tend to be shorter than the item not chosen. This raises the interesting possibility that the item judged old on a 2AFC trial can be predicted by looking at which of the two items has the shorter CID identification RT.

This relationship, however, is not uniquely predicted by the single-system model. A "dual-system" model, which is identical in all respects to the single-system model except that the value of f for each item is sampled independently for identification and recognition (such that one value of f is sampled to generate an item's RT and then *another* value of f is sampled from the same distribution to generate its value of J_r), would also predict this effect. As long as the mean *f* for old items is greater than that of new items, old items will tend to have shorter RTs than new items and will also be judged old more often than new items. There will therefore be trials on which the item judged old also has the shorter RT meaning that there will be some 2AFC trials that can be predicted from identification RTs, even if the sources of *f* were independent for each task. Thus, an item's value of *f* for each task need not be the same (as is assumed by the single-system model) in order for this effect to occur.

One way of teasing apart the predictions of the single- and dual-system versions of the model is to present all possible combinations of old and new item trials to participants: That is, 2AFC trials containing one old and one new item (an old-new trial), two new items (a new-new trial), and two old items (an old-old trial). Although it is somewhat unconventional to present old-old and new-new 2AFC trials in recognition, the models make different predictions for each type of trial. The single-system model predicts that the item judged old on any type of 2AFC trial will have the shorter identification RT. In contrast, while the dual-system model predicts that the outcome of old-new trials can be predicted by RTs, the outcome of old-old and new-new 2AFC trials cannot be predicted by RTs: when the means of the *f* distributions are identical for both items in a 2AFC trial, even though one of the items may have a higher *f* than the other (and is therefore more likely to be judged old), this item will not necessarily have a greater *f* when its RT is generated because *f* is re-sampled for each item. Thus, if the outcome of the 2AFC can be predicted at levels greater than chance, regardless of the type of 2AFC trial (old-old, new-new, old-new), then this would support the single-system model. However, if the only trials that can be predicted by RTs are old-new trials then this would favour the dual-system model.

In the study phase of Experiments 10 to 12, words were presented using the CID procedure. At test, participants again identified (old or new) items in the CID procedure but after every two trials the words from those trials were presented again in clear view for a 2AFC judgment. (This test phase task is henceforth referred to as the CID-2AFC task.) Experiment 10 attempted to establish whether an effect could be established for old-new, new-new and old-old trials. Experiment 11 was essentially a replication of Experiment 10. In Experiment 12 an attempt was made to directly test a further prediction of the single-system model.

7.1 Experiments 10–12

7.1.1 General Method

The general method of Experiments 10–12 is first described, and details of the differences between each experiment are given later.

7.1.1.1 Participants

The participants in Experiments 10 and 11 were recruited from a psychology subject database, and those in Experiment 12 participated in order to fulfill a first-year laboratory class requirement. All reported having normal or corrected-to-normal vision, reported English as their first language, and were paid for their participation. They were tested individually in sound-dampened cubicles. Each experiment was fully automated and the experimenter was not present in the room during the course of the experiment.

There were 37 participants in Experiment 10, 32 in Experiment 11, and 92 in Experiment 12. Their ages ranged from 18 to 39 (M = 20.4 years).

7.1.1.2 Materials and Design

All stimuli were words selected with the constraint that they had 4 letters, had a frequency of occurrence of 5 to 250 per million (Kucera & Francis, 1967) and a maximum score of 500 on imagability and concreteness scales (Coltheart, 1981).

Two word lists were constructed (each consisting of 100 words in Experiments 10 and 11, and 48 words in Experiment 12) to be the old or new stimuli, counterbalanced across participants. Twenty additional words were selected for each experiment with the same constraints as the other words. These words were presented in the study phase as primacy and recency buffer trials. None of these words appeared at test.

7.1.1.3 Procedure

On each study trial a word was presented using the CID procedure. At the start of each trial a mask (a row of hash marks ####) was presented for 500 msec to orient the participant. Next, a word was presented in lowercase 20 pt Courier font for 17 msec. The mask was then presented in slightly larger 26 pt Courier font for 233 msec, forming a 250 msec presentation block. The word presentation was then repeated, this time with the stimulus duration increasing by 17 msec, and the presentation of the mask again following for the remainder of the presentation block. Presentation continued in this way, with the total stimulus plus mask time remaining constant, until a response was made or the stimuli presentation time equaled that of the presentation block (250 msec).

Participants were told to press a button when they could identify the word, and then name it aloud (in Experiment 10 and 11) or type it on a keyboard (Experiment 12). There were 120 study trials in Experiments 10 and 11, consisting of 100 target trials, 10 primacy trials, and 10 recency trials. There were 68 study trials in Experiment 12 which comprised 48 target trials, 10 primacy trials, and 10 recency trials.

Each word at test was also presented using the CID procedure, but after every two CID trials both words were presented again, side by side (with the left and right positions randomly assigned) for a 2AFC recognition judgment. CID trials were arranged into an equal number of old-old, new-new, old-new and new-old pairs (where the first pair member is the type of item that was presented first). The latter two types of pairs were included to counterbalance the order of presentation of old and new items. The assignment of words to the pairs was random. In Experiments 10 and 11 there were 100 pairs of test trials in total, arranged into 25 old-old, new-new, old-new and new-old pairs. In Experiment 12 there were 48 pairs of test trials in total, arranged into 12 old-old, new-new, old-new and new-old pairs. In the test phase instructions, participants were again told that words would gradually clarify into view and that they must press a button when they could identify each word and then name it aloud. In addition, they were told that after some trials, two words would be presented with the question “Which is the old word?” They were told that an old word was a word that was presented in the first stage of the experiment and that they should indicate which word they thought was old by pressing a button underneath that word (marked “left word” or “right word”).

If a word was not identified before the termination of a CID trial (3750 msec) then the corresponding 2AFC recognition trial was not presented. Any misidentification

CID trials were excluded from all subsequent analysis and, if the error occurred at test, the corresponding 2AFC trial was not analysed.

An alpha level of .05 was used for all statistical tests, and all *t*-tests were two-tailed. The assumption of sphericity was tested using Mauchley's *W* statistic. Huynh-Feldt's correction (Huynh & Feldt, 1976) was applied to the degrees of freedom when the assumption of sphericity was violated.

7.2 Experiment 10

7.2.1 Experiment 10 Results

There were very few misidentifications at study and test (each participant made an error on less than 1% of trials). The mean RT at study was 1330 msec (SE = 40). Recognition performance on old-new and new-old 2AFC trials was measured using percent correct, where a correct response was defined as correctly judging the old word to be old. Recognition performance was significantly greater than that expected by chance (50%) (M = 78.9%, SE = 1.4), $t(36) = 20.16$, $p < .001$. Table 7-1 shows the identification RTs for old and new words (in this and subsequent experiments). Priming was shown by significantly shorter identification RTs to old than new items, $t(36) = 8.83$, $p < .001$ (SE difference = 6 msec). There was no significant correlation between the amount of priming and recognition (percent correct) across participants, $r(36) = .08$, $p = .65$.

Table 7-1. *Identification RTs (msec) for new and old items in Experiments 10–12*

	M identification RT		*SD* identification RT	
	new	old	new	old
Exp. 10	1241	1184	235	223
Exp. 11	1331	1275	255	233
Exp. 12	1320	1259	287	273

7.2.1.1 Prediction of 2AFC Responses By Identification RTs

For the sake of brevity, a "predicted 2AFC trial" refers to a trial on which the item that was judged old was also the item which had the shorter identification RT. The percentage of predicted 2AFC trials was calculated for each pair type. There was no significant difference between the percentage of predicted 2AFC trials for old-new (M = 56.1%) and new-old (M = 54.4%) trials, $t(36) = 0.58$, $p = .57$ (SE difference = 2.9), indicating that there was no effect of presentation order, and so the prediction data for this pair type was collapsed for all subsequent analysis.

One sample t-tests revealed that the percentage of predicted old-new 2AFC trials was greater than expected by chance (50.0%), M = 55.3%, SEM = 1.2, $t(36) = 4.70$, $p < .001$. There was a similar, but only marginally significant trend for new-new trials, M = 53.4%, SEM = 1.78, $t(36) = 1.93$, $p = .062$. In contrast, the percentage of predicted old-old 2AFC trials was not significantly different from chance, M = 49.2%, SEM = 1.5, $t(36) = -0.53$, $p = .60$.

A repeated measures ANOVA revealed a significant difference between the percentage of predicted 2AFC trials for each pair type, $F(2, 72) = 4.63$, $p = .016$. Further analysis revealed that a significantly greater percentage of old-new 2AFC trials could be predicted than old-old 2AFC trials, $t(36) = 3.43$, $p = .002$. Similarly, a greater number of new-new trials could be predicted than old-old trials, but this difference was only marginally significant, $t(36) = 1.74$, $p = .09$. There was no significant difference between new-new and old-new trials, $t(36) = 0.98$, $p = .33$.

7.2.1.2 Differences Between the Identification RTs of Pair Members

In order to check that the identification RTs of the items presented for each 2AFC trial did in fact differ, and to get some idea about the size of this difference, the absolute difference between the identification RTs of the items on each 2AFC trial was calculated, and the mean across participants was determined. A repeated measures ANOVA indicated that the mean absolute difference in identification RTs within each pair of trials differed according to whether the trial was old-new ($M = 276$ msec, $SEM = 13$), old-old ($M = 266$ msec, $SEM = 14$) or new-new ($M = 299$ msec, $SEM = 18$), $F(2, 72) = 5.15$, $p = .012$. Further comparisons indicated that the mean absolute difference in RTs between the items of new-new pairs was significantly greater than that of old-old pairs, $t(36) = 2.68$, $p = .011$, and also old-new pairs, $t(36) = 2.09$, $p = .044$. However, the mean difference between the items of old-old pairs was not significantly different to that of old-new pair types, $t(36) = 1.28$, $p = .21$.

This analysis confirms that the identification RTs of the items in each pair did actually differ, but that the size of this difference varied according to the type of pair. The magnitude of the difference was greatest between the items of new-new pairs and smallest between the items of old-old pairs. Furthermore, the magnitude of the difference was greater for new-new pairs than old-new pairs. This latter finding is interesting when considered with the prediction by RTs results: The greater discrepancy between the identification RTs of items in new-new trials suggests that there are larger differences in *f*, but, this difference did not manifest itself in a greater percentage of predicted new-new than old-new trials. Furthermore, there was a difference in the RTs to items of old-old trials, but this difference did not manifest itself in the prediction data. On the other hand, it should be noted that the difference in old-old, new-new, and old-new trials is confounded with baseline identification RTs. RTs to new items are longer than old items, which could explain why the variability in the RTs to the items of new-new pairs is greatest.

7.3 Experiment 11

The results of Experiment 10 offered mixed support for the single-system model: as predicted, the outcome of the 2AFC could be predicted for old-new and (weakly) new-new trials, but the absence of an effect for old-old trials was not predicted by the model. Experiment 11 was a replication of Experiment 10, but the following changes were made in attempt to see whether the outcome of old-old 2AFC trials could be predicted by RTs, and also to throw additional light on the findings from Experiment 10: First, the 2AFC instructions were modified to encourage participants to select the item that they were more confident was old. The differences in the identification RTs between the items of old-old pairs in Experiment 10 suggest that there are differences in *f* of the items of old-old pairs, but for some reason this is not reflected in the prediction by RT data. Instructing participants to select the item they are more sure is old may encourage them to select the item which has the greater strength, and an effect for old-old items may emerge. Second, the 2AFC judgment latency—the latency from the presentation of the 2AFC probe to the onset of the judgment response—was measured.

Differences in recognition latencies between pair types may help to throw light on the nature of the effect.

7.3.1 Results

There were very few misidentifications at study (each participant made an error on approximately M = 2% of trials). The mean identification RT at study was 1414 msec (SE = 46). There were very few misidentifications at test (approximately 1% of trials). 2AFC recognition performance, as measured by percent correct on old-new and new-old trials, was significantly greater than that expected by chance alone (M = 80.4%, SE = 1.8), $t(31) = 17.25$, $p < .001$. Priming was shown by significantly faster identification latencies to old (1275 msec) than new items (1331 msec), $t(31) = 6.23$, $p < .001$ (SE difference = 9). There was no significant correlation between priming and recognition scores $r(31) = .02$, $p = .92$.

7.3.1.1 Prediction of 2AFC Trials by Identification RTs

In a similar manner to Experiment 10, the percentage of predicted 2AFC trials was calculated for each type of pair. There was no significant effect of order for old-new (M = 56.6%) and new-old (M = 51.6%) pairs, $t(31) = 1.65$, $p = .11$ (SE difference = 3.0), and so the prediction data for these pairs were collapsed for all subsequent analysis.

The percentage of old-old 2AFC outcomes that could be predicted by RTs was slightly larger than in Experiment 10, suggesting that the instructional manipulation was somewhat effective, however, the effect was only marginally significantly greater than chance (50%), M = 53.2%, SEM = 1.6, $t(31) = 2.03$, $p = .051$. The percentage of predicted old-new 2AFC trials was significantly greater than chance, M = 54.1%, SEM =

1.13, $t(31) = 3.66$, $p < .001$, as was the percentage of predicted new-new 2AFC trials, $M = 56.5\%$, $SEM = 1.8$, $t(31) = 3.68$, $p = .001$.

A repeated measures ANOVA indicated that, unlike Experiment 10, there was no significant difference between the percentage of predicted 2AFC trials for each pair type, $F(2, 62) = 1.41$, $p = .25$. However, for consistency with Experiment 10, pairwise comparisons were conducted: There was no significant difference between old-old and old-new pair types, $t(31) = 0.53$, $p = .60$, and no significant difference between new-new and old-old pair types, $t(31) = 1.44$, $p = .16$. There was also no significant difference between new-new and old-new pairs, $t(31) = 1.17$, $p = .25$. Thus, like Experiment 10, old-new and new-new outcomes could be predicted by RTs to the same extent, but it is unclear whether old-old outcomes could be.

7.3.1.2 Differences Between the Identification RTs of Pair Members

In a similar manner to Experiment 10, the absolute difference in identification RTs between pair members was calculated. A repeated measures ANOVA indicated that the mean absolute difference in RTs for items within pairs differed across old-old ($M = 275$ msec, $SEM = 15$), old-new ($M = 282$ msec, $SEM = 14$) and new-new ($M = 310$ msec, $SEM = 14$) 2AFC trials, $F(2, 62) = 3.94$, $p = .02$.

As was found in Experiment 10, the mean absolute difference in RTs between the items of new-new pairs was significantly greater than that of old-old pairs, $t(31) = 2.52$, $p = .02$, and also old-new pairs $t(31) = 2.14$, $p = .04$. As in Experiment 10, the mean difference in RTs between the items of old-old pairs was not significantly different to that of old-new pair types, $t(31) = 0.57$, $p = .58$.

7.3.1.3 2AFC Recognition Latencies

There was no effect of order on the mean recognition latency for old-new (*M* = 1548 msec, SEM = 77) and new-old (*M* = 1538 msec, *SEM* = 78) pairs, $t(31) = 0.14$, $p = .89$, and so the judgment latency data for these pairs were collapsed for all subsequent analysis. A repeated measures ANOVA indicated a significant difference between the 2AFC judgment latencies for each type of 2AFC trial, $F(2, 62) = 5.86$, $p = .007$. Paired-sample *t*-tests showed that the latencies for new-new trials (*M* = 1796 msec, *SEM* = 87) were significantly longer than for old-new trials (*M* = 1543 msec, *SEM* = 69), $t(31) = 4.16$, $p < .001$, and also old-old trials (*M* = 1644 msec, *SEM* = 101), $t(31) = 2.15$, $p = .039$. The difference between old-old trials and old-new trials was not significant, $t(31) = 1.13$, $p = .27$.

7.4 Experiment 12

Given the small size of the 2AFC-predicted-by-identification-RT effects, the aims of Experiment 12 were firstly, to replicate the overall pattern of results from Experiments 10 and 11 and secondly, to increase the size of these effects. The single-system model predicts that as σ_f increases, the percentage of trials that can be predicted by RTs will also increase (for all pair types). When σ_f is higher, the average discrepancy between the values of f of the 2AFC items will be greater. The relative size of f of the items are therefore more likely to persist even after being combined with noise (for the generation of J_r and the RT), and the item that has a larger value of f is likely to have a shorter identification RT and the greater value of J_r. When σ_f is relatively low, the average discrepancy in f between items is smaller and the relative size of f of the two items is more easily distorted by the addition of noise. The percentage of trials on which the item with the greater J_r also has the shorter RT will therefore be lower when σ_f is low. One way of manipulating σ_f across conditions might be by varying the frequency of occurrence. Frequency manipulations affect recognition, for example, recognition performance is typically better for low frequency words than high frequency words (e.g., Glanzer & Adams, 1985). In an attempt to directly test this prediction of the model, there were two conditions in Experiment 12: one in which σ_f was manipulated in order to be low and another in which σ_f was manipulated to be high.

Two sets of stimuli lists were constructed, both had the same median frequency (the median was used as the measure of central tendency because the distributions of frequency were positively skewed), but in one list the variability of frequency of occurrence was low and in the other it was high. If differences in frequency variance are akin to changes in σ_f , the model predicts that the percentage of trials that can be predicted on the basis of RTs will be larger in the group with high frequency variance stimuli lists than the low frequency variance group.

In Experiment 12, two lists of 48 words were compiled for the high- and low-frequency variance conditions. Each list acted as the old or new stimuli within each condition, counterbalanced across participants. There was no overlap in stimuli between lists within each condition.

The median frequency of occurrence (Kucera & Francis, 1967) of the low-frequency variance group was 63.5, interquartile range = 62.8, and that of the high-frequency variance group was 60.0, interquartile range = 156.5. A Kruskal-Wallis test indicated that there was no significant difference in the median frequency of occurrence between groups. There was also no significant difference between the median frequency of occurrence when comparing the stimulus lists within the high versus low frequency variance conditions.

7.4.1 Results

There were very few misidentifications at study (*M* < 2% of trials). The mean RT at study was 1430 msec (*SE* = 33). There were very few misidentifications at test (*M* = 2% of trials). There were no significant effects of the frequency manipulation or significant interactions of this factor with any other which suggests that the manipulation was not effective and so all data were collapsed across the frequency condition. Recognition performance was significantly greater than that expected by chance, *M* = 81.9% correct, *SEM* = 1.0, $t(91) = 33.52$, $p < .001$. Priming was also significantly greater than chance, $t(91) = 8.08$, $p < .001$ (*M* = 61 msec, *SEM* = 8), indicating that old items at test were identified faster than new items. Again, no significant correlation was observed between priming and recognition, $r(91) = .07$, $p = .51$.

7.4.1.1 Prediction of 2AFC Trials by Identification RTs

There was no effect of order for old-new (*M* = 55.7%) and new-old (*M* = 54.9%) pairs, $t(91) = 0.38$, $p = .70$, and so the prediction data for these pairs were collapsed for all subsequent analysis. One sample *t*-tests revealed that 2AFC judgments could be predicted by RTs at levels greater than chance (50%) for old-new trials (*M* = 55.2%, *SEM* = 1.1), $t(91) = 4.59$, $p < .001$, and also new-new trials (*M* = 54.1%, *SEM* = 1.4), $t(91) = 2.96$, $p = .004$. However, the percentage of old-old trials that could be predicted by RTs was not greater than chance (*M* = 51.3%, *SEM* = 1.6), $t(91) = .80$, $p = .43$.

Despite the similar trend in prediction data to Experiments 10 and 11, a repeated measures ANOVA revealed no significant difference between the percentage of trials that were predicted for each pair type, $F(2, 182) = 2.43$, $p = .11$. However, planned comparisons revealed that, in line with Experiment 10, significantly more old-new trials could be predicted than old-old trials, $t(91) = 2.21$, $p = .029$. There was a trend for more new-new trials to be predicted than old-old trials but this was not significant, $t(91) = 1.45$, $p = .15$. As was found in Experiments 10 and 11, there was no significant difference between old-new and new-new pair types, $t(91) = 0.55$, $p = .58$.

7.4.1.2 Differences Between the Identification RTs of Pair Members

Again, the absolute difference between the identification RTs of pair members was calculated. A repeated measures ANOVA indicated that, unlike Experiments 10 and 11, the mean difference between the identification RTs of pair members did not differ for old-old (M = 286 msec, SEM = 11), old-new (M = 286 msec, SEM = 11) and new-new (M = 300 msec, SEM = 10) pairs, $F(2, 182) = 1.05$, $p = .35$. The magnitude of the difference for old-old pairs was not significantly different to that of new-new pairs, $t(91) = 1.13$, $p = .26$, or old-new pairs, $t(91) = 0.05$, $p = .96$. The magnitude of the difference for old-new pair types was not significantly different to that of new-new pair types, $t(91) = 1.29$, $p = .20$.

This analysis indicates that, contrary to Experiments 10 and 11, although there were differences in the RTs to items within pair types, the magnitude of this difference did not differ reliably for each type of pair.

7.4.1.3 2AFC Recognition Latencies

There was no effect of order on recognition latencies for old-new (M = 2210 msec, SEM = 87) and new-old (M = 2268 msec, SEM = 85) pairs, $t(91) = 0.45$, $p = .65$, so the data for these pairs was collapsed for all subsequent analysis. A repeated measures ANOVA indicated that the 2AFC judgment RTs significantly differed across pair types, $F(2, 182) = 19.03$, $p < .001$. Paired-samples t-tests revealed that participants took significantly longer to make 2AFC judgments on new-new trials (M = 2976 msec, SEM = 139) than old-new trials (M = 2239 msec, SEM = 58), $t(91) = 5.20$, $p < .001$, and also old-old trials (M = 2427 msec, SEM = 99), $t(91) = 4.39$, $p < .001$. RTs to old-old trials were longer than old-new trials but, as was found in Experiment 11, this difference was not significant, $t(91) = 1.84$, $p = .07$.

7.5 Summary of Experiments 10–12

Given the similarity between the procedures of Experiments 10–12, the results were pooled and analysed as one data set. In the subsequent analysis, no significant effects of Experiment (10, 11, or 12) were found when it was included as a factor. The one exception to this was for recognition judgment latencies, which significantly differed between Experiments 11 and 12. Thus, these latencies were not analysed as one data set.

Percent correct was significantly greater than that expected by chance (M = 80.9%, SE = 0.7), $t(160) = 42.47$, $p < .001$. Priming was shown by significantly faster identification latencies to old (1245 msec) than new items (1304 msec), $t(160) = 12.11$, $p < .001$ (SE difference = 5). There was no significant correlation between priming and recognition scores $r(160) = .06$, $p = .42$.

7.5.1 Prediction of 2AFC trials by Identification RTs

The predicted 2AFC trial data collapsed across Experiments 10–12 is shown in Figure 7-1. There was no effect of order for old-new (*M* = 55.9%) and new-old (*M* = 54.1%) pairs, *t*(160) = 1.22, *p* = .23, and so the prediction data for these pairs were collapsed for all subsequent analysis.

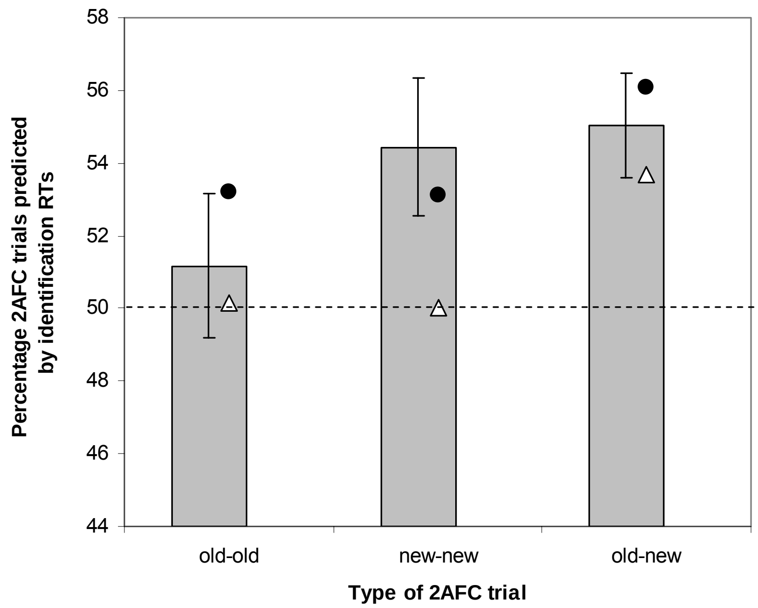

Figure 7-1. Percentage of 2AFC trials on which the item judged old also had the shorter identification RT (predicted 2AFC trial). Bars indicate data collapsed from Experiments 10 to 12, error bars indicate 95% confidence intervals of the mean. The closed black circles are the simulation results of the single-system model, and the open triangles are the results of the dual-system model.

One sample *t*-tests revealed that the outcome of old-old 2AFC trials could not be significantly predicted by RTs at levels greater than chance, *M* = 51.2%, *SEM* = 1.0, *t*(160) = 1.16, *p* = .25, but that of old-new 2AFC trials could, *M* = 55.0%, *SEM* = 0.7,

$t(160) = 6.92$, $p < .001$, and so could the outcome of new-new 2AFC trials, $M = 54.4\%$, $SEM = 1.0$, $t(160) = 4.62$, $p < .001$.

A repeated measures ANOVA revealed a significant difference between the percentage of trials that were predicted for each pair type, $F(2, 320) = 5.40$, $p = .005$. Further analysis revealed that there was a significant difference between old-old and old-new pair types, $t(160) = 3.31$, $p = .001$, and a significant difference between new-new and old-old pair types, $t(160) = 2.39$, $p = .018$. However, there was no significant difference between new-new and old-new pairs, $t(160) = 0.48$, $p = .63$.

7.5.2 Differences Between the Identification RTs of Pair Members

A repeated measures ANOVA indicated that the magnitude of the difference between the identification RTs of pair members differed across old-old, new-new and old-new pair types, $F(1.93, 308.16) = 5.46$, $p = .005$. The magnitude of the difference for new-new pairs ($M = 302$ msec, $SEM = 8$) was significantly greater than old-new pairs ($M = 283$ msec, $SEM = 7$), $t(160) = 2.61$, $p = .01$, and also old-old pairs ($M = 279$ msec, $SEM = 8$), $t(160) = 2.80$, $p = .006$. The magnitude of the difference for old-new pairs was not significantly different to that of old-old pairs, $t(160) = 0.52$, $p = .60$.

7.6 Simulation Study 5: CID-2AFC Performance

7.6.1 Single-System Model

In order to simulate performance in the CID-2AFC task (rather than the CID-R task), the decision rule for recognition needed to be modified. Signal-detection models of 2AFC typically assume that participants compare the strengths of the two alternatives and the item with the greater strength is judged old (e.g., Macmillan & Creelman, 2005).

This decision rule was adopted here and on each simulated 2AFC trial the item with the larger value of J_r was judged old.

For each old and new item, a single value of f was randomly sampled on each trial from the relevant distribution. The value of f was then combined with one source of noise (Equation (5-1)) to generate a RT which formed the basis of the priming data. The same value of f was then combined with another source of noise (Equation (3-2)) to determine J_r, and on each 2AFC trial, the item with the greater value of J_r was judged old. As was the case for the analysis of the experimental data, a 2AFC trial was classified as "predicted" if the item that was judged old also had the shorter RT. Thus, the same memory signal (familiarity) contributed to priming and recognition.

Because of the variability in results across Experiments 10 to 12, the data collapsed across experiments were simulated rather than the data from each individual experiment. Simulations were conducted using the same number of trials as Experiments 10 and 11 (100 old and 100 new stimuli) and with 10,000 simulated participants. The parameter values from Simulation Study 2 (simulation of Experiment 9, a replication of Stark & McClelland, 2000) were used as starting values for this simulation because of the similarities between Experiment 9 and Experiments 10 to 12 (there was priming and recognition in a CID paradigm, and the participants were normal adults). The values of the parameters used in this simulation are shown in Table 7-2. It was necessary to lower μ from its starting value to ensure that the priming effect and recognition percent correct approximated the data. The value of b was also lowered, which can be justified by the overall shorter identification RTs in these experiments than in Experiment 9. No other parameter values were varied; the aim was to observe the model results for the percentage of trials that could be predicted by identification RTs when the parameter values were fixed in this way.

The simulation results for the priming effect lay within the 95% confidence interval—*M* model = 67 msec; *M* Experiments 10 to 12 = 59 msec; 95% + = 68 msec, 95% - = 49 msec—as did the identification RTs to old and new items (see Figure 7-2). However, the simulation result for recognition percent correct was just below the 95% confidence interval—*M* model = 78.9% correct, *M* Experiments 10 to 12 = 80.9%; 95% + = 82.3%; 95% - = 79.5%. The mean absolute difference in the identification RTs to items in a pair was also analyzed. For all types of pair, the magnitude of this difference was slightly larger than was observed in the data and did not differ across pairs—old-old = 331 msec; new-new = 330 msec; old-new = 335 msec.

Table 7-2 *Parameters of the single- and dual-system models for Simulation Study 5*

Symbol	Meaning	Value
σ_f	Standard deviation of familiarity distributions (new/old)	0.20
σ_r	Standard deviation of recognition noise	0.20
σ_p	Standard deviation of priming noise	290
μ	Mean familiarity of old items	**0.32**
b	RT intercept	**1300**
s	RT-Familiarity slope	210

Note. **BOLD** indicates that the parameter was varied from Simulation Study 2 to fit the data.

Figure 7-1 shows the results of the single-system model for the percentage of 2AFC trials on which the item judged old also had the shorter identification RT. The error bars on the experimental data are 95% confidence intervals. The results from simulations with the single-system model (Figure 7-1, black circles) are within these intervals for old-new and new-new trials, but not old-old trials. More importantly, the qualitative pattern of experimental results is *not* predicted by the model: For all trials it predicts that the item judged old in a 2AFC task will also tend to have a shorter identification RT, that this effect will be greatest for old-new trials, and the effect for old-old and new-new trials will not differ. This is in contrast to the experimental data where an effect of equal magnitude was found for both new-new and old-new trials (though it is worth noting that the predicted difference between old-new and new-new trials was relatively small, 3%), but no effect was found for old-old trials.

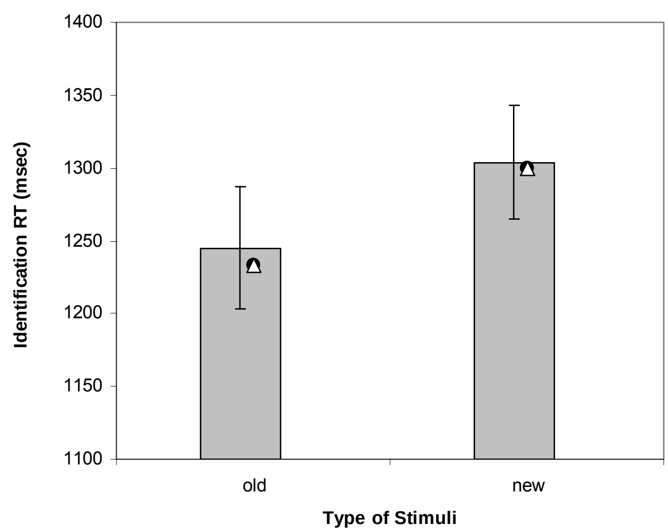

Figure 7-2. Model results for identification RTs to old and new stimuli in the CID-2AFC task. Closed black circles are the results of the single-system model. Open triangles are the results of the dual-system model.

The single-system model predicts the greatest effect for old-new trials because on these trials the value of f for each item is drawn from different distributions and the difference between f of the items on a 2AFC is therefore greatest (on average, when compared with old-old and new-new trials where the values of f for each item are drawn from the same distribution). When the difference is large, the addition of noise is less likely to result in the item with a lower f having the greater value of J_r or shorter RT. In other words, the addition of noise is less likely to distort the true relationship between the fs of the items because there is a greater difference between the fs initially: An item with the higher value of f is more likely to have the greater value of J_r and shorter RT.

The predicted effect is smaller for old-old and new-new pairs because the difference in f of the items is smaller (on average). The addition of noise is therefore more likely to distort the relative sizes of f across tasks, meaning that there will be fewer 2AFC trials on which the item judged old also has the shorter RT.

7.6.2 Dual-System Model

The results of the above simulation suggest that the single-system model cannot explain all of the findings from the CID-2AFC task. Of particular concern for this model is the lack of an effect for old-old trials. Perhaps a dual-system model, in which different sources of memory contribute to priming and recognition, can explain the pattern of performance? The simplest modification to the single-system model that would allow it to embody a dual-system model is to sample the values of f independently for the generation of the RT and J_r (rather than using the same value of f to generate the RT and J_r of a single item). All other parameters and their values can be kept identical to the single-system version of the model (Table 7-2). Thus, in this dual-system version of the model, a value of f is sampled for an item and combined with one source of noise to generate its identification RT (Equation (5-1)). To generate J_r for the same item, *another* value of *f* is sampled from the same distribution of *f* and combined with a different, independent source of noise (Equation (3-2)).

A simulation was carried out with the dual-system model using the same number of items and participants as the previous simulation. The priming effect fell within the 95% confidence interval of the data—*M* model = 67 msec; *M* Experiments 10–12 = 59 msec; 95% + = 68 msec, 95% - = 49 msec—as did the identification RTs for old and new stimuli (see Figure 7-2, open triangles). However, the result for recognition percent correct was just below the 95% confidence interval—*M* model: 78.8% correct, *M* Experiments 10–12: 80.9%; 95% + = 82.3%; 95% - = 79.5%. The mean absolute difference in the identification RTs to items in a pair was also analyzed. For all types of pair, the magnitude of this difference was slightly larger than was observed in the data and did not differ across pairs—old-old = 331 msec; new-new = 330 msec; old-new = 335 msec.

The results of the model for the prediction of 2AFC by RTs are presented in Figure 7-1 (open triangles) and it can be seen that two out of three of the model results lie within the confidence intervals of the data: Unlike the single-system model, the dual-system model correctly predicts that the outcome of old-old trials cannot be predicted by RTs, and the outcome of old-new trials can. However, it incorrectly predicts that there will be no effect for new-new trials.

The dual-system model predicts that the outcome of old-new trials can be predicted by RTs at levels greater than chance even though the sources of *f* are independent. How can this be explained? This arises because the mean *f* of old items is greater than that of new items and so the values of *f* that are sampled for the RT and J_r

are likely to be greater for the old item than the new item (even though the values of f will not necessarily be identical). There will therefore be old-new trials on which the item with the greater value of J_r also happens to have the shorter identification RT.

In contrast, for old-old and new-new pairs, one item is not prima facie more likely to have the greater f because the mean f of each pair member is the same. There is no guarantee that an item that has the greater value of f for the generation of the RT will also have the greater value of f when f is sampled again for the generation of J_r. Thus the outcome of old-old and new-new trials is not predicted by RTs in the dual-system model.

Note that the dual-system result for old-new trials is not as large as that predicted by the single-system model (see Figure 7-1): In the dual-system model, although the item with the greater value of f for the generation of J_r will tend to have the greater f for the generation of the RT, this will not always be the case because of random sampling. In the single-system model, the item with the larger value of f for generation of J_r will always have the greater value of f for generation of the RT (because the values of f are identical for J_r and the RT); the relationship between the RT and J_r will therefore be stronger in the single-system model.

7.7 Discussion of Experiments 10–12

Experiments 10–12 showed that the response on 2AFC recognition trials can be predicted by identification RTs, but only when both of the items in the trial are new, or one is old and the other is new. On these types of trial (new-new and old-new), there was a tendency for the item judged old to also have the shorter identification RT. However, responses on trials where both items were old could not be predicted by identification RTs. These findings have important implications for the single-system model which assumes that the same source of familiarity drives identification RTs and recognition judgments.

The single-system model predicted that responses on all types of 2AFC trial could be predicted by identification RTs, and that the effect would be greatest for old-new trials. The absence of an effect for old-old trials is inconsistent with the prediction of the model, as is the failure to detect a greater effect for old-new than new-new trials (but note that the difference predicted by the model for the latter effect was small). The findings could therefore be taken as evidence against the formalization of the single-system model presented here and against the notion that the source of memorial evidence driving recognition and priming is the same, at least on old-old 2AFC trials.

The findings also have implications for a dual-system version of this model in which the f used to generate an item's value of J_r and RT is assumed to be independent. The dual-system model correctly predicted that old-new 2AFC responses could be predicted by identification RTs and that old-old responses could not. However, it could not explain why the outcome of new-new trials can be predicted by identification RTs. Thus, neither the single- or dual-system versions of the model can fully account for the pattern of prediction of 2AFC by identification RT data.

Could the single-system model be modified to account for the old-old data? The absence of an effect on these trials seems inconsistent with findings from CID-R paradigms which have reported at least numerical trends for the identification RTs of old items judged old to be shorter than those of old items judged new (Johnston et al., 1985; Johnston et al., 1991; Stark & McClelland, 2000; but see Conroy et al., 2005).

Perhaps the absence of an effect is due to the difference in the type of judgment being made: Participants are being asked to make a comparative judgment of oldness in 2AFC rather than an absolute judgment, however, this does not prevent them from making absolute judgments of oldness for each item before giving their 2AFC response. For example, participants might have decided that both items on an old-old trial were old; indeed, after the experiment was over some participants reported that they thought that both items were old on some trials. If this occurred, then either of the alternatives might have been selected as the old item because the participant knew that in doing so they would complete the trial correctly (to indicate which item was old). This may have occurred even though the instructions of Experiments 11–12 were modified from Experiment 10 to require participants to select the item that they were more confident was old.

This type of decision process could be incorporated into the single-system model: on a 2AFC trial the value of J_r of an item is first assessed individually against a criterion (as was the case in the simulation of old/new recognition performance in Simulation Studies 1–4), and if J_r exceeds the criterion for both items then either word is judged old with an equal probability. If only one value of J_r exceeds the criterion, then that item is judged old. If neither of the items exceeds the criterion then the item with the greatest J_r is judged old. This would decrease the model estimate for the percentage of old-old trials that can be predicted by RTs (in line with the data) while still allowing effects for old-new and new-new trials.

One might similarly ask how the dual-system model could be modified to account for the new-new 2AFC data. It could be the case that the f used to generate an item's RT and J_r is independent on some occasions but on others is the same. This idea that a task thought to rely heavily on one type of memory can be "contaminated" by another form of memory fits in with proposals made by some dual-system accounts (e.g., Conroy et al., 2005) (e.g., that implicit memory can contaminate performance in explicit memory tasks and vice versa). For example, on new-new trials the same f might be used to generate an item's RT and J_r, whereas on old-new and old-old trials the values of f for an item's RT and J_r might be independent. A parameter could be introduced into the model representing the probability with which the value of f for an item's RT is the same as the f for J_r. The value of this parameter could vary according to the combined J_r of the items on a 2AFC trial. If this probability was lowest for items on new-new trials and highest for items on old-old trials, then the model would predict that, in line with the data, a higher number of new-new 2AFC trials could be predicted by RTs than old-old trials.

A remaining question is whether other models of recognition can account for these results (e.g., Rotello, Macmillan, & Reeder, 2004; Wixted, 2007; Yonelinas, 1994; Yonelinas, Kroll, Dobbins, Lazzara, & Knight, 1998). For example, consider a model which is similar to the single-system model presented here, but which also incorporates a recollection component (as in Yonelinas' dual-process model, Yonelinas, 1994; Yonelinas et al., 1998). In this model, old (but not new) items are assumed to be recollected with a probability p. If an item on a 2AFC trial is recollected, it is judged old. If both items are recollected, then either alternative is chosen with an equal probability. If recollection does not occur for any item on a 2AFC trial then the judgment is based on f. Recollection will therefore occur on old-old and old-new trials (but not new-new trials). Because judgments which are based on recollection are unrelated to f (and hence, the identification RT), estimates of the percentage of old-old and old-new trials predicted by RTs would not be as large as in the single-system model. The prediction for old-old trials may therefore be closer to chance, and that of old-new trials may be more like that of new-new trials. Thus, a model in which judgments are based upon recollection and familiarity may be able to correctly reproduce the pattern of prediction data.

Finally, the results from recognition latencies and the mean absolute differences between the RTs to items in the 2AFC also indicate that there may be something unique about new-new trials. Participants took longer to make their 2AFC response on new-new trials compared to old-old and old-new trials, suggesting that there is greater uncertainty on these trials, or perhaps the development of some strategy on these trials. This is consistent with the idea that the attribution of fluency to oldness is more likely in the absence of a clear memory signal (e.g., Verfaellie & Cermak, 1999). The magnitude of the mean absolute difference in RTs between the items of new-new trials was also greater than that of old-old and old-new trials. This latter result was not predicted by either version of the model, but, interestingly, is consistent with previous findings showing that the variability in RTs to new items is greater than that of old items (e.g., Logan, 1988).

In sum, Experiments 10–12 reveal the limitations of the single- and dual-system versions of the signal-detection model. Specifically, the single-system model does not explain why the outcome of old-old 2AFC trials cannot be predicted by identification RTs and the dual-system model does not explain why the outcome of new-new 2AFC trials can be predicted by identification RTs. The results suggest the following possibilities which could be investigated in future research: 1) recognition judgments are not based upon the same memory strength variable which drives priming (at least on old-old 2AFC trials); 2) the characterization of the 2AFC decision rule in the single-system model may not be correct. A decision rule in which each item is first assessed individually against a criterion before making the 2AFC judgment may be more accurate; 3) the results may be indicative of a "hybrid" model: there may be differential reliance on the f associated with the generation of RTs versus the f associated with generation of J_r, and this reliance may depend on the overall J_r of the items presented on a 2AFC trial; 4) other models of recognition memory such as Yonelinas' (1994) dual-process model may be able to account for the pattern of prediction by RT data.

Chapter 8: General Discussion

The aim of the present thesis was to present a single-system signal-detection model of priming, recognition and fluency and to use it to reexamine some behavioural evidence that has been interpreted in favour of the notion that priming and recognition are mediated by distinct implicit and explicit memory systems. The core assumptions of the model (presented in Chapter 3) are 1) that priming and recognition are driven by the same memory strength signal, and 2) that this signal is subjected to independent sources of random noise for each task. Crucially, the variance of the noise associated with priming tasks is typically assumed to be greater than that of recognition tasks. This assumption follows from suggestions that priming is influenced by more factors that are unrelated to memory compared to recognition (Ostergaard, 1992, 1998) and also from evidence demonstrating that priming tasks are typically not as reliable as recognition tasks (Experiments 5–8, Chapter 4; Buchner & Brandt, 2003; Buchner & Wippich, 2000; Meier & Perrig, 2000). In contrast to multiple-systems accounts (e.g., Gabrieli, 1999; Squire, 1994; Squire & Knowlton, 2000; Tulving & Schacter, 1990), the model makes no distinction between implicit and explicit memory. In Chapter 1, evidence that has been taken to support a multiple-systems view of priming and recognition was reviewed. To what extent can the model account for this evidence?

8.1 Unconscious Memory

The greater noise variance associated with priming tasks means that the model predicts that the sensitivity of the priming task will not exceed that of a comparable recognition task and, therefore, that priming will not occur when recognition is at chance. In other words, it predicts that unconscious memory cannot be demonstrated (at least by the methods proposed by Schacter et al., 1989; Reingold & Merikle, 1988). An attempt to replicate some key evidence for the existence of unconscious memory was presented in Experiments 1−4 of Chapter 2. Merikle and Reingold (1991) provided a compelling demonstration of unconscious memory by showing that the sensitivity of an indirect task was reliably greater than that of a direct task at certain points at test. What was so compelling about this demonstration was that their study implemented the logic of the relative sensitivity approach and, in doing so, overcame many theoretical and methodological issues which plague attempts to demonstrate unconscious influences. However, despite extensive efforts to replicate this result in Experiments 1−4, no evidence for unconscious memory was obtained: The sensitivity of the priming task never exceeded that of the recognition task, even when steps were taken to reduce performance on the latter to near chance.

Furthermore, similar results were found in Experiments 5–8 of Chapter 4 which used a more conventional priming task (perceptual identification). Like Experiments 1–4, Experiments 5–8 also used a manipulation of selective attention at encoding to determine whether priming for uncued stimuli would occur in the absence of recognition for these stimuli (e.g., as found by Eich, 1984; Johnston & Dark, 1985). There was no evidence of priming in the absence of recognition in Experiments 5–8 and priming never exceeded recognition, even when performance on the latter was at chance in Experiment 7. Thus, these results of Experiments 1–8 are in line with the prediction of the model—that the sensitivity of priming tasks will not exceed that of recognition.

What of other evidence for unconscious memory? As mentioned in Chapter 2, many other compelling demonstrations of unconscious memory have proven difficult to replicate (Kunst-Wilson & Zajonc, 1980, vs. Fox & Burns, 1993, Newell & Shanks, 2007; Mandler et al., 1987 vs. Seamon et al., 1998). This resembles research in other fields (e.g., implicit learning) where many studies claiming to demonstrate unconscious memory or unconscious knowledge have been shown to have methodological or theoretical flaws, or have not been successfully replicated (e.g., in human conditioning: Lovibond & Shanks, 2002; the sequential reaction time task: Wilkinson & Shanks, 2004; the contextual cuing task: Smyth & Shanks, 2007; the Iowa Gambling Task: Maia & McClelland, 2004; artificial grammar learning: Tunney & Shanks, 2003; the weather prediction task: Lagnado, Newell, Kahan, & Shanks, 2006).

Evidence supporting Jacoby and colleagues' process dissociation procedure (Jacoby, 1991) has been interpreted as evidence for memory without awareness (e.g., Jacoby et al., 1989). However, this procedure has been subjected to intense theoretical and methodological scrutiny and the status of this evidence is therefore unclear. For example, one criticism is that the process dissociation procedure does not allow for separate estimates of intention and awareness (see Richardson-Klavehn, Gardiner, & Java, 1996, for a discussion).

Another type of evidence for priming in the absence of recognition was addressed in Experiment 9 in Chapter 5. Stark and McClelland (2000) showed that priming could occur for items in a CID-R task, even though they were not recognized. Experiment 9 successfully replicated this pattern and Simulation Study 2 in Chapter 5 demonstrated that the model could account for this finding relatively straightforwardly because of the signal-detection process used to generate a recognition response: The memory strength of old items judged new falls below the threshold of evidence required for an old judgment to occur. Priming still occurs for these items because the average strength of these items is greater than that of new items (which also do not exceed the threshold). As discussed above, more challenging for the model would be evidence of priming when overall recognition performance is at chance, or evidence that the sensitivity of an indirect task is greater than that of a comparable direct task.

Clearly, the issue of whether unconscious representations or knowledge exist is controversial (Perruchet & Vinter, 2002; Shanks, 2005; Shanks & St. John, 1994). The position here is that if multiple-systems theories of memory are to incorporate a distinction between implicit and explicit forms of memory (e.g., Squire, 1994) then convincing, reliable evidence for an unconscious form of memory should be available. In the absence of such evidence, a more parsimonious notion is that the content of the memory which drives performance on priming and recognition tasks is accessible to awareness. This conclusion is consistent with other single-system views (Kinder & Shanks, 2001, 2003; Perruchet & Vinter, 2002; Shanks & St. John, 1994).

8.2 Functional Dissociations

The model predicts that variables will tend to have similar effects on priming and recognition. This is because the same memory source drives priming and recognition. However, the magnitude of the effect that is predicted for priming is smaller than for recognition, owing to the greater noise variance typically associated with priming measures. Experiments 5−8 in Chapter 4 investigated the effects of a manipulation of selective attention at encoding on priming and recognition and confirmed this prediction: priming and recognition were lower for uncued study items than cued items, but the difference was much greater for recognition than priming. The effects of the selective attention manipulation resembles that which are produced by other variables such as levels of processing (e.g., Brown & Mitchell, 1994) and the administration of benzodiazepines (e.g., Hirshman et al., 1999). The effect that these variables have on priming is often explained in terms of the contamination of priming with explicit memory (e.g., Hamann & Squire, 1996; but see Richardson-Klavehn & Gardiner, 1998, for other accounts of levels of processing effects). Rather than explaining these results in terms of the differential nature of implicit and explicit memory systems, or in terms of the contamination of priming with explicit memory, the results of Simulation Study 1 in Chapter 4 suggest that these results can be explained from a single-system perspective.

Single dissociations, in which a variable affects recognition but has no (true) effect on priming, are not predicted by the model. Reports of this type of dissociation are relatively common and are often considered to be evidence that an explicit form of memory can be selectively influenced. However, the model predicts that single dissociations can arise simply because of a failure to reject the null hypothesis (see also Dunn, 2003; but see Richardson-Klavehn et al., 1999, for a counterexample). For example, in Chapter 4, the model was applied to a single dissociation produced by a dual-task manipulation of attention at encoding (Mulligan, 2003, Experiment 1). Given close fits to the recognition data, the model predicted an effect on priming that was small enough in magnitude that a failure to detect it could have arisen because of low power (see Appendix I). Thus, when single dissociations are found, they are not necessarily incompatible with the single-system model.

A limitation of the model in its current state is that it does not explain why particular manipulations, such as changes in modality between study and test, should have larger (negative) effects on priming than recognition (Jacoby & Dallas, 1981); or why variables such as read-generate manipulations should have opposite effects on priming and recognition (Jacoby, 1983). These dissociations seem to imply that priming is functionally distinct from recognition, consistent with multiple-systems views (e.g., Wagner & Gabrieli, 1998). It is important to note though that these findings do not rule out a single-system view in general. TAP explains these dissociations in terms of the extent to which priming and recognition tasks depend upon perceptual versus conceptual processing without postulating distinct implicit and explicit memory systems (e.g., Roediger, 1990; Roediger et al., 1989). Furthermore, Kinder and Shanks (2003) suggest that changes in modality can be accounted for by the SRN by introducing modality-specific input layers into the single-system model (see discussion in Chapter 1).

One could speculate on how the single-system model might be able to account for the type of dissociation produced by changes in modality or read-generate manipulations. These types of dissociation clearly indicate that priming is dependent on identical physical forms of the item being presented at study and test (Tulving & Schacter, 1990). When the physical form is changed priming can be reduced and variables that might normally increase priming do not necessarily do so. Perhaps these results are indicative of a multi-dimensional signal-detection process in which there are different dimensions for modality-specific and modality-unspecific memorial evidence.

This type of extension to signal-detection models has been proposed to explain various recognition phenomena (see e.g., Rotello et al., 2004, whose model of recognition includes different axes for specific and global memory strength). This issue requires further investigation.

8.3 Stochastic Independence

If the true correlation between priming and recognition performance is zero, then this would falsify the model, which predicts a non-zero correlation. Some evidence for a correlation was found in Experiment 8 in Chapter 2, but in general, priming and recognition performance were not reliably correlated across experiments in this thesis (e.g., in Experiments 6–7, 9). However, in almost every case, the model was shown to provide a fit to the observed correlation. The model reproduces very low correlations because of the independent sources of noise associated with performance in each task. This suggests that the failure to observe a correlation between priming and recognition is not inconsistent with the single-system model. This explanation of low inter-task correlations is similar to that of other accounts which have emphasized the importance of considering the influence of non-memorial factors on priming performance (e.g., Kinder & Shanks, 2003; Ostergaard, 1992).

8.4 Population Dissociations

In Simulation Study 4 of Chapter 6, the model was applied to dissociation evidence from a recent study with amnesics (Conroy et al., 2005). The amnesics in Conroy et al.'s (2005) study showed relatively intact priming and fluency effects despite impaired recognition (relative to controls). This was interpreted as evidence that priming and recognition are mediated by independent memory sources, and also that fluency from priming does not contribute to recognition. These results were successfully simulated by the model by assuming that there is a greater degree of noise in the encoded memory signal and also in the assessment of that signal (or in the placement of the decision criterion from trial-to-trial) in amnesics, relative to controls. Thus, the dissociation between priming and recognition in amnesia, which has been shown many times (e.g., Cermak et al., 1985; Graf et al., 1984), and is often considered to be some of the strongest evidence for multiple-systems, can be explained by the single-system model.

Furthermore, for the severely amnesic MTL group in Conroy et al. (2005), the model simulated relatively unimpaired priming when recognition was very close to chance. This result is very similar to that of the individual E. P. (Hamann & Squire, 1997a), suggesting that even this striking dissociation may not be inconsistent with the single-system model (see Kinder & Shanks, 2001, for an even more compelling simulation of this result).

The model goes beyond previous single-system accounts (Kinder & Shanks, 2001, 2003) by also accounting for the relatively intact fluency effects in amnesics. The inclusion of the noise parameters allows the single-system model to reproduce low estimates of the contribution of fluency from priming to recognition (Conroy et al., 2005; see also Poldrack & Logan, 1997). Low estimates of the contribution of fluency and priming to recognition seem to weigh against the proposal made by dual-process theories of recognition that the fluency from priming is one basis for recognition (e.g., Jacoby & Dallas, 1981; Mandler, 1980). However, these low estimates need not be taken as evidence for independent memorial bases of priming and recognition, as has been suggested (Conroy et al., 2005). The single-system model provides an alternative perspective and is able to reproduce these low estimates of the contribution of priming to recognition despite the fact that priming, recognition, and fluency are all manifestations of a common memory source.

The dissociation shown by individuals with occipital lobe damage (Gabrieli et al., 1995) is challenging for the model to account for. As with modality effects (see above), it is unclear how the model in its current state could simulate relatively intact levels of recognition despite impaired levels of priming. Although this dissociation is consistent with a multiple-systems view, it is important to note that this dissociation is not incompatible with a single-system approach in general: The SRN reproduces this dissociation by assuming that individuals with right occipital lobe damage have a deficit in visual processing rather than a specific impairment in implicit memory (Kinder & Shanks, 2003).

8.5 Neuroimaging

As reviewed in Chapter 1, findings that priming and recognition are associated with distinct functional neuroanatomies and electrophysiological patterns at retrieval (e.g., Rugg et al., 1998; Schott et al., 2005), and also at encoding (e.g., Schott et al., 2002; Schott et al., 2006), are consistent with a multiple-systems view (e.g., Tulving & Schacter, 1990). Furthermore, consistent with the view that some variables selectively influence explicit memory and not priming, Schott et al. (2002) showed that levels of processing had no effect on the ERP correlates of later priming. Similarly, no effect of levels of processing has been found in the analysis of brain oscillations for later priming (Duzel et al., 2005).

It is important to note though that some commonalities have been reported, which could be construed as being consistent with a single-system view. Although Schott et al. (2005) found that priming and recognition were associated with different functional neuroanatomies—items which were primed (and not recognized) in a word-stem completion task were associated with a different pattern of activity to remembered items—they also found that the hippocampus/medial temporal lobe (MTL) showed decreased activity for primed items. This latter result runs contrary to the widely-held (multiple-systems) view that priming is not dependent on the hippocampus. Schott et al. (2005) suggested that the MTL activity may have been associated with novelty detection rather than priming. However, this finding is at least consistent with the single-system view that the MTL is involved in priming (Jernigan, Ostergaard, & Fennema-Notestine, 2001; Ostergaard & Jernigan, 1993; Ostergaard, 1999).

In another study, Turk-Browne, Yi, and Chun (2006) found commonalities in the neural correlates of the encoding processes leading to priming and recognition memory. Subsequent priming was correlated with haemodynamic response reductions in hippocampal regions during encoding. These correlations were only found for items which were subsequently remembered, and not for items subsequently forgotten, suggesting an association between priming and recognition. Turk-Browne et al. (2006) interpreted these findings as evidence that implicit and explicit memory can rely on similar encoding processes and representations.

Despite these commonalities, these results could still be considered to be more in line with a memory-systems view than a single-system one. For example, the MTL deactivations observed by Schott et al. (2005) were consistent with other priming-related deactivations observed in the ventral visual stream (areas typically associated with stimulus identification). Such ventral visual stream deactivations are usually thought to indicate the neural correlate of the increased cognitive processing efficiency indexed by behavioural priming measures, or reduced numbers of neurons responding (see Schott el al., 2006, for a discussion). By contrast, remembering was associated with *activations* in areas linked to the limbic system (i.e., regions typically associated with explicit memory formation). The single-system model as it currently stands does not account for the difference in neuroanatomies associated with priming and recognition observed in this study, and it does not explain why priming was associated with haemodynamic deactivations whereas successful remembering was associated with activations. Similarly, in an MEG study, Duzel et al. (2005) found that priming-related oscillatory changes at encoding occurred in ventral visual stream areas during an early time window in which words themselves were identified. In addition, Schott et al. (2002) have shown that the timing of the neural events related to priming at encoding precedes that of the neural events related to encoding for later explicit memory. Again, the single-system model does not provide an account of such timing differences, and does not account for the differences in localization of the electrophysiological activity (Duzel et al., 2005). In sum, the majority of the neuroimaging evidence remains a challenge for the single-system model.

8.6 Item-Level Dissociations

In Simulation Study 3 of Chapter 5, the model was applied to findings from studies that have classified identification RTs according to the recognition outcome (i.e., hit, miss, false alarm, or correct rejection). Two item-level dissociations were evaluated to determine whether they provide evidence for distinct memorial bases of priming and recognition. Stark and McClelland's (2000) observation of priming for items judged new (Experiment 9, Simulation Study 2) was shown to be consistent with the model (see above). The model can also explain the results of Johnston et al. (1985) (Simulation Study 3). Johnston et al. (1985) found that the relationship between identification RTs to misses and false alarms was not the same at low and high levels of recognition performance. The RT (miss) < RT (false alarm) result which was evident when recognition was high was thought to be indicative of an additional memory search process (see e.g., Mandler, 1980) that was not present when recognition was lower (as indicated by RT (false alarm) < RT (miss)). Highly counterintuitively, these results were successfully simulated by the single-system model in Simulation Study 3. Again, the inclusion of noise in the model (specifically the decision noise associated with recognition) was crucial in allowing the model to reproduce these results (cf. Shanks & Perruchet, 2002; Shanks et al., 2003). Thus, contrary to previous interpretations, the item-level dissociations reported by Stark and McClelland (2000) and Johnston et al. (1985) are not inconsistent with a single-system account.

8.7 CID-2AFC

Experiments 10–12 in Chapter 7 tested the single-system model in a novel task, the CID-2AFC task. The fact that the same source of memory drives recognition judgments and identification RTs in the single-system model leads it to make an interesting prediction: an item judged old on a 2AFC trial will also tend to have the shorter identification RT, regardless of the combination of old/new items presented as alternatives on the 2AFC trial. This prediction was confirmed in Experiments 10–12 for old-new 2AFC trials. However, a dual-system version of the model, in which the sources of *f* were independent for priming and recognition, also predicted this outcome. What distinguished the models was their estimate of the percentage of old-old and new-new 2AFC trials predicted by identification RTs. The single-system model predicted that the outcomes of both types of trials could be predicted by RTs, but the dual-system model predicted that the outcomes of both types of trial could not. The results showed that responses on new-new trials could be predicted (to a similar extent to old-new trials), but the outcome of old-old trials could not be predicted by RTs. Thus, neither version of the model could fully account for the pattern of results. This highlights a limitation of the single-system model (and dual-system version of the model): The absence of an effect for old-old trials suggests that the same memory signal does not drive the identification RT and the recognition judgment (at least on these trials). However, at the end of Chapter 7, several promising avenues for future investigation were suggested which included modifying the 2AFC decision rule in the single-system model and exploring the parameter space of other models of recognition memory.

8.8 Other Future Directions

Signal-detection models have been used extensively by researchers attempting to understand recognition (see e.g., Wixted, 2007). This framework is widely accepted as a plausible one in which to develop theories of recognition (but see Gardiner, Richardson-

Klavehn, Ramponi, 1998), and many models of recognition include some role for signal-detection theory (e.g., Gillund & Shiffrin, 1984; Murdock, 1982, 1983; Hintzman, 1984, 1988). A debate which has persisted in the recognition memory literature is whether recognition should be described with a "single-process" signal-detection model, containing only a single strength-of-evidence axis, or whether a dual-process model, in which recognition also depends upon a recollection component, provides a better account (for reviews see e.g., Dunn, 2004; Mandler, 1980; Wixted, 2007; Yonelinas, 2002). It remains to be seen how priming can be accommodated into these types of models. It may be informative to do so, particularly given that some dual-process theories include some role for priming (Jacoby & Dallas, 1981; Mandler, 1980; Yonelinas et al., 1995). Understanding how priming can be incorporated into models of recognition could therefore help to inform this debate, and may contribute to our general understanding of memory phenomena in the context of formal models.

Finally, the present thesis has simply sought to show that certain data patterns are not inconsistent with a single-system perspective. In doing so it adds to other single-system computational accounts of implicit and explicit memory phenomena (Kinder & Shanks, 2001, 2003; Nosofsky & Zaki, 1998; Zaki et al., 2003). However, an important question for future research is whether the single-system model should be preferred over a multiple-systems version of the model. This question could be answered by directly pitting distinct quantitative predictions of the single- and dual-system versions of the model against each other (as was attempted in Chapter 7). Furthermore, a more formal process of model selection could be conducted by using fitting procedures, and by comparing the flexibility of the models (e.g., Pitt, Kim, & Myung, 2003). Whether or not the model stands up to future tests, we believe that it is an important step forward to have a formal, quantitative model of priming, recognition and fluency to use as a benchmark.

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Appendix 1: Simulation of Mulligan (2003), Experiment 1

Mullgan (2003, Experiment 1) presented a challenging pattern of data for the model: He showed that a dual-task manipulation of attention at encoding affected recognition, but had no effect on priming (in a perceptual identification task). This is challenging for the model because the model predicts that manipulations of attention will affect *both* priming and recognition, even though the effect on priming might be quite small. This data set was therefore simulated to determine the magnitude of the effect that is predicted for priming.

Mulligan's experiment used a 2 (old, new) x 2 (priming, recognition) x 2 (full-attention, divided-attention) design, in which old-new status was a within-subjects factor and the other two factors were between subjects factors. The following simulation also used this design. There were therefore 8 data points to fit (hit and false alarm rates for each attention condition for both tasks). The same number of items were used as in Mulligan's Experiment 1 (45 old and 45 new items for each task). The parameter values for the simulation are given in Table A-1. The σ_μ parameter was not necessary (because there were no correlations to simulate) and was therefore set to zero. The value of *C* was set as the midpoint between the mean of the old and new *f* distributions in each condition. The simulation results for the hit and false alarm rates are presented in Figure A-1, and the sensitivity (Hits - FAs) results are presented in Figure A-2.

The main objective in this simulation was to determine what the predicted effect of attention on priming would be after μ and β_c were varied by hand to give close fits to the recognition data. The values of μ and β_c used (Table A-1) gave close fits to the recognition hit and false alarm rates (see Figure A-1), and also recognition sensitivity (Figure A-2). The crucial result can be seen in Figure A-2: Given this fit to the recognition data, the model predicted that the difference in Hits - FAs between the full and divided attention conditions in the priming task would be .06 (the difference in terms of d' = 0.19).

It was necessary to lower the value of T from its value in Simulation Study 1 (from 2.8 to 2.1) in order to fit the priming hit and false alarm rates. (However, if the value of T was kept at 2.8 then the predicted effect of attention was very similar: Hits - FAs = .07, in terms of d' = 0.20.)

Table A-1 *Parameter values for simulation of Mulligan (2003) Experiment 1*

Symbol	Meaning	Value
σ_f	Standard deviation of familiarity distributions (new/full attention/ divided attention)	0.2
σ_r	Standard deviation of noise associated with recognition (constrained to equal σ_f)	0.2
σ_p	Standard deviation of noise associated with priming	1.0
μ	Mean familiarity of divided-attention items	**0.27**
β_c	Proportional increase in mean of full- relative to divided-attention items	**1.75**
T	Increase to target item strength within an identification trial	**2.1**

Note: **BOLD** indicates the parameter was varied (from its value in Simulation Study 1) to fit the data.

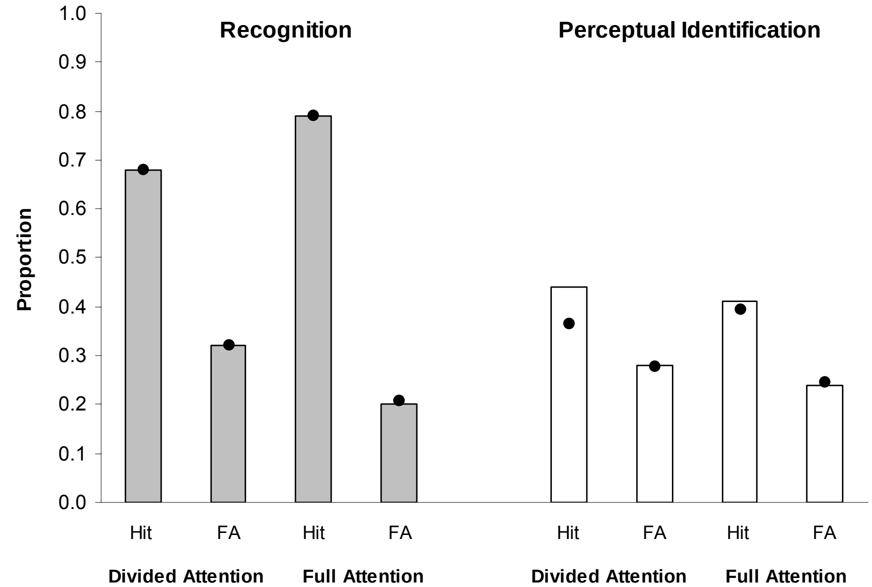

Figure A-1 Proportion of hits and false alarms as a function of attention at encoding and task. Bars indicate Mulligan's (2003), Experiment 1 data; black circles indicate model results from 10,000 simulated participants.

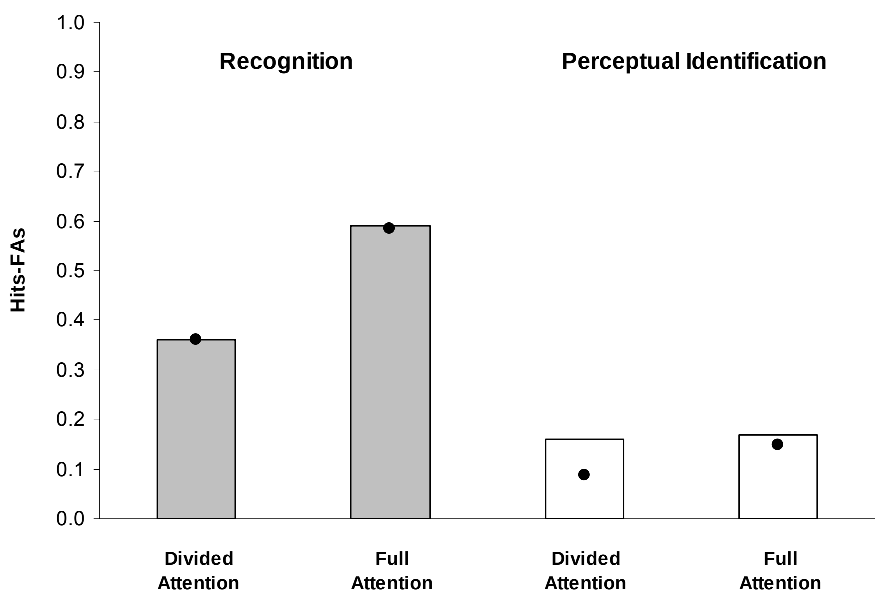

Figure A-2 Mean sensitivity (Hits - FAs) as a function of study status and attention at encoding. Bars indicate Mulligan's (2003), Experiment 1 data; black circles indicate model results from 10,000 simulated participants.

Mulligan reported that the size of the effect of attention for recognition was d = 1.73 (Cohen, 1988) (calculated from values of d' for each condition), and that the power to detect an effect half this size in the priming task was .95 (N = 32, α = .05, directional test). However, what is the power of Mulligan's experiment to detect an effect of d' = 0.19 predicted by the model? Using the effect size for recognition reported by Mulligan, the pooled variance Mulligan used to calculate Cohen's d can be estimated as:

$$d = (M1 - M2)/\sigma_{\text{pooled}}$$

$$1.73 = (1.88 - 1.01)/\sigma_{\text{pooled}}$$

$$\sigma_{\text{pooled}} = 0.50$$

This value of σ_{pooled} can then be used to calculate the size of the effect predicted by the model in Mulligan's priming condition: d = 0.19/0.50 = 0.38. The power of Mulligan's Experiment 1 to detect an effect of this size on priming was .44 (N = 32, α = .05, directional test), which is substantially lower than the power value he reported. This suggests that the failure to find an effect of attention on priming may have been due to insufficient power. Thus, the single dissociation reported by Mulligan (2003, Experiment 1) is not necessarily inconsistent with the single-system model.