Inter-Item Associations and Memory for Order

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Author's Declaration

I hereby declare that I am the sole author of this thesis. This is a true copy of the thesis, including any required final revisions, as accepted by my examiners.

I understand that my thesis may be made electronically available to the public.

Abstract

Remembering the order in which a sequence of events occurred can be an invaluable function of memory. Although important, the quality of memory for order information has been shown to be significantly affected by the way in which the information was encoded. That is, some encoding conditions lead to better memory for order than others. This dissertation presents a systematic examination of the features of encoding tasks that disrupt memory for order. Chapter I examines the effects of *semantic* versus *non-semantic* processing, *item-specific* versus *item-generic* processing, and *item-specific* versus *relational* processing on memory for order, revealing that any type of response-required task disrupts memory for order, unless that task is relational in nature. Chapter II examines the role of processing time in the disruption of memory for order and demonstrates that preserving response-free study time benefits memory for order, suggesting that response tasks disrupt memory for order because they take time away from the encoding of relational information. Chapter III introduces a novel procedure for examining memory for order—an order recognition task—which provides a new method for testing relational memory, allowing for conceptual replications and enhancing the generalizability of findings surrounding encoding tasks and memory for order. Taken together, the experiments in this dissertation demonstrate that poor memory for order is the direct result of reduced time for processing inter-item relations.

Acknowledgements

Portions of this dissertation (Experiments 1 to 4) were published by Tanya R. Jonker and Colin M. MacLeod in the *Journal of Experiment Psychology: Learning, Memory, & Cognition* (Jonker & MacLeod, 2014; for the full citation, turn to the Reference section). Copyright © 2015 by the American Psychological Association. Adapted with permission. Some of the statistical test values differ slightly between this dissertation and the published values because follow up analyses resulted in the correction of some minor errors in coding the raw data. The updated tests and means are reported here. The correction of these errors affected the outcome of three tests reported as marginal in the published version: In Experiment 1, the interaction between list type and encoding condition was reported as marginally significant in Jonker & MacLeod but was no longer found to be significant; in Experiment 1, the *t*-test comparing orthographic encoding to silent reading in mixed lists was reported as marginally significant in Jonker and MacLeod but is reported here as significant; in Experiment 3, the interaction between

list type and encoding condition was reported as marginally significant in Jonker and MacLeod but was found to be non-significant after error correction. None of these changes alter the claims made in the original article.

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Introduction

Remembering when events occurred is in many cases essential for successful performance at current tasks. For example, if you are thirsty while hiking in backcountry, and you remember collecting water from a stream as well as inserting purification tablets into water, it would be very important to remember which of those two events had occurred more recently: If you had more recently put in purification tablets, then the water in your canteen is safe to drink; but if you had more recently collected the water—your memory for putting in purification tablets was for the water that you had already consumed—then the water that you have in your canteen right now is not safe to drink. This example demonstrates that it is not only important to remember *what* events have occurred in your personal past, but also *when* those events occurred.

Remembering when an event occurred can be accomplished in a number of different ways. To provide an example, imagine that while running errands you first visited the gym, then the pet store, and then the grocery store. When trying to remember the order in which you visited these places, you could rely on several different approaches. For example, you could perhaps think about the gym and determine that the memory of having been there is faint and weak, at least when compared to your memory of having been at the grocery store, and therefore you could conclude that you visited the gym before the grocery store. This type of assessment would involve you relying on the strength of the familiarity of the memories, with the implicit assumption that the familiarity of a memory decays over time and therefore that less familiar memories are older memories (e.g., Brozinsky, Yonelinas, Kroll, & Ranganath, 2005; Yonelinas & Levy, 2002; Hintzman, 2004, 2005). A second method could involve binding the memory to some type of temporal marker, such as the hour and minute at which something occurred, and using memory for the temporal markers to reconstruct the sequence of events (e.g., Brown,

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Preece, & Hulme, 2000). For example, you could remember that you went to the gym at 2:30 pm and to the pet store at 3:45 pm; you could then use this temporal information to conclude that you went to the gym before the pet store. A third method could involve remembering the associations between the events. For example, you could remember that while at the gym you planned your route to the pet store, and that at the pet store you decided that the canned cat food was too expensive and that you would instead buy it at the grocery store. These types of cognitions create *inter-item associations*, which here are defined as the binding of two otherwise discrete items. They can be used to recall which pairs of events occurred together in time.

It is this last type of memory that is the focus on this dissertation.

Relational Information vs. Item-Specific Information

Much research has focused on the role that inter-item associations—or relational information—play in memory. Relational information is of particular interest to memory researchers because it can be contrasted with item-specific information. Item-specific information is information about the unique features of the individual item, with some examples being semantic features (e.g., the item is typically found in the ocean, is smaller than the size of an apple, and can be eaten raw) and articulatory features (e.g., the word is pronounced "oi-ster"). Relational information, on the other hand, emphasizes the object in comparison to others, with some examples being relative size (e.g., the item is smaller than the previously presented item) or order (e.g., in the list of words, this word was presented just prior to the word "helmet").

For over thirty years, the distinction between item-specific and relational information has been influential with respect to the goal of understanding what is being encoded during various types of encoding (Einstein & Hunt, 1980; Guynn et al., 2014; Hunt & Einstein, 1981). Underlying this distinction is the fundamental tenet that some encoding tasks enhance

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information about individual items in memory whereas others enhance information about the connections between items in memory. For example, Nairne, Riegler, and Serra (1991) examined the effect of generation (e.g., having the participant generate the study word "oyster" from a nearly complete word fragment like "oys_er") versus silent reading on recognition memory for the item and reconstruction of the serial order of items from the study list, under the assumption that the former test measures item-specific information whereas the latter test measures relational information.

During the recognition test, participants were presented with individual words and were to indicate whether they had seen each word during the study phase (each word could either have been silently read or generated from a cue); this test is thought to measure item-specific information because each test word is presented alone without any associative information and so the participant must rely on the strength of the memory for the individual item to make a recognition judgment. During the test of memory for order, in contrast, participants were presented with all of the studied words in a scrambled order and were to reconstruct the studied order. This order reconstruction test is thought to be a measure of relational information because it requires participants to reconstruct the sequence of all of the studied words in relation to one another. It should, however, not be dependent on item-specific information because the items are provided to the participant obviating any need to rely on item-specific information to activate and recall the items from memory.

In their study, Nairne and colleagues (1991) found that participants were more likely to recognize words that had been generated relative to words that had been silently read—suggesting that more item-specific information is encoded during generation—but that they were more likely to correctly reconstruct the order of silent read words relative to generated words—

suggesting that more relational information is encoded during silent reading. It is intriguing that silent reading, the "weaker" form of encoding in the sense of depth of processing (Craik & Lockhart, 1972), led to better memory for order than did generation, despite silent reading leading to poorer memory for the items themselves.

The Item-Order Account

In a recent review and meta-analysis, McDaniel and Bugg (2008) provided Nairne and colleagues' (1991) findings, as well as those of other investigators, as support for the hypothesis that participants differentially encode relational and item-specific information depending on the encoding task. Specifically, McDaniel and Bugg argued that uncommon stimuli or encoding tasks, such as generation, require some interpretation, which results in rich encoding of item-specific features. This rich encoding results in better recognition of generated items than of silently read items because an item recognition test relies primarily on item-specific information. It comes at a cost, however, to tests of memory for the order in which the items were studied: Generation disrupts the normal encoding of order information that occurs in the more common silent reading task, resulting in poorer memory for order.

Critical to the item-order account is the observation that these effects depend on the composition of the list: When various encoding tasks are the subject of interest, they can be examined either in *mixed lists*, in which case some items in the list are encoded under one task and others under the other task, or in *pure lists*, in which case all items in a given list are encoded under the same task. When it comes to these two types of lists, McDaniel and Bugg (2008) proposed that item-specific information for uncommon stimuli or encoding tasks is encoded irrespective of the makeup of the list. That is, in a mixed list or a pure list, item-specific information will be well-encoded for uncommon stimuli and encoding tasks because this type of

information is unique to the item itself and does not depend on surrounding items. Relational information, on the other hand, does depend on surrounding items. Therefore, relational information will be well-encoded only when two conditions are met: 1) the encoding task or stimuli are relatively common and therefore do not impede relational encoding, and 2) the encoding task for nearby items is also relatively common, allowing for inter-item associations to be encoded. Thus, relational information—order information—will not be well-encoded in a mixed list because the presence of the uncommon task will disrupt the encoding of inter-item associations. In summary, then, item-specific information will be well-encoded for unusual stimuli or encoding tasks irrespective of the list type, whereas relational information will only be well-encoded for common processing tasks in a pure list.

In recent work, my colleagues and I examined item-specific and relational information in the production effect (Jonker, Levene, & MacLeod, 2014). The production effect contrasts reading aloud and reading silently with the robust finding that recognition of the items read aloud is more accurate than that of the items read silently, particularly in mixed lists (e.g., MacLeod, Gopie, Hourihan, Neary, & Ozubko, 2010) and sometimes in pure lists (Bodner, Taikh, & Fawcett, 2014; Fawcett, 2013). These findings on recognition tests suggest that item-specific information is stronger following reading aloud compared to reading silently, and the superiority of reading aloud occurs regardless of the list design (mixed vs. pure; although list design seems to influence the size of the effect, with larger effects for mixed lists). Thus, if item-specific information is better encoded during production, I reasoned that perhaps relational information would be superior following silent reading compared to production. This would suggest that the encoding of item-specific information comes at the cost of encoding relational information in the production effect.

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To understand how these two encoding tasks influence memory for relational information, my colleagues and I examined the effect of production on memory for order (Jonker et al., 2014). We found that in pure lists, participants better reconstructed the order of words read silently relative to words read aloud, whereas in mixed lists, order reconstruction of silent and aloud words did not differ and was poor relative to memory for order for pure lists of silently read words. This suggests that in pure lists participants encode relatively more relational information for silent items compared to pure lists of aloud items and mixed lists.

This pattern in memory for order has been observed across a range of encoding tasks, such as the enactment effect (acting out vs. passively reading sentences or watching others act; e.g., Engelkamp & Dehn, 2000), the bizarreness effect (bizarre images or sentences vs. common ones; e.g., McDaniel, DeLosh, & Merritt, 2000; McDaniel, Einstein, DeLosh, May, & Brady, 1995), the perceptual interference task (partially masked vs. unmasked items; e.g., Mulligan, 1999), and the word frequency effect (infrequent vs. common words; e.g., DeLosh & McDaniel, 1996). Thus, this is a robust pattern extending across a wide range of encoding tasks and paradigms.

Rationale for the Present Work

Although the pattern of memory for order being superior following common encoding in pure lists compared to either mixed lists or pure lists of unusual coding is widespread across the memory literature, it is not clear which features of these various encoding tasks and stimuli disrupt memory for order. In their article, McDaniel and Bugg (2008) speculated on what disrupts the encoding of relational information: "[L]ess typical presentation formats or stimuli [e.g., generation, reading aloud, bizarre items] attract or require attention for individual item processing and reduce encoding of order information" (p. 240). This statement has two

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implications: (1) the typicality of the task is important, such that uncommon tasks will disrupt the encoding of memory for order; and (2) decreased encoding of order information is a by-product of attention to item-specific processing.

Although McDaniel and Bugg (2008) make clear statements about the conditions that should result in poor memory for order (unusual stimuli or presentation formats, which direct attention to item-specific processing), the roles of attention and task commonality have not been systematically examined. Therefore, the purpose of this dissertation is to examine the encoding conditions that lead to poor memory for order.

The first chapter of this dissertation presents a systematic examination of the features of various encoding tasks that produce poor memory for order. Specifically, I explore the possible roles of *semantic versus non-semantic* processing, *item-specific versus item-generic* processing, and *item-specific versus relational* processing during encoding to determine how these types of processing influence memory for order. In this chapter, I attempt to determine whether the typicality of the encoding task is important, as has been claimed by McDaniel and Bugg (2008).

In the second chapter, I examine the role of time in the disruption of memory for order. Specifically, I examine whether a disruptive encoding task must occur *during* item presentation to disrupt memory for order, and whether extending item presentation time can ameliorate the cost due to an atypical encoding task on memory for order. Collectively, these two experiments address the question: Does the presence of a disruptive encoding task within a list have a negative effect on memory for order irrespective of item-processing time, or is memory for order closely related to the amount of task-free processing time that one has?

In the third chapter, a final experiment addresses whether the order memory effect that has been observed—silent study resulting in better memory for order than an encoding task

requiring an overt response—extends beyond tests of order reconstruction to a novel order

recognition test. This work introduces a new method for testing memory for order to a domain

that has relied almost exclusively on one test type: the order reconstruction test. Importantly, the

introduction of this novel test provides a new method for testing memory for inter-item

associations that can be used for conceptual replications and that extends the generalizability of

this domain of research.

CHAPTER I

Chapter I presents a systematic examination of the features of encoding tasks that disrupt memory for order. In three experiments, I examine each of *deep versus shallow* processing, *item-specific versus item-generic* processing, and *item-specific versus relational* processing in terms of their effect on memory for order. In doing so, these experiments speak to the features of various tasks that disrupt memory for order. McDaniel and Bugg (2008) hypothesize that uncommon encoding tasks or stimulus features result in relatively poor memory for order because the tasks or features attract attention to item-specific processing at the cost of the encoding of relational information. Thus, according to the item-order account, item-specific processing and redirection of attention are necessary for the disruption of memory for order. In this chapter, the experiments address the importance of the type of processing inherent in the encoding task (Experiments 1 and 3), of attention to interpreting the item (Experiment 2), and of the typicality of the encoding task (Experiment 3).

Experiment 1

McDaniel and Bugg (2008) have argued that elaborative processing enhances item-specific processing at the cost of the encoding of relational information; however, it is unclear which types of elaboration disrupt memory for order. The purpose of Experiment 1 was to determine whether item-specific elaboration must be semantic in nature to disrupt memory for order, given that elaborative tasks typically call for greater semantic analysis. For some of the known design effects, the uncommon encoding process is indeed plausibly semantic. For example, the generation effect typically involves word-stem completion from some sort of cue. This cue can be semantic in nature, as is the case for antonym generation (e.g., *good – b_*). The enactment effect might also involve deeper semantic processing for the enacted items because

participants might activate more of the semantic features of the sentences when they have to act them out themselves. Furthermore, in the case of the bizarreness effect, bizarre stimuli might result in more semantic processing due to the unusual relations among the features of the stimuli.

In Experiment 1, therefore, I examined whether the common thread underlying disrupted memory for order is semantic elaboration. To accomplish this, participants were to make either a semantic judgment ("Is this a living thing?") or an orthographic judgment ("Does this word have an 'o' in it?"). Memory for order for pure lists (involving exclusively one encoding task) and mixed lists (involving both encoding tasks) was assessed using an order reconstruction task, in which all list items were presented to participants at test in a scrambled order and participants were to correctly reorder the items in their studied sequence. If semantic elaboration uniquely disrupts the encoding of order information, then a manipulation encouraging semantic elaboration should result in poorer memory for order than a manipulation encouraging orthographic elaboration. Alternatively, it is possible that any sort of item-specific processing impairs the encoding of order information. If this is the case, then any manipulation involving an item-specific judgment—whether semantic or orthographic—should result in poor memory for order when compared to silent reading.

Method

Participants. Twenty-six students from the University of Waterloo (5 male, 21 female) with an average age of 19.8 participated in exchange for partial course credit. Participants were eligible for the study only if they reported fluency in written and spoken English, normal or corrected-to-normal vision, and normal color vision.

Materials and Procedure. Two hundred seventy-six common nouns with word frequency scores lower than 500 were selected from the MRC psycholinguistic database

(Coltheart, 1981). From this set of words, 24 lists of eight words each were constructed. Words were randomly selected for each participant and were not repeated across lists. Twelve lists were assigned to be pure lists, with four lists in each of the three encoding conditions: semantic judgment, orthographic judgment, and silent reading. The remaining 12 lists were mixed lists. Each mixed list involved two of the three processing types (semantic, orthographic, silent reading) and four of the eight words were randomly assigned to each of the two processing types; there were four of each type of mixed list (semantic-orthographic, semantic-read, orthographic-read). In the experiments reported in this dissertation, I was primarily interested in the results from the pure lists; mixed lists were included to ensure that new encoding conditions were not resulting in relatively poor relational memory irrespective of list type (i.e., mixed or pure). If there were significant differences in memory for order in the mixed lists, this would suggest that the new encoding manipulations were not functioning in the same way as other design effects because the literature tells us that, in mixed lists, memory for order for elaborated items should be equivalent to that for silent reading.

Participants completed 24 blocks of study and test. Each block began with a study list in which eight words were presented individually, each for 2 s at the center of a computer monitor and with a 500-ms inter-stimulus interval. Participants were instructed to silently read blue words, to say "yes" or "no" to the question "Does this word have an 'o' in it?" for white words, and to say "yes" or "no" to the question "Is this a living thing?" for red words. They were not made aware ahead of time whether a list would be pure or mixed or which the type(s) of encoding would occur. After studying the eight items, participants completed a 30-s distractor task, during which they saw a random series of single digits (1 to 9). They were to indicate with a key press whether each digit was odd or even. The distractor trials, included to minimize the

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influence of the potentially powerful recency effect that would otherwise result for such short lists, were self-paced and continued until 30 s had passed.

During the test phase, the eight study items were presented on the screen in a scrambled order in a vertical list; all words were presented in black font on a white background. Participants were to write the words on a sheet of paper in the order that they remembered them having appeared during study. Participants had as much time as they needed to reconstruct the studied order of the words. Importantly, this was not a recognition or item-recall task because the participants were provided with all of the study words. Instead, this test measures participants' ability to remember the relative order of studied items only. For a summary of the procedure of this and all subsequent experiments, see Table 1.

Prior to the 24 experimental blocks, participants familiarized themselves with the tasks in a single practice block. All three encoding conditions (semantic, orthographic, read silently) were presented during this practice block. The data from this block were not analyzed.

Results and Discussion

As in previous work (e.g., Jonker et al., 2014; Nairne et al., 1991), a strict scoring criterion was used: Items were scored as correctly ordered only if they were written in their exact serial position. For example, if "muffin" was studied in serial position 3, it was scored as correct only when placed on the third line of the test sheet. This is a very conservative method for scoring because it ignores the relative accuracy of placing "muffin" in position 4 as opposed to position 8. However, I opted to use this method because in previous work (Jonker et al., 2014), I have employed different methods for scoring data and found no differences in the patterns across list type and encoding conditions. Therefore, as I was only interested in the

Experiment	Encoding		Test
	Pure	**Mixed**	
1	silent semantic orthographic	all combinations	order reconstruction
2	silent key press aloud	all combinations	order reconstruction
3	silent semantic independent semantic relational	all combinations	order reconstruction
4	silent key press aloud silent with key press between key press with key press between	combinations of encoding conditions 1, 2, and 3	order reconstruction
5	silent with short encoding silent with long encoding semantic with short encoding semantic with long encoding	none	order reconstruction
6	silent semantic	none	size test order recognition

Table 1. A breakdown of the encoding conditions, list types, and test types for all experiments.

differences across lists—and all lists were scored in the same strict way—the strict method was preferred.

Accuracy was scored as the proportion of items correctly ordered. For mixed lists, the proportions were computed separately for each encoding condition. Thus, for a mixed list containing semantic and orthographic encoding conditions, semantic and orthographic items would each be scored out of a total of 4, not 8.

A 2 x 3 repeated-measures analysis of variance (ANOVA) assessed the effects of list type (mixed, pure) and encoding condition (silent, semantic, orthographic) on the proportion of items correctly ordered. There were significant main effects of both list type and encoding condition, $F(1,25) = 27.52$, $MSE = .01$, $p < .001$, $\eta_p^2 = .52$, and $F(2,50) = 17.70$, $MSE = .01$, $p < .001$, $\eta_p^2 = .42$, respectively, but the interaction was not significant, $F(2,50) = 2.18$, $MSE = .02$, $p = .12$, $\eta_p^2 = .08$. A one-way repeated-measures ANOVA assessed the effect of encoding condition on order-reconstruction accuracy for mixed lists only. The effect of encoding condition in mixed lists was marginally significant, $F(2,50) = 2.90$, $MSE = .01$, $p = .07$, $\eta_p^2 = .10$. Although in mixed lists silent reading led to slightly better memory for order relative to semantic encoding, the effect was only marginally significant, $t(25) = 1.95$, $SE = .03$, $p = .06$, $d = 0.38$; the effect of silent reading compared to orthographic encoding was significant, $t(25) = 2.13$, $SE = .03$, $p = .04$, $d = 0.42$. As mentioned earlier, because I am interested primarily in the pure lists, I report these mixed list analyses just for completeness. These findings were not replicated in any of the following experiments; particularly, it was not replicated in Experiment 3, which employed similar encoding conditions.

Of main interest were the differences in memory for order of the pure lists. A one-way repeated-measures ANOVA assessed the effect of encoding condition on order reconstruction

accuracy for the pure lists. This effect was significant, $F(2,50) = 11.11$, $MSE = .02$, $p < .001$, $\eta_p^{\ 2}$ = .31, and follow-up analyses revealed that memory for order was better for silently read items than for orthographic items, $t(25) = 5.33$, $SE = .03$, $p < .001$, $d = 1.04$, or for semantic items, $t(25) = 3.53$, $SE = .04$, $p = .002$, $d = 0.69$. However, memory for order for orthographic and semantic items did not differ, $t(25) = 0.73$, $SE = .04$, $p = .47$, $d = 0.14$. Thus, as can be seen in Figure 1, memory for order was poorer following both semantic and orthographic elaboration than following silent reading. Therefore, clearly the encoding task does not have to involve semantic elaboration to disrupt memory for order. Instead, relative to silently reading items, both types of item-specific processing disrupted memory for order equivalently.

Although these two encoding tasks have not previously been compared to memory for order following silent reading, they have previously been compared to each other at least twice (Naveh-Benjamin, 1990; Tehan, Fallon, & Randall, 1997). In both cases, memory for order was found to be superior following semantic encoding compared to non-semantic encoding. In the present experiment, this same trend was observed (see Figure 1), but the effect was not found to be significant. However, there are important differences between the present experiment and the previous work that could underlie the differing outcomes. Tehan et al. (1997) found that participants were better at reordering lists following semantic encoding when compared to orthographic encoding, but this effect seemed to be restricted to cases where the items of the list were all categorically similar to one another (but see Experiment 1 of Tehan et al.). Naveh-Benjamin (1990) also found that participants were better at reordering lists of semantically-judged items (i.e., rate how expensive the item is) compared to acoustically-judged items (i.e., write the first rhyming word that comes to mind). However, the acoustic encoding condition required participants to produce extra items (i.e., rhyming items), essentially doubling the
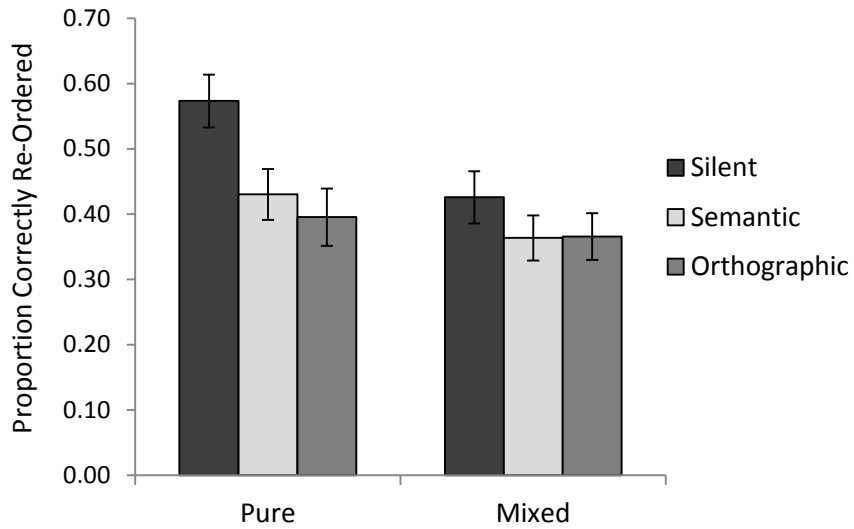
Figure 1. Experiment 1: Proportions of items that were correctly assigned to their studied

positions on the order reconstruction test. Error bars represent one standard error of the mean for

each condition[1].

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[1] The error bars in the graphs in this dissertation reflect SEs rather than confidence intervals to
give a visualization of the variability within each condition.

number of items for the list, which might have resulted in substantial interference, negatively influencing memory for order. Finally, in both Naveh-Benjamin's work and Tehan and colleagues' work, list size was much greater. Naveh-Benjamin's test involved order reconstruction of a 20 item list, and Tehan and colleagues had participants reorder lists after the presentation of six 6-item lists (36 items). Thus, in both cases, list size was much larger than in the present experiment (8 items). Some or all of these factors might affect whether a difference is observed following semantic versus non-semantic judgment tasks.

Experiment 2

Experiment 1 demonstrated that both semantic and orthographic encoding can disrupt memory for order, and that they can do so equivalently. A key feature of the semantic task ("Is this a living thing?") and of the orthographic task ("Does this word have an 'o' in it?") is that they both involve item-specific processing; that is, the response that is made in these tasks depends on the unique features (semantic or orthographic) of the particular word presented during that trial. McDaniel and Bugg (2008) theorize that it is this item-specific processing that is critical for disrupting order memory because item elaboration reduces attention to the encoding of relational information. Therefore, in Experiment 2, I explored whether *item-specific* processing is indeed necessary for disrupting memory for order, or whether any type of processing task—even one that does not require item elaboration—disrupts memory for order. To examine this, I introduced an encoding task that required a response, but not an item-specific or elaborative response.

To date, encoding tasks that disrupt memory for order have involved both item-specific processing and an overt response (e.g., an orthographic judgment task requires examining the specific orthographic features of an individual word and then a key press in response to the

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orthographic features). Thus, memory for order could be disrupted by (1) item-specific

processing, or (2) making a response. If making a response alone disrupts memory for order,

then it would not matter whether that response is item-specific. Instead, the response could be

item-generic, in that it would not be contingent on the unique features of the presented item, and

yet would still disrupt memory for order. The item-generic task used in Experiment 2 involved a

simple key press. It was a generic response because it did not differ based on the unique features

of each word: Irrespective of the word itself, participants were to press the "Enter" key.

Previous work has demonstrated that a generic key press or saying "yes" results in no memorial

benefit relative to words that were read silently (MacLeod et al., 2010, Experiment 4), lending

support to the claim that a generic key press does not enhance item-specific processing.

If item-specific processing is necessary to disrupt memory for order, then memory for

order for the key-press words should be similar to that for the silent reading condition because a

key press is not item-specific. However, if making any sort of overt response—whether item-

specific or generic—disrupts memory for order, then order reconstruction performance for key-

press lists should be poorer than for silently read lists.

Method

Participants. Twenty-eight students from the University of Waterloo (7 male, 21

female) with an average age of 20.7 participated in exchange for partial course credit.

Participants were eligible for the study only if they reported fluency in written and spoken

English, normal or corrected-to-normal vision, and normal color vision.

Materials and Procedure. The materials and procedure were nearly identical to those of

Experiment 1, with one main difference: Rather than reading silently, making a semantic

judgment, or making an orthographic judgment during the study task, participants read words

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silently (in yellow), read words aloud (in blue), or read words silently and pressed the "Enter"

key (in red). The key-press condition was selected because it required an overt response that was

not specific to the unique features of the individual word.

Results and Discussion

A 2 x 3 repeated-measures ANOVA assessed the effects of list type (mixed, pure) and

encoding condition (silent, aloud, key press) on the proportion of items correctly ordered. The

main effects of list type and encoding condition were significant, $F(1,27) = 14.57$, $MSE = .02$, p

$= .001$, $\eta_p^2 = .35$, and $F(2,54) = 3.35$, $MSE = .02$, $p = .04$, $\eta_p^2 = .11$, respectively. Their

interaction was also significant, $F(2,54) = 10.88$, $MSE = .01$, $p < .001$, $\eta_p^2 = .29$. A one-way

repeated-measures ANOVA assessing the effect of encoding condition on order reconstruction

accuracy for the mixed list revealed no effect of list type, $F(1.58,42.54) = 1.84$, $MSE = .01$, $p =$

$.17$, $\eta_p^2 = 06^2$. Of principal interest, however, were the pure lists because few differences have

been reported in memory for order as a function of encoding condition in mixed lists.

A one-way repeated-measures ANOVA assessed the effect of encoding condition on

order reconstruction accuracy for the pure lists only. The effect was significant, $F(2,54) = 8.53$,

$MSE = .02$, $p = .001$, $\eta_p^2 = .24$, and follow-up analyses revealed that memory for order for

silently read items was better than that for aloud items, $t(27) = 3.67$, $SE = .04$, $p = .001$, $d = 0.69$,

replicating previous work (Jonker et al., 2014). Furthermore, memory for order for silently read

items was also better than that for key-press items, $t(27) = 3.54$, $SE = .04$, $p = .001$, $d = 0.67$, and

memory for order for the aloud items and for the key-press items did not differ, $t(27) = 0.39$, SE

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[2] Mauchley's test of sphericity revealed that the assumption of homogeneity was violated for this
analysis, $\chi^2(2) = 8.16$, $p = .02$. Therefore, the Greenhouse-Geisser correction for degrees of
freedom was used.
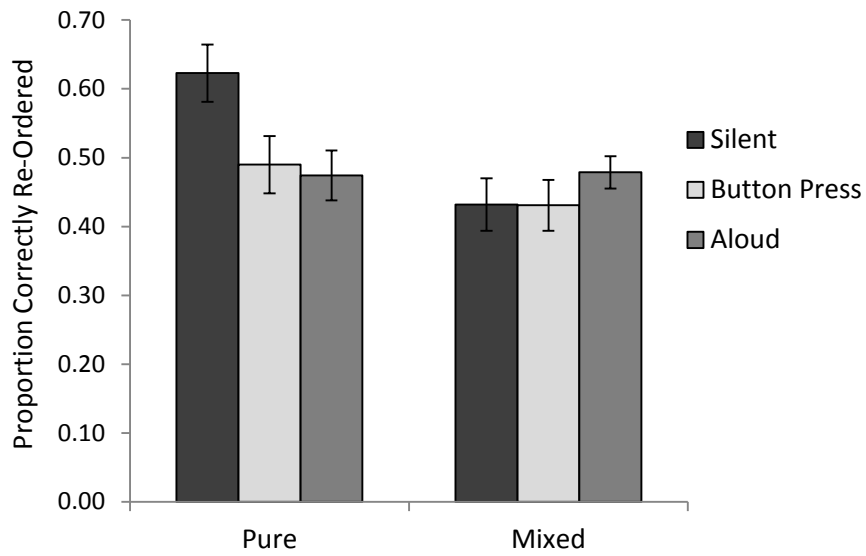
Figure 2. Experiment 2: Proportions of items that were correctly assigned to their studied

positions on an order reconstruction test. Error bars represent one standard error of the mean.

$= .04$, $p = .70$, $d = 0.07$. As shown in Figure 2, memory for order was poorer following both reading aloud and making a key press compared to memory for order following silent reading. These findings indicate that the encoding task does not have to be specific to the unique features of the presented word to disrupt memory for order. Instead, even an item-generic task can reduce ability to reconstruct order.

Experiment 3

At this point, any processing that required an overt response has disrupted memory for order (i.e., reading aloud, semantic judgment, orthographic judgment, key press). In fact, it has not mattered whether the response was item-specific or generic; having to produce a response consistently disrupted memory for order. This raises an important issue: Does a task that requires a response reduce memory for order because making an overt response is disruptive in and of itself, or does it reduce memory for order because a processing task is not relational in nature?

To address this question, Experiment 3 employed an encoding task that required an overt response but that encouraged relational encoding. This task required participants to compare the current word with the previous word ("Is this object larger or smaller than the previous object?") and was contrasted with a task that required participants to compare the present word with a constant ("Is this item larger or smaller than the average chair?"). In both cases, the participant had to make an overt response, but the former case encouraged relational processing whereas the latter case encouraged item-specific processing.

If it is relational encoding that promotes memory for order, then there should be no difference in memory for order between the items that were read silently and the items for which participants made a relational judgment. In either case, relational processing will occur and

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order information will be encoded; only the non-relational encoding in the independent size judgment task will show poorer memory for order. If, however, any sort of overt response to a word—relational or otherwise—disrupts memory for order, then there should be no difference between items to which participants made a relational size judgment versus an independent size judgment, with both showing worse memory for order than silent reading.

Method

Participants. Thirty-one students from the University of Waterloo (16 male, 15 female) with an average age of 19.8 participated in exchange for partial course credit. Participants were eligible for the study only if they reported fluency in written and spoken English, normal or corrected-to-normal vision, and normal color vision.

Materials and Procedure. The materials and procedure were nearly identical to those of Experiment 1. The main difference was the encoding tasks, which involved reading silently (in blue), making an independent semantic judgment ("Is this object larger or smaller than the average chair?"; in white), or making a relational semantic judgment ("Is this object larger or smaller than the previous object?"; in red). Of the 24 lists, there were six of each type of pure list, and two of each type of mixed list (read-independent, read-relational, independent-relational). Mixed lists were fewer because pure lists were the primary focus.

Results and Discussion

A 2 x 3 repeated-measures ANOVA assessed the effects of list type (mixed, pure) and encoding condition (silent, independent, relational) on the proportion of items correctly ordered. Both the main effects of list type and encoding condition were significant, $F(1,30) = 23.09$, $MSE = .02$, $p < .001$, $\eta_p^2 = .44$, and $F(2,60) = 5.44$, $MSE = .02$, $p = .007$, $\eta_p^2 = .15$, respectively, but their interaction was not significant, $F(2,60) = 1.53$, $MSE = .01$, $p = .23$, $\eta_p^2 = .05$. For the

mixed lists, a one-way repeated-measures ANOVA assessing the effect of encoding condition on order reconstruction accuracy revealed no effect of list type, $F(2,60) = 0.98$, $MSE = .01$, $p = .38$, $\eta_p{}^2 = .03$.

As previously, the differences in memory for order in the pure lists were of main interest. A one-way repeated-measures ANOVA revealed a significant effect of encoding condition on order reconstruction accuracy, $F(2,60) = 6.05$, $MSE = .02$, $p = .004$, $\eta_p{}^2 = .17$. Replicating the effect of Experiment 1, memory for order was better for silently read items than for independently judged items, $t(30) = 3.51$, $SE = .03$, $p = .001$, $d = 0.63$. As a novel extension, memory for order for relationally judged items was found to be better than that for independently judged items, $t(30) = 3.08$, $SE = .03$, $p = .004$, $d = 0.55$, and memory for order for relationally judged items did not differ from that for silently read items, $t(30) = 0.36$, $SE = .04$, $p = .73$, $d = 0.06$. The results are shown in Figure 3.

This experiment replicated the finding that memory for order was poorer following an independent semantic judgment (originally shown in Experiment 1), and provided an important extension to our understanding of encoding tasks and memory for order. That is, encoding tasks that are relational in nature produce relatively good memory for order, as memory for order following relational judgments was equally good as that following silent reading. Therefore, an encoding task that encourages relational processing will preserve memory for order, even though this task requires an overt response. This emphasizes that the relational nature of the encoding task is of key importance, not the overt response, and addresses the importance of the typicality of the task. McDaniel and Bugg (2008) have argued that atypical or uncommon encoding tasks "attract or require attention for individual item processing and reduce encoding of order
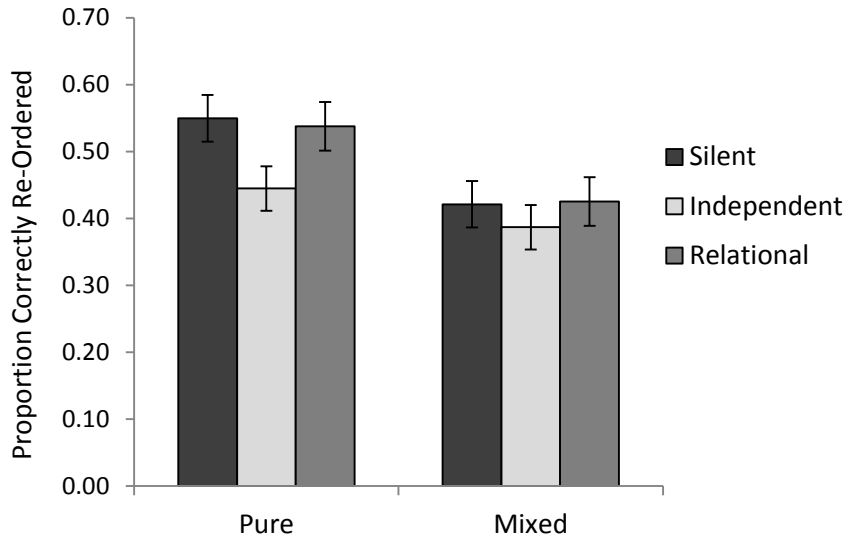
Figure 3. Experiment 3: Proportions of items that were correctly assigned to their studied

positions on an order reconstruction test. Error bars represent one standard error of the mean.

information" (p. 240), which to some degree is true in that atypical tasks attract attention. However, atypical encoding tasks do not necessarily reduce the encoding of order information; if the task is relational itself, then the attention devoted to that task will benefit memory for order. This result clearly indicates that it is relational processing, not atypicality, that is the active ingredient in good order memory.

CHAPTER II

The previous chapter revealed that memory for order was best under two conditions: reading silently or processing relationally. In contrast, any non-relational task that required a response tended to produce poorer memory for order, even if that task was as simple as a generic key press. This distinction raises an important issue: Do response-requiring tasks disrupt memory for order because they disrupt the *continuity* from one item to the next and therefore prevent or reduce the encoding of inter-item associations, or do they disrupt memory for order because they take *time* away from the processing of relations among items? The present chapter explores the role of continuity versus processing time on memory for order.

Experiment 4

When performing a non-relational response-requiring task in a list, it is necessary to abandon focus on inter-item relations and rehearsals and instead devote some time to processing the response required by that item. This required response might disrupt memory for order because it means that there is less time to encoding inter-item associations. For example, if an item is presented for 2 s and the participant must read the item aloud, which takes approximately 1 s, then there is only 1 s left to encode inter-item associations. Thus, memory for order might be poorer following reading aloud than following reading silently because when reading aloud participants have only half the time to encode inter-item associations compared to when reading silently. This would not be the case for tasks that are relational in nature (as in Experiment 3) because the time devoted to interpreting the stimulus and producing a response would involve relational encoding in and of itself. However, any non-relational task could disrupt memory for order because interpreting and providing a response would take time away from the encoding of inter-item associations.

Alternatively, it could be the case that the presence of the response task disrupts the *continuity* from one item to the next. That is, items are not processed continuously because as soon as another item is presented, the participant must stop any sort of ongoing inter-item processing to make an overt response. Disrupting the continuity or flow of the list could negatively influence memory for order because it could decrease inter-item rehearsals, clear working memory, or disrupt other processes that relate items from nearby serial positions. Under this hypothesis, memory for order depends not on the amount of time spent encoding silently, but instead on whether a disruption (i.e., a response-requiring task) occurs in the list.

To test the importance of processing time versus continuity for memory for order, a processing task was positioned either *during* the presentation of a word or *between* the presentations of words. When the response task occurred between words, it was incorporated into the list, which should disrupt the continuity from one item to the next, but the response task did not take processing time away from individual items because it occurred between presentations. Thus, if poor memory for order is simply a matter of reducing time for relational encoding, then a key press between items should have no negative effect on memory for order. However, if the presence of a response task disrupts the flow from one item to the next, then its presence between items should result in poorer memory for order, similar to the cost observed for a key press during the processing of the item.

The response task used in the present experiment was the key-press task from Experiment 2. The key press occurred either during the 2 s presentation of a word or between words, which allowed me to test whether continuity or time affect memory for order. A secondary goal of this experiment was to replicate the somewhat surprising finding of Experiment 3 that memory for order was poorer following key-press lists compared to silently read lists.

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Method

Participants. Forty students[3] from the University of Waterloo (9 male, 31 female) with an average age of 19.3 participated in exchange for partial course credit. Participants were eligible for the study only if they reported fluency in written and spoken English, normal or corrected-to-normal vision, and normal color vision.

Materials and Procedure. The materials and procedure were nearly identical to those of Experiment 2, but some of the pure silent and pure key-press lists included an additional task: These lists each had six asterisks (*) randomly assigned to appear in red between words, and participants were to press the "Enter" key whenever they saw an asterisk (see Figure 4). The asterisk disappeared once they had pressed "Enter." Repetitions of asterisks were not disallowed; therefore, sometimes asterisks appeared sequentially.

As in the previous experiments, there were 24 lists; the distribution of the lists was as follows: 4 pure silent reading, 4 pure key press, 4 pure silent reading with asterisks, 2 pure key press with asterisks, 4 pure aloud, 6 mixed lists (no asterisks). Each mixed list involved two of the three types of encoding manipulations (read silently, key press, read aloud). I was interested primarily in the pure silently read lists and pure silently read lists with asterisks. The key-press and aloud lists were included to replicate the finding of Experiment 2. Two key-press lists with asterisks were included so that the asterisk manipulation was not exclusive to the silently read lists.

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[3] In all other experiments, the target sample size was 30. Research assistants were instructed to collect a few participants beyond 30, and any deviation from 30 was due to fluctuations in participation. Experiment 4 was the only exception. Here, research assistants were instructed to collect data from 40 participants. A larger *n* was selected a priori to address the fact that there were more conditions in this experiment (8, rather than 4 or 6); therefore it was expected that estimates would be slightly less reliable and more power was desired.
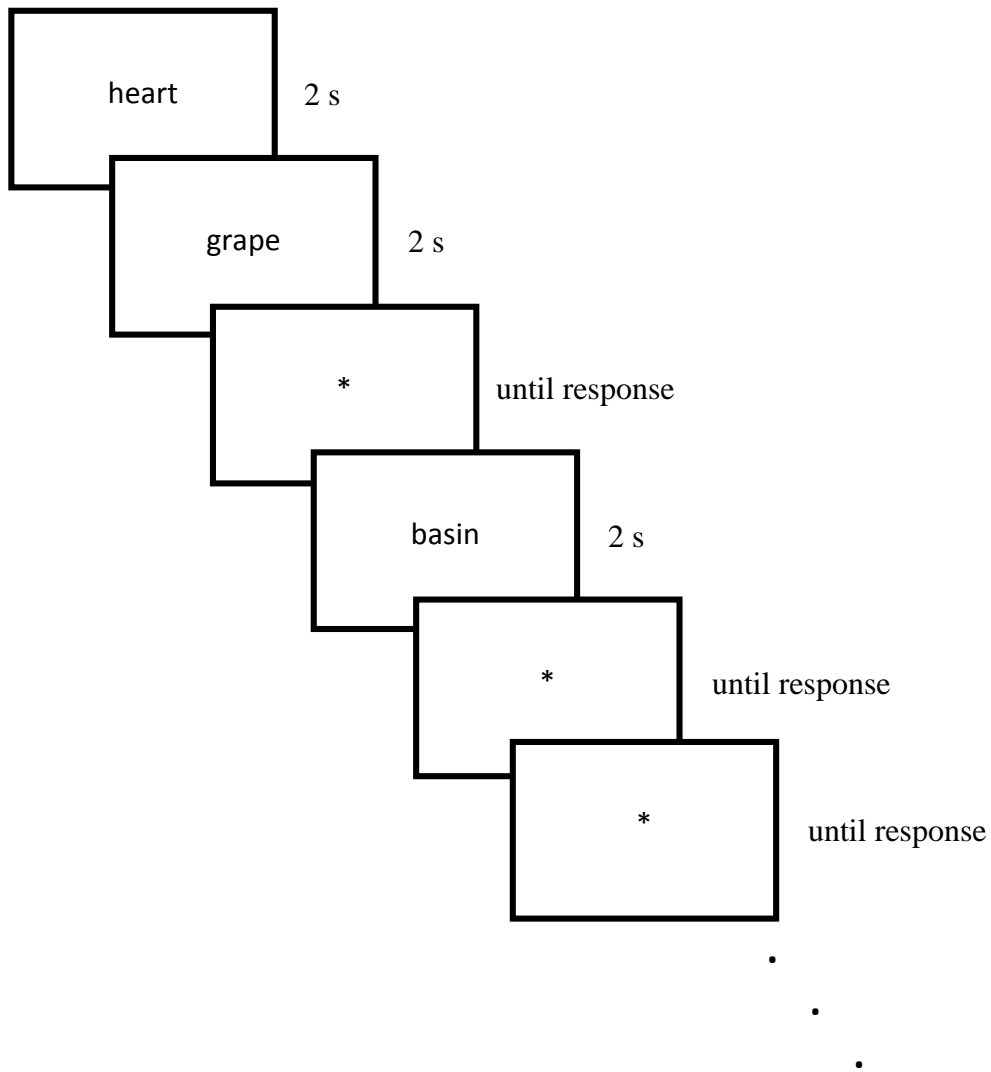
Figure 4. Example of a list with asterisks between the presentations of individual words. Each

asterisk remained on the screen until the participant pressed "Enter."

Results and Discussion

  Two omnibus ANOVAs were conducted. The first assessed whether the effects from Experiment 2 were replicated. This was a 2 x 3 repeated-measures ANOVA that examined the effects of list type (mixed, pure) and encoding condition (silent, aloud, key press; lists with asterisks were not included in this analysis) on the proportion of items correctly ordered. The main effects of list type and of encoding condition were both significant, $F(1,39) = 15.70$, $MSE = .02$, $p < .001$, $\eta_p^2 = .29$, and $F(2,78) = 6.58$, $MSE = .02$, $p = .002$, $\eta_p^2 = .14$, and their interaction was also significant, $F(2,78) = 5.67$, $MSE = .02$, $p = .005$, $\eta_p^2 = .13$.

  To follow up on these effects, a one-way repeated-measures ANOVA assessed the effect of encoding condition on order reconstruction accuracy for the mixed list; as in the previous experiments, it revealed no effect of list type, $F(2,78) = 0.61$, $MSE = .02$, $p = .55$, $\eta_p^2 = .02$. A second one-way repeated-measures ANOVA assessed the effect of encoding condition on order reconstruction accuracy for the pure lists. The effect of encoding condition was significant, $F(2,78) = 10.66$, $MSE = .02$, $p < .001$, $\eta_p^2 = .22$, and follow-up analyses revealed that memory for order for silently read items was better than that for aloud items, $t(39) = 4.61$, $SE = .03$, $p < .001$, $d = 0.73$, and was marginally better than that for key-press items, $t(39) = 1.74$, $SE = .03$, $p = .09$, $d = 0.46$. Furthermore, memory for order for key-press items was better than that for aloud items, $t(39) = 2.92$, $SE = .03$, $p = .006$, $d = 0.46$.

  The first two findings replicate those of Experiment 2; however, the finding that memory for order for key-press items was better than for aloud items was novel. Possibly this finding emerged because the key-press condition takes less time (and/or is easier or less variable) than reading aloud, leaving more time for relational encoding. However, I could not assess this possibility because response latencies during encoding were not recorded. Also, if the effect

were due to time or difficulty, then it is unclear why a similar effect was not observed in Experiment 2. Another possible explanation is that the inclusion of the asterisk manipulation led to increased practice with key pressing, making the task less demanding; this would not be the case for the aloud condition. This seems plausible because participants made more key presses over the course of this experiment than they did in Experiment 2, whereas the aloud condition did not change. If the key-press manipulation becomes easier or more fluent over the course of the task, then participants might have more time to devote to encoding relations, which would be consistent with the processing-time hypothesis. In any case, the general pattern of results from Experiment 2 was replicated.

To address the hypothesis of interest, a subsequent 2 x 2 repeated measures ANOVA assessed the effects of encoding condition (silent, key press) and the presence of asterisks (asterisks, no asterisks) on the proportion of items correctly ordered. This analysis revealed a replication of the key press effect: Participants better reconstructed the order of silently read lists relative to key-press lists, $F(1,39) = 6.40$, $MSE = .02$, $p = .02$, $\eta_p^2 = .14$. There was, however, no effect of asterisk, $F(1,39) = 0.25$, $MSE = .03$, $p = .62$, $\eta_p^2 = .01$, and no significant interaction, $F(1,39) = 0.02$, $MSE = .02$, $p = .89$, $\eta_p^2 < .01$. As can be seen in Figure 5, for pure lists of silently read items, memory for order without intervening asterisks did not differ significantly from that with intervening asterisks, $t(39) = 0.28$, $SE = .03$, $p = .77$, $d = 0.04$.

These results replicated the pattern of Experiment 2, and extended the findings by demonstrating that including a key-press manipulation *between* words did not disrupt memory for order, whereas a key press *during* words did. Thus, these findings suggest that various processing tasks might disrupt memory for order because they use up some of the time that would otherwise be devoted to encoding order information, but these findings suggest that poor

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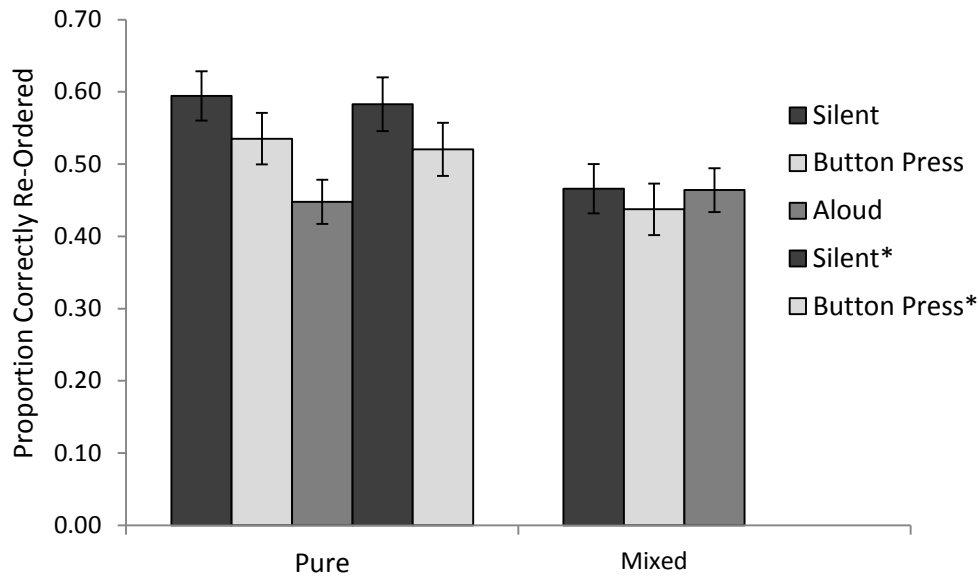
Figure 5. Experiment 4: Proportions of items that were correctly assigned to their studied positions on an order reconstruction test. Conditions "Silent*" and "Button Press*" were lists that involves silent reading and key pressing, respectively, as well as asterisks between items. Error bars represent one standard error of the mean.

memory for order is not the result of simply disrupting the overall flow or continuity of the list. It is worth acknowledging that I cannot and do not claim that the null hypothesis is true in this case; more specifically, I cannot state that including a task between the stimulus presentations has *no* effect on memory because this test might not have been sensitive to the effect of an intervening task on relational memory. However, the purpose of Experiment 4 was to determine whether disrupted order memory was due solely to the presence of any processing task in a list and this was found not to be the case because order memory for silent lists with asterisks was superior to order memory for lists that involved a key press to each item. Therefore, if there is an effect of an intervening task on relational memory, it is very small and is not the driving force underlying the connection between response tasks and poor memory for order.

Experiment 5

Experiment 5 continued my exploration of the role of processing time in memory for order. Here, I tested whether extended processing time would increase memory for order, even if a response-requiring task occurred during item presentation. In Experiment 4, a response task between items had no negative effect on memory for order, leading me to tentatively conclude that processing time—rather than list continuity—is important for memory for order. However, this conclusion was based on a null result. Therefore, the present experiment addresses the same question with the predicted outcome being a significant difference, rather than a null result.

In the present experiment, the duration of item presentation was manipulated as well as the encoding task. Specifically, items were encoded through either silent reading or semantic judgment, and item presentation time was either short or longer.

If list continuity is important for memory for order, then any list employing a non-relational response task should result in poor memory for order, irrespective of presentation time,

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because the task would disrupt the encoding of inter-item associations. In this experiment, therefore, presenting items for a longer duration should improve memory for order for silently read lists—because more time could be devoted to relational encoding—but should have no effect on memory for order for lists with an independent semantic encoding task—because this task would disrupt continuity irrespective of presentation duration. In other words, an interaction would be expected.

Alternatively, if memory for order depends on the amount of time that one can devote to relational processing, and elaborative encoding tasks tend to occupy some of the time that would otherwise be devoted to relational processing, then presenting items for a longer duration should compensate for the time devoted to the response-requiring task. In this case, the size of the difference in memory for order between reading silently and making a semantic judgment should remain the same even after increasing the presentation duration of each item during study. In other words, no interaction would be expected.

This latter outcome seems more probable given the results of Experiment 4. However, Experiment 4 failed to reject the null, which was in favor of the hypothesis that time underlies the effect of encoding task on memory for order. The present experiment tests these two opposing predictions (continuity vs. time) by employing a design which looks for support for the alternative hypothesis rather than a null result.

Method

Participants. Thirty-five students from the University of Waterloo participated in exchange for partial course credit. The data of two participants were removed for non-compliance with the encoding tasks; their mean accuracy for silent and semantic judgments (withhold response and produce response, respectively) was more than 3 SDs below the mean

(.52 and .57, as compared to $M = .95$, $SD = .04$). Included participants (11 male, 22 female) had an average age of 20.6 years (age was not reported by one participant). Participants were eligible for the study only if they reported fluency in written and spoken English, normal or corrected-to-normal vision, and normal color vision.

Materials and Procedure. The materials and procedure were nearly identical to those of Experiment 1. The main difference was that participants encoded words either by silently reading them (in yellow) or by making a semantic judgment on them (living or non-living thing; in blue). All 24 lists were pure lists. Half of them involved silent reading and half involved semantic judgments. For each list type, half of the lists had items presented for a short duration (2 s each) and half had items presented for a longer duration (4 s each). Thus, there were six lists of each type (silent-short, silent-long, semantic-short, semantic-long).

Results and Discussion

A 2 x 2 repeated-measures ANOVA assessed the effects of encoding condition (silent, semantic) and encoding duration (2 s, 4 s) on the proportion of items correctly ordered. Both main effects were significant, $F(1,32) = 9.62$, $MSE = .01$, $p = .004$, $\eta_p^2 = .23$, and $F(1,32) = 22.62$, $MSE = .02$, $p < .001$, $\eta_p^2 = .41$, respectively. Importantly, however, the interaction was not significant, $F(1,32) = 0.98$, $MSE = .01$, $p = .33$, $\eta_p^2 = .03$. The lack of interaction demonstrates that extending encoding time did not have a differential effect on memory for order for silently read lists, $t(32) = 3.45$, $SE = .03$, $p = .002$, $d = 0.60$, compared to semantic lists, $t(32) = 4.37$, $SE = .03$, $p < .001$, $d = 0.76$. That is, as shown in Figure 6, making a semantic judgment on words resulted in equivalent reduction in memory for order compared to silent reading regardless of whether words were encoded for a short duration, $t(32) = 2.73$, $SE = .03$, $p = .01$, $d = 0.47$, or for a long duration, $t(32) = 2.30$, $SE = .02$, $p = .03$, $d = 0.40$.
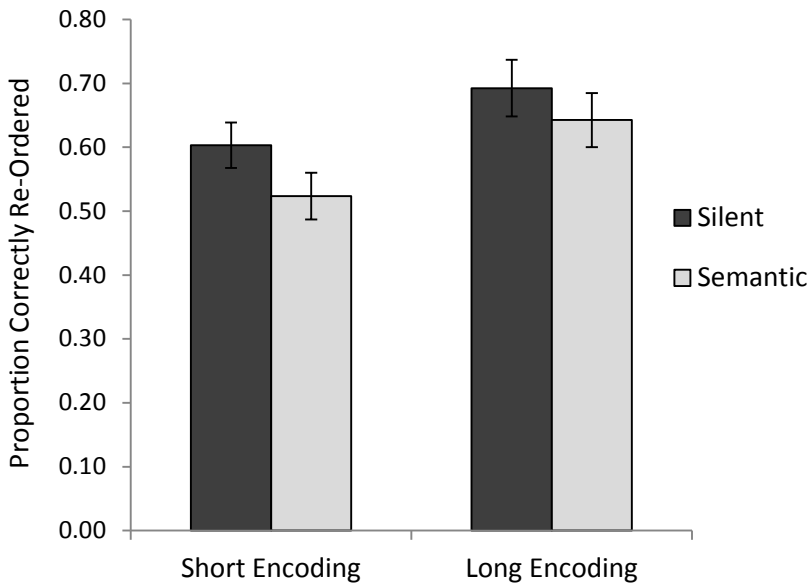
Figure 6. Experiment 5: Proportions of items that were correctly assigned to their studied

positions on an order reconstruction test. Error bars represent one standard error of the mean.

Therefore, an encoding task that disrupts the silent study of words reduces memory for order, but this reduction can be overcome or compensated for by increasing encoding time (silent short vs. aloud long, $t(32) = 1.32$, $SE = .03$, $p = .20$, $d = 0.23$). These findings corroborate the findings from Experiment 4 and suggest that elaborative encoding tasks reduce memory for order because they take some processing time away from the encoding of inter-item associations (Francis & Baca, 2014). However, they do not disrupt the encoding of order information entirely; once the elaborative encoding task has been completed, further processing can then be devoted to encoding relational information.

In this chapter, I found no evidence for the importance of list continuity, as there was no effect of asterisks in Experiment 4 and no interaction between encoding type and encoding duration in this experiment. Instead, I found support for the role of processing time, such that longer processing times can benefit memory for order even when a non-relational response task is present.

CHAPTER III

The majority of research on memory for order and relational information focuses on order reconstruction tests (McDaniel & Bugg, 2008). Although order reconstruction tests produce fairly consistent results, the use of one type of test severely limits the generalizability of the conclusions drawn from the experiments. There might be particular features of the order reconstruction test (e.g., that all items from the list are re-presented during the test) that produce the pattern of results observed and that therefore would not be observed on other measures of order memory. Therefore, Chapter 3 introduces another way of examining order memory using a test that has many different surface features.

Experiment 6

The novel test introduced here, like the order reconstruction test, emphasizes inter-item associations. In this test, participants are shown a target word from the study list, along with two other words from the study list. Participants are to indicate which of those two words immediately followed the target word. In other words, participants must identify consecutive pairs.

This order recognition test differs from an order reconstruction test in a number of important ways. First, it involves only a subset of the study list (three items) rather than the entire list; second, it places no emphasis on *where* in the list the target word occurred (i.e., serial position), but only on memory for the pairing between the target word and the subsequently presented word; third, it involves a single trial with a single key press, which provides more control and allows for more straightforward response-time analyses. Therefore, the primary goal of this experiment is to determine whether the superiority of silent reading relative to other types of encoding will be replicated using a novel test of order memory.

The secondary goal of Experiment 6 was to examine the types of information that are available following different types of processing. For this reason, I employed both the novel order recognition test and a speeded semantic test. This allowed me to examine whether various encoding tasks facilitate processing on tests of relational memory versus semantic memory. This relies on the principle of transfer-appropriate processing, which is the idea that memory is superior when the processes engaged during encoding are similar to the processes engaged during test (Morris, Bransford, & Franks, 1977). If making semantic judgments enhances item-specific information, then responses on an item-specific task should be facilitated (i.e., faster and/or more accurate), and if silent reading enhances relational information, then responses on an order test should be facilitated.

Method

  Participants. Thirty students from the University of Waterloo participated in exchange for partial course credit. Participants (9 male, 21 female) had an average age of 19.9 years. Participants were eligible for the study only if they reported fluency in written and spoken English, normal or corrected-to-normal vision, and normal color vision.

  Materials and Procedure. The materials were identical to those of Experiment 1. During study, participants either read words silently (in blue), or they were to indicate whether the object was larger or smaller than the size of a microwave (in white). In the latter case, participants were to respond orally by saying "larger" or "smaller" and a research assistant was present to ensure compliance. Responses were oral to ensure that the surface features of the study task would be different from those of the test, which also included a size judgment task. All other details of the study procedure were identical to those of Experiment 1. Following study, participants then completed the parity judgments distractor task described in Experiment

1. Memory for items from the study list was tested in one of two ways in each block, via an order test or via a size test.

Order test. For this test, participants were shown a single word in the center of the screen (target), along with two other words, one in each of the bottom left and bottom right corners of the screen (see Figure 7). Of these two words, one had occurred immediately following the target in the study list, and the other had occurred four positions later. Participants were to indicate with a key press which of the two items had immediately followed the target during study; they were encouraged to respond as quickly and accurately as possible. There were two test trials per block, and target words were always selected from positions 2 and 4 of the study list, meaning that response options were from positions 3 and 6 and 5 and 8, respectively. This was done to avoid recycling items within a block and to avoid the item from serial position 1.

Size test. For this test, participants were shown a single word from the study list in the center of the screen and they were to indicate with a key press whether that object is larger or smaller than an average chair (see Figure 7). Participants were encouraged to respond as quickly and accurately as possible. All items from the study list were tested in a random order. Thus, there were eight test trials for the size test. For this test, ratings are subjective; therefore, there was no accuracy measure and analyses focus only on response time.

Each test phase began with a 2-s reminder to respond as quickly and as accurately as possible, and each test trial was preceded by a screen (2 s) with a question to cue the participant to the test type ("which came next?" for the order test, and "larger or smaller than a chair?" for the size test).

Study

Order Test

oyster

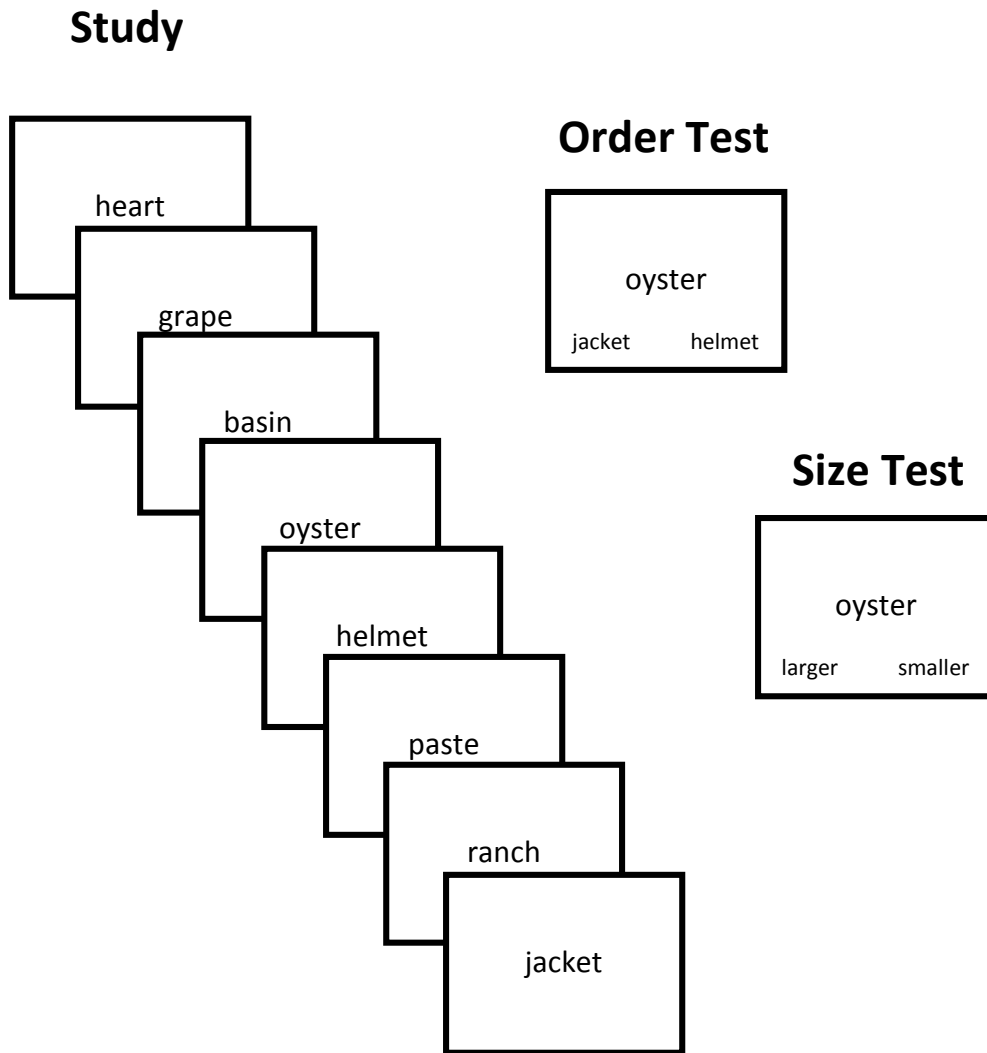jacket   helmet

Size Test

oyster

larger   smaller

*Figure 7.* Example of the two different types of tests in Experiment 6.

There were 24 lists in this experiment, half allocated to each encoding type (silent reading, semantic judgment). For each set of 12 lists, four included the size test, and eight included the order test. This was done to compensate for the fact that the size test involved more trials (8) than the order test (2), thus resulting in 32 data points for the size test and 16 data points for the order test for each encoding condition.

Results and Discussion

The responses to the size judgment tests were subjective and therefore accuracy was not measureable for this test; instead, for this test, the dependent variable of interest was response time (RT) only. Thus, here I first focus on accuracy results for the order test, and then on a direct comparison of RT results for both the size and order tests.

Accuracy. A paired-samples *t*-test comparing mean accuracy of deeply encoded lists compared to silently read lists for the order tests revealed a significant effect of encoding condition, $t(29) = 2.21$, $SE = .04$, $p = .04$, $d = 0.40$, which is shown in Figure 8, Panel A. This pattern replicates that produced on order reconstruction tests (e.g., Figures 1, 3, and 6). This experiment demonstrates that participants were better able to recognize which items occurred in close proximity after having encoded a list through silent reading than they were after having encoded a list while making semantic judgments, further supporting the claim that reading silently leads to better order memory by establishing stronger inter-item associations.

Response time. Anticipatory responses to test trials were removed (< 300 ms). This resulted in the removal of a total of 3 trials from all order test data, and one from all size test data. Furthermore, RTs were included for correct trials from the order test, and any responses that were 2.5 SDs slower than the mean for the relevant condition for each individual participant were removed. This resulted in the removal of 0.8% of the accurate trials from the order test (no
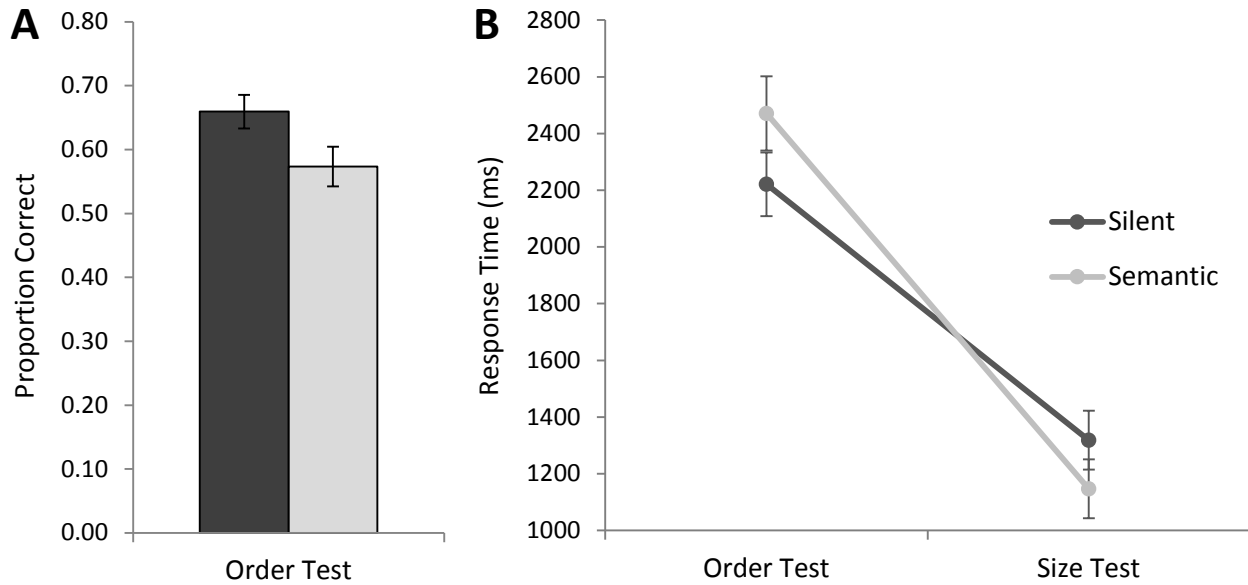
Figure 8. Experiment 6. Panel A displays the proportions of trials on which participants correctly identified the subsequently presented item on the order recognition test. Panel B displays the response times for correct trials from the order test and all trials from the size test. Error bars represent one standard error of the mean.

more than one trial was removed as an outlier for any one participant). After these exclusions, the data of one participant were excluded entirely from the RT analysis because this participant only had two eligible trials in the semantic condition; all other participants had at least 6 trials per encoding condition for the order test ($M = 9.9$).

A 2 x 2 repeated-measures ANOVA assessed the effects of encoding condition (silent, semantic) and test type (order, size) on test RT. There was a main effect of test type, with faster overall responses to the size test than to the order test, $F(1,28) = 96.40$, $MSE = 429644.84$, $p < .001$, $\eta_p^2 = .78$, but no main effect of encoding condition, $F(1,28) = 0.21$, $MSE = 75127.36$, $p = .65$, $\eta_p^2 = 01$. As can be seen in Figure 8, Panel B, this absence of a main effect of encoding condition was due to a crossover interaction between encoding condition and test type, $F(1,28) = 18.35$, $MSE = 59983.54$, $p < .001$, $\eta_p^2 = .40$. For correct responses on the order test, participants responded more quickly after having encoding through silent reading compared to size judgments, $t(28) = 2.52$, $SE = 86.61$, $p = .02$, $d = 0.47$, whereas on the size test, participants responded more quickly after having encoded by making size judgments compared to reading silently, $t(28) = 4.02$, $SE = 42.63$, $p < .001$, $d = 0.75$.

This crossover interaction occurred despite the fact that the features of the tests differed from the features of the encoding tasks, even for the two size judgment tasks (e.g., spoken vs. key-press responses, compared to a microwave vs. a chair). The results from this experiment replicate the general pattern that has been found in the preceding experiments. Importantly, this experiment demonstrates that the order memory effect that is typically found using order reconstruction tests (silent reading > elaborative encoding) can be found on other types of tests, at least on those that emphasize inter-item associations. This extends the generalizability of the pattern of results because the order recognition test used in the present experiment differs from

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an order reconstruction test in a number of ways. For example, the order recognition test involves only three items rather than the entire test list, demonstrating that the effect can be observed even when the entire study context (the complete list) is absent. Furthermore, the order recognition test placed no emphasis on the serial position of the target word, whereas an order reconstruction test requires the participant to place each item in its correct serial position. This demonstrates that the order reconstruction results are not the product of participants' knowledge of generally *where* in the study list the item was presented (i.e., temporal memory) but instead supports the argument that order reconstruction is the result of superior memory for inter-item associations.

General Discussion

In this dissertation, memory for order was consistently found to be superior for items from lists that were read silently at the time of study. It did not matter whether the alternative encoding task involved semantic versus orthographic encoding (Experiment 1) or item-specific versus item-generic responses (Experiment 2): With only two theoretically diagnostic exceptions (discussed in the following two paragraphs), order memory was disrupted following response-requiring tasks as compared to silent reading. Thus, Experiments 1 and 2 suggest that any sort of encoding task requiring an overt response disrupts memory for order.

There were two exceptions to this general finding, both providing further insight into the influence of encoding conditions on memory for order. The first exception was when the encoding task itself emphasized relational processing. In Experiment 3, a semantic task that required rating the size of an object relative to the size of the previously presented object resulted in good memory for order, a rating that required inter-item comparison. In sharp contrast, when the size judgment was made not to the previous list item but to a standard (i.e., compared to the size of an average chair), this independent semantic judgment had a negative effect on memory for order relative to reading silently or making a relational semantic judgment. This result emphasizes the importance of processing inter-item relations for good performance on an order reconstruction test. Thus, although an elaborative encoding task typically disrupts memory for order, when that task is itself relational in nature, then memory for order will be equivalent to the case where no additional elaborative encoding task exists (e.g., reading silently).

A second condition that can benefit memory for order is increasing study time, even in cases in which an elaborative encoding task must be completed. In Experiment 5, extending the study time from 2 s to 4 s enhanced memory for order irrespective of the encoding task. In fact,

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a total of 4 s of encoding time for semantically-judged items resulted in order reconstruction performance comparable to 2 s of silent reading, suggesting that the additional 2 s of encoding time can offset the cost that is typically observed following elaborative encoding. Furthermore, moving the response task to occur between items rather than during, as in Experiment 4, can also preserve memory for order, presumably because this version of a response-requiring task does not take time away from the encoding of inter-item associations.

Experiments 4 and 5 dispel the possibility that order memory is poorer because the presence of a non-relational encoding task disrupts the continuity of the list and thereby one's ability to encode inter-item associations. In Experiment 4, a key-press task occurred *between* the presentations of items and had no negative effect on memory for order. In fact, performance on lists of silently read items with key presses between items was equivalent to performance on lists of silently read items with no key presses. In Experiment 5, extending encoding time ameliorated the negative effect of a response-requiring task on memory for order. These two results demonstrate that the presence of a task requiring an overt response is not negative in and of itself, and instead support the claim that memory for order is influenced by the amount of unconstrained study time that can be devoted to items and particularly to the encoding of inter-item associations.

Thus, Experiments 1 and 2 demonstrate conditions that can harm memory for order—namely, overt responding that is item-specific or generic—while Experiments 3, 4, and 5 demonstrate conditions that can improve memory for order—namely, a relational encoding task, processing between rather than during item presentation, or adding extra response-free processing time.

In a final experiment, I demonstrated that the order memory differences between silently read and elaborately encoded lists are not limited to order reconstruction tests. Specifically, I found superior performance for silently read items on an order recognition test emphasizing successive items, providing a conceptual replication of the effect using a novel measure. This extends the generalizability of the order reconstruction findings and provides researchers with a different tool that can be used to examine memory for inter-item associations. This new test method can be used to provide conceptual replications and converging evidence within this domain of research.

Order Memory and the Item-Order Account

In their review, McDaniel and Bugg (2008) postulate that unusual or uncommon tasks or stimuli attract attention, and that it is the capturing of attention that disrupts relational encoding. The present work further delineates the parameters of the item-order account and sheds light on the mechanisms underlying memory for order.

Important insight into the conditions that affect memory for order comes from Experiment 3. In this experiment, I found that atypical encoding does not necessarily lead to poor memory for order; instead, an encoding task that is unusual but that emphasizes relational encoding actually can *preserve* memory for order. Under these conditions, in fact, memory for order for relationally encoded words did not differ significantly from memory for order for silently read words. It may be best, then, to think of atypical encoding tasks as ordinarily emphasizing item information over relational information but to recognize that this is not true of all atypical encoding tasks and that atypical encoding tasks exist that instead emphasize relational processing.

Further insight into the conditions that negatively influence memory for order comes from Experiments 2 and 4. In these experiments, I found that processing during encoding does not have to be item-specific or elaborative to disrupt memory for order: A generic key press disrupted order memory even though this task was not based on the unique features of the presented word. This indicates that having to make a response has a negative effect on memory for order, irrespective of whether that response is item-specific or generic. This result is not predicted by McDaniel and Bugg's (2008) item-order account. According to their account, relational processing is disrupted when the encoding task encourages item-specific processing. Despite not involving item-specific processing, the generic key-press condition nevertheless disrupted memory for order.

It is worth noting at this point that I observed no differences in memory for order for the mixed lists, apart from a small effect in favor of silent items from mixed lists seen only in Experiment 1 and not subsequently replicated. At face value, this lack of differences in mixed lists is consistent with McDaniel and Bugg's (2008) item-order account: Memory for order should be disrupted in any list that contains elaborative, item-specific processing because the presence of this processing will reduce relational encoding. In all cases of mixed lists, memory for order was disrupted relative to memory for order for pure lists of silently read items, and memory reconstruction scores for mixed lists were approximately equivalent to reconstruction scores for pure lists involving an elaborative processing task (e.g., pure aloud lists). This suggests that engaging in a non-relational processing task disrupts relational encoding because the participant cannot devote as much time to relational encoding.

However, it is interesting to note the special case of the read-relational mixed list from Experiment 3. This list type contains two types of processing, both of which have been found to

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produce relatively good memory for order in pure lists in the present work. Thus, if memory for order is entirely a function of relational encoding, then it would be reasonable to expect that order reconstruction performance for the read-relational mixed list would be equivalent to performance for pure read and pure relational lists, or at the very least that it would be superior to that of the other mixed lists. However, this was not the case: Order reconstruction performance for the read-relational mixed list did not differ from that for other lists (read-relational = .41, read-independent = .42, relational-independent = .40). This suggests that task switching may result in a cost to memory for order, or that the strategies used during silent reading versus relational semantic judgments are very different from each other and are incompatible within a mixed list. Further research on this matter is needed, however, before any strong conclusions can be made.

Conclusion

From the research reported in this dissertation, I suggest a key modification to our understanding of order memory and the item-order account. Specifically, I propose that a stimulus or encoding process will disrupt order memory only when (1) it is attention-grabbing (either by being atypical or by requiring an overt response) and (2) it does not encourage relational encoding among list items. In other words, from the present results, I conclude that order memory is encoded either by default (i.e., during silent reading) or when an explicit task encourages relational encoding, but that this process is easily disrupted if processing time must be devoted to a non-relational task or to a distinct stimulus feature (as in the case of the bizarreness and word frequency effects, see McDaniel & Bugg, 2008).

Taken together, then, the experiments reported in this dissertation suggest that all that is important for memory for order is relational encoding, but that the encoding of relational

information does not occur automatically (Naveh-Benjamin, 1990; cf. Hasher & Zacks, 1979). Instead, any attention-grabbing factor during the encoding phase (either stimulus or task feature) reduces the ability to encode relations among items except when the task itself encourages relational encoding. This would imply that unconstrained study time or a task that emphasizes relations between items is necessary for encoding inter-item associations.

This work highlights how we remember different features of an event, depending on the tasks and demands that we experience during encoding. In cases where time is limited and one is required to respond to the environment, it is likely that inter-item associations are not being well-encoded. Therefore, when one is remembering a series of episodic events, such as filling a canteen with river water and purifying collected water, if that individual were very distracted or performing other tasks during those events (e.g., in conversation with a friend, or checking a compass and noting position on a map), then that person should be wary of his/her memory for the order of those events, because memory for order was likely to be poor.

One of the striking features of this work on memory for order is that it reveals circumstances under which less elaborative encoding is actually better. But discovering this hinges on the appropriate test. Relational information—as measured using an order reconstruction test or order recognition test—is actually better encoded when simply reading as opposed to performing a more elaborative orienting task. This fits with the longstanding tradition of transfer-appropriate processing, too, in that memory performance depends not just on the nature of the encoding, or even on the nature of the test, but on the match between what was encoded and what the test requires. Encoding can occur along many dimensions, and we can only determine which dimensions have been encoded by using suitable measures of memory.

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