Artificial Grammar Learning in a Dual Task Paradigm.

Barbara Cochran
Louisiana State University and Agricultural & Mechanical College

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ARTIFICIAL GRAMMAR LEARNING IN A DUAL TASK PARADIGM

A Dissertation

submitted to the Graduate Faculty of the
Louisiana State University and
Agricultural and Mechanical College
in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy

in

The Department of Psychology

by

Barbara Cochran
B.A., Southeastern Louisiana University, 1989
M.A., Louisiana State University, 1992
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Dedication

To Larry, Jason, Brad, Erin, and Shelly who taught me things that cannot be learned in school. And to my parents, teachers, and friends who encouraged me to persevere.
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Abstract

A series of experiments explored the effects of performing concurrent secondary tasks on learning letter strings created with a finite state artificial grammar.

Experiments 1-2 compared a task which disrupted organized encoding to a task which simply required holding information in memory while encoding strings, as well as to two control tasks. Participants performing the disruptive task were worse at judging the grammaticality of test strings than were participants in the two single task control groups. Performance of the memory load group fell between the disruptive task group and the control groups, but was not significantly different from either.

Experiments 3-4 compared the effects of secondary tasks which consistently grouped letters frequently seen together or consistently interrupted letters frequently seen together in grammatical strings. Disrupting frequent letter groups inhibited learning to a greater extent than grouping frequent chunks; however, predicted facilitatory effects for chunking frequent groups of letters were not found.

Experiment 4 also tested the effects of secondary task stimuli differing on relative verbalizability, finding very little difference amongst the three types of stimuli tested.
Additionally in Experiments 2 and 4, the ability to detect ungrammatical strings with violations in various locations was tested. These results replicated previous findings in the grammar learning literature, with errors at the beginnings and ends of strings easier to detect than those in the middle.

Findings of this research indicate that it is possible to learn artificial grammar strings under dual task conditions; however, performing any type of secondary task is likely to inhibit learning somewhat. The extent of disruption may depend on the processing demands of the secondary task. Overall results indicate that a full explanation of the effects of secondary tasks on grammar learning may require a two factor model including both limited capacity processing resources and necessary organization of study strings.
Chapter 1 - Introduction

Artificial Grammar Learning in a Dual Task Paradigm

Learning is defined as a relatively permanent change in potential behavior that results from experience. This is a simple straightforward definition which was investigated primarily in conditioning paradigms using animals for the first half of this century. After World War II, however, challenges to the dominance of psychology by radical behaviorism together with revived interest in mental processes ushered in a new era in psychology. Theorists turned their attention to factors influencing human behavior, including learning, and scientific investigation of human learning processes began in earnest (Leahey, 1991).

Research involving human learning has yielded some controversial theories regarding the fundamental nature of the learning process. Some theorists assert that human learning is a unitary phenomenon with one basic underlying process which cannot proceed without conscious attention (Newell & Simon, 1972; Anderson, 1990; Tulving, 1989). Others, however propose two distinct processes. One process is said to be intentional and strategic, resulting in knowledge of which the learner is aware, while the other process is neither intentional nor strategic, resulting in knowledge of which the learner is largely unaware, but which influences behavior nonetheless (Lewicki, 1986; Reber, 1993; Berry & Dienes, 1993; Cleeremans, 1993). This second
learning process, referred to by some as "implicit learning," is said to be distinct in several ways which will be described in the section below (Reber, 1993; Berry & Dienes, 1993; Cleeremans, 1993). The purpose of the experimental investigation which is the subject of this paper is to investigate one of the characteristics of implicit learning that is said to distinguish it as a separate psychological process.

Distinguishing Characteristics of Implicit Learning

Much of the implicit learning research in recent years has been devoted to providing evidence of a dissociation between the two proposed learning processes. According to Berry and Dienes (1993), implicit learning is characterized by four features which distinguish it as a separate process. First, implicit learning shows specificity of access. What Berry and Dienes mean by this is simply that implicitly acquired knowledge seems to be difficult to elicit under conditions which are different from the learning situation. Participants in implicit learning experiments normally have difficulty communicating knowledge of how they make judgments or perform task (Reber, 1989; Dienes, Broadbent, & Berry, 1991; Lewicki, Hill, & Bizot, 1988; Kushner, Cleeremans, & Reber, 1991). There is also evidence that while forced-choice tests reveal that learning has occurred, participants' confidence ratings very often are not related
to performance (Chan, 1992 as reported in Berry & Dienes, 1993).

Another aspect of this specificity characteristic is that researchers often report a lack of transfer when surface elements of the task change (Willingham, Nissen, & Bullemer, 1989; Stadler, 1989; Berry & Broadbent, 1988; Squire & Frambah, 1990; Berry, 1991). However, some evidence for transfer of implicitly acquired knowledge when the surface features of the stimulus set change has been reported in the literature (Reber & Allen, 1978; Mathews, Buss, Stanley, Blanchard-Fields, Cho, & Druhan, 1989). Taken together, experimental evidence points to the fact that implicitly acquired knowledge seems to be surprisingly specific. It usually seems to be tied to the surface characteristics of the situation and may be difficult to access apart from the conditions under which it was acquired.

The second feature of implicit learning listed by Berry and Dienes (1993) is that implicit learning tends to be associated with incidental learning conditions. In most implicit learning paradigms, the participants are not told that they are to learn the structure of the stimulus set. Rather they are given another task, such as memorizing a set of letter strings, or simply responding in a specified way to the location of a light on a computer screen. Acquiring knowledge of the structure of the stimulus set happens
simply from exposure to the set, not from any intentional analysis of the stimuli. A common finding in the implicit learning research is that participants who learn under incidental conditions display as much learning, and sometimes more learning than do participants who are instructed to discover the structure (Reber, 1976; Mathews, et al., 1989).

The third characteristic associated with implicitly acquired knowledge, according to Berry and Dienes (1993), is that it gives rise to a phenomenal sense of intuition. Medin and Edelson (1988), Reber (1989), and Chan (1992, as reported in Berry & Dienes, 1993) all report that while participants may not be able to explain why they made the decision they did, they insist the answer they gave just "seems right."

The final characteristic of implicit learning stated by Berry and Dienes (1993) is that implicit learning is robust. Implicitly acquired knowledge is said to be robust across time and in the face of psychological or neurological disorder. In addition, implicit learning is assumed to be robust in dual task situations. Evidence supporting the first factor in Berry and Dienes' robustness assertion is sparse; however, Reber and Allen (1978) reported that participants in an artificial grammar experiment performed above chance on a classification task two years after initial exposure to grammatical strings. There is much more
evidence supporting the second hypothesis. Many studies have shown that while explicit learning suffers, implicit learning is robust in the face of psychological and neurological disorder (Abrams & Reber, 1988; Knowlton, Ramus, & Squire, 1992; Nissen & Bullemer, 1987; Squire & Frambach, 1990).¹

Evidence for the robustness of implicit learning under dual task conditions is sparse, however, and reported results are mixed. This despite the fact that evidence for inhibited explicit learning under dual task conditions is plentiful. Some researchers have demonstrated that while secondary tasks interfere with explicit acquisition of primary task knowledge, they have no effect, or even facilitate implicit acquisition of knowledge necessary to perform the primary task (Hayes & Broadbent, 1988; Cohen, Ivry & Keele, 1990). Other researchers, however, have reported equally disruptive effects of secondary tasks on implicit and explicit learning (Nissen & Bullemer, 1987; Dienes, 1991).

The robustness assumption is important in supporting the existence of implicit learning as a separate process.

¹ In addition to the properties listed by Berry and Dienes (1993), Reber (1993) suggests that implicit learning is not affected by age, developmental level, or IQ. Reber (1993) also argues that implicit learning is a process that shows cross-species commonality, comparing implicit learning studies to conditioning experiments in the animal learning literature. In fact, Reber (1989) believes that implicit learning is a process of considerable antiquity, antedating the capacity for conscious control of thought.
It is important because if learning can be demonstrated in situations which preclude the intentional, limited capacity requirements of the ordinary explicit learning situation, this learning must be accounted for by a separate psychological process. In other words, if the assumption is made that a secondary task would always cause deficits to explicit learning, a process which, by definition, requires limited capacity processing resources, robustness of implicit learning under dual task conditions provides support for the dissociation of the two distinct processes. The series of experiments which are the subject of this investigation was designed to test the general hypothesis that implicit learning is robust under dual task conditions. First, a brief summary of dual task paradigms in general will be presented. Then, research on implicit learning under dual task conditions in the three most widely used implicit learning paradigms will be described, as well as one study which does not fall under one of the major paradigms. Finally, theoretical issues explored in the present investigation will be presented and the research conducted will be described.

**Dual Task Paradigms**

An interesting way of investigating dissociations involving hypothetically different cognitive processes is dual-task research. Since the focus of this research is the effects of dual tasks on implicit learning, this section
will relate, briefly, the assumptions related to dual-task paradigms, and describe the various types of research which employ dual tasks.

In general, a dual-task paradigm is an experimental situation in which the participant is required to perform two different tasks simultaneously. For example, the participant may be instructed to search a visual display for a target element, responding with a key press each time the target is detected, while generating a sequence of random numbers or counting backwards from some specified three digit number by threes. Another example would be an experiment in which the participant is asked to track a visual element on a computer screen using a hand controller while simultaneously responding to messages presented auditorily. Dual-task paradigms are a popular method for studying processing and response limitations of human participants (Gopher, 1990).

One major reason for the popularity of dual-task research is that it enables the experimenter to observe and measure the effects of task variables which would be impossible to isolate in single-task situations. Systematically varying task elements allows the researcher to pit variables against one another, exposing the effects of just one of multiple elements which are part of a complex task. Task elements include features of the stimulus, such as modality, type, quality, and rate of presentation,
features of the required response such as mode and complexity, and features of the participant, such as level of practice, as well as general environmental conditions. Based on the interaction of the two tasks, the researcher can use one task to decompose and define the elements of the other task, as well as infer components of the processing system itself.

Dual-task research has included investigations into the control of attention, measurement of mental workload, accessing attentional allocation through Performance Operating Characteristic (POC) methodology, the nature of processing resources, and the pattern of interference, or lack thereof, between two experimental tasks. The latter line of research is of primary interest in these studies.

As seen in the descriptions to follow, much of the research on robustness under dual task conditions has focused on the dissociation between the effects of dual tasks on implicit versus explicit processing. The findings of this body of research are used as a basis for the investigations performed in this study; however, since the fact that a secondary task does cause deficits in explicit learning is well established, the comparison between implicit and explicit tasks will not be made in these studies. Specifically, the experiments which are the focus of this study will investigate the effects of a concurrently performed secondary task on the learning of letter strings.
created with an artificial grammar and the use of that knowledge to judge grammatical strings not studied. Although the point has been widely debated in recent years (Brooks & Vokey, 1991; Perruchet & Pacteau, 1991), the ability to judge the grammaticality of artificial grammar letter strings, whether they have been seen during study or not, is considered by some researchers to be acquired implicitly (e.g., Reber, 1989; Mathews, et al., 1989).

Implicit Learning under Dual Tasks Conditions

Implicit learning has been investigated primarily within three broad research paradigms: dynamic systems tasks, serial pattern learning, and artificial grammar learning. Even though implicit learning is widely believed to be resistant to interference from a secondary task, dual task studies are relatively rare. Because the present study focuses on this little researched assumption, this review will include related dual task studies in all three paradigms, as well as one study not falling under any of the major paradigms. In order to focus on results which may be expected from the experiments which are the focus of this dissertation, the review of implicit learning in dual task paradigms will be divided into three categories rather than being organized by paradigm. The categories are: studies finding facilitatory effects of a secondary task, studies finding no effect of a secondary task, and studies finding inhibitory effects of a secondary task. Each paradigm will
Facilitation of implicit learning in dual task situations. An influential early study reporting facilitation of learning in a dual task paradigm was published by Hayes and Broadbent (1988) using Berry and Broadbent’s (1988) dynamic systems task. In the dynamic systems paradigm, participants attempted to control the output of a dynamic computer system by adjusting the input. The relationship of input to output was determined by a mathematical formula so that no one input was associated with one particular output. Broadbent and his colleagues were the first to use this paradigm to study unconscious or implicit learning processes.

In the Hayes and Broadbent (1988) study, there were two different conditions defined by two different underlying relationships between input and output. One of the relationships was salient, or easy to detect, and one of the relationships was nonsalient, or very difficult to detect. Hayes and Broadbent claimed that these two different conditions induced two different modes of learning, selective mode (corresponding to explicit learning) and unselective mode (corresponding to implicit learning).

According to Hayes and Broadbent, s-mode (selective mode) learning operates through a mechanism Broadbent called "abstract working memory." Abstract working memory is a
system for conscious abstraction of meaning from any environmental situation and is used for intentional problem solving. Using abstract working memory, participants select variables which seem relevant to the task situation. Then they encode the frequency of occurrence and co-occurrence of these relevant variables in a conscious, strategic manner. The resulting knowledge base is both available to conscious awareness and verbalizable.

Abstract working memory, however, has a limited processing capacity, presenting a problem if the stimulus set is large and its structure is complex. In a large stimulus set with a complex structure, relevant relationships may not be salient enough to be easily detected using abstract working memory. In these situations, Hayes and Broadbent (1988) claim that u-mode learning is best suited to the task because it does not utilize limited capacity abstract working memory.

According to Hayes and Broadbent (1988), the u-mode system is sensitive to frequencies of occurrence and co-occurrence of features in the environment whether participants strategically encode these features or not. Thus, the structure of the stimulus environment is encoded without conscious abstraction of the structure. The resulting knowledge from this type of learning is largely unavailable to conscious awareness and not verbalizable; however, this knowledge does affect behavior.
Hayes and Broadbent (1988) reasoned that if s-mode requires abstract working memory, a limited capacity resource, and u-mode does not, a secondary task which occupies some part of the limited processing capacity would interfere with s-mode learning, but not with u-mode. The secondary task used by Hayes and Broadbent was random number generating. The secondary task did interfere with learning in the condition with the salient relationships, as predicted; however, the surprising result was that a slight facilitatory effect was found in the condition with the nonsalient relationships. Hayes and Broadbent speculated that people in the nonsalient task may have been trying to use s-mode which was ineffective and was interfering with u-mode learning. Reber (1990) has also proposed that strategic problem solving (i.e., looking for the rules of a complex stimulus set) will not work if the structure is not salient enough to be detected consciously.

According to Hayes and Broadbent (1988), the secondary task prevented the participants from attempting to consciously solve the problem, thus freeing u-mode to operate unimpeded. Based on this research, Hayes and Broadbent claim that if the task is one which is best suited to s-mode learning, a secondary task will hurt performance; however, if the task is one which is best suited to u-mode learning, a secondary task should facilitate performance, or at least not hurt it. This claim seems to be the catalyst
which has prompted most other investigations of the robustness issue.

The second set of experiments to be described in this section, Cochran and McDonald (1992), is different from other experiments reported in this paper because it involves a special case of implicit learning; that of language acquisition. Language learning in a natural setting has many characteristics similar to laboratory tasks used to investigate implicit learning. For example, in natural language acquisition, the stimulus set is rule governed, the rules are not learned through intentional processes, and the knowledge base is largely unconscious. But, according to Chomsky (1986) humans are endowed with a genetic predisposition to learn the structure of language. To date, there is no convincing evidence to contradict Chomsky's claim. Thus, even though language is learned implicitly, language learning must be considered somewhat different from other types of implicit learning. These experiments will be described, however, because they do report facilitated learning in a dual task situation which is similar to that of the other experiments described herein.

In the study phase of these experiments, Cochran and McDonald (1992) had participants read the printed version of an English sentence. Then participants viewed a version of the same sentence expressed in Pidgin Signed English, a manual language system which incorporates the morphology of
American Sign Language and the word order of English. Each of the sentences was constructed using a verb which requires agreement, a derivation in which the direction of the sign is changed to agree with the arguments of the verb. This derivation produces verb movement which points out who performed the action of the verb and who the recipient or object of the action was. For example, in the sentence, "I give you the book," the movement of the verb would start at the signer (I) and move toward the person being addressed (you.) If the sentence were changed to "You give me the book," the movement of the verb would start in the direction of the addressee (You) and move toward the signer (me.)

Participants received four of eight training sentences with movement going from the signer to the addressee and four with movement going in the opposite direction. Thus, across all eight study verbs, participants saw verbs moving in both directions which were correctly instantiated in each sentence; however, they did not see the two different movement directions using one single verb. In addition to the primary task of learning to sign the sentences, half the participants were required to perform a demanding tone counting task.

After a short retention interval designed to prevent rehearsal, each participant was asked to sign sixteen sentences. In addition to signing each of the eight verbs in the sentence context in which they were studied,
participants also had to sign the verbs in sentences which required reversing the direction of movement from the studied version, in order for the verb to agree with its arguments.

In this experiment, participants in the dual-task condition were more likely to adjust the direction of the verb in new sentences (not studied) to agree with the arguments of the verb, than were participants not required to perform a secondary task during the study phase. Participants in the single task condition performed at chance on verbs in the new sentence context. Results were explained in terms of Newport's (1988, 1990) "Less is More" hypothesis. According to Newport, children's limited cognitive processing capacity forces them to encode language in small pieces, perhaps morphemes, giving them an advantage in mastering the internal structure of the complex system. Adults' superior ability to process information, on the other hand, actually hurts language learning by enabling them to encode holistic units (i.e., whole words), never analyzing the structure.

According to Cochran and McDonald (1992), participants in the single task condition learned and then produced the verbs in a holistic manner. Participants learning under dual task conditions which limited their capacity to process the verbs, may have been forced to learn in a componential, child-like way which, according to Newport (1990) may be
better for mastering complex systems. Similar results were also found in a second and third experiment.

In summary, these experiments provide evidence for facilitated learning of the structure of a complex stimulus set by having participants perform a concurrent cognitive task.

To date, these are the only two studies reporting facilitated learning under dual task conditions; however, the test of robustness could be said to be a lack of interference, rather than facilitation of implicit learning under dual task conditions. Studies reporting a lack of interference with implicit learning under dual task conditions are reported in the next section.

Implicit learning without inhibition under dual task conditions. Using an adaptation of an arcade-type computer game called "Save the Whale," Porter (1991) performed an experiment which he compared to Hayes and Broadbent (1988). Berry and Dienes (1993) labeled Porter's study a conceptual replication of Hayes and Broadbent, but there are differences between the two studies which limit the extent to which these two studies are comparable.

Porter claims that his two tasks are analogous to Broadbent's salient and nonsalient tasks; however, there are several notable differences between Porter's tasks and those of Hayes and Broadbent. The first difference is that Hayes and Broadbent's participants typed an input which was
followed by a system output, to which participants responded by typing another input. Thus, even though there is a dynamic relationship between input and output, the task could be described as having discrete trials. Porter's participants, on the other hand, continuously interacted with the system, controlling movement of several object on the computer display. Another difference is that while Hayes and Broadbent's participants only took part in one version of the task (either salient or nonsalient), Porter's participants were required to perform both tasks simultaneously. Nevertheless, based on Hayes and Broadbent's results, Porter predicted that a secondary task would interfere with the explicit task, but not with the implicit task.

In the first experiment, Porter's secondary task was subvocal rehearsal of strings of letters, and there were three different levels of difficulty. The prediction was that increasing memory load would increase interference with the explicit task but not with the implicit task. Porter indeed found this.

In his second experiment, Porter changed his secondary task. The first level was a pure control with no secondary task. In the second level, participants were required to repeat aloud a fixed sequence of numbers while simultaneously performing the experimental tasks. In the third level participants were required to generate a random
sequence of numbers while simultaneously performing the experimental tasks. He also added a condition in which the secondary task was repeating the words "left," "right," "up," and "down," speculating that these direction words might cause semantic interference with controlling movement of the whale; however, there was no difference between this condition and the fixed number condition. Again, Porter reported that the degree of processing demand as defined by the three levels of the secondary task had no differential effect on the implicit task, but significantly interfered with performance of the explicit task with interference increasing as processing demands increased. Thus, while Porter did not find facilitated implicit learning under dual task conditions, he did report that the implicit task was robust under dual task conditions whereas the explicit task was not.

To summarize Porter's findings, several types of concurrent tasks caused a decline in performance for the explicit task, with increased memory load and increased processing demands causing greater disruption. On the implicit task, however, dual task conditions had little effect, regardless of the type or level of difficulty of the secondary task. Thus, on the surface, Porter's data seem to support the contention that implicit learning is robust in dual task situations. Of course, the alternative
explanation that Porter's implicit task is simply easier and thus less susceptible to disruption must also be considered.

Another experiment finding a dissociation between two different learning situations under dual task conditions was conducted by Cohen, Ivry, and Keele (1990) using the serial pattern reaction time task introduced by Nissen and Bullemer (1987). In this paradigm, a light appears in one of three or four locations on a computer screen, and participants are instructed to respond by pressing a key which corresponds to the location of the light. The sequence of locations can either be random, or form a repeating pattern. Results show a significant decrease in reaction time on repeated sequences relative to random sequences.

In their first experiment, Cohen et al. required participants to perform a simultaneous tone counting task as they responded to the location of a target which appeared in either a simple five-element repeating sequence, or a random series of locations. Learning was measured by the degree to which reaction times for participants in the repeating sequence condition became faster than the reaction times for participants in the random condition. In addition to the primary task, participants were required to simultaneously perform a tone counting task which had two levels of difficulty. Participants in the easy tone counting condition were required to count from 25 to 50 tones per 100-trial block, while participants in the difficult tone
counting task were given 50 to 75 tones per 100-trial block. During the first phase of the experiment participants performed two 30-trial blocks of practice trials, then ten 100-trial blocks with a brief rest between blocks.

In the second phase of the experiment, there was no secondary tone counting task. The primary task was switched from simply responding to the current location of the target to predicting the next location of the sequence when a stimulus appeared on the screen. During this phase, both groups of participants saw the structured sequence. This second phase was intended to assess participants' awareness of the sequence. Experimenters reasoned that if the secondary task was sufficiently demanding to prevent awareness of the sequence, participants should not perform very well on the prediction task, even though decreased reaction times may have indicated that they learned the sequence during the first phase.

Cohen et al. (1990) found a significant difference in the reaction times for the structured sequence versus the random location condition, a difference which increased with trial block; however, they found that secondary task difficulty had no effect on sequence learning. Participants in both the easy and the difficult conditions were able to learn the sequential pattern as evidenced by reduced reaction times. On the prediction task, participants who had been in the structured sequence condition in the first
phase performed no better than participants who had been in the random condition; thus, there was no evidence that participants were consciously aware of the sequence.

In two subsequent experiments, Cohen et al. (1990) compared performance on three different types of sequences under dual task conditions, as well as under single task conditions. The three task types were (1) a simple linear structure just like the sequences used in the first two experiments, (2) a complex sequence in which each element occurred twice, but after a different position each time, and (3) a hybrid sequence, which contained two unique positions and two repeating positions. There was no random condition in this experiment; however, after 10 blocks of structured sequence trials, all participants were given two blocks of random trials, then two more blocks of structured sequence trials. The measure of learning was the difference in reaction time between the random blocks and the structured sequence blocks. Again, the secondary task was tone counting. Cohen et al. (1990) reported that participants in the linear and hybrid sequence groups showed evidence of learning the sequence under dual task conditions; however, participants in the complex sequence condition did not. Participants in the single task conditions had no trouble learning all three types of sequences. Thus, Cohen et al. conclude that while learning of a simple sequence with unique associations can occur.
under dual task conditions, learning of a complex sequence with hierarchical relationships may not occur.

The results of this experiment, while demonstrating a dissociation between tasks using two different types of stimuli under dual task conditions, seem to be opposite from results reported by Hayes and Broadbent (1988) and Porter (1991). In the two previously described experiments, the addition of a secondary task caused disruption of the tasks in which the relationships were simple and salient, tasks hypothesized to be best suited to explicit learning. The addition of the secondary task, however, had no effect, or even slightly facilitated tasks in which the relationships were complex and nonsalient, tasks hypothesized to be best suited to implicit learning. Cohen et al. (1990), on the other hand, report that the task with the complex relationship was disrupted by the secondary task; whereas, the two tasks with simpler relationships were not disturbed. It is clear that the relationship of dual task situations to implicit learning is not a simple one.

In another investigation of serial learning under dual tasks, Frensch, Buchner, and Lin (1994) using the same primary and secondary tasks as Cohen et al. (1990) replicated the finding that participants can learn the different types of sequences equally well under single task conditions; however, they report a different finding under dual task conditions. According to Frensch et al.,
participants demonstrated the ability to learn both simple and complex associations under dual tasks conditions, although simple associations were learned better.

In an additional experiment, Frensch et al. replicated their findings in the previous experiment and also explored the relationship between the primary task and the onset time of the secondary task stimulus (a tone). Frensch et al. reported that varying the onset of the secondary task stimulus affected learning of both simple and complex associations in a similar manner. In general, they found that participants who saw the target and heard the tone simultaneously learned the sequence better than participants who heard the tone 300 ms after the appearance of the target, who in turn learned better than participants who heard the tone 700 ms after the target appeared. This pattern occurred both for participants who learned simple sequences and for those who learned complex sequences. The researchers explain this result in terms of a systematic effect of participants' scheduling of the two tasks (primary versus secondary). One important result of this study is the demonstration that varying a simple feature of the secondary task, such as onset time of the stimulus, can have a dramatic effect on learning. Given this result, it is not unreasonable to assume that different types of secondary tasks may differentially affect implicit learning in any paradigm.
Of the experiments described thus far, two have reported facilitated implicit learning under dual task conditions, and three have reported that implicit learning may proceed without decrement under dual task conditions. The remaining four experiments described in this section report inhibitory effects of a secondary task on implicit learning. Thus, these four studies argue against the robustness of implicit learning in dual task situations.

Inhibition of implicit learning by dual tasks. The first to report that dual tasks inhibited learning of a task which is generally considered to be learned implicitly, were Nissen and Bullemer (1987). Using the serial reaction time paradigm described above, Nissen and Bullemer had participants perform eight 100-trial blocks in which the location of the target was either a structured sequence or determined randomly. Structured sequences were comparable to Cohen et al.'s complex sequence in which every location occurred twice followed each time by a different location. Note that this type of stimulus is a complex sequence because there are not unique relationships between pairs of locations. Results indicated that there was a significant difference in reaction times between the two groups by the second block of trials and that reaction time steadily decreased across trial blocks for participants in the structured sequence condition, while participants in the
random condition showed little decrease in reaction time across blocks.

In the second experiment, Nissen and Bullemer (1987) added a secondary tone-counting task, similar to the one described previously. Participants were required to practice the serial reaction time task while concurrently counting tones, and were then required to perform a prediction task similar to the one described in Cohen et al. (1990). Nissen and Bullemer reported that single task conditions displayed significantly more learning than did dual task conditions, but that there was no difference for the dual structured sequence task versus the dual random task, indicating that participants under dual task conditions did not learn the sequence. In the prediction task also, Nissen and Bullemer reported that participants under dual task conditions evidenced no more knowledge about the structure of the sequence than did participants in the random condition. Participants in the single task condition, however, demonstrated considerable knowledge of the sequence.

In Experiment 3, Nissen and Bullemer (1987) had participants complete four blocks of either dual task structured sequence trials, or dual task random trials, then switch to single task trials. They reported that there was no evidence of prior learning of the sequence under dual task conditions when participants were switched to single
task conditions. Thus, Nissen and Bullemer's results fail to support the robustness of implicit learning under dual task conditions.

Several investigations of sequence learning under dual task conditions have also been conducted by Stadler (1993, 1995). In one study, Stadler (1993) demonstrated that interrupting the organization of a serially presented repeating pattern by randomly inserting pauses into the sequence significantly reduced learning, while inserting pauses in a consistent way actually produced a slight, but significant facilitation. More recently, Stadler (1995) demonstrated that interrupting organized encoding by forcing secondary task processing at random points also interfered with learning the stimulus set. Participants who were required to count tones randomly inserted into the sequence performed significantly worse than control participants who did not count tones. Furthermore, Stadler showed that a different type of secondary task which only created a memory load interfered significantly less than the task which interrupted organization of the stimulus set. Stadler's organizational explanation is quite different from previous accounts of the effect of a secondary task on implicit learning. This interesting hypothesis will be discussed in more detail later.

Dienes, Broadbent, and Berry (1991) examined the robustness of implicit learning under dual task conditions.
using the artificial grammar learning paradigm. A typical grammar learning experiment consists of a learning phase in which participants are given a subset of letter strings generated by a particular finite state grammar such as the one depicted in Figure 1, with instructions to either memorize, or simply observe the strings. Participants are typically not informed of the regularity in the set of letter strings, nor of the fact that a second testing phase is to follow. After a predetermined amount of exposure, participants are informed that the strings were created according to a set of rules (the grammar), and are asked to perform a forced choice test in which they must discriminate grammatical from ungrammatical strings. Test items are usually composed of old grammatical items (grammatical strings which were seen during learning), new grammatical items (grammatical strings which were not seen during learning), and ungrammatical items which may be constructed by either changing some letters of grammatical items to violate the rules of the grammar, or by randomly arranging the letters of the stimulus set.3

2 The amount of exposure during the learning phase is determined by the experimenter, and may differ from one experiment to the next. Some researchers require subjects to learn the study strings to some specified criterion while others simply expose all subjects to an equal number of repetitions of the study strings.

3 Note that even though researchers in this paradigm refer to "the grammar" as if they expect participants to induce the same set of rules that the researcher had in mind when the strings were created, they are aware of the fact
that several "weakly equivalent grammars" could produce the same set of strings. Usually what is actually meant is that participants are sensitive to the regularity in the set of strings, and "behave" as if they have learned the researchers set of rules.
In the second of two experiments, Dienes, Broadbent, and Berry (1991) instructed participants to either memorize a subset of grammatical letter strings from a particular artificial grammar, or to search for the rules governing construction of the letter strings. In addition, some participants were required to perform a random number generating task which they were allowed to practice for five minutes before beginning the primary task.

After the training phase of the experiment, participants were tested in three ways to assess their knowledge of the grammar. First, they were given a standard string discrimination test. Then they were given a fragment of a string and asked whether particular letters could follow the fragment in a grammatical string. Finally, they were asked to report how they judged the grammaticality of strings (free report).

Dienes et al. (1991) report that on both the discrimination task and the task which probed for knowledge of letter order, participants in the dual task conditions performed significantly worse than participants in the single task conditions. There was no effect of instruction type (memory versus rule discovery) and instruction type did not interact with dual versus single task. In addition Dienes et al. report that using the participants' stated rules on the free report test to predict classification performance indicated that participants were better at
classification than at explicitly stating the rules; however, even in the relatively insensitive free report test, dual task conditions were significantly worse than single task conditions.

Dienes, Broadbent, and Berry (1991), speculated that perhaps the grammar learning tasks (both the memory and rule discovery types) rely on some limited capacity resource, such as Baddeley's (1987) phonological loop. In other words, encoding grammar strings requires verbal rehearsal. If this is true, artificial grammar learning of any kind is likely to be disrupted by a concurrent task which also requires verbal rehearsal because it interferes with the acoustic or articulatory encoding of the grammar strings. They also express a belief that grammar learning may be a much more explicitly based task than either sequence learning (e.g., Nissen & Bullemer, 1987) or dynamic systems control (e.g., Hayes & Broadbent, 1988). Evidence for this assumption, according to Dienes, Broadbent, and Berry, is that when participants are probed in the right way (i.e., questioned about the grammaticality of specific letters in specific locations) they evidence knowledge which equals their ability to classify strings. They further speculate that this explicit component may be another reason that the secondary task interferes with grammar learning.

As seen in the research described above, the Hayes and Broadbent's (1988) finding of facilitation of implicit
learning under dual task conditions has not been conceptually replicated. A direct replication of Hayes and Broadbent has also failed to find this effect. Green and Shanks (1993) reported the results of five experiments in which they attempted replication of Hayes and Broadbent, without success. Green and Shanks reported that, contrary to the findings of Hayes and Broadbent, addition of a concurrent random number generating task caused significantly greater deficits in performance of the nonsalient (implicit) version of the task than of the salient (explicit) version.

In their first attempt to replicate Hayes and Broadbent, Green and Shanks used a different method for equating initial learning in the two groups, which they admit could be responsible for the different findings. Thus, in the next two experiments, they attempted a direct replication of Hayes and Broadbent. In these two experiments, Green and Shanks again report that the secondary task disrupted performance significantly more for participants in the implicit condition than in the explicit condition.

With regard to the question of robustness of implicit learning under dual task conditions, Green and Shanks' results indicate that the secondary task interrupted performance on both tasks, but the effect on the nonsalient version (implicit) of the task was greater because,
according to Green and Shanks, the task is simply more difficult. This failure to replicate calls into question not only Hayes and Broadbent's results, but the whole robustness issue. If robustness cannot be demonstrated, does that indicate that implicit learning does not exist as a separate process? If two learning processes do exist, interference on both processes must be explained in terms other than Hayes and Broadbent's capacity model.

Computer simulation of sequence learning under dual task conditions. Recently, Cleeremans (1993) reported several studies of implicit learning using a sequence learning task similar to Nissen and Bullemer's (1987). Although he did not include a dual task condition in his empirical experiments, he did simulate the effects of dual task conditions on sequence learning using an SRN (simple recurrent network) computer model. He found that this computer model could capture the effects of dual tasks on sequence learning reported by Cohen et al. (1990) by simply adding normally distributed random noise to the input. Cleeremans (1993) asserts that there is no need to postulate elaborate mechanisms to account for the effects of dual tasks on sequence learning. The secondary task may simply cause some of the stimuli to be encoded incorrectly. Cleeremans explains that the effect of this noisy input is to disrupt the model's long-term representation of the structure of the sequence. The model can still develop
short term representations and perform the task with simple sequences, but it cannot build up a representation of a hierarchical structure as it normally does. Cleeremans (1993) does not specifically address other implicit learning paradigms with respect to dual task situations, thus, whether this explanation can account for the overall findings in the implicit learning literature remains to be seen.

Summary of Research on the Robustness Issue

To summarize research findings thus far, even though there is some evidence to support the claim that implicit learning is robust under dual task conditions, (Hayes & Broadbent, 1988; Cohen, et al., 1990; Porter, 1991; Frensch, et al., 1994; Stadler, 1995), there is also evidence that adding a concurrent task can inhibit learning in these tasks (Nissen & Bullemer, 1987; Cohen, et al., 1990; Dienes, et al, 1991; Green & Shanks, 1993; Stadler, 1995). In fact, there is sufficient evidence, according to some, to call into question the existence of a separate implicit learning process (Green & Shanks, 1993).

Assuming that a separate implicit learning process does exist, a logical conclusion, based on this contradictory evidence, is that not all implicit learning tasks are created equal. In other words, processing demands may be vastly different from one task to another causing any concurrent secondary task to interact differently with
different primary learning tasks, but this does not necessarily refute the claim that all paradigms involve processes which are implicit in nature.

But, while a difference in processing demands for various implicit learning tasks can explain differences found across paradigms, it does not explain contradictory findings reported within each paradigm (e.g., Nissen & Bullemer, 1987 versus Cohen et al., 1990) Careful review of the literature reveals that researchers have used several different secondary tasks or variations of a task within each paradigm, leading to the conclusion that the secondary tasks must also differ in processing demands. Simply adding a secondary task does not always have the same result. In fact, it is not unreasonable to assume that a different result could occur with each unique combination of primary and secondary task.

A different explanation of the effects of dual task conditions on implicit learning. Recently, the proposal that different types of secondary tasks may affect implicit learning in different ways has been explored within the serial reaction time paradigm by Stadler (1995). According to Stadler, a secondary task could interfere with learning in two different ways. First, the secondary task may occupy some portion of a limited resource leaving too little of the resource to process the primary task resulting in failure to encode the stimulus set. This type of interference seems to
be the focus of many theorists. In fact, both theorists who claim that implicit learning should not be affected by secondary tasks and those who claim that it should have a tendency to explain effects in terms of some limited capacity resource and whether the two tasks tap this same resource pool.

A second type of explanation for secondary task interference, however, does not depend on the claim that tasks compete for a limited quantity resource (Stadler, 1995). This explanation is based on the assumption that whether stimuli are encoded implicitly or not, they must be organized in some consistent way. As recounted above, Stadler (1993, 1995) demonstrated that interrupting the organization of a serially presented repeating pattern by randomly inserting pauses into the sequence can significantly reduce learning and that interrupting organized encoding by forcing secondary task processing at random points will also interfere with learning the stimulus set. Stadler also showed that a different type of secondary task which only created a memory load interfered significantly less than the task which interrupted organization of the stimulus set.

Stadler’s (1995) organizational explanation not only accounts for his own data, but it is also relevant to the findings of other researchers in this paradigm. For example, in the experiment by Frensch et al. (1994),
presenting the tone after the target may interrupt
organization of the sequence, while presenting target and
the tone simultaneously may not. Nissen and Bullemer's
(1987) and Cohen et al.'s (1990) data can also be explained
assuming that complex sequences require more organization,
rather than simply more attention. In other words, longer
sequences may have to be encoded for the relevant
associations to be learned in complex structures; therefore,
a secondary task is more likely to interrupt organization in
complex sequences than in simple ones.

An important research question is whether the logic
proposed by Stadler (1995) may be applied to other implicit
learning tasks. For example, some theorists have proposed
that learning legal bigrams and trigrams is the way
participants initially organize encoding of artificial
grammar strings (Servin-Schreiber & Anderson, 1990). Would
a secondary task that simply occupied some portion of
processing capacity have the same disruptive effect as one
which interrupted the association of adjacent letters? An
alternative explanation for performance in the Dienes, et
al. (1991) dual task grammar learning experiment is that the
demanding secondary task (random number generating)
interrupted the encoding of letter associations. There is
also the possibility, however, that the authors were correct
in assuming that competition for a limited capacity resource
could account for the results. This question could be
addressed by comparing performance on a grammar learning task under two different secondary task conditions, one which simply occupies processing capacity, and one which interrupts encoding of pairs of letters.

An additional issue is whether those rare examples of facilitation of implicit learning in secondary task situations may be caused by a secondary task which has the effect of aiding organization of the stimulus set by interrupting in certain places. Servin-Schreiber and Anderson (1990) demonstrated that "chunking" grammar strings at study can either facilitate or inhibit string discrimination performance relative to a whole string control group, depending on the nature of the chunking. Strings which were chunked to emphasize the structure of the grammar, or the regularity of the stimulus set facilitated learning, while strings which were chunked in a way which concealed the structure or regularity inhibited learning. Based on Servin-Schreiber and Anderson's results, a secondary task which happened to have an organizing effect, rather than disrupting the organization, could in fact facilitate learning.

The present research investigation was designed to investigate the hypothesis that different types of secondary tasks may interfere with implicit learning of the regularity in a set of artificial grammar strings to differing degrees depending on the specific demands of the secondary task and
how that task interacts with the primary task of learning the letter strings. In addition, the question of whether a secondary task may facilitate learning by organizing encoding of the stimulus set in a beneficial way was addressed.

Finally, to examine whether deficits in learning under dual task conditions are caused by competition for a phonologically based resource (Dienes et al., 1991), the availability of verbal labels for the secondary task stimuli will be manipulated and its effect on learning regularities of the stimulus set will be investigated.
Chapter 2 - Experiment 1 and Experiment 2

Effects of Different Secondary Tasks on Learning Artificial Grammar Strings

The purpose of the first two experiments in this study was to test the hypothesis that a secondary task which disrupts organized encoding of letter strings created with an artificial grammar has a more detrimental effect on learning than does a secondary task which simply occupies some portion of a limited capacity processing resource. Organized encoding was operationally defined as learning associations between adjacent letters in grammatical letter strings. This definition is based on previous research reporting that participants in grammar learning experiments evidence learning of bigrams (legal pairs of letters) and trigrams (legal triplets) which can account for their performance at test (Dulany, Carlson, & Dewey, 1984; Servin-Schreiber & Anderson, 1990; Perruchet & Pacteau, 1990). Learning in the present series of experiments was assessed by the ability of participants to discriminate grammatical from ungrammatical strings in a grammatical judgment test which followed training.

Experiment 1

Specifically addressed in this first experiment was the question of whether simply occupying some portion of

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4 Experiment 1 was actually run as Pilot Experiment 1 and Pilot Experiment 2a. Methodology for the two experiments was exactly the same, and all four conditions were run simultaneously in each experiment. Both
participants' limited processing capacity produces the same degree of learning inhibition as disrupting organized encoding of strings. In this experiment two different types of secondary tasks were paired with a grammar learning task. In addition to the two dual task conditions, two control conditions in which participants were not required to perform secondary tasks were included for comparison.

The first condition in Experiment 1 was the pure, single task control that established a baseline to which the other conditions could be compared. The second condition was the dual task condition designed to disrupt encoding of adjacent pairs of letters (learning of bigrams). The third condition was a matched control in which participants were exposed to the secondary task stimulus but were instructed to ignore the stimulus. The fourth condition was a dual task designed to simply occupy some portion of processing capacity while the participants performed the primary task. Based on research in the serial pattern learning literature (Stadler, 1995), the disruptive dual task was predicted to inhibit learning to a greater degree than was the memory load dual task. If Dienes et al.‘s (1991) hypothesis that the dual task prevents artificial grammar learning because it occupies a limited capacity resource necessary for experiments used the same subject population, and both were run within a 12 week period of time. Thus, data were combined and analyzed as one experiment in order to increase statistical power.
encoding the letter strings is correct, the two different types of tasks should inhibit learning to an equal extent.

In addition to the primary hypotheses investigated in this experiment, one addition question was tested. In the grammar learning literature there has been a history of controversy over the type of knowledge gained in artificial grammar learning experiments. Reber (1969, 1989) proposes that participants gain abstract knowledge of the structure of the stimulus set as evidenced by the ability to detect grammatical items not previously seen. Several other theorists agree that exemplar knowledge cannot account for participants' performance (Mathews et al., 1989; Squire & Frambach, 1990). Some theorists argue, however, that participants in grammar learning experiments gain knowledge of specific study exemplars and that performance on the grammaticality judgement test can be accounted for by such knowledge (Brooks & Vokey, 1991; Dulany et al., 1984). Thus, knowledge in grammar learning experiments has traditionally been assessed by having participants judge two types of grammatical test items, old grammatical items seen at study and new grammatical items not previously seen.

The current experiments were not designed to discriminate whether participants learned abstract knowledge of grammatical strings in general or only specific knowledge of studied exemplars. This question was not the focus of this experiment. However, old grammatical as well as new
grammatical items were included on the test to determine whether participants could apply what they learned at study to items not previously seen. This is not to imply that the ability to detect new grammatical items indicates abstract knowledge of the grammar, simply that participants are able to apply what they learned to items beyond those that they studied.

Based on previous research (Mathews et al., 1989), participants are expected to be able to detect grammatical items they have not seen; however, they should be even more accurate at judging the grammaticality of items they saw during study.

Method

Participants. One hundred and fifty-seven undergraduate students volunteered to participate in this experiment in return for extra credit points in introductory level Psychology courses at Louisiana State University. The population of such courses is composed of approximately equal proportions of males and females, and is representative of a wide range of college majors, as well as racial and ethnic backgrounds. Students were allowed to participate in only one experiment in this series.

Before beginning the experiment, participants were informed of their rights as volunteers, including the right to confidentiality, and were required to sign an informed consent agreement (see Appendix for an example). After the
experiment, participants were debriefed as to the nature and purpose of the experiment and were immediately issued vouchers for their extra credit points.

Participants were run in groups ranging in number from 5 to 20 and were randomly assigned to each of the four conditions by order of entry into the laboratory. There were 41 participants in the pure control condition, 39 in the disruptive dual task condition, 42 participants in the matched control condition, and 35 in the memory load dual task condition.

Apparatus and stimuli. The experiment was run on IBM personal computers. Participants were seated approximately 50 cm from the computer monitor and typed responses using the computer keyboard.

Stimuli for the primary task were composed of sequences of capital letters of the set; S, T, V, P, X. Sequences of letters were created with the finite state grammar shown in Figure 1 on page 28 which was used by Reber (1967, 1969) in several experiments. This particular grammar generates 43 letter strings if string length is limited to a range of 3 to 7 letters. Forty of these letter strings were selected at random for the study set, then randomly divided into two separate study lists with the stipulation that the two lists were equated on string length (see Appendix for complete list of study strings). Half the participants received one
set of 20 strings at study and half the participants received the other 20 strings.

Rather than presenting letter strings in the typical holistic mode, letters were displayed one at a time in their respective positions with underlined blank spaces acting as place holders for the other letters of that particular string. This manipulation was used to ensure that disruption of letter associations would be possible. The belief was that presenting strings holistically, as is usually done in grammar learning, would make it possible for participants to simply ignore secondary task stimuli and encode letter associations as they would normally do in a grammar learning experiment. Stimuli for the secondary task were arrows pointing either up (↑) or down (↓). Arrows appeared between the letter spaces.

At test, 80 strings of letters were presented in holistic form. Forty of the test strings conformed to the rules of the grammar and 40 were ungrammatical. Of the 40 grammatical letter strings, 20 were strings seen at study and 20 were novel strings. Ungrammatical strings were created by randomly changing from one to three letters of a grammatical string to a letter which could not legally occur in that particular position.

Responses during the training phase of the experiment were made using five specially labeled keys across the top of the keyboard; 5, 6, 7, 8, 9. Keys were labeled with the
letters; S, T, V, P, X respectively. Participants responded to arrows using the arrow key pad at the right of the keyboard. The computer responded only when participants used designated response keys. During the test phase, responses were made by typing either G for good letter strings or B for bad ones using the G and B keys on the computer keyboard.

**Design and procedure.** Upon entering the laboratory, participants were seated at a computer and given a set of written instructions. After reading the instructions, they were asked by the experimenter if they understood the task, and clarifications were made if they indicated that they did not understand. The study phase of the experiment was presented as a short-term memory task. Participants saw the stimuli then recalled them immediately. No mention was made of the regularity in the stimulus set nor of the fact that a discrimination test would follow. Thus, there was no reason for participants to explicitly notice the similarity across letter strings since each trial apparently had nothing to do with previous trials.

There were four different study conditions (see Table 1). The first condition was the single task pure control condition. In this condition participants simply saw the letters appear one at a time in the sequence of spaces, then responded by entering the sequence of letters using the designated keys. The computer prompted participants to type
the first letter by displaying a blinking cursor above the first blank. If a correct response was made, the letter appeared briefly in the blank; however, if an incorrect response was made, a pound symbol (#) appeared in the blank. Then the blinking cursor moved to the next blank, prompting a response. After entering the entire sequence, feedback was given in the form of percent of letters entered correctly. Then the computer prompted the participant to press the enter key to reveal the next study string.

Participants in all study conditions saw the set of 20 study strings three times during the first phase of the experiment making a total of 60 training trials.

The second study condition was the disruptive dual task condition. During study, these participants saw sequences of letters similar to the ones seen in the pure control condition, except that random sequences of arrows were inserted between the letters. In other words, participants saw a string of blanks on the computer screen. A letter would briefly appear in the first blank, then an arrow would briefly appear between the first two blanks. Then a letter would briefly appear in the second blank and so on. After the entire sequence had been presented, the participant was prompted to enter the letter sequence using the designated keys, then to enter the arrow sequence using the arrow key pad. Feedback was then given in the form of percent correct for the letter sequence and percent correct for the arrow
sequence. Then the computer prompted the participant to press enter to reveal the next study string.

The third study condition was the matched control condition. Participants in this condition saw the stimuli presented in the same manner as participants in the disruptive dual task; however, they were instructed to ignore arrows and were not required to type arrow strings. They responded by typing letter sequences only and were given feedback in the form of percent correct for letter sequences.

<table>
<thead>
<tr>
<th>Table 1. Illustration of study task methodology used in Experiment 1</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Study Condition</strong></td>
</tr>
<tr>
<td>Example Strings</td>
</tr>
<tr>
<td>Pure Control ▶ T X X T V V</td>
</tr>
<tr>
<td>Disrupt Task ▶ T X X T V V</td>
</tr>
<tr>
<td>Matched Control ▶ T X X T V V</td>
</tr>
<tr>
<td>Memory Load ▶ ↑ ↑ ↑ ↑ ↑ T X X T V V</td>
</tr>
<tr>
<td><strong>Expected Responses</strong></td>
</tr>
<tr>
<td>Control Conditions Letters ▶ T X X T V V</td>
</tr>
<tr>
<td>Feedback Letters ▶ % Correct</td>
</tr>
<tr>
<td>Dual Tasks Letters ▶ T X X T V V</td>
</tr>
<tr>
<td>Arrows ▶ ↑ ↑ ↑ ↑</td>
</tr>
<tr>
<td>Feedback Letters ▶ % Correct</td>
</tr>
<tr>
<td>Arrows ▶ % Correct Arrows</td>
</tr>
</tbody>
</table>

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The fourth study condition was the memory load dual task condition. In this condition participants saw the arrow sequence first and were required to hold it in memory while the letter sequence was being presented. After presentation of the letter sequence, participants were prompted to enter the letter sequence and then the arrow sequence. They then received feedback in the form of percent correct on the letter sequence and percent correct on the arrow sequence and were prompted to press enter to reveal the next study string.

After 60 study trials consisting of three passes through the set of 20 study strings, participants in all four conditions were informed that study sequences were created using a set of rules called a grammar. They were told that they would now be shown a series of letter strings, some of which conformed to the grammar and some which did not. They were instructed to respond as each string appeared on the computer screen by typing a G if they thought the string was good, or grammatical and a B if they thought the string was bad, or ungrammatical. After the test, participants filled out a post-experimental questionnaire which asked them to describe how they performed the primary and secondary tasks. Information contained on the questionnaires proved to be of limited value, however, because questions were broad and open ended
allowing participants to respond in vague terms which were
difficult to interpret.

Results and Discussion

Examination of the data revealed that participants
seemed strongly biased to respond positively, that is, to
call all test strings grammatical. This bias resulted in
significantly more correct responses for grammatical test
items ($M = .70$) than for ungrammatical test items ($M = .46$),
$F(1, 156) = 178.61, p < .0001$. To correct for response bias,
scores were transformed into d-prime ($d'$). A one-way
between-subjects ANOVA on $d'$ revealed a significant effect
of study condition, $F(3, 153) = 6.92, p < .0005$. As shown in
Table 2, performance in the pure single task control
condition was best, followed closely by the matched control
condition, with the memory load dual task and the disruptive
dual task coming in third and fourth, respectively.

According to the results of a Newman-Keuls multiple
comparison procedure on these means, the disruptive dual
task condition was significantly worse than the pure
control, $Q(3, 153) = 5.47, p < .05$, as well as significantly
worse than the matched control, $Q(3, 153) = 5.37, p < .05$,
indicating that the disruptive secondary task did cause a

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$d'$ is computed by converting the proportion of
hits (grammatical items judged grammatical) and the
proportion of false alarms (ungrammatical items judged
grammatical) to standard scores ($Z$ scores) then subtracting
the latter from the former. For a complete explanation of
correction of response bias using $d'$ see Macmillan and
significant decrement in learning, as predicted. No other comparisons were found to be significant.

Table 2. Proportion correct grammaticality judgments for old and new test items and d-prime by study task condition for Experiment 1.

<table>
<thead>
<tr>
<th>Study Condition</th>
<th>n</th>
<th>d'</th>
<th>Old Items</th>
<th>New Items</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pure Control</td>
<td>41</td>
<td>.69 (.58)</td>
<td>.75 (.16)</td>
<td>.74 (.15)</td>
</tr>
<tr>
<td>Disruptive Task</td>
<td>39</td>
<td>.24 (.38)</td>
<td>.63 (.18)</td>
<td>.62 (.17)</td>
</tr>
<tr>
<td>Matched Control</td>
<td>42</td>
<td>.68 (.67)</td>
<td>.74 (.16)</td>
<td>.71 (.17)</td>
</tr>
<tr>
<td>Memory Load</td>
<td>35</td>
<td>.42 (.36)</td>
<td>.73 (.11)</td>
<td>.71 (.16)</td>
</tr>
</tbody>
</table>

* standard deviations for each score are given in parentheses

The pattern of means in Experiment 1 replicates the pattern reported by Stadler (1995) in the serial pattern learning paradigm. However, Stadler found that the difference between a task which disrupted organization and a memory load task was statistically significant. One possible explanation for the failure to achieve statistical significance between the two conditions in the current experiment could be the large within group variability in the d-prime measure (standard deviations are reported in Table 2).

To determine the extent to which participants in the four different study tasks were able to use the information gained at study to judge new grammatical strings which they had not previously seen, percent of correct grammatical
judgements on the two different types of grammatical test items was also examined. Recall that grammatical test items were of two types, old grammatical items which were seen during study, and new grammatical items which were not seen during study. One purpose of this analysis was to determine whether participants evidenced the ability to use knowledge gained to judge exemplars which were not from the study set. The other purpose was to confirm the study condition differences found in the $d'$ analysis.

Data were analyzed in a $4 \times 2$ mixed design analysis of variance, with study condition (pure control, disruptive dual task, matched control, and memory load dual task) as a between-subjects variable and test item type (old grammatical test items and new grammatical test items) as a within-subjects variable. This analysis revealed a significant main effect of study task, $F(3, 153) = 5.02$, $p<.005$. This effect was due to the fact that, overall, participants in the disruptive dual task condition exhibited lower performance than did participants in the other study conditions. A Newman-Keuls multiple comparison procedure collapsed across old and new test items reveals that participants in the disruptive dual task condition performed significantly worse on grammatical test items than did participants in the pure control task, $Q(3, 153) = 4.96$, $p<.05$, the matched control task, $Q(3, 153) = 4.38$, $p<.05$, or the memory load task, $Q(3, 153) = 3.87$, $p<.05$. Thus, the
results of this analysis indicate that on grammatical test items participants in the disruptive dual task condition did not exhibit the same degree of learning as did participants in the memory load dual task. This result must be viewed with caution, however, because these data are not corrected for the response bias which could be operating differently in the different conditions.

There was also a marginal effect of test item type, $F(1, 153) = 3.10, p<.1$. This was caused by the fact that participants were slightly better at judging the grammaticality of items which they had seen during training. Participants were also able to effectively judge the grammaticality of new test items, however. Performance was significantly better than chance for all groups on old items, $F(1, 156) = 276.63, p<.0001 (M = .71)$, as well as new items, $F(1, 156), p<.0001 (M = .70)$ indicating that participants seemed to be able to use knowledge gained at study to judge both items seen at study and those not previously seen.

The main conclusion to be drawn from Experiment 1 is that in artificial grammar learning, just as in serial pattern learning, interrupting organized encoding of the training stimuli seems to result in a significant decrement in learning. Significant evidence that interrupting organized encoding of the stimulus set may have a different effect on learning artificial grammar strings than simply

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occupying some portion of participants' limited processing capacity was not found; however, the pattern of means did replicate the results of Stadler (1995).

The results of Experiment 1 did not provide support for the hypothesis that different secondary tasks may interact with a given primary task in different ways, affecting learning to the extent that they interrupt organized processing of the primary task stimuli. However, as acknowledged, there were problems with interpreting the results of the first experiment. For example, the hypothesis which seemed to be supported by the pattern of data and results in the proportion correct measure for grammatical items was not supported by significant results in the d-prime measure which corrected for the strong response bias.

In addition, the response bias suggests inadequacies in the testing procedure. High variability suggests that the experimental manipulation may not be as strong as desired, and also that the test may not be sensitive enough to detect differences in learning. Procedural changes in Experiment 2 were intended to correct problems experienced in Experiment 1.

**Experiment 2**

In the first experiment, the study task manipulation proved to be weaker than desired. The reason could have been that requiring participants to type letter strings
first and then type the arrows may have encouraged them to try to hold the letter string in memory as a whole. Thus, the manipulation of interrupting association of letters may have been weakened, even though participants never saw the letter string as a whole. In Experiment 2, rather than instructing participants in the disruptive secondary task condition to respond by typing the letter string and arrow string sequentially, they were instructed to type the letter strings with the arrows inserted just as they were presented on the computer screen. This simple change in the way participants respond should alleviate the problem of possible whole string memorization and strengthen the experimental manipulation.

Another problem in Experiment 1 was the response bias at test which resulted in many more correct responses on grammatical than on ungrammatical items. The response bias made it difficult to determine how much learning had actually occurred during study because it rendered the test relatively insensitive. These problems were overcome by changing the test from a simple yes-no grammatical judgment to a two alternative, forced choice test. In this new test, each test item was created by pairing one grammatical string with an ungrammatical string created by changing one letter of the grammatical string. Participants were required to choose the grammatical string from the pair of letter strings shown on the computer screen.
Another change in the new test was the way ungrammatical strings were constructed. In the previous two experiments, ungrammatical strings were created by randomly changing one or more letters of a grammatical string; however, no measure was taken of how accurate participants were at detecting different types of violations. In Experiment 2, ungrammatical strings were constructed in a systematic way making it possible to make predictions based on previous research about how difficult detecting ungrammatical strings with particular types of violations would be.

Previous research (St. John & Shanks, in press; Gomez & Schvaneveldt, 1994; Perruchet & Pacteau, 1990; Mathews, et al, 1989) has shown that people are sensitive to positional information contained in grammatical strings suggesting that they may also be differentially sensitive to violations of grammaticality occurring at different locations within the string. Perruchet and Pacteau demonstrated that people are better able to reject ungrammatical strings if the violations occur near the beginning or the end of the string. According to a more recent study by St. John and Shanks, people also seem to be sensitive to violations involving repeating letters which indicate recursions in the grammar. St. John and Shanks demonstrated differential sensitivity to location of grammatical violation by varying violations across five different locations. Ungrammatical
strings in Experiment 2 were patterned after those used by St. John and Shanks and are described in detail below in the methods section.

Method

Participants. Eighty students participated in this experiment in return for extra credit points in undergraduate psychology courses at Louisiana State University. Students were from the same population described in Experiment 1. Twenty students participated in each of the four conditions. Participants were run in groups of up to 20 and were randomly assigned to each of the four conditions by order of entry into the laboratory.

Apparatus and stimuli. The experiment was run on the same computers as the previous experiment. Stimuli for the training phase were the same as described in Experiment 1. Responses during the training phase of the experiment were also made in the same manner.

Stimuli for the test were 40 pairs of letter strings consisting of one grammatical and one ungrammatical string. Twenty of the grammatical strings were those seen during the study phase of the experiment, and 20 were grammatical strings not seen at study. Recall from Experiment 1 that half the participants saw 20 of the grammatical strings at study and half saw the other 20. Thus, across all participants, each test string was presented equally often during the study phase of the experiment.
Each ungrammatical string was created from the grammatical string with which it was paired (see Appendix for a complete list of test items). Violations of grammaticality were accomplished by changing one letter of the grammatical string; therefore, strings of each pair were matched for string length. Location of the violation was varied across the set of test items as in St. John and Shanks (in press) to test for differential sensitivity to violation location.

Five violation locations were selected based on information from previous research (St. John & Shanks, in press; Gomez & Schvaneveldt, 1994; Perruchet & Pacteau, 1990; Mathews, et al, 1989), and predictions were made about the relative difficulty of detecting ungrammatical strings with violations in the various locations (see Table 3 for examples of violation locations). Violations were accomplished by changing the letter at the particular location to another letter which occurs in grammatical strings, but not in that particular location. Letters were not added or omitted, nor was more than one violation made per string because these were considered different factors which must be tested separately from location. Violation

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6 The five violation locations selected for this study were not, nor were they intended to exhaustive. They were simply representative of locations mentioned in previous research to which participants are differentially sensitive.
locations and predictions based on sensitivity to these locations are described in the following paragraph.

The first location selected was the first position of the grammar string. Since the grammar only has two legal beginning letters, ungrammatical strings with first letter violations were predicted to be easily detected by participants in all study conditions across both old and new test items. The second violation location selected was the second position of the string. Because previous research indicates that people are especially sensitive to beginning bigrams (Perruchet & Pacteau, 1990, 1991), second letter violations were also predicted to be relatively easy to detect, though possibly not as easy as first letter violations.

Valid ending bigrams are also thought to be salient in discriminating grammaticality (Perruchet & Pacteau, 1991), thus, the third violation location chosen was the penultimate position. Changing the letter in this position resulted in a string with an illegal ending bigram. Since there are only three ending bigrams, participants were expected to be relatively sensitive to this violation also.

The fourth violation location was intended to test sensitivity to legal repetitions. To accomplish this violation a letter was changed to create an illegal repetition somewhere within the string. These ungrammatical strings were predicted to be more difficult to detect than
the three types previously described, but easier than the fifth type of ungrammatical string. The fifth violation location involved changing a letter which was neither part of a beginning or ending bigram, nor part of a repeating sequence, to a letter which could not legally occur in that particular location. This violation was called a nonrepeating middle letter violation. Research has shown that people are not as sensitive to internal bigrams (Perruchet & Pacteau, 1991), thus, participants were expected to be relatively insensitive to this type of violation.

| Table 3. Examples of five different violation locations used in Experiment 2 and Experiment 4 |
|-----------------------------------------------|-------------------|
| Ungrammatical       | Grammatical       |
| First Letter Violation | VSSXXVPS         | TSSXXVPS         |
| Beginning Bigram Violation | PSPXTVPS       | PVPXTVPS         |
| Ending Bigram Violation     | TXXTVTS         | TSSTVPS         |
| Illegal Repetition          | TXXVXXV      | TXXVXPXV         |
| Nonrepeating Middle         | PVSXV       | PVPXV           |

Eight ungrammatical letter strings were created with each violation location described above for a total of forty ungrammatical distractors. These letter strings were then paired with the strings from which they were created for presentation during the testing phase of the experiment (see Appendix for a list of ungrammatical test items).
**Design and procedure.** The design was a 4 x 2 x 5 mixed design with the between-subjects factor, study task, having four levels corresponding to the four different study conditions described in Experiment 1. The only difference in Experiment 2 is the change in study task methodology intended to strengthen the disrupting manipulation (see Table 4). Participants in the disruptive dual task condition were prompted to enter the letter sequence with the arrows inserted between the letters just as the strings were presented, rather than to enter letters and then arrows. Feedback was given in the form of percent correct for the letter sequence and percent correct for the arrows just as in Experiment 1. Giving feedback on arrows separately was intended to insure that participants attended to the arrows at least as much as they did to the letter strings. There were also a single task pure control, a matched control instructed to ignore the secondary task stimuli, and a dual task memory load task which were conducted exactly like comparable conditions in Experiment 1.

In addition to the between-subjects manipulation, there were two within-subjects variables, test item type with two levels (old test items which had been seen at study and new test items which had not been seen) and violation location with five levels (first letter, beginning bigram, ending
bigram, illegal repetition, and nonrepeating middle letter) as described above.

Table 4. Illustration of change in study task manipulation in Experiment 2

| Study Condition |
|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|
| Example Strings | Pure Control → T X X T V V | Disrupt Task → T ↑ X ↑ X ↓ T ↓ V ↓ V | Matched Control → T ↑ X ↑ X ↓ T ↓ V ↓ V | Memory Load → ↑ ↑ ↓ ↓ ↓ T X X T V V |
| Control Conditions | Letters → T X X T V V | Feedback Letters → % Correct | Feedback Letters → % Correct Arrows | Feedback Letters → % Correct Arrows |
| Dual Tasks | Disrupt Task → T ↑ X ↑ X ↓ T ↓ V ↓ V | Memory Load → Letters → T X X T V V | Arrows → ↑ ↑ ↓ ↓ ↓ |
| Feedback | Letters → % Correct | Arrows → % Correct Arrows |

After 60 study trials participants in all conditions were informed that the study strings conformed to a set of rules called a grammar and were told that they would be given a test. At test, participants were shown the 40 pairs of letter strings described above. Participants were instructed to move the cursor under the string which they believed conformed to the rules of the grammar, or was most like the strings they saw at study, and type G for good, or
grammatical. This experiment was intended to replicate Experiment 1 using the new methodology.

To review the predictions based on the combination of conditions and previous research findings, if secondary tasks have the effect of simply occupying some portion of limited capacity processing resource which is required to encode the grammar strings, we would expect all secondary tasks to cause similar decrements in grammar learning. Second, if implicit learning of the grammar proceeds without this limited capacity resource, as some claim, none of these secondary tasks should hurt grammar learning. And third, if secondary tasks occupy some limited capacity resource preventing ineffective explicit strategies, as Hayes and Broadbent (1988) suggest, assuming that learning the regularity in a set of strings best proceeds without explicit processing, as has been proposed by some theorists (Reber, 1989; Hayes & Broadbent, 1988; Mathews, et al. 1989), we may expect any type of secondary task to actually facilitate grammar learning.

On the other hand, if organized encoding of the letter strings is the most important factor, the different secondary tasks could have different effects. A disruptive secondary task has greater potential for interrupting organized encoding than a simple memory load task; thus, the condition with the disruptive secondary task should do worse than the memory load task condition.
Based on previous results, participants were expected to display more knowledge of old grammatical test items than of new test items, although learning should be evidenced on both old and new test items. And finally, based on previous research in the grammar learning literature, violations of grammaticality in the first position were expected to be easiest to detect, with second letter or beginning bigram violations and penultimate letter or ending bigram violations second. Illegal repetitions should be more difficult to detect than beginning or ending violations but should be more detectable than nonrepeating middle letter violations.

Results and Discussion

Data were analyzed by a 4 x 2 x 5 mixed design ANOVA. To review, there were four levels of the between-subjects variable, task (pure control, disruptive dual task, matched control, and memory load dual task). In addition, there were two within-subjects variables, test item type with two levels (old test items which had been seen at study and new test items which had not been seen) and violation location with five levels (first letter, beginning bigram, ending bigram, illegal repetition, and nonrepeating middle letter).

The analysis revealed a significant main effect of task type, $F(3,76) = 4.98, p<.005$, which did not interact with either of the within-subjects variables; therefore, means for the four task types were collapsed across test item type.
and violation location for further examination. A Newman-Keuls multiple comparison procedure was performed to
determine which means were significantly different. This
analysis revealed that the two control conditions were not
different from each other (see Table 5 for means). The
disruptive dual task condition performed significantly worse
than the pure control condition, $Q(4, 19) = 4.99, p < .01$, and
also worse than the matched control, $Q(4, 19) = 4.02, p < .05$.
These results support the hypothesis that disrupting
learning of letter associations with a secondary task
produces a decrement in learning.

Table 5. Proportion correct by study task condition for
Experiment 2

<table>
<thead>
<tr>
<th>Study Condition</th>
<th>n</th>
<th>Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pure Control</td>
<td>20</td>
<td>.76 (.16)</td>
</tr>
<tr>
<td>Disruptive Task</td>
<td>20</td>
<td>.63 (.18)</td>
</tr>
<tr>
<td>Matched Control</td>
<td>20</td>
<td>.74 (.16)</td>
</tr>
<tr>
<td>Memory Load</td>
<td>20</td>
<td>.68 (.11)</td>
</tr>
</tbody>
</table>

* standard deviations for each score are given in parentheses

The mean of the memory load dual task condition fell
between the matched control and the disruptive dual task,
but was not statistically different from either one. The
memory load task, however, was marginally different from the
pure control, $Q(4, 19 = 3.12), p < .1$. This seems to indicate
that requiring participants to simply hold information while
encoding grammar strings may produce some decrement in learning, a result which was not predicted but which is consistent with Dienes et al.'s (1990) assessment of grammar learning as requiring some limited capacity resource.

The pattern of means for the four study conditions in Experiment 2 replicates that of Experiment 1, and again statistical differences were found to support the claim that disrupting organization inhibited grammar learning. There was no statistically significant evidence, however, that a simple memory load caused less inhibition of learning than did disrupting organization.

In addition to the task effect, there was a significant main effect of test item type $F(1, 76) = 6.75, p<.05$. The test item effect was due to the fact that participants, in general, were better at discriminating old grammatical items ($M = .73$) than new grammatical items ($M = .68$) from the ungrammatical strings, a small but significant difference, $Q(1, 79) = 3.74, p<.05$. This result is interesting, given the fact that the difference in performance on old and new test items in the first experiment was only marginally significant. This supports the idea that the test used in Experiment 2 may have been more sensitive than the test used in Experiment 1.

Overall performance on the old test items, $F(1, 79) = 1852.14, p<.0001$, as well as new test items, $F(1, 79) = 1857.12, p<.0001$, was significantly above chance,
indicating that participants learned to discriminate grammatical from ungrammatical strings, even if the particular grammatical string had not been seen previously. This result supports previous findings (e.g., Reber, 1969; Mathews et al., 1989) that the knowledge participants gain enables them to go beyond merely identifying grammatical strings seen at study.

As predicted, violation location also significantly affected participants’ discrimination performance, $F(4, 304) = 23.41, p < .0001$. A Newman-Keuls multiple comparison was performed to determine significant differences between cell means. Participants were significantly better at detecting ungrammatical strings when the violation occurred in the first position than in other positions. This was expected but the proportion correct for first position violations was a little lower than might be expected given that there are only two valid beginning letters in this grammar (see Table 6). Violations of the first letter were detected better than beginning bigram violations $Q(2, 304) = 6.99, p < .01$, and better than violations of the ending bigram, $Q(1, 304) = 4.13, p < .01$.

Ending bigram violations were easier to detect than violations of the beginning bigram, $Q(1, 304) = 2.87, p < .05$, a result which was not really expected as beginning bigrams and ending bigrams were predicted to be of equal salience. Perhaps this result is due to the fact that there are four
different grammatical beginning bigrams, but only three different ending bigrams, thus the opportunity to see any given bigram in the set of 20 training items was greater for ending bigrams than for beginning bigrams. Greater exposure to a particular letter pair during study, in addition to having fewer letter associations to encode, could result better memory for any particular bigram (Anderson, 1989). If you think of memory for valid bigrams as knowledge participants used to detect grammatical strings, fewer bigrams seen more often could produce better memories which were more likely to be used at test.

Table 6. Proportion correct across study tasks by violation location for Experiment 2

<table>
<thead>
<tr>
<th>Violation Location</th>
<th>N</th>
<th>Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td>First Letter</td>
<td>80</td>
<td>.85 (.19)</td>
</tr>
<tr>
<td>Beginning Bigram</td>
<td>80</td>
<td>.70 (.19)</td>
</tr>
<tr>
<td>Ending Bigram</td>
<td>80</td>
<td>.76 (.21)</td>
</tr>
<tr>
<td>Illegal Repetition</td>
<td>80</td>
<td>.62 (.26)</td>
</tr>
<tr>
<td>Non-repeating Middle Letter</td>
<td>80</td>
<td>.60 (.19)</td>
</tr>
</tbody>
</table>

* standard deviations for each score are given in parentheses

Another explanation for this effect could lie in the particular letter pairs which composed the ends of grammatical strings. Valid endings were "XS," which sounds like the English word excess, "PS," which is a familiar abbreviation, and "VV" which is both visually and auditorily

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salient. According to a classic study by Bower (1970), people recall letter bigrams and trigrams better if the group of letters as a whole is meaningful. For example, the two trigrams FBI and CIA would be recalled better than the three bigrams FB, IC, and IA. In fact, in post experimental questionnaires, some participants reported that they tried to "make words" to aid short term retention for groups of letters. Of course this explanation is merely post hoc speculation based on observed results.

Distractors with illegal repetitions were significantly more difficult to detect than those with first letter, $Q(3, 304) = 10.80, p<.01$; or ending bigram violations, $Q(2, 304) = 6.67, p<.01$; and also more difficult than those with beginning bigram violations, $Q(1, 304) = 3.80, p<.01$. Recall that illegal repetitions were expected to be harder than violations at the beginning or ends of strings, but easier to detect than violations of nonrepeating middle letters; however, detection of illegal repetitions were not found to be significantly different from nonrepeating middle letter violations. Finally, as predicted, ungrammatical strings with violations of nonrepeating middle letters were significantly harder to detect than beginning letters, $Q(4, 304) = 11.67, p<.01$; beginning bigrams, $Q(2, 304) = 4.68, p<.01$; or ending bigram violations, $Q(3, 304) = 7.54, p<.01$.

Even though detecting some types of violations was harder than detecting others, participants were able to
reject ungrammatical strings significantly more often than would be expected by chance for first letter violations, \( F(1, 79) = 278.13, p < .0001 \); for beginning bigram violations, \( F(1, 79) = 86.51, p < .0001 \); for ending bigram violations, \( F(1, 79) = 126.11, p < .0001 \); for illegal repetitions, \( F(1, 79) = 17.82, p < .0001 \); and for nonrepeating middle letter violations, \( F(1, 79) = 21.24, p < .0001 \) (see Table 6 for means).

There are at least two possible interpretations of this pattern of results. First, as previous researchers have noted (Perruchet & Pacteau, 1991), participants may be differentially sensitive to beginnings and endings of grammatical strings. Another possible explanation, however, is that the illegal repetitions and nonrepeating middle letter violations may have been contained in longer strings which made detection more difficult. Examination of the test strings revealed that distractors with illegal repetitions and nonrepeating middle letter violations were not significantly longer than distractors with other violations, \( F(4, 35) = .08, p = .99 \). Thus, it seems reasonable to conclude that this pattern confirms previous research showing that people attend to string beginnings and endings more than other letters in the string (Perruchet & Pacteau, 1991).

Although the effects of the two within-subjects variables described above, while not always consistent with
predictions, seem reasonable, they must be interpreted with caution because there was a marginally significant interaction between test item type and violation location, $F(4, 304) = 2.29, p = .06$. Examination of the means collapsed across study condition reveal that the source of this marginal interaction seems to be the differences in the participants' ability to use information gained on old test items to detect violations in test items they had not seen at study across the five different violation locations. As shown in Figure 2, there was essentially no difference in performance on old and new test items when violations involved first letters or ending bigrams. There were, however, marginally significant differences between performance on old and new test items when violations involved beginning bigrams, $Q(5, 304) = 3.5, p < .1$; illegal repetitions, $Q(5, 304), p < .1$; and nonrepeating middle letters, $Q(5, 304) = 3.19, p < .1$. To summarize, participants were better at discriminating grammatical from ungrammatical test items if they had seen the item at study given that violations did not involve the first letter or the ending bigram. In other words, detection of violations in old strings was better than detection of violations in new strings unless those violations occurred at the beginning or ending of strings. This result is interesting because it indicates that being able to detect violations in exemplars not previously seen depends on the location of the
Figure 2. Proportion correct for violation location by test item type in Experiment 2. Chance performance is .5. Error bars represent 95% confidence intervals.
violation. This supports previous research finding that participants are differentially sensitive to the beginnings and endings of strings (Dulany, et al., 1984; Perruchet & Pacteau, 1991).

Several general conclusions can be drawn from the results of Experiment 2. First, the fact that the effect of study condition was the same as it was in Experiment 1 indicates that the new study methodology produced essentially the same pattern of learning as the old methodology. The second conclusion that may be drawn is that since the new test revealed significant and marginally significant differences where no differences were found before, the new testing method seems to be more sensitive than the old test. In addition, the within group variability was less, either due to the more sensitive test or perhaps the strengthened experimental manipulation, indicating that the strengthened study manipulation together with the new testing procedure is an improved paradigm. Note also that the results are much easier to interpret since the dependent measure is percent correct rather than $d'$. A more important result of Experiment 2 is that the pattern of means for the four study conditions replicates the pattern found in Experiment 1, as well as the pattern reported by Stadler (1995). The second replication of this pattern prompted a power analysis and computation of effect
size to determine whether the failure to find statistically significant differences between the memory load dual task condition and the disruptive dual task condition may be due to a lack of statistical power.

According to Stevens (1986), lack of statistical power can be due to high within group variability. In Experiment 1 the variability of \( d' \) was viewed as a problem contributing to the failure to find significant results. However, Stevens states that there are two other problems which may contribute to lack of statistical power, small effect size and inadequate sample size.

A common measure of effect size is \( d \), which is defined by Cohen (1977) as the number of standard deviation units by which two groups differ. According to Cohen, \( d = .2 \) is a small effect, \( d = .5 \) is a medium effect, and \( d = .8 \) or larger is a large effect. Effect size for the difference between the memory load condition and the disruptive dual task condition was relatively small in both Experiment 1 (\( d = .35 \)) and Experiment 2 (\( d = .42 \)).\(^7\) According to Cohen (1977), power resulting from the effect sizes observed and sample sizes used was .32 for Experiment 1 and .24 for Experiment 2. Given that a power of .8 is recommended to obtain statistically significant results, it seems reasonable to

\(^7\) Effect size was computed using the pooled variance estimate from the overall analysis rather than from only the two groups in question because the effect was observed within the context of the four experimental conditions.
assume that a lack of statistical power could account for the failure to find significant results in these two experiments.

Using formulas suggested by Hinkle, Wersma, and Jurs (1988) for estimating required sample size, approximately 120 participants per group would be required to detect an effect as small as found in Experiment 1, and 90 participants per group would be required to detect an effect as small as the one observed in Experiment 2. Cohen’s (1977) power tables confirm these estimates. Thus, it is possible that conducting these experiments with increased sample size would result in a statistically significant difference between the disruptive dual task group and the memory load group.

Results of the power analysis, along with duplication of the pattern of means which replicate Stadler’s (1995) results seem to indicate that even though the effect of type of secondary task may be small, it is a phenomenon which warrants further investigation.

Another interesting finding is that of the marginally significant difference between the memory load and the pure control condition. Recall that although differences were not significant, the pattern of means was similar in Experiment 1. This could indicate that adding a secondary task which simply occupies some portion of processing capacity may produce some deficit in learning. This finding
supports Dienes et al.'s (1991) contention that grammar learning depends on a limited capacity resource. The result, however, does not address the question of whether this effect is because the grammar strings must be rehearsed in verbal memory in order to be encoded or simply caused by the fact that the grammar learning task is largely explicit. Future research should explore this issue further.
Chapter 3 - Experiment 3 and Experiment 4

Dual Tasks which Organize or Disrupt Organization

The third experiment in this series was concerned with a different question relevant to this line of research, specifically whether adding a secondary task may actually facilitate learning in an implicit learning task. The only artificial grammar experiment using a dual task paradigm reported a significant decrement in learning under dual task conditions (Dienes et al., 1991); however, research in other implicit learning paradigms has reported facilitated learning when a secondary task was added (Hayes & Broadbent, 1988). The general explanation for this phenomenon has been that the secondary task has the effect of preventing ineffective explicit strategies allowing the more effective implicit learning processes to operate unimpeded (Hayes & Broadbent, 1988; Porter, 1991). Another possible explanation for facilitation is that disrupting encoding in particular places or at particular intervals may have the effect of organizing the primary task stimuli in a beneficial way. Stadler (1993) found that disrupting repeating serial patterns in a consistent manner with pauses produced a slight facilitation in learning while disrupting in an inconsistent way inhibited learning.

Servin-Schreiber and Anderson (1990) demonstrated that grouping letters of artificial grammar strings at study in a way which emphasized structural regularities in the stimulus...
set facilitated learning, while grouping letters in a way which made regularities harder to notice inhibited learning. They called these two methods of grouping letters **good chunking** and **bad chunking** respectively. Essentially, Servin-Schreiber and Anderson's (1990) good-chunked strings always grouped high frequency bigrams and trigrams and runs (indicating recursions in the grammar). For example, the typical string, \texttt{PTTTTVPS}, contains one recursive run, \texttt{TTTT} and ends with a trigram frequently seen in grammatical strings, \texttt{VPS}. The well chunked version of this string would be \texttt{P\textdagger TT\textdagger VPS}. Bad chunking, on the other hand, breaks up runs and frequent bigrams and trigrams. For example a badly chunked version of this string would be \texttt{PTT\textdagger TV\textdagger PS}.

Experiment 3 was intended to test the hypothesis that adding a secondary task which chunked grammar strings in a way which made structural regularities salient may have an organizing effect thus, facilitating learning. Chunking strings in a way which concealed regularities, on the other hand, may hurt performance (see Appendix for list of good chunk training strings and bad chunk training strings) by making regular features of grammatical strings less salient.

**Experiment 3**

Experiment 3 was a 2 (good chunk versus bad chunk) x 2 (dual task versus single task) between-subjects design. In the two dual task conditions, the secondary task was designed to chunk the letter strings. Good chunk training
consistently presented bigrams and trigrams which frequently appeared in grammatical strings. Bad chunk training consistently presented groups of letters in a way which broke up bigrams and trigrams frequently appearing in grammatical strings. Chunking conditions were modeled after Servin-Schreiber and Anderson's (1990) chunked string study conditions.

In the two single task matched control conditions, participants saw the stimuli presented just as in the dual task conditions, but were instructed to ignore the secondary task stimuli.

Method

Participants. Seventy-six students participated in Experiment 3 in return for extra credit points in undergraduate Psychology courses at Louisiana State University. Students were from the same population described in Experiment 1 and used in Experiment 2; however, no student had participated in either of the previous two experiments.

Due to a technical difficulty with computer equipment, data from only 12 out of 18 participants in the dual task good chunk training condition were available for analysis. In the dual task bad chunk training condition 20 students participated, while 20 students participated in the matched control good chunk condition, and 18 participated in the
matched control bad chunk condition. Participants were run in groups in the same manner as previous experiments.

**Apparatus and stimuli.** The apparatus and stimuli for Experiment 3 were the same as in the first two experiments. The only difference in the training stimuli was that in the four chunking conditions in Experiment 3, arrows appeared between some adjacent letters in order to force organization of the letter strings into good chunks or bad chunks (as defined by Servin-Schreiber and Anderson, 1990), rather than appearing between every pair of letters as in the disruptive dual task condition in Experiment 1. Good and bad chunked strings were equated on number of chunks. Again, half the participants received one set of 20 grammatical strings at study and half the participants received another 20 grammatical strings.

At test, 80 strings of letters were presented in holistic form (not chunked) to be judged as grammatical or ungrammatical by participants. Forty of the test strings conformed to the grammar and 40 were ungrammatical. Of the 40 grammatical letter strings, 20 were strings seen at study and 20 were novel strings. Responses during the training phase of the experiment were made just as they were in previous experiments.

**Design and procedure.** As mentioned, this 2 x 2 design had two levels of chunk type (good chunk versus bad chunk) and two levels of task type (dual task versus single task).
The procedure for this experiment returned to the methodology of Experiment 1. Although Experiment 3 was concerned with a different research question, it was run concurrently with Experiment 1, and thus, utilized the same experimental procedures.

The first study condition was the beneficial chunk dual task condition (or good chunk training condition). During study, these participants saw sequences of letters similar to the ones seen in the first two experiments, but rather than inserting arrows between each pair of letters, arrows were inserted in specific locations in each letter sequence, forming chunks which, according to Servin-Schreiber and Anderson (1990), make the structural regularity of the stimulus set more salient. After the entire sequence had been presented, the participant responded as in Experiment 1 (see Table 7 for an example).

The second study condition (bad chunk training condition) was the same as the first, except that arrows were inserted in positions which chunked letter strings in a way which concealed the structural regularity, according to Servin-Schreiber and Anderson (1990). Bad chunks essentially broke up frequent bigrams, trigrams and repetitions found in grammatical strings.

The two additional conditions were matched control conditions in which participants saw the stimuli presented in the same manner as participants in the two dual task
conditions, but were instructed to ignore arrows and to respond by typing letter sequences only. After 60 study trials participants were given the grammatical judgement test used in Experiment 1.

Table 7. Illustration of chunking methodology used in Experiment 3

<table>
<thead>
<tr>
<th>Study Condition</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Example Strings</strong></td>
</tr>
<tr>
<td>Good Chunk ► T ↑ X X ↓ T V V</td>
</tr>
<tr>
<td>Bad Chunk ► T X ↑ X T V ↓ V</td>
</tr>
<tr>
<td>Good Chunk Matched Control ► T ↑ X X ↓ T V V</td>
</tr>
<tr>
<td>Bad Chunk Matched Control ► T X ↑ X T V ↓ V</td>
</tr>
</tbody>
</table>

**Expected Responses**

<table>
<thead>
<tr>
<th>Dual Tasks</th>
<th>Good Chunk</th>
</tr>
</thead>
<tbody>
<tr>
<td>Letters ► T X X T V V</td>
<td></td>
</tr>
<tr>
<td>Arrows ► ↑ ↓</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Bad Chunk</th>
</tr>
</thead>
<tbody>
<tr>
<td>Letters ► T X X T V V</td>
</tr>
<tr>
<td>Arrows ► ↑ ↓</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Feedback</th>
<th>Letters ► % Correct</th>
</tr>
</thead>
<tbody>
<tr>
<td>Arrows ► % Correct Arrows</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Control Conditions</th>
</tr>
</thead>
<tbody>
<tr>
<td>Letters ► T X X T V V</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Feedback</th>
</tr>
</thead>
<tbody>
<tr>
<td>Letters ► % Correct</td>
</tr>
</tbody>
</table>

**Results and Discussion**

The primary measure used in Experiment 3 was d' to correct for the response bias as in Experiment 1. Data were

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analyzed in a 2 (good chunk versus bad chunk) x 2 (dual task versus single task) between-subjects ANOVA.

This analysis revealed a marginally significant effect of dual task versus single task, $F(1, 66) = 2.82, p<.1$. In general, performance in the single task condition was better than in the dual task condition (see Table 8 for means). This result does not support the prediction that chunking strings in a beneficial way at study may facilitate learning; however, as stated earlier, there are numerous problems with this paradigm.

Table 8. Proportion correct grammaticality judgments for old and new test items and d-prime by study task condition for Experiment 3

<table>
<thead>
<tr>
<th>Study Condition</th>
<th>n</th>
<th>d'</th>
<th>Old Items</th>
<th>New Items</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dual Good Chunk</td>
<td>12</td>
<td>.41 (.64)</td>
<td>.66 (.25)</td>
<td>.58 (.22)</td>
</tr>
<tr>
<td>Dual Bad Chunk</td>
<td>20</td>
<td>.33 (.41)</td>
<td>.72 (.16)</td>
<td>.67 (.13)</td>
</tr>
<tr>
<td>Control Good Chunk</td>
<td>20</td>
<td>.56 (.48)</td>
<td>.76 (.15)</td>
<td>.69 (.14)</td>
</tr>
<tr>
<td>Control Bad Chunk</td>
<td>18</td>
<td>.54 (.42)</td>
<td>.67 (.18)</td>
<td>.66 (.16)</td>
</tr>
</tbody>
</table>

* standard deviations for each score are given in parentheses

A 2 (good versus bad chunk type) x 2 (dual versus single task) x 2 (test item type) mixed design ANOVA was also run to determine whether participants were able to use knowledge gained to judge grammatical strings they had not seen at study.
This analysis resulted in a significant effect of test item type, $F(2, 132) = 8.22, p<.01$, because participants again performed better on old test items which they saw during training ($M = .71$) than they did on new test items they had not seen ($M = .66$). Again, however, performance proved to be better than would be expected by chance for participants in all training conditions on old items, $F(1, 69) = 94.33, p<.0001$ and on new items, $F(1, 69) = 68.65, p<.0001$.

This analysis of grammaticality judgments also resulted in a marginal study task type by chunk type interaction, $F(1, 66) = 3.14, p<.1$. Collapsing the means across old and new test items revealed an unexpected pattern with single task control participants performing better with good chunk training ($M = .72$) versus bad chunk training ($M = .66$), but dual task participants performing better with bad chunk training ($M = .69$) than good chunk training ($M = .62$). This result, however, was not significant and proved to be due to the response bias. When ungrammatical test items were averaged into the group means, participants with good and bad chunk training exhibited similar performance in the dual task conditions, and in single task control conditions.

To summarize the results of Experiment 3, the chunking variable did not seem to have the expected result. The lack of significant results, however, could be attributed to the weakness of the experimental manipulation, the inability of
the test to detect differences, large variability in the d-prime measure or to the relatively small number of participants resulting in a lack of statistical power.

The next experiment in this series was an attempt to replicate the pattern of means of Experiment 3 using the new methodology and more sensitive test used in Experiment 2. The goal of Experiment 4 was to find support for the hypothesis that breaking up, or chunking the primary task stimuli in different ways with secondary task stimuli can cause different patterns of learning, perhaps even facilitatory effects. In addition, a test of Dienes et al.'s (1991) implication that the verbalizability of the secondary task stimuli may affect grammar learning was tested by adding two additional types of secondary task stimuli judged to be relatively less verbalizable than the arrows used in previous experiments.

Experiment 4

The first four conditions in Experiment 4 included the two chunking and comparable ignore control conditions from Experiment 3. This was intended to further investigate the hypothesis that a secondary task may have two different effects on grammar learning. Specifically, hypotheses tested were that a secondary task may inhibit learning by disrupting organized encoded or may facilitate learning by having an organizing effect on encoding of the primary task stimulus set.
In addition, two new types of secondary task stimuli were used to test Dienes et al.'s (1991) explanation of the effect of a dual-task paradigm on artificial grammar learning. Recall that Dienes et al. proposed that random number generating and learning letter strings share a limited capacity resource, possibly Baddeley's (1987) phonological loop. According to Dienes et al., the random number generating task prevented participants from forming sensitivity to the positional dependence of bigrams specifically because there was not enough of this required resource to do so. This explanation implies that a secondary task in which the stimuli are verbally rehearsed should interfere with learning grammar strings while a secondary task in which distractors are not verbally rehearsed should interfere less. Of course, if the secondary task is simply disrupting organized encoding of the primary task stimulus as Stadler (1995) claims, any type of secondary task stimuli has the potential to interfere to the degree that it disrupts organization.

To test Dienes et al.'s (1991) hypothesis, a secondary task which interrupted organized encoding with stimuli judged to have easily accessible verbal labels was compared with two other types of secondary task stimuli which were judged to be less easily verbally labeled. One of the 

8 Verbalizability of secondary task stimuli was judged by graduate students in the Psychology Department at LSU.
alternative distractor types was simply two characters with less readily available verbal labels than the arrows previously used. The other alternative distractor type was vertical displacement of the letter chunk on the computer screen. (New distractor types will be described in detail below in the method section.) This distractor type allowed testing whether chunking the string without inserting a character had the same effect as disrupting with an inserted character. The single task control conditions using the shifted distractor type could also be compared to Stadler's (1995) pauses condition in which serial patterns were interrupted with pauses rather than a secondary task. Stadler found that disrupting organization with pauses had much the same effect as disrupting with a secondary task, even though the pauses did not require any cognitive activity on the part of the participant.

To summarize predictions for Experiment 4, the chunking secondary tasks were expected to have an effect on the organization of primary-task stimuli, with good chunking which accented frequent features of grammatical strings facilitating learning, while bad chunking which made regularity less salient inhibiting learning. In addition, if results replicate those of Stadler (1993), bad chunking should cause a decrement in learning in the matched control condition in which participants were not required to recall the secondary task stimuli.
In addition, Dienes et al.‘s (1991) explanation that secondary tasks are likely to affect grammar learning to the extent that they interfere with verbal rehearsal of the letter strings was tested. In this manipulation, different types of secondary task stimuli judged to have less readily available verbal labels than the arrows used as distractors in previous experiments were used. Stimuli with less readily available labels were predicted to interfere with grammar learning to the same extent as readily verbalizable stimuli if they disrupt organization of the stimulus set.

Method

Participants. Three hundred and twenty-eight undergraduate psychology students participated in this experiment in return for extra credit points in psychology courses at Louisiana State University. Students were from the same population described in Experiment 1.

In the four conditions with arrows as distractor stimuli, 27 participants were run in the dual task good chunk training condition, 27 participants were run in the dual task bad chunk training, 26 participants were run in the matched control with good chunk training, and 27 were run in the matched control with bad chunk training. Data from 5, 4, 4, and 5 participants were omitted from the four conditions listed above respectively. These 18 participants, run in one experimental session, talked to each other during the experiment despite instructions from
the experimenter to refrain from talking. Thus, data collected during this particular session was considered contaminated by failure to follow instructions and was omitted from the analysis.

In conditions with ASCII characters as distractor stimuli, 29 participants were run in the dual task good chunk training condition, 26 were run in the dual task bad chunk training condition, 27 were run the matched control condition with good chunk training, and 26 were run in the matched control condition with bad chunk training. Data from 2 participants in the matched control condition with good chunk training were unavailable because of equipment failure. Data from 1 participant in the matched control condition with bad chunk training were omitted because this participant had been in a previous experiment in this series which was contrary to instructions.

In conditions with shifted distractors, 29 participants were run in the dual task good chunk condition, 28 were run in the dual task bad chunk condition, 29 were run in the matched control with good chunk training, and 27 were run in the matched control with bad chunk training. Data from 2 participants in the matched control with bad chunk training were unavailable because of equipment failure.
Participants were run in groups of up to 20 and were randomly assigned to each of the twelve different conditions by order of entry into the laboratory.\textsuperscript{9}

**Apparatus and stimuli.** The experiment was run on the same computers as the first three experiments. Stimuli for the study phase were the same as described in Experiment 3 above, with the exception that conditions were added using two new types of secondary task stimuli which were expected to be relatively less verbalizable than the arrows used as secondary task stimuli in the previous experiments.

The first new type of secondary task stimuli was a set of two (only two arrows were used in previous experiments) characters from the American Standard Code for Information Interchange (ASCII) which were judged to be relatively difficult to name, specifically character 198 ( gà ) and character 177 ( ¶ ). Of course, people can name anything if they wish to do so, but the expectation was that selecting characters which did not resemble letters, numerals, or easily named characters, coupled with the time pressure during the experiment would prevent all but the most inventive participants from creating names for these characters. Another consideration in selecting these characters was that they be distinguishable on the computer

\textsuperscript{9} All 12 conditions were not run concurrently; however, participants in all conditions came from the same subject pool and all conditions were run within a 10 week period of time.
screen but not in a way that was easily verbalizable, for example, by pairs of words such as dark and light or left and right (see Table 9 for example).

The second type of less verbalizable secondary task stimuli was spatial displacement of the letters of the primary task stimuli, or shifted distractors. This type of stimuli was chosen because there is some evidence that spatial location is encoded separately from identification of the object (Hasher & Zacks, 1979). In these conditions, rather than inserting characters into the grammar strings, the letters were shifted either up or down on the computer screen and participants were required to remember the change in spatial location when they typed in the string (see Table 10).

Responses during the training phase of the experiment were made in the same way as previously described. For the new types of secondary task stimuli, the arrow keys were appropriately labeled with the ASCII characters, or participants were instructed to use the arrow keys to indicate a shift the spatial position of the letters. Stimuli for the test were the same as test stimuli in Experiment 2.

Design and procedure. The tasks in this experiment were patterned after those in Experiment 3, that is, strings were chunked with the secondary task stimuli. The training methodology, however, was taken from Experiment 2. Recall
that to strengthen the experimental manipulation, participants typed training strings just as they were presented on the computer screen. In addition, the test from Experiment 2 which required participants to choose between a grammatical and an ungrammatical string was used.

Table 9. Illustration of methodology using ASCII distractors in Experiment 4

<table>
<thead>
<tr>
<th>ASCII Study Condition</th>
</tr>
</thead>
<tbody>
<tr>
<td>Example Strings</td>
</tr>
<tr>
<td>Good Chunk</td>
</tr>
<tr>
<td>Bad Chunk</td>
</tr>
<tr>
<td>Good Chunk</td>
</tr>
<tr>
<td>Matched Control</td>
</tr>
<tr>
<td>Bad Chunk</td>
</tr>
<tr>
<td>Matched Control</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Expected Responses</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dual Tasks</td>
</tr>
<tr>
<td>Good Chunk</td>
</tr>
<tr>
<td>Bad Chunk</td>
</tr>
<tr>
<td>Feedback</td>
</tr>
<tr>
<td>Letters ▶ % Correct</td>
</tr>
<tr>
<td>Symbols ▶ % Correct Symbols</td>
</tr>
<tr>
<td>Control Conditions</td>
</tr>
<tr>
<td>Feedback</td>
</tr>
</tbody>
</table>

This experiment was a 2 x 2 x 3 x 2 x 5 design, with three between-subjects factors and two within-subject factors. The three between-subjects factors were task type with two levels (dual task, matched control in which the secondary task stimuli were presented but ignored by
participants), chunk type with two levels (good, bad chunks), and secondary task type with three levels (arrows, less verbalizable ASCII characters, shifted distractors). The within-subjects factors were test item type (old, new) and violation location (first letter, beginning bigram, ending bigram, illegal repetition, and nonrepeating middle letter violation) just as in Experiment 2.

Table 10. Illustration of methodology using shifted distractor condition in Experiment 4

<table>
<thead>
<tr>
<th>Shifted Study Condition</th>
<th>Example Strings</th>
<th>Dual Tasks</th>
<th>Feedback</th>
<th>Control Conditions</th>
<th>Feedback</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Good Chunk</td>
<td>Good Chunk</td>
<td>Letters</td>
<td>Letters</td>
<td>Letters</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Position</td>
<td>Position</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Bad Chunk</td>
<td>Bad Chunk</td>
<td>% Correct</td>
<td>% Correct</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Good Chunk</td>
<td>Matched Control</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Matched Control</td>
<td>Matched Control</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Expected Responses

Letters ► % Correct
Position ► % Correct
This experiment was conducted in the same manner as previous ones. In the first two study conditions, stimuli were presented exactly the same as in the two conditions in Experiment 3 in which secondary task stimuli (arrows) were inserted in specified positions to chunk the letter sequences in a way hypothesized to be either beneficial or detrimental (good or bad chunks). The difference in these two conditions and the two in Experiment 3 is the way participants were instructed to respond. Participants in this experiment were required to type the letter sequences with the arrows inserted exactly as they saw them presented as in the new methodology used in Experiment 2. As added encouragement to actually verbally rehearse the secondary task stimuli, participants were instructed to move their mouth as if they were saying the letter string as the stimuli appeared on the screen. They were also instructed to say "up" if they saw an up arrow and "down" if they saw a down arrow. The third and fourth conditions were matched control conditions comparable to the previous two dual task conditions. In these conditions, participants were simply instructed to ignore the arrows.

The fifth and sixth conditions in this experiment were the same as the first two conditions, with the exception that the hypothesized less verbalizable ASCII characters were used as secondary task stimuli. In these conditions participants were instructed not to verbally rehearse the
secondary task stimuli. They were told that previous research indicated that characters like these were remembered much better if people tried to visualize the characters rather than name them. They were still encouraged to mouth the letter string, but to pause and visualize the ASCII character when it appeared on the screen. The seventh and eighth conditions were matched control conditions comparable to the previous two conditions.

The ninth and tenth conditions were the same as the first two conditions with the exception that shifts in the spatial location of letters on the computer screen were inserted as a secondary task rather than characters. In these two conditions participants were required to remember the shifts in spatial location and to shift the letters appropriately using the arrow keys when they typed the letter string back in. Here again participants were instructed not to try to verbalize the shifts in spatial location, but rather to visualize the shifts in spatial location on the computer screen. The eleventh and twelfth conditions were comparable matched control conditions. After training, participants in all 12 conditions were informed of the existence of the grammar, and given the two alternative forced choice test described in Experiment 2.

To review predictions, different types of distractors were not expected to differentially disrupt learning. Dual
task participants trained on good chunks were expected to show facilitated performance over matched control participants. Dual task participants trained on bad chunks were expected to show inhibited performance compared to dual task participants trained on good chunks. If the results of this experiment replicate Stadler's finding that disrupting organization can inhibit performance even when participants are not required to perform a secondary task, performance of matched control participants in the bad chunk training conditions should also be worse than participants in the single task control condition with good chunk training.

Old grammatical test items were expected to be detected better than new grammatical items in general, but new grammatical items were still expected to be detected at an above chance level, based on previous experiments. Also, based on previous results, the difference on old and new test items was expected to be much smaller on strings with first letter or ending bigram violations, because these two locations seem to provide salient cues which participants are able to use to judge the grammaticality of new strings.

In general, first letter violations were expected to be easiest to detect, with ending bigram violations second, and beginning bigram violations third, although it is possible that these two violation types may produce similar results, based on research in the artificial grammar literature. Finally, based on research in the grammar learning
literature in general, illegal repetitions should be detected better than nonrepeating middle letter violations, although this was not the result in Experiment 2.

**Results and Discussion**

The primary analysis for this experiment was a 2 x 2 x 3 x 2 x 5 mixed design ANOVA with three between-subjects variables and two within-subjects variables. Between-subjects variables were task type with two levels (dual task, matched control), chunk with two levels (good, bad), and distractor type with three levels (arrows, ASCII characters, shifted distractors). Within-subjects variables were test item type with two levels (old, new) and violation location with five levels (first letter, beginning bigram, ending bigram, illegal repetition, nonrepeating middle letter violation).

This analysis revealed a significant main effect of task, $F(1, 293) = 42.5, p<.0001$ with participants in control conditions ($M = .75$) performing better overall than participants in dual task conditions ($M = .66$). This was not totally unexpected since control conditions have produced better performance than comparable dual task conditions throughout the previous experiments. This finding supports the findings of researchers (Green & Shanks, 1993; Nissen & Bullemer, 1987) who claim that adding a secondary task can hurt performance on any primary task.
There was also a significant effect of chunk, $F(1, 293) = 7.78, p<.01$, with collapsed means revealing that participants receiving good chunk training ($M = .73$) performed better in general than participants receiving bad chunk training ($M = .69$).

There was, additionally, a significant task by chunk interaction, $F(1, 293) = 4.23, p<.05$, which must be taken into account when interpreting the previously mentioned main effects. In general, participants trained on good chunks did better than participants trained on bad chunks; however, this difference was greater in the dual task conditions. To further explicate this interaction, post-hoc t-tests using the Bonferroni correction for multiple t-tests were conducted for the two task types. This procedure revealed that the significant effect of chunking occurred only in the dual task conditions, $t(155) = 3.44, p<.001$. In the matched control conditions in which participants were instructed to ignore the secondary task distractor, there was not a significant effect of chunk (see Figure 3). The hypothesis that a secondary task may be facilitatory if it organizes the primary task stimuli in a beneficial way found no support in this experiment.

There was a main effect of the within-subjects factor, test item type, $F(1, 293) = 34.79, p<.0001$. This resulted from the fact that participants were better able to recognize old test items as grammatical ($M = .73$) than they
were to recognize new test items as grammatical ($M = .69$); however, this effect is qualified by the interaction of test item type with other variables.

Test item type entered into a significant three-way interaction with chunk and distractor type, $F(2, 293) = 3.34$, $p < .05$. This was the only significant effect in which distractor type was involved. To explicate this interaction, means were collapsed across task and violation location (See Figure 4). When old test items are compared to new test items, it is apparent that shifted distractors had a different effect on learning the grammar strings than did characters inserted into the letter strings as secondary task stimuli. There was little difference between old and new test items for participants who were trained on good chunks in the arrows and ASCII distractor conditions. Apparently, participants trained on good chunks with characters inserted as secondary task stimuli were good at using what they learned at study to judge new test items which they had not seen. There was, however, a significant difference between old and new test items for participants trained on bad chunks in the arrows, $Q(6, 293) = 4.11$, $p < .05$; and in the ASCII distractor conditions, $Q(6, 293) = 5.36$, $p < .05$. Participants trained on bad chunks with arrows or ASCII characters were not worse at judging the grammaticality of old test items than were participants trained on good chunks with these two types of distractors.
Participants in these two distractor conditions who were trained on bad chunks were, however, worse at using what they learned to judge new test items than were participants trained on good chunks. This is not to say, however, that they were unable to judge the grammaticality of new test items. Even though they were worse on new test items than on old test items, they were still significantly above chance on both old and new test items (see Table 11).

The pattern of performance in the arrow distractor and ASCII distractor condition was not replicated in the shifted distractor condition, however. In the shifted distractor condition, participants who were trained on good chunks did significantly better on old test items than on new test items, $Q(6, 293) = 5.27, p<.05$. That is to say, they were not as able to use knowledge to judge new test items as were participants in the other two distractor conditions. Participants trained on bad chunks with shifted distractors, on the other hand, performed equally well on old and new test items; however this was due to the fact that performance on old test items was worse relative to other distractor types.

In other words, participants trained on good chunks with shifted distractors were not as good at using what they learned at study to judge new test items as were participants in the other distractor conditions. For participants trained on bad chunks, performance of the
Figure 3. Proportion correct for task by chunk in Experiment 4. Group n's are given in parentheses. Chance performance is .5. Error bars represent 95% confidence intervals.
Figure 4. Proportion correct for chunk type by distractor type by test item type for Experiment 4. Group n’s are given in parentheses. Chance performance is .5. Error bars represent 95% confidence intervals.
shifted distractor group was generally lower than performance of participants in other distractor conditions on old items, but about the same as participants in other distractor conditions on new items. Although performance for this group was above chance on old test items as well as on new test items. It seems as though training on bad chunks with shifted distractors inhibited learning of studied strings as well as the ability to use knowledge gained to judge new test items.

Table 11. Statistics for tests against chance performance on chunk by test item type interaction in Experiment 4

<table>
<thead>
<tr>
<th></th>
<th>Good Chunk</th>
<th>Bad Chunk</th>
</tr>
</thead>
<tbody>
<tr>
<td>Arrow Distractors</td>
<td>F(1, 43)</td>
<td>F(1, 43)</td>
</tr>
<tr>
<td>Old Test Items</td>
<td>206.76****</td>
<td>98.03****</td>
</tr>
<tr>
<td>New Test Items</td>
<td>146.48****</td>
<td>59.85****</td>
</tr>
<tr>
<td>ASCII Distractors</td>
<td>F(1, 53)</td>
<td>F(1, 53)</td>
</tr>
<tr>
<td>Old Test Items</td>
<td>154.36****</td>
<td>152.01****</td>
</tr>
<tr>
<td>New Test Items</td>
<td>126.38****</td>
<td>73.63****</td>
</tr>
<tr>
<td>Shifted Distractors</td>
<td>F(1, 57)</td>
<td>F(1, 57)</td>
</tr>
<tr>
<td>Old Test Items</td>
<td>183.14****</td>
<td>63.32****</td>
</tr>
<tr>
<td>New Test Items</td>
<td>129.97****</td>
<td>69.70****</td>
</tr>
</tbody>
</table>

**** p<.0001

To further explore this interaction, post experimental questionnaires were examined to determine if participants in
the shifted distractor conditions may have had a different strategy which caused this apparent decrement in performance on training strings in the bad chunk training condition. In general, questionnaires were not very informative because participants gave vague answers to the open ended questions.

Recall that both participants in the shifted distractor group and in the ASCII distractor group were instructed to visualize the secondary task stimuli. A problem in interpreting the questionnaires was the number of participants whose response could not be interpreted as either visualizing or verbalizing. However, a slightly higher percentage of participants in the shifted distractor condition trained on bad chunks explicitly stated that they were attempting to visualize the secondary task (shifted spatial position) than did participants in the ASCII distractor condition. Percentage of participants explicitly stating that they visualized distractors were 56% for the shifted condition and 43% for the ASCII condition.

It is also not clear whether this different strategy could have caused the difference in performance, or whether the performance difference simply resulted from the difference in shifting the position of the letter chunks on the visual display as opposed to inserting characters in the letter string. This is an important research question which needs further exploration.
The prediction that disrupting organized encoding can decrease performance even if no concurrent secondary task is performed failed to find support, although, note must be taken that this experiment did not adequately test this hypothesis. Recall that participants were instructed to ignore the secondary task stimuli. Since performance was very similar to the pure control condition in Experiment 2, the assumption must be that they simply encoded the letter strings without regard for the interrupting stimuli. This remains a topic which must be addressed in future research.

The last variable tested in the primary analysis was violation location. There was a main effect of this variable, $F(4, 1172) = 75.68 = p<.0001$ which replicated the results of Experiment 2. However, understanding the effect of violation location in this experiment requires examining how this variable interacted with other variables (see Table 12).

First, there was a violation location by task interaction, $F(4, 1172) = 2.49, p<.05$. To further explore this interaction, a one-way ANOVA was conducted for each violation type. As shown in Figure 5, control participants were significantly better than were dual task participants at detecting ungrammatical strings with all types of

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10 Experiment 2 and Experiment 4 used the same methodology, the same subject pool, and were run within the same 12 week time period.
violations, but the differences were not of equal magnitude for all violation locations. In general, differences were larger on strings with ending bigram violations and strings with nonrepeating middle letter violations.

Table 12. Proportion correct across study tasks by violation location for Experiment 4

<table>
<thead>
<tr>
<th>Violation Location</th>
<th>N</th>
<th>Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td>First Letter</td>
<td>305</td>
<td>.83 (.18)</td>
</tr>
<tr>
<td>Beginning Bigram</td>
<td>305</td>
<td>.70 (.19)</td>
</tr>
<tr>
<td>Ending Bigram</td>
<td>305</td>
<td>.77 (.20)</td>
</tr>
<tr>
<td>Illegal Repetition</td>
<td>305</td>
<td>.64 (.24)</td>
</tr>
<tr>
<td>Non-repeating Middle Letter</td>
<td>305</td>
<td>.60 (.22)</td>
</tr>
</tbody>
</table>

* standard deviations for each score are given in parentheses

To fully understand this interaction, significant differences between performance of each group on strings violated in the five different locations was examined also. A Newman-Keuls multiple comparison procedure revealed that for control participants, first letter violations were significantly easier to detect than beginning bigram violations, illegal repetitions, and nonrepeating middle letter violations. Ending bigram violations were detected better than beginning bigram violations, and beginning bigram violations were detected better than illegal repetitions and nonrepeating middle letters, although all of these differences were only marginally significant. Ending
bigrams were detected significantly better than either illegal repetitions, or nonrepeating middle letters, but there was no significant difference between detection of illegal repetitions and nonrepeating middle letter violations for participants in the control condition (see Table 13).

In the dual task condition the pattern of means was similar, but the pattern of significant differences was not. As in the control condition, first letter violations were significantly easier to detect than were beginning bigrams, illegal repetitions, and nonrepeating middle letter violations. But unlike control participants, dual task participants also found first letter violations marginally easier than ending bigram violations. Beginning bigrams were detected significantly better than nonrepeating middle letter violations, a difference which was only marginal in the control condition. Ending bigram violations were detected significantly better than were illegal repetitions, and nonrepeating middle letter violations. And finally, illegal repetitions were detected significantly better than nonrepeating middle letter violation, a difference which was very small in the control condition.

In summary, it seems reasonable to propose that the dual task condition apparently had the effect of magnifying differences in discrimination performance between strings with violations in the five different locations. In
Figure 5. Proportion correct for violation location by task in Experiment 4. Group n's are given in parentheses. Chance performance is .5. Error bars represent 95% confidence intervals.
general, the pattern was the same, but the differences were
greater in the dual task condition. The only exception to
this statement is the difference between detection of ending
and beginning bigram violations in which case the
differences were relatively small and nonsignificant in both
task conditions.

Table 13. Statistics for significantly different pairs of
violation locations by task type in Experiment 4

<table>
<thead>
<tr>
<th>Task Condition</th>
<th>Statistics</th>
</tr>
</thead>
<tbody>
<tr>
<td>Single Task Control</td>
<td>Q(5, 1172)</td>
</tr>
<tr>
<td>First Letter - Beginning Bigrams</td>
<td>5.87**</td>
</tr>
<tr>
<td>First Letter - Illegal Repetitions</td>
<td>8.76**</td>
</tr>
<tr>
<td>First Letter - Nonrepeating Middle</td>
<td>8.87**</td>
</tr>
<tr>
<td>Ending Bigrams - Illegal Repetitions</td>
<td>6.63**</td>
</tr>
<tr>
<td>Ending Bigrams - Nonrepeating Middle</td>
<td>6.73**</td>
</tr>
<tr>
<td>Dual Task Condition</td>
<td>Q(5, 1172)</td>
</tr>
<tr>
<td>First Letter - Beginning Bigrams</td>
<td>5.70**</td>
</tr>
<tr>
<td>First Letter - Illegal Repetitions</td>
<td>8.76**</td>
</tr>
<tr>
<td>First Letter - Nonrepeating Middle</td>
<td>12.63**</td>
</tr>
<tr>
<td>Beginning Bigrams - Nonrepeating Middle</td>
<td>6.93**</td>
</tr>
<tr>
<td>Ending Bigrams - Illegal Repetitions</td>
<td>5.44**</td>
</tr>
<tr>
<td>Ending Bigrams - Nonrepeating Middle</td>
<td>9.31**</td>
</tr>
<tr>
<td>Illegal Repetitions - Nonrepeating Middle</td>
<td>3.87*</td>
</tr>
</tbody>
</table>

* p<.05   ** p<.01

There was also a violation location by chunk
interaction, \( F(4, 1172) = 2.44, p<.05 \). To better understand
this interaction, a one-way ANOVA on chunk type was run for
each violation location. The interaction was apparently
caused by the fact that violations of the ending bigram were
not detected as well by participants who were trained on bad chunks as they were by participants trained on good chunks, $F(1, 304) = 15.32, p < .0001$. Recall that bad chunk training broke up frequent bigrams with the secondary task stimuli. The prediction was that this would result in worse performance at test because participants would not be able to use knowledge of these frequent bigrams for identifying grammatical strings. In the case of ending bigrams, this certainly seems to be a plausible explanation of performance (see Figure 6).

Chunking of strings at training, however, had less effect on strings with violations in other locations. Differences were not as large between performance of participants trained on good chunks and participants trained on bad chunks in detection of strings with first letter violations, beginning bigram violations and illegal repetitions. There was little difference in performance on strings with nonrepeating middle letter violations. Since the chunking manipulation was designed to break up or accent frequent or salient bigrams, it is possible that it had little effect on letter associations in the middle of the string simply because they were in the middle.

In addition to the two previously described interactions, there was a test item type by violation
location interaction, $F(4, 1172) = 2.87$, $p<.05$. Analyses of performance by violation location showed that participants were significantly better at detecting first letter violations in old strings they had seen before than in new strings, $F(1, 304) = 9.48$, $p<.005$ (see Figure 7).

Beginning bigram violations were also significantly more likely to be detected in old than in new test items, $F(1, 304) = 7.49$, $p<.01$, as were nonrepeating middle letter violations, $F(1, 304) = 20.41$, $p<.0001$. Detection of illegal repetitions was marginally better on old test items than on new items. There was, however, no significant difference between performance on old and new test items on ending bigram violations. Performance on old and new test items with all five types of violations was better than would be expected by chance.
Figure 6. Proportion correct for violation location by chunk in Experiment 4. Group n's are given in parentheses. Chance performance is .5. Error bars represent 95% confidence intervals.
Figure 7. Proportion correct for violation location by test item type in Experiment 4. Chance performance is .5. Error bars represent 95% confidence intervals.
Chapter 4 - Summary and Conclusions

The research in this series of experiments was concerned with the extent to which concurrent secondary tasks interfere with implicit learning of an artificial grammar system. Many theorists give a limited processing capacity explanation for the effects of secondary tasks on implicit learning, whether they predict facilitation, no interference, or inhibition of implicit learning under dual task conditions. For example, some researchers say that implicit learning is not dependent on limited capacity resources, and that a secondary task should not interfere with learning (Hayes & Broadbent, 1988; Porter, 1991; Frensch et al., 1994). Others claim that implicit learning is restricted by the same limited capacity processing which affects explicit learning in dual task situations (Nissen & Bullemer, 1987; Green & Shanks, 1993). Still others claim that whether a limited capacity resource is required depends on the complexity of the primary task stimuli (Cohen et al., 1990).

Cleeremans (1993) gives a slightly different explanation. Using a computer simulation of serial pattern learning, Cleeremans demonstrated that there is no need to postulate a limited capacity resource to explain secondary task interference in serial pattern learning. Simply adding noise to the input causes ineffective long term
representation of the stimulus set which, in turn produces a
deficit in learning on some primary tasks but has little
effect on others, depending on the complexity of the task.

Stadler (1995) proposes an explanation conceptually
similar to Cleeremans' (1993). According to Stadler,
disrupting the organized encoding of the stimulus set is
what causes learning deficits. In a series of serial
pattern learning experiments Stadler demonstrated that the
nature of the secondary task determined the extent to which
learning was disrupted. Stadler agrees with Cleeremans that
there is no need to postulate a limited capacity resource to
explain the effects of a secondary task on implicit
learning. By simply defining the organization required to
learn the primary task, it should be possible to predict the
effect a given secondary task is likely to have.

In the only published study on the effects of dual task
conditions on artificial grammar learning to date, Dienes et
al. (1991) reported that a concurrent secondary task
inhibited learning. Dienes et al. hypothesized that
learning artificial grammar strings may require verbal
rehearsal which, according to Baddeley (1987), depends on a
limited capacity resource, and that any secondary task which
also requires this resource is likely to interfere with
grammar learning. Dienes et al. also claimed evidence that
grammar learning has a strongly explicit component relative
to other paradigms traditionally used to investigate
implicit learning. This fact, according to Dienes et al., can account for the fact that dual task conditions interfere with grammar learning while learning in other implicit paradigms is not inhibited by adding a secondary task.

The experiments reported in this paper tested the degree to which several different types of concurrent secondary tasks affected artificial grammar learning as compared to single task control conditions. After an initial training period, participants' knowledge of grammatical strings was tested with either a yes/no grammatical judgement test or a two alternative forced choice test.

In Experiment 1, the effect on grammar learning of two different types of secondary tasks was compared. In addition the two dual task conditions were compared to two single task control conditions. One of the secondary tasks was designed to disrupt organized encoding defined as learning associations between successive letters in grammatical strings. The other secondary task was designed to simply occupy some portion of limited processing capacity while letter strings were encoded. This manipulation was intended to test Stadler's (1995) organizational explanation of learning disruption caused by secondary tasks against Hayes and Broadbent's (1988) and Nissen and Bullemer's (1987) capacity explanation.
Scores were transformed to $d'$ to correct for an observed response bias. Analyses on the d-prime measure indicated that the pure control group and the matched control group performed equally well. The dual task which disrupted encoding of letter associations produced a significant learning deficit relative to the pure single task control, as well as to the matched control. Performance in the memory load dual task condition fell between the matched control and the disruptive dual task, but both these differences failed to reach statistical significance.

Results of Experiment 1 supported the hypothesis that disrupting organized encoding of the letter strings would inhibit learning of artificial grammar strings. The hypothesis that disrupting organization would interfere with learning to a greater extent than simply occupying some portion of limited processing capacity did not find statistically significant support.

A new training methodology introduced in Experiment 2 was intended to strengthen the training task manipulation and to possibly reduce within group variability. In addition, the new testing procedure was intended to eliminate response bias at test and to make results easier to interpret. Another innovation was violating ungrammatical test strings in a systematic way to test for
differential sensitivity to grammatical violations at different positions in the letter strings.

Results of Experiment 2 replicated Experiment 1 with participants in the pure single task control and the matched control significantly better than those in the disruptive dual task. Performance of memory load participants again fell between the matched control and the disruptive dual task and was not significantly different from either. The memory load dual task condition, was, however, marginally different from the pure control group.

As mentioned earlier, the new testing methodology in Experiment 2 revealed a difference in performance between old test items and new test items which was only marginally significant in Experiment 1. Seemingly, there was a difference between knowledge for individual test items seen at study and the ability to use that knowledge to judge new grammatical strings which was not adequately measured by the first type of test. Performance was above chance on both old and new test items, however, indicating that participants may have been doing more than simply memorizing study strings. The limits of this particular study do not allow distinction between the two theoretically opposed positions of how such judgements are made. That is, whether new test items were judged by using abstract knowledge or by simply noticing similarity of new test items to old test items was not tested here.
Additionally, the new test provided the opportunity to test sensitivity to violations at various locations within grammatical strings. As expected, participants were most sensitive to first letter locations, with little difference between old and new test items on strings violated in this salient position. This result indicates that knowledge of valid first letters could be used to judge strings not previously seen. Violations of the penultimate position, or ending bigram violations were detected second most frequently. In this type of violation also, there was little difference between old and new test items. Second letter, or beginning bigram locations, though easily detected, were not as salient as first letters or ending bigrams. Illegal repetitions and nonrepeating middle letter violations were most difficult to detect with no difference between the detection of these two violations. Unlike performance on the first two violation types, participants' ability to detect the latter three types was better on old test items than on new test items. This result indicates that although participants were able to use knowledge they learned to judge strings they had not seen at study, this usable knowledge was not equal for every part of the grammar strings. Participants were particularly sensitive to which letters were legal at the beginning and ends of strings.

Results of Experiment 2 again supported the hypothesis that disrupting organized encoding of the grammar strings at
study inhibited learning. In addition, the prediction that violations would be easier to detect in strings seen at study than in new grammar strings found statistical support; however, this effect was limited to violations in locations other than the beginning and endings of strings. Data also supported the hypothesis that participants would be able to use knowledge gained at study to detect violations in grammar strings they had not seen.

The hypothesis of primary interest, however, again failed to find statistically significant support. Specifically, although participants in the memory load condition performed somewhat better than did participants in the disruptive task condition, the difference was not statistically significant. The result of effect size and power analyses indicated that the reason for failure to find significant difference between these two dual-task groups could have been the combination of a small effect and inadequate sample size. In addition, large within-group variability was thought to have contributed to the problem in Experiment 1.

Experiment 3 returned to the original methodology of Experiment 1 to explore the possibility that a secondary task could have two different effects on the organization of the primary task. Rather than always producing disruption of organization, under certain circumstances, the secondary task was predicted to have a beneficial organizing effect.
This effect was predicted based on Servin-Schreiber and Anderson’s (1991) chunking effect, and Stadler’s (1993) consistent pauses condition which produced facilitated learning of the primary task stimulus set.

Secondary task stimuli were used in Experiment 3 to chunk study strings in either a beneficial way or a disruptive way, according to Servin-Schreiber and Anderson’s (1991) classification. Experiment 3 produced an effect of task, with control participants performing better than dual task participants, but no significant effect of type of chunk. The lack of significant results in this experiment was thought to be primarily due to the same methodological problems experienced in Experiment 1; therefore, in Experiment 4 the chunking manipulation was attempted using the new methodology.

To review the design of Experiment 4, there were two levels of task type, disruptive dual task and matched control single task. There were also two levels of chunk, good chunk training which emphasized features of grammatical strings by grouping high frequency bigrams and trigrams and bad chunk training which broke up high frequency bigrams and trigrams in training items.

The pattern of means of Experiment 4 replicated that of Experiment 3. Analyses also revealed a significant task by chunk interaction in which good chunk training was significantly better than bad chunk training, but only for
the dual task group, not for the matched control. Overall, however, control participants always out performed dual task participants.

These results indicate that chunking strings with a secondary task which grouped high frequency bigrams and trigrams was not as detrimental to performance as a secondary task which broke up high frequency bigrams and trigrams. If bad chunking is considered to disrupt organization of the stimulus set while good chunking preserves it, this finding supports the hypothesis that disrupting organized encoding inhibits learning. It also supports the result that a secondary task may inhibit learning, even if it does not disrupt organization. Thus, the results of this experiment replicate the findings of the first two experiments.

The chunking manipulation did not produce facilitated performance relative to the matched control, as expected based on the effects Servin-Schreiber and Anderson (1991) obtained from chunking study strings. The real test of facilitation, however, is how performance of participants receiving good chunk training compares to the baseline established by the single task pure control participants in Experiment 2. Comparing mean performance of the pure control group (M = .76) to the mean performance of the dual task group receiving good chunk training (M = .70) reveals that no facilitation occurred. In fact, the difference
between this dual task condition and the pure control was very similar to the difference between the dual task and its own matched control condition.

One explanation for the failure to replicate Servin-Schreiber and Anderson's facilitation effect is the difference in presentation of the stimuli in the two experiments. In Servin-Schreiber and Anderson's paradigm, participants saw the whole grammar string with spaces inserted to produce chunks. Participants were not instructed to remember anything about the chunking. Thus, there was no additional memory load associated with chunking the strings, and participants could have benefited from seeing the whole string as well.

In the paradigm used in this experiment, dual-task participants never saw a whole letter string presented. But since control participants never saw a whole string either, it is unlikely that this factor can account for the failure to find enhanced performance of the group trained on good chunks relative to the matched control group. The difference could be due the fact that as letters were presented one at the time, chunking was accomplished by inserting a secondary task stimulus which was also to be remembered. Thus, the chunking manipulation itself increased the memory load for participants in the dual task conditions relative to the single task conditions. The additional memory load could account for the fact that dual
task participants always performed worse than single task control participants even when letter strings were chunked in a beneficial way. However, this still does not explain why facilitation was not found in the single task control condition trained on good chunks.

As mentioned above, the matched control groups saw the secondary task stimuli but were instructed to ignore it. Since strings were presented with secondary task stimuli inserted which supposedly chunked the study strings, an organizing effect which could have facilitated performance was predicted. When mean performance for the matched control participants trained on good chunks (M = .76) is compared to the pure control condition from Experiment 2 (M = .76), it is clear that no facilitation occurred.

Examination of the mean performance for the matched control participants trained on bad chunks (M = .75) reveals that this group also performed at a level comparable to the pure control group from Experiment 2. In fact there appears to be no difference what so ever in performance between control participants who saw the strings with and without the secondary task stimuli inserted. Since participants in the matched control conditions were instructed to ignore the secondary task stimuli, and performance matches participants who did not see the secondary task stimuli, it seems reasonable to assume that they followed instructions and behaved as if the secondary task stimuli were not present.
This could explain the lack of facilitation. Participants who ignored the secondary task stimuli encoded strings in the same way as participants who did not see the strings chunked. In order to test whether good chunking actually could facilitate learning in this paradigm, it would be necessary to use a secondary task which would ensure that participants encoded strings as good chunks, but would not create an additional memory load at the same time. For example, as grammar strings were presented, a secondary task stimulus could appear between adjacent letters and participants could be required to make a judgement about the orientation of a secondary task stimulus immediately upon its presentation rather than being required to remember it until the entire grammar string was presented. In order to adequately test the hypothesis that chunking with a secondary task may have a facilitatory effect, further research should be conducted.

In addition to task type and chunk, a third between-subjects variable was added in Experiment 4. By adding two new distractor types which were judged to be relatively less verbalizable than the arrows used in previous experiments, Dienes et al.'s (1991) implication that the verbalizability of the secondary task stimuli may affect the extent to which the secondary task interferes with learning was tested. The two new distractor types were ASCII characters and shifted distractors. In the shifted distractor condition,
rather than a character inserted into the letter string, participants saw the letters on the computer screen displaced vertically and were required to remember the changes in position as a secondary task.

There was no main effect of distractor type; however, this variable did enter into a significant three-way interaction with chunk type and the between-subjects variable test item type (old versus new strings). Specifically, participants in the shifted distractor condition who trained on good chunks were not as good at using what they learned to judge new test items as were participants in the other distractor conditions. Their performance on old test items was as good as participants in the other conditions, however. In addition, participants in the shifted distractor condition who trained on bad chunks performed worse on old test items than participants in the other distractor conditions, while their performance on new test items was no worse than that of participants in the other distractor conditions. In other words, training on good chunks with shifted distractors did not seemed to hurt specific knowledge of old items, but did inhibit participants' ability to use knowledge gained to judge new strings, relative to training with other distractor types. Training on bad chunks with shifted distractors also hurt specific knowledge of training items relative to other distractor types, although the reason that this happened is
not clear. It seems as though the research reported here has barely begun to explore the effects of secondary task stimuli type on artificial grammar learning. Further research is needed to fully understand these effects.

Experiment 4 also revealed that the effect of violation location interacted with several other variables. In general, the pattern of mean performance for detection of different violation locations was similar to that of Experiment 2 with the exception that detection of illegal repetitions was better than detection of nonrepeating middle letter violations, a difference which was not found in Experiment 2. This difference was only significant, however, in the dual task groups and not in the control groups. In general, differences between detection of the five different violations was greater in the dual task groups than in the control groups, indicating that performing the chunking secondary task may have emphasized some positions in training strings making violations in some locations more salient than violations in other locations.

There was also a violation location by chunk interaction. Post hoc tests revealed that participants trained on good chunks were better at detecting ungrammatical strings with violations in all locations than were participants trained on bad chunks although the difference was significant for ending bigram violations only. Violation location also interacted with test item
type because participants in all groups were better at using knowledge gained to judge new test items when the ungrammatical string of the test pair had an ending bigram violation. Clearly, participants were able to use knowledge of valid ending bigrams to judge items they had seen, as well as items they had not seen. Perhaps because there were only three valid ending bigrams making them salient cues, or perhaps simply because the visual salience or semantic association of the ending pairs was easy to remember.

In addition to conclusions from each individual finding, several general conclusions can be drawn from this set of experiments. First, the results of these experiments taken together tentatively support the idea that a simple limited processing capacity explanation for the effect of a secondary task on learning of artificial grammar strings may not be sufficient. Although the findings must be viewed with caution due to failure to attain statistical significance, the task which only required holding information in memory did not seem to inhibit learning to the same extent as the task which disrupted organization. The fact that this small effect was obtained in two experiments with relatively few participants indicates that more research on this question should be conducted and that perhaps an alternative explanation is required.

Another explanation for the effects of a secondary task on implicit learning is Stadler’s (1995) organizational
hypothesis. Specifically, his idea is that any secondary task disrupts primary task learning to the extent that it disrupts organized encoding of the stimulus set to be learned. According to Stadler, this is the best explanation for the effects of secondary tasks on serial pattern learning. Results of these experiments, however, indicate that the organizational hypothesis alone may not be the best explanation of the effects of a secondary task on artificial grammar learning. While disrupting organized encoding of the letter strings did inhibit learning relative to the comparable control condition, requiring participants to maintain information in memory while encoding inhibited learning also.

Results of these experiments seem to indicate that learning artificial grammar strings may require both organized encoding of the letter strings at study and some minimal amount of a limited capacity resource. A definitive conclusion on this matter must await further research, however, because the disrupting stimulus in these experiments was held in memory along with the grammar string as it was being encoded. Thus, the disruptive task also had a memory component. In order to provide a conclusive result, future research must include a task which simply interrupts processing while strings are encoded but does not require that anything be held in memory.
The second general conclusion is that even a secondary task which disrupts in a way which should promote organization of the stimulus set, may not facilitate artificial grammar learning. In fact, in these experiments, the task which was predicted to organize encoding of strings inhibited learning relative to the single task control condition. This effect could have occurred because the chunking task increased memory load also, and as previous results indicated, holding information in memory inhibited learning. The beneficial organization effect might better be tested by interrupting with a task which does not require holding information in memory while strings are encoded.

The third general conclusion is that, based on performance greater than would be expected by chance, dual-task participants were able to detect violations in five different locations indicating that they learned positional dependencies of letters in grammatical strings. Dienes et al. (1991) reported that participants performing a concurrent random number generating task were unable to learn such dependencies. In these experiments, not only did participants display better than chance knowledge of strings they had studied, but in addition, were able to use their knowledge to judge strings they had not seen.

The fourth conclusion is that different types of secondary task stimuli affected learning differently. Shifted spatial position as a secondary task did not produce
the same pattern of results as inserting characters into the letter string as evidenced by the three-way interaction between distractor type, chunk type and test item type. Whether this difference was because the spatial shift really was less verbalizable than the other two types is difficult to determine based on information collected in the one experiment which explored this issue.

It is clear from this limited information that the issue of secondary task stimulus type needs to be explored further. Perhaps the verbalizability of the secondary task stimuli could be made more distinguishable. For example, some researchers have used snowflake patterns as relatively nonverbalizable stimuli (Neath, 1993). Perhaps using snowflakes versus characters with easily verbalizable labels, such as arrows could clarify the effect of this variable. A condition could also be added in which participants are required to perform articulatory suppression while encoding the grammar strings to rule out competition for Baddeley’s (1987) phonological loop as an explanation for secondary task learning interference.

Finally, a very simple conclusion which can be drawn from this set of experiments is that the type of test used, and structure of ungrammatical test items are important influences on the results obtained in grammar learning experiments. This conclusion should cause careful re-
examination of many findings in the grammar learning literature and the conclusions drawn from them.

In summary, this set of experiments demonstrates that artificial grammar strings can be learned under dual task conditions, and furthermore, that knowledge gained may be applied to determine the grammaticality of strings not seen during training. Additionally, although some degree of learning decrement was produced by all secondary tasks tested here, the nature of the task, as well as how it disrupted organization of the primary task seemed to affect the degree to which learning was inhibited. This research also demonstrated that not only the nature of the secondary task, but the nature of the secondary task stimuli is likely to have an effect on learning, although the exact determinants of this factor were not fully explored by these studies.

Returning to the original topic which prompted this research, the question must be asked again, "Is implicit learning robust under dual task conditions?" As mentioned in the introduction, empirical evidence for the robustness of implicit learning in dual task situations to date has been scarce, and results have been far from conclusive. The primary cause of the lack of conclusive evidence may be what Reber (1993) refers to as "the polarity fallacy (p. 23)." Reber (1993) explains that in order to validate the existence of implicit learning as a psychological
phenomenon, researchers and theorists have discussed implicit and explicit learning as though they were two separate, independent activities which do not interact. The truth, according to Reber (1993), is that it is much more likely that the two processes are complementary and cooperative.

Berry and Dienes (1993) also describe implicit-explicit learning as lying on a continuum, rather than being binary in nature. According to Berry and Dienes (1993) and Cleeremans (1993), most tasks involve both implicit and explicit components, and the particular mixture of learning which occurs is determined by the situation. This fact makes it very difficult, indeed, to say with conviction that implicit learning is robust, or is not robust based on results using one and only one paradigm, such as artificial grammar learning.

What can be said, based on the experiments reported herein, is that participants are able to gain knowledge of artificial grammar strings under dual task conditions, and that they can use that knowledge to judge the grammaticality of both strings they saw at study and of strings they have not previously seen. In this sense implicit learning can be said to be robust under dual task conditions because it was not completely suppressed by addition of the secondary task. This is provided that grammar learning is assumed to be at least partially based on an implicit process.
On the other hand, learning was inhibited by the concurrent performance of a secondary task. This learning decrement could be attributed to the fact that implicit learning suffers, or is not entirely robust, under dual task conditions. Alternatively, it could be attributed to the fact that artificial grammar learning is not purely an implicit process. Research has demonstrated that participants are able to give explicit justifications for choices which can account for discrimination performance (Dulany, et al., 1984; Perruchet & Pacteau, 1990, 1991) indicating that grammar learning is partially explicit.

If Dienes et al.'s (1991) assessment that grammar learning is largely explicit is true, the research presented here could be interpreted to indicate that explicit learning may not suffer to the extent commonly believed under dual tasks situations (Hayes & Broadbent, 1988; Porter, 1991). However, to counter Dienes et al.'s claim, research has shown that participants incapable of the level of strategic processing as those usually tested demonstrate learning in artificial grammar experiments (Knowlton, Ramus, & Squire, 1992; Roter, as reported in Reber, 1993). Thus, it is reasonable to assume that artificial grammar learning has an implicit component.

Careful consideration of previous research within the artificial grammar paradigm indicates that it is not unreasonable to argue that performance in artificial grammar
learning reflects both implicit and explicit processes. This being the case, a definitive answer to the question of robustness of implicit learning must wait until the relative contributions of implicit and explicit learning are assessed. The results obtained here, together with Dienes et al.'s (1991) results do support the contention that learning under dual task conditions in the artificial grammar paradigm is dependent on the nature of the interaction between the primary and secondary tasks. Perhaps this interaction is dependent on the particular combination of implicit and explicit learning which takes place in a given situation. Future research must address the extent to which artificial grammar learning reflects implicit versus explicit processing.

Another important question which must be addressed in any research is the extent to which the results may be generalized, or used to predict behavior in the general population. The generalizability of findings in this series of experiments is limited by the fact that participants were not selected from the general population but were university students. Although the undergraduate population of most universities is much more diverse in terms of gender, age, race, and socioeconomic status than it was in previous generations, the fact remains that university students in general score well above average on tests of cognitive ability which could have affected the findings of this
research. Notice should be taken, however, that the experiments were designed so that previous knowledge or skill involving use of the computer were of no benefit. Additionally, experimental stimuli were such that previous knowledge of such systems by the participants was highly unlikely. Nonetheless, caution should be exercised when generalizing findings to populations which were not represented among the participants of the experiments.

If there is a real life implication involved in the hypotheses tested in this series of studies, it must lie in the fact that as the pace of everyday life increases, people rarely go about any task without being interrupted. Many times people are required to perform two tasks simultaneously. Does the interruption, or performing that second task interfere with knowledge gained about the first task? For example, can a secretary learn the subtleties of his or her new job situation while constantly being interrupted by a ringing telephone? The results of the present research support the idea that secondary tasks do interfere to some degree with cognitive processing of the first task; however, the extent of the decreased processing efficiency may depend on the specific processing demands of the two tasks. Of course, again, these results must be viewed cautiously because of limitations described above. But even with its limitations, basic laboratory research involving how two demanding tasks may interact and influence
performance could have an impact on development of technology to help deal with the ever increasing pace of life. Investigation of the interaction of complex tasks not only promises new insight into basic psychological phenomena, but perhaps solutions to some of life’s applied problems as well.
References


St. John, & Shanks, D. R. (in press). Implicit learning from an information processing standpoint. In D.


## Appendix

### Study List A

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**A:** Items from study list A  
**B:** Items from study list B

Note: Second column contains ungrammatical test items. Grammatical and ungrammatical items were presented in random order at test.
Informed Consent Form

I understand that all participants in this experiment are volunteers and that I can withdraw at any time from the experiment and that I have been or will be informed as to the nature of the experiment, that the data I provide will be anonymous and my identity will not be revealed without my permission, and that my performance in this experiment may be used for additional approved projects, that I shall be given an opportunity to ask questions prior to the start of the experiment and after my participation is completed my questions will be answered to my satisfaction.

Date ________________________________  Signature ________________________________
Vita

Barbara Cochran graduated Summa Cum Laude from Southeastern Louisiana University in 1989 with a Bachelor of Arts in Psychology. While at Southeastern, she worked as a research assistant in the Department of Psychology. As a result of the research done at Southeastern, Barbara co-authored a paper which was published in the psychology journal, Memory and Cognition, as well as several presentations and posters presented at the annual meetings of the Psychonomic Society.

Barbara entered the graduate program at Louisiana State University in the fall of 1990 working as a research assistant and a teaching assistant in the Psychology Department while pursuing her graduate degrees. Her research interest include language acquisition, implicit or incidental learning, and the nature and acquisition of generative systems.

After earning a Master of Arts degree in Psychology in 1992, she began teaching Psychology courses at Louisiana State University in addition to continuing her research. She has also been the instructor for a Continuing Education independent study course for one year and is currently writing another independent study course.

In the future, Barbara hopes to remain in South Louisiana while pursuing a career conducting research and teaching Psychology.
DOCTORAL EXAMINATION AND DISSERTATION REPORT

Candidate: Barbara Cochran

Major Field: Psychology

Title of Dissertation: Artificial Grammar Learning in a Dual Task Paradigm

Approved:

Janet McDonald
Major Professor and Chairman

Dean of the Graduate School

EXAMINING COMMITTEE:

Date of Examination:

7/19/96

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