THE EFFECT OF VARYING LEVELS OF REINSTATEMENT ON PRESCHOOLERS' MEMORY FOR LOCATION

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Thesis submitted to the

Department of Psychology

in partial fulfilment of the requirements for the

Masters of Science Degree

Department of Psychology

Memorial University of Newfoundland

1992

St. John's Newfoundland

Abstract

Although there has been a recent increase in research concerning the possible beneficial influence of various factors on retention-test performance, most of these factors have been examined in isolation. The present experiment was conducted in order to compare within one study some of the factors which are known to affect performance on retention tests, permitting a direct comparison of these effects. Various types (or levels) of event re-presentation (reinstatement treatments) were employed, namely, a test trial, a study trial, a reactivation treatment, or no reinstatement treatment (control) during the retention interval. Although these factors involve differing levels of reinstatement treatments, the question was whether they would differentially affect subsequent recall performance. A paired-associate task involving the learning of the locations of 16 familiar objects (item-location pairs) by 3-year-olds was used. Three weeks later, three of the four groups of children were visited again and exposed to one of three reinstatement treatments. The reinstatement treatment was employed in all cases to only half of the original study set. The fourth (control) group was not visited during this interval. This was followed 1 week later by a retention test, consisting of four consecutive test trials, on the locations of all 16 items using a cued-recall procedure. It was found that: (a) reinstatement treatment, regardless of method, was shown to be an effective way of increasing the amount recalled; (b) study was the superior type of reinstatement treatment, with no significant differences between a test trial and a reactivation treatment; (c) reinstatement treatment applied to part of a list appeared to show some spread to other list items, but this effect did not reach significance; (d) hypermnesia was observed across test trials, independent of other factors. These results replicate previous findings that study is the best method of increasing future recall. In addition, the results also attest to the powerful effect of test trials on retention-test performance as well as point to the importance of using more than one test trial in order to fully assess the contents of memory.

Acknowledgements

I would like to express special thanks to my advisor, Dr. Mark L. Howe, for all his encouragement, help, and time in the conducting of this research and writing of this paper. I would also like to thank Dr. Mary L. Courage for her many helpful comments and suggestions; as well as all her time; and my employer, Dr. Julia T. O'Sullivan, for her encouragement and for all the space she has given me over the past year in order to complete this paper. In particular, I would like to thank my family for all their support and patience throughout the course of my studies. I know there must have been times when they were wondering when it would finally be all over!

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Interest in children's retention has recently been fuelled by questions arising in the legal system concerning the reliability of young children as witnesses. Research has focused on the accuracy of children's memory, especially on determining which factors influence the reliability of children's memory between the time that they witness an event and the time they recall it (the retention interval). In particular, one major concern in this area has been to determine what factors enhance a child's memory for an event. A variable that has been found to play a major role in increasing performance on long-term retention tests is event reinstatement. I will first present and discuss three possible levels of event reinstatement, followed by an experiment designed to compare these processes.

Originally, reinstatement was defined as "periodic partial repetition of an experience such that it maintains the effects of that experience through time" (Campbell & Jaynes, 1966, p. 478). Campbell and Jaynes (1966) proposed that the presentation of weekly shock "reinstatements" over their 1-month retention interval served to prevent or forestall forgetting. Thus, they viewed reinstatement as a procedure which maintained the strength of a response that had been learned previously. Later, Spear and Parsons (1976) reformulated the concept of reinstatement. They demonstrated that the use of periodic repetitions throughout the retention interval was not necessary in order to produce superior long-term retention-test performance but rather that a single exposure prior to the end of the retention interval produced similar results. They defined this reminder-like procedure as a "reactivation treatment." Spear and Parsons (1976) found that a single shock exposure presented 24 hours prior to their 28-day retention test restored performance to its original post-training level. From this, they concluded that through the use of a reactivation treatment forgetting could be overcome or alleviated. Thus, the major difference between Campbell and Jaynes' (1966) and Spear and Parsons' (1976) use of the terms "reinstatement" and "reactivation treatment," respectively, was a procedural one. Campbell and Jaynes' reinstatement procedure involved periodic repetitions of an event several times after the initial exposure which they proposed functioned to <u>prevent</u> forgetting from occurring whereas Spear and Parsons' reactivation treatment involved a single reminder-like event presented shortly before the retention test in order to <u>overcome</u> forgetting that had already occurred.

As pointed out by Rovee-Collier and Hayne (1987) forgetting is operationally defined as "a decrement in performance after a retention interval" (p. 200). There are two alternate theoretical accounts for forgetting (also see

Rovee-Collier & Hayne, 1987, p. 199-200). One accounts for forgetting in terms of input or storage failure (the memory is no longer available in it's original form), whereas the other states that forgetting is the result of retrieval failure (the memory may be available but it has become inaccessible). Campbell and Jaynes (1966) proposed that their use of repeated "reinstatement" presentations over the retention interval served to maintain the memory over time. However, Spear and Parsons' (1976) demonstrated that a single reminder just prior to the retention test also served to increase later retention-test performance. This later finding may be taken as evidence that a forgotten memory may actually be available and intact but may have become inaccessible over the retention interval and that the 'reactivation treatment' serves to increase the accessibility of the memory attributes.

However, it is now realized that forgetting is not a simple all or none process (see Howe & Brainerd, 1989) but rather a matter of degree. Thus, the procedure (whether termed reinstatement or reactivation treatment) could serve to forestall further forgetting as well as to overcome forgetting that had already occurred, and it may not be practical to employ such procedural and theoretical distinctions. Furthermore, in recent years, the terms reinstatement (for example, Gatti, Pais, & Weeks, 1975;

Hoving, Coates, Bertucci, & Ricco, 1972) and reactivation (for example, Hars & Hennevin, 1990; Rovee-Collier & Shyi, 1992) have come to be used interchangeably in the literature, often leading one to perceive them as being two different names for the same concept. In an effort to clarify these two terms for the purpose of the present thesis, I have operationally defined a reinstatement treatment to refer to <u>any</u> re-experiencing of the original event which may result in superior performance on long-term retention tests for the original experience. We could postulate that many levels of re-experience (or reinstatement treatment) are possible. This re-experiencing could theoretically vary from little or no re-experiencing to a complete re-experiencing of the event. A reactivation treatment, on the other hand, will refer to one specific type of re-experience. Specifically, a reactivation treatment is defined here as a specific type of reinstatement treatment whereby a simple reminder of the originally learned event (i.e., some subset of cues or features from the original event) is presented to the subjects after acquisition and before the long-term retention test. Therefore, a reinstatement treatment is the general term which refers to any replication of an event and which by definition includes a reactivation treatment.

In addition to reactivation treatments there are other types of event re-presentation (reinstatement procedures). Two of the most important are multiple study opportunities and multiple testing sessions. Theoretically, both of these factors can be thought of as reinstatement treatments. Specifically, if one concedes that no two study opportunities are identical (e.g., contextual features may shift and/or different subsets of the features may be encoded during each study opportunity [see Flexser & Tulving, 1978]), then, at least hypothetically, each study trial is different from every other study trial. Even though a later study trial would involve the re-presentation of the original item-response pairs, this subsequent study trial can never be a complete replication of the original event because there will always be some external and/or internal stimuli that cannot be controlled and may be different due to the simple fact that time has elapsed. In fact, if the study trial is not part of a consecutive series of study trials, but rather, occurs after a considerable time delay (e.g., days or even weeks), then the contextual features may have changed to an even larger degree between study sessions and may represent only a partial, not a complete, replication of the original event. Consequently, a study trial which occurs several days or weeks after an initial study, but prior to a retention test, may, in theory, not be identical to the initial study opportunity but rather constitute only a partial replication (or reinstatement) of that event.

Similarly, a test trial can be conceived of as involving some level of replication of the original event. A test trial, in particular a cued-test trial, involves presenting subjects with part of the original event and asking them to recall the remainder (in the absence of immediate feedback). In this way part of the original event is re-experienced to some degree. The level of re-experiencing may depend upon the proportion of the items that the subject recalls. If the subject gets all items correct, then the test trial may in reality act similarly to a study trial. During a test trial though, unlike a study trial, the subject is given no indication from the experimenter as to the accuracy of his or her responses. During a study trial, in contrast, the experimenter is providing the subject with accurate pairings. A test trial, like a simple reactivation treatment, would in most cases involve a less comprehensive re-exposure to the original event than would a study trial. Thus, an interpolated test trial or a reactivation exposure may both involve lesser degrees of reinstatement treatment than does a study trial. Of course, the lowest possible level of re-exposure would be the total absence of such an opportunity, or no reinstatement treatment.

Three factors have been proposed to increase performance on long-term retention tests: (a) multiple study opportunities, (b) reactivation exposures, and (c) multiple testing sessions. These three factors involve varying amounts of reinstatement treatments. A more comprehensive discussion of these three reinstatement procedures will be contained in subsequent sections.

Study Effects

One factor traditionally viewed as most effective in increasing recall, both in adults and children, is for a person to have multiple experiences with an event (multiple study trials). There appears to be little disagreement in the literature with the concept that memory for recurring events is more accurate than memory for one-time occurrences. It has been shown that multiple study trials at acquisition lead to better memory performance and better performance at retention than a single study trial (Howe, 1991; Slamecka & Katsaiti, 1988). Howe (1991) examined Kindergarten and Grade 2 children's recall of a story after acquisition. Subjects were either exposed to one study trial or criterion learning. Howe found that the criterion group recalled significantly more propositions. This effect should also hold true if the study trial occurs days or even weeks after the initial study trial. Although criterion learning was utilized for this study, one would assume that after a 3-week delay some forgetting had occurred. Previous studies have demonstrated forgetting after shorter time periods, even when criterion learning was employed. Howe, Kelland, Bryant-Brown, and Clark (1992) showed forgetting over a 16-day interval, and Howe (1991) showed forgetting after 9 days. The introduction of a study-trial reinstatement treatment during a retention interval should also serve to increase subsequent recall by increasing the amount of material remembered at that point in time. This method would parallel current teaching methods whereby something may be studied over a period of days or a student may be required to re-study material covered in class as part of his or her homework. Review classes or tutorials would also reflect the belief that later repetition of study facilitates recall.

Reactivation-Treatment Effects

Other studies (Hars & Hennevin, 1990; Hoving et al., 1972; Rovee-Collier & Shyi, 1992) have also shown that simply being re-exposed to some cue(s) or feature(s) present in the original event, i.e., a reactivation treatment, leads to substantial benefits on later retention tests. Even though very little work has been conducted on the effects of reactivation treatments with preschoolers or even older children, reactivation-treatment effects are not new in the area of memory research. An extensive body of research has been implemented with animals (for review, see Miller, Kasprow, & Schachtman, 1986) and with human infants (for review, see Rovee-Collier & Shyi, 1992). In the following sections I will provide a brief overview of not only the small amount of research that has been conducted on the topic of reactivation procedures with young children (Hoving & Choi, 1972; Hoving et al., 1972), but also of that conducted with both animals and human infants.

Reactivation-Treatment Effects in Animals

The use of reactivation procedures is not new in the animal literature (for review, see Miller et al., 1986). It has often been referred to as pretest cuing and defined empirically as "a cuing procedure consisting of exposing the subject to some part of the original learning situation without submitting it to a complete learning trial" (Hars & Hennevin, 1990, p. 365). Although terms including reinstatement, reactivation, and cuing have been used interchangeably to refer to the same or a similar concept in the literature, these studies all deal with reactivation treatments, as defined in the present paper. Animal studies have shown improved retention-test performance with the use of pretest cuing using long-delay retention intervals (Deweer, Sara, & Hars, 1980; Gatti et al., 1975; Hars & Hennevin, 1990). The early work on the topic of reactivation procedures carried out by Campbell and Jaynes (1966) was conducted on rats. They found that when young rats were presented with partial repetitions of their original training experience during the retention interval there was decrease in the learned fear performance deficit. This effect was not contingent on learning that occurred through the brief repetitions (reactivation episodes) themselves, because a control group receiving only the reactivation treatment with no prior conditioning did not show this learned fear.

A more recent instance in the animal literature was a series of studies conducted by Hars and Hennevin (1990). Their experiment involved maze running in rats. The rats were given a mild shock if they entered blind alleys. Twenty-five days later the experimental rats were given shock as a cue (reactivation treatment), followed shortly thereafter by a retention test. These rats showed improved retention-test performance over that of their non-reactivated counterparts. Hars and Hennevin were successful in illustrating how memory performance can be modified by the pretest presentation of a cue related to the target memory.

Similar studies have been conducted by Gatti et al. (1975) and Deweer et al. (1980). Both studies found that a simple reminder via re-exposure to some stimuli from the original training was sufficient to produce a significant decrease or elimination of a performance decrement over the 25-day retention interval, which the authors interpreted as curbing forgetting. A more comprehensive overview of the relevant animal literature can be found in either Hars and Hennevin (1990) or Miller et al. (1986).

The more recent question has been, whether reactivation treatments which have been utilized to reduce long-term retention-test performance deficits in animals can also have similar effects with humans. The largest body of research with humans in this area has been conducted by Rovee-Collier and her associates (Rovee-Collier & Hayne, 1987; Rovee-Collier, Patterson, & Hayne, 1985; Rovee-Collier & Shyi, 1992). She examined the effects of reactivation treatments on human infants' memory of a conditioned foot-kick response.

Reactivation-Treatment Effects in Human Infants

Rovee-Collier and her associates (for review, see Rovee-Collier & Shyi, 1992) have examined infant memory performance using a conjugate reinforcement paradigm. Infants, ranging in age from 2 to 6 months, who learned a particular contingency (e.g., the relation between leg kicks and movement of an overhead mobile), were tested for immediate and delayed retention. Their standard reactivation-treatment paradigm involved: (a) allowing sufficient time following training for forgetting to occur, (b) the presentation of a reactivation treatment (some cue or feature from the original acquisition session, such as simply returning the child to a highly distinctive training context or placing the infant in an infant seat with the mobile overhead, etc.), and (c) testing for retention of the response at some point after the reactivation treatment (usually 24 hours). Rovee-Collier and her associates found that following the reactivation treatment, infants' performance remained at the same high rate on the long-term retention test as it was on the retention test immediately after training, which had occurred days or even weeks earlier. They found, in contrast, that both of their control groups [(a) infants who received no reactivation treatment, and (b) infants who received a reactivation treatment without prior training on the contingency] responded at their baseline rates. Their research has shown that by using a reactivation procedure one can extend an infant's memory. In particular, reactivation procedures may increase long-term memory performance to the extent that "infants might be able to remember for weeks, months, or perhaps even years" (Rovee-Collier & Hayne, 1987, p. 231).

Reactivation-Treatment Effects in Young Children

Hoving et al. (1972) set out to determine if reactivation-procedure effects found in animals (for review, see Miller et al., 1986) were also present in children. Children between the ages of 5 and 11 years were divided into three groups. Two groups were required to learn a standard paired-associate task and to relearn the same task again 8 weeks later. For one of these two groups (reactivation-treatment group), the pairs were repeated in a story 4 weeks after the initial learning session. A third group received only the reactivation treatment (the pairs presented in a story) followed by the learning of the pairs 4 weeks later. In this design retention of the original learning was measured by the number of test trials required to reach criterion on the final session, the assumption being that the fewer trials required, the greater the retention. Hoving et al. (1972) found that the group who had the pairs repeated to them in a story (reactivation treatment) during the retention interval required significantly fewer trials to relearn the pairs 8 weeks later. Through the addition of the third group they were able to show that this increased retention-test performance was not due to learning caused by the reactivation treatment, because exposure to the pairs alone was not sufficient to produce learning of the pairs. Their results provided evidence that a brief and relatively indirect "reminder" presented during the retention interval was sufficient to produce superior long-term retention-test performance in these children.

Hoving and Choi (1972) were concerned with determining which types of cues were necessary and sufficient to produce reactivation-treatment effects. Forty first graders learned a paired-associate task and relearned the same task 8 weeks later. Reactivation treatment occurred 4 weeks after acquisition. At this time the children were divided into five groups differing in the type of reactivation-procedure cues used. While one group received no reactivation treatment, the other four were exposed to either the stimulus items only, response items only, stimulus items paired with response items, or the ten stimulus items plus ten additional items. Their results indicated that presentation of the response was necessary in order to produce reactivation-treatment effects and that presentation of the stimulus items alone or with the additional items did not improve memory performance. Thus it was necessary, at least for their task, to present the response items during the reactivation session in order for the facilitating effects of the reactivation treatment to occur. Through their research Hoving and his associates have been successful in showing that the effects of a reactivation procedure previously exhibited in animals (e.g., Hars & Hennevin, 1990; Miller et al., 1986) also occurred in children from 5 to 11 years of age.

Test Effects

Test effects can be looked at from two perspectives: (a) by examining the effects of consecutive testing within a retention session (hypermnesia), or (b) by examining the effects of tests occurring across retention sessions (reinstatement procedure). Recently, Howe (1991; Howe & Brainerd, 1989; Howe et al., 1992) has shown that multiple test trials lead to substantial increases in recall. The concept of a net increase in recall across consecutive test trials, hypermnesia, is not a new concept (for review, see Howe et al., 1992). Hypermnesia was first studied by Ballard (1913), who found increased recall as a function of test trials with such stimuli as nonsense syllables, poetry, meanings of latin nouns, diagrams, prose, and ideas. Recently, considerable research has focused on this phenomenon. Erdelyi (1982, 1984) has found reliable increases in the amount recalled across successive tests with college students. Similar results have also been observed by Runquist (1986, 1987). Although these studies were conducted with adults, recently Howe (1991) conducted similar research on young children. Howe found that reminiscence (the recollection of something which was previously unrecallable) occurred across test trials and increased the probability of correct factual recall in a story situation involving the use of misleading information in children as young as 5 years. Hypermnesia has been shown to occur with a large variety of stimuli as well as across a large age range (see Howe et al., 1992).

However, a test trial reinstatement procedure refers to a process whereby a single test trial is presented after acquisition and before the long-term retention test(s) which may lead to superior performance on the long-term retention test(s). In a recent review article, Richardson (1985) pointed out that interpolated retention tests (between acquisition and the final long-term retention test) can have substantial positive effects on delayed retention-test performance. He stated that interpolated recall tests may produce a 'resistance to forgetting.' The majority of the studies in the past have concentrated on this effect in adults. However, recently Howe et al. (1992) have found a significant decrease in performance decrements following the administration of a previous test of retention in children as young as 7 years.

Fivush and Hammond (1989) studied test effects with young children in a different context. They examined the effects of repetition of the experience and time since experience on 2-year-olds' ability to recall novel play events. Twenty children participated in unusual laboratory play events. Half of them returned twice, once after 2 weeks and again 3 months later. The other half returned only once, after the 3½-month retention interval. Fivush and Hammond found that their repeated exposure group, who received a recall test during the 3½-month retention interval, recalled as many items after 3½ months as they did at 2 weeks, with the 3½-month only group recalling significantly less items overall. The results of these studies indicate that an interpolated test trial seems to guard against subsequent long-term retention-test performance deficits. Similar to study, this method of repeated testing (i.e., mid-terms, final exams, etc.) is analogous to that utilized in many school systems. (Although presumably students would restudy the material in preparation for an upcoming test. However, in some cases, testing may occur in the absence of study opportunities as in the case of surprise tests.)

Summary

The literature presented here demonstrates that by introducing either an additional study session, test trial, or reactivation treatment during a retention interval, one can effectively increase performance on long-term retention tests. In past research these three factors have been studied in isolation, and to date no research has been published comparing these three elements directly. Thus, although it is evident that these three factors differ procedurally, the question remains as to whether, when compared directly, they exhibit relatively different effects on long-term retention-test performance. In other words, is one method more effective at increasing retention-test performance over long intervals than another?

The Robustness of Preschool Children's Memories

Because the present study is concerned with the memories of preschool children, a cursory overview of a few relevant findings pertaining to young children's event memory follows. (For a comprehensive review, see Howe & Courage, in press) Briefly, for some time it was assumed that young children had very poor memories. Recently, however, people have realized that this is not necessarily the case (for reviews, see Howe & Brainerd, 1989; Howe & Courage, in press). Recent studies have found that children develop a robust memory system early in life. A study by Hudson and Nelson (1986), comparing 3-, 5-, and 7-year-olds' scripts and episodic memories, found no differences in the actual memory limitations between these three age groups. They proposed that children organize and retrieve their autobiographical memories similarly to adults. Fivush and Hammond (1989) examined an even younger group of children. They investigated 2-year-olds' ability to recall novel play events over a 3½-month retention interval. No significant effects of age were found between the 24- and 28-month-olds, and children's recall was generally accurate. Bauer and Shore (1987) looked at yet younger groups. Elicited imitation was used to examine 17- to 23-month-olds recall of event sequences using both immediate and 6-week recall tests. They found that, although memory for familiar event sequences was superior to that of novel ones, these young children could reenact the events quite adeptly.

The studies presented here are merely a few of the many which demonstrate that even very young children are quite adept at recalling events which have occurred in both the recent and distant past. As Fivush, Gray, and Fromhoff (1987) concluded, "2-year old children are recalling a great deal of accurate, organized information about personally experienced events, even if those events occurred in the distant past" (p. 408).

Although people once thought of young children's memories as being unreliable and inaccurate, many now realize that more credibility should be given to the memories of even very young children. As Rovee-Collier and her associates (Rovee-Collier & Hayne, 1987; Rovee-Collier, Patterson, et al., 1985; Rovee-Collier & Shyi, 1992) have recently shown, even an infant's memory can be quite good. Although the effects of reinstatement treatments have been investigated in both infants and young school-age children, no equivalent investigation of the matter has been conducted with preschoolers.

Spread of Reinstatement-Treatment Effects

Finally, it is important to consider how reinstatement treatments might affect memory performance for items that were not directly re-experienced. Recent work by Rovee-Collier and her colleagues' with 3-month-olds may provide some insight into these effects. Rovee-Collier has found that merely re-exposing the infants to the highly distinctive training context (a highly distinctive bumper pad) was an effective reminder 2 weeks after the conclusion of training (Rovee-Collier, Griesler, & Earley, 1965). Thus, re-presentation of only contextual cues was sufficient to increase performance on long-term retention tests within the context of her conjugate reinforcement paradigm. It was not only the stimulus-response association that was remembered over the retention interval, but also the context in which it had been learned. This raises the possibility that some degree of "spread of reinstatement-treatment effects" may occur. Thus, are parts of the original learning experience which do not receive any direct reinstatement treatment actually being indirectly reinstated, or are the effects of a reinstatement treatment confined to the part(s) of the original event which directly received the reinstatement treatment?

There is some disagreement in the literature (for review, see Anderson, 1980) as to what the nature of a 'trace' (or cognitive unit) really is, but many researchers would concede that even if each item within a list context is stored separately there exists some degree of interconnectedness among them. As long as one allows for some degree of association among list items, it is conceivable that spread of reinstatement-treatment effects could occur.

Evidence that some degree of "association" occurs within a to-be-remembered list can be appropriated from the literature on context effects. For example, Smith (1979) found that university students remembered significantly more when they were returned to the room in which they had originally studied the list than when tested in a different environmental context. This area of research can be taken as evidence that at least some amount of contextual information is stored with a learned list and thus some degree of association between items (and context) must exist as a whole.

Alternatively, research in the area of part-set cuing (for review, see Nickerson, 1984) may lead one to conclude that individual items within a to-be-remembered list may not necessarily be associated in such a way as to facilitate recall. Within a part-set cuing paradigm, subjects are typically given a relevant set of cues and asked to recall words from a list they have just learned. These cues may be either a subset of the actual list words, category names (representative of the to-be-remembered list), or category instances (which represent the categories included in the original list but are not actual list members). Nickerson (1984) pointed out that the majority of studies in this area have often found inhibition effects rather than facilitation. However, Nickerson also points out that "a better understanding is needed, both of the conditions under which facilitation and inhibition occurs, and of why either occurs when it does" (p. 531). As Basden (1973) pointed out, one problem that exists with many of these studies is that it is not at all clear how well learned the list was. Most of the part-set cuing research has involved very few study trials; in most cases only one. When Basden (1973) employed criterion learning to a part-set cuing procedure, he did indeed find that the cued subjects retained significantly more criterion items than did those subjects who had not received the cues. In a recent article Sloman, Bower, and Rohrer (1991) proposed that part-set cuing inhibition "was governed in part by an incongruency principal: Inhibition occurs to the extent that [part-set] cues induce a retrieval framework different from that used to encode list items" (p. 974). When comparing the use of incongruent cues, congruent cues, and no cues Sloman et al. (1991) found that inhibition only occurred with the group who received the incongruent cues.

Hence, the notion of a possible spread of reinstatement-treatment effects is not necessarily contradictory to the findings with respect to the part-set cuing literature. In fact, Sloman et al. (1991) found no inhibition when congruent cues were used, and when Basden (1973) employed criterion learning within a part-set cuing paradigm, facilitation effects emerged. Thus, one could actually predict the appearance of facilitation effects through the use of congruent cues within a criterion-learning procedure based on the part-set cuing literature.

As pointed out by Slamecka and Katsaiti (1988), two types of hypotheses as to the origin of reinstatement effects stand out in the literature: learning (or storage-based) hypotheses and retrieval-based hypotheses. According to the learning hypothesis proposed by Slamecka and Katsaiti, a prior re-presentation of items (traditionally a test trial) acts to increase learning (restorage) of whatever items can be remembered by virtue of their rearousal during the reinstatement procedure. On the other hand, pure retrieval-based hypotheses would suggest that reinstatement-treatment benefits result from the re-presentation of items providing the subject with an additional opportunity to enhance the retrieval skills that are necessary for later recall. These retrieval skills are enhanced by the prior re-presentation being regarded as an opportunity for the subject to hone their skills in retrieving the list items.

If one accepts Slamecka and Kaitsaiti's (1988) learning hypothesis, then clearly spread of reinstatement-treatment effects are not possible, because the reinstatement effects would not only be confined to the items which were directly re-experienced, but also only to the subset of the directly re-experienced items which the subject could remember at that time. In contrast, spread of reinstatement-treatment effects could be possible via a retrieval-based hypothesis. If the re-presentation of items acts to hone the retrieval skills relevant to this item set as a whole so that items studied in the same list context become more readily accessible on later tests, then these improved retrieval skills should increase the accessibility of all the items studied in that list context and not only those which were actually re-experienced (and remembered). This is assuming a commonality of retrieval operations across tests. If, on the other hand, the prior test instead activates an irrelevant set of operations, then it will not improve recall over a control group.

Slamecka and Katsaiti (1988) performed a series of experiments designed to compare these two theories. Their basic procedure involved presenting the subject with 30 paired associates for either one, two, or three study trials. Half of the subjects were then given a filler task while the other half were given a cued-recall test on the 30 pairs. All subjects were subsequently given a filler task followed by an immediate cued-recall test on one third of the pairs. All subjects returned twice more, after 1 and 5 days, for two more recall tests, each on a different third of the pairs.

According to Slamecka and Katsaiti, one major difference between the learning and retrieval-based hypotheses is their predictions regarding observed rates of forgetting over multiple retention tests. With a retrieval-based hypothesis, stress is placed on the honing of retrieval skills, and through this extra retrieval practice a subject shows improved retention-test performance. Slamecka and Katsaiti (1988) state that "a retrieval-based notion would have to predict a lesser loss rate for the experimental subjects, because their specially practiced skills are assumed to be declining more slowly than the unpracticed skills of the controls" (p. 721). According to their learning hypothesis, on the other hand, differences should be observed in terms of absolute numbers of items recalled and not in rates of forgetting. That is, the experimental group's recall should be superior on both (or all) of the retention tests, but its rate of loss over the retention interval(s) should be similar to that of the control group. Slamecka and Katsaiti's results provided support for the learning hypothesis with college students. They found the predicted difference in absolute numbers, as observed on the immediate retention test, and not in the rate of forgetting, as the observed slopes did not differ from Day 1 to Day 5. Slamecka and Katsaiti's findings are in contrast to the majority of the work done in the area of repeated testing (e.g., Runquist, 1986, 1987) where forgetting is pointed out as being a retrieval phenomenon and that memory storage is stationary.

An alternative to these two theories has recently been proposed by Howe and Brainerd (1989). Their trace-integrity model of long-term retention provides a framework for interpreting changes in storage- and retrieval-based components of amnesia (net decrements in performance) and hypermnesia (net increments in performance). According to this model, changes in retention performance could arise through alterations in the trace itself (changes in what is stored), changes in the accessibility of the trace (changes in the retrievability), or both. Here, storage and retrieval can be seen as components of a single underlying memory continuum, which they labelled "trace integrity," whereby storage equals early stages of integration and retrieval equals later stages of integration. Memory traces are viewed as collections of primitive elements that have become bonded during encoding. Memorability is thus determined by the degree to which these bonds are intact. Traces that are intact following the acquisition of information may undergo disintegration across the retention interval leading to forgetting of either the retrieval or storage sort. In either case, redintegration provides the mechanism that can lead to trace reinstatement in the storage-based and/or the retrieval-based form. Redintegration is a process in which the activation of some of the trace's features spreads to the other features in the trace (Horowitz & Prytulak, 1969), producing a net increase in the trace's level of integration. Thus, forgetting may be due to failures in availability, failures in accessibility, or both. Hence, this theory, too, would allow for the possibility of some degree of spread of reinstatement-treatment effects occurring.

Howe and Brainerd (1989) also suggested that a distinction must be made between the effects of successes and errors on later retention tests. Not only did Slamecka and Katsaiti (1988) claim that a prior test on some of the original items did not affect the remaining items, but also, that it was only that subset of items for which the subject provided a correct response that resulted in better recall. A correct response, they argued, provided the subject with an additional 'study' trial and it was this 'study' trial that lead to better retention-test performance. Slamecka and Katsaiti (1988) found evidence, with college students, to support the notion that a prior test on some of the original items did not act to facilitate recall on later retention test of the other items, but they never provided a direct comparison of errors versus successes. They merely stated that the learning hypothesis made the claim that it was only the items which the subject could remember at that time which would result in beneficial effects on later retention-test performance and that their data favoured the learning hypothesis. Yet, Howe and Brainerd (1989) suggested that, in their own way, errors too can have beneficial effects across retention-test trials. Howe et al. (1992) presented a series of experiments with both children and adults which provided evidence that both errors and successes on prior tests can have beneficial effects on subsequent long-term retention-test performance.

A major procedural difference existed between the studies conducted by Howe (Howe & Brainerd, 1989; Howe et al., 1992) and the work of Slamecka and Katsaiti (1988). While Howe's work used criterion learning, Slamecka and Katsaiti used a fixed number of study trials. It has been pointed out (for reviews, see Howe & Brainerd, 1989; Howe et al., 1992) that a fixed number of acquisition trials fails to equate levels of learning across subjects and items. Thus, easier items are learned better than more difficult ones. Howe (1991), in a direct test of one-trial versus criterion learning, has shown that significantly more storage failures occurred within the one-trial learning paradigm. Thus, it is possible that the results obtained by Slamecka and Katsaiti may reflect unmeasured variations in original learning. It is possible in Slamecka and Katsaiti's study that it was only the easier items that were fully integrated into the trace and that the harder items were not completely stored and thus were not an integral part of the trace. Thus, these items may not have been susceptible to spreading activation. In addition, once criterion learning is imposed evidence can be found to support the idea that errors, as well as successes, can have beneficial effects on retention (see Howe, 1991).

Present Study

Although previous research has been conducted to examine the effects of an interpolated study trial, test trial, and reactivation treatment on a later retention test, no study has been published to date (not with children nor with human infants or animals) looking at how different levels of reinstatement procedures might differentially affect forgetting. The present study was conducted in order to examine the relative effects of these three reinstatement treatments within the realm of one study and to see, in addition, whether reinstatement-treatment effects would spread to items which did not receive any direct reinstatement treatment.

These questions are of interest for several reasons. First, these three levels of reinstatement treatments all pertain to real-life situations that can be found in eyewitness testimony and may be of importance in legal cases. These concepts also play an integral part in our education system. Which factor has the most beneficial effect on learning? Clearly, these three levels of reinstatement treatments differ substantially in method, but do their effects on retention-test performance really differ? Is one method more effective at improving subsequent recall performance than another?

In order to effectively compare these factors, the following study used an event which would be of real-life significance to preschoolers, a type of hide-and-seek game. Subjects first learned 16 item-location pairs to a fixed criterion then, following a 3-week interval, were subjected to one of four conditions. The four conditions represented four different levels of reinstatement treatments: (a) a study trial on half of the original items, (b) a cued-recall test on half of the original items, (c) a reactivation session on half of the original items (by simply showing subjects the items), or (d) no reinstatement treatment (a control group which was not visited at all during the retention interval). They were then tested, via four consecutive test trials, 1 week later on all 16 of the original items. The purpose of the 3-week delay between the original acquisition and the reinstatement treatment was to ensure that enough time had elapsed for forgetting to occur. The control group, which received no reinstatement treatment, served as a baseline measure of the 'normal' rate of forgetting over the entire 4-week time frame. It was predicted that all three reinstatement procedures would serve to increase performance on subsequent long-term retention tests above that of the control condition. Four consecutive test trials were used in the retention-test phase to enable us to look for hypermnesia across consecutive test trials (see Howe et al., 1992). The application of the reinstatement treatment to only half the items provided a test of whether reinstatement-treatment effects spread to the items which did not directly receive a reinstatement treatment. If the reinstatement-treatment effects spread, then recall for the eight items which did not receive a reinstatement treatment should be similar to that of the eight items which did receive a reinstatement treatment. The level of performance on the long-term retention tests in the control group, however, should be significantly less (i.e., control < 8 non-reinstated items ≈ 8 reinstated items). If reinstatement-treatment effects do not spread, then recall of the eight non-reinstated items (those which did not have a reinstatement treatment applied to them) would be expected to be worse than that of the eight items which did receive a reinstatement treatment, with the non-reinstated items being approximately equal in recall to the control group (i.e., control ≈ 8 non-reinstated items < 8 reinstated items). The use of this within-subjects manipulation allowed for each subject to act as his or her own control, thus, resulting in a more powerful test.

The use of a test-trial reinstatement procedure allowed for an examination of the power of errors versus successes at enhancing long-term recall in preschoolers. This type of investigation can only be conducted with the items which received the reinstatement treatment within the test condition because measures of previous successes and errors only exist here. If, as Slamecka and Katsaiti (1988) claimed, only correct responses act to reinstate (maintain, or re-kindle) the trace, then performance on the long-term retention tests for the items which the subjects recalled correctly should be significantly better than that of the items on which they erred (i.e., correct > errors). However, if errors also served to reinstate the trace (for preschoolers), then performance on the long-term retention tests for the items on which the subjects erred should be similar to that of the items which they recalled correctly (i.e., correct ≈ errors). The item set within the test-trial reinstatement-treatment group which received a direct reinstatement treatment allowed for testing of these effects. Comparisons of the long-term retention-test performance of those items for which correct versus incorrect responses were given to on the first test trial (the reinstatement-treatment test trial) for the subset of eight items which were tested at the time of reinstatement treatment will be discussed.

The present design allowed for examination of whether the effects of a reinstatement treatment applied to one item set would spread to the other non-reinstated item set (which did not have the reinstatement treatment applied to it) as well as allowing for examination of whether the relative power of the three reinstatement treatments differed for the items which directly received a reinstatement treatment than for those which did not. It is possible that reinstatement-treatment effects may spread in some conditions (for example, test trials) but not for others. In this case, the 'hierarchy' of the reinstatement procedures may be different for those items which have received an 'indirect' reinstatement treatment than for those which received a direct reinstatement treatment. (For example, a possible order at retention-test performance for the directly reinstated items may be test > study > reactivation treatment but for the indirectly reinstated items it may be totally different, reactivation treatment > study > test). These present manipulations allowed for the investigation of hierarchial differences between the reinstatement procedures when applied directly and indirectly. These results, if present, would show up in a condition by item set (reinstatement treatment vs. not) interaction.

Method

Subjects

The subjects were 100 (50 males and 50 females) preschool children (mean age = 3 years 3.73 months; SD =

4.13 months). All children were registered in one of the preschool/daycare programs in the St. John's area at the time of testing. Only those children for whom consent was obtained from both the director of the preschool/daycare centre and the child's parent(s) were tested. (See Appendix A for parental consent form.)

Stimuli

The stimulus set consisted of 16 different familiar small toy objects (an airplane, ball, book, car, box of cards, cow, box of crayons, eraser, glasses, rubber man, mouth organ, rose, scissors, spoon, watch, and yo-yo). The items were chosen so as to be familiar to children in this age range and were from different semantic categories (except, of course, the toy category).

Design and Procedure

The experiment consisted of three major phases, an acquisition phase, a reinstatement-treatment phase, and a retention-test phase. The acquisition phase involved the memorization of the correct location of 16 items. The reinstatement-treatment phase involved either showing the subjects eight (half) of the original items which they previously saw being hidden (reactivation treatment), giving them an additional study trial on eight item-location pairs, or giving them a cued-recall test on eight item-location pairs, with the control group not being visited at all during this time (no reinstatement treatment). The long-term retention-test phase involved testing the children for their memory of the locations of all 16 items. Subjects were randomly assigned to one of the four conditions with the stipulation that there be an approximately equal number of males and females in each condition. During all sessions the children were tested individually in a quiet room at their preschool/daycare centre.

Acquisition. The acquisition phase involved a series of study and test trials that were continued until the child reached a criterion of remembering the correct locations of all 16 items for two consecutive test trials. Prior to the first study trial, the experimenter showed the child all 16 items and asked him/her to name them. For any item which the child did not name, the experimenter told the child what it was. (This happened very rarely and occurred exclusively with two items, box of cards and yo-yo. All problems were labelling problems and did not involve the children not actually knowing what the items were. One major problem may have been the fact that the cards were in a box rather that actually being visible. All of the children who did not know what the yo-yo was called, knew how to use it.)

On the first study trial the experimenter took one item at a time and hid it somewhere in the room so that the child saw exactly where it was located. While doing this, the experimenter also said: "See, the X is hidden under (or in, etc.) the Y." The experimenter continued this until all 16 items were hidden. The hiding places for each item remained constant for every child at that preschool/daycare centre. The experimenter then said: "Okay now I want you to try and tell me where each thing is hiding. I am going to name something and I want you to show me where it is. Do not take any of the things out of their hiding places or peek at them[1], just stand next to where you think each thing is and point to the place where it is hiding. Okay now, where do you think the X is?" The child was then asked where each individual item was until all 16 items were tested. When all 16 items were tested, the experimenter went around to all of the hiding places and showed, as well as told, the child which item was hidden there. This procedure was used for all study trials after the initial one, with a series of study and test trials continuing in this manner until the child successfully located all 16 items on two successive test trials. The order of re-showings across study trials and cuing across test trials was randomized for each child across trials, with the provision that a minimum of five items intervene between the study and test of any item. This was done in order to control for serial position and short-term memory effects, respectively.

Reinstatement treatment. All children in the three reinstatement-treatment conditions returned with the experimenter to the test room in their preschool/daycare centre 3 weeks after acquisition. Equal numbers of children were randomly assigned to one of the four conditions at this time. The control group of children received no reinstatement treatment and thus was not visited until the end of the 4-week retention interval. The first reinstatement-treatment group of children was shown eight of the original 16 items and allowed to handle them for a few minutes while talking to the experimenter (reactivation treatment). The second group was given one study trial (with no test phase) on eight of the original items (as per the acquisition procedure), while the third group was given one cued-recall test trial (with no study phase) on eight of the original items (as per the acquisition procedure). The 16 item set was randomly divided into two sets of eight items for the purpose of reinstatement treatment. Half of the children in each of the reinstatement-treatment conditions were randomly assigned to each subset of eight items.

Retention-test phase. All children returned with the experimenter to the test room 4 weeks after the original acquisition phase for a retention test. The retention test involved four successive cued-recall test trials (without further study opportunities), like the test trials in the acquisition phase. Items were tested in random order across trials (as in the acquisition phase). Children did not see the item-location pairs again prior to, nor during, this testing. At the end of the four trials the child was shown where the items were and were praised for how well they did.

Results

As previously discussed, there is a potential confound between performance at acquisition and performance at retention tests in studies of long-term retention. Even though an attempt was made to control for differences in the levels of learning across items and subjects at acquisition, by using criterion learning, we still cannot make the assumption that learning was equivalent for each item and each subject at the end of acquisition (see Underwood, 1964). For example, in the absence of formal modelling (see Howe, 1991; Howe et al., 1992), it is quite possible that unmeasured levels of over-learning may have occurred for some subjects and on some items. A normal analysis of variance may not control for such problems because an analysis of variance on the acquisition data is not sensitive enough to detect differences in over-learning that may exist between the conditions. As pointed out in Howe (1991), there are many problems with the procedures and analytic methods used to deal with retention-test data, and a more analytically precise measure must be used, such as his proposed formal trace-integrity model. However, if one wishes to use more traditional ways of analyzing these types of data, one possibility remains: to perform an analysis of covariance using total errors at acquisition as the covariate. In this way, any discrepancies in over-learning that may occur at acquisition will be accounted for by the covariate and partialed out of the analysis.

One analytical problem still remains with the present data. Using traditional linear modelling techniques, there is no direct way of analyzing this design. The data lend themselves to a 2 (Item Set: reinstatement treatment vs. not) X 3 (Reinstatement-Treatment Condition) X 4 (Trial) + 1 (Control group) analysis of covariance. There is no appropriate analytical tool available to analyze this design. Thus, we are left with two possible modes of analysis: (a) a 7 (Sub-conditions) X 4 (Trials) analysis of covariance or (b) a 2 (Item Set: reinstatement treatment vs. not) X 4 (Condition) X 4 (Trial) analysis of covariance.

The first design involves the splitting up of the three reinstatement-treatment conditions into six sub-conditions, one each for the reinstatement-treatment and non-reinstatement-treatment item sets. This procedure results in seven conditions: study/reinstated items, study/non-reinstated items, test/reinstated items, test/non-reinstated items, reactivation/reinstated items, reactivation/non-reinstated items, and control. This analysis also leads to a violation of the analysis of variance/covariance's assumption of independence. When analyzing the data via this design we are treating the item sets which receive a direct reinstatement treatment and those which do not within each condition as being independent, which they are not, because the reinstated/non-reinstated item manipulation is a within-subjects manipulation.

The second design involves leaving presence of reinstatement treatment across item sets as a within-subjects factor and having four overall conditions (including the control). In order to accomplish this, the control group's items must be split in half, resulting in a 'reinstated' and a 'non-reinstated' item set. This split represents the same within-subjects manipulation as exists within the study, test, and reactivation-treatment conditions. Although this procedure is arbitrary and does not represent any real manipulation, it does not violate any of the assumptions of the analysis of variance/covariance. Splitting the items from the control group also provides for an item check to ensure that no differences existed between the subsets. The 16-item set was broken down in the exact same way as they were for the experimental conditions with the same eight items being assigned to each subset as for the reinstatement-treatment conditions and the same counterbalancing done when assigning each subset to either the reinstatement-treatment or non-reinstatement-treatment subset for each subject.

Because the second design does not violate any statistical assumptions and it has the greater intuitive appeal, it is the one which will be reported here. (It should be noted that when the first design was employed and corrected F's were used [to correct for the problem of the conditions being correlated] the same results were found.)

The proportion of locations correctly recalled on the retention tests was analyzed using a 2 (Item Set: reinstatement-treatment items vs. not) X 4 (Condition: study vs. test vs. reactivation treatment vs. control) X 4 (Trial) analysis of covariance, where the item set and trial factors were within-subjects and condition was a between-subjects factor. The total number of errors at acquisition was the covariate. The covariate (errors at acquisition) was found to have a significant effect on retention-test performance [$F(1,95) = 22.28$, $p < .001$, $MS_e = 0.2185$, $r^2 = 0.1569$]. Thus, we were correct in taking this into account, as the acquisition and retention-test results were confounded. With this confound removed, we have a much purer analysis of the retention-test data. The results of this analysis will be presented in terms of the factors investigated.

Reinstatement-Treatment Effects

A main effect of item set [$F(1,95) = 14.54$, $p < .001$, $MS_e = 0.0446$] was found, indicating that the items which received the reinstatement treatment ($M = 0.8016$) were better recalled than those which did not (the non-reinstated items) ($M = 0.7462$). Thus, the reinstatement treatments employed did improve performance on the long-term retention tests for the re-exposed items.

Hypermnesia

A main effect of trial [$F(3,288) = 15.97$, $p < .001$, $MS_e = 0.0078$] was also observed. As is evident in Figure 1, recall increased across test trials. Thus, hypermnesia did occur. Further analysis of this effect (Newman-Keuls, $p < .05$) indicated that recall increased significantly across trials from Trial 1 ($M = 0.7400$) to 2 ($M = 0.7706$) to 3 ($M = 0.7900$) but levelled off after Trial 3 with Trial 4 ($M = 0.7950$) showing no significant increase in recall over Trial 3. This (hypermnesia) effect was independent of both condition and item set, as no significant interactions with trial were found.

Hierarchy and Spread of Reinstatement-Treatment Effects

There was no main effect of condition [$F(3,95) = 1.83$, $p > .05$, $MS_e = 0.2185$], indicating that, overall, one type

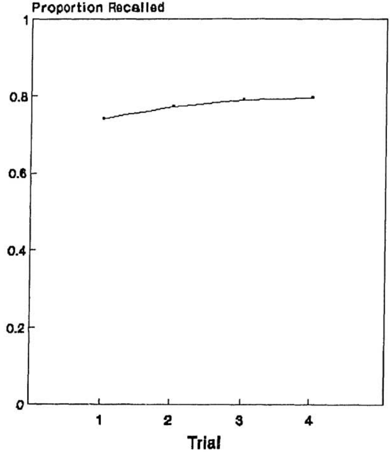

Figure 1. Mean proportion recalled on the long-term retention test as a function of test trial (based on the adjusted means obtained from the analysis of covariance).

of reinstatement treatment was not superior to another nor to the control group. Yet, the Condition X Item Set [$F(3,95) = 3.45$, $p < .05$, $MS_e = 0.0446$] interaction was significant. (For means, see Table 1.) Post-hoc tests carried out on the adjusted means obtained from the analysis of covariance (Newman-Keuls, $p < .05$) revealed that: (a) recall of the items which received a reinstatement treatment in all three of the experimental conditions (study, test, and reactivation treatment) was better than that of the control group (either the 'reinstated' or the 'non-reinstated' subset); with study being superior to both test and reactivation (i.e., study > test = reactivation > control); (b) none of the three experimental sets of items which were <u>not</u> re-presented (study/non-reinstated, test/non-reinstated, nor reactivation/non-reinstated items) differed significantly from either the control group, nor each other (i.e., study = test = reactivation = control); and (c) the 'reinstated' and 'non-reinstated' items did not differ within either the control (as would be expected because this was only a division based on convenience rather than any actual experimental manipulation), test, or reactivation groups; but recall of the 'reinstated' items was found to be superior within the study group (i.e., reinstated items > non-reinstated items for study group; reinstated = non-reinstated for test, reactivation, and control groups).

Table 1

Mean Proportion Recalled as a Function of Condition and Reinstatement Treatment (based on the adjusted means obtained from the analysis of covariance)

Condition	Item Set		
	Reinstatement Treatment	No Reinstatement Treatment	Overall
Study	0.8800	0.7502	0.8151
Test	0.8145	0.7757	0.7951
Reactivation	0.8033	0.7514	0.7773
Control	0.7088	0.7075	0.7081
Overall	0.8016	0.7462	

Thus, although recall of the items within the three experimental conditions which did not receive a direct reinstatement treatment was better than that of the control group and worse than that of the items which did receive a direct reinstatement treatment, such differences were not reliable. In summary, if ones looks at each level of reinstatement treatment separately, what was found was that although the items which received a reinstatement treatment differed from those which did not for the study condition, they did not differ from each other for the other three conditions (test, reactivation treatment, and control).

What this interaction tells us is that the reinstatement treatment behaved differently for the different conditions and thus led to different hierarchies. First, for the reinstatement-treatment item sets, a distinct hierarchy was evident, where study was seen to be the superior alterative over both a test trial and a reactivation treatment. Although test and reactivation were not significantly different from each other, they were superior to control (thus, study/reinstated > test/reinstated ≈ reactivation/reinstated > control). No hierarchy was evident in the item sets which did not receive a direct reinstatement treatment as the four conditions did not differ significantly.

A hierarchy of the 'directness' of the reinstatement procedure is evident from the means obtained; with the items which received a direct reinstatement treatment exhibiting the highest recall (M = 0 8326 [mean of the study, test, and reactivation reinstated items]), the items which may have received some indirect reinstatement-treatment effects (or non-reinstatement treatment) next (M = 0.7591 [mean of the study, test, and reactivation non-reinstated items]), and the control (or no reinstatement-treatment condition) exhibiting the poorest recall (M = 0.7081); but these differences (direct reinstatement treatment > indirect reinstatement treatment > no reinstatement treatment) did not reach significance. Thus, the presence of any spread of reinstatement-treatment effects is statistically inconclusive.

The Influence on Retention-Test Performance of Successes Versus Errors

To examine whether a success on the final long-term retention test was more likely given that a success also occurred on the earlier test-reinstatement trial, a Z-test was conducted on the conditional and unconditional probabilities of successful recall. Here, the conditional probability is the probability that an item that was correct on the reinstatement-treatment test trial was also correct on the first retention-test trial. The unconditional probability is the overall probability of getting these test/reinstated items correct on the first trial of the retention-test sequence. The idea here is that, consistent with Slamecka and Katsaiti (1988), if the conditional probability is significantly higher than the unconditional probability, then correct responses on the test trial at reinstatement treatment are the best predictors of correct responses on the retention test. If no significant difference between the two are observed, then correct responses on the reinstatement-treatment test trial do not serve as a better predictor of correct responses on the retention test than does the overall fact of being presented these items during the reinstatement treatment. This test revealed [$Z(1) = 2.22$, $p < .05$] that the conditional probability (0.90) was significantly higher than the unconditional probability (0.80). Thus, as Slamecka and Katsaiti (1988) would predict, getting a response correct on the reinstatement-treatment test trial was a better predictor of a correct response on the first retention-test trial.

Discussion

The present study examined whether (a) a hierarchy of effective reinstatement treatments exists, (b) hypermnesia is dependent or independent of the type of reinstatement procedure employed, (c) reinstatement of part of a list spreads to other list members, and (d) successes on a reinstatement-treatment test trial best predict future successes on long-term retention tests. Each of these issues will be discussed in subsequent sections.

One important aspect that emerges from the present study is the actual robustness of the memories of young preschoolers. Even the control group, which received no form of reinstatement treatment, recalled more than 70% of the pairs over a 4-week retention interval, with the other groups recalling more than 80% of the items which received a reinstatement treatment. This alone provides a clear indication of the memory capabilities of these young children. As has been previously pointed out (e.g., Fivush et al., 1987; Hudson & Nelson, 1986), obviously one should no longer dismiss the recollections of young children as necessarily being entirely unreliable or inaccurate. In fact, it is evident that even preschoolers and infants (see Rovee-Collier & Shyi, 1992) are capable of recalling a great deal of accurate information about past events.

Reinstatement-Treatment Effects

Clearly the present findings indicate that re-experiencing (reinstatement treatment) an event, whether through an additional study experience, test trial, or reactivation exposure, serves to enhance 3-year-olds' recall for that event (within the context of the present study). Thus, reinstatement-treatment effects previously observed with animals (e.g., Deweer et al., 1980; Gatti et al., 1975; Hars & Hennevin, 1990), human infants (e.g., Rovee-Collier & Hayne, 1987; Rovee-Collier & Shyi, 1992), and older children (Hoving & Choi, 1972; Hoving et al., 1972) are also relevant for preschoolers.

In the present study differences were observed as a function of the type or level of reinstatement treatment employed. As expected, all three manipulations, study, test, and reactivation treatment, served to increase retention-test performance above that of the control group. As one would also expect, study was the superior method of reinstatement treatment for the items which received a direct reinstatement treatment. But what about the other two conditions? With respect to the items which received a direct reinstatement treatment, a test-trial reinstatement treatment was shown to be as effective as a reactivation treatment. Specifically, for items which received a direct reinstatement treatment, a study opportunity was best, followed by test and reactivation (i.e., study-reinstated items > test/reactivation-reinstated items > control). Therefore, consistent with conventional thought, study was once again shown to be the superior method of improving performance on subsequent long-term retention tests.

The fact that a test-trial reinstatement treatment was just as effective at improving long-term retention-test performance as a reactivation session, would probably not be a surprising finding to Howe et al. (1992) because they found that a test-trial session introduced during either a 16- or 30-day retention interval served to attenuate forgetting on a later retention test in children as young as 7 years. In fact, for a group given an interpolated 16-day retention test during a 30-day retention interval, retention-test performance was shown to be just as good on Day 30 as it was on Day 16. Comparable results were also observed with college students by Runquist (1986, 1987). The present finding confirms that such effects are also evident with preschoolers and are comparable in strength to traditional reactivation-treatment effects.

Hypermnesia

Once again, hypermnesia appeared as a strong aspect of multiple test trials within a retention test. The results of the present study provide strong support for the concept of hypermnesia previously reported by several researchers (for review, see Richardson, 1985). Howe et al. (1992) found hypermnesia in children as young as 7 years, and the present research shows this effect in children as young as 3 years. Recall increased by an average of 4.5% across the retention-test sequence despite the absence of intervening study opportunities. An important finding which emerged from the present data is that the presence of hypermnesia was independent of the manipulations employed. Hypermnesia occurred in the same strong and consistent way regardless of the type of reinstatement treatment that was implemented during the retention interval (study, test, or reactivation treatment) and independent of whether this reinstatement treatment was implemented at all (reinstatement-treatment items, non-reinstatement items, or control [no reinstatement treatment]). None of these factors had any significant effect on hypermnesia. Regardless of the resultant increase in retention-test performance due to a reinstatement treatment, further increases in the form of hypermnesia continued to emerge. Hypermnesia is therefore a very enduring property of consecutive test trials, independent of other factors.

The preceding findings also indicate the importance of using more than one trial on retention tests in order to be sensitive to what is in memory, as has been attested to by Howe and Brainerd (1989). If only one test trial is employed, we may not be measuring the full extent of what is in memory. Through the addition of further trials it becomes obvious that there is more information in memory than a single test trial can measure. Studies of retention must therefore take this factor into account within their designs and should implement multiple test trials in order to fully assess the contents of memory.

Spread of Reinstatement-Treatment Effects

Although the indirectly reinstated items (non-reinstatement-treatment item subset) were recalled better for all three 'reinstatement-treatment' groups (study, test, and reactivation) than for the control (no reinstatement treatment) group and worse than the items which received a direct reinstatement treatment (i.e., directly reinstated items > indirectly reinstated items > no reinstatement-treatment items), these differences were not large enough to reach significance. It may be the case, though, that because the indirect (or non-) reinstatement-treatment items did not differ significantly from the items which did receive a direct reinstatement treatment, for the subjects in the test and reactivation groups, that some degree of reinstatement-treatment effects must have spread or else these items would have been recalled in a similar quantity to that of the control items and significantly below that of the items which did receive a direct reinstatement treatment. This pattern is not unlike typical patterns that are often seen across test trials where, in a three trial recall-test sequence, for example, recall increases significantly from Trial 1 to Trial 3 but Trial 2 does not differ significantly from either. In this case, clearly most would accept the notion that recall increased steadily from Trial 1 to Trial 3. Thus, I propose that this is what may be going on in the case of the 'spread' of reinstatement-treatment effects; recall performance increases steadily from the no reinstatement-treatment (control) group to the indirectly reinstated items, to the items which were exposed to a direct reinstatement treatment (although significance is not obtained).

The fact that the items for the experimental conditions, which did not receive a direct reinstatement treatment, neither showed significant differences from the items which did receive a direct reinstatement treatment nor from the no reinstatement-treatment control groups' items, may actually be telling us something about memory. If reinstatement procedures only function to hone the retrieval skills relevant to this item set as a whole, then the amount recalled for the experimental groups items which did not receive any direct reinstatement treatment should have been comparable to that of their items which did receive a direct reinstatement treatment, because anything which serves to improve relevant retrieval skills should do so for all the items studied in that list context. On the other hand, if reinstatement procedures only operate at a storage level, then it would only facilitate recall of those items that were actually re-presented. Thus, recall for the items which were not re-presented should then be comparable to that of the control (no reinstatement treatment) group. The actual picture which emerged from the present study did not represent either of these scenarios but rather fell somewhere in between the two. Thus, any hypothesis about the effects of reinstatement treatments may also need to take both points into account.

One such theory has been previously proposed by Howe and Brainerd (1989), who suggested that trace reinstatement (or redintegration) could occur in two ways; (a) by increasing trace accessibility and (b) by increasing trace availability; and that one does not necessarily occur in the absence of the other. Their trace-integrity hypothesis views storage and retrieval as aspects of a single factor, namely, the extent to which the features which comprise the trace are bonded together to form a unitary whole (trace integrity), and it is not necessarily a matter of one occurring in the absence of the other, but rather that both may occur in unison. Howe and Brainerd stated that in order to independently measure changes in storage and retrieval one needs to implement a more sensitive measure of recall changes than those traditionally employed. They proposed a stochastic model which can be employed to procure more exact estimates of the extent to which each (restorage and retrieval relearning) occurs on recall. Through the implementation of their model, with children as young as 7 years, Howe and Brainerd have found evidence of the occurrence of both storage and retrieval relearning due to repeated testing, with storage relearning being more common than retrieval relearning.

In the absence of formal modelling, no firm conclusions as to the extent to which storage and retrieval relearning occurs in the present context can be generated. However, one may be justified in concluding that it is likely that both storage and retrieval relearning occurred to some degree. It is unlikely to be the case, as Slamecka and Katsaiti (1988) claimed, that changes which occur due to reinstatement procedures (e.g., test trials) are solely due to changes in the stored trace, but rather one has to allow for the fact that even if restorage is the prevalent process, some degree of retrieval relearning occurs.

Although there is some ambiguity with regard to the present findings and their implications as to the occurrence of any spread of reinstatement-treatment effects, these findings are not unlike those found in the part-set cuing literature. Although criterion learning was employed in the present design, with some forgetting observed over the retention interval, and the cues (reinstatement treatments) would appear to have been congruent, they resulted in slightly better, but not significant, improvements in long-term retention-test performance. This finding is consistent with previous findings in the area of part-set cuing procedures (for example see, Basden, 1973; Sloman et al., 1991) whereby congruent cues have been observed to have effects in the neutral to positive range, rather than inhibitory effects, on recall performance.

Errors Versus Successes

A second point brought out by Slamecka and Katsaiti (1988), although not directly measured, as a claim of the learning hypothesis was that "the prior test is an occasion for the augmented learning of whatever items can be remembered" (p. 725). If this is indeed the case, then one would expect the subset of items which resulted in correct responses (successes) on the reinstatement-treatment test trial to show a greater probability of being recalled correctly on the long-term retention tests than those which were not recalled correctly (errors), even when criterion learning was employed.

The results of the present study do support the notion that correct responses serve as a better predictor of future recall success than do errors. However, this conclusion does not provide us with a comprehensive picture of the effect of errors on recall. It is possible that the procedure employed may have lead to this effect. The procedure involved cuing with no feedback followed by a 1-week delay between it and the next retention test. It may be the case that a success on the final long-term retention test is more likely given that a success also occurred on the earlier test-reinstatement trial, but is it necessarily the case that errors have no effect at all? This question was not directly assessed here. What is necessary is a measure that looks at the entire relationship between successive successes and errors as a continuum; investigating how each one affects the next in turn (e.g., across the four consecutive test trials within a retention test). Thus, it is possible that, in their own way, errors may have a substantial effect on increasing later recall, and what is necessary in order to properly test for this is a more sensitive statistical measure such as the stochastic model employed by Howe et al. (1992). It is not necessarily the case here that errors do not serve to reinstate the trace but rather that the correct responses appear to be the more powerful reinstators. Also, the present data set is fairly limited (25 subjects with eight items each) and may not allow for a true test of this theory.

Conclusions and Recommendations

In conclusion, the present study has provided evidence that factors which have previously been found to increase retention-test performance in nonhuman animals, infants, and school-age children (Hoving & Choi, 1972; Hoving et al., 1972) also serve to increase performance on long-term retention test for preschoolers. As previously shown in older samples, an interpolated study trial (Slamecka & Katsaiti, 1988), test trial (Howe et al., 1992), or even a simple reactivation procedure (Hoving & Choi, 1972; Hoving et al., 1972) are all factors which can serve to increase recall over long time frames in preschoolers. Again, study was found to be the superior form of reinstatement treatment.

One major implication of the present study is that in order to provide a comprehensive representation of long-term memory one may need to consider both the possibility of changes in trace accessibility and trace availability. Neither process alone could conclusively explain the lack of significant differences (for the test and reactivation-treatment conditions) between both the reinstatement-treatment items and the non-reinstatement-treatment items and the non-reinstatement-treatment items and the control group. A theory which contains only one in the absence of the other is sure to miss part of the overall picture. Indeed, memory is a complex system that can not be represented by one rigid view, but rather must allow for varying degrees of each facet of memory.

Although this study has provided evidence that the effects of reactivation procedures previously observed with animals (e.g., Deweer et al., 1980; Gatti et al., 1975; Hars & Hennevin, 1990), human infants (e.g., Rovee-Collier & Hayne, 1987; Rovee-Collier & Shyi, 1992), and older children (Hoving & Choi, 1972; Hoving et al., 1972), and more generally, reinstatement-treatment effects, are evident with preschoolers, it has raised many questions for future research. Further study allowing for a more precise examination of the differential effects of varying levels of reinstatement treatments, in particular a test trial versus a reactivation treatment, as well as for the spread of possible reinstatement-treatment effects, is necessary. Further manipulation of the time intervals between study and reinstatement treatment (T_1) and reinstatement treatment and recall (T_2) (study --T_1-- reinstatement treatment --T_2-- recall) would also be of conceptual importance in determining some optimal time delay to implement between these factors. Findings of this type could have significant implications for both educational and legal policies as to the most suitable time delay to employ between the initial encounter with an event (or lesson) and later exposures to (or testing/questioning about) that event.

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Footnotes

[1] This was done in order to prevent the child from seeing what object was in that location if they made an error or even if they were correct. Such an opportunity would have constituted an additional (if they were correct) or misleading (if they made an error) study trial and could have lead to a spurious correct or incorrect answer to this item later on in the retention test sequence.

Appendix A

Parental Consent Form Used.

- (See next three pages).

Fall 1990

Dear Parent:

We are requesting your permission to have your child participate in a study on memory in young children. We are interested in how children search for hidden objects and how well they remember the object-location pairings over a 4-week interval. Little is known about these retention processes despite their obvious practical significance.

The study (and your child's participation) is straightforward and has the approval of the University and your child's preschool/daycare. A FEMALE researcher will visit your child for 3 brief sessions (the first will be for 20 minutes, the second for 5 minutes, and the third for 15 minutes) in a room at their preschool/daycare. At the first session, your child will be asked to play a "hide-and-seek game" where he/she will be shown some objects (e.g., a ball, a toy airplane, etc.) and the researcher will hide these objects around the room. Following this, your child will be asked to retrieve all of the hidden objects. The process will continue until your child remembers the objects and their locations. At the second session (3 weeks after the first visit) the researcher will visit briefly with your child to remind him/her of the "game" they played when the researcher visited last. Finally, at the third session (1 week after the second session), your child will be asked to find the hidden objects again.

This project will begin shortly and will run until December. At the end of the project, a summary report of the findings will be made available to those who are interested (e.g., parents and teachers). NOTE: The identities of the individual children will be kept in the strictest confidence. All reports of this research, published and otherwise, will safeguard the identities of the individuals who participated in this project.

Again, we would appreciate your permission to have your child's participation in this project. Please fill out the attached page and return that portion to your child's preschool/daycare. Also, because this study involves three visits with each child over a 4-week period and we need to carefully schedule each child's participation, please indicate (on the attached form) any time(s) which your child will not be attending the preschool/daycare up until

December (e.g., vacations, etc.). Should you have any questions, please do not hesitate to contact your child's preschool/daycare, Lynn Bryant-Brown (737-3985), or Dr. Mark L. Howe (737-4411). Thank-you very much for your cooperation!

Cordially,

Lynn M. Bryant-Brown, B.Sc.
Graduate Student
Department of Psychology
Memorial University

Mark L. Howe, Ph.D.
Associate Professor
Department of Psychology
Memorial University

PLEASE PRINT **RETURN THIS PORTION ONLY**

Child's Name: ______________________________

Preschool/Daycare: ______________________________

Child's Date of Birth: ______________________________
(Day) (Month) (Year)

Please check one: ( ) My child may participate

( ) My child may not participate

Times which my child will be absent from his/her preschool/daycare:

____

____

Parental Signature: ______________________________

Today's Date: ______________________________
(Day) (Month) (Year)