VISUAL AND PHONOLOGICAL HEBB EFFECTS

Repetition learning in the immediate serial recall of visual and auditory materials

Mike P. A. Page and Nick Cumming

University of Hertfordshire, Hatfield, UK

Dennis Norris

MRC Cognition and Brain Sciences Unit, Cambridge, UK

Graham J. Hitch and Alan M. McNeil

University of York, York, UK

Abstract

In five experiments a Hebb repetition effect, that is, improved immediate serial recall of an (unannounced) repeating list, was demonstrated in the immediate serial recall of visual materials, even when use of phonological STM was blocked by concurrent articulation. The learning of a repeatedly presented letter-list in one modality (auditory or visual) did not transfer to give improved performance on the same list in the other modality. This result was not replicated for word lists, however, for which asymmetric transfer was observed. Inferences are made about the structure of short-term memory and about the nature of the Hebb repetition effect.

Introduction

Since the early work of Hebb (1961) it has been known that immediate serial recall (ISR) performance improves for a list that is repeated unannounced every three trials or so. In common with others, we will refer to this as the Hebb repetition effect. Subsequent investigations have been relatively few in number and have addressed issues such as the extent to which recall and/or rehearsal are necessary factors (Cohen & Johansson, 1968; Cunningham, Healy & Williams, 1984,; Page, Cumming, Norris, Hitch & McNeil, 2005), whether partial repetition is sufficient (Schwartz & Bryden, 1971), and the extent to which explicit recognition of the repetition is required for the performance advantage to accrue. With regard to the latter, the available evidence (Hebb, 1961; McKelvie, 1987) indicates that explicit knowledge of the list repetition is not necessary for a recall advantage to obtain, a finding that suggests a link between the Hebb repetition effect and the larger literature on so-called implicit serial learning (ISL). Surprisingly, however, only Stadler (1993) has explored the connection in any depth (in addition to providing a useful review). He studied implicit serial learning in the context of a serial reaction time task, modified by the introduction of nonrepeating filler sequences to make it more like the paradigm used by Hebb. The results again indicated that awareness of the repetition was not a necessary factor, in this case in the reliable speeding of responses to items in repeating sequences.

Although Stadler's (1993) work suggests a link between ISL and the Hebb repetition effect, some caution is necessary. Tests of the Hebb repetition effect involve explicit attempts to perform immediate serial recall of a relatively small number of lists, with regular repetitions of (usually) a single list separated by the recall of nonrepeating fillers. By contrast, ISL tasks usually involve many more, cyclic repetitions of a single repeating list, with the sole requirement that participants react to the presentation of each item as it occurs, typically by pressing a corresponding button. In ISL, therefore, there is absolutely no recall demand. Similar caveats apply when relating the Hebb effect to implicit learning more generally. For example, artificial grammar learning (AGL; Reber, 1967; etc.), is perhaps the paradigmatic example of implicit learning, but in this field the question of implicitness usually refers to participants' awareness of the rules underlying sequences to which they are exposed. Of course, in the Hebb repetition paradigm, there are no such rules of which participants might become aware. Having said this, many researchers take the view that much of what has been termed AGL is heavily dependent on the (possibly implicit) learning of frequently occurring sequences or sequence fragments (e.g., Brooks & Vokey, 1991; Dulany, Carlson, & Dewey, 1984; Johnstone & Shanks, 1999; Perruchet, 1994; Redington & Chater, 1996). It is this possibility, therefore, rather than the particular question regarding implicitness itself, that suggests a common thread running between AGL, ISL and the Hebb repetition effect. Again perhaps surprisingly, there is little if any acknowledgement of the potential link to be found in the AGL literature. Seeing the Hebb repetition effect in this wider theoretical context, we take the view that students of the effect can best contribute to the related ISL and AGL literatures by carefully delineating the mechanisms by which sequences can be transferred from short-term to long-term memory, even over the course of a small number of trials.

In this context, the Hebb repetition effect has come to be seen as important in helping to inform and extend models both of ISR and of the processes that link ISR with other cognitive abilities, such as language learning. Burgess and Hitch (1999) and Henson (1998) have speculated on how their computational models of ISR might simulate the Hebb repetition effect, and the former have proposed a specific mechanism. They proposed that the effect results from strengthened position-item associations, but some recent experimental data (Cumming, Page & Norris, 2003) are inconsistent with this proposal. This illustrates how an understanding of the Hebb repetition effect has the potential to influence theories of serial recall.

Several models of ISR (Burgess & Hitch, 1999; Henson, 1998; Page & Norris, 1998) have been developed within the framework of the working memory (WM) model (Baddeley, 1986; Baddeley & Hitch, 1974). Within this framework, immediate serial recall of verbal materials is hypothesized as being supported by a structure known as the phonological loop. The loop itself comprises a phonological store and an articulatory control process. This articulatory process is assumed to permit the phonological recoding of visual materials and to allow subvocal rehearsal of the store's content. According to the classical WM model, recoding of visual materials into the phonological store is blocked by concurrent articulation (CA), normally comprising repeated utterance of an otherwise irrelevant word. For visual materials, therefore, CA results in the abolition of effects such as the phonological similarity effect (PSE; Estes, 1973; Levy, 1971; Murray, 1968; etc.) and the irrelevant sound effect (ISE; Colle & Welsh, 1976; Salamé & Baddeley, 1982; etc.). By contrast, auditory presentation is assumed to result in direct access to the phonological store, allowing these effects to persist even in the presence of CA throughout presentation and recall. It should be noted that Besner (1987) reviewed convincing evidence, including that from Baddeley and Lewis (1981) and Besner and Davelaar (1982), that suggested that it was not phonological recoding for the purposes of lexical access that was prevented by CA, but rather the "formation or utilization" of such a code for the purposes of ISR. We will return to this point later in the paper.

The WM framework proposes a partitioning of resources, with specialized stores and processes dedicated to the storage and recall of particular types of material over the short term. As indicated above, the data have given relatively strong support to the idea of a phonological loop component and research has concentrated on characterizing this component in detail. Less work has been devoted to the characterization of other components, at least with respect to the ISR task. It is clear that participants are able to perform this task at levels considerably better than chance even when access to the phonological loop for visually presented materials is blocked by CA. While it is true, therefore, that the loop seems the component of choice for ISR, resulting in generally higher levels of recall, other reasonably effective systems must exist. Whatever these systems are doing when access to the loop is denied, the general patterns seen in the relevant ISR data, as reflected in serial position curves indicating characteristic primacy and recency advantages, are rather similar to those that result from loop access. This has led some people, most notably Jones and colleagues in the context of their O-OER (Object-Oriented Episodic Record) model, to propose a unitary, amodal, ordered store which is involved in a functionally equivalent fashion in the retention of auditory, visual and spatial sequences (Jones, Farrand, Stuart & Morris, 1995; Macken & Jones, 1995). It is difficult, however, to reconcile this position either with the patterns of interaction seen in the data (see Larsen and Baddeley, 2003, and the accompanying discussion, for much more on this issue) or with the dissociations seen in the neuropsychological literature (Basso, Spinnler, Vallar, & Zanobio, 1982; Hanley, Young, & Pearson, 1991; de Renzi & Nichelli, 1975; Trojano & Grossi, 1995; Warrington & Shallice, 1969).

The original WM model did not specify how serial order information is stored when use of the phonological loop is prevented. Baddeley (2000) has recently attempted to rectify this situation somewhat with his proposal of an episodic buffer. One of the functions of this buffer is to act as a back-up store when the loop is not available. Nonetheless, the detailed functioning of such a buffer remains sketchy, as does its relation to, and interaction with, other components of the WM model. It has also been suggested that the other principal subsystem of the WM model, namely the visuo-spatial sketchpad, is able to play a role in nonphonological short-term memory for sequences. This is most obviously the case with spatial sequences such as those employed in the Corsi block-tapping task, and links have been made between such spatial recall and the planning of motor sequences (Smyth, Pearson, & Pendleton, 1988). However, there have also been some findings indicating visual similarity effects in serial and free recall (Avons & Mason, 1999; Hue & Erickson, 1988; Logie, Della Sala, Wynn, & Baddeley, 2000; Smyth, Hay, Hitch & Horton, 2003; Walker, Hitch, & Duroe, 1993) indicative of at least some involvement of a visual memory system in these tasks.

The partitioning of resources in the working memory model, between phonological, visual and spatial subsystems, raises the question of whether this partitioning extends to the long-term memory systems with which working memory interacts. Taking the Hebb repetition effect as an example of such interaction, it is important to know whether this effect is seen equally for the different modalities of input. It is, as yet, unclear whether Hebb repetition effects can be seen across a wide range of materials in different modalities and, if so, whether these different Hebb effects might depend on a single amodal system for long-term sequence learning or, alternatively, whether each subsystem of working memory interfaces with its own, modality-specific long-term learning resource. One of the principle purposes of our paper is to explore this and related issues. As will be seen, between these two extreme positions (amodal vs modality-specific learning) lie a number of possible schemes that combine elements of each. This is particularly the case where lexical material is involved. Before we discuss these issues in further detail, however, we present some more foundational data regarding Hebb repetition effects in different modalities.

In the standard procedure used to demonstrate the Hebb repetition effect, it is likely that participants make use of the phonological loop in performing ISR. In our early experiments, we confirm that this is the case and compare this with conditions under which use of the phonological loop is blocked. There are strong suggestions that the loop plays an important role in the long-term learning of sequential information. Baddeley, Gathercole and Papagno (1998) have assembled a considerable amount of evidence that a major function of the phonological loop is to assist in the acquisition of phonological word-forms as a component of vocabulary learning. Both word-form learning and the Hebb repetition effect involve the relatively quick learning of a repeatedly presented sequence of categorically perceived auditory items, with the aim of subsequent recognition and production. This similarity raises the possibility that the Hebb repetition effect will prove a useful laboratory analogue of the word-form learning process. To make this concrete, it is at least worth considering whether the learning of, say, the repeated auditory list “BJRQ”, is related to the learning of the repeatedly presented novel word “beejayarcue”, particularly given Gupta’s (2005) work that highlights the similarities between standard ISR and the recall of multisyllable nonwords. Given the evidence that word-form learning is dependent on the phonological loop, it is worth establishing whether the Hebb repetition effect for verbal materials also depends on the loop. We addressed this question in our first experiment.

Experiment 1

Introduction

In Experiment 1, we tested for a Hebb repetition effect under conditions that blocked use of the phonological loop. This blocking was accomplished in the standard way by requiring participants to perform concurrent articulation (CA) throughout presentation and recall of visually presented materials. Because the CA was anticipated to reduce performance on the ISR task, we shortened the lists in CA conditions to ensure that any Hebb repetition effect was not masked by a floor in performance. We also included some rhyming items in both auditory and visual conditions as an independent check on whether the phonological loop was used. This experiment was designed to test the hypothesis that the Hebb repetition effect for verbal materials results from use of the phonological loop and that the effect will not be seen when access to the loop is blocked. A relatively large number of participants were used here, therefore, to give adequate power to detect any interaction between CA and

Hebb repetition.

Method

Participants.

There were 72 participants drawn from the MRC Cognition and Brain Sciences Unit (CBU) volunteer panel. There were 53 females and 19 males with a mean age of 21.5 years (range 16–35). All were native speakers of English and each was paid a small fee for their participation.

Materials.

The materials comprised two blocks, each containing 4 practice and 48 experimental lists. Each participant performed one block with CA and one without, with the participants allocated equally and randomly to each of the two block-orders. Lists in the CA block comprised five letters; lists in the no-CA block comprised eight letters. These different list lengths had been arrived at as a result of pilot work, to give approximately equal levels of baseline performance. The letters used were taken from the full set of consonants, with the exception of W that has a three-syllable name. No letter was repeated in a given trial and no list was repeated other than as part of the Hebb repetition manipulation. Letters were not permitted to occupy the same position as they had in the previous list, nor were any three-letter alphabetic runs (e.g., PQR) permitted. In 40 of the lists in a given block, it was ensured that none of the letters in a list rhymed with any other. However, in the remaining eight lists participants were presented with letter-lists in which alternate positions were occupied by letters with rhyming names. In four of these alternating lists, the rhyming letters occupied odd-numbered positions; in the remainder, letters in even-numbered positions rhymed. The rhyming letters were drawn from the set

B, C, D, G, P, T and V. These lists of alternating confusability were included to give a within-list measure of the phonological similarity effect (PSE). It is well known (e.g., Baddeley, 1968; Henson, Norris, Page & Baddeley, 1996; etc.) that confusable letters in such mixed lists suffer in their levels of ordered recall relative to the nonconfusable letters from the same list. The PSE has been used, and is used here, as an index of the involvement of the phonological loop in ISR.

The repetition manipulation followed the standard procedure, with every third list being identical, starting with the third list. There were therefore 16 presentations of each repeated list. The filler lists, that acted as the control lists against which repeated-list performance was measured, were the nonrepeating lists immediately before the repeating list. Neither the repeating lists nor the comparison filler-lists were lists of alternating confusability.

Within the constraints given above, each participant saw a unique set of lists.

<u>Procedure</u>

Letters were presented one at a time in uppercase, 48-point, Arial font at the centre of a computer screen at a distance of about 40cm from the participant. Each letter remained on screen for 500ms and the screen remained empty for 100ms before the next letter appeared. 100ms after the offset of the final letter, all the letters from the most recent list reappeared on the screen, arranged in a “noisy” circle (i.e., a circle with some spatial jitter added) of approximate mean radius 8cm around a central question mark. Participants were asked to click the mouse on the letters in the order in which they had been presented, clicking on the question mark to indicate an omitted letter. Although there was brief visual feedback (a colour change) to indicate that a letter had been clicked, this did not persist, so it was possible for participants to repeat a letter. It was not, however, possible to recall a letter that was not in the stimulus list. It is important to note that the arrangement of list-letters around the response screen was determined randomly on each trial, including for repeated lists. It was, therefore, not the case that a correct response to the repeats of the repeating list involved the same spatial pattern of clicks on each occasion. This is important given Fendrich, Healy and Bourne's (1991) finding of time savings in digit-sequence typing when, using a remapped number-pad, spatial tapping-order is maintained even though the to-be-typed digit-sequence is changed. After the participant had clicked the appropriate number of responses, they were able to advance to the next trial by pressing the spacebar.

For the block involving CA, participants were required to repeat the word "racket" aloud, at a rate of about two to three per second, throughout both presentation and recall of each list. The experimenter was present throughout the experiment so as to ensure compliance with this instruction.

Results

For all experiments presented here an item was scored as correct if it was recalled in the correct position in the list. The values for mean proportion correct over the Hebb repetitions and matched fillers for the two levels of CA are shown in Figure 1. Regression lines have been added to indicate the gradients of improvement in each case. Throughout this paper the extent of any Hebb repetition effect was measured by taking, for an individual subject, the gradient of these regression lines in mean proportion items correct across the repetitions of the repeating list and separately across the corresponding filler lists. This measure captures any improvement in recall of the repeated list while allowing control for any nonspecific practice effect as would be evident in any improvement across the matched fillers. It is a better measure than simply the average percent correct for repeated lists and fillers because, on average, performance on each type of list should be equated for the first presentation of each, thus diluting the ability to measure any improvement on repeating lists. Each participant supplied four gradient values: for repeated-lists and fillers under CA and no-CA conditions. A positive gradient implies improvement across repetitions. These gradient values were then entered into a 2 list-types (repeated/filler: within) by 2 CA conditions (CA/no-CA: within) by 2 block-orders (between) mixed-factor ANOVA.

Mean values for the gradients are given in Table 1. The ANOVA revealed a main effect of list-type, $F(1, 70) = 28.7, MSE = .00026, p < .001$, with repeated lists showing a higher gradient over repetitions than fillers, and a main effect of CA condition $F(1, 70) = 12.1, MSE = .00027, p = .001$, with gradients being lower under CA. There was, however, no reliable interaction between list-type and CA condition ($F < 1$, with power to detect such an interaction estimated at .85), indicating that the advantage for repeating lists over filler lists did not differ across CA conditions. A subsidiary ANOVA confirmed the presence of a repetition effect in the CA condition alone, $F(1, 70) = 11.1, MSE = .007, p = .001$. It is clear that CA was not sufficient to remove the Hebb repetition effect. There was no main effect of block-order ($F < 1$) and this factor did not interact reliably with any other (all Fs < 1).

With regard to the phonological similarity effect, the mean proportions correct (note the change in dependent variable from the gradient measure used above) for the two levels of confusability as they varied with CA condition are given in Table 2. These means strongly suggest that a confusability effect found in the absence of CA is abolished in its presence. (The apparent improvement with CA is due to the shorter list length used.) This pattern was confirmed by the results of a 2 (CA-conditions) by 2 (confusability) repeated-measures ANOVA, which revealed main effects of CA, $F(1,71) = 27.7, MSE = .025, p < .001$, and confusability, $F(1,71) = 13.3, MSE = .0074, p = .001$, with a significant interaction between the two, $F(1,71) = 15.5, MSE = .0059, p < .001$. We also note that the mean proportion correct on normal filler lists in the no-CA condition was .514, which is not significantly different from the performance on nonconfusable items in lists of mixed confusability, $t(71) = 0.693, s.e. = 0.013, p = .49$. This is exactly the data pattern that has been found previously (Baddeley, 1968; Henson, et al., 1996) and that has been simulated by several computational implementations of the WM model. Finally, the mean performance on filler lists with CA was .552, which suggests that we succeeded in using a variation in list length to match approximately performance across CA conditions.

Discussion

The first experiment clearly shows that a Hebb repetition effect persists even when access to the phonological loop is blocked by CA. The gradient of improvement is numerically (and, in fact, significantly) lower across list repetitions with CA than under no CA, but when performance on the accompanying filler-lists is taken into account there is no interaction between CA and list-type. The fact that CA caused a drop in ISR performance (or more particularly a comparable level of performance on much shorter lists) and abolished the phonological similarity effect, indicates that CA was effective in blocking use of the phonological loop.

The strong hypothesis that access to the phonological loop is necessary to produce a Hebb repetition effect, has clearly been falsified. Both the data reviewed in the Introduction, and the fact that CA abolished the PSE in this experiment, suggest that different short-term stores underly ISR performance for visual materials depending on CA condition. It remains a possibility, however, that some longer-term amodal store underlies the repetition improvement. This issue will be taken up again in later experiments. First, however, we will investigate how Hebb repetition effects depend on CA for a different type of visual material.

Experiment 2

Introduction

In the previous experiment we used letter sequences as our stimuli. While it is undeniable that individual letters are visual objects, they are tied very closely to speech and language via the reading process. The fact that they form part of an evolutionarily late-acquired reading system arguably makes them rather atypical as visual objects. In this experiment we used sequences of pictures as our stimuli to see if the results obtained in Experiment 1 generalized to other types of visual object. The use of pictures also gave us an additional dimension to manipulate. In our letter lists, repetition comprised repeated presentation of exactly the same stimulus. With pictures we were able, in one condition, to change the specific pictures from one presentation of a repeating list to the next while maintaining their category names. Specifically, if the to-be-repeated list consisted of a picture of a cat, followed by a picture of a shoe, followed by a picture of a bike, and so on, we were able, on the next presentation of that list, to present a picture of different cat, followed by a picture of different shoe, followed by a picture of a different bike, and so on. In this way, the repetition manipulation changed from being a repetition of the same pictures in the same order, to being a repetition of the same picture-categories in the same order but with different pictures. This manipulation gave us an opportunity to investigate whether the Hebb repetition effect depends upon the repetition of specific visual tokens or simply of their categories. If learning were to have taken place for the category-repetition materials, we could ask whether this was itself dependent on verbal recoding. As in the first experiment, therefore, we required the task to be performed both with and without CA. Given that we found a Hebb repetition effect for visual materials under CA in the first experiment, we predicted the standard exact-repetition condition to show repetition learning regardless of CA. Assuming that participants would subvocally name the pictures, we also predicted an improvement with the category-repetition stimuli in the absence of CA. For the fourth condition, there are two possibilities, each of which would be of theoretical interest. First, there may be a category-repetition effect even under CA; this would suggest that repetition learning occurs after some degree of abstraction (e.g. semantic categorization) of the stimulus items has already taken place. Second, there may be no category-repetition learning under CA, a result that would suggest the learning of a sequence of specific visual objects and would, usefully, confirm the effectiveness of the CA manipulation in preventing verbal recoding (see Discussion).

Method

Participants.

There were 40 participants drawn from the undergraduate communities of the Universities of York and Hertfordshire. Each was rewarded with either course credit or a small fee.

Materials.

Each participant saw six practice trials, during half of which they were required to perform CA. There followed 86 experimental trials divided into two blocks. Half the participants performed CA in the first block, half in the second block. Each list contained seven pictures drawn from a set of 120 pictures. The set of pictures contained 12 digital photographs of each of 10 different categories of object. The object categories were bikes, cats, dogs, fish, houses, mugs, phones, shoes, taps (faucets) and trees. These categories were chosen because they were familiar, easily recognized, and had single-syllable category-names none of which rhymes with any other. Participants were familiarized with these category-names and were told they might use them in performance of the experimental task. The pictures were equated for the area they occupied on the computer screen (approximately 16000 pixels) but varied to a small degree in their aspect ratios. The pictures are available for viewing from the corresponding author.

Half the participants (balanced for block-order) saw repeating lists that represented identical repetition; we shall call this the identical-repetition condition. For each of the participants in the identical-repetition condition there was another participant who saw exactly the same filler lists but with the repeating lists replaced by lists that replicated the identical-repetition category-order but used different exemplars from the category on each repetition; we shall call this the category-repetition condition. There were 12 repetitions of each repeating list, spaced at every third list beginning on the third list. Given that there were 12 exemplars in each category, each repetition in the category-repetition condition was able to use a different exemplar from each category. As in the previous experiment the matched fillers were taken to be the lists that preceded the corresponding repeating list repetition (though see below for some qualification of this statement).

Lists were generated randomly for each pair of subjects, with no repeated item or category within a list, and no item in the same position as an item from the same category in the previous list.

Procedure.

Pictures were presented one at a time in the centre of a computer screen placed approximately 40cm from the participant. Each picture remained on the screen for 750ms and the screen remained empty for 150ms between successive picture presentations. Exactly 150ms after the offset of final picture, all seven pictures appeared on the screen arranged in a noisy circle as in Experiment 1. In this case, no opportunity was provided to indicate an omitted item; the option was little used in Experiment 1, which is perhaps not surprising given that all the list items are present on the screen throughout recall. Participants were required to click on the pictures in the correct serial order. Again, the arrangement of the pictures around the noisy circle was random on each trial including repetition trials. After seven pictures had been clicked, a 3s countdown appeared on the screen before the next trial began.

In the CA block, participants were required to repeat the word “racket” aloud, at a rate of two to three per second, throughout both presentation and recall of the picture lists. In the no-CA block, participants were required to tap their finger at approximately the same rate. In both cases, compliance was monitored by the experimenter.

Results

Figure 2 displays values for the proportion correct recall across the 12 repetitions of the repeating lists and their matched filler lists, for the two secondary-task conditions and separately for the two groups of participants (identical-repetition and category-repetition). Regression lines have been added to give an impression of the gradients of improvement across repetitions for the various conditions and the gradient values of the regression lines fitted separately to each participant's data are used as the dependent variable in all the following analyses. A 2 repetition-types (identical/category: between) by 2 list-types (repeating/filler: within) by 2 secondary-tasks (tapping/CA: within) by 2 block-orders (between) mixed-factor ANOVA was performed on the gradient values. The mean gradients are given in Table 3. The ANOVA revealed a significant main effect of list type, $F(1, 36) = 18.7, MSE = .00083, p < .001$, indicating a higher improvement-gradient for repeated lists relative to fillers, and a main effect of the between-participant factor repetition-type, with an overall advantage for the identical-repetition condition, $F(1, 36) = 4.5, MSE = .00080, p = .040$. These main effects were qualified by a significant three-way interaction between repetition-type, list-type and secondary task, $F(1, 36) = 7.3, MSE = .00045, p = .011$. This three-way interaction appears to stem from two possible sources: first, the weak repetition effect observed for category-repetition lists under CA; second, an unusually high gradient for the filler lists in the identical-repetition condition under tapping.

This was borne out by the results of planned t-tests that tested the repetition/filler gradient difference for the identical- and category-repetition lists under CA and tapping. There were significant repetition effects for the identical-repetition under CA and for the category-repetition under tapping, $\underline{t}(19) = 4.3, s.e. = 0.0078, p < .001$ and $\underline{t}(19) = 3.8, s.e. = 0.0063, p = .001$, respectively. The other two comparisons, namely for identical-repetition under tapping and for category-repetition under CA, failed to reach significance, $\underline{t}(19) = 1.7, s.e. = .0080, p = .109$ and $\underline{t}(19) = 0.826, s.e. = .0094, p = .419$ respectively.

These failures to find a repetition effect seem to have rather different explanations. For identical-repetition under tapping, there is a relatively high gradient of improvement across repetitions; the failure to find a repetition effect appears, therefore, to result from the unusually high gradient across its comparison fillers. To test whether this was the case, we performed another comparison, this time using a second set of filler items, namely the filler lists located one trial later than the repeating lists rather than one trial earlier. It is only convention that has led us to use the fillers preceding repeating lists as their comparison set rather than the succeeding lists. The gradient across this second set of fillers was 0.0026, which was not only more in line with other filler gradients but was also significantly lower than the gradient of 0.025 across the relevant identical-repetition condition, $\underline{t}(19) = 3.1, s.e. = .0074, p = .003$. It therefore appears that there is a reliable identical-repetition effect under tapping that was masked by a type II error in the initial analysis resulting from an unusually high filler-gradient. This result makes good sense: it would be odd to find no identical-repetition effect under tapping when we have already established an identical-repetition effect under CA.

The second case involved a lack of a category-repetition effect under CA. Given the relatively low gradient value under these conditions, this seems to represent a genuine weakening, if not abolition, of the repetition effect. To investigate this possibility, we performed two more analyses. First we compared the category-repetition gradient under CA with its second set of matched fillers (i.e., those following the repeating list), as we had above. Once again, the comparison indicated no reliable difference, $\underline{t}(19) = 0.95, s.e. = .009, p = .36$, even though the power to detect an effect of a size equal to that seen in the corresponding identical-repetition condition was .99. We also entered the gradient values from the CA conditions alone into a 2 repetition conditions (identical/category) by 2 list-types (repeating/filler) by 2 block-orders mixed-factor ANOVA. In this subsidiary analysis, there was a main effect of list-type, with repeating gradients being higher than filler gradients, $F(1, 36) = 11.2, MSE = .00076, p = .002$, and a reliable interaction between list-type and repetition condition, $F(1, 36) = 4.4, MSE = .00076, p = .043$, indicating that there was a significantly stronger identical-repetition effect than category-repetition effect under CA. In summary, unlike in the other three conditions, there is no evidence of a reliable repetition effect for category-repetition lists under CA.

Discussion

The important conclusion to be drawn from Experiment 2 is that we have once again obtained a reliable Hebb repetition effect for visual material presented and recalled under CA, when the material is identical on each repetition. This bolsters the results of the first experiment, in that both show a repetition effect when access to the phonological loop is blocked. In the first experiment, we can be reasonably sure that our CA secondary- task successfully blocked loop access, both because of the effect on levels of performance and because the PSE was abolished. The fact that in the second experiment there was no reliable category-repetition effect under CA, allows us to make the same conclusion here with regard to the success of the secondary-task manipulation. Briefly, if participants had been able to recode the pictures phonologically (subvocal naming) while performing CA, then we would have expected to find that a category-repetition effect in these circumstances, given that the name-sequence would be maintained across category-repetition lists. This assumes that participants would not be tempted to give different names to the different pictures within a category, an assumption easily justified given both the lack of any incentive to complicate the task in this way and, more importantly, the finding of a reliable category-repetition effect under tapping.

To summarize, the results of the first two experiments convincingly demonstrate a Hebb repetition effect for visual materials from familiar visual categories even when access to the phonological loop is successfully blocked.

Experiment 3

Introduction

The fact that Hebb repetition effects persist even when concurrent articulation (CA) blocks access to the phonological loop raises several important issues. First, does the Hebb repetition effect result from the operation of a single, amodal store upon which the phonological and the visual short-term stores converge? Or are there independent longer-term stores, each associated with a given modality of input? Second, if there are independent longer-term stores, do they operate in a similar way? Our third experiment was intended to go some way towards answering these questions. We addressed the first question by testing whether list learning in one modality transfers to give improved performance on the same list in another modality. To make a start in answering the second question, we investigated whether the repetition learning that we see for visual materials presented under CA is accomplished by the strengthening of position-item associations. Cumming, et al. (2003), used a transfer technique to establish that the standard Hebb repetition effect does not seem to depend on such strengthening (see also Hitch, Fastame, & Flude, 2005). Here we used the same technique for both the auditory/phonological and the visual modalities, both in an attempt to replicate the earlier result with phonological materials and to generalize it to the visual modality. Clearly, our two questions are not independent: if we find that a single store underlies learning with repetition in the two modalities, then we would expect the same result to hold across modalities with regard to position-item association.

Method

Participants.

There were 48 participants (26 female, 22 male) drawn from the MRC Cognition and Brain Sciences Unit volunteer panel. Mean age was 22.7 years (range 16–40). Each was paid a small fee for their participation.

Materials.

The materials were similar to those used in the first experiment and were generated in the same way unless otherwise noted. We presented participants with seven-item lists of consonants. Because of the transfer paradigm used in this experiment, it was necessary to decide upon a single list-length to be used in both the auditory modality and for visual presentation under CA. A list-length of seven items seemed to offer a good compromise between making the auditory condition too easy and the visual condition too difficult.

The letter-lists were presented and recalled in one of two ways: they were either presented auditorily with spoken recall; or they were presented visually with mouse-click recall (as before) and with CA throughout the trial. For auditory presentation and spoken recall we would expect the phonological loop to be used. For visual presentation and mouse-click recall, all under CA, we would not expect the phonological loop to be used, particularly given the results of the first two experiments. The lists were presented in four blocks. Modality alternated across blocks, so if a participant performed spoken recall of auditorily presented lists in the first block of trials, then they would see lists presented visually under CA in the second block, returning to auditory lists in the third block, and so on. Half the participants heard auditory lists in blocks 1 and 3 and saw visual lists in blocks 2 and 4; for the remainder the pattern was reversed.

In each of the first two blocks there were 43 lists with a different repeating list being presented on every third list, starting with the third list in each block. Each repeating list was repeated 12 times within its block. This allowed us to test, for each participant, whether there would be a reliable Hebb repetition effect for each modality, as predicted given earlier results. For the Hebb repetition effect, the corresponding fillers were the lists immediately preceding lists, as before. In addition, in each of these first two blocks, there were four lists of alternating confusability (two starting with a confusable and two starting with a nonconfusable letter). These were included for exactly the same reason as in the first experiment, namely, as a check that participants were using the phonological loop for auditory trials, with loop access being successfully blocked for visual presentation under CA. Finally, at the end of each of the first two blocks we included a transfer list that tested for the strengthening of position-item associations in the same way as was done in Cumming et al. (2003). For each block the transfer list was located four trials after the presentation of the last regular repetition of the repeating list. This transfer list had alternating letters in exactly the same positions as they had occupied in that repeating list; none of the remaining letters occupied the same position as they had in the repeating list (e.g., the transfer list QMRKHPZ derived from the repeating list QKRPHMZ). If position-item strengthening underlies the Hebb repetition effect (e.g., Burgess & Hitch, 1999) then we should see improved performance on those items that occupy the same positions as they did in a repeating list, provided that the repeating list has been recently and reliably learned relative to fillers. This manipulation is exactly that used by Cumming et al.. To summarize, the first two blocks were presented in different modalities (balanced for order) and tested independently for Hebb repetition effects, phonological similarity effects and positional transfer effects.

We also tested for transfer of learning between modalities. Specifically, two of the filler lists in the second block (the 8th list and the 26th list in the block) were replaced by a repetition of the repeating list from the first block, but now in a different modality from that in which that repeating list had been presented and (it was hoped) learned. If repetition learning transfers between modalities, we would expect these repetitions of the first-block repeating list to be recalled better in the second block than matched fillers. This, of course, supposes that any repetition learning seen in the first block has not “worn off” by the time the second-block test lists are presented. It was partly to check for this possibility that the third and fourth blocks were presented.

The third and fourth blocks contained only seven lists, five of which were filler lists and two of which were test lists. The test lists were the third and the sixth lists in these blocks. Each of the test lists was a repetition of a repeating list, one from the first block, the other from the second block. The third list in the third block was a repetition of the first-block repeating list, now back in its original modality of presentation; the sixth list of the third block was a repetition of the second-block repeating list, in a different presentation modality from that in which it had originally been presented. Similarly, the third list in the fourth block was a switched-modality repetition of the first-block repeating list; the sixth list in the fourth block was a same-modality repetition of the second-block repeating list. The somewhat complex design of the experiment is illustrated in Figure 3.

We predicted repetition learning in both modalities and, given evidence for separate short-term stores and the lack of stimulus abstraction evident in Experiment 2, we tentatively predicted no transfer between modalities. In summary, therefore, our predictions were: that a Hebb repetition effect would be observed independently in each of the first two blocks; that the first-block repeating list would not show improved performance relative to fillers when first presented in the other modality in the second block; that the first-block repeating list would, however, show retained improved recall when presented in its original modality in the third block; and that the second-block repeating list would not show improved performance when tested in the switched modality of the third block, but would benefit when tested again in its original modality in the fourth block. We further predicted that neither of the Hebb repetition effects seen in the first two blocks would be reflected in improved performance in the relevant positional-transfer lists for those items that retained their repeating list positions (as Cumming et al., 2003, had found under their conditions). Finally, we predicted an interaction of the phonological similarity effect with CA to confirm, as for the earlier experiments, the success of the CA manipulation in blocking use of the phonological loop.

There remains a question regarding the second presentation, in the second block, of the first-block repeating list, and the presentation of the same list again in the fourth block. Consistent with our prediction of modality-specific Hebb repetition effects, it seemed possible that there would be a small improvement across these lists, building up with the three repetitions of that list within the second-block modality. That is, although the first test in the second block of the first-block repeating list is predicted to show no benefit relative to fillers, the second (second block) and third (fourth block) tests of that same list in the switched modality might show some improvement due to repetition in that switched modality. Given that there were only three presentations of the relevant list in the switched modality, we anticipated that any improvement in performance would be small. Nonetheless, we considered it worth testing.

Procedure.

For the auditory materials, the letter-names spoken by a male were digitally recorded at a 22kHz sample rate and 16-bit resolution. Their perceptual centres were aligned so that the letters sounded naturally paced in any reordering. The letters were presented at a rate of one every 750ms and the order of their spoken recall was recorded for later marking. The auditory lists were presented over headphones at a comfortable volume and participants were required to tap their finger throughout presentation and recall at a rate of about two to three taps per second.

The presentation and recall of visual materials was accomplished in the same manner as in the CA condition of Experiment 1.

Results

The results of the experiment are relatively complex but can be broken down into a number of independent analyses. The first thing to be established was whether there were Hebb repetition effects observed in the first two blocks of trials — without reliable Hebb repetition effects most of the other analyses would make little sense. Each participant saw a repeating list in each of the two presentation modalities, with order of presentation balanced across participants. We therefore entered gradients across repeated and matched filler lists, calculated for each participant in each modality, into a 2 list-types (repeating/filler: within) by 2 modalities (auditory/visual: within) by 2 block-orders (between) mixed-factor ANOVA. The mean gradients for the various conditions are given in Table 4. The ANOVA revealed a main effect of list-type, $F(1, 46) = 16.6, MSE = .00045, p < .001$, with gradients across repetitions exceeding those across fillers. There were no other reliable main effects or interactions (all Fs < 1). It is clear, therefore, that there were significant repetition effects in the first two blocks regardless of modality or block-order. This was confirmed by planned t-tests which showed a significant difference between gradients across repeating and filler lists for both the auditory modality, $t(47) = 3.39, s.e. = .0039, p = .001$, and the visual modality,

$t(47) = 2.66, s.e. = 0.0044, p = .011$.

Next we looked to establish the success of our CA manipulation, by looking for an interaction between phonological confusability and modality in the lists of alternating confusability. We entered the relevant proportions correct (note the change in dependent variable from now on—gradients are only appropriate for the repetition-effect analysis) into a 2 confusability (confusable/nonconfusable: within) by 2 modalities (auditory/visual: within) by 2 block orders (between) mixed factor ANOVA. The means for the various conditions are shown in Table 5. The ANOVA revealed a main effect of confusability, $F(1, 46) = 18.3, MSE = 0.013, p < .001$, indicating poorer performance with the confusable letters, and a main effect of modality, $F(1, 46) = 43.4, MSE = 0.045, p < .001$, indicating significantly poorer performance for visual presentation with CA. These main effects were qualified by a significant interaction between confusability and modality, $F(1, 46) = 10.31, MSE = .0096, p = .002$. Inspection of the means indicates that a reliable effect of phonological confusability for auditory presentation, $t(47) = 5.3, s.e. = .022, p < .001$ was abolished for visual presentation under CA $t(47) = 1.1, s.e. = .021, p = 0.27$, even though the power available to detect an effect of a size equivalent to that seen in the auditory condition was greater than .99. There were no other significant main effects or interactions. Table 5 includes means for standard filler lists that contained no rhyming items. These once again show that nonconfusable items in mixed lists are recalled at essentially the same level as are matched items from pure nonconfusable lists. All these results are entirely consistent with previous work and indicate the success of the CA manipulation in Experiment 3.

Next we looked to see whether any of the benefit that accrued to the repeating lists transferred to those items that maintained their repeating-list position in the relevant same-modality transfer lists. We entered the relevant proportions correct into a 3 item-type (in same position as in repeating list; in different position; in matched filler: within) by 2 modalities (auditory/visual: within) by 2 block-orders (between). The means for the various conditions are given in Table 6 together with those from the relevant repeating lists. We found a main effect of modality, $F(1, 46) = 39.6, MSE = 0.15, p < 0.001$, indicating better performance for auditory presentation, but no other main effects or interactions. This suggests that there is no advantage for maintaining list position relative to a recently learned repeating list, consistent with Cumming et al. (2003). Planned comparisons comparing performance on items from the same positions as in the repeating lists, items from different positions, and items from filler lists revealed no reliable differences in performance, but performance in each of these conditions differed reliably from performance in the final repetition of the repeating list (all $\underline{p}$s $< .001$).

Next, we tested for the transfer of repetition learning across modalities. Each participant saw a visual test of a previous block's auditory repeating-list and an auditory test of a previous visual block's repeating-list, with the order of these two being balanced across participants. They also saw an auditory test of a previous auditory repeating list (from two blocks earlier) and a visual test of a visual repeating list (ditto), again balanced for order. Performance on each of these four test-lists was compared with the mean performance of the nearest four filler-lists (because there are only four available filler lists in the third and fourth blocks), and the test-filler differences were entered into a 2 block-orders (between) by 2 modalities-of-original-repetition (auditory/visual: within) by 2 relative-modality-of-test (same/different: within) mixed-factor ANOVA. The means for the various conditions are shown in Table 7. The results revealed a main effect of relative-modality-of-test, $F(1, 46) = 9.5, MSE = .051, p = .004$, indicating a larger test/filler difference when a test list was in the same modality as the original repeating list. No other main effects or interactions reached significance. Planned t-tests revealed that the mean test-filler difference reliably exceeded zero for the same-modality test, $\underline{t}(47) = 6.7, s.e. = 0.022, p < .001$, but not for the switched modality test, $\underline{t}(47) = 1.67, s.e. = 0.028p = .102$. The power to detect an effect of size equal to that seen in the same-modality test was greater than .99. Hebb repetition effects therefore persist when the repeating list is tested two blocks after the block in which it was learned and in the original modality; there is no advantage, however, when the repeating list is tested in the other modality in the block immediately after that in which it was learned. There is no evidence, therefore, of crossmodal transfer of repetition learning.

Finally, and somewhat by way of an aside, we tested to see whether there was an improvement across the three switched modality repetitions of the first-block repeating list (two in the second block and one in the fourth block). No reliable improvement was found.

Discussion

The results of Experiment 3 fulfill several functions. First, they replicate the findings from the first two experiments of a Hebb repetition effect for visual materials under CA. Second, by showing an interaction between CA and phonological similarity they reinforce the conclusion that this CA had been effective in blocking use of the phonological loop. Third, they replicate

Cumming et al.'s (2003) result by demonstrating that the Hebb repetition effect is not consequent on a strengthening of position-item associations, as well as extending this result to the visual modality. Fourth, and most important, they show that the learning of a repeating list is relatively persistent, but does not transfer across modalities.

Experiment 4

Introduction

The finding in Experiment 3, that the learning of a repeating letter-list appears not to transfer across modalities, is interesting but is subject to the criticism that both presentation modality and response modality were changed simultaneously. It might have been that the list in the switched modality was recognized as familiar but had previously been learned in association with a response plan that was inappropriate in the changed setting (e.g., a visually driven response plan in the context of a list requiring spoken recall). This would be potentially consistent with a hybrid model, in which list learning and subsequent list-recognition would be based on amodal long-term store, but in which the associated learned response plans would be modality specific. Of course, such a hybrid model still contains an element of modality-specific learning at the response level.

In order to explore this possibility further, we once again presented subjects with lists, some of which repeated. We then tested recall of the learned lists under conditions in which either the presentation modality or the response modality was switched, but not both. Because lists learned in one modality (e.g., auditory) can be later tested in two ways (either auditory presentation with a mouse-click response under CA, or visual presentation under CA together with a spoken response), it was considered necessary that each participant was exposed to (and, we hoped, learned) two repeating lists in each modality. Thus each participant received both a block of auditorily presented lists for spoken recall and a block of visual lists under CA for mouse-click recall, with each block containing two repeating lists appropriately interleaved with fillers.

In an earlier attempt to run this experiment we used letter lists, but the large degree of item overlap between the four repeating lists and their interleaved fillers conspired to eliminate any reliable Hebb repetition effect (cf. Cumming, Page, Norris, Hitch, McNeil, 2005). Clearly in the absence of any reliable Hebb repetition effect in the learning blocks, there was no point in examining any putative transfer. To remedy this situation, we were forced to enlarge our item-pool by replacing the letters with a set of single-syllable words. This had the advantage that we could use distinct subsets of the words for each of the repeating lists and for their fillers. Such a manipulation has been shown by Cumming et al. (2005) to increase the strength of the Hebb repetition effect. Of course, the decision to use words instead of letters was not taken lightly, not least because (as will be seen) it complicates the interpretation of any differences that we observed. Nevertheless, it was not considered unreasonable to test the generality of the preceding results by employing a range of potential stimuli.

Method

Participants.

There were 32 participants (16 female, 16 male) drawn from the MRC Cognition and Brain Sciences Unit volunteer panel. Mean age was 19.9 years (range 16–26). Each was paid a small fee for their participation

Materials.

Participants were presented with lists of seven words for immediate serial recall. All words were single-syllable nouns taken from the Celex corpus of English words containing 4–6 letters. Eight sets of seven words were selected subject to the following constraints: no words in a set shared either their vowel (in spoken British English) or their onset consonant (consonant cluster). The written frequency of the sets were roughly equated. In addition to these sets, two sets of words that could be arranged into alternating phonologically confusable (rhyming) and nonconfusable word-lists were chosen. These were chosen such that the spellings were different for each of the rhyming items in the set (e.g., bait, plate and weight) in order to minimize any potential visual similarity effects.

Each of the first two blocks comprised 36 lists, with all lists in a given block presented either auditorily for spoken recall with tapping throughout or visually (one-at-a-time) with mouse-click recall with CA throughout (as for previous experiments). Half the participants received auditory lists in the first block and visual in the second, vice versa for the remaining participants. Each of the learning blocks comprised alternating presentation of two repeating lists, each of which was repeated eight times. Each repetition was preceded by a filler list composed of list-items taken from a different item-set - as before, these served as controls in the assessment of the improvement across repetitions. In any given block, therefore, there were eight presentations each of two repeating lists derived from different item-sets, each presented on every fourth list and regularly interleaved with two sets of filler lists that were also each derived from distinct item-sets. The remaining four lists in a block were lists of alternating confusability that were included, as previously, in an attempt to establish whether participants were using the phonological loop to perform ISR. Specifically, they were lists in which list-items in either odd (two lists) or even (the other two lists) positions shared a rhyme. These lists were inserted into the first two blocks as the 5th, 14th, 23rd and 32nd list in each block. Different item-sets were used for the lists that began with a confusable, and those that began with a nonconfusable. Two of each type of list were included in each of the first two blocks.

The third and fourth blocks were once again used to test for transfer of the learned lists across modalities. In the third block, lists were presented with the same modality of presentation as were the lists in the first block, but with the alternate modality of response. Similarly, in the fourth block lists were presented in the same modality as were lists in the second block, but again with the alternate modality of response. As an example, if the lists in the first block had been presented auditorily for spoken recall under tapping, then lists in the third block were presented auditorily for mouse-click recall under CA, and lists in the fourth block were presented visually under CA but for spoken recall. In each of the third and fourth blocks, recall was tested for one of the repeated lists from the first block and one of the repeated lists from the second block. The particular lists chosen were counterbalanced across subjects. In this way, both types of transfer list (changed presentation modality or changed response modality) were tested for lists learned in each of the two original modalities. In addition to the transfer lists, the third and fourth blocks each contained ten filler lists derived from the same item-sets as the filler lists in the first and second blocks respectively. The transfer lists were the fifth and ninth lists in each of the third and fourth blocks, counterbalanced for order across subjects.

To permit their auditory presentation, each of the words was recorded in a British-English male voice using a Marantz PMD670 digital audio recorder and a studio-quality microphone. All files were 800ms in length and each was p-centered, as described previously, so that word-lists sounded regular when files were combined in any order.

Procedure.

Any given trial began with either the words "Start saying 'Racket' " or the words "Start Tapping" depending on the presentation modality. This instruction remained on the screen for 1 second. For auditory presentation with tapping, the words were presented at a regular rate of one word every 850ms. For visual presentation with CA, the words were presented individually in a 48-point font in the center of the screen, at an IOI of 850ms. Each word remained on the screen for 750ms with consecutive words separated by a blank screen of 100ms in duration. 1500ms after the onset of the last item, the recall instruction was presented on the screen. In the first two blocks, in which response modality was the same as presentation modality, this either comprised the words "Recall Now - speaking" or "Recall Now - clicking" followed at an interval of 750ms by the relevant item-set for mouse-click recall. In the third and fourth blocks, the recall cue was "Recall Now - speaking" for responses that were to be spoken, or "Start Saying "Racket" now" followed at an interval of 750ms by the relevant item-set for mouse-click recall. Between each block and its successor, participants were invited to take a break, and the presentation and response styles for the upcoming block were described.

Results

As for previous experiments, we first checked for reliable learning of the repeating lists, of which there were two in each of the first two blocks. As before, gradients of improvement in mean proportion correct recall were compared for repeating lists and filler controls. The relevant gradients were entered into a two block orders (between) by two modalities (within) by two list-types (repeating/filler: within) mixed-factor ANOVA. This revealed a main effect of list-type, $F(1, 30) = 40.0, MSE = .001, p < .001$, with the gradients for repeating lists (means of 0.031 and 0.038 for auditory and visual respectively) reliably exceeding those for fillers (0.012 and 0.000 respectively). There were no other reliable main effects or interactions. Thus participants exhibited reliable learning of repeating lists.

We next checked to see if there was a reliable disadvantage for recall of rhyming items in lists of alternating confusability. The relevant values of proportion correct recall were entered into a two block-orders (between) by two modalities (within) by two levels of confusability (within) mixed-factor ANOVA. The analysis revealed a pattern that was not as clear as had been hoped. There was a main effect of confusability, $F(1, 30) = 5.98, MSE = .008, p = .021$, with confusables (rhyming items) recalled worse than nonconfusables, but there was no reliable interaction between modality and confusability, $F < 1$. There was, however, a reliable three-way interaction between these two factors and block order, $F(1, 30) = 4.85, MSE = .008, p = .035$. Examination of the means revealed that participants who experienced visual lists in the first block and auditory lists in the second block showed a marginal effect of confusability in the expected direction, $F(1, 15) = 4.46, p = .052$, that was qualified by a modality by confusability interaction that was in the predicted direction and independently reliable for this group, $F(1, 15) = 4.84, MSE = .008, p = .044$; the other half of participants showed no effect of confusability, $F(1, 15) = 1.75, MSE = .008, p = .21$, and no interaction of this factor with modality, $F < 1$. It appears, therefore, that the predicted interaction of confusability with modality was only reliable for one half of subjects.

Next we examined performance on the transfer lists in blocks three and four. In each case, the dependent variable was performance on the relevant transfer list minus average performance on the six surrounding fillers (i.e., those with the same presentation and response demands). Positive transfer was thus evident to the extent to which the resulting values exceeded zero. Transfer values were entered into an ANOVA in which the within-participant factors were the modality of the originally repeated list and whether presentation modality or response modality was maintained during the transfer test. This two block-orders (between) by two original modalities of repetition (within) by two (presentation/response maintained: within) mixed-factor ANOVA revealed no reliable main effects but one reliable three-way interaction between all the factors, $F(1, 30) = 5.71, MSE = 0.059, p = .023$. Closer examination of the pattern underlying this interaction revealed that for the participants who experienced visual lists in the first block, and who showed the predicted interaction between confusability and modality, there was particularly strong transfer for lists that had been learned auditorily/spoken and were later tested with visual presentation and spoken response. By contrast, for the other half of the participants, there was strong transfer for lists that had been learned auditorily/spoken and were later tested with auditory presentation and mouse-click response. Despite both these, the transfer advantage for lists learned auditorily did not quite constitute a reliable main effect, $F(1, 30) = 3.16, MSE = 0.065, p = .086$. Whatever the explanation for this reliable three-way interaction, performance on the transfer lists showed one clear and important effect, namely that all transfer values were reliably above zero, all $t(31) > 2.7$, all $p \leq .01$. That is to say, whether it was presentation or response modality that was changed, and whether it the original modality of learning was auditory or visual, transfer lists were recalled better than controls. The means transfer values are shown in Table 8.

Discussion

The original purpose of the Experiment 4 was to check whether transfer of learning would be seen when word-lists retained their response mode. In fact, reliable transfer was seen when previously learned word-lists retained either their response mode or their presentation mode. To spell out the implication of this result, it is perhaps useful to discuss the two learning modalities separately.

First consider those lists that were learned with auditory presentation and a spoken response. It appears that when the same lists are presented visually under CA, sufficient contact is made with a previously learned representation that spoken recall can benefit from consequent access to the previously learned spoken response. This implies that for visual presentation CA does not block access to a list that has been learned auditorily. We might relate this finding to the point made by Besner (1987), who reviewed evidence that CA did not block the recoding of visually presented words sufficient to perform homophony judgement (for both words and nonwords) or sufficient to produce a pseudohomophone effect in the recall of nonword-lists (Besner & Davelaar, 1982). As noted earlier, this would imply that phonological recoding is itself not blocked by CA, although maintenance of a whole list so recoded is at the very least strongly discouraged, as evidenced by the abolition of the PSE. As long as visual words are individually recoded at presentation then, it appears that they can access a representation of a list learned auditorily provided that they arrive in the learned order. This is reminiscent of the fact that the STM patients mentioned earlier can recognize words (and presumably overlearned word-sequences such as idioms, though we are not aware of this having been tested) that they have learned previously, despite not being able to retain new word-lists in a phonological form.

Now consider the case for which the transfer test for a list previously learned auditorily, involved auditory presentation and mouse-click response. Given the auditory stimulus, access to the representation of the previously learned auditory list is uncontroversial. It seems, therefore, from the positive transfer, that consequent activation of a previously learned spoken response is sufficient to benefit mouse-click recall even under CA. This is consistent with the fact that CA has never been thought to be sufficient to block read-out from the phonological store, as evidenced by the persistence of the PSE for auditory presentation under CA throughout presentation and recall.

Turning to those lists that were learned with visual presentation and mouse-click response, all under CA. A transfer list involving visual presentation would, again uncontroversially, be able to access the previously learned representation. It appears, therefore, that the consequent activation of a learned mouse-click response is sufficient to drive a spoken response at transfer. This suggests that, for word-lists at least, the learned mouse-click response specifies an ordered set of lexical items rather than anything more particularly visual.

Monsell (1985) pointed out that lists of words (perhaps unlike lists of letters, see later) are unlikely to be represented in a specifically visual form, and proposed exactly such a orthographic-lexical store.

Finally, a transfer list involving auditory presentation appears to be able to access the representation of a list learned visually, and perhaps, therefore, lexically, with the consequent activation of a learned mouse-click response leading to the observed transfer. In all these cases, the proposed cross-modal access mechanisms are essentially identical to those in the bimodal interactive activation model of lexical representation (e.g., Ferrand & Grainger, 2003). As its name suggests, this model assumes separate phonological and orthographic lexicons, but with links between the two allowing access to each from either modality. A similar process of cross-modal access (as opposed to dual access to a single amodal store) has also been proposed by McKone and Dennis (2000) to explain positive cross-modal short-term repetition priming that is nonetheless weaker than that seen when prime and probe share the same modality.

Of course, the foregoing discussion assumes different storage for word-lists learned auditorily under tapping and those learned visually under CA. An alternative explanation would assume that lists are represented at a single abstract lexical level whatever the modality of input. In order to account for the usual abolition of the PSE by CA, such an explanation would have to assume that the PSE only emerges when the lexically stored list is converted into a speech output plan, a move that is only encouraged in the absence of CA at output. Even then, the persistence of the PSE for auditory presentation and CA throughout presentation and recall would remain something of an enigma. Nonetheless, it is difficult on the basis of Experiment 4 to choose between two accounts: One explains the transfer observed in this experiment by reference to a single abstract, probably lexical-level, list representation that is learned regardless of input modality and applies regardless of output modality; the other, as developed above, explains transfer by appealing to separate representations for lists learned in different modalities, that can be flexibly accessed, if necessary via online recoding, and flexibly applied in different output situations. We will return to consideration of these two possibilities after describing our final experiment.

Experiment 5

Introduction

Experiment 5 addressed an anomaly that emerged as a result of Experiment 4. Both the explanations for the positive transfer found in all conditions of that experiment are apparently able to predict transfer of learning even when, as in Experiment 3, both presentation modality and response modality are changed relative to those of the previously learned list. If such a pattern were to emerge for words, that would constitute a different pattern from that seen with respect to the letter-lists of Experiment 3. Experiment 5 was therefore a reasonably direct replication of Experiment 3, using word-lists in place of letter-lists

Method

Participants.

There were 32 participants (14 female, 18 male) drawn from the MRC Cognition and Brain Sciences Unit volunteer panel. Mean age was 20.6 years (range 17–28). Each was paid a small fee for their participation

Materials.

The word-sets were the eight sets used for Experiment 4, and the same audio recordings were used. The design follows closely that of Experiment 3, with two full-length blocks during which a single list was repeatedly presented on every third list, starting with the third list. Once again there were four lists of alternating confusability in each of the first two blocks to test for the presence of a PSE. Four distinct word-sets were used in each block, with different sets in each block. In a given block the four distinct sets were used separately to generate the repeating list, the lists of alternating confusability, and two different families of intervening filler lists (thus maintaining parity of list-item frequency between repeating lists and their matched fillers). As in the third experiment, there followed two short transfer-blocks, each of which contained a test of both previously repeated word-lists. These were, therefore, either in changed modalities of both presentation and recall, or in their original modalities as previously learned, as per Experiment 3.

Procedure.

The procedure was the same as that for Experiment 4, except that auditorily presented lists were always followed by an instruction to speak the response, and lists presented visually under CA were always followed by an instruction for mouse-click response with CA continuing throughout.

Results

As usual, we first analyzed the results from the first two blocks to check for the presence of reliable learning of the two repeating lists, one in each modality. Values of the gradient in proportion correct were compared in a two block-orders (between) by two modalities (within) by two list types (repeated/filler: within) mixed-factor ANOVA. This revealed a significant main effect of list type, $F(1,30) = 74.9, MSE = 0.017, p < .001$, with improvements gradients for repeating lists exceeding those for fillers, and an interaction between modality and list-type, $F(1,30) = 15.4, MSE = 0.017, p < .001$, that indicated a reliably bigger difference in gradients for visual (means of .043 and .002 for repeated and fillers respectively) than for auditory lists (.031 and .015 respectively). Nevertheless, paired-samples t-tests revealed that for each modality the gradient for repeated lists exceeded that for fillers, $t(31) = 9.1, s.e. = 0.032, p < .001$ and $t(31) = 3.3, s.e. = 0.033, p.002$ for visual and auditory lists respectively. Once again, therefore, in addition to the standard auditory effect, a reliable Hebb repetition effect has been found for verbal materials presented visually under conditions of CA. There were no other reliable main effects or interactions.

Next, we tested for the presence of a PSE in the lists of alternating confusability. Values of mean proportion correct were entered into a two block orders (between) by two modalities (within) by two levels of confusability (within) mixed-factor ANOVA. This revealed a reliable main effect of confusability, $F(1,30) = 10.8, MSE = 0.017, p = .003$ indicating better performance for nonconfusable list-items (means of .51 and .44 for auditory and visual modalities respectively) than for confusables (means of .38 and .43 respectively). There was also the predicted modality by confusability interaction, $F(1,30) = 10.3, MSE = 0.012, p = .003$, and there were no other reliable main effects or interactions. Thus, CA seemed successfully to have prevented use of the phonological loop for visual presentation.

Finally, we tested for transfer of learning when a previously learned word-list was presented for recall in a changed modality of both presentation and response. Transfer values were calculated as the performance on each of the two transfer lists (switched modality and original modality) in a given block minus the mean performance on fillers in that block. The analysis was the same as that for Experiment 3, with transfer values entered into a two block-orders (between) by two learned modalities (within) by two test-types (switched or original modality: within) mixed-factor ANOVA. This revealed a reliable interaction between test-type and learned modality, $F(1,30) = 6.1, MSE = 0.042, p = .02$, and no other significant main effects or interactions. Importantly, planned t-tests revealed that all transfer values reliably exceeded zero, all $t(31) > 6.6$, all $p < .001$, indicating that unlike in Experiment 3, all conditions resulted in positive transfer of learning. Table 9 shows the transfer values for the two modalities in the switched and original modalities. We also included in this table values reflecting the advantage for repeated lists over fillers in the relevant learning-block as measured for the average performance of the last two repetitions of the repeating list. This clarifies the nature of the interaction between original modality and test type. Essentially, for lists learned auditorily with spoken response, transfer is complete: lists learned in this way are recalled equally well relative to nearby fillers whether they are presented and recalled in switched or original modalities. By contrast, for lists learned with visual presentation under CA, transfer is not complete. There is a marked and reliable, $t(31) = 2.95, s.e. = 0.39, p = .006$, drop in the transfer to the switched modality relative to performance when the original learned modality is reinstated. This was in spite of the fact that learning in the original modality is numerically, though not reliably, stronger for the visual modality.

Discussion

The pattern of results found here for word-lists was different from that found previously for letter-lists. There was reliable transfer of learning even when modalities of presentation and recall were both changed simultaneously. Nonetheless, the transfer was not symmetrical, with lists learned with auditory presentation under concurrent tapping transferring their advantage to an opposite-modality test more completely than lists learned with visual presentation under CA.

In Experiment 4, we had found that learning of word-lists transferred when those lists were either presented or recalled in the alternate modality. In the subsequent discussion, two possibilities were raised, both of which were able to predict transfer for the word-lists in Experiment 5: first that learning was modality specific but that cross-modal access to and read-out from the learned representation was possible even under CA for visual materials; second, that list-learning was carried out at an abstract and amodal level, possibly an abstract lexical level, thus guaranteeing access and read-out independent of test and response modality. There are a number of aspects to the results of Experiment 5 that lead us tentatively to prefer the former explanation. First, there is the asymmetry in the transfer, by which transfer is complete for lists learned auditorily under tapping, but not for lists learned visually under CA. It is not obvious how an outright amodal account could deal with this asymmetry. A modality-specific account might note that for the lists learned auditorily, the proposed cross-modal access mechanism, namely phonological access by printed words, is far better practiced than the equivalent for visually learned lists, that is access to the orthogrpahic lexicon by auditorily presented words. Note that the advantage for lists learned auditorily was almost reliable in Experiment 4. Second, there is the abolition of the PSE by CA and visual presentation. If lists are stored only amodally, then one might expect the PSE to be abolished too for written recall of auditory materials with CA throughout. Notwithstanding the observations of Jones, Macken and Nicholls (2004; see Baddeley, 2005, for a response), this does not seem to be generally the case.

Finally, a further reason for preferring an explanation that includes an element of modality-specificity is that it accounts much more naturally for the results of Experiments 2 and 3. Taking Experiment 3 first, it is difficult for an outright amodal account to explain the lack of transfer observed in that experiment. Having said that, the difference in the patterns observed in Experiments 3 and 5 still requires an explanation, even if it is necessarily rather ad hoc. We suggest that the recall of letter-lists and of word-lists differ in the extent to which they can involve genuinely visual processes as opposed to lexical processes. Monsell (1985) discussed the distinction between a visual store and a lexical store accessed visually; he suggested that the former would be more appropriate to storage of letter lists, with the latter better suited to word-lists, both under conditions in which the preferred phonological coding is rendered impractical by CA. (We should note, however, Logie et al., 2000, did find evidence for some visual coding of both letter- and word-lists.) If it is true that letter-lists are more prone to truly visual storage than to "lexical" storage when use of the default phonological code is blocked, then this would explain why transfer was not evident in Experiment 3. By contrast, the (albeit asymmetric) transfer evident in Experiment 5 might be explained by the use of a more abstract orthographic lexical code like that Monsell suggested.

Finally, turning briefly to Experiment 2, the fact that we could observe no learning for picture lists under CA when the actual pictures changed across "repetitions" while maintaining their lexical referents (e.g., house, bike, etc.), supports the conclusion from Experiment 3 that abstract lexical representations are not automatically accessed and used for the recall of every type of material under all conditions.

General Discussion

The results of five experiments demonstrate strongly that the Hebb repetition effect in ISR extends beyond circumstances in which the phonological loop is used. In all five experiments we were able to observe the learning of repeating lists whether or not phonological coding was evident. At least as far as the working memory model is concerned, this breaks any strong link between the Hebb repetition effect and storage in the phonological loop. In Experiment 2, we showed that repetition learning of lists of pictures was found, even under CA, when the pictures remained the same across repetitions. The fact that the learning was abolished under CA when only the order of the picture-categories was repeated, strongly suggests that this result is not simply a consequence of phonological recoding. Experiment 3 established that repetition learning of visual materials did not transfer to items that individually retained their learned positions in the context of another list. This was consistent with the previous findings of Cumming et al. (2003) for phonologically recoded lists. Experiments 3 to 5 further explored the transfer of learning across modalities and pointed to a rather different pattern for letter-lists and word-lists.

In the introduction we hypothesized a relationship between the Hebb repetition effect and the learning of phonological word-forms. The data presented here suggest that even if this relationship holds, the Hebb repetition effect operates in a much wider domain than that of just phonologically stored material. In an earlier version of this paper that included just the first three experiments presented here, we suggested that the lack of transfer observed in Experiment 3 demonstrated that what is learned during repetition learning of auditory lists with spoken recall is "very much confined to the auditory/phonological modality, as opposed to being some more general, unimodal, longer-term representation". Clearly the results of Experiments 4 and 5 have forced us to modify that position somewhat. As detailed above, we still believe that the results are more consistent with a set of parallel modality-specific stores, but it is clear that in the case of word-lists in particular, a more abstract, probably lexical, store is theoretically possible as a locus for sequence learning. We note, however, that such an account does not seem not seem able to account for all the data presented here, let alone ISR data more generally, at least not without considerable extension.

Finding a Hebb repetition effect for visual materials adds to the many similarities that have been found in the patterns observed for ISR across modalities. The serial position curves are generally similar, with extensive primacy and one- or two-item recency effects. (This pattern was repeatedly seen in the serial position curves from the experiments presented here.) Moreover, transposition errors tend to involve adjacent items, and similarity effects can be observed in both auditory and visual modalities. These resemblances have been noted before and have themselves tempted some to posit an amodal ordered store for ISR (e.g., Jones, et al., 1995). The detailed data fail to support such a position, however. As has been repeatedly shown, and replicated three times here, the phonological similarity effect is abolished when CA is employed with visual materials, though this is not the case with auditory presentation. Interestingly Smyth, et al. (2003) have gone on to show that a visual similarity effect (in ISR of unfamiliar faces) is not abolished by CA, ruling out even more strongly any general effect of CA in abolishing similarity effects simply by virtue of moving performance nearer to some effective floor. CA also removes irrelevant sound effects for visual presentation (Salamé & Baddeley, 1982; etc) and Logie (1986) has shown dissociations between the effects of irrelevant speech and irrelevant pictures on sequence recall via either rote rehearsal (phonological) or a peg-word mnemonic (visual). There is a double dissociation in the neuropsychological literature, with some patients impaired in visual and/or spatial recall but not in phonological ISR, and with others showing the opposite pattern (Basso, et al., 1982; Hanley, et al., 1991; de Renzi & Nichelli, 1975; Trojano & Grossi, 1995; Warrington & Shallice, 1969). To this large collection of evidence, we tentatively add the result from Experiment 3 (qualified of course by those from Experiments 4 and 5), namely that learning of certain materials appears to be confined within modality.

Finally, we will give a brief idea of how we think these data might fit in with previous efforts to model immediate serial recall. We shall do so with reference to the primacy model of Page and Norris (1998), a quantitative model that builds upon the qualitative foundation provided by the working memory framework. The data relating to auditory ISR, or ISR for visual materials in the absence of CA, are consistent with previous applications of the working memory framework and, hence, the primacy model. For instance, the phonological similarity effect, particularly as seen in lists of alternating confusability, is assumed to be a result of access to the phonological loop, and is well modelled by the primacy model.

Cumming, et al. (2003) have outlined (p. 61) a skeletal account of the Hebb repetition effect within an extended primacy model, that builds on earlier quantitative models by Nigrin (1993) and Page (1993; 1994). This earlier work posited chunk nodes that represent long-term sequences via primacy gradients in their incoming weighted connections, these weighted connections having been learned by reference to the short-term activation gradient that lies at the heart of the (later) primacy model. A node representing the chunk ABC, has a stronger weighted connection from the node representing A, than that from the node representing B, in turn stronger than that from the node representing C. A chunk node is designed to activate and to support improved recall when, and only when, list items arrive in the order indicated by their incoming weight gradient. Although the details of the extended model are beyond the scope of this article (and interested readers can refer to the earlier quantitative work), two things should be clear even from this cursory description.

First, that the learning of a new chunk would be dependent on the presence of an activation-gradient representing item-order within that chunk: without the activation gradient, there is no way of establishing the equivalent primacy gradient in the new chunk's incoming weights. In this way chunk learning, and by possible extension word-form learning, will require a functioning short-term memory store, as is consistent with the detailed case made by Baddeley et al. (1998). While it is the presence of a short-term representation of serial order in working memory that permits the establishment of a long-term memory represention of the corresponding list or list-chunk, the learning itself extends far beyond the limted life-span of the working memory system. Cumming et al. (2005) have shown that the learning seen in a typical Hebb repetition exeriment (i.e., that accomplished over about ten repetitions of a list) can still be observed three months later. The process of list-learning establishes a new long-term memory chunk that can itself be activated in working memory via later recognition and can thereby assist in recall of the learned list.

Second, it should also be clear that the learning of a repeating list will not transfer to a list in which only alternate items maintain their repeating-list positions. The chunk corresponding to the learned list will only activate properly in response to the same list-items in the same order. This constraint is not met for alternate-item transfer lists tested here and in Cumming et al. (2003), which would account for the lack of transfer observed experimentally.

None of the mechanism discussed in the immediately previous paragraphs relates specifically to the auditory/phonological modality. Although the available data suggest modality-specific stores, they also suggest that the sequencing and learning mechanisms within those stores are similar in nature. The simplest extension of the primacy model would, therefore, be to assume a parallel primacy gradient in the visual system, that stores order across recently active visual objects. A Hebb repetition effect in this modality would then be seen to result from precisely the same mechanism of chunk acquisition as outlined above. This would account for the lack of evidence for position-item strengthening in either modality. Given this parallel ordering mechanism, why should the phonological loop be the system of choice for the recall of lists of visually presented materials, say letters, in the absence of CA? We propose, along with Baddeley (2000), that the benefit of verbal recoding is that it places the material in a domain (audition/speech perception/speech production) for which serial order is of prime importance and within which there exist mechanisms, such as subvocal rehearsal, for maintaining order over the short term. In short, the inner voice and inner ear are specialized in the maintenance of serial order (a crucial component of speech) in a way that an "inner projector" and an "inner eye" (i.e., the machinery for visual imagery) are not. This difference is, we suggest, enough to encourage participants to use phonological recoding wherever possible, while maintaining the ability to fall back on a largely unrehearsable visual store when access to the loop is blocked. Whichever system participants use, however, repeated presentation of a list will result in a Hebb repetition effect.

References

Avons, S. E., & Mason, A. (1999). Effects of visual similarity on serial report and item recognition. Quarterly Journal of Experimental Psychology: Human Experimental Psychology, 1, 217-240.

Baddeley, A. D. (1968). How does acoustic similarity influence short-term memory? Quarterly Journal of Experimental Psychology, 20(3), 249-264.

Baddeley, A. D. (1986). Working memory. Oxford: Clarendon Press.

Baddeley, A. D. (2000). The episodic buffer: a new component of working memory? Trends in Cognitive Sciences, 4(11), 417–423.

Baddeley, A. D., Gathercole, S., & Papagno, C. (1998). The phonological loop as a language learning device. Psychological Review, 105(1), 158-173.

Baddeley, A. D., & Hitch, G. J. (1974). Working memory. In G. A. Bower (Ed.), Recent advances in learning and motivation (Vol. 8, pp. 47–90). New York: Academic Press.

Baddeley, A. D., & Lewis, V. (1981). Inner active processes in reading: The inner voice, the inner ear, and the inner eye. In A. M. Lesgold & C. A. Perfetti (Eds.), Interactive processes in reading. Hillsdale, N.J.: Lawrence Erlbaum Associates.

Basso, A., Spinnler, H., Vallar, G., & Zanobio, M. E. (1982). Left hemisphere damage and selective impairment of auditory verbal short-term memory: A case study. Neuropsychologia, 20(3), 263-274.

Besner, D. (1987). Phonology, lexical access in reading, and articulatory suppression. Quarterly Journal of Experimental Psychology, 39A, 467-478.

Besner, D., & Davelaar, E. (1982). Basic processes in reading: Two phonological codes. Canadian Journal of Psychology, 36, 710-711.

Brooks, L. R., & Vokey, J. R. (1991). Abstract analogies and abstracted grammars: Comments on Reber (1989 and Mathews et al. (1989). Journal of Experimental Psychology: General, 120, 316-323.

Burgess, N., & Hitch, G. J. (1999). Memory for serial order: A network model of the phonological loop and its timing. Psychological Review, 106(3), 551-581.

Cohen, R. L., & Johansson, B. S. (1967). Some relevant factors in the transfer of material from short-term to long-term memory. Quarterly Journal of Experimental Psychology, 19(4), 300-308.

Colle, H. A., & Welsh, A. (1976). Acoustic masking in primary memory. Journal of Verbal Learning and Verbal Behavior, 15, 17-32.

Cumming, N., Page, M., & Norris, D. (2003). Testing a postional model of the Hebb effect. Memory, 11(1), 43-63.

Cumming, N., Page, M. P. A., Norris, D., Hitch, G. J., & McNeil, A. M. (2005). Repetition frequency and item set size in the Hebb repetition task. Manuscript submitted to Journal of Memory and Language.

Cunningham, T. F., Healy, A. F., & Williams, D. M. (1984). Effects of repetition on short-term retention of order information. Journal of

Experimental Psychology: Learning, Memory, and Cognition, 10(4), 575-597.

Dulany, D. E., Carlson, R. A., & Dewey, G. I. (1984). A case of syntactical learning and judgement: How conscious and how abstract? Journal of Experimental Psychology: General, 113, 541-555.

Elman, J. L. (1990). Finding structure in time. Cognitive Science, 14, 179-212.

Estes, W. K. (1973). Phonemic coding and rehearsal in short-term memory for letter strings. Journal of Verbal Learning and Verbal Behavior, 12(4), 360-372.

Fendrich, D. W., Healy, A. F., & Bourne, L. E. (1991). Long-term repetition effects for motoric and perceptual procedures. Journal of Experimental Psychology: Learning, Memory, and Cognition, 17(1), 137-151.

Ferrand, L., & Grainger, J. (2003). Homophone interference effects in visual word recognition. Quarterly Journal of Experimental Psychology, 56A, 403-419.

Gupta, P. (2005). Primacy and recency in nonword repetition. Memory, 13, 318-324.

Hanley, J. R., Young, A. W., & Pearson, N. A. (1991). Impairment of the visuo-spatial sketch pad. Quarterly Journal of Experimental Psychology: Human Experimental Psychology, 1, 101-125.

Hebb, D. O. (1961). Distinctive features of learning in the higher animal. In

J. F. Delafresnaye (Ed.), Brain mechanisms and learning (pp. 37–46). Oxford: Blackwell.

Henson, R. N. A. (1998). Short-term memory for serial order: The start-end model. Cognitive Psychology, 36(2), 73-137.

Henson, R. N. A., Norris, D. G., Page, M. P. A., & Baddeley, A. D. (1996). Unchained memory: Error patterns rule out chaining models of immediate serial recall. Quarterly Journal of Experimental Psychology: Human Experimental Psychology, 1, 80-115.

Hitch, G. J., Fastame, M., & Flude, B. (2005). How is the serial order of a verbal sequence coded? Some comparisons between models. Memory, 13, 247-258.

Hue, C., & Erickson, J. R. (1988). Short-term memory for Chinese characters and radicals. Memory and Cognition, 16(3), 196-205.

Johnstone, T., & Shanks, D. R. (1999). Two mechanisms in implicit artificial grammar learning? Comment on Meulemans and Van der Linden (1997). Journal of Experimental Psychology: Learning, Memory, and Cognition, 25, 524-531.

Jones, D., Farrand, P., Stuart, G., & Morris, N. (1995). Functional equivalence of verbal and spatial information in serial short-term memory. Journal of Experimental Psychology: Learning, Memory, and Cognition, 21(4), 1008-1018.

Larsen, J. D., & Baddeley, A. D. (2003). Disruption of verbal STM by irrelevant speech, articulatory suppression and manual tapping: Do they have a common source? Quarterly Journal of Experimental Psychology, 56A, 1249-1268.

Levy, B. A. (1971). The role of articulation in auditory and visual short-term memory. Journal of Verbal Learning and Verbal Behavior, 10, 123-132.

Logie, R. H. (1986). Visuo-spatial processing in working memory. Quarterly Journal of Experimental Psychology: Human Experimental Psychology, 2, 229-247.

Logie, R. H., Sala, S. D., Wynn, V., & Baddeley, A. D. (2000). Visual similarity effects in immediate verbal serial recall. Quarterly Journal of Experimental Psychology: Human Experimental Psychology, 3, 626-646.

Macken, W. J., & Jones, D. M. (1995). Functional characteristics of the inner voice and the inner ear: Single or double agency? Journal of Experimental Psychology: Learning, Memory, and Cognition, 21(2), 436-448.

McKelvie, S. J. (1987). Learning and awareness in the Hebb digits task. Journal of General Psychology, 114(1), 75–88.

McKone, E., & Dennis, C. (2000). Short-term implicit memory: Visual, auditory, and cross-modality priming. Psychonomic Bulletin and Review, 7(2), 341-346.

Monsell, S. (1985). Components of working memory underlying verbal skills: A “distributed capacities view”. In H. Bouma & D. G. Bouwhuis (Eds.), Attention and performance X: Control of language processes (p. 327-350). Hove, UK: Lawrence Erlbaum Associates.

Murray, D. J. (1968). Articulation and acoustic confusability in short-term memory. Journal of Experimental Psychology, 78(4, Pt. 1), 679-684.

Nigrin, A. L. (1993). Neural networks for pattern recognition. Cambridge, MA: MIT Press.

Page, M. P. A. (1993). Modelling aspects of music perception using self-organizing neural networks. Unpublished doctoral dissertation, University of Wales College of Cardiff, Cardiff, UK.

Page, M. P. A. (1994). Modelling the perception of musical sequences with self-organizing neural networks. Connection Science, 6(2/3), 223-246.

Page, M. P. A., Cumming, N., Norris, D., Hitch, G. J., & McNeil, A. M. (2005). Exploring the importance of list recall in the Hebb repetition effect. Manuscript submitted to Memory and Cognition.

Page, M. P. A., & Norris, D. (1998). The primacy model: A new model of immediate serial recall. Psychological Review, 105, 761–781.

Perruchet, P. (1994). Defining the knowledge units of a synthetic language: Comment on Vokey and Brooks (1992). Journal of Experimental Psychology: Learning, Memory, and Cognition, 20, 223-228.

Reber, A. S. (1967). Implicit learning of artificial grammars. Journal of Verbal Learning and Verbal Behavior, 6, 855-863.

Redington, M., & Chater, N. (1996). Transfer in artificial grammar learning: a reevaluation. Journal of Experimental Psychology: General, 125, 123-128.

Renzi, E. de, & Nichelli, P. (1975). Verbal and non-verbal short-term memory impairment following hemispheric damage. Cortex, 11(4), 341-354.

Salamé, P., & Baddeley, A. D. (1982). Disruption of short-term memory by unattended speech: Implications for the structure of working memory. Journal of Verbal Learning and Verbal Behavior, 21(2), 150-164.

Schwartz, M., & Bryden, M. P. (1971). Coding factors in the learning of repeated digit sequences. Journal of Experimental Psychology, 87(3), 331-334.

Smyth, M. M., Hay, D. C., Hitch, G. J., & Horton, N. J. (2003). Serial position in the visual-spatial domain: Reconstructing sequences of unfamiliar faces. Manuscript submitted for publication.

Smyth, M. M., Pearson, N. A., & Pendleton, L. R. (1988). Movement and working memory: Patterns and positions in space. Quarterly Journal of Experimental Psychology: Human Experimental Psychology, 40(3-A), 497-514.

Stadler, M. A. (1993). Implicit serial learning: Questions inspired by Hebb (1961). Memory and Cognition, 21(6), 819-827.

Trojano, L., & Grossi, D. (1995). Phonological and lexical coding in verbal short-term memory and learning. Brain and Language, 51(2), 336-354.

Walker, P., Hitch, G. J., & Duroe, S. (1993). The effect of visual similarity on short-term memory for spatial location: Implications for the capacity of visual short-term memory. Acta Psychologica, 83(3), 203-224.

Warrington, E. K., & Shallice, T. (1969). The selective impairment of auditory verbal short-term memory. Brain: A Journal of Neurology, 92(4), 885-896.

Author Notes

The work presented here was supported by a grant from the UK Biotechnology and Biological Sciences Research Council.

We would like to thank Jane Hall, Sally Butterfield and Kathryn Daw for their invaluable help in running some of the experiments reported here. We would also like to thank Dr Elinor McKone, Prof. Dylan Jones and several anonymous reviewers for their useful comments.

All correspondence and requests for reprints should be sent to Mike Page (m.2.page@herts.ac.uk) at the Dept. of Psychology, University of Hertfordshire, Hatfield, UK, or Nick Cumming (nick.cumming@plymouth.ac.uk) at the Dept. of Psychology, University of Plymouth, Plymouth, UK.

Table 1: Mean (s.e.) gradients in proportion correct for Exp 1.

	Filler lists	Repeating lists
No CA	0.004 (0.002)	0.015 (0.002)
CA	-0.002 (0.002)	0.008 (0.002)

Table 2: Mean (s.e.) proportion correct for confusability conditions of Exp. 1

	Nonconfusable items	Confusable items
No CA	0.51 (0.02)	0.43 (0.02)
CA	0.57 (0.02)	0.57 (0.02)

Table 3: Mean (s.e.) gradients in proportion correct for Exp. 2

Repetition type		Filler lists	Repeating lists
Identical	Tapping	0.012 (0.005)	0.025 (0.005)
	CA	-0.006 (0.005)	0.028 (0.007)
Category	Tapping	-0.005 (0.005)	0.019 (0.005)
	CA	0.000 (0.005)	0.007 (0.007)

Table 4: Mean (s.e.) gradients in proportion correct for Exp. 3

	Filler lists	Repeating lists
Visual with CA	0.004 (0.003)	0.016 (0.004)
Auditory no CA	0.003 (0.003)	0.016 (0.003)

Table 5: Mean (s.e.) proportion correct in mixed lists and controls in Exp. 3

Modality	Confusability condition		
	C in mixed	N in mixed	N in filler
Visual with CA	0.37 (0.03)	0.39 (0.03)	0.39 (0.02)
Auditory no CA	0.53 (0.03)	0.64 (0.03)	0.64 (2.6)

Table 6: Mean (s.e.) proportion correct for the positional-transfer conditions in Exp. 3

	Transfer condition			
	Different pos.	Same pos.	Filler list	Repeating list
Visual with CA	0.28 (0.04)	0.39 (0.04)	0.38 (0.04)	0.58 (0.05)
Auditory no CA	0.63 (0.04)	0.62 (0.04)	0.67 (0.04)	0.82 (0.03)

Table 7: Mean (s.e.) test/filler differences in prop. correct for transfer tests in Exp. 3

Original repetition modality	Test modality	
	Visual with CA	Auditory with tapping
Visual with CA	0.14 (0.04)	0.03 (0.03)
Auditory with tapping	0.06 (0.04)	0.15 (0.02)

Table 8: Mean (s.e.) test/filler differences in prop. correct for transfer tests in Exp. 4

Original repetition modality	Test type	
	Changed presentation	Changed response
Visual with CA	0.15 (0.06)	0.17 (0.04)
Auditory with tapping	0.26 (0.06)	0.22 (0.05)

Table 9: Mean (s.e.) test/filler differences in prop. correct for original repetition effect and transfer tests in Exp. 5

Repetition modality	Repetition advantage	Test modality	
		Visual with CA	Auditory with tapping
Visual with CA	0.50 (0.03)	0.46 (0.04)	0.29 (0.04)
Auditory with tapping	0.29 (0.04)	0.35 (0.05)	0.33 (0.05)

Figure Captions

Figure 1. Graph showing the mean proportion correct for repeating lists and matched fillers in Experiment 1.

Figure 2. Graph showing the mean proportion correct for repeating lists and matched fillers in Experiment 2. The upper panels describe performance in the identical-repetition condition; the lower panels describe performance in the category-repetition condition.

Figure 3. The repetition and test structure for Experiment 3, for a subject who heard auditory lists in the first block. The two different repeating lists are shown dark and shaded. Each is tested in both blocks three and four, which differ in their presentation/response modalities.

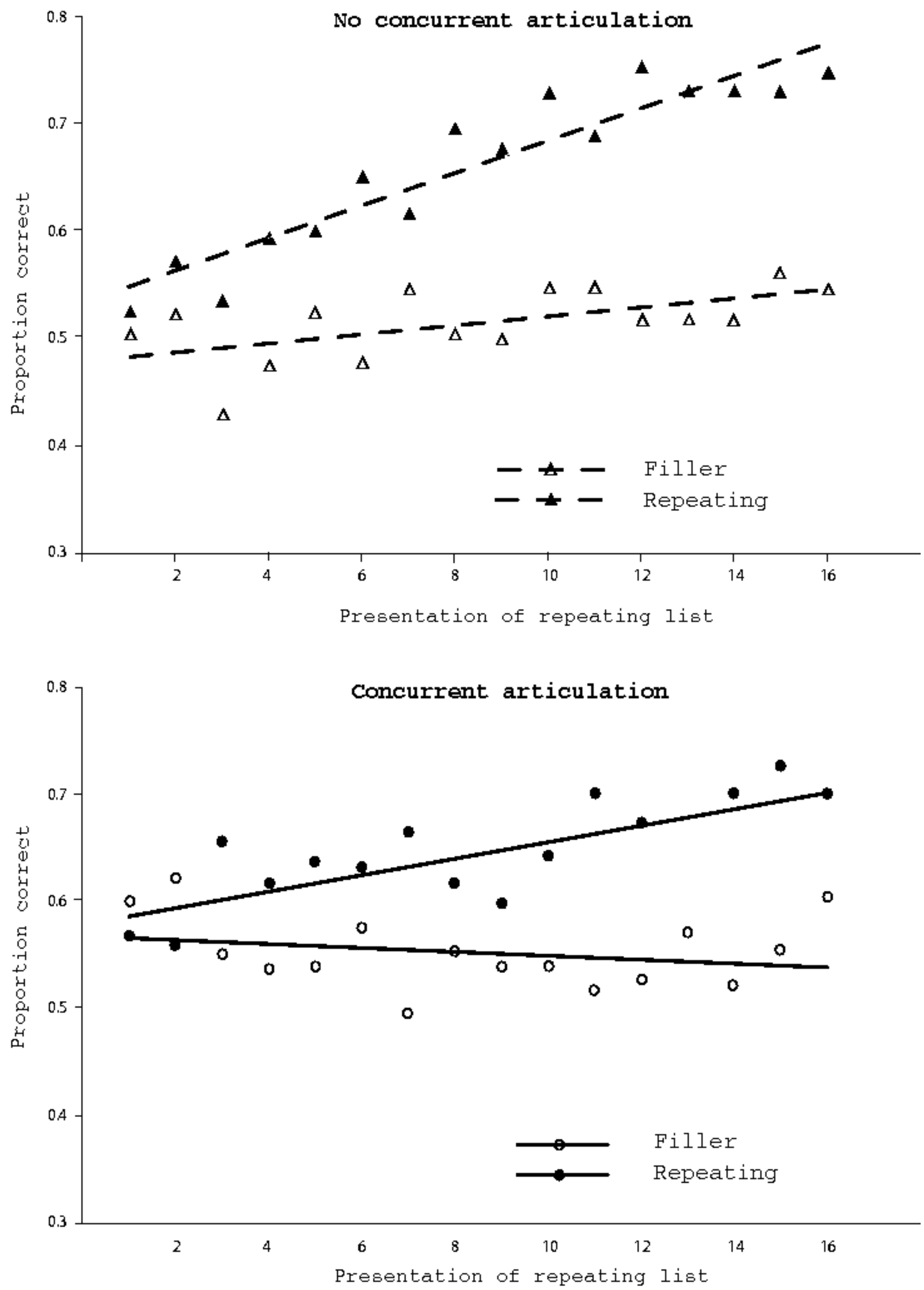
No concurrent articulation
Proportion correct
0.8
0.7
0.6
0.5
0.4
0.3
2
4
6
8
10
12
14
16
Filler
Repeating
Presentation of repeating list
Concurrent articulation
Proportion correct
0.8
0.7
0.6
0.5
0.4
0.3
2
4
6
8
10
12
14
16
Filler
Repeating
Presentation of repeating list

Figure 1:

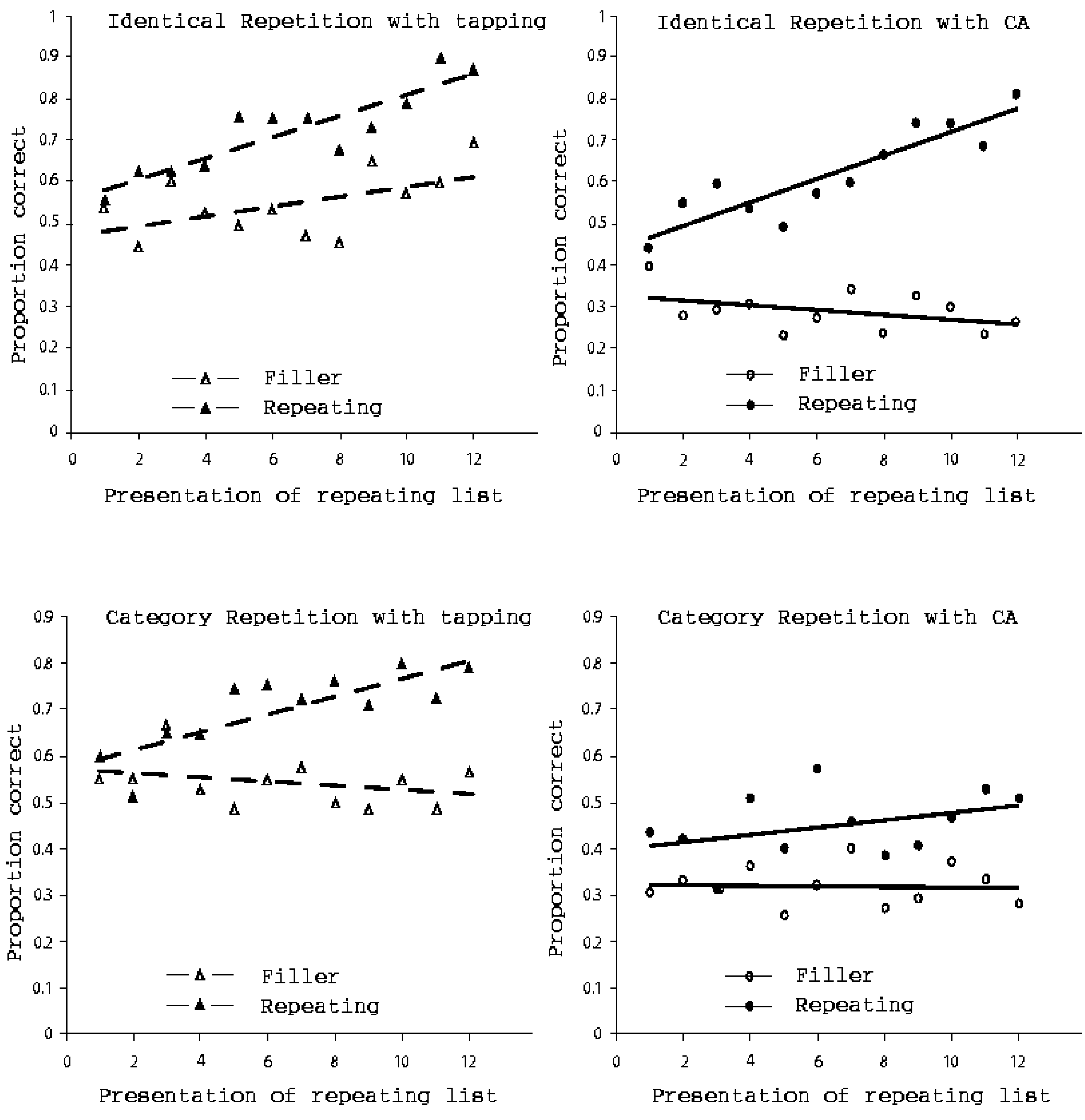
Identical Repetition with tapping
Proportion correct
Presentation of repeating list
Filler
Repeating
Identical Repetition with CA
Proportion correct
Presentation of repeating list
Filler
Repeating
Category Repetition with tapping
Proportion correct
Presentation of repeating list
Filler
Repeating
Category Repetition with CA
Proportion correct
Presentation of repeating list
Filler
Repeating

Figure 2:

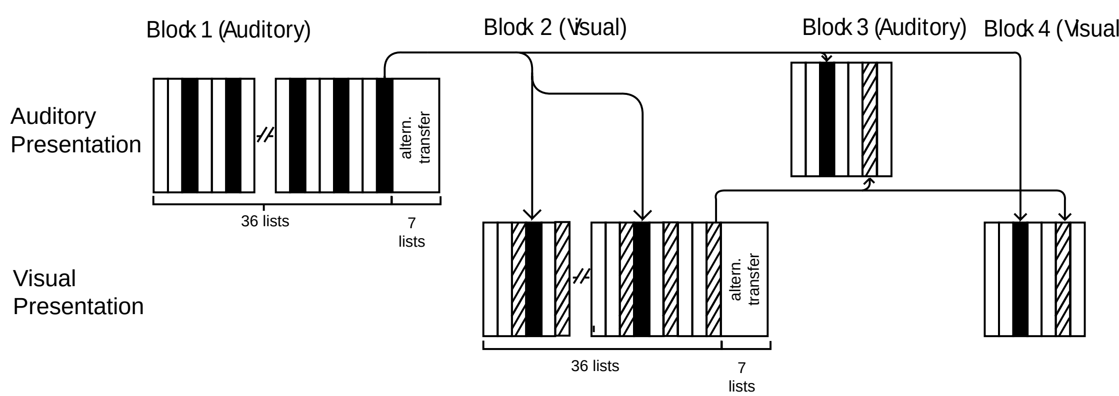
Block 1 (Auditory)
Block 2 (Visual)
Block 3 (Auditory)
Block 4 (Visual)
Auditory Presentation
Visual Presentation
altern. transfer
36 lists
7 lists
altern. transfer
36 lists
7 lists

Figure 3: