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ORIGINAL ARTICLE

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Effects of a secondary task on "implicit" sequence learning: learning or performance?

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Abstract Traditionally, implicit learning has been defined in terms of a lack of awareness of the process and products of learning. In the face of a number of conceptual and empirical difficulties with this definition, it has recently been suggested instead that the critical feature of implicit learning is that it proceeds without making any demands on attentional resources. As disconfirmatory evidence for this, we describe the results of two experiments which each used a sequential reaction time task. With a tone-counting secondary task, measures of sequence learning were significantly affected by whether training occurred under single- or dual-task conditions, regardless of whether testing took place under single- or dual-task conditions.

Introduction

The purpose of the present article is to examine a new conception of implicit learning, recently proposed in several publications by P. Frensch (Frensch, 1998; Frensch, Lin, & Buchner, 1998; Frensch, Wenke, & Rünger, 1999) and others (e.g., Cleeremans, 1997; Hayes & Broadbent, 1988; Heuer & Schmidtke, 1996; Jiménez & Méndez, 1999; Schmidtke & Heuer, 1997; Stadler, 1995). Traditionally, implicit learning has been defined as learning which takes place incidentally, in the absence of deliberate hypothesis-testing strategies, and which yields a knowledge base that is inaccessible to consciousness. Researchers have been unable to agree, however, that this standard definition picks out a meaningful psychological category (see Shanks & St. John, 1994, and accompanying commentaries): For instance, evidence that implicitly acquired knowledge is unavailable for conscious access has been highly controversial. In response to this situation, Frensch (1998) and the other authors referred to above have suggested that the feature that really characterizes implicit learning as a distinct process is that it makes no demands on attentional resources: Implicit learning, unlike explicit learning, can proceed normally in the presence of concurrent resource-demanding tasks and therefore qualifies as an automatic process. The present article scrutinizes some of the key evidence supportive of this new conception.

At first glance this "attentional" definition of implicit learning faces a number of problems. To begin with, many publications have cast doubt on the general notion of automaticity (e.g., Cheng, 1985; Kahneman & Chajczyk, 1983; Styles, 1997). Genuinely automatic cognitive processes which make no demands on central capacity have been very hard to find. For instance, on the basis of the Stroop effect, word reading is often assumed to be a prototypical automatic process, but Kahneman and Chajczyk (1983) presented evidence that Stroop interference is diluted by the presence of additional words in the display and concluded that even word reading is therefore not fully automatic.

Secondly, several implicit learning studies appear to have shown that the addition of a secondary task has an adverse effect on learning. For example, consider the sequential reaction time (SRT) task which is the focus of the present article. In this task, a target such as a dot appears in one of several possible locations on each trial and the participant presses as fast as possible a response key assigned to that location. Instead of appearing at random across a series of trials, however, the target follows a predictable sequence of locations and the issue is whether participants learn (implicitly) this sequence. Learning is measured chronometrically by changing the sequence after a number of training blocks; an increase in RTs on the transfer sequence is evidence that participants

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have learned something about the training sequence and were using their knowledge to anticipate the target location on each trial, thus achieving rapid RTs. Using this task, Cohen, Ivry, and Keele (1990, Exp. 4) obtained evidence suggesting that a concurrent tone-counting task reduced sequence learning. That is, switching from the training sequence to the transfer sequence (which was in fact a random sequence) had a small effect on RTs in a dual-task group, whereas in a single-task group the switch led to a more substantial increase in RTs. This seems to imply that implicit sequence learning is attention-demanding, contrary to Frensch's proposal. The tone-counting task required attentional resources and left participants with insufficient capacity to learn the target sequence. Other studies have confirmed that the RT increase on transfer trials is smaller under dual-task than single-task conditions (e.g., Frensch & Miner, 1994; Stadler, 1995), although an experiment by McDowall, Lustig, and Parkin (1995, Exp. 1) failed to obtain any such difference. [The effects of secondary tasks on implicit learning in the SRT task are thoroughly reviewed by Hsiao and Reber (1998) and Goschke (1997).]

Frensch (1998) does not dispute these empirical results but points out that they have an alternative interpretation. In these experiments, participants in the dual-task condition performed the tone-counting task both during the training blocks and during the transfer block. Thus, Frensch argues, it is possible that the results reflect a performance effect rather than a learning deficit. Participants may learn as much about the sequence under dual- as under single-task conditions, but may be less able to express that knowledge when tested with a concurrent task. This "suppression hypothesis" – the hypothesis that dual-task testing conditions adversely affect the expression of sequence knowledge – is supported by the following finding: Suppose participants are trained on a sequence under single- or dual-task conditions and are then tested under both single- and dual-task conditions. The suppression hypothesis predicts that the measure of sequence learning (the RT increase on the transfer block) will be lower on the dual-task than on the single-task test, regardless of training conditions, since the former but not the latter will suppress the expression of sequence knowledge. Experiments testing this prediction have been somewhat contradictory (see Curran & Keele, 1993; Frensch et al., 1999), but to cut a long story short, there is now solid evidence in support of the prediction. For example, in participants trained under dual-task conditions, Frensch et al. (1999) obtained significantly lower transfer scores on a dual-task than on a single-task test.[1]

The obvious way to avoid the difficulty created by suppression is to train some participants under dual-task conditions and others under single-task, and then test all participants under identical conditions (e.g., under single-task conditions). This brings us to the central issue to be explored in the present article: Frensch et al. (1998, Exps. 1a and b) report a pair of experiments essentially of this sort, the results of which indicate that a concurrent task during the training stage has no effect on sequence learning per se.

In Exp. 1a of Frensch et al. (1998), participants trained for 7 blocks of trials on a repeating sequence, with each block comprising 16 repetitions of a 9-location sequence. The sequence was ABCDEADFC for some participants and ABCDECFBE for others, where A-F refer to 6 screen locations (the assignment of A–F to the actual screen locations was varied across participants). On blocks 8 and 9 the structured sequence was replaced by a quasi-random sequence (in which the frequency of each location was the same as in the structured sequence), and then on blocks 10 and 11 the original sequence was reinstigated. Frensch et al. computed the difference in mean RTs between quasi-random blocks 8 and 9 versus sequence blocks 7 and 10 and took this transfer score as their measure of sequence knowledge.

For all participants, the test blocks (7–11) were conducted under single-task conditions. The major independent variable was the presence of a secondary task during the training stage. This task, which has been used in many similar experiments (e.g., Cohen et al., 1990; Nissen & Bullemer, 1987), involved presentation of a high- or low-pitched tone in the interval between the visual targets. Participants were required to count the number of high-pitched tones during each block and report the number at the end of the block. For some participants the training stage (blocks 1–7) was conducted mainly under single-task conditions, whereas for others most of the training blocks included a secondary task. Specifically, for group 2-DT/5-ST, the first two blocks were run under dual-task (DT) conditions but the remaining five blocks were single-task (ST); for group 4-DT/3-ST the first four blocks were run under dual-task conditions and the remaining three were single-task; and for group 6-DT/1-ST the first six blocks were run under dual-task conditions and the remaining block was single-task. Hence, the two extreme groups (2-DT/5-ST, 6-DT/1-ST) compare conditions of mainly single-task and mainly dual-task training.

The key question is whether this manipulation of training conditions affects sequence learning in circumstances where testing is conducted under identical (single-task) conditions. The results were clear: Transfer scores were very nearly identical (approx. 85 ms) in the three groups. Thus, participants learned the sequence equally well regardless of the inclusion of a secondary task. In their Exp. 1b, Frensch et al. (1998) replicated this pattern but in a situation where sequence knowledge was now assessed under dual-task conditions. Here the learning effect was smaller, with transfer scores of about

[1] At least, this is the case when the secondary-task stimuli appear in a random order. When they too follow a structured sequence, transfer scores can be as large (or even larger) in dual-task as in single-task tests (Schmidtke & Heuer, 1997). Schmidtke and Heuer (1997) attribute this to integration of the visual and auditory sequences, but this is not relevant to the present issue.

55 ms, but again the scores did not vary as a function of how many training blocks included the secondary task.[2] The fact that the scores were lower overall in this experiment supports the suppression hypothesis: The inclusion of a secondary task during the testing phase tends to reduce transfer scores.

Similar results have been reported by other researchers. Seger (1997) and Cleeremans and Jiménez (1998) found nearly identical transfer scores in participants trained under single- or dual-task conditions when they were tested under identical conditions. Also, some data reported by McDowall et al. (1995, Exp. 3) support the same conclusion. These authors trained one group of subjects for five blocks under single-task conditions and another group for four blocks under dual-task followed by a final block under single-task conditions. On block 5, the mean RT of the two groups was comparable. The absolute level of RTs is probably a poor measure of sequence knowledge, compared to the effect of transfer to a random sequence, but nevertheless these results are consistent with the view that sequence learning under single- and dual-task conditions does not differ.

Schvaneveldt and Gomez (1998, Exp. 3) found evidence consistent with Frensch's hypothesis, albeit with one important proviso. These authors used probabilistic rather than deterministic sequences, in which each trial had a 90% chance of being consistent with an underlying sequence and a 10% chance of being inconsistent. The difference in RTs to these probable and improbable stimuli provided a continuous measure of sequence knowledge. Schvaneveldt and Gomez obtained an RT difference of 51 ms at the end of the training stage in a single-task group and a difference of 56 ms in a group trained under dual-task conditions and then switched to single-task testing. Again, sequence learning (measured by RT) under single- and dual-task conditions did not differ noticeably. The proviso is that error rates (an error being an incorrect keypress) were higher in the single-task group at the end of the training stage than in the dual-to-single task group during the test stage. If we assume that better sequence knowledge generates more errors with this version of the SRT task (because a participant who knows the underlying sequence is more likely to incorrectly anticipate the "consistent" location on an inconsistent trial), then the error data suggest that sequence learning was after all somewhat better in the single-task group. Jiménez and Méndez (1999), like Schvaneveldt and Gomez, found no effect of a secondary task on the learning of a probabilistic sequence as expressed chronometrically. However, in this study the tendency for single-task participants to make more anticipation errors, although slightly greater than in the dual-task group (see their Fig. 2), was not reliable, leading Jiménez and Méndez to argue for the attentional independence of implicit sequence learning. The basis of the different outcomes obtained by Schvaneveldt and Gomez and Jiménez and Méndez remains unclear.

A study by Heuer and Schmidtke (1996, Exp. 1), which again used tone counting as the secondary task, obtained a small but reliable difference between groups trained under single- and dual-task conditions and then tested under single-task conditions. However, compared to the designs used by Frensch and his colleagues, this study is not ideal. The single-task test phase immediately followed training for participants trained under single-task conditions, whereas the comparable test for participants trained under dual-task conditions occurred somewhat later in the experiment, after a dual-task test [see footnote 2 for a similar observation regarding Frensch et al.'s (1998) Exps. 2a and b]. The possible contaminating effects of the prior test in the group trained under dual-task conditions are unknown. Thus, although the results of these various studies are contradictory, the experiments reported by Frensch et al. (1998), Exps. 1a and b) seem to come closest to the ideal of a design specifically intended to allow the performance and learning accounts to be distinguished.

These studies are important because they tend (putting aside Heuer and Schmidtke's data) to support a conception of implicit learning in which the role of attention is rather different from that seen in more typical (explicit) learning tasks: Full attention seems not to be necessary for implicit sequence learning to proceed normally. On the other hand, there are some reasons why the results should be regarded with a certain amount of caution. For example, Frensch et al. (1998) gave all of their groups both single- and dual-task training, rather than giving one group just single-task training and another group just dual-task. In addition, the training conditions of even the most extreme groups (2-DT/5-ST vs. 6-DT/1-ST) only differed on 4 blocks of trials. The design Frensch et al. adopted therefore tends to reduce the likelihood of obtaining a group difference in transfer scores and their study may, therefore, constitute a fairly conservative test of the experimental hypothesis.

Moreover, Frensch et al. included in their analysis all participants whose average tone-counting error on the dual-task training blocks was 20% or less. This is a very liberal criterion and means that participants were included in the analysis who may have been allocating

[2]In their Exps. 2a and b, Frensch et al. (1998) trained participants under either single- or dual-task conditions, prior to testing all participants under both single- and dual-task conditions. Transfer scores were generally lower in the dual-task tests than in the single-task ones, consistent with the suppression hypothesis. However, transfer scores in the single-task test were larger for participants given single-task training than for those given dual-task training. On the basis of this, Frensch et al. (p. 95) softened their position slightly and concluded that, in addition to affecting performance, the secondary task does after all affect sequence learning to some degree. However, this concession is unwarranted since the comparison between groups trained under single- and dual-task conditions was confounded in these experiments with the point at which the single-task test was administered. For the group trained under single-task conditions, the test occurred on trial blocks 5 and 6 while for the group trained under dual-task conditions, it occurred on blocks 7–10. The experimental designs of Frensch et al.'s Exps. 1a and b avoid this confound and provide no hint of an effect of the secondary task on sequence learning.

minimal attention to the secondary task. Such participants would be expected to show large transfer scores since, functionally, they are performing the task just like single-task participants. Naturally, a strong test of the experimental hypothesis requires some evidence that dual-task participants were indeed concentrating to an adequate level on the secondary task. It is not clear why Frensch et al. adopted this liberal criterion rather than the more common criterion of 10% (e.g., Cohen et al., 1990). In our experiments, participants were excluded if they made more than 10% errors on average.

Thirdly, Frensch et al. used training and transfer sequences that have a number of undesirable properties.[3] For instance, inspection of the training sequences (ABCDEADFC and ABCDECFBE) reveals immediately that they contain no reversals, that is, occasions on which the target moves back to the location it occupied on the last-but-one trial (e.g., ABA). In contrast, the quasi-random sequence presented in the test stage does contain reversals. Suppose participants learn the abstract feature of the training sequences that they contain no reversals. At any moment during the training phase the participant knows that the target will not appear in 2 of the 6 possible locations: the location of the last trial (since there are no immediate repetitions) and the last-but-one location. In the test phase, the target does sometimes appear in the reversal location, and RTs would be expected to be particularly slow on such trials. Hence, the transfer scores Frensch et al. obtained may be inflated; in fact, it is possible that many participants had no specific sequence knowledge at all. In that case, the fact that the transfer scores did not differ is uninformative.

The presence versus absence of reversals is only one feature that differs between the training and test sequences used by Frensch et al. Reed and Johnson (1994) have identified several such factors (e.g., rate of coverage, the mean number of trials required to see the target appearing in each of the possible locations) and have provided an elegant method for avoiding these difficulties. Rather than switching participants to a quasi-random sequence, they are transferred to a sequence that is structurally identical to the training sequence but which is instantiated differently in terms of assignment to screen locations.

In the present experiment, therefore, we conducted a conceptual replication of Exp. 1a of Frensch et al., but we presented one group with only single-task training blocks and another with only dual-task blocks, and we used Reed and Johnson's sequences to avoid the problems described above. In Exp. 2 we tested participants under dual-task conditions (as in Exp. 1b of Frensch et al.) as well as under single-task conditions.

[3]The problem described in this paragraph is not specific to the experiments reported by Frensch et al. (1998): with slight alterations, it applies to almost all research using the SRT task, including that of Heuer and Schmidtke (1996), Schmidtke and Heuer (1997), and Stadler (1995). Researchers have been slow to recognize the methodological points raised by Reed and Johnson (1994) and to adopt the procedures they developed for dealing with them.

Experiment 1

The training and test sequences in our experiments were A = 1-2-1-3-4-2-3-1-4-3-2-4 and B = 4-2-4-3-1-2-3-4-1-3-2-1, where 1–4 are screen locations. These sequences are structurally identical and are related by the transformation 1↔4. They are balanced for simple location and transition frequency. Each location (e.g., 1, 2, 3, 4) occurs three times in each 12 trial sequence, and each possible transition (e.g., 1-2, 1-3, 1-4, etc.) occurs once. However, at the level of three (or more) consecutive locations the two sequences differ. Reed and Johnson (1994) gave sequences of three locations the name second order conditionals (SOCs), which refers to the fact that the next location in the sequence of dot movements can be predicted from the last two locations. For example in sequence A, 1-2 is always followed by 1, whereas in sequence B, it is always followed by 3. Because the sequences are structurally identical, any increase in RTs in the test block must reflect sequence knowledge rather than the confounding of structural properties such as the frequency of reversals.

Method

Participants

Eighty-four psychology undergraduates at University College London carried out the experiment as part of a course requirement. Participants were randomly assigned to three groups: Single/Repeating, Dual/Repeating, or Dual/Nonrepeating. After eliminating 13 participants who made more than 10% tone-counting errors (see below), there were 26 participants in the Single/Repeating group, 22 in the Dual/Repeating group, and 23 in the Dual/Nonrepeating group.

Procedure

Stimulus presentation, RT measurement and response recording were all implemented on Elonex PC-333 IBM compatible PCs with 33-cm color monitors and standard QWERTY keyboards. Participants in the Dual/Repeating and Dual/Nonrepeating groups were told that they were taking part in a simple choice RT experiment designed to see how fast people can become at responding to the location of a stimulus when they have to perform a concurrent tone-counting task. For participants in the Single/Repeating group, the secondary task was not mentioned. All participants performed 14 blocks of 96 trials in the training phase. They then performed a free generation task to assess explicit knowledge.

Serial RT task

Four boxes were presented at the bottom of the computer screen drawn with white lines against a blue background. The boxes were 3.5 cm wide and 2 cm deep. A dot (2 mm in diameter) appeared in the center of one of these boxes on each target location trial. Target locations are referred to as 1–4 from left to right. Participants were instructed to indicate locations 1–4 as quickly as possible using the V, B, N, and M keys located across the bottom of the keyboard,

Table 1. Design of Exp. 1 (*NRPT* nonrepeating, *SOC1*/*SOC2* second-order conditional sequences)

Group	Blocks 1–10	Block 11	Block 12	Blocks 13–14
Single/Repeating	Single SOC1	Single SOC1	Single SOC2	Single SOC1
Dual/Repeating	Dual SOC1	Single SOC1	Single SOC2	Single SOC1
Dual/Nonrepeating	Dual NRPT	Single SOC1	Single SOC2	Single SOC1

respectively. They responded to locations 1 and 2 with the middle and index fingers of their left hands, respectively, and to locations 3 and 4 with their index and middle fingers of their right hands, respectively.

Each block of target-location trials began at a random point in the sequence, and thereafter targets appeared according to the sequence that corresponded to the particular condition and block type. A target-location trial ended when a participant pressed the correct key, at which time the target was erased. The next trial began 200 ms later. Response latencies were measured from the onset of the targets to the completion of correct responses. RTs from the first two trials of each block were excluded from the analysis.

Sequence information

Two different SOC sequences (A and B above) were used and these are taken from Reed and Johnson (1994, Exp. 2). For approximately half the participants in the Single and Dual/Repeating groups, the training sequence (designated SOC1) was A and the test sequence (SOC2) was B. For the remaining participants these were reversed. For participants in the Dual/Nonrepeating group, the test sequences were counterbalanced across sequences A and B in a similar way. Each of these 12-item sequences was repeated 8 times in each block of 96 trials. We also constructed a non-repeating SOC sequence (NRPT) for group Dual/Nonrepeating, in a similar way to Reed and Johnson (1994), to provide simple location and transition frequency information that matched the two repeating SOC sequences. This was accomplished by (a) selecting eight different 12-target SOC sequences, each of which consisted of the 12 possible target transitions, and (b) ordering these SOC sequences to form a series of 96 target locations that exhibited the desired location and transition frequencies. Reed and Johnson's nonrepeating sequence contained no reversals but, since one reversal occurred in each SOC cycle, reversals were included in our nonrepeating sequence.

Table 1 gives the experimental design for the main RT phase. During blocks 1–10, the dot followed sequence SOC1 for participants in groups Single/Repeating and Dual/Repeating, while the nonrepeating sequence was presented to participants in group Dual/Nonrepeating. Participants in group Single/Repeating performed the RT task alone, while participants in groups Dual/Repeating and Dual/Nonrepeating performed the secondary task as well. On blocks 11–14, all groups were treated identically. On block 11, sequence SOC1 was used under single-task conditions. Participants in groups Dual/Repeating and Dual/Nonrepeating were informed prior to this block that they were no longer required to perform the tone-counting task, but that they should continue to respond to the target as rapidly as possible. On block 12 sequence SOC2 was used, and on blocks 13–14 sequence SOC1 was re-introduced. The relative slowing down on block 12 compared to blocks 11 and 13 provided the main index of sequence knowledge.

Tone-counting task

In each block of dual-task RT trials, a 100-ms computer generated tone was emitted 100-ms after each correct target location response. Each tone was randomly determined to be either low (1,000 Hz) or high (2,000 Hz), and participants were instructed to count the number of high tones emitted during each block of trials. At the end of each block, participants were asked to provide their count. If they made less than 5% errors they were informed that their tone counting was accurate and asked to continue their good performance. If a participant made 5% or more errors, they were told their error percentage and encouraged to try harder to attend to their tone-counting accuracy.

Free generation test

After block 14, participants performed a test to assess their sequence awareness. They were told that they would have to do a slightly different task in the final block of trials. There would no longer be any tones for them to count, nor would their RTs be measured. Instead, we wanted them to press the keys 96 times, attempting to freely generate the training sequence that they saw in the RT phase. They were told that each time they pressed a key, a dot would appear in the appropriate box and that this dot would remain on the screen until they pressed a further key. They were told not to worry if their memory of the sequence was poor, just to try to generate any sequences of key presses that seemed familiar. The dot moved whenever the participant pressed one of the four specified keys. This free generation task has been widely used in previous research as a test of the extent to which sequence knowledge is available to consciousness (Perruchet & Amorim, 1992; Shanks & Johnstone, 1999). Although the issue of whether sequence knowledge is consciously accessible was not our primary

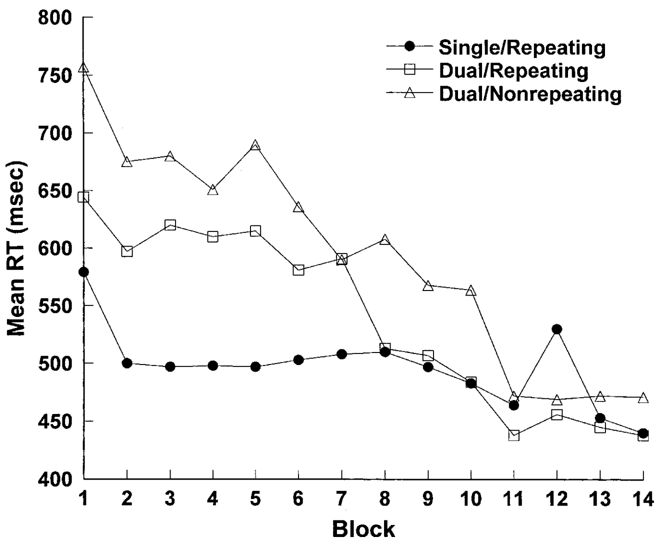

Fig. 1. Mean reaction times across blocks of trials in Exp. 1. Groups Single/Repeating and Dual/Repeating were trained on sequence SOC1 on blocks 1-10, while group Dual/Nonrepeating was trained on a nonrepeating sequence. Group Single/Repeating performed under single-task conditions in all blocks, while groups Dual/Repeating and Dual/Nonrepeating performed under dual-task conditions in blocks 1–10 prior to the removal of the secondary task on block 11. Sequence SOC1 was used for all groups on blocks 11 and 13–14, while sequence SOC2 was used on the transfer block, block 12 (*SOC* second order conditional)

interest in this research, we included the free generation test in this experiment to provide some additional data on this controversial issue.

Results

Using the same criterion as Cohen et al. (1990) and Reed and Johnson (1994), participants were eliminated from our analysis if their average tone-counting accuracy was in error by more than 10%. On this basis, a total of 13 participants were excluded from the analysis, 6 from the Dual/Repeating group and 7 from the Dual/Nonrepeating group.

Figure 1 presents mean RTs for each of the three groups across blocks. Participants in group Single/Repeating rapidly reached a stable level of short RTs, which they maintained across the training blocks. Participants in groups Dual/Repeating and Dual/Nonrepeating were slower initially, but on blocks 8–10 RTs were equivalent in the Single/Repeating and Dual/Repeating groups, suggesting that participants in the latter group had developed the skill of combining the two tasks with minimal interference of tone counting on RTs. A two-way analysis of variance (ANOVA) on blocks 1–10, with group (Single/Repeating, Dual/Repeating, Dual/Nonrepeating) as a between-subjects variable and block as a within-subjects variable revealed significant effects of block, $F(9, 612) = 21.97$, $MSE = 5{,}976$, $P < 0.001$, and group, $F(2, 68) = 6.83$, $MSE = 162{,}677$, $P < 0.005$, as well as an interaction between block and group, $F(18, 612) = 3.75$, $MSE = 5{,}976$, $P < 0.001$.

On block 11, all participants performed the SRT task under single-task conditions, and no RT difference was present. A one-way ANOVA on the mean RTs across groups found no significant difference, $F(2, 68) = 1.36$, $MSE = 5{,}414$.

The principal data concern the changes in RTs on block 12. For group Single/Repeating, the introduction of sequence SOC2 was accompanied by a very substantial increase in reaction times, but RTs returned to their earlier level on blocks 13–14. For group Dual/Repeating, a very small increase in RTs occurred on block 12, with RTs again returning to their earlier level on blocks 13–14. For group Dual/Nonrepeating, no change occurred across the test blocks. To assess sequence knowledge, we computed a difference (D) score based on the difference between the RT on block 12 and the average RT on blocks 11 and 13. The mean D scores for the three groups are shown on the left of Fig. 2. An ANOVA on these scores revealed a significant group effect, $F(2, 68) = 21.64$, $MSE = 1{,}766$, $P < 0.001$. The critical result was that D scores were about 60 ms higher in group Single/Repeating than group Dual/Repeating, and this difference (using the error term from the ANOVA) was reliable, $t(68) = 4.70$, $P < 0.001$, demonstrating less evidence of sequence learning under dual-task conditions. Indeed, although D scores were greater by about 20 ms in the Dual/Repeating group than the Dual/Nonrepeating group, this difference was not significant, $t(68) = 1.44$, $P > 0.1$, although with a larger sample size it would perhaps be statistically reliable. D scores were significantly greater than zero in the Dual/Repeating group, however, $t(21) = 1.93$, $P < 0.05$ (one-tailed).

Error rates were fairly low and consistent across blocks, although higher in group Single/Repeating than in the other two groups. On average participants made 11.9 location errors per block in group Single/Repeating, 3.9 in group Dual/Repeating, and 5.6 in group Dual/Nonrepeating, $F(2, 68) = 8.90$, $MSE = 49.0$. The elevation of errors under single-task conditions is consistent with previous research and is probably attributable to a greater proportion of incorrect anticipation responses (see Schvaneveldt & Gomez, 1998), consistent with more sequence knowledge.

We next analyzed the free generation data to gauge the extent to which sequence knowledge was consciously accessible. There are many ways of analyzing such data (see Shanks & Johnstone, 1999) but here we present just one. Each participant generated a string of 96 consecutive keypresses. We computed the total number of generated sub-sequences which were consistent with the participants' training sequence, SOC1. Thus, if a participant was trained on sequence A (= 1-2-1-3-4-2-3-1-4-3-2-4) and at some point generated the sub-sequence 3-4-2-3-1-3, this would count as five sub-sequences of length two (3-4, 4-2, 2-3, 3-1, and 1-3), three sub-

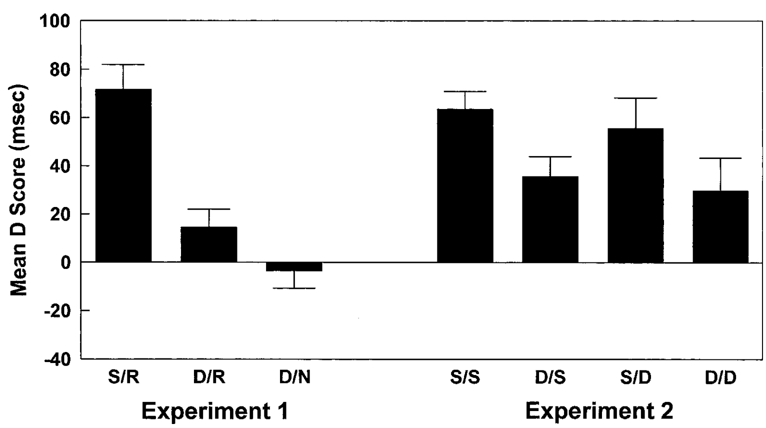

Fig. 2. Mean D score (RTs on the transfer block minus the average RTs on the preceding and subsequent blocks) in each group of Exps. 1 and 2. *Error bars* indicate standard errors (*RT* reaction time, *S/R* Single/Repeating, *D/R* Dual/Repeating, *D/N* Dual/Nonrepeating, *S/S* Single/Single, *D/S* Dual/Single, *S/D* Single/Dual, *D/D* Dual/Dual)

Table 2. Free generation data in Exp. 1

Group	Length											
		2	3	4	5	6	7	8	9	10	11	12
Single/Repeating	SOC1	2406	1014	495	222	109	49	22	12	6	1	0
	SOC2	2406	940	429	161	57	17	6	2	1	0	0
Dual/Repeating	SOC1	2030	858	428	229	130	79	52	35	22	13	7
	SOC2	2030	792	359	141	63	32	15	7	3	1	0
Dual/Nonrepeating	SOC1	2131	837	352	154	71	33	15	5	2	0	0
	SOC2	2131	836	357	144	57	22	8	2	0	0	0

sequences of length three (3-4-2, 4-2-3, and 2-3-1), two of length four (3-4-2-3 and 4-2-3-1), and one of length five (3-4-2-3-1) from the training sequence. As a control, we also computed the total number of generated sub-sequences which were consistent with sequence SOC2 which was used in the transfer test. Sequence knowledge is accessible to consciousness to the extent that participants generate more SOC1 than SOC2 sub-sequences.

Table 2 presents the results. In the Dual/Nonrepeating group participants generated sub-sequences consistent with the SOC1 and SOC2 sequences about equally often, as expected. The slight tendency towards generating SOC1 sub-sequences is consistent with the fact that participants were exposed to that sequence on two training blocks, whereas SOC2 was presented on only one. In contrast, both groups Single/Repeating and Dual/Repeating showed marked tendencies to generate fragments of SOC1 rather than SOC2. For example, on seven occasions the entire sequence was correctly generated in the Dual/Repeating group. These results therefore confirm previous reports (Perruchet & Amorim, 1992; Shanks & Johnstone, 1999) that sequence knowledge in the SRT task can be freely generated.

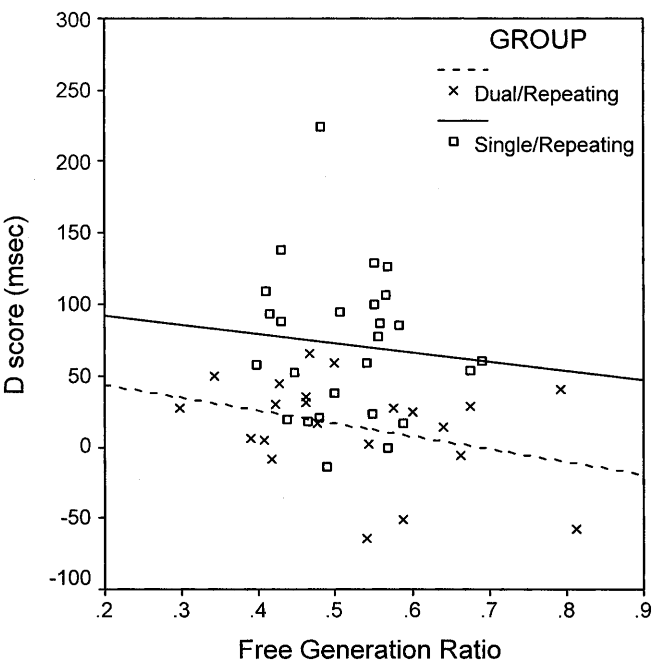

Fig. 3. Mean D scores plotted against free generation ratios for each participant in groups Single/Repeating and Dual/Repeating of Exp. 1. Best-fitting linear regression lines are also plotted

Frensch et al. (1998) made the point that the claim that sequence learning can be independent of attentional demands only applies to participants lacking any explicit knowledge, and hence removed from their analyses participants who appeared to possess explicit knowledge. It is important to note that in the present experiment, however, the difference between groups Single/Repeating and Dual/Repeating in their mean D scores is not attributable to "contamination" by participants with explicit knowledge of the sequence. If we define a 'free generation ratio' as the proportion of generated sub-sequences of length three that are consistent with SOC1 relative to the total number of generated sub-sequences of length three that are consistent with either SOC1 or SO2, we obtain a measure which should have a value of 0.50 in participants completely lacking explicit knowledge of their training sequence and a value greater than 0.50 in participants with sequence knowledge (see Shanks & Johnstone, 1999, for a detailed analysis of such ratios). Note that the conclusions presented below remain unchanged when other sequence lengths are used to index explicit knowledge.

Figure 3 shows these ratios plotted against D scores in the two critical groups, Single/Repeating and Dual/Repeating. It is fairly clear from the figure that the single-dual difference in D scores is unrelated to the degree of explicit knowledge: the regression lines are almost perfectly parallel. Thus, the group difference cannot be attributed to a small number of participants in the Single/Repeating group who possessed large amounts of explicit knowledge combined with large D scores. A multiple linear regression in which D scores were regressed onto free generation ratios and training conditions (single or dual task) revealed a highly significant contribution of training conditions ($P < 0.001$) but no contribution of free generation ratios ($P > 0.1$). Hence, the difference in sequence learning between groups Single/Repeating and Dual/Repeating appears to be unrelated to variations in explicit knowledge.

Discussion

The present findings are straightforward: Under common testing conditions, sequence knowledge is substantially greater in a group trained under single-task conditions than in one trained under dual-task conditions. We thus fail to replicate the null effect reported by Frensch et al. (1998). At variance with the attentional hypothesis of implicit learning, the results suggest that the division of attention impairs sequence learning.

Our results replicate those of McDowall et al. (1995, Exp. 3), but also suggest a reason why their data do not support the view that sequence learning is equivalent under single- and dual-task conditions. Recall that these authors trained one group of subjects for five blocks under single-task conditions and another group for four blocks under dual-task followed by a final block under single-task conditions. On block 5, the mean RT of the two groups was comparable. On the face of it, this finding indicates equivalent levels of learning in the two groups. Similarly, in our experiment, the mean RT of participants in the Dual/Repeating group was the same as that of participants in the Single/Repeating group on block 11. Despite this, the switch to SOC2 revealed that the latter had more sequence knowledge. This strongly implies that the absolute level of RTs is a poor measure of sequence knowledge, compared to the effect of transfer to a random sequence. The omission of a transfer test is a major shortcoming in McDowall et al.'s experiment.

The present results are also consistent with the pattern obtained by Schvaneveldt and Gomez (1998) in their error data. Although they obtained no RT difference between a single- and a dual-task group, the former produced more errors than the latter. If we assume that errors represent greater sequence knowledge, then their results are consistent with those obtained here.

Experiment 2

In Exp. 1 we restricted ourselves to testing sequence knowledge under single-task conditions, but the case against the attentional hypothesis would plainly be stronger if a comparable set of results emerged under dual-task testing conditions. On the basis of the results of Exp. 1, our prediction would be that transfer scores would again be lower in a dual- than in a single-task training group, even if testing were conducted for both groups under dual-task conditions (contrasting with the results obtained by Frensch et al., 1998, Exp. 1b). We might anticipate D scores to be lower overall under dual- than under single-task testing conditions (because of suppression), but we would still anticipate a group difference. We test this prediction in the present experiment and we also include conditions to replicate Exp. 1. In a factorial design, participants were trained under either single- or dual-task conditions prior to being tested under single- or dual-task conditions.

Method

Participants

Seventy-two undergraduates at University College London took part in the experiment. Participants were randomly assigned to four groups constructed according to whether training took place under single- (groups Single/Single and Single/Dual) or dual-task (groups Dual/Single and Dual/Dual) conditions. After eliminating 14 participants who made more than 10% tone-counting errors (see below), there were 19 participants in group Single/Single, 20 in group Single/Dual, 10 in group Dual/Single, and 9 in group Dual/Dual.

Procedure

Except where specifically mentioned, the procedure was identical to that of Exp. 1. During blocks 1–8, participants in groups Single/Single and Single/Dual performed the RT task alone, while participants in groups Dual/Single and Dual/Dual performed the secondary task as well. The sequence (SOC1) was A for roughly half the participants in each group and B for the remainder. On blocks 9–11, groups Single/Dual and Dual/Dual performed the SRT task combined with the tone-counting task, whereas the other two groups performed it alone. Participants in group Single/Dual were informed prior to this block about the tone-counting task. On block 10 sequence SOC2 was used, and on block 11 sequence SOC1 was re-introduced. Only one block with sequence SOC2 followed the transfer block. Participants in this experiment did not perform the free generation test.

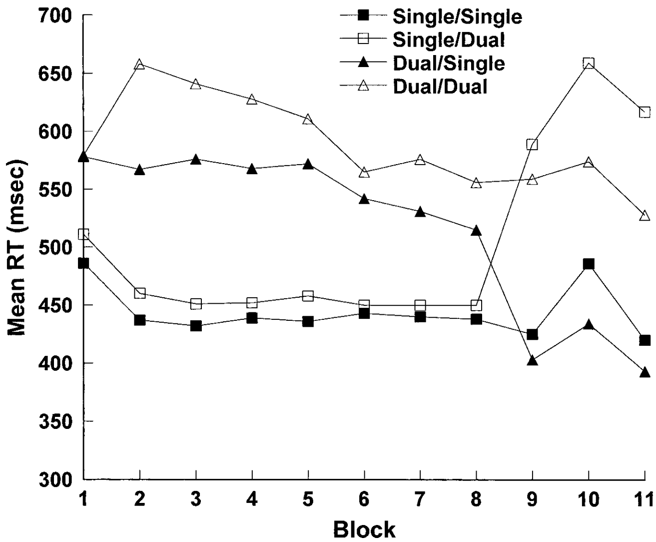

Fig. 4. Mean reaction times across blocks of trials in Exp. 2. Groups Single/Single and Single/Dual performed under single-task conditions in blocks 1–8, while groups Dual/Single and Dual/Dual performed under dual-task conditions. The secondary tone-counting task was performed concurrently with the RT task on blocks 9–11 in groups Single/Dual and Dual/Dual, while groups Single/Single and Dual/Single performed these blocks under single-task conditions. Sequence SOC1 was used for both groups on blocks 1–9 and 11, while sequence SOC2 was used on the transfer block, block 10

Results and discussion

Figure 4 presents mean RTs for each group across blocks. Participants in groups Single/Single and Single/Dual rapidly reached a stable level of short RTs which they maintained across blocks 1–8. Participants in groups Dual/Single and Dual/Dual were considerably slower. A three-way ANOVA on blocks 1–8, with training (single vs dual) and testing (single vs dual)[4] conditions as between-

[4]This is of course a dummy variable.

subjects variables and block as a within-subjects variable, revealed significant effects of block, $F(7, 378) = 13.02$, $MSE = 1{,}156$, $P < 0.001$, and training conditions, $F(1, 54) = 32.65$, $MSE = 50{,}344$, $P < 0.001$, but no effect of testing conditions, $F(1, 54) = 1.95$, $MSE = 50{,}344$. The Block × Training conditions interaction was also significant, $F(7, 378) = 12.67$, $MSE = 1{,}156$, reflecting the fact that participants speeded up more across blocks under dual than single task conditions. Lastly, the Block × Testing conditions, $F(7, 378) = 2.24$, $MSE = 1{,}156$, and Block × Training × Testing conditions interactions, $F(7, 378) = 2.04$, $MSE = 1{,}156$, $P = .05$, were also significant. These latter interactions appear to be due to the fact that participants in group Dual/Dual were somewhat slower overall than those in group Dual/Single. On block 9 the new conditions came into effect and RTs were now considerably longer in the two groups receiving dual-task conditions (groups Single/Dual and Dual/Dual). Between blocks 8 and 9 there was an almost perfectly symmetrical relationship between the speed-up of RTs in group Dual/Single and the slowdown in group Single/Dual. Block 9 also reveals a form of behavioral contrast: single-task responding is slower after single- than dual-task training (also evident in Exp. 1), while dual-task responding is faster after dual- than single-task training.

The principal data concern the change in RTs on block 10. Contrasting with the results of Frensch et al. (1998, Exp. 1b), the increase was largest in groups Single/Single and Single/Dual than in the other two groups, for whom the increase was very small. That is to say, there was more disruption in responding in the groups trained under single-task conditions than in those trained under dual-task conditions, regardless of testing conditions, and this is consistent with the secondary task interfering with sequence learning. Figure 2 shows this pattern more clearly. An ANOVA on the mean D scores revealed a reliable effect of training conditions, $F(1, 54) = 5.03$, $MSE = 1{,}832$, but no effect of testing conditions and no interaction, $F < 1$ in both cases.

Error rates were low and consistent across blocks. On average participants made 6.9, 5.7, 5.5, and 6.2 location errors per block in groups Single/Single, Single/Dual, Dual/Single, and Dual/Dual, respectively. Neither the main effect of training conditions, the main effect of testing conditions, nor the interaction was significant, $F < 1$ in each case.

Overall these results are very straightforward: they confirm that under the conditions used here, sequence learning is impaired by a secondary task. We replicated the results of Exp. 1, with D scores being larger in group Single/Single than in group Dual/Single, but we also found the same pattern under dual-task testing conditions. Although testing conditions had an overall effect on RTs (which were longer under dual- than single-task conditions), they had no detectable effect on the expression of sequence knowledge, which continued to be greater for those participants trained under single-task conditions.

General discussion

The results of the present experiments are consistent in suggesting that attention cannot be divided without detrimentally affecting implicit sequence learning. This is most clear in Exp. 2 where dual-task training conditions impaired sequence learning, independently of testing conditions. Our results are in conflict with those of Frensch et al. (1998) in two respects. First, in both experiments we obtained greater learning scores in groups trained under single-task conditions than in groups trained under dual-task conditions, regardless of the testing conditions: in their comparable experiments (Frensch et al., Exps. 1a and 1b), no such difference was evident. Secondly, our findings do not lend support to the suppression hypothesis. Recall that the suppression hypothesis states that dual-task testing conditions suppress the expression of sequence knowledge: group differences are flattened out by the secondary task. The evidence for this hypothesis comes from a number of experiments: for instance, Frensch et al. (Exps. 2a and b) trained participants on a repeating sequence under single- or dual-task conditions and then tested them under both single- and dual-task conditions. Transfer scores were generally lower on the dual- than on the single-task test, regardless of training conditions. In another study, Frensch et al. (1999) trained participants under dual-task conditions and tested them first under dual- and then single-task conditions, and again found that transfer scores were lower on the dual- than on the single-task test.

In contrast, sequence knowledge in Exp. 2 was not better expressed under single- than under dual-task testing conditions: there was no overall effect of testing conditions in the ANOVA described above. Indeed, in one specific comparison we find evidence of a "reverse" suppression effect, in that D scores were numerically greater in group Single/Dual than in group Dual/Single. This is contrary to the suppression hypothesis because, according to Frensch et al., the two groups should have learned the sequence equally but the former group should have suffered suppression in the test stage.

Why do our results conflict with those of Frensch and his colleagues? The experiments differ in many ways but we contend that the use of within-subjects designs in previous suppression experiments is a significant concern. If participants are first tested under (say) dual-task conditions and then under single-task conditions, the possibility arises of contamination of the later test by the earlier one. We have very little reason to discount the possibility of such contamination. In Exps. 1 and 2 this issue was circumvented by the use of between-subjects designs. The suppression hypothesis predicts larger D scores under single- than dual-task testing conditions. Yet the pattern of results was contrary to this. We contend that Frensch et al.'s conclusion – that tone counting has no effect on transfer scores provided that common testing conditions are used – is not in general correct. Our results therefore challenge the idea that implicit learning can be usefully distinguished from explicit learning on the basis of its attentional requirements, as Frensch (1998; Frensch et al., 1998; Frensch et al., 1999) and others (e.g., Cleeremans, 1997; Hayes & Broadbent, 1988; Heuer & Schmidtke, 1996; Jiménez & Méndez, 1999; Schmidtke & Heuer, 1997; Stadler, 1995) have suggested.

Equally, our results lend no support to the idea that implicit sequence learning is accurately characterized by a lack of awareness of the process and products of learning. Participants in Exp. 1 were able to freely generate the sequence they were trained on, implying that they did have conscious access to their knowledge of the sequence and that, if implicit learning is defined in terms of a lack of awareness, then knowledge acquired in the SRT task is not implicit (e.g., Perruchet & Amorim, 1992; Perruchet, Bigand, & Benoit-Gonin, 1997; Shanks & Johnstone, 1999; Shanks & Perruchet, in press). Note also that it cannot be argued that our conclusions about the role of the secondary task would have been different if we had excluded participants with some degree of explicit knowledge from the analyses. In Exp. 1 there was no evidence that the difference between groups Single/Repeating and Dual/Repeating in their mean D scores was attributable to "contamination" by participants with explicit knowledge of the sequence.

To this point our discussion has focused entirely on studies that have used tone counting as the secondary task. For completeness, we now turn to a brief consideration of other secondary tasks. As a number of researchers have noted (Frensch et al., 1998; Heuer & Schmidtke, 1996; Schmidtke & Heuer, 1997; Stadler, 1995), even if a secondary task does affect sequence learning, the locus of this need not be at the level of competition for attentional resources. The effects of a secondary task may be due, for example, to specific interference rather than competition for central capacity. There is now a sizable body of work attempting to isolate the exact mechanisms by which different secondary tasks might affect performance.

Stadler (1995) used a memory-load secondary task in the expectation that this would be a "purer" attention-demanding task than tone counting. Compared to a single-task group, participants who memorized a seven-letter string at the outset of each block of SRT trials showed a significantly reduced transfer effect when shifted to a random sequence, although the effect was much smaller than that caused by tone counting. Stadler (1995, Exp. 2) downplayed this finding because of a post-hoc reanalysis of the data according to whether participants were aware or not of the sequence, and concluded that implicit sequence learning is not attention demanding. In unaware participants, the difference in sequence learning between the memory-load group and the single-task control group was reduced. However, the difference was not eliminated and loss of statistical power makes the reduction hard to interpret. There remains clear evidence of an overall disruption of sequence learning as a result of the memory load. Interpretation is made problematic, though, because Stadler's experiments confounded learning with performance: the secondary task was present in both the training and transfer blocks. Furthermore, Reed and Johnson (1994) have documented a number of problems with the sequences Stadler used, and Willingham, Greenberg, and Thomas (1997) were unable to replicate some of his findings. Thus, it is difficult to draw firm conclusions from these studies.

Heuer and Schmidtke (1996) pointed out that the tone-counting task has two components, classifying each tone as high or low and memorizing the current number of tones. In contrast to the findings of Stadler (1995), they (Heuer & Schmidtke, 1996, Exp. 2) found that sequence learning was completely unaffected by two secondary tasks (the verbal and visuo-spatial tasks of Brooks, 1967), which impose a memory load without additional stimulus processing, whereas it was affected by a task (pressing a foot pedal in response to a high-pitched but not a low-pitched tone) requiring stimulus processing without a memory load. On the assumption that the Brooks secondary tasks were to some degree attention demanding, Heuer and Schmidtke's data represent very strong evidence that sequence learning in the SRT task does not require central attentional resources; so long as an appropriate secondary task is used (i.e., one that does not require stimulus processing in the response-stimulus interval of the main task), no interference of sequence learning will be observed.

However, some explanation is needed of the discrepant results obtained by Heuer and Schmidtke (1996), who found no effect of a memory-load secondary task, and Stadler (1995), who did. Moreover, Heuer and Schmidtke's studies can again be criticized on the grounds that they used training and transfer sequences which were not structurally identical and, hence, which do not control for factors such as the frequency of targets at each location or rate of reversals (see footnote 3). Thus, it would be premature to conclude on the basis of these memory-load studies that implicit sequence learning does not require attention.

Jiménez and Méndez (1999) used a probabilistic sequence learning task in which the target stimulus could be one of four symbols; as well as reacting to the location of each target, dual-task participants had to count the frequency of two of the symbols. This secondary task had no detectable effect on sequence learning. Jiménez and Méndez speculated that the use of a probabilistic sequence was critical in their study for revealing a form of learning which is independent of attention. The puzzle, however, is why Schvaneveldt and Gomez' (1998) study, in which a probabilistic sequence was again used, did reveal an effect of divided attention. We are currently studying the effects of divided attention with probabilistic sequences in our own laboratory in the hope of shedding some light on this paradox.

Finally, what do the present results tell us about the theoretical distinction between implicit and explicit knowledge? From a broader perspective, it would be very surprising if implicit learning turned out, in general, not to require attentional resources. Several studies using tasks other than SRT suggest that memory for unattended material is extremely poor and often non-existent even when indirect memory tests are used (for a review see Cowan, 1995, ch. 6). For instance, Carlson and Dulany (1985) found no learning of probabilistic letter-based categories when they presented the critical stimuli in the uncued (i.e., unattended) part of a brief visual display and measured learning via a subsequent indirect classification task; however, learning did occur if the critical stimuli were presented in the cued part of the display. If the indirect memory test used by Carlson and Dulany is taken to provide a measure of implicit learning, then the fairly clear conclusion – consistent with that reached in the present work – is that implicit learning does under some conditions make attentional demands. As far as explicit learning and memory are concerned, there is little doubt that the division of attention usually has a dramatic effect (e.g., Glucksberg & Cowen, 1970).

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