Implicit sequence learning with competing explicit cues

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Previous research has shown that the expression of implicit sequence learning is eliminated in a choice reaction time task when an explicit cue allows participants to accurately predict the next stimulus (Cleeremans, 1997), but that two contingencies predicting the same outcome can be learned and expressed simultaneously when both of them remain implicit (Jiménez & Méndez, 1999). Two experiments tested the hypothesis that it is the deliberate use of explicit knowledge that produces the inhibitory effects over the expression of implicit sequence learning. However, the results of these experiments do not support this hypothesis, rather showing that implicit learning is acquired and expressed regardless of the influence of explicit knowledge. These results are interpreted as reinforcing the thesis about the automatic nature of both the acquisition and the expression of implicit sequence learning. The contradictory results reported by Cleeremans are attributed to a floor effect derived from the use of a special type of explicit cue.

Defining implicit learning is a difficult task, mostly because it is a concept that revolves around multiple criteria. Frensch (1998), for example, quoted a dozen different definitions, and argued that most of them would “fall into one of the four possible categories that result from crossing the unconscious/unaware versus nonintentional/automatic distinction with the learning-only versus learning-plus-retrieval distinction” (p. 55). Frensch himself has favoured a notion of implicit learning relying exclusively on the nonintentional/automatic nature of the acquisition processes. However, although a definition exclusively based on the features of the acquisition processes can be taken as appropriate to define the phenomenon at hand, many authors continue to use this label to refer to processes of learning that both (1) run nonintentionally and without awareness at the time of acquisition, and (2) produce behavioural effects that depend on an unconscious and nonintentional retrieval of the relevant knowledge (see Cleeremans, Destrebecqz, & Boyer, 1998, for a recent review). We have argued elsewhere that both criteria may be operationally necessary if the effects of learning...
could be argued to modify the nature of the ongoing acquisition processes (Jiménez, 1997). Specifically, if the lack of intention to learn depends critically on the absence of an online conscious apprehension of the relevant knowledge, then any operational definition of implicit learning would require that the experimental preparations would successfully prevent not only the initial intention to learn, but also the acquisition of any conscious knowledge that could lead the learner to adopt an intentional stance over the course of learning. Implicit learning paradigms, therefore, should be assessed against both of these criteria.

**Probabilistic sequence learning**

The subparadigm of probabilistic sequence learning has been shown to provide a good preparation for analysing these implicit learning processes (Cleeremans, 1997; Cleeremans & Jiménez, 1998; Cleeremans & McClelland, 1991; Jiménez & Méndez, 1999; Jiménez, Méndez, & Cleeremans, 1996). In this subparadigm, participants are presented with a serial reaction-time (SRT) task in which they are instructed to respond as fast and as accurately as possible to the location of a stimulus that appears on each trial at one of several possible locations on a computer screen. Unbeknown to participants, the location of the stimulus on each trial follows a sequence that, instead of being structured as a fixed and repetitive series, as used to be the case in the standard, sequence-learning paradigm (e.g., Cohen, Ivry, & Keele, 1990; Nissen & Bullemer, 1987), is generated on the basis of the output of a noisy, finite-state grammar. The incidental instructions, together with the complex and probabilistic nature of the sequence, might ensure that participants do not search for sequential rules and that, even if they happen to note any regularity, they would not base their performance on such knowledge, given that any fixed rule would be falsified by the continuous appearance of exceptions. Furthermore, the probabilistic nature of those sequences also allows for an online assessment of sequence learning, thus blurring the differences between the acquisition and retrieval stages.

Research with this preparation has shown that probabilistic sequence learning is mostly unconscious, and that it is produced regardless of the participants’ intention to learn (Jiménez et al., 1996). Furthermore, learning in these conditions occurs even when participants have to perform a secondary task on a different dimension of the stimuli, and when this second dimension is arranged as a competing cue that predicts next location with greater accuracy than does the sequence of previous locations (Jiménez & Méndez, 1999). Taken together, all these results suggest that this kind of sequence learning is acquired and applied automatically, regardless of participants’ intentions and without a conscious apprehension of most of the relevant regularities that affect performance. Therefore, such learning can be taken as a paradigmatic case of implicit learning, satisfying the full set of requirements proposed by Frensch (1998).

**Interactions between explicit and implicit sequence learning**

An interesting issue arising from previous research within this paradigm has to do with the relationships observed between the acquisition and expression of simultaneous sources of explicit and implicit learning. This question is of obvious applied interest, because in most real settings skill learning can be taken as a blend between implicit and explicit learning. Moreover, this question is of theoretical interest too, because it has important bearings on the issue of
whether implicit learning can be seen as an autonomous process relying on resources independent from those used by explicit learning. Finally, if the interaction between implicit and explicit learning could be proved to be different from the interaction produced between two simultaneous sources of implicit learning, then it would also be important from a methodological point in view, in that it could provide researchers with an independent criterion to distinguish between implicit and explicit learning effects.

Cleeremans (1997) conducted a set of experiments with a dual-stimulus setting that are relevant to this point. In these experiments, participants had to respond to a six-location, SRT task, and the location at which the stimulus was presented on each trial was generated by following a noisy, finite-state grammar. In addition to the main stimulus (a dot), another cue (an X) was presented on each trial under one of the six stimulus locations, indicating the location at which the next stimulus was going to appear with different validities, which ranged from 20% to 80% for Experiment 1, and which were fixed at 100% for Experiment 2. The results of this latter experiment showed that participants did explicitly apply the information provided by this valid cue, and that the use of this cue eliminated the expression of sequence learning. However, sequence learning itself was not impaired even under these conditions, and could be revealed through a set of neutral trials, on which the cue was removed. Hence, Cleeremans concluded that explicit learning about a valid cue interfered with the expression, but not with the acquisition of implicit knowledge about a probabilistic sequential structure.

If explicit learning seems to interfere with the expression of implicit sequence learning, other results have shown that two processes of implicit sequence learning can proceed simultaneously without producing significant interference with each other, neither at the acquisition nor at the expression stages. Mayr (1996), for example, presented participants with a task in which they had to identify different objects that appeared on each trial at one of the four quadrants of a computer screen, and included two completely independent contingencies, related respectively with the sequence of objects to be identified and with the sequence of locations in which they appeared. He observed that both the sequence of objects and the sequence of locations could be acquired and expressed independently, without any competition arising between them. Jiménez and Méndez (1999), on the other hand, showed that this competition did not arise between two cues that predicted the same outcome either. In this case, participants had to respond to a four-location SRT task where the stimulus was one of four different shapes (*, x, ?, !). On each trial, the location of the stimulus followed a probabilistic sequence, but was also predicted with a validity of 80% by the identity of the last shape. In some conditions, participants had to pay attention to these shapes, because they were instructed to keep a running count of the number of target shapes (typically, * and x) presented during each block. In those dual-task conditions only, participants did implicitly learn about the specific relationship established between the categories to which each shape belonged (i.e., target vs. distractor shapes) and the locations that were more likely to follow them. Interestingly, participants who were not told to perform any task with the shapes did not learn about these contingencies, but both groups learned about the sequential structure of locations, and expressed this sequence learning to an equivalent extent.

These results can be taken as pointing to an important difference between the ways in which implicit and explicit knowledge is acquired and, more specifically, about the conditions under which each of them could affect performance. Thus, implicit learning would be accrued automatically with experience, with the only condition that learners should pay attention to
the predictor elements, and would produce its effects in an uniformly facilitative way. Explicit learning, on the other hand, would be acquired only when the cue is sufficiently salient, or when the learners have the explicit goal of breaking through a rule, and it would express itself selectively through a deliberate decision that, in turn, would require an active inhibition of any alternative source of priming. This general hypothesis resembles Posner and Snyder’s (1975) classic distinction between controlled and automatic effects, as expressed for instance in their postulate of two different components of attention: A fast automatic, inhibitionless process, and a slow, limited-capacity, and conscious mechanism. A number of classic studies have shown that automatic, unconscious effects tend to be uniformly facilitative, whereas effects derived from conscious expectancies can produce both facilitation and inhibition (e.g., Cheesman & Merkle, 1986; Marcel, 1980; Neely, 1977). The present experiments aim to test the somewhat related hypothesis that only explicit learning effects could produce inhibitory effects over the expression of a simultaneous source of implicit sequence learning, whereas learning implicitly about a set of predictive contingencies would not affect either the acquisition or the expression of any other source of implicit learning.

THE EXPERIMENTS

To investigate that issue, we arranged an SRT task similar to that used by Jiménez and Méndez (1999), generating the sequence of locations on the basis of the same noisy, finite-state grammar, and using as stimuli the same set of four different shapes (*, x, ?, !), which could also be used by the learners as alternative cues to predict the next location. In the explicit conditions, we changed the instructions so as to assure that participants did explicitly learn about the predictive relationship arranged between shapes and locations. In addition, to maintain participants’ reliance on these cues, we also increased their validity up to 100% during the first training sessions, so that the presentation of a given shape predicted the next location with complete accuracy. In Experiment 1, we used a dual-task preparation, in order to make sure that all participants would pay attention to the shapes and, therefore, that all of them would learn about the relationship arranged between shapes and locations, either explicitly or implicitly. In Experiment 2 we arranged two similar conditions of explicit vs. implicit instructions under a single-task preparation, so as to answer a number of questions that had been left open by the results obtained in Experiment 1.

EXPERIMENT 1

The main goal of Experiment 1 was to assess the differential effect that learning, either implicitly or explicitly, about the set of contingencies established between shapes and locations (what we will call, for short, “shape” learning) would bear on the expression of learning about the sequential constraints imposed by a noisy, finite-state grammar (what we will call “grammar” learning). Previous experiments with the same stimulus arrangement had shown that only participants who paid attention to the shapes did learn about their predictive value, and so all participants were required to perform a counting task that required them to pay attention to these shapes. In those previous experiments too, grammar learning began to be significant around the third training session, and so we arranged a first training phase composed of four sessions in which the shapes predicted next location with total accuracy. However, we also
needed to assess whether participants were really learning about the shapes and using them to anticipate next locations, and so we introduced a test phase at the fifth session, where the validity of these cues was reduced to 80%, thus allowing a comparison between responses given to locations that were accurately signalled by the previous shape and those given to locations that were not accurately signalled in this way. Finally, if there was any difference in grammar learning between conditions of explicit and implicit shape learning, we needed to ascertain whether such a difference was attributable to either an acquisition or an expression deficit. To address this issue, we arranged a second training phase composed of two more sessions (sixth and seventh) in which the shape validity was again raised to 100%, and then a second test phase at the final, eighth session, in which the shapes were removed and replaced by a different symbol (0) to eliminate any possible interference produced by the shapes on the expression of grammar learning.

The grammatical structure was maintained unchanged throughout the experiment, but their probabilistic nature allowed an on-line assessment of grammar learning, by comparing responses given to legal and illegal transitions. Tests of explicit shape learning were also introduced after each session for the explicit condition, and only at the end of the experiment for the implicit condition. To assess the explicit knowledge about the grammar, all participants were presented with three additional blocks of a cued generation task at the end of the eighth session, using the symbol “0” to avoid any interference between shape and grammar learning. During these three blocks, participants were told to respond to each trial by pressing the key corresponding to the location where they guessed the next stimulus was going to appear, according to any regularity that they could have noticed throughout the training period.

**Method**

**Participants**

Sixteen students from the University of Santiago participated in the experiment. Eight were randomly assigned to either the explicit condition (henceforth, Condition E) or the implicit condition (henceforth, Condition I). Participants were paid about $14 for participating in the study, and earned an additional $23 to $45 depending on performance (see later).

**Apparatus and display**

The experiment was run on IBM-compatible computers. The display consisted of four dots arranged in a horizontal line on the computer screen and separated by intervals of 3 cm. Each screen position corresponded to a key on the computer keyboard. The spatial configuration of the keys was entirely compatible with the screen positions (i.e., the key farthest to the left corresponded to the screen position farthest to the left, etc.). The stimulus was a small white shape 0.35 cm high that appeared on a black screen and was centered 1 cm above one of the four dots. The shape could be one of the following four signs: x, *, ?, or !. The timer was started at the onset of the stimulus and was stopped by the participant’s response. The participant’s response also produced the removal of the stimulus. The response-to-stimulus interval (RSI) was 240 ms.

**Tasks**

The experiment consisted of an SRT task followed by a cued generation task. The SRT task was carried out during eight sessions, each composed of 20 blocks of 155 trials. During this task, participants
were required to respond as fast and as accurately as possible on each trial, by pressing the key corresponding to the current location of the stimulus. In addition to this task, all participants were also told to perform a counting task during Sessions 1 to 7, in which they should keep a running count of the number of “target shapes” that had appeared so far and to report on that number after each block of trials. During the eighth session, the shapes were replaced by the symbol “0”, and hence the counting task was discontinued. The cued generation task was presented immediately after the eighth session and consisted of three blocks of 155 trials each. During this task, both the specific sequence and the environment (i.e., the RSI, the characteristics of the screen, and the keyboard layout) remained unchanged with respect to the eighth session, but participants were told to predict the next location of the stimulus instead of reacting to the location of the current one. The only other difference between the generation and the SRT tasks was that during generation there was no time pressure whatsoever, so as to enable participants to maximize the use of any conscious knowledge that they might consider relevant to that prediction task.

Procedure

Most participants performed the task at a rate of two sessions per day, although there were exceptional cases in which they were allowed to perform up to three sessions in a single day, provided that these were separated by a lapse of at least 2 hours. The participants were told that the goal of the experiment was to analyse the effects of extended practice on performance in relatively simple tasks. They were also informed about the structure of the experiment and about how to place their fingers on the keyboard. Both accuracy and speed were emphasized as means of increasing earnings. Participants were also instructed to simultaneously keep a count of the number of trials in which either “x” or “*” occurred. The experimenter emphasized that it was not necessary to separate the occurrences of “x” from those of “*”, but only to keep a count of how many times any one of them had been presented so far in a given block. Participants were urged to be accurate in performing this counting task to maintain acceptable earnings. No explicit priority was established between these two tasks, but it was emphasized that a counting error of more than five occurrences would cancel out the incentives for that block. One participant assigned to the explicit condition was unable to cope simultaneously with the SRT and the counting tasks and was replaced. The earnings for each session were determined through a formula that weighted the three factors of reaction time, percentage of hits, and counting errors1.

Participants in Condition E were told about the existence of a predictive relationship between each shape and a different location, and about the usefulness of trying to discover these relationships and to use them as a way to increase both the speed and accuracy of their responses. Their knowledge of these contingencies was tested after each session through a questionnaire that required them to report on these relationships graphically, by drawing arrows that related each shape with the expected location for the next trial. On the contrary, participants in Condition I were not told about the existence of these relationships, and their knowledge about them was only tested at the end of the experiment, by means of a similar questionnaire composed of three items. The first item asked participants about whether they had noticed any relationship between the shape of each stimulus and the location at which the next stimulus appeared. If the answer was affirmative, then they were required to proceed to a second question in which they were asked to report on these relationships in the same graphic format as that used for participants in Condition E. Finally, a third item required them to express their subjective confidence in each of

1The formula computed earnings in pesetas as follows: \( E = (\text{ACC} – 84) \times (880 – \text{RT}) \times \left(\frac{(15 – \text{Error})}{10}\right) \times 0.078 \), where E represents earnings, ACC represents percentage of correct responses to the SRT task for that session, RT stands for the average reaction time, and Error represents the counting error per block averaged over the 20 blocks of the session. For Session 8, where the counting task was not required, this latter factor was removed.
the four statements regarding the relationships between shapes and locations, by marking points in four confidence scales graduated between 1 (no confidence) and 7 (very sure).

**Stimulus generation**

Stimuli were generated on the basis of the same finite-state grammar as that used by Jiménez and Méndez (1999), with a small proportion of random stimuli (20%) interspersed with structured stimuli. The grammar is re-entrant, which means that the first and the last nodes are the same, so as to allow the generation of an indefinite number of grammatical labels. As shown in Figure 1, the grammar was designed to contain each label (A, B, C, or D) in two different arcs, pointing to different successors in each case, so that considering one previous label only indicated that any other label was about equally likely after the current one. An analysis of the conditional probabilities of each transition showed that repetition of a label was only allowed with a probability of .11, because of random substitution, and that all the non-repeating transitions had similar conditional probabilities, ranging between .29 and .31 (see Jiménez & Méndez, 1999). Considering two consecutive elements, however, allowed one to discriminate between legal and illegal transitions. For instance, after the label C, which can point to Nodes 3 and 0, all the labels A, B, and D are legal. However, the series AC unambiguously points to Node 3 and thus predicts the next label to be D, whereas the series DC, which points to Node 0, correspondingly predicts the next label to be either A or B. Given that it is always necessary to consider two previous events to discriminate on the state of the system (i.e., on its current node), a sequence generated on the basis of this grammar could be considered as a probabilistic version of an “ambiguous” sequence, to borrow Cohen et al.’s (1990) terminology.

The sequence of locations was generated by following four steps. First, a sequence of 24,000 grammatical labels was generated on the basis of the grammar, by selecting an arc coming out of the current node and recording the corresponding label on each trial. The current node was set to be Node 0 on the first trial of each block and was updated on each trial to be the node pointed to by the selected arc. Second, 20% of these grammatical labels were replaced at chance by a different label. Third, a set of five completely random labels were added at the beginning of each block of 150 trials, to serve as a buffer of unrecorded trials. Fourth, the resulting set of 24,800 labels was used to determine the screen position at which each stimulus would appear by following a counterbalancing arrangement, so that each label corresponded to each screen position for exactly two of the eight participants in each condition.

As for the generation of the shapes, it simply proceeded from the total sequence of 24,800 labels, with the constraint that each shape at trial $t$ should predict a specific label (i.e., location) at trial $t + 1$ with perfect accuracy for Sessions 1 to 4 and 6 to 7, and should predict the same label with a validity of 80% during Session 5. For Session 8, only the symbol “0” was presented. The mapping between shapes and labels was selected arbitrarily, and was as follows: * predicted A; x predicted B; ? predicted C, and ! predicted D. The use of this set of arbitrary contingencies did not mean that each shape predicted the same screen location for all participants, given that the mapping between labels and locations was counterbalanced among them. However, these predictive constraints indirectly determined the number of target shapes that appeared at each block. This number still varied from 63 to 91, with a mean of 78 and a standard deviation of 5.2.

![Figure 1](image-url)  
**Figure 1.** Finite-state grammar used to generate the stimulus material.
Results

Before presenting the most relevant results of this experiment, concerned with the differences obtained in grammar learning between conditions of explicit and implicit shape learning, we report on some preliminary data regarding (1) participants’ performance of the counting task; (2) the explicit shape learning as assessed through self-reports; (3) the expression of this shape learning during the SRT task, as it could be assessed during the fifth session; and (4) the explicit grammar learning as inferred from participants’ performance on the generation task.

**Counting task**

The counting task was performed with great accuracy, yielding an average deviation from the correct number of targets of less than 1 (0.96 and 0.64 respectively, for Conditions E and I). An analysis of variance (ANOVA) conducted with condition (E vs. I) and session (1 to 7) as independent variables, using these deviations averaged over sessions as the dependent variable, showed that deviations decreased significantly with practice, from 1.53 in Session 1, to 0.38 in Session 7, $F(1, 6) = 8.89, MSE = 3.01, p < .0001$. The effect of condition was not significant, $F(1, 14) = 1.5; MSE = 2.813$, and the interaction of Condition × Session showed an interesting pattern, which, however, did not reach statistical significance, $F(1, 6) = 2.19, MSE = 0.74, p = .051$, but $p > .09$ for any epsilon-corrected test. This pattern showed that deviations from the correct number of targets were slightly higher during the first session for Condition I, but that these values decreased more for this condition during the second session, yielding an asymptotic level around a value of 0.3 from the third session on. Participants in Condition E, on the contrary, made slightly fewer errors during the first session, but their decrease was slower over the following sessions and never reached the high levels of accuracy obtained by participants in Condition I.

**Explicit shape learning**

We expected that the explicit instructions provided to participants in Condition E, together with the perfect reliability of the shapes as predictors of the next location, would guarantee a perfect apprehension of this set of contingencies from the very beginning of the experiment. However, the explicit reports obtained after each session showed that this explicit knowledge was less than perfect in a number of participants. Only three out of the eight participants in Condition E reported the full set of contingencies accurately after the first session. Two more participants began to produce completely accurate reports after the second session, one did it after the third session, and still one more started to report the accurate relationships just after the fifth session. The remaining participant systematically mistook the locations predicted by each one of the target and distractor shapes, attributing to each member of these categories the location predicted by the other member of the same category. However, although these results indicated that it was relatively difficult for participants to try to break through the rules that related each shape with the following location while they were simultaneously engaged in the counting task, they also indicated that, over the fifth session, most participants did explicitly know about the correct predictive relationships.

As for the participants presented with Condition I, their explicit knowledge of these contingencies could only be tested at the end of the experiment. Five out of the eight participants
presented with this condition reported that they did not believe that there was any relationship between the shape of each stimulus and the location at which at next one appeared. The remaining three participants answered this question affirmatively, and hence reported on the set of contingencies that they believed had been arranged between shapes and locations. One of them reported accurately the locations that followed each of the two target shapes, ‘“@”’ and “x”, and rated these reports respectively with confidence scores of 4 and 5, but she mistook the location that followed each of the two distractor shapes. Another participant only reported accurately on the location that followed the shape ‘“@”’, with a confidence rate of 6. Finally, the remaining participant did not report accurately on any of these relationships, but showed the same pattern of shifted prediction observed in one participant from Condition E, where the location predicted by each of the target and distractor shapes was wrongly attributed to the other member of the same category.

In sum, if these results did not allow us to claim that all participants in Condition E did explicitly learn about the full set of contingencies from the very beginning of the experiment, or that none of the participants in Condition I did learn anything about this set of contingencies, we can at least assume that these two groups differed considerably in their explicit shape learning as shown, for instance, by the fact that none of the participants in Condition I did accurately report on the full set of contingencies even at the end of training, whereas by the fifth session all but one of those presented with Condition E did know perfectly about them. Therefore, we concluded that the experimental manipulation had been efficacious in promoting a different orientation toward shape learning, and in producing a different amount of explicit knowledge in each condition.

**Shape learning as expressed on the SRT task**

If the previous analysis showed a clear difference between Conditions E and I with respect to the shape learning expressed through self-reports, it could be expected that this difference would show up in performance during the fifth session of the SRT task, where about 20% of the locations were not accurately cued by the previous shape. For Condition E, responding to the unlikely “nonsignalled” trials should be slower and less accurate than responding to the most common “signalled” trials. As for participants in Condition I, previous studies (Jiménez & Méndez, 1999) had shown that participants presented with dual-task conditions could also learn implicitly about these predictive relationships, but that they did it in a different way. Specifically, that study showed that participants learned a predictive relationship between the categories to which each shape belonged (i.e., target vs. distractor shapes) and the locations that were more likely to follow each of them. To avoid these two sources of learning being confused, we made two different comparisons: first, between signalled and nonsignalled trials and, second, between what we called “coherent” and “noncoherent” trials. Signalled trials were those in which the location was accurately predicted by the previous shape (i.e., Location A after shape “@”, Location B after shape “x”, Location C after shape “*”, and Location D after shape “!”). Nonsignalled trials were those trials that were not signalled in this way (e.g., Location B, C, or D after shape “@”, and so on). Coherent trials were those nonsignalled trials in which the location that would have been predicted by a given shape actually appeared following the other shape from the same category (e.g., Location B after shape “*”). Noncoherent trials, finally, were those nonsignalled trials in which a location that would have
been predicted by a target shape actually appeared following distractor shape, and vice versa (e.g., Location C or D after shape “*”). A difference observed between responses to signalled and to nonsignalled trials could be accounted for by both a genuine signal learning and by some form of category learning. For example, if participants learned that counting responses tended to be followed by either Location A or B, then it could be expected that they would respond more efficiently to signalled trials (e.g., when A appeared after “*” than to the average of the nonsignalled trials (when B, C, or D appeared after “*”). However, a difference between responses to coherent and to noncoherent trials could only be accounted for by category learning, given that both the coherent and the noncoherent trials were equally nonsignalled. Hence, this latter difference would indicate the acquisition of a form of learning that was incidental, in that it was not concerned with the type of contingency that participants in Condition E had been instructed to search, but related an obligatory response (the counting vs. non-counting decisions) with the locations that were more likely to follow each decision. To analyse what specific kind of learning was expressed through performance in each condition, we computed separately the average reaction times (RTs) and the error rates for each of these four types of trial for each condition. The average results are shown in Figure 2.

As can be readily observed, there were important differences between responses to each of these types of trial. Responses to signalled and coherent trials were faster and more accurate than responses to nonsignalled and noncoherent trials, and this pattern of results was very similar for both Conditions E and I, using either RTs or error rates as the dependent variable. A set of ANOVAs conducted on each of these two dependent variables showed that RTs to signalled trials were faster than RTs for nonsignalled trials, $F(1, 14) = 296.14, MSE = 37,196.28, p < .0001$, that RTs to coherent trials were also faster than RTs for noncoherent trials, $F(1, 14) = 142.68, MSE = 77,018.31, p < .0001$, that error rates were smaller in response to signalled trials than in response to nonsignalled trials, $F(1, 14) = 32.95, MSE = 523.26, p < .0001$, and that these error rates were also smaller for coherent trials than for noncoherent trials, $F(1, 14) = 30.87, MSE = 953.75, p < .0001$. In none of these four analyses were there either significant effects of condition or significant interactions of Condition $\times$ Type of Trial (all $Fs < 1$).

![Figure 2](image-url)  
**Figure 2.** Reaction time (left panel) and error rates (right panel) for signalled (s), nonsignalled (ns), coherent (c), and noncoherent (nc) trials obtained during the fifth session for conditions of explicit (E) and implicit (I) shape learning. The bars represent the standard error of the means.
Thus, despite the differences between conditions in explicit shape learning, it seemed that such differences did not show up during the SRT task and that, on the contrary, their patterns of results were very similar and expressed exclusively the effect of some (arguably implicit) category learning. Indeed, responses to signalled trials were only slightly more efficient (i.e., about 5 ms and 0.7 percentage points) than responses to coherent trials, whereas responses to coherent trials were about 100 ms faster and 11 percentage points more accurate than responses to noncoherent trials. It seems, therefore, that the explicit shape knowledge was barely useful under the conditions arranged in this experiment, presumably because the task of deciding about whether or not to count any given shape interfered with any deliberated attempt to use that shape as an explicit cue to anticipate next location. At the same time, however, the fact that this counting task was obligatory in both conditions, and required participants to make either a counting or a non-counting decision on each trial, allowed the acquisition and expression of a form of category learning that related these decisions with the locations that were more likely to follow each of them.

**Explicit grammar learning**

To assess whether there was any evidence of grammar learning expressed through direct generation task, we first conducted an ANOVA on the percentages of correct guesses produced on each of the three successive generation blocks for each condition. The average percentages of correct generation trials over these three blocks were 28.7 and 28.9, respectively, for participants in Conditions E and I, which means that generation decisions were only slightly better than those expected by chance (chance level, 25%), but not better than what could be expected if participants would have taken into account the obvious fact that repetitions were relatively unlikely (chance level for participants who would avoid generating repetitions, around 29%). The difference between conditions was not significant in this analysis, \( F(1, 14) = 0.036, \text{MSE} = 0.725 \). Neither was the effect of block significant, although it showed a trend to produce better generation levels during the second block (30.6%) than those obtained during either the first or the third blocks (27.9% in both of them), \( F(2, 28) = 3.12, \text{MSE} = 38.79, p = .06 \). Finally, the interaction Block × Condition did not approach significance either, \( F(2, 28) = 0.73, \text{MSE} = 9.08 \).

These results are compatible with the claim that participants lacked any explicit knowledge about the sequential constraints imposed by the grammar. However, as we have pointed out elsewhere (Jiménez et al., 1996), the measures of generation accuracy may tend to underestimate learning when applied to a probabilistic structure in that even perfect knowledge of the rule system could result in less than perfect prediction performance, precisely because several successors are possible after each context, and therefore participants may generate a grammatical successor that happens to be different from the specific element that would appear next. Hence, we needed a measure of grammar learning that would allow us to assess whether the average generation probability of any event differed, depending on whether it was or was not consistent with the context set by previous events, and regardless of whether or not the generation response turned out to be a correct guess of the next location. In other words, we should assess whether participants generated each location more often when it was a grammatical successor of the previous locations than when it was nongrammatical.
To undertake this analysis we defined two comparable sets of grammatical and nongrammatical sequences. Table 1 shows these two sets of sequences, together with their associated conditional probabilities (i.e., the probability of appearance of the last event of each sequence, in the context provided by its first two events). We selected these 16 sequences by following the same criteria as those discussed in Jiménez and Méndez (1999). Briefly, for each possible grammatical sequence considered (e.g., BA–D) there should be another nongrammatical sequence that could be matched to the grammatical one for all but its first element (e.g., CA–D), so that the grammaticality of its last element would depend critically on the identity of the first one. In addition, and to avoid undesirable, short-term priming effects, immediate and alternating repetitions (e.g., BA–A or AB–A) were not considered. With this arrangement, we analysed whether these grammatical successors were generated more often than their nongrammatical counterparts, by computing the conditional probability with which participants generated the last element of each sequence in the context provided by its first two elements, and then averaged these probabilities separately over the full set of either grammatical or nongrammatical sequences for each participant, thus obtaining two indices of the probability with which he or she generated grammatical and nongrammatical responses. An ANOVA conducted on these scores with condition and grammaticality as independent variables showed that all these successors were generated equally often in Conditions E and I, $F(1, 14) = 1.73, MSE = 0.01$, that grammatical successors were not generated more often than nongrammatical successors (.32 vs. .30), $F = (1, 14) = 1.9, MSE = .005$, and that this lack of discrimination between grammatical and nongrammatical cases was equally attributable to both conditions, $F(1, 14) = 0.043, MSE = 0.0001$. Thus, we concluded that the measures of learning obtained from the generation task did not sustain the claim that participants had acquired any grammar learning that could be used by them to directly discriminate between predictable and unpredictable locations.

### Table 1
List of the relevant grammatical and nongrammatical sequences and their associated conditional probabilities

<table>
<thead>
<tr>
<th>Sequences</th>
<th>Grammatical</th>
<th>Non-grammatical</th>
</tr>
</thead>
<tbody>
<tr>
<td>CP(BA–D)</td>
<td>.620</td>
<td>CP(CA–D) = .133</td>
</tr>
<tr>
<td>CP(DA–C)</td>
<td>.347</td>
<td>CP(BA–C) = .157</td>
</tr>
<tr>
<td>CP(AB–C)</td>
<td>.625</td>
<td>CP(DB–C) = .147</td>
</tr>
<tr>
<td>CP(CB–D)</td>
<td>.362</td>
<td>CP(AB–D) = .147</td>
</tr>
<tr>
<td>CP(AC–D)</td>
<td>.605</td>
<td>CP(BC–D) = .166</td>
</tr>
<tr>
<td>CP(DC–B)</td>
<td>.364</td>
<td>CP(AC–B) = .128</td>
</tr>
<tr>
<td>CP(CD–A)</td>
<td>.390</td>
<td>CP(BD–A) = .148</td>
</tr>
<tr>
<td>CP(BD–C)</td>
<td>.618</td>
<td>CP(AD–C) = .158</td>
</tr>
</tbody>
</table>

*Note:* CP = conditional probability. CP(BA–D) means conditional probability of appearance of the location D after the sequence BA, averaged over all training sessions.
Implicit grammar learning

As we have shown through the previous subsections, Conditions E and I did not differ either in their explicit grammar learning or in the shape knowledge that they expressed through the measures of performance during the fifth session of the SRT task, but they differed considerably with respect to their explicit shape learning, as expressed through self-reports. From these preliminary results, it was difficult to anticipate whether or not the grammar learning expressed through the SRT task should differ between them. On the one hand, if participants in Condition E had far more explicit knowledge about the relationships arranged between shapes and locations than did participants in Condition I, then this knowledge could be expected to selectively interfere with the expression of grammar learning in the former condition. On the other hand, however, if it was the deliberate use of this explicit knowledge, rather than the mere fact of having acquired such knowledge, which could be taken to interfere with the expression of any other knowledge, then the previous analyses, by indicating that participants in Condition E made little use of shape learning, would incline one to expect similar grammar learning effects arising in both conditions.

To assess whether participants in these conditions learned about the sequential structure contained in the stimulus material, and whether they expressed this grammar learning to an equivalent extent through their performance of the SRT task, we compared their responses to trials that corresponded to the last element of sequences that appear in the selected list of either grammatical or nongrammatical sequences (see Table 1). Indeed, if participants became progressively sensitive over training to the constraints expressed by the grammar, one would expect to observe a progressive facilitation for responding to grammatical stimuli compared with their responding to nongrammatical stimuli. Such an effect could be observed as an increase in either speed or accuracy, and so we used both RTs and hits percentage as dependent variables. Figure 3 shows these results separately for each dependent variable, for each condition, session, and type of trial.

As can be seen from the figure, participants in both conditions did progressively learn about these grammatical constraints, responding faster and more accurately to grammatical trials as training proceeded. Of special interest was to analyse (1) whether the grammar learning effects differed between conditions through the first four training sessions, where the relationships between shapes and locations were completely reliable, and (2) whether any possible deficit in the expression of grammar learning selectively observed for Condition E would disappear at the final, eighth session, when the shapes were replaced by a neutral symbol.

As for the first of these issues, two ANOVAs were conducted respectively on RTs and hits percentages, with condition (E vs. I), practice (Sessions 1 to 4), and type of trials (grammatical vs. nongrammatical) as independent variables. For RT, this analysis showed that the effect of practice, \( F(3, 42) = 84.57, MSE = 204,140.79, p < .0001 \), and type of trial, \( F(1, 14) = 85.59, MSE = 25,458.96, p < .0001 \), were significant, but not the effect of condition \( (F < 1) \). The interaction of Practice \( \times \) Type of Trial, which could be taken as the main index of learning, was significant too, \( F(3, 42) = 11.08, MSE = 965.70, p < .0001 \), but no other interaction approached significance. A similar pattern of results was observed by using hits percentage as the dependent variable. In this case, neither the effect of condition nor that of practice approached significance, but the effect of type of trial was significant, \( F(1, 14) = 9.45, MSE = \)
24.85, \( p < .01 \), and so was the interaction of Practice \( \times \) Type of Trial, \( F(3, 42) = 6.65, MSE = 6.26, p < .01 \). No other interaction approached significance.

These results clearly showed that grammar learning occurred in both conditions, and that it was expressed to an equivalent extent, at least up to the fourth session. Moreover, if no effect of interference was produced in Condition E by the presence of competing, explicit cues, then no release from such interference could be expected to occur at the eighth session, after the removal of these cues. Accordingly, the ANOVAs conducted respectively on RTs and hits percentage to compare the effects of grammar learning as expressed during the seventh and the eighth sessions did not produce any significant, third-order, interaction of Session \( \times \) Type of Trial \( \times \) Condition. The effects of session were significant for both RTs, \( F(1, 14) = 46.32, MSE = 33,658.49, p < .0001 \), and hits percentages, \( F(1, 14) = 10.17, MSE = 79.17, p < .01 \) indicating that responses were slower and less accurate for the eighth session, when the cues were removed. The effects of type of trial were also significant in both analyses, \( F(1, 14) = 138.51, MSE = 20,674.84, p < .0001 \), for RTs, and \( F(1, 14) = 30.13, MSE = 221.34, p < .0001 \), for hits percentage, showing that the differences between responses to grammatical and nongrammatical trials persisted during these two sessions. No other effect or interaction approached significance in any of these analyses.
Discussion

The results of Experiment 1 bring forward further evidence on the robustness, and hence arguably on the automatic nature, of the processes of implicit sequence learning. They show again that this sequence learning can be acquired and indirectly expressed in the course of an SRT task, even though participants perform it under dual-task conditions, and even when this knowledge cannot be used by them to directly anticipate the next location in the course of a similar, cued generation task. Furthermore, these results also suggest that, at least under the dual-task conditions arranged in this experiment, the expression of implicit sequence learning is not hindered by participants having explicit knowledge about a competing cue that predicts the next location with complete accuracy. Indeed, participants in Condition E, who were explicitly told about the existence of these relationships between shapes and locations, and who had considerable knowledge about these contingencies as judged by self-reports, did not use this knowledge better than participants in Condition I. The analysis conducted on the fifth session of the SRT task indicated that participants' performance did not reflect shape learning, but a rather different sort of learning that we have called “category learning”, and that related the decision made for each trial on the counting task with the locations that were more likely to appear after each decision. Crucially, these contingencies were not those that participants in Condition E were instructed to search for, nor did they generally fit with those expressed through participants' self-reports, so that we qualified this category learning as implicit.\(^2\) In any case, the fact that the conditions of dual task prevented participants in Condition E from relying on such explicit shape learning made it impossible for us to assess whether the use of explicit cues would have hindered the expression of implicit sequence learning. Hence, to ascertain that issue, we conducted a second experiment in which a similar arrangement was presented under single-task conditions.

EXPERIMENT 2

Experiment 2 closely mirrored Experiment 1, with the only change being that the counting task was removed. This removal was aimed not so much to guarantee that participants in Condition E would explicitly learn about the relationship between shapes and locations, but to allow them to use this knowledge deliberately to anticipate the following location. In addition, this change could be expected to eliminate the category learning that was observed in Experiment 1, given that no shape categorization was required.

Method

Participants

Sixteen students from the University of Santiago participated in the experiment. Eight of them were assigned to either Condition E or I. They were paid about $16 for participating, and earned between $20 and $35, depending on performance.

\(^2\) However, the fact that there were a number of self-reports that contained the specific error of attributing to each shape the location that was predicted by the other member from the same category might suggest that, perhaps, this category learning could have been more explicit than we first thought.
Procedure

The apparatus and display were exactly the same as those used for Experiment 1. The SRT and generation tasks were also analogous, with the only exception being that participants in Experiment 2 were not told to perform any counting task, and hence they should not categorize shapes as either targets or distractors. The earnings for each session were determined through a formula that was identical to that used in Experiment 1, except by the obvious fact that the counting error factor was not included. Participants in Condition E were told about the existence of a predictive relationship between each shape and the following location, and were also urged to use these shapes as cues to increase both the speed and accuracy of their responses. Their explicit shape learning was also tested after each session by means of a graphic self-report. As for participants in Condition I, they were not told about the existence of this relationship, and hence were not expected to learn anything about it, according to previous experiments conducted with similar procedures (Jiménez & Méndez, 1999). Therefore, their shape learning was only assessed through the measures of performance, during the fifth session of the SRT task.

Results

We report in turn the results obtained with regard to: (1) the explicit shape knowledge as assessed through self-reports exclusively for those participants assigned to Condition E; (2) the effects of shape learning as assessed through the measures of performance during the fifth session of the SRT task; (3) the explicit knowledge of the grammatical constraints, as manifested through the generation task; and (4) the effects of grammar learning expressed in each condition through the measures of performance.

Explicit shape learning

All participants in Condition E reported the complete set of contingencies early in training. Seven of them produced a fully correct report just after the first session. The remaining participant confused the successors of the shapes “*” and “!” on her first report, but she corrected this error after the second session, and produced a completely accurate report from then on.

Shape learning as expressed through the SRT task

Figure 4 shows the average RTs and error rates produced by participants assigned to either condition in response to different types of trial during the fifth session of the SRT task. The comparison between coherent and noncoherent trials only made sense in Experiment 1, where the counting task provided a criterion to form two shape categories (targets vs. distractors). However, this comparison was maintained here just for control purposes. As can be readily observed from the figure, participants in this experiment did not discriminate between coherent and noncoherent trials, and so neither the effect of type of trial (coherent vs. noncoherent) nor the interaction of Condition × Type of Trial approached significance in the analyses conducted with RTs and error rates as dependent variables (all Fs < 1). The effect of condition was, nevertheless, significant in these analyses, F(1, 14) = 14.05, MSE = 47,178.24, p < .01 for RTs, and F(1, 14) = 5.72, MSE = 143.65, p < .05 for error rates, thus showing that participants in Condition E responded less efficiently to both coherent and non-coherent trials than did participants in Condition I. This was precisely the result to be expected if only those participants assigned to Condition E had learned about the relationships between shapes and
locations, and if such shape learning hindered their responses to any location different from the signalled one.

As for the comparison between signalled and nonsignalled trials, the results also indicated that explicit shape learning was selectively expressed by participants assigned to Condition E. Hence, the corresponding ANOVAs conducted on RTs and error rates, with condition and type of trial (signalled vs. nonsignalled), uniformly showed significant effects of type of trial, $F(1, 14) = 18.95, \text{MSE} = 11,325.12, p < .001$, for RTs, and $F(1, 14) = 13.55, \text{MSE} = 81.60, p < .01$, for error rates, and significant interactions of Condition $\times$ Type of Trial, $F(1, 14) = 16.15, \text{MSE} = 9553.56, p < .01$, for RTs, and $F(1, 14) = 11.85, \text{MSE} = 71.103, p < .01$, for error rates. The effect of condition was also significant for RTs, $F(1, 14) = 6.40, \text{MSE} = 13,836.16, p < .05$, but not for error rates. As a whole, this pattern of results did strongly suggest that: (1) participants in Condition I did not learn anything about the shapes; (2) participants in Condition E did learn about these cues, and expressed this learning as a difference between their responses to signalled and nonsignalled trials, both in terms of RTs and of error rates; and (3) such difference was manifested mainly as an effect of interference observed in response to nonsignalled trials. The magnitude of this effect was of about 75 ms and over 6 points in the error rate.

**Explicit grammar learning**

The average percentages of hits during the generation task were 28.2 and 25.9, respectively, for participants in Conditions E and I. Again, these scores fall between those expected by pure chance (25%) and those expected for participants who would apply the rule of avoiding repetitions (29%). The ANOVA conducted on these percentages with conditions and blocks as independent variables did not produce any significant effect or interaction.

As for the analysis of whether participants generated each location more often when it was a grammatical rather than a nongrammatical successor of the previous trials, we considered the same two sets of grammatical and nongrammatical sequences listed in Table 1, and followed the same strategy already described for Experiment 1 to compute the average probabilities.
with which each participant generated either grammatical or nongrammatical locations over the whole generation task. The ANOVA conducted on these scores with condition and grammaticality showed no indication that participants would generate grammatical successors more often than nongrammatical successors (.31 vs. .29), $F(1, 14) = 1.02, MSE = 24.5$, or that participants in each condition differed, either in general, $F(1, 14) = 1.02, MSE = 155.76$, or in the way in which they generated each type of successor, $F(1, 14) = 0.14, MSE = 4.35$. Therefore, just as in Experiment 1, we concluded that participants did not use grammar learning to directly discriminate between predictable and unpredictable locations during the cued generation task.

**Implicit grammar learning**

As shown by the previous analyses, participants in either condition from this experiment did not differ in the way in which they used grammar learning to respond to the direct generation task, but they did so in the way in which they used the information provided by the shapes to anticipate the next location in the course of the SRT task. Indeed, as revealed by the measures of performance obtained through the fifth session, only participants in Condition E considered these shapes, whereas responding to signalled and nonsignalled trials was not different for participants in Condition I. Therefore, if the use of any explicit knowledge could be expected to interfere with the expression of implicit sequence learning, then we should find implicit grammar learning to be selectively impaired in Condition E, at least during the first four sessions in which the shapes were absolutely valid as predictors of the next location. Moreover, if this effect of interference could be accurately characterized as an expression deficit, then it should disappear during the eighth session, when the competing cues were removed.

To assess these two predictions, we proceeded in the same way as that described for Experiment 1, using both RTs and hits percentages of responses to either grammatical or nongrammatical trials as the dependent variables, and analysing (1) whether the grammar-learning effects differed between conditions over the first four training sessions, and (2) whether any possible deficit in the expression of grammar learning arisen in Condition E disappeared at the final, eighth session, when the shapes were replaced by a neutral symbol.

Figure 5 summarizes these results as averages for each measure, for each condition and session, and for both grammatical and nongrammatical trials. As can be seen, the effect of grammar learning was immediately apparent in both conditions, and it produced a difference between responses to grammatical and nongrammatical trials that amounted to approximately 30 ms and about 2 percentage points over the fourth session. The ANOVA conducted on RTs with session (1 to 4), condition (E vs. I), and type of trial (grammatical vs. nongrammatical) showed significant effects of session, $F(3, 42) = 99.25, MSE = 110,903.46, p < .0001$, and type of trial, $F(1, 14) = 64.88, MSE = 16.868.25, p < .0001$, as well as an interaction of Session $\times$ Type of Trial, $F(3, 42) = 8.66, MSE = 616.10, p < .0001$. No other effect or interaction approached significance in this analysis. As for the ANOVA conducted on hits percentage with the same independent variables, it only produced a significant effect of type of trials, $F(1, 14) = 15.60, MSE = 96.60, p < .01$. 

Taken together, these results strongly suggest that grammar learning influenced performance of participants in both Conditions E and I, and that there was no difference between them in the way in which this learning was manifested in each condition, despite the fact that participants presented with Condition E were simultaneously exploiting the information provided by an alternative—and far more valid—cue. The fact that participants in Condition E were considering these alternative cues (i.e., the shapes) to respond more efficiently to the SRT task could be inferred not only from the previous analyses of responding to signalled and nonsignalled trials during the fifth session, but also from the sudden increase in RT selectively observed in Condition E between the fourth and fifth sessions, when the validity of these cues decreased from 100% to 80%. An ANOVA conducted on RTs with condition (E vs. I) type of trial (grammatical vs. nongrammatical), and session (4 vs. 5) showed that the relevant interaction of Session × Condition was significant in this case, $F(1, 14) = 35.27$, $MSE = 10,816.00$, $p < .0001$.

The performance of participants in Condition E during the fifth session was also informative about the potential interaction arising between the expression of shape and grammar learning. Indeed, only during this session was it possible to analyse the crossed effects of any of these cues, because only in this session were signalled and nonsignalled trials combined with grammatical and nongrammatical trials. However, two ANOVAs conducted on RTs and hits percentage for participants in this Condition E showed no significant interaction between the
effects of these two types of cue. The main effect of grammar learning was significant for both dependent measures: $F(1, 7) = 16.72, MSE = 4056.75, p < .01$, for RTs; $F(1, 7) = 10.26, MSE = 54.34, p < .05$, for hits percentage; and so were the main effects of shape learning: $F(1, 7) = 16.08, MSE = 34,080.08, p < .01$, for RTs; $F(1, 7) = 13.69, MSE = 345.19, p < .01$, for hits percentage. The interaction between these cues did not reach significance in any of these analyses: $F(1, 7) = 2.04, MSE = 248.09$, for RTs; $F(1, 7) = 2.31; MSE = 7.70$, for hits percentage.

Finally, if participants' reliance on an explicit cue did not produce any interference on the expression of implicit grammar learning, as observed in Condition E, then no release from such a hypothetical interference could be expected to arise in this condition at the eighth session, after the removal of these explicit cues. As can be observed from the figure, such removal produced a selective increase in RTs for participants assigned to Condition E but, crucially, such an increase was equivalent for both grammatical and nongrammatical trials, and hence it could not be attributed to an increase in the expression of grammar learning. This pattern of results was confirmed through an ANOVA conducted on RTs for Condition E, with session (7 vs. 8) and type of trial (grammatical vs. nongrammatical) as independent variables. Both the effects of session, $F(1, 7) = 35.69, MSE = 58,473.45, p < .001$, and of type of trial, $F(1, 7) = 33.97, MSE = 7,953.76, p < .001$, were significant, but not the interaction of Session × Type of Trial ($F < 1$).

Discussion

The results of this experiment do not confirm our initial hypotheses regarding the interaction between explicit and implicit sequence learning, and they show instead that implicit learning effects can be expressed regardless of the existence of a simultaneous source of explicit information, and even when participants deliberately used such information to respond to the SRT task. The preliminary analyses conducted on shape learning for Condition E showed that participants in this condition did explicitly learn, and acted upon, the set of contingencies that had been established between shapes and locations, whereas participants in Condition I did not acquire such knowledge. However, the analyses conducted on the expression of grammar learning during the SRT task also showed that the implicit facilitation effects derived from the grammatical structure of the material were expressed in a similar way for participants in both conditions, regardless of the acquisition and use of that explicit shape learning.

A first concern that could be raised against this conclusion has to do with the power of this design to detect a difference between conditions in the amount of grammar learning manifested through participants' performance in the SRT task. Indeed, our first purpose was not to compare different amounts of grammar-learning effects, but to conceptually replicate the conditions provided by Cleeremans (1997) in his Experiment 2 to completely prevent the expression of sequence learning in our Condition E. Instead, we have found that sequence learning can be expressed concurrently with the learning about another cue, and that the amount of learning is not significantly different between these two conditions. We conducted a post hoc analysis of the power of our design to detect such a difference through the measures of RT (i.e., a triple interaction of Condition × Session × Type of Trial) using the G·Power program (Buchner, Faul, & Erdfelder, 1992). For a medium-size effect, $f = .25$, and with $\alpha = .05$, we
obtained a noncentrality parameter $\lambda = 57.14$. Taking into account the violation of sphericity for this effect ($\varepsilon = .654$, according to the Greenhouse–Geisser estimate) we computed the power from epsilon-corrected values of both $\lambda$ and the degrees of freedom, but still obtained evidence that the design was powerful enough to detect such a medium-size effect ($1-\beta = .99$). As for the interaction of Condition × Type of Trial, we obtained $\lambda = 14.86$ and a power of $1-\beta = .95$ for the same values of alpha and effect size. We are aware, however, that this power should not be generalized to any other effect or interaction, and specially for those that do not involve the effect of type of trial. However, one of the main goals of these experiments was precisely to investigate such interaction in the most efficient way, and that is why we emphasized the necessity of using probabilistic sequences that could include within-subjects and simultaneous measures of responses to both grammatical and nongrammatical trials. In the face of these results, we could at least assert that these experiments have enough power to detect the main effects that we were looking for.

It seems, therefore, that the lack of significance of the difference between conditions in the expression of grammar learning cannot be attributed to a lack of sensitivity of our design to detect such a difference. But now, how can one reconcile the conclusion that participants in our Conditions E and I did express equivalent grammar-learning effects with the results reported by Cleeremans (1997), who showed no expression of learning of a similar grammar in the presence of another explicit cue? We surmise that the most plausible answer to this question could rely on the specific nature of the cues employed in each study. More precisely, the lack of grammar-learning effects observed in Cleeremans’ Experiment 2 could likely be due to a floor effect derived from the use of a particularly powerful type of cue. Indeed, Cleeremans signalled next location by using another stimulus that appeared simultaneously with the target one and that occupied the location at which the next target was going to appear. Attentional research with the cost–benefit paradigm (e.g., Posner, 1980; Warner, Juola, & Koshino, 1990) has shown that peripheral cues do capture people’s attention in an exogenous, automatic way even when they are not valid, and that valid cues promote the development of a further component of endogenous, voluntary allocation of attention (Juola, Koshino, & Warner, 1995). In the case of Cleeremans’ Experiment 2, these peripheral cues were absolutely valid, and hence both attentional components collaborated to produce a great facilitation effect that, on the average, produced choice RTs below 200 ms from the third session on. This exceptionally fast level of response for a six-choice RT task can undoubtedly be considered as anticipatory (see Willingham, Nissen, & Bullemer, 1989, for a similar reasoning) and would hardly leave room for any grammar learning to be manifested. In contrast, we arranged an arbitrary mapping between cues and locations, and no cue was presented at the cued location. Thus, for our cues to facilitate responding to the next location it was necessary that participants would not only

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3 In computing this parameter, we followed Buchner, Erdfelder, and Paul’s (1997) indications on the analysis of within-subjects effects on repeated measures analyses. Thus, we computed the noncentrality parameter $\lambda$ according to the following formula:

$$\lambda = N \ast m \ast f^2 / (1 - \rho \rho)$$

where $N$ is the total number of participants (16), $m$ is the number of levels of the repeated measures factor(s) ($2 \times 4$), $f^2$ is the square of the effect size (.0625), defined according to the conventions of Cohen (1988), and $\rho$ is the population correlation between the individual levels of the repeated measures effect, which was estimated from the whole set of RTs obtained through these experiments (rho = .86).
learn about this arbitrary mapping, but also that they would go through a number of intermediate processes that consisted of considering the identity of each shape, retrieving the rule appropriate for that item, and applying that rule so as to be prepared for responding to the predicted location. The fact that all these processes take time and, hence, that RTs do not drop to a floor level by relying exclusively on such explicit cues, might explain why even those participants that had a perfect knowledge about these relationships still showed latencies around 300 ms and manifested the effect of grammar learning.

GENERAL DISCUSSION

The primary goal of this research was to test whether the use of explicit knowledge about a predictive relationship would compete with the expression of implicit sequence knowledge under conditions that were slightly different from those tested by Cleeremans (1997). Our results have clearly indicated that such a competition is not generalizable to the contingencies arranged in this series of experiments and, more precisely, that the facilitation effects derived from grammar learning could be expressed simultaneously with either the explicit shape learning yielded by participants in the Condition E from Experiment 2, or with the implicit category learning manifested by all participants in Experiment 1.

Indeed, Experiment 1 showed that participants could barely use their explicit shape knowledge when they were engaged in a secondary task, thus suggesting that attentional load does really prevent the expression of such explicit learning, but it also demonstrated that participants in these conditions could not only exploit the grammatical structure of the material, but also the contingencies that related the category of each shape (i.e., the counting vs. non-counting response required by each of them) with the locations that were more likely to appear next. In Experiment 2, the removal of this secondary task allowed participants in Condition E to use their explicit knowledge about the shapes, but still this shape learning did not impair the expression of grammar learning, as compared to the effects observed in the Condition I from this experiment, in which there was no evidence of shape learning. The grammar learning observed in both experiments can be taken as implicit as judged by both the lack of intention to learn (which would be reassured for participants in Condition E by the fact that they could already predict each location with complete accuracy by relying on another cue), and by the fact that this grammar learning was better manifested through an indirect measure during the SRT task than through a comparable direct measure of learning obtained during the generation task (see Jiménez et al., 1996 for a discussion of the implications of such a pattern of results). As a whole, these results provide further evidence on the robustness of this implicit sequence learning, that seems to be produced regardless of the presence of either implicit or explicit competing cues, and independent of attentional load, with the only constraint that participants should pay attention to the predictor elements in order to learn about them.

Learning and expression of implicit and explicit knowledge

These results can be considered as somehow disappointing for our methodological purposes, in that they do not allow us to distinguish between implicit and explicit knowledge in terms of whether or not they interfere with any other source of implicit information. At an applied level, it is important to note that, if the results of Experiment 1 showed that the expression of
explicit shape learning was impaired by attentional load, yet participants continued to act upon the sequential constraints derived from the grammar, hence suggesting that implicit learning could be used as a potential safeguard against attentional overload. The main question that arises at this point is, however, of theoretical interest, and it asks why the performance based on a set of explicit contingencies that are presented as frequently as other competing, but implicit, contingencies should break down more easily than the performance based on those contingencies that remain implicit. This pattern of results may strike many readers as implausible, in that it seems to show that, under otherwise comparable training conditions, deliberate processing produced learning results that were weaker than those derived from incidental conditions. However, we think that there may be some arguments that could help us to integrate these results with the common-sense notion that controlled processing does usually provide the best conditions for learning.

On the one hand, we should assume that the effects of learning, both implicit and explicit, result from an effective training, rather than from the mere repetition of any given contingency in the environment. Thus, although explicit learning of a sequential contingency can occur in a few trials, its effects on the performance of a speeded task could ultimately depend on the automatization of this learning—that is, on the probability that the prediction of the next location could be achieved before the next stimulus does actually appear. But automatization, just like implicit learning, results from the repetition of a sequence of actions performed by the learners, and not from the mere fact that the sequential regularity is presented in the environment. Hence, only if participants systematically consider their explicit knowledge to respond to each trial of the SRT task could this knowledge be expected to develop strong response tendencies. But, on the other hand, if explicit facilitation is conceived as the product of highly controlled and voluntary, but therefore slow, processes of inference, then it is perfectly reasonable to assume that during the first training stages, and especially in conditions of attentional overload, the time gained by using such explicit contingencies would not compensate for the time needed to apply them, so that people would likely give up this explicit strategy and base their responses on any other information available. On the contrary, the effects of implicit sequence learning are typically obtained from cues that the learner must obligatorily process over thousands of trials in order to fulfill the requirements of the task, and hence their predictive value would be encoded automatically, regardless of the complexity of the underlying contingencies, and even if they are initially as useless as the explicit contingencies. With practice, however, these associative processes would group up, and produce strong facilitation effects that could resist attentional overload.

Hence, this reasoning would explain why the successive locations in the course of an SRT task, or the category of each shape during the counting task, could both be automatically encoded as predictors of the next location, whereas participants only use the explicit relationship between shapes and locations when they have enough resources available to do it. This reasoning is coherent with a number of studies that have recently found that implicit sequence learning is more related with the specific responses required by the orienting task than with any other salient, but task-irrelevant, feature of the stimulus (e.g., Nattkemper & Prinz, 1997; Ziessler, 1998). Importantly, such conclusions should not necessarily mean that sequence learning exclusively reflects the association between motor responses and their consequences but, rather, that people would automatically associate every successive event to which they pay attention (Logan & Etherton, 1994; Logan, Taylor, & Etherton, 1996), or every successive
cognitive operation that they perform with any given information. Hence, the notion of “response” should be extended beyond its “motor” connotations, to include any kind of processing operation performed by the learner (Ziessler, 1998). In this way, such a “response” account would converge with the “episodic-processing” accounts (e.g., Whittlesea, 1997). which claim that people's encoding of the events would heavily depend on how they process them, and that the effects of prior experiences on future performance would also depend on the similarity existing between the encoding and the transfer domains. Crucially, this could be true for both implicit and explicit learning, but in the explicit case both the encoding and the transfer tasks are partially under the strategic control of the learner, whereas in the implicit case they are programmed by the trainer.

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