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Synthetic Grammar Learning: Implicit Rule Abstraction or Explicit Fragmentary Knowledge?

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3 experiments were designed to demonstrate that classifying new letter strings as grammatical (i.e., conforming to a set of rules called a *synthetic grammar*) or ungrammatical may proceed from fragmentary conscious knowledge of the bigrams constituting the grammatical strings displayed in the study phase, rather than from an unconscious structured representation of the grammar, as Reber (1989) contended. In Experiment 1, grammaticality judgments of subjects initially studying grammatical letter strings did not differ from judgments by subjects learning from a list of the bigrams making up these strings. In Experiment 2, judgments about nongrammatical strings composed of valid bigrams placed in invalid locations were extremely poor, although better than chance. In Experiment 3 the explicit knowledge of bigrams as assessed by a recognition procedure appeared sufficient to account for observed performance on a standard test of grammaticality.

A widely held model of cognition endows human subjects with the ability to implicitly abstract the regularities or high-level rules embodied in richly structured stimulus domains. Over the last 20 years, this general model has received strong empirical support in the field of artificial grammar learning from extensive work by Reber and his associates (e.g., Reber, 1967; Reber & Allen, 1978; Reber, Kassin, Lewis, & Cantor, 1980; see Reber, 1989, for a review). In a typical experiment, subjects first study a set of letter strings generated from a synthetic grammar that defines authorized letters and the permissible transitions between them. The grammar used by Reber and his associates in several experiments, which also served in Dulany, Carlson, and Dewey's (1984) and in our experiments, consisted of five letter consonants (M, R, T, X, V) and the set of transition rules shown in schematic form in Figure 1. Some instances of letter strings that this grammar generates are MTTVRX, VXVT, or VXM. After studying some representative exemplars, subjects are asked to categorize new grammatical and nongrammatical letter strings. Nongrammatical items (e.g., MVRTR) are formed from the same subset of letters, but they violate transition rules. Most subjects are able to perform this task with better-than-chance accuracy. This finding is interpreted as evidence of subjects' ability to abstract the structural nature of the stimulus environment. In addition, because subjects appear to be unable to verbalize formal rules describing the structure, this abstraction is thought to be an implicit, unconscious process.

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In the last few years, these findings have received additional support from studies based on other paradigms. For instance, Lewicki, Hill, and Bizot (1988) studied the ability of subjects to improve their performance when the location of a response signal was determined by the pattern of its location on specific earlier trials. After extended practice, subjects exhibited a substantial decrease in reaction time to target signals with predictable locations, although they were unable to articulate any of the complex rules that regulated the sequence of trials (cf. also Lewicki, Czyzewska, & Hoffman, 1987; McKelvie, 1987; Millward & Reber, 1972; Nissen & Bullemer, 1987). Implicit learning has also been evident in process control tasks. For instance, in one of Berry and Broadbent's (1988) experiments, subjects were required to imagine that they were in charge of a city transportation department and were instructed to maneuver the time interval between buses to reach and maintain a specified load of passengers per bus. With training in a computer-simulated interactive situation, subjects were able to make appropriate adjustments, although they were unable to verbalize the actual function relating both variables (cf. also Broadbent, Fitzgerald, & Broadbent, 1986; Hayes & Broadbent, 1988; Stanley, Mathews, Buss, & Kotler-Cope, 1989). On the whole, these studies apparently provide consistent evidence that human subjects can unconsciously abstract environmental regularities and use this tacit knowledge to improve performance. This ability is considered to be highly relevant to accounts of how humans cope with the complex environment of everyday life, and the domain of relevance of this ability is believed to extend to perceptual and social behavior, language, and so on.

However, this framework is at variance with other lines of research, which contend that human learning needs to be mediated by conscious thought. One of the better empirically grounded arguments comes from works on human conditioning. Converging lines of evidence suggest that awareness of the relations between conditioned and unconditioned stimuli is a necessary condition for autonomous as well as motor

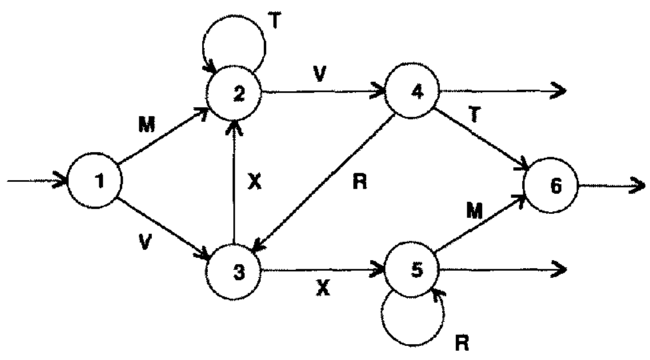

Figure 1. Schematic diagram of the grammar used in the present experiments. (This grammar is taken from Reber & Allen, 1978, and Dulany, Carlson, & Dewey, 1984.)

conditioning (for a review, see Dawson & Furedy, 1976; Perruchet, 1979, 1980). Similar conclusions may be inferred from data from operant conditioning paradigms (Brewer, 1974). At first glance, it appears somewhat paradoxical that abstraction of complex rules from highly structured domains can proceed implicitly, whereas abstraction of the simple and salient relation between two events in the impoverished context of conditioning calls for awareness. Moreover, several theories from other fields of inquiry also reject the possibility of any tacit learning processes. A case in point is provided by Shiffrin and Schneider's (1977) proposal that long-term memory modification is exclusively a function of controlled processing (see also Fisk & Schneider, 1984). Revision of Anderson's (1983) ACT* model also excludes the eventuality of unconscious learning (e.g., Lewis & Anderson, 1985).

These empirical and theoretical developments suggest that the reliability of findings supporting nonconscious complex learning should be reassessed. In a critical comment recently published in this journal, Brody (1989) argued that the usual method of assessing consciousness through subject's verbal reports is less than optimal, and he recommended the use of more rigorous procedures. We agree with Brody, and our study is an attempt to respect these objectives, as will be made clear later. However, another potential problem, which is perhaps of greater importance, is to determine the exact nature of the knowledge whose availability to consciousness should be examined. Our study is primarily directed toward *what* to measure, rather than *how* to measure.

No one questions the fact that the only knowledge of interest is the one that is really used to perform the task. The viewpoint developed by D. E. Broadbent, P. Lewicki, A. Reber, and their associates is that subjects learn the formal rules that experimenters use to generate, or at least describe, their experimental situations. Intuitive behavior is thought of as the end product of the unconscious application of the very same rules as the ones used to consciously conceptualize the task domain. As Smolensky (1988) pointed out, this idea is a traditional assumption in cognitive science and we refer to here as the *traditional* position. Now suppose that subject performance observed in the above-mentioned studies, although seemingly dependent on the application of abstract rules, is the outcome of entirely different information processing operations. A search for the awareness of complex rule knowledge would miss the point and would lead to inferring illegitimately that processing occurs at an unconscious level. This line of reasoning finds formal support in the contemporary development of connectionist models within the field of artificial intelligence. Computer simulations inspired by these models have proved that seemingly rule-dependent behavior may be mimicked by a cognitive architecture that never performs rule abstraction. Hunt (1989) showed that connectionist models apply equally well to diagnostic tasks in which full information is given on the stimulus material regardless of the subject's behavior (an analog to artificial grammar situations or P. Lewicki's procedures) as to a process-control task in which subjects are informed only of the effects of their responses on the system state (an analog to D. E. Broadbent and his co-workers' experimental settings). Moreover, Hunt showed that connectionist models more accurately portray subjects' behavior than do rule-based models, at least for the diagnosis problem.

This suggests that there may be value in scrutinizing what is really learned in situations designed to provide evidence for nonconscious complex learning. In this article, we deal with artificial grammar learning, which has the longest experimental past. In this research field, there are at least two other explanations for success in grammaticality assessment besides its being a result of the unconscious abstraction of formal grammar.

One explanation is based on the examplar-based categorization model (e.g., Medin & Smith, 1981). Rather than assuming that subjects extract conjunctive or disjunctive features defining categories, the model postulates that a new item is classified according to its global ressemblance with previously memorized individual instances of this category. Brooks (1978, 1987) developed a model of this type to account for implicit grammar learning. He argued that subjects draw analogies between test items and specific stored instances of grammatical items and ground the decision for well-formedness on the degree of observed resemblance. In several studies, researchers have tested this model by manipulating the grammatical status of test items and their resemblance to initial examples independently. The results provide qualified support for the use of an analogy strategy (Vokey & Brooks, cited by Brooks, 1987). However, these processes can account for global performance only under specific task constraints favoring memory for individual letter strings, such as the intensive repetition of a small number of items during the learning phase. In more standard conditions, a substantial part of performance, especially for a subset of subjects, remains unexplained (McAndrews & Moscovitch, 1985; Reber & Allen, 1978). Although important, the problem of the weight of an analogy strategy in different conditions will not be addressed here.

The second alternative to the traditional explanation is provided by Dulany et al. (1984, 1985), who argued that subjects learn and infer a multitude of simple rules rather than an integrated representation of a formal grammar. These microrules are thought to be limited in scope and imperfectly valid, in the sense that they cannot lead to correct decisions on the well-formedness of letter strings in all cases. They are, however, sufficient to account for the observed imperfect level of performance. In addition, these microrules are thought to be available to conscious awareness. Dulany et al. (1984) provided straightforward experimental evidence for this. When their subjects were given a test item and were asked either to underline the part of the item that made it grammatical or to cross out the part that made it ungrammatical, they performed reasonably well. Of more importance is that individual grammars generated from the marked features predicted grammatical judgements without significant residual. The ability to identify specific features that motivate grammaticality judgments of letter strings runs counter to both abstraction and analogical strategies as put forward by Reber (1989) and Brooks (1978, 1987), respectively, insofar as both interpretations are based on subjects' handling letter strings as integrated units.

The procedure used by Dulany et al. (1984), however, was not aimed at revealing the actual content of knowledge that motivates the marking of a feature. Suppose, for instance, that a subject crosses out the letter T in the string MVRTR. This may be because T is though to be an invalid letter whatever the context, or an invalid letter in the fourth position in a string, or an invalid letter in the next-to-last position, and so forth. Alternatively, this subject may judge that the pair RT (or TR, or the triplet RTR) is ungrammatical, or ungrammatical only in this specific context, and so on. Earlier results from Reber's laboratory shed light on this point. To evaluate the form and structure of knowledge resulting from exposure to representative exemplars of a synthetic grammar, Reber and Lewis (1977) analyzed performance on a subsequent anagram task. They observed that anagram solutions generated a few minutes after the learning phase revealed impressive knowledge of permissible two-letters patterns without concomitant knowledge of their location. They noted that subjects "in their introspective report ... frequently mentioned bigram patterns as particuliarly salient and relatively codeable" (Reber & Lewis, 1977, p. 344). In a subsequent study, Reber and Allen (1978) confirmed that "clearly, a considerable proportion of subjects' articulated knowledge can be characterized as an awareness of permissible and nonpermissible letter pairs" (Reber & Allen, 1978, p. 210).

The experiments reported in this article have been designed to explore the perspectives opened by Dulany et al.'s (1984) conceptual framework. Prompted by Reber's and his associates' previous observations, we focused on the role of knowledge of permissible pairs of letters in grammaticality judgments and on the related issue of availability of this knowledge to consciousness. The fact that subjects may acquire other pieces of information—namely, acceptable first and last letters—was taken for granted on the basis of prior evidence (e.g., Reber & Allen, 1978; Reber & Lewis, 1977). However, we hypothesized that when items whose nongrammaticality stems from unacceptable first or last letters are discarded from the analyses, (a) the fragmentary knowledge of permissible pairs of letters, irrespective of the position of these pairs in the strings, is sufficient to account for grammaticality judgment, and (b) this knowledge is available to awareness.

In Experiment 1, we drew a comparison between grammaticality judgments performed after standard exposure to representative strings and those performed after segmented presentation of the individual pairs of letters making up these strings. This condition was intended to preclude the abstraction of complex rules. We expected both conditions to generate the same level and pattern of performance. In Experiment 2, the learning phase was no longer manipulated, but the test was modified in order to distinguish between (a) ungrammatical items whose violation of grammaticality stemmed from the presence of nonpermissible pairs of letters and (b) items in which a permissible pair of letters was placed in a wrong location. Our hypothesis for Experiment 2 was that judgments would only exhibit better than chance accuracy in the former case.

In Experiment 3, we tested for the awareness of the knowledge of pairs of letters. The awareness assessment procedure departed from the one used in previous studies. Typically, subjects are asked for undirected introspective reports after the testing session. In our Experiment 3, the subjects were administered a recognition task that appears to be far more sensitive an index of awareness than do free reports (this issue is considered in more detail in the General Discussion section). This task was administered to separate groups of subjects at the same time that standard groups were making grammaticality judgments, which thus eliminated any effect of forgetting or testing-generated interference. Furthermore, the recognition task was designed so that subjects would respond unambiguously to the question of whether the conscious knowledge of permissible pairs of letters was sufficient to account for correct grammaticality judgments. To test this point, we quantitatively simulated grammaticality judgments on the basis of recognition data, and the results were compared with actual judgments.

General Method

Material

The grammar used in the three experiments was adapted from that of Reber and Allen (1978) and Dulany et al. (1984; see Figure 1). All the items (study and test) were typed in capital letters.

Procedure

Subjects were 264 first-year university students majoring in psychology. No subject participated in more than one experiment or group. The data from 2 additional subjects were discarded because they returned their response sheets blank.

Subjects participated in groups of 7–37. For each experiment, subjects were randomly assigned on an alternating basis to experimental conditions within test groups. To make this possible, the instructions specific to each group were provided on separate sheets of paper.

For all subjects, the session comprised a learning phase and a test phase. The following description of the procedure concerns the standard experimental conditions; specific manipulations introduced in some groups are described in the Method sections for individual experiments.

Each subject first received two booklets in a folder. Oral instructions were given to remove the first booklet from the folder and to read the test printed on the front page. Whatever the specific instructions, this page ended with the sentence "Do not turn the page before the signal." When the signal was given, subjects studied the items (typically the grammatical strings of letter) displayed on the remaining pages of the booklet for 10 min. These papers were then collected by the experimenter.

Subjects were orally requested to take the second booklet out of the folder and to read the front page, which again ended with the sentence "Do not turn the page before the signal." When the signal was given, subjects were allowed to look at the items (typically the set of grammatical and ungrammatical strings of letters) on the answer sheets. They had to cross out the items that they felt were ungrammatical. The experimenter paced this phase by asking the subjects to move to the next item every 5 s.

The subjects were told that a complete debriefing about the experiments was scheduled for a future lecture.

Scoring

In most studies, well-formedness assessments are scored as the proportion or percentage of correct responses over the total number of items. We report scores in this form when comparison with other findings in the literature is pertinent. This scoring mode raises no real problem when half the test items are grammatical and the other half are ungrammatical, which is the usual case. However, in our experiments, the specific purpose of some analyses made it impossible to balance items in this way. When the number of grammatical and ungrammatical test items differed, the percentage of correct responses was sensitive to the response criterion. For this reason, we calculated a difference score (hereafter referred to as the *D* scores), which is defined as the percentage of ungrammatical items correctly categorized minus the percentage of grammatical items mislabeled as ungrammatical. A *D* score of zero corresponds to random responding, regardless of the partition of items or the criterion of responses (this scoring method is comparable, for instance, to the procedure used in recognition memory studies that consists of subtracting the false alarm rate from the hit rate).

Experiment 1

Two groups of subjects, hereafter termed the standard or S groups, learned from usual letter strings, with incidental or intentional instructions. These instructions were, respectively, diverted from or oriented toward search for the rules that constituted the grammar. It is clear that the labels *incidental* and *intentional* are ill-suited to our interpretive framework because if the knowledge underlying a grammaticality judgment is unrelated to these rules, both kinds of instructions would be better qualified as incidental in nature. This factor was incorporated into the study to parallel current grammar learning literature, without particular expectation.

The same material, after segmentation into pairs of letters, was presented to a third group (P group). All the subjects were subsequently asked to judge the grammaticality of a common set of test items. We hypothesized that the performance of the P group would not differ from the performance of the S groups.

Method

Materials. For the S groups, the study items were the 20 letter strings used by Dulany et al. (1984). The strings were made up of 102 letters and were typed in a single column in random order.

The P group was shown the same number of letters, which, however, were paired for presentation. The 51 resulting pairs were chosen so that the frequency of occurrence of each pair best matched its frequency of occurrence in the letter strings from which it was extracted. To illustrate: Pair XV occurred six times in the S groups' letter strings in a total of 82 pairs (the reference value is here 82 and not 51 because each letter of a string, except the first and the last ones, enters into two different pairs). Therefore XV was presented $(6/82) \times 51 = 3.73$ times, rounded off to 4 times, to the P group. The pairs of letters were typed in three columns in random order (see Table 1).

The test items were taken from Dulany et al. (1984). The 50 letter strings (25 grammatical and 25 ungrammatical strings) were repeated once. The items were presented in a four-page booklet; each page contained a column of 25 items. Order of presentation was randomized.

Procedure. Eighty-nine subjects were divided in the three groups. For the incidental ($N = 30$) and intentional ($N = 29$) S groups, the instructions given to the implicit and explicit groups of Dulany et al.'s (1984) experiments were translated into French. Only minor rephrasing was required to adapt the text to our arrangement.

At the beginning of the study phase, the P group subjects ($N = 30$) were asked to learn pairs of letters for 10 min. They were told that pairs were made up of the letters M, R, T, V, X. Before the test phase, the subjects were told that half of the strings displayed on the answer sheets were arranged so that each pair of contiguous letters came from the list previously studied, and half of the strings contained new combinations of the same pool of letters. The remaining instructions were identical to the ones given to the standard groups.

Results and Discussion

The mean percentage of correct judgments exceeded the expected value of 50 for the incidental S group ($M = 63.3$), $t(29) = 10.99$, $p < .001$, and the intentional S group ($M = 60.2$), $t(28) = 6.77$, $p < .001$). Because these scores did not

Table 1
Study Items Used in Experiment 1 (P Group)

Column 1	Column 2	Column 3
VR	VX	RX
RX	TV	XM
VT	RR	XV
TT	MT	RM
VX	RX	VR
MT	VR	MT
RR	XV	VX
VX	TT	MV
TV	VR	XR
MV	MV	TT
VR	XV	VR
TV	VT	RX
RX	VX	RR
XT	XR	VT
XR	TV	XT
RR	XT	TV
XV	RX	VX

differ significantly, $F(1, 57) = 2.51$, $p > .10$, data from both standard groups were pooled for subsequent analyses.

The overall score for the P groups was 57.2%, which also exceeded the expected value, $t(29) = 6.38$, $p < .001$, but remained lower than the score for the standard groups, $F(1, 86) = 8.42$, $p < .01$.

These scores included judgments of ungrammatical test strings in which nongrammaticality stemmed from a nonpermissible initial letter (the grammar authorized any final letter). Such items were especially easy to categorize for subjects who inspected grammatical strings during the study phase, whereas correct judgments were obviously impossible for subjects who inspected pairs of letters. A proper test of our hypothesis involves withdrawing these items from analyses.

When scores were conditionalized on the 84 (of 100) remaining letter strings, they were practically equivalent. The mean scores were 59.30% (SD = 7.57%) for the S groups and 61.03% (SD = 8.82%) for the P group, $F(1, 86) = 0.91$. This result provides evidence that subjects studying pairs of letters performed as well as did subjects studying letter strings, when analysis bears on the relevant data.

Overall, the criterion of response differed between groups. The percentage of items judged as ungrammatical was lower for the P group than for the S groups (*M*s = 38.27 vs. 49.55), $F(1, 86) = 9.46$, $p < .01$. This may indicate that the same scores were obtained in different ways as a function of the material initially displayed. However, the data base for this computation included the 16 items in which the first letter was a basis for categorization. After we withdrew these items from analysis, the difference between groups fell to a nonsignificant value (*M*s = 38.49% vs. 44.7%), $F(1, 86) = 2.53$, $p > .10$.

One could argue that the slight remaining difference is sufficient to artificially inflate the performance of the P group in grammaticality judgments. The 84 relevant items include more grammatical (50) than nongrammatical (34) items, thus producing a bias toward positive over negative well-formedness assessments. To rule out this interpretation, we calculated a difference score insensitive to the response criterion (see the General Method *Scoring* section). The D scores for the S and P groups again were nearly equivalent (Ms = 17.24 vs. 18.34), $F(1, 86) = 0.11$.

Experiment 1 thus showed that the performance observed in typical artificial grammar-learning studies does not demonstrate subjects' purported ability to abstract a complex grammar. We observed highly comparable performance when subjects had the opportunity to learn only from pairs of letters, which thus precluded the formation of any complex knowledge.

Only one letter string on the test list that we used (after Dulany et al., 1984) failed to be correctly classified when subjects used the pieces of information that we assume they acquired. In this letter string (VRRRM), all the pairs of letters are permissible, and V is an authorized initial letter; however, this item is nongrammatical because VR cannot be generated by the grammar at this location in the string. What happens when the test is made more sensitive to the knowledge pertaining to the positional context of permissible pairs? In Experiment 2 we investigated this issue.

Experiment 2

In this experiment, all the subjects learned from letter strings; changes from the typical procedure concerned the test phase. Violation of grammaticality stemmed from the presence of nonpermissible pairs (NPs) of letters for half of the test strings and from a (permissible) pair of letters placed in a wrong location (nonpermissible order, or NO) for the other half of the strings. We hypothesized that subjects could correctly judge the NP items as ungrammatical but would fail to categorize NO items, under both incidental and intentional instructions.

We assessed presence or absence of learning by comparing the performance of experimental subjects with the performance of control subjects for grammatical strings displayed during the study phase that were replaced by strings of letters in random order. The use of control groups in this context calls for comment because in most grammar-learning experiments, including our first one, observed performances are compared against the expected value for chance response. Although this procedure is satisfactory in most cases, it may not be reliable to test for presence or absence of learning. Dulany et al. (1984) reported that the mean proportion of correct responses of control subjects who had no opportunity to study grammatical letter strings significantly exceeded the expected value of .50. Because our main hypothesis concerned the absence of learning in a specific condition, we compared the scores of experimental subjects with those of controls in order to cancel the potential effect of factors such as a priori differences in the impression of well-formedness elicited by test strings.

Method

Materials. The letter strings displayed in the study phase for the experimental groups were the same as those in standard groups in the previous experiment. For the control groups, a new set of 20 letter strings was generated. These new letter strings matched the letter strings for the experimental group with regard to a number of features, including length of strings, nature and frequency of letters, and number of repetitions. Within these constraints, letters were rearranged in a pseudorandom order.

The 72 test letter strings were the same for all subjects. There were 24 grammatical items, selected from the 25 used in Experiment 1; 24 nongrammatical items with one nonpermissible pair of letters (NP); and 24 nongrammatical items with one pair of letters placed in a wrong location (NO). Both kinds of nongrammatical items had acceptable first and last letters. All the items were matched for length: Each set was composed of 2 three-letter strings, 6 four-letter strings, 6 five-letter strings, and 10 six-letter strings. In addition, NP and NO items were matched for the position of nongrammatical pairs within the strings: nongrammatical pairs occurred six times in Position 1, seven times in Position 2, six times in Position 3, three times in Position 4, and twice in Position 5 (see Table 2).

Procedure. One hundred seventeen subjects were randomly assigned to incidental experimental (N = 27), intentional experimental (N = 29), incidental control (N = 31), and intentional control (N = 30) groups. The instructions for incidental and intentional groups were identical to those used in Experiment 1 and were the same for experimental and control groups. The experimental and control groups only differed with respect to stimulus material displayed in the study phase.

Table 2
Test Items Displayed in Experiment 2

	Nongrammatical	
Grammatical	NP	NO
MVT	*VM*V	*VR*X
VXV	V*XX*	M*VX*
VXRR	*MX*VT	*VR*XV
VXRM	*MR*XT	*VR*XM
MTVT	V*XX*V	M*VX*V
MTTV	M*TX*R	M*VX*R
VXTV	M*VV*T	M*VX*M
VXVT	MT*VV*	MT*VX*
MTTTV	*MM*VRX	*VR*XRM
MVRXM	M*TR*RR	M*VX*RR
MVRXR	VX*TM*V	VX*RX*M
VXRRR	VX*RV*T	VX*MT*V
VXTTV	VX*TX*M	MT*VX*V
VXVRX	VXR*RT*	MTT*VX*
VXTTTV	*MX*VRXM	*VR*XRRR
MTTVRX	*VM*TTTV	*VR*XTTV
MVRXVT	V*XX*VRX	M*VX*VRX
MTVRXV	M*VM*TVT	M*VX*RRR
MTVRXR	VX*MR*XV	VX*RX*RM
VXVRXR	MV*RT*TV	MT*VX*VT
MTTTVT	VXV*RV*X	VXV*RR*M
VXRRRM	MTT*TM*V	VXT*VX*M
VXVRXV	MTTV*TR*	MTTT*VX*
MVRXRM	VXRR*RT*	MTTV*RM*

Note. NP = nonpermissible pairs, NO = nonpermissible order. Nonpermissible pairs of letters are in italics.

Results and Discussion

The *D* scores are shown in Table 3. Simple proportions are not reported because the strong disproportion of grammatical and nongrammatical test strings made them sensitive to response bias.

Scores for control subjects did not differ from zero in any of the conditions displayed in Table 3 ($t < 1$ in all cases). An analysis of variance (ANOVA) was performed with two between-subjects factors (experimental vs. control; incidental vs. intentional) and one within-subject factor (NP vs. NO). As a whole, subjects in the experimental groups had higher scores than did subjects in the control groups (*M*s = 13.35 vs. −1.03), $F(1, 113) = 39.41$, $p < .001$. This implies that subjects benefited from inspecting grammatical items rather than randomly generated strings of letters.

Table 3
Experiment 2: D Scores for Four Groups of Subjects, for Nonpermissible Pairs (NP) and for Nonpermissible Order (NO) Items

	Nature of nongrammatical items			
	NP		NO	
Instructions/conditions	*M*	*SD*	*M*	*SD*
Incidental				
Experimental	21.75	15.57	7.25	13.45
Control	1.07	14.59	−2.28	13.13
Intentional				
Experimental	17.67	16.49	6.75	11.69
Control	−1.12	12.28	−1.80	14.69

When analyses were restricted to experimental groups, incidental instructions were slightly more efficient than intentional instructions (*M*s = 14.5 vs. 12.21). However, no significant effect was observed for instruction, $F(1, 54) = 0.46$, or for the Instruction × Nature of Items interaction, $F(1, 54) = 0.54$. Nature of items appeared to have had a pronounced effect on performance: the *D* score was much higher for NP than for NO (*M*s = 19.63 vs. 6.99), $F(1, 54) = 47.76$, $p < .001$. Thus, regardless of type of instructions, subjects made much better judgments when nongrammaticality stemmed from the presence of one nonpermissible pair of letters than from a permissible pair of letters in a wrong location.

Nevertheless, there was evidence of some reduced form of learning in these latter items. When comparisons were conditionalized on NO strings, experimental groups still performed significantly better than control groups, under incidental instructions, $F(1, 56) = 7.44$, $p < .01$, as well as intentional instructions, $F(1, 57) = 6.10$, $p < .05$. More fine-grained analyses were carried out in order to investigate what subjects learned. These analyses indicated that learning mainly concerned the first pair of the letter strings: For all the experimental subjects, the mean *D* score was 14.01 when only the initial pair was taken into account and 5.49 in the other cases. However, such post hoc analyses are suggestive at best, because the location of pairs was not independent of other factors such as the nature of the letters and the length of the strings.

Overall, Experiments 1 and 2 indicated that subjects' knowledge underpinning grammaticality judgments mainly covers individual authorized pairs of letters, although some more subtle forms of knowledge may be brought to light when the test is especially designed to do so.

In Experiment 3 we addressed a related issue, one pertaining to the availability in consciousness of knowledge concerning permissible pairs of letters.

Experiment 3

After studying grammatical strings, the subjects performed a recognition task that specifically concerned the main pieces of information that we hypothesized to be the basis for judgments of grammaticality: the pairs of letters making up the grammatical strings. We then quantitatively simulated grammaticality judgments on the basis of this knowledge, and the results were compared with those collected in Experiments 1 and 2 (recall that subjects participating in all of our experiments were randomly drawn from the same population and were tested in similar conditions).

Method

Materials. The letter strings displayed in the study phase were the same as those used for the standard groups in Experiment 1 and the experimental groups in Experiment 2. In the test phase, the 25 different pairs of letters generated by the combination of the five letters of the grammar were displayed in a single column in random order. Facing each pair were six boxes marked from left to right "−−−," "−−," "−," "+," "++," and "+++."

Procedure. Fifty-eight subjects were assigned to two groups of 29 subjects each. The groups differed only with respect to the instructions (incidental vs. intentional) given at the beginning of the study phase. Instructions were identical to the ones used in Experiments 1 and 2.

Before the test phase, subjects were told that about half of the pairs of letters on the answer sheet were part of the letter strings previously studied and half of the pairs of letters had not been displayed previously. They were asked to indicate the status of each pair on a 6-point scale, as often used in memory recognition studies (Murdock, 1982). Significance of each of the six symbols displayed on the response sheets was explained on the instruction page (e.g., "−−−" corresponded to *You are sure that this pair was never present in the strings studied*, and "+++" corresponded to *You are sure that this pair was part of one or several strings of letters*). Subjects had to put an *X* in the box corresponding to their choice. They were encouraged to respond on all items.

Results

The responses on the 6-point scale were scored from 1 (*sure no*) to 6 (*sure yes*); (see Table 4). The scores were higher for the old pairs (i.e., pairs included in letter strings) than for new pairs; results from the intentional and incidental groups were nearly equivalent, $Fs(1, 28) = 39.44$ and 36.72, $ps < .001$. Data from intentional and incidental groups were pooled for subsequent analysis.

Although the mean difference (about 1.11 scale units) between old and new pairs appears moderate, it is worth noting that discrimination was fairly good. In terms of overall group means, subjects scored lower on only three old pairs (RM, XM, and XT) than on the highest new pair (MR). These three old pairs were, not surprisingly, those that occurred the least frequently in the letter strings studied; they appeared, respectively, only 1, 2, and 4 times, whereas other pairs occurred up to 10 times. Overall, the correlation between recognition scores and frequency of occurrence computed on the old pairs was .61 ($p < .05$).

In order to simulate grammaticality judgments presumably accounted for by explicit knowledge, we needed a decision rule. We postulated that subjects judged ungrammatical any letter string that contained at least one nonrecognized pair of letters. However, because subjects had to respond on a 6-point scale, the borderline between recognition and nonrecognition was not clearly defined. When a simulation was carried out after we classified the pairs scored 1 or 2 (i.e., marked "−−−" or "−−") as nonrecognized, the percentage of letter strings judged ungrammatical was lower than the percentage actually observed in Experiments 1 and 2. In contrast, when the pairs scored 3 (i.e., marked "−") were also considered to be nonrecognized, the percentage of letter strings judged ungrammatical was notably higher than that of the actual ones. This led us to adjust the cutoff point as a function of individual response criteria; for each subject, we initially carried out a simulation by classifying the pairs scored 1 or 2 as nonrecognized; if the percentage of letter strings judged ungrammatical was found to be lower than 25% of all the letter strings, pairs scored 3 were also classified as nonrecognized, and the simulation was run again. On this basis, the mean percentage of strings judged ungrammatical were 49.2 when the simulation was run on the test items used in Experiment 1 and 52.06 when simulation was run on the test items used in Experiment 2. These values were highly comparable with the actual values (49.55% and 50.84%, respectively).

What proportion of correct grammaticality judgments can be explained by conscious knowledge of permissible pairs? First, in test used in Experiment 2, half of the ungrammatical strings contained a nonpermissible pair of letters, and half included a permissible pair of letters in a wrong location. Simulation of performance was clearly irrelevant for the latter items because knowledge of correct pairs serves no purpose in guiding classification in any way. Hence these items were excluded from the simulation. The simulated *D* score for the remaining items was 18.53. This value is close to the observed values in the experimental groups (19.64), $F(1, 111) = 0.12$. However, generalizing this result may be premature because the strings were especially designed to make the knowledge of pairs relevant.

A simulation was then run on the test in Experiment 1, which is prototypical of the tests used in the literature (see Dulany et al., 1984, p. 549). Only the 16 items (of 100) for which nongramaticality stemmed from a nonpermissible initial letter were excluded from analysis. Simulated performances were again virtually identical to the observed ones (M = 18.92 and 17.24, respectively), $F(1, 114) = 0.23$.

Table 4
Recognition Scores for Old and New Pairs of Letters

	Incidental instructions		Intentional instructions	
Type of pair	*M*	*SD*	*M*	*SD*
Old	4.42	0.54	4.43	0.60
New	3.32	0.77	3.30	0.77

General Discussion

Summary of Results

The main findings can be summarized in four points: (a) When subjects were exposed to separate pairs of letters that constituted longer letter strings, they performed subsequent grammaticality judgments as well as did subjects who had the opportunity to study the letter strings. (b) When subjects learned from the letter strings, they performed better than chance in a subsequent test of recognition bearing on the separate pairs of letters that constituted these strings. As in paired associate learning, recognition rate correlated with the frequency of occurrence of pairs. (c) When we simulated the strategy of making a judgment of nongramaticality for any test string containing at least one unrecognized pair of letters, the resulting performances matched nicely with the observed ones. (d) When the ungrammatical status of a test item stemmed from the wrong location of a permissible pair of letters, well-formedness assessments were poor but nevertheless significantly better than chance.

The Role of Bigram Knowledge in Grammaticality Judgments

The terms *abstract* and *complex* are loosely defined. However, in our context, fragmentary knowledge of the pairs of consecutive letters included in the items may hardly be considered abstract and complex, inasmuch as it involves only pairwise association learning. Hence positive evidence that this knowledge (eventually along with the knowledge of the first and last letter of items) is sufficient to account for performance in typical tests of grammatically runs counter to the contention that above-chance performance on these tests is proof of the human ability to abstract complex rules embodied in letter strings.

There is, however, a possible objection. According to Reber (1989), the bigram knowledge acquired in an artificial-grammar-learning setting is not the simple product of the processes underlying standard paired associate learning; rather, it reflects the deep structural characteristics of the grammar. This contention is essentially based on an intriguing experimental result reported by Reber and Lewis (1977), which deserves close examination.

In this study, the grammaticality test was replaced by an anagram task. The bigrams occurring in subjects' solutions to the anagrams were tabulated, and their frequencies were compared against (a) the frequency of occurrence of the bigrams composing the strings actually displayed in the study phase and (b) the frequency of occurrence of the bigrams composing the full set of strings generated by the formal grammar. The correlation coefficients were, respectively, .04 and .72. Reber (1989) concluded that the failure of the correlation involving the study phase bigrams "to be different from zero suggests that subjects were not solving the anagrams on the basis of superficial knowledge of frequency of bigrams" (p. 226). Rather, the correlation pattern indicates that subjects "clearly acquired knowledge that can be characterized as deep, abstract, and representative of the structure inherent in the underlying invariance patterns of the stimulus environment" (Reber, 1989, p. 226).

These findings call for several cautionary remarks. First, selecting the strings instantiating the grammar for the study phase from the entire set of possible strings drastically reduces the variability of bigram frequency. The variance of the distribution of the bigram frequency of selected items is about one ninth of the corresponding value calculated from the whole sample (3.85 vs. 35.89; these values were calculated from the material presented in Appendix A in Reber & Lewis's 1977 article). Low variance obviously makes it difficult to obtain high correlations. A second remark stems from a careful examination of the experimental design. As a result of selection, some bigrams were underrepresented (*U* bigrams) in the displayed items, in the sense that their frequency was lower in the displayed items than in the whole sample, whereas other bigrams were overrepresented (*O* bigrams). The observed correlations reflect the fact that *U* bigrams are more easily learned than *O* bigrams. In Reber's perspective, this outcome would stem from the fact that *U* bigrams are more central to the deep grammar structure than suggested from displayed examplars, whereas the reverse is true for *O* bigrams. However, the differences in rate of learning between *U* and *O* bigrams may be accounted for by more trivial factors. Subjects who are asked to solve anagrams start from strings of letters, which impose severe constraints on the nature of the pairs that may be produced. Reber and Lewis obtained these strings by scrambling the grammatical strings that were not initially displayed. Frequency of their constitutive letters necessarily mirrored that of the displayed items: Letters underrepresented in this material were overrepresented in the displayed strings, and vice versa. As a consequence, subjects were prompted to produce bigrams whose frequency exhibited better fit with the bigram frequency in the whole sample than in the displayed strings. This tendency may have been further reinforced by another powerful bias, linked to the selected material. The concrete items that deviated most from representativity were VV, TV, XX, and TT in the *U* bigrams and PV, XS, and PX in the *O* bigrams (deviation is operationalized here by the signed difference in the number of occurrences of bigrams in the two sets of strings). Thus *U* bigrams are doublets[1] or extremely common abbreviations, but *O* bigrams have no really salient characteristics to help memorization.

Our studies provide no empirical data on the distinction between the bigram frequency in the observed items and in the whole sample because the two sets of data were highly related in our material ($r = .92$), and we know of no replication attempts of Reber and Lewis's (1977) experiment. Pending further empirical data, the available results provide no compelling reason to review our conclusion that judgments in typical tests of grammaticality mainly involve the ubiquitous ability to learn pairwise associations.

Bigram Knowledge and the Issue of Awareness

As argued at length in the introduction, the controversial issue of awareness is tightly linked to the answer to the question "What is learned?" If, in consonance with the traditional assumption of cognitive science (Smolensky, 1988), the knowledge underlying grammaticality judgments is seen as the end product of a process of abstraction of complex rules, investigations of awareness will naturally focus on these postulated rules. From the first exploration in the field (Reber, 1967), such knowledge has consistently appeared to be unavailable to consciousness. However, another outcome emerges if one assumes that grammaticality judgments can proceed from other forms of knowledge. When search was oriented toward knowledge of permissible pairs of letters, as in our experiments, subjects were found to be aware of

[1] The fact that doublets are easier to learn than pairs of different letters seems intuitively valid. This is confirmed in Experiment 3 in various ways. For instance, invalid doublets scored notably lower on the recognition scale than other invalid bigrams (2.32 vs. 3.69), $t(57) = 9.78$, $p < .001$. This result cannot be accounted for by the frequency of bigram occurrence in study strings or in the whole strings that the grammar can generate because these pairs never occur.

fragments of information sufficient to account for grammaticality judgments.

Methods of assessment of conscious knowledge call for comment. Brody (1989) showed that the widespread use of undirected verbal reports in grammar-learning studies is not optimal for the examination of unconscious processes. Our use of a recognition procedure is consonant in general with Brody's recommendation, and Reber (1989) ackowledged that "this procedure is, in principle, superior to that used by most researchers, who mainly browbeat their subjects into telling what they know" (p. 231). In addition, one form or another of recognitionlike procedures is considered the method of providing the best operationalization of consciousness by most researchers in other fields of inquiry. In the area of human classical conditioning, for instance, methodological advances over the past 15–20 years have provided evidence that recognition tests have a greater construct validity than does recall in assessments of conscious knowledge of conditioned/unconditioned relationships (e.g., Dawson & Reardon, 1973). Similar methodological considerations emerged in the literature on subliminal perception in the 1950s (e.g., Eriksen, 1958); a recrudescence of studies on this issue has taken place over the past 10 years, the main objective of which is to test whether a target stimulus presented subliminally may prime subsequent processing of the same stimulus or a semantic associate (e.g., Dark, Johnston, Myles-Worsley, & Farah, 1985) or influence some evaluative judgment (e.g., Mandler, Nakamura, & Van Zandt, 1987). In most of these studies, the proof that the initial stimulus was not consciously identified is given by chance-level performance on a subsequent forced-choice recognition procedure.

Despite the common contention that recognition procedures provide the best available assessment of conscious knowledge, their use is still open to criticism. The recent literature on the distinction between explicit and implicit memory sheds light on this point. Explicit memory tests imply that subjects make conscious reference to a specific learning episode. In contrast, implicit memory is revealed when a previous episode influences performance on a test that does not require conscious recollection of this episode (for a review, see Schacter, 1987). Recognition tests are conventionally assigned to the explicit component of memory. Studies on amnesic patients (e.g., Squire & Cohen, 1984) and investigations focusing on individual differences patterns with normal subjects (e.g., Perruchet & Baveux, 1989) provide converging evidence that recognition data parallel performance on free-recall and cued-recall tests and depart from performance on implicit memory tasks. However, several memory theorists take the position that recognition may entail both explicit and implicit processes. For instance, Jacoby (1983) argued that the relative fluency of perception, which relies on implicit processes, may be used as a cue for discriminating old from new items in a recognition task, thus making a variable contribution to recognition judgments over and beyond a directed memory search factor. This dual component of recognition memory is supported by some behavioral (e.g., Johnston, Dark, & Jacoby, 1985) and neuropsychological (Smith & Halgren, 1989) data. It is obvious that information recognized from some feeling of familiarity stemming from the relative ease of perceptual processing without the retrieval of the specific traces of the initial episode can hardly be considered to be a component of conscious knowledge. As a consequence, the use of recognition tests may inflate the explicit knowledge that researchers intend to assess. Studies in this area stress the need to develop new, more reliable techniques for assessment of subjects' explicit knowledge. Future researchers could reap the benefits from the method suggested by Tulving (1985) and used by Gardiner (1988), in which subjects are directly asked to discriminate between items they can consciously recollect and those they recognize on some other basis.

When Subjects Acquire More Complex Knowledge

The foregoing developments focus on the fact that the conscious knowledge of permissible bigrams and authorized initial and final letters account for performance on standard tests of grammaticality. Using a specially designed test in Experiment 2, we observed that subjects may learn a bit more than these elements. The supplementary knowledge revealed by this new test is apparently severely restricted and seems to be related to those pairs of letters that can occur in the first location. However, the earlier literature also provides some evidence that subjects may acquire relatively complex knowledge in artificial grammar experiments. For instance, Reber and Lewis (1977) reported the gradual emergence of knowledge of the overall positional configurations of letter pairs in their anagram task when the sessions were repeated over a 4-day period. Dulany et al. (1984) also furnished indirect evidence of configural learning when they compared the predictive validity of the reported features in and out of their positional context. Are these convergent findings evidence of an unconscious rule-induction process, which gives access to a tacit representation of the formal grammar?

We stress once again that the issue of awareness ought to be postponed until the mode of processing has been identified clearly. A number of processing models may be put forward. According to one, subjects proceed by progressive, step-by-step accumulation of piecemeal knowledge. For instance, subjects who initially learned bigrams could acquire triplets with more extensive training. This suggests that grammar learning could proceed by progressive constructions of bigger chunks, a learning model that Newell and Rosenbloom (1981) thought applied to perceptual motor skills as well as to a broad range of cognitive behavior. In our case, the primitive chunks could be the individual letters, and gradually, with learning, chunks with a span of two, three, and eventually more primitive elements could be formed. If this kind of hypothesis can be confirmed empirically, the next step would be to test whether subjects have a conscious representation of the chunks, through some form of recognition procedures. A second interpretation was put forward by Brooks (1978, 1987), who contended that when subjects are given a grammaticality judgment task, they proceed by drawing analogies between test items and remembered exemplars of study items. This interpretation also clearly accommodates configural learning. Although Brooks himself took no definite position on the issue of awareness, the use of an analogy strategy is often referred to as the conscious facet of thought (e.g., McAndrews & Moscovitch, 1985). A third explanation draws on explicit rule abstraction processes, such as those that are the main focus of traditional concept-learning and problem-solving experiments. For instance, the gradual emergence of configural learning evident in Reber and Lewis's (1977) study may be accounted for in these terms: The repetition of the anagram task over 4 days must have given subjects a clear representation of task requirements when they had new opportunities to learn about the grammar. Thus they may have engaged in an explicit mode of learning, including hypothesis formulation, rule testing, mnemonic strategies, and so on.

Overall, convergent data indicate that subjects may acquire more knowledge than permissible bigrams and first and last letters of strings, especially with extended practice in artificial-grammar-learning settings. However, to date there is no evidence that this complex knowledge is acquired unconsciously and tacitly maintained.

Relations With Previous Research in Related Fields

Synthetic grammar learning is commonly acknowledged as the main paradigm providing experimental evidence for the human propensity to abstract unconsciously the complex rules embodied in complex environmental modifications. However, studies based on other paradigms have recently reached the same general conclusion (e.g., Hayes & Broadbent, 1988; Lewicki et al., 1987, 1988; McKelvie, 1987; Nissen & Bullemer, 1987). At first glance, this upsurge of convergent results limits the bearing of our findings. However, close examination of the recent studies suggests that all share the fundamental drawbacks that we pointed out in the specific context of grammar learning. As a rule, subjects are faced with a very complex situation, at least within assigned time limits, and their learning ability is assessed through a significant improvement in some crude indicator of performance, such as reaction time, error rate, and so on. These indices may lead to the reliable conclusion that subjects learn about the situation; however, procedures of this type are poorly adapted to showing that subjects learned the rules, or a subset of the rules, that the experimenter used to generate the stimuli. By and large, researchers in recent studies have not entertained the possibility that what is thought to be the end product of complex rule abstraction processes can be reduced to other, far simpler pieces of knowledge, which may in turn be available to consciousness. In addition, they rely on verbal free reports to assess conscious knowledge instead of on more sensitive measurement techniques such as recognition procedures. These studies warrant a close reexamination along the methodological guidelines delineated earlier.

As a case in point, Perruchet, Gallego, and Savy (in press) examined Lewicki et al.'s (1988) study. Lewicki et al.'s subjects had to react to a target signal whose location was determined by complex sequential rules; according to the authors, the resulting pattern of response latencies indicated the building up of sophisticated processing algorithms that the subjects were unable to verbalize. Perruchet et al. (in press) observed that a by-product of the constituting rules was an alteration in the relative frequency of particular events in the whole sequence of trials, and they showed that the subjects' performance simply reflected this far more elementary informational content. One of the main pieces of knowledge that subjects really acquired was that the target tended to move through all its possible locations before returning to a previously occupied one. Furthermore, this knowledge could be identified in a subsequent explicit prediction task.

As a whole, our critical reanalyses of the works of A. Reber, P. Lewicki, and their co-workers show that what has been taken as evidence for unconscious abstraction of complex rules is merely the manifestation of simple pieces of knowledge available to subjects' awareness (Perruchet, 1988). Further work is required before this kind of conclusion can be extended to other experimental approaches, such as the one developed by the Oxford laboratory research group (e.g., Berry & Broadbent, 1988; Hayes & Broadbent, 1988; see also Stanley et al., 1989). Sanderson's (1989) recent study suggests that the dissociation between performance and conscious knowledge that this approach is intended to illustrate can be far less clear-cut than was previously acknowledged.

Declarative and Procedural Knowledge Toward an Integrative Framework

The issue of the relation between declarative and procedural forms of knowledge has generated considerable debate in comtemporary cognitive psychology. Some theorists, particularly J. R. Anderson (e.g., Lewis & Anderson, 1985), contend that procedural knowledge always proceeds from a declarative form of representation, whereas others (Broadbent, 1987; Reber & Allen, 1978) defend the opposite view. Similar divisions can be found in the field of developmental psychology, in the contrasts between Bruner's theoretical framework (e.g., Bruner, 1970) and that of Piaget (e.g., Piaget, 1974). In animal learning literature as well, declarative knowledge is claimed to anticipate (e.g., Dickinson, 1980), or, on the contrary, to originate from (e.g., Thompson et al., 1984) procedural knowledge. Some of these discrepancies may stem from terminological ambiguities or fuzziness. Nevertheless, the problem of the precedence of one or another form of knowledge is a crucial and unresolved issue.

This problem has obvious bearing on the main focus of this article. Our work shows that at least some of instances of procedural knowledge without concomitant declarative support turn out to be flawed by erroneous assumptions about the very nature of procedural knowledge. This outcome lends credence to the position that advocates the primacy of declarative over procedural knowledge; empirical data favoring the opposite point of view are linked to failure in assessing the actual data base knowledge on which behavioral modifications are grounded. This explanation fits a framework postulating the eventuality of qualitative discontinuities in the learning process. When faced with a complex situation, human subjects may initially engage in some simple and ubiquitous coping processes. These processes may be the ones underlying pairwise association and frequency learning. They would produce immediate improvement in performance, such as a decrease in the number of erroneous responses. However, the resulting adaptation would be essentially probabilistic in nature, in the sense that subjects would be unable to give the correct response or the best solution in all cases. For instance, in the context of artificial grammar, even perfect knowledge of bigrams did not allow subjects to classify any new letter strings correctly; likewise, the knowledge of frequency of particular sequences in Lewicki et al.'s (1988) paradigm does not exhaust the information content of the situation. Further improvements in performance are only possible through qualitative shifts in processing modes. These processing modes could be of various types, including hypothesis testing, logical inference, and all the controlled processes that subjects may use when engaged in explicit rule discovery. The fundamental error in the argument that procedural knowledge anticipates declarative knowledge may stem from the belief that behavioral modifications observed in the early stages of learning are underpinned by some component of knowledge that may be acquired only after extended training under specific conditions. A multilevel conception of learning, positing that roughly similar behavioral modifications may refer to successive, qualitatively different contents of knowledge, makes the current claim of a dissociation between declarative and procedural knowledge unwarranted. This speculative standpoint concerns only the relation observed during the acquisition phase, and it makes no prediction as to dissociations emerging during a later, automatization phase.

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