ARTIFICIAL GRAMMAR LEARNING AND THE TRANSFER OF SEQUENTIAL DEPENDENCIES

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By degrees I made a discovery of still greater moment. I found that these people possessed a method of communicating their experience and feelings to one another by articulate sounds. I perceived that the words they spoke sometimes produced pleasure or pain, smiles or sadness, in the minds and countenances of the hearers. This was indeed a godlike science, and I ardently desired to become acquainted with it. . .
. . . reading had puzzled me extremely at first; but, by degrees, I discovered that he uttered many of the same sounds when he read as when he talked. I conjectured, therefore, that he found on the paper signs for speech which he understood, and I ardently longed to comprehend these also; but how was that possible, when I did not even understand the sounds for which they stood as signs? p108/110

Mary Shelley (1818, 1992)

for Rachel

ABSTRACT

Exposure to sequences of elements constrained by an artificial grammar enables observers to classify new sequences as being either well- or ill-formed according to that grammar. Moreover, participants are also able to transfer their knowledge of the grammar to sequences composed of novel vocabulary elements. Two principle theories have been advanced to account for these effects. The first argues that participants learn grammatical rules that are abstract in the sense of being independent of vocabulary; even in a new vocabulary sequences can be classified on the basis of rule-adherence (e.g. A. S. Reber, 1989). The second argues that participants memorise the exemplars and subsequently classify new sequences on the basis of how similar they are to those exemplars (e.g. L. R. Brooks & J. R. Vokey, 1991). To assess the relative contributions of these two modes of representation this Thesis re-defines these theories in terms of the information that can be used to classify sequences in a novel vocabulary. The rule-based account of transfer is predicated on the abstraction of sequential dependencies between both repeating and non-repeating elements. These can be applied in a novel vocabulary by inducing the correspondences between vocabularies *across* sequences. The exemplar-based account of transfer is predicated on memory for dependencies between repeating elements alone. The similarity of new sequences can be determined on the basis of this form of dependency on a *sequence-by-sequence* basis. A review of the empirical literature suggests that both kinds of information can be transferred to a novel vocabulary. Seventeen experiments confirm this conclusion, but demonstrate that participants are not equally sensitive to the different forms of sequential dependence. Finally, the contributions of these two modes of classification can be dissociated by altering the frequency distribution of the training exemplars. These findings inform both theories and computational models of artificial grammar learning and general cognitive processes.

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AUTHORS DECLARATION

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Richard John Tunney

CHAPTER ONE

ABSTRACT KNOWLEDGE AND IMPLICIT COGNITION

1.1 INTRODUCTION

One of the most enduring questions in experimental psychology concerns the mechanisms underlying learning. Do we learn by remembering each new item of information or do we learn by inducing rules? It is the human ability to learn complex information and to then adapt and generalise it to entirely new situations that ensures our place in evolution. As Gentner and Medina (1998, p263) point out, "we routinely carry out feats of reasoning that are beyond the capabilities of other species." It is fitting then that our ability to transfer knowledge acquired in one situation to another that differs in perceptual form is often taken as strong evidence that the representation of knowledge is abstract and rule-like. Moreover it may be this mode of representation that allows us to learn and use the most unique of human faculties—language. This chapter reviews the evidence that the transfer of complex sequential knowledge, such as grammar from one vocabulary to another, is predicated on an abstract mode of representation.

One historical view, *The Doctrine of Formal Discipline* (e.g. Wallace, 1910), held that general reasoning abilities were predicated on abstract knowledge. Training in abstract reasoning would transfer to specific everyday tasks. For example, training in formal logic might transfer to skill in chess. Cheng, Holyoak, Nisbett and Oliver (1986) found this not to be the case. Participants trained in formal logic make the same errors on the Wason selection task (1961) that untrained participants do—they do not transfer their 'abstract' knowledge of formal logic to other forms of reasoning. Indeed De Groot (1965) famously found that skill in chess could be accounted for by distributed and specialised memory for examples of successful moves. In contrast, Thorndike (1931) in his theory of *Identical Elements* argued that knowledge could only generalise and transfer to novel situations when the two tasks shared common surface elements, as such his theory of transfer is not predicated on abstract knowledge. These early views presupposed that the ability to generalise and transfer were functions of general intelligence.[1] But consider problem solving. A good deal of research has considered whether knowing how to solve one problem enables us to solve another that is superficially different yet structurally similar. Gick and Holyoak (e.g. 1980; 1987) presented participants with Dunckers' Radiation problem (1945): Where a patient has an inoperable tumour that can be destroyed by radiation. Although weak radiation will not harm normal flesh, radiation strong enough to destroy the tumour will. Participants are asked how they would treat the patient. Typically only 10% of participants solve this problem spontaneously. The solution is of course to surround the tumour with a barrage of weak radiation that will converge on the tumour. However, if people are asked to memorise a superficially different problem and its solution, for example about a general's attempts to capture a castle, the success rate increases to 80% when participants are instructed to use the solution. Participants not instructed to use the solution of the memorised problem fail to complete the analogous problem. Keane (1987) demonstrated that if a third problem is used that is intermediate in similarity between the original (source) and target (novel) problem, then its solution is more likely to be used spontaneously than that of the more dissimilar problem. The point is that people can spontaneously draw an analogy between the two problems by mapping corresponding features between the two domains (for example between the surgeon and the general, and the tumour and the castle). As Thorndike (1931) recognised, this type of pure research is crucial to developing theories of epistemology and ultimately pedagogy.

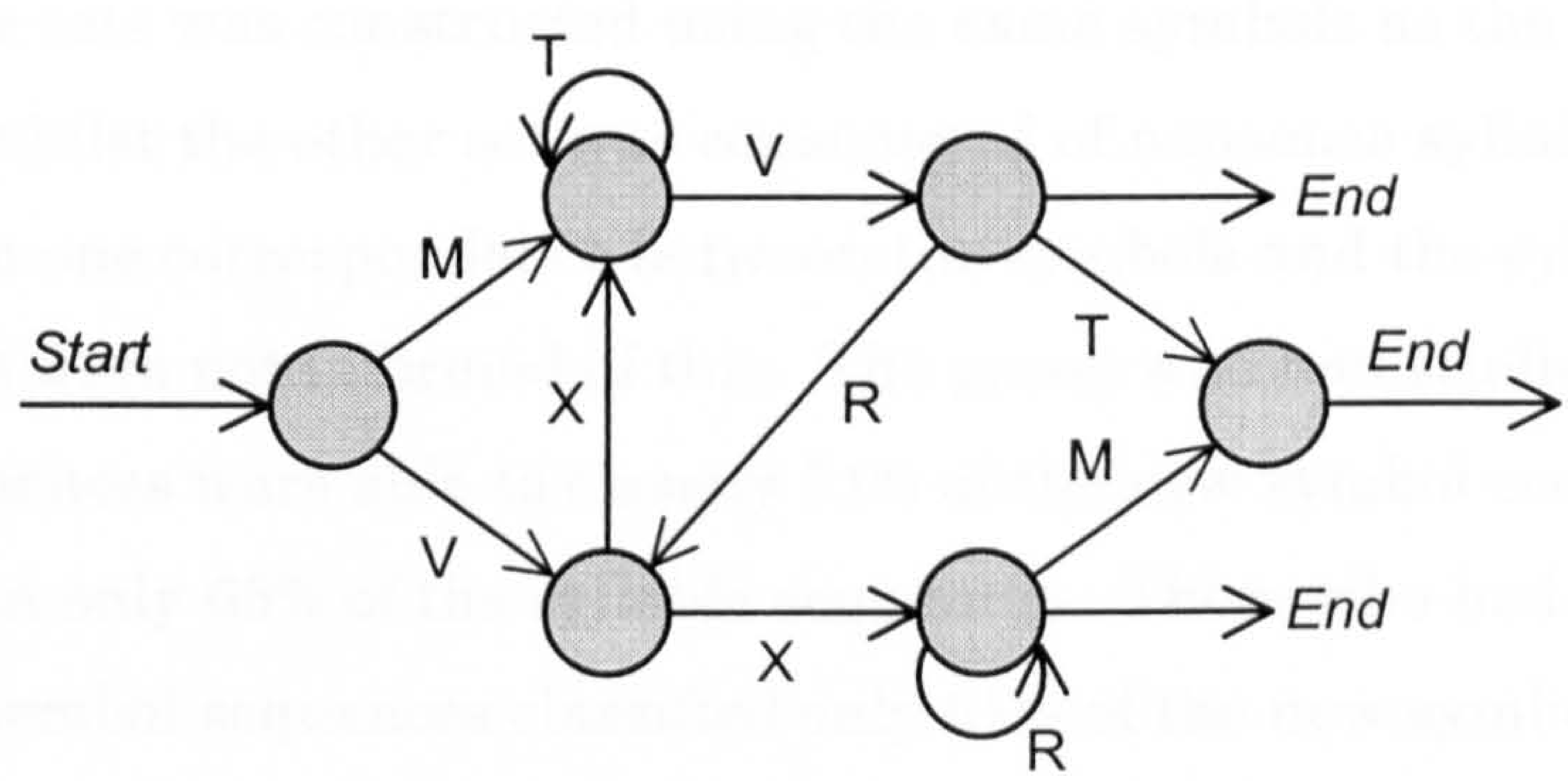
Consider the case of natural language. Knowledge of grammar must be abstract in the sense that it is independent of both vocabulary and modality (see Chomsky, 1980). For example, the same syntactic structure applies to arbitrarily different sentences that have entirely different words

[1] Indeed the WAIS-R retains a transfer task as a measure of speeded processing.

and meanings (e.g. *The boy kicked the ball* → *The scandal rocked the boat*). Moreover, even if the words are entirely novel (e.g. *dup pel soged dup jix rudly*) their function and even probable meaning can often be determined from their sequential location and their relationships with other words. Furthermore, grammatical knowledge is modality independent in that it applies equally to speech and to writing (even as in English where there is not a direct correspondence between syllable and grapheme). As Gentner (1989, see also Gentner & Medina, 1998; Gleitman, 1992; Gleitman & Gleitman, 1993) notes people do not just map isolated elements, they map the systematic structural relations between elements in each domain.

A good deal of research has investigated the extent to which the grammar of an artificial language is represented independently of the vocabulary elements of that language. For example, Reber (1969) found incremental learning savings when participants were required to memorise, over successive learning periods, sequences of letters generated by a finite-state grammar (see Figure 1.1). These learning savings were preserved when the participants were asked to memorise sequences composed of *different* letters generated by the same grammar, but were not preserved when the grammar was changed (this effect was replicated by Mathews, Buss, Stanley, Blanchard-Fields, Cho, & Druhan, 1989). In a second experiment, after the learning phase participants were informed that an unspecified set of rules generated the original sequences. Subsequently they were able to classify previously unseen sequences in both the same and a novel vocabulary, as being either well- or ill-formed according to the rules of the grammar. Remarkably, although participants might recall fragments of the exemplar sequences they are often unable to verbally justify their decisions. A number of workers have extended these findings in a number of ways. For example, Altmann, Dienes and Goode (1995) have shown that participants are able to transfer grammatical knowledge from sequences of spoken nonsense syllables to sequences of graphic symbols. Gomez and Gerken (1999) have demonstrated that even twelve-month old infants can learn sequences in one vocabulary (composed of spoken nonsense syllables) and later exhibit preference for that grammar despite a change in vocabulary. Knowlton and

Squire (1996) found that this ability to transfer grammatical knowledge to novel vocabularies is preserved in amnesia. Findings such as these have led Reber (e.g. 1993) and others to claim that the knowledge that allowed participants to classify sequences was *implicit* in the sense of being relatively inaccessible to introspection and verbal report, and was *abstract* in at least two senses. First, rather than representing the training exemplars themselves participants represent the rules used to generate them. These rules allow participants to generalise to new sequences. Second, because participants are able to transfer that knowledge to novel vocabularies these rules are abstract in the sense that they are independent of the vocabulary in which they are acquired.



Grammatical sequences are generated by traversing the grammar from left to right adding letters to the sequence, e.g. *Start M T V T End.*

Figure 1.1: Finite-state grammar used by Reber (1967)

These are clearly issues of phenomenology and representation, but what is the relationship between the two? Can simple memory for instances, or fragments of instances, account for participants' ability to classify previously unseen sequences in an entirely new vocabulary? Many workers have argued that in order to transfer knowledge participants must unconsciously (or consciously) learn something of the grammar itself (i.e. the sequential ordering of elements) and that this knowledge must be abstract in the sense of being independent of the original vocabulary (e.g. Manza & Reber, 1997). Alternatively, others have argued that this effect can be accounted for by either conscious or unconscious memory for training exemplars (e.g. Brooks & Vokey, 1991; Shanks & St. John, 1994; Whittlesea & Wright, 1997). Similar debates have surrounded the representation of undeniably conscious experiences such as mental imagery (e.g. Pylyshyn, 1981; Kosslyn, 1981).

A key to how knowledge of artificial languages is represented lies in the finding that participants are generally less able to classify sequences in a novel vocabulary than they are in the same vocabulary as the training exemplars. For example Altmann *et al.* (1995) reported an experiment where participants were asked to study sequences of symbols that were either constrained by a grammar or scrambled. Later they were asked to decide whether two new sets of sequences belonged to the same grammar or not. One of these sets was constructed using the same symbols as the training exemplars whilst the other set was constructed of nonsense syllables. There was a one-to-one correspondence between the symbols and the syllables, but participants were not informed of this. The group who had studied exemplar symbol sequences were able to classify 71% of the new symbol sequences correctly, but only 65% of the syllable sequences. Those who had studied scrambled symbol sequences classified only 51% of the new symbol sequences correctly and 49% of the syllable sequences correctly. If we take the control participants ability to classify sequences as the minimum possible (51% in the source vocabulary and 49% in the novel vocabulary), and trained participants' classification performance as the maximum possible, then the classification of sequences in the novel vocabulary was approximately 76% of that in the source vocabulary — a 24% cost of changing vocabularies. Participants are also able to transfer knowledge across both vocabularies and modalities. For example, in their Experiment 3 Altmann *et al.* asked participants to listen to spoken versions of the syllable sequences used in their Experiment 4, and to later classify sequences of symbols. These participants were able to classify 58% of the graphic symbols correctly, relative to untrained controls who classified only 47% correctly. Clearly in both of these studies participants must have learned (implicitly or explicitly) that each syllable *named* each symbol. Similarly, in their Experiment 1 Altmann *et al.* contrasted cross-modal (letters to tones, or tones to letters) and within-modal (tones to tones, or letters to letters) classification. In the within modal condition participants were able to classify 58% of the sequences correctly, whilst in the cross-modal condition they classified only 55% of the sequences correctly. Relative to control performance (59%) cross-modal performance was only 66% of that seen in the within-modal condition. Such cross-modal transfer effects are in some respects to be expected; after all, in natural language we apply (roughly) the same grammar to production, comprehension, reading and writing. However, these findings are indicative that participants are unable to transfer all of the knowledge acquired in one vocabulary or modality to another. It is not clear whether this reflects an inability to induce some of the correspondences between vocabulary elements in the source and novel domains, or whether it is a consequence of some knowledge being tied to the specific perceptual features of the vocabulary in which it was acquired.

Dienes and Altmann (1997) questioned whether this was due to at least some knowledge being tied to the vocabulary in which it was acquired. To test this idea Dienes and Altmann (Experiment 1) made the mapping between the two domains transparent. Because they used colours for one vocabulary and colour names for another the correspondence between elements in the two vocabularies was obvious. However, the results replicated the usual transfer deficit — performance in the novel vocabulary was only 76% of (or 24% less than) that seen in the source vocabulary. This difference could not have been due to problems in computing a mapping between the two domains because it was transparent. In their Experiment 2, Dienes and Altmann observed that there was a cost associated with transferring explicit but not subjectively implicit knowledge. This finding is in contrast to the characteristics of implicit memory that seems to be largely domain specific (Berry, Banbury & Henry, 1997). These studies indicate that at least some knowledge is abstract enough to be applied in a novel vocabulary or even modality, but the failure to apply all of the knowledge that is available in the source vocabulary suggests that at least some of that knowledge is not abstract. But what information could participants learn from exemplar sequences that enables them to correctly classify previously unseen sequences, in either the same or a different vocabulary, as being well- or ill-formed according to that grammar. The following section discusses a number of ways in which knowledge of an artificial language could be described as abstract.

1.2 THE REPRESENTATION OF KNOWLEDGE

Whilst the distinction between abstract and episodic modes of representation is less controversial than the implicit-explicit distinction it is perhaps the more difficult of the two to investigate. The criteria to demarcate abstract knowledge from non-abstract knowledge are less well defined and have received little in the way of either theoretical discussion or empirical enquiry. This section begins with an exploration of the kinds of information encoded by a grammar that participants could learn from exemplar sequences. Subsequent sections discuss different notions of how participants might represent an artificial language and how these correspond to different forms of abstract knowledge.

1.2.1 On the nature of grammatical dependence

If participants do learn something of the rules that generate an artificial language what might they be like? All grammars, whether artificial or natural, can be construed as knowledge of the constraints upon the sequential ordering of the vocabulary elements of which the language is composed. These constraints can be partitioned into different kinds: for instance, between the sequential dependencies of *repeating* elements and of *non-repeating* elements. In the first case, where one pattern of elements occurs, the grammar determines the occurrence later in the sequence of another identical pattern. In the second case, where one element or pattern of elements occurs, the grammar determines the occurrence of a different element or pattern of elements elsewhere in the sequence. Sensitivity to the first kind of sequential dependency does not imply sensitivity to the second. Consider the artificial language shown in Figures 1.2. and 1.3 that was used by Altmann *et al.* in their Experiments 3 and 4.

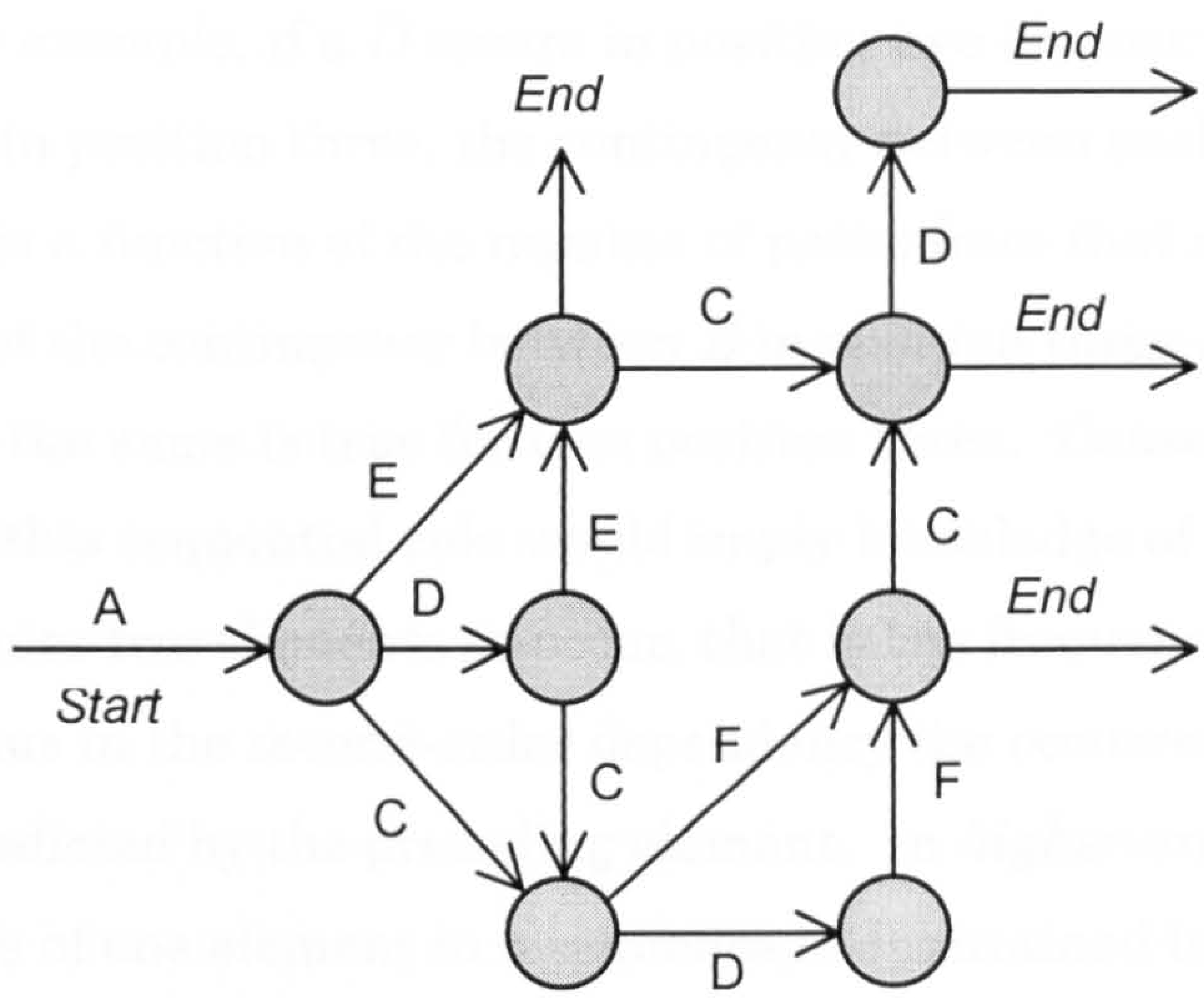
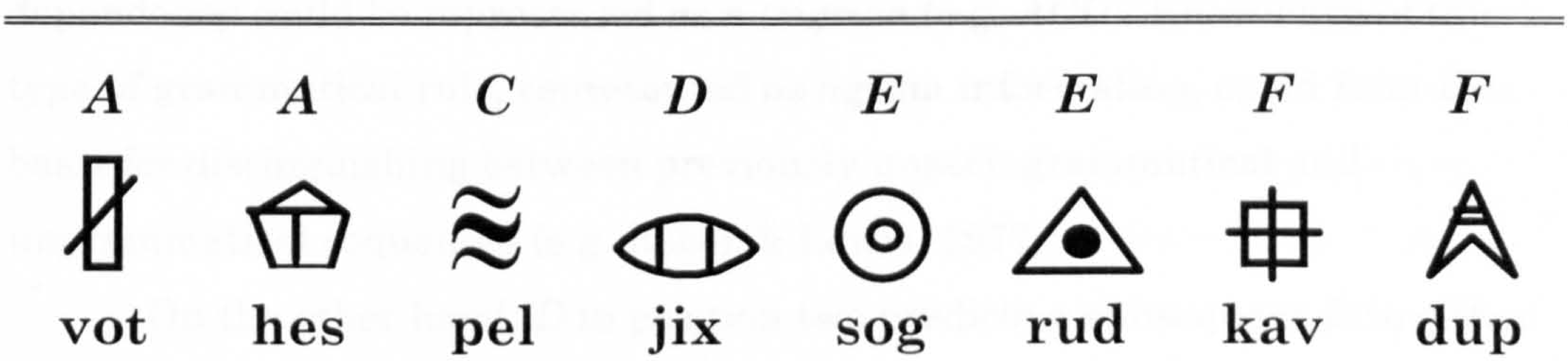


Figure 1.2: Finite-state grammar used by Altmann *et al.* (1995)



Note that there are two elements that correspond to *A*, *E*, and *F*. This serves to increase the number of unique sequences that the grammar can generate.

Figure 1.3: The vocabularies used by Altmann *et al.* (1995)

Grammatical sequences are generated by traversing the paths of the grammar adding vocabulary elements to the sequence. The occurrence of any given vocabulary element is clearly dependent upon the occurrence of a preceding element. A *first-order dependency* refers to the frequency of occurrence of individual elements in isolation. For example the grammar in Figure 1.2 has the constraint that only *A* (*hes* or *vot*) may begin a sequence, and that they may not occur elsewhere in a sequence. Because there are only two elements in this location, they must be equifrequent across all the sequences that the grammar can generate. In contrast a *second-order dependency* refers to the contingency between two adjacent vocabulary elements. For example, if a *D* occurs in position two it must be followed by either *E* or *C* in position three, the contingency between each element and the previous one is a function of the number of paths from that node. In this case the strength of the contingency between *E* in position three and *D* in position two is .5, and the same is true for *C* in position three. Consequently, knowledge of this sequential rule would imply knowledge of the frequency with which these two elements co-occur, that is the frequency of the *bigrams DE* or *DC*. Thus in the second-order dependency the occurrence of one element is predicted by the preceding element. In *higher-order dependencies* the occurrence of one element in a sequence is determined by two or more preceding elements. Consider the lower path in Figure 1.2. If a *D* is to occur in position three it is dependent upon the occurrence of *A* in position one and *C* in position two. As in the second-order dependency, the higher-order dependency could be represented as a *trigram* (e.g. *ACD*). Knowledge of this type of grammatical rule, represented as *n*gram information, could form the basis for distinguishing between previously unseen grammatical and ungrammatical sequences (e.g. Reber & Lewis, 1977).

On the other hand, *D* in position two predicts a subsequent *D* in either position five (if the sequence is more than five elements long), position six, or position seven. In these cases, the occurrence of one element determines the occurrence of an *identical* element elsewhere. The relevance of the dependency between *identical* elements is that this 'repetition structure' could also form the basis for distinguishing between sequences generated by the grammar (which obey the pattern), and sequences generated at random, which may not obey the pattern (e.g. Whittlesea & Dorken, 1993).

The importance of distinguishing between these two forms of sequential dependency lies in the way that they can be applied in a novel vocabulary. Brooks and Vokey (1991, see also Whittlesea & Dorken, 1993), for example, pointed out that there is a certain similarity between sequences such as *ADCDF* and sequences such as *PQLQR* that is not shared with sequences such as *HDUWD* — the former can be characterised as sharing the repetition pattern _ X _ X _, which is distinct from the pattern associated with the third sequence _ X _ _ X. If none of the training exemplars conformed to this last pattern, it could be classified as 'ungrammatical' on the basis of this difference. The task of inducing a mapping between a pattern of repeating elements in one vocabulary and another is relatively trivial. As the above example illustrates, the well-formedness of any sequence that contains repeating elements can be determined on the basis of whether that pattern had been previously seen in a training sequence. The classification of sequences according to this form of grammatical dependency could proceed on a *sequence-by-sequence* basis and need not be based on knowledge induced *across* the test sequences. Contrast this with the considerable problem of transferring a representation of sequential dependencies between several elements that do not repeat within any sequence onto their counterparts in another vocabulary.

The induction of a mapping between non-repeating elements across vocabularies, and subsequent classification of sequences containing them, can only be accomplished on the basis of knowledge induced *across* whole training and test sets and not on a sequence-by-sequence basis. For example, if one *n*gram (*MTV*) is highly frequent and another highly infrequent (*PQR*) in the training set, and in the test set (presented in another vocabulary), one *n*gram[2] is highly frequent (*XYZ*) and another highly infrequent (*ABC*), then the two pairs can be mapped onto each other on the basis of their statistical distributions across the training and test sets. Alternatively, the system could attempt to compute all possible mappings that allowed each successive sequence at test to be classified as grammatical. Thus, it might compute all possible mappings that enabled one sequence to be classed as grammatical,

[2] *n*grams represent fragments of sequences of length *n*.

and then compute which of these enabled another sequence to be classed as grammatical, and so on. Of course, such a mechanism would seem better adapted to situations that did not include ungrammatical stimuli. Nonetheless, in this case also, a mapping could only be effected by computing *across* test sequences. In both these cases knowledge of grammatical sequences can be abstract in the sense of being transferable to sequences that differ in perceptual form.

But can we be sure that when participants classify a sequence in a novel vocabulary they are transferring second- or higher-order dependencies at all? Might not a feature frequency account, based on the transfer of first-order dependency information be more parsimonious, albeit less interesting? This seems intuitively plausible if, as claimed, the correspondences between vocabularies are mapped on the basis of the frequency with which individual elements occur, and co-occur with other elements. For example, if participants are asked to memorise a set of sequences that all begin with *MT* and are subsequently asked to decide which of the three sequences *MTVX*, *MVTX*, and *MTXV* conform to those exemplars, they ought to endorse the first and last sequences, but reject the second. We might conclude that participants had learned that *T* was sequentially dependent upon *M* (a second-order dependency). Alternatively we could conclude that the sequence *MVTX* had been rejected because participants had never seen a *V* in second position in the exemplar sequences (an illegal first-order dependency). Now if that classification test had been in a different vocabulary than the exemplars, *AEOU AOEU* and *AEUO*, we could draw two analogous conclusions. Either participants might reject the second sequence because the second-order dependency *MT* could be mapped onto the sequences 1 and 3, but not onto sequence 2. Or more parsimoniously we could conclude that participants had rejected sequence 2 because *O* occurs only infrequently in that position whereas *T* had occurred in the second position of every exemplar. That is, knowledge of a first-order and not a second-order dependency had been transferred. Although knowledge of a first-order dependency is in itself abstract in the sense that it can be mapped onto elements in a novel vocabulary, it does not reflect sequential knowledge.

This section has described the kinds of information encoded in an artificial language and distinguished between different forms of sequential dependency that could form the basis for discriminating between grammatical and ungrammatical sequences in a novel vocabulary. The following section discusses what it means for representation to be 'abstract' in terms of how it might differ from episodic knowledge or memory for instances.

1.2.2 Abstract knowledge of artificial languages

Reber (1969) originally argued that knowledge of an artificial grammar was abstract in two ways. First, Reber argued that participants automatically learn the rules underlying a stimulus array—in the case of artificial languages participants might learn grammatical rules. This issue is explored by determining what knowledge participants can apply to previously unseen sequences in the same vocabulary as learning. Second, Reber argued that knowledge of an artificial language was abstract in the sense that it could be applied in situations that are considerably different in form—in particular that grammatical information learned in one vocabulary could be applied in another. The extent to which knowledge of an artificial language is abstract in this sense is given by the drop in performance between classification in the same vocabulary as the training exemplars and classification in a novel vocabulary. This section discusses these claims in turn.

Abstract knowledge is knowledge of rules

One way that knowledge can be described as abstract is that a stimulus array is summarised as a set of rule-like representations. In the case of artificial grammar learning the rules used to generate the sequences could be *abstracted* from the training exemplars. This is a simple concept learning idea — rather than remembering all the squares that we had ever seen we simply use a formula such as 'all squares have four 90° angles.' The point is that *the sum of the information that is represented about the training exemplars is less than the sum of the information contained in the training* *exemplars but applies equally to any other category members*. The second important feature of this definition of 'abstract' is that it occurs at the point of acquisition. Of course this definition does not imply that knowledge of rules is abstract in the sense of being domain-general knowledge; this issue is discussed in the next section. Smith, Langston and Nisbett (1992) argued that behaviour is rule-based if no difference is observed in behaviour towards previously seen and previously unseen category members. In contrast, episodic-based processing is reflected by graded responses to old and new category members (e.g. decision time might reflect similarity). A satisfactory rule is one that correlates with the objective rules used to generate the stimuli enough to permit new sequences to be either fluently processed or correctly classified. There are many rules to a grammar, and each one may only apply to a subset of sequences. In addition each rule may only apply to portions of a sequence and differ in complexity. The issue here is that we should not expect (as Reber did) participants to learn veridical, that is perfect rules, but they must be generative.

Dulany, Carlson and Dewey (1984) investigated whether the information that participants were able to apply in the same vocabulary as training exemplars consisted of rule-like knowledge. They presented participants with exemplar sequences to be memorised. Later they were presented with new sequences (in the same vocabulary) to be classified as either well- or ill-formed according to the training exemplars. In addition participants were asked to underline the part of each test sequence that made them grammatical and to cross the part of sequences that made them ungrammatical. For example, given the sequence *MTRTV* it could be classified ungrammatical and crossed like so *M~~TR~~TV*. Such responses were formulated as decision rules in the sense that they contained a *subject* (one or more vocabulary elements) and a *predicate* (grammatical status). Such rules (e.g. *if TR* occurs in positions 2 and 3 *then* respond 'ungrammatical') accurately predicted participants' classification performance without significant residual. Dulany *et al.* demonstrated that participants abstracted fragments of exemplars that correlated with the finite-state rules of the grammar. Could this kind of information be described as rule-like? If the information that participants learn from exemplars is to be described as rule-like it must encode something of the sequentiality of the grammar whilst containing less information than was present in the exemplars.

Do these fragments encode sequentiality? The average size of each rule (i.e. how many vocabulary elements it encompassed) increased with the length of each sequence. Moreover the best fit for the rules occurred when they were considered location specific (i.e. when they were applied in the same position in a sequence that participants had reported them). If a rule contains two or more elements that are location specific they are by definition sequential, albeit fragmentary and correlated.

Do these fragments represent abstractions? On average each reported rule could be equally applied to four test sequences. Each rule applied equally to the exemplar sequences seen during training and generalised to the previously unseen sequences at test. That is each fragment, and the set of fragments as a whole 'summarises information across a series of learning episodes'. One difference between this view and Reber's position is that these types of rules are informal and fragmentary — they do not necessarily correspond to the finite-state rules that generate and describe the sequences. This in itself is not a problem for the abstractionist view because few would argue that participants abstract a veridical representation of a finite-state grammar or any other formal concept (see Miller & Chomsky, 1963). What is important here is each fragment encompassed more than one vocabulary element, and allowed the correct classification of more than one sequence, thus correlating with the finite-state grammar. As for the issue of awareness: This on-line direct measure is exhaustive. Consequently participants clearly had a high degree of explicit knowledge concerning the legal positions of vocabulary elements and *n*grams. Although knowledge of rules can be elicited on a direct test (and a substantial amount *is* available to free report), participants may still be responding below a subjective threshold of awareness and believe that they are guessing (Dienes & Berry, 1997). The information that participants learn from a series of training exemplars can be described as abstract in the sense that it contains less information that the exemplars themselves, but be used to determine the category membership of previously unseen sequences in the same vocabulary as those exemplars. The primary difference between this view and Reber's is that the rules are correlated with the grammar and are *not* necessarily veridical. The following section questions the extent to which summary information can be applied to vocabularies.

On narrow vs. broad abstractions

The preceding section suggested that knowledge of an artificial language could be abstract in the sense that it represented a summary of the information contained in a series of exemplars of a category. Reber's second definition of abstract knowledge is that it is domain-independent.

> "Abstract codes contain little, if any, information pertaining to the specific stimulus features from which they were derived; the emphasis is on structural relationships among stimuli."
>
> (Reber, 1993, p121).

This is a very strong claim. If people do not represent the perceptual features of a stimulus array what do they represent? A representation is an internal state whose function it is to covary with some external stimulus or its properties (Dienes & Perner, 1996, 1999; Dretske, 1988). Thus a representation can be the consequence of encoding a particular stimulus, in some veridical way. For example the participants might learn that the letter *M* can occur in a sequence. The subsequent representation *'M'* covaries with *M* as it is seen in that language, including its distributional properties. Alternatively a representation could encode a less tangible property of the stimulus rather than the perceptual identity (*M*) of the stimulus, for example the representation *'X'* could covary with a pattern that *M* forms in the stimulus, for example whether it repeats or not. This latter representation could covary with other elements even in a different vocabulary so long as the new elements share that property (i.e. they might also repeat in the same way) whereas the former representation would not.

Shanks (1995) makes an appropriate distinction between *narrow* and *broad* abstractions (where an abstraction is a rule in the sense described above). A narrow abstraction is one that can only apply to novel stimuli that are composed of the same elements as the training exemplars. In contrast a broad abstraction is one that applies to novel stimuli that might be composed of different elements than the training exemplars.

Let us consider the distinction between narrow vs. broad abstractions in terms of the decision rules that Dulany *et al.* (1984) devised to predict participants' classification performance in their experiment and the range of information that a grammar encodes. Unfortunately Dulany *et al.* did not determine whether these decision rules could be applied to sequences composed of different vocabulary elements. But we can ask what properties those abstractions might possess that would allow them to be transferred to a novel vocabulary.

The question concerns what information is encoded as the condition part of each decision rule, and whether that information is preserved in a novel vocabulary. A narrow abstraction leading to a decision rule might resemble a statement such as '*if* the sequence begins *MTV then* it is grammatical' (and *MTV* co-varies with *MTV* and nothing else). This narrow abstraction in itself does not support the transfer of the knowledge that it encodes. Such a representation would not allow fluent processing or classification of stimuli in a novel vocabulary without an additional mapping process, for example inducing that *M* in the source vocabulary corresponds to *Q* in the new vocabulary. This would require participants to encode the distributional properties of elements in both the training exemplars in the source vocabulary and test sequences in the novel vocabulary. In contrast, if the condition is predicated upon some property of the grammar that is readily apparent even in a novel vocabulary no further process of induction would be required to determine the correspondences between vocabularies. Such a representation would allow the fluent processing and classification of sequences in a novel vocabulary. One such form of information encoded by a grammar is the sequential dependence of identical elements. The correspondence between a repeating element in one vocabulary and another is readily apparent solely on the basis that they share a property that is independent of perceptual form—they repeat. The transfer of this form of information could proceed on a sequence-by-sequence basis and would not, necessarily require participants to resort to distributional information.

So both narrow and broad forms of abstraction can support the transfer of information from one vocabulary to another. The information encoded within a narrow abstraction may only be applied to a novel vocabulary by inducing *across* sequences the correspondences between elements in both the training exemplars and the test sequences. In contrast the information encoded within a broad abstraction can be applied on a *sequence-by-sequence* basis because it is readily apparent even in a novel vocabulary and requires no addition process of induction. The differences between the broad and narrow forms of abstraction appear to correspond to the distinction between sequential dependencies between repeating and non-repeating elements. So how knowledge is *applied* in a novel vocabulary provides a useful criterion for determining whether the information that participants learn from a series of training exemplars is abstract in the sense of being independent of vocabulary.

In sum rules as abstractions (summary information) may be either narrow or broad. This section has considered the decision rules that might be used to classify a sequence as grammatical or not. Narrow abstractions require the condition part of each decision rule to be mapped onto new surface features in order to be transferred. Since the new surface features are unknown until presentation they cannot be mapped until testing begins. In contrast, in broad abstraction new sequences should be automatically and fluently processed irrespective of their perceptual form. So whether knowledge is applied on a *sequence-by-sequence* basis or applied *across* sequences is indicative of whether the knowledge represents a broad or a narrow form of abstraction.

Summary

Knowledge can be abstract in the sense that it is represented as a set of rules learned from exemplars of a category that can later be used to decide whether new items belong to the same category or not. Such decision rules can encode the abstract sequential dependencies between vocabulary elements, that is they can be predicated on the sequential rules of the grammar. Furthermore, those sequential rules can be either narrow or broad. In the narrow abstraction the sequential rules are tied to the specific perceptual features of the source vocabulary, but could be transferred to a novel vocabulary if participants also encode the distributional properties of elements in both the source and the novel vocabularies. The representation of this form of knowledge could be captured in a series of fragmentary rules abstracted from training exemplars. In contrast, the broad abstraction could encode other properties of those component elements such as repetition. Decision rules based on broad abstractions would transfer readily to novel domains or vocabularies that share the same properties but differ in perceptual form.

However, rather than being predicated upon rule abstraction grammatical information could, in principle, be represented entirely in episodic form. Episodic accounts of artificial grammar learning are in some respects functionally equivalent to the abstractionist account described above in the sense that they also support the correct classification of previously unseen sequences that either share the same or do not share the perceptual form (vocabulary) of the training exemplars. The first of these accounts, the fragmentary account, is not entirely incompatible with the rule-based account outlined above. The exemplar-based account is predicated upon a fundamentally different representation of grammar, but shares the notion of broad and narrow forms of abstract knowledge. This exemplar-based account represents a strong contender for an alternative to the abstractionist and fragmentary theories of artificial grammar learning.

1.2.3 Episodic memory for fragments of exemplars

One influential theory of artificial grammar learning does not assume that participants encode the rules used to generate sequences. Recall that Dulany *et al.* (1984) demonstrated that participants could identify the individual parts of sequences that made them either grammatical or ungrammatical. Dulany *et al.* interpreted these responses as fragmentary rules that correlated with the grammatical rules. Perruchet and Pacteau (1990) provided an alternative interpretation. They suggested that participants remembered (that is, stored episodically) isolated fragments (elements that tended to co-occur) of exemplars. When presented with new sequences, participants consider those that contain fragments that have been seen before as grammatical, and those that contain new fragments as ungrammatical. Importantly, this model considers the location of those fragments in a sequence, with the exception of initial and terminal fragments, as irrelevant. Moreover whilst such knowledge is abstract in the sense that less information is represented than was present in the training exemplars, it is entirely dependent on the vocabulary in which it was acquired, and according to Perruchet and Gallego (1997; see also Perruchet & Vintner, 1998) cannot be described as an encoding of sequential rules. This account has become very influential in conceptualising the bigram as *the* unit of knowledge acquired in artificial grammar learning. In the form described by Perruchet and Pacteau (1990) simple episodic memory for fragments does not support transfer (Perruchet, 1994), However, Redington and Chater (1996) describe a mechanism that allows memory for fragments to be mapped onto *n*gram information in a novel vocabulary irrespective of location.

The weakness of this model is its claim that participants do not encode the position of the fragments of training sequences, with the exception of initial and terminal *n*grams. This issue has received substantial amount of empirical enquiry and provides a means to distinguish between memory for fragments and rule-abstraction. This section will begin by presenting evidence to suggest that participants do in fact encode positional information, and argue that memory for fragments is concomitant with (narrow) rule abstraction. Subsequently this section reviews some evidence that the information encoded within such fragments can be transferred to a novel vocabulary by mapping *n*gram information abstracted from training exemplars on *n*gram information in a novel vocabulary.

Perruchet and Pacteau (1990) demonstrated that memory for bigrams is sufficient to account for classification performance in the same vocabulary as exemplars. In their Experiment 1, Perruchet and Pacteau (1990) asked participants to study either bigrams or whole sequences, later both groups classified whole sequences in the same vocabulary. The two groups differed only in that the groups trained on whole sequences were able to utilise knowledge of initial and terminal bigrams. When sequences that contained illegal initial and terminal bigrams were removed from the analysis the groups did not differ in their classification performance.

In order to demonstrate that participants do not encode positional information (apart from initial and terminal bigrams) Perruchet and Pacteau (Experiment 2) trained participants on either whole sequences or scrambled versions of the same sequences that were composed of legal bigrams in positions not permitted by the grammar. Later participants were presented with a set of grammatical sequences, and two sets of ungrammatical sequences. The first contained one illegal bigram and the second contained one misplaced legal bigram. Overall trained participants were extremely sensitive to sequences that contained an illegal bigram, but were only marginally more sensitive to sequences that contained a legal bigram in an illegal location than controls. Finally, participants were trained on the whole sequences used in their Experiment 1. The test phase consisted of a direct recognition-rating test for legal and illegal bigrams (participants were asked to rate how sure they were for each decision). There was a significant correlation between the recognition scores and the frequency of occurrence of those bigrams in the exemplar sequences (i.e. *chunk strength*). These ratings accurately predicted the classification performance observed in their first experiment. On this basis Perruchet and Pacteau argued that participants do not encode positional information.

But is there any evidence that participants do encode positional information? The difference between characterising fragmentary knowledge (i.e. *n*grams) as either rules or episodes is not in whether they form decision rules, both can; but in whether they encode the sequentiality of the grammar including positional information. Consider the bigram *AB*, clearly it contains information about sequential dependence because *A* is followed by *B*, rather than the *A* following *B*. Thus even episodic knowledge of fragments is an encoding of sequential dependence and so we might expect participants to know what elements might follow others in at least the same vocabulary as the training exemplars. Dienes, Broadbent and Berry (1991) contrasted classification performance with both verbal report and a letter continuation task. The letter continuation task was considered an exhaustive test of the sequential knowledge that people might use to classify sequences, if participants know some rules of the grammar or remember fragments then they should be able to predict what letters can follow others. After training participants were presented with fragments of sequences (e.g. *MTV...?*) and asked what letters could follow. The letter continuation task correlated with classification performance to a greater extent than did verbal report, but could not entirely account for indirect classification performance suggesting some residual knowledge that was not revealed. Reber and Lewis (1977) reported a similar experiment in which participants were first asked to study exemplars and then to reorder scrambled sequences. In reordering the scrambled sequences participants were extremely sensitive to the positions where individual letters could legitimately occur, correctly placing 93% of letters in grammatical positions (averaged over the four days). There was a linear relationship between position order and sensitivity, with greatest sensitivity for the initial position (.97 correct) and least sensitivity for the last position (.80). In addition participants were also sensitive to which elements co-occurred as legitimate bigrams (.76). Again sensitivity was greatest for the initial bigram (.82) and least for the terminal bigram (.70). However, knowledge of bigrams irrespective of position was greater than knowledge of bigrams dependent upon position. Finally participants were also sensitive to trigram information (.60) again with a linear relationship with position (.64 initial and .60 terminal).

These studies indicate that in at least the source vocabulary participants are able to apply information that includes sequential dependencies between vocabulary elements and are sensitive to where and how often they might occur. To what extent are participants sensitive to this kind of information in a novel vocabulary?

Fragment based transfer

Redington and Chater (1996) describe a mechanism that allows simple memory for *n*gram fragments to be mapped onto a novel vocabulary. Their mechanism learns permissible fragments from exemplar sequences. Consistent with Perruchet and Pacteau's (1990) theory the models classify new sequences in the source vocabulary as grammatical if they contain old fragments and ungrammatical if it considers new fragments. In order to classify sequences in a novel vocabulary the models must map fragments from the source vocabulary onto fragments in the novel vocabulary by a process of induction. The mapping of vocabulary elements must be consistent within each sequence (i.e. contain no novel fragments) otherwise that sequence would be rejected. The mapping must then be consistent across the proportion of the test set that is grammatical (i.e. it must be applicable in usually 50% of all sequences). For example, if every exemplar begins with *MS, MV,* or *VX,* and at test participants see sequences such as *JDHBHF, BFHHHH,* and *JBHFJ,* participants can induce that if J begins a sequence it can be followed by one of two letters. Hence *J* in the novel vocabulary corresponds to *M* in the source vocabulary, and *B* corresponds to *V* as it can begin a sequence and follow *M.* Similarly *F* must correspond to *X* because it is the only element that can follow *V,* and so on. The potential of this mechanism to simulate empirical data is discussed further in Section 1.3.1. The issue here is whether there is any evidence that participants can transfer *n*gram information to a novel vocabulary. The criteria for the application of *n*gram information in a novel vocabulary are essentially the same as in the source vocabulary. Participants must be sensitive to the position of *n*grams and frequency based information. Such information could be utilised to map a narrow abstraction acquired in one vocabulary onto *n*gram information in another.

In order to determine whether participants were sensitive to positional information in a novel vocabulary Gomez and Schvaneveldt (1994) replicated the procedure used in Perruchet and Pacteau's Experiment 1. Training exemplars consisted of either whole sequences or legal bigrams. The test set contained new grammatical sequences, ungrammatical sequences that contained an illegal bigram, and ungrammatical sequences that contained a misplaced legal bigram. Half the participants classified sequences in a different vocabulary than the training exemplars, whilst the remainder classified sequences in the same vocabulary. As in the Perruchet and Pacteau study participants trained on bigrams were marginally sensitive to sequences that contained illegal bigrams but tended to endorse sequences that contained misplaced legal bigrams as grammatical. In the novel vocabulary these participants were unable to correctly classify any of the three types of test sequence. In contrast, participants trained on whole sequences were able to reject *both* types of ungrammatical sequence in *both* vocabularies, although in the novel vocabulary sensitivity was greater for sequences containing illegal bigrams than for those that contained misplaced bigrams. Clearly participants are more sensitive to illegal bigrams than they are to misplaced legal bigrams, but participants *are* sensitive to positional information even in a novel vocabulary. Of course, the assumption that participants encode bigrams rather than fragments of different lengths is arbitrary. In their Experiment 4, Gomez and Schvaneveldt (1994) asked participants to study exemplar sequences, bigrams, or trigrams. Again there were two types of ungrammatical sequence, those containing an illegal bigram, and those containing a misplaced legal bigram. These were presented in either the same or a novel vocabulary. Participants trained on whole sequences were able to correctly classify all three types of sequence in both vocabularies and so must have encoded and transferred positional information. Participants trained on trigrams were only sensitive to sequences containing illegal bigrams, but only in the same vocabulary as training. In contrast participants trained on bigrams were not sensitive to any type of sequence in either the same or new vocabulary. See Manza and Reber (1997) for a similar experiment. So contrary to the fragmentary accounts described by Perruchet and Pacteau (1990) and Redington and Chater (1996) participants do encode positional information, and can transfer this information to a novel vocabulary. These are important findings because they suggest that knowledge of fragments is concomitant with the abstraction of the kinds of grammatical information discussed earlier, rather than consisting of simple episodic fragments.

To what extent can we regard this kind of *n*gram knowledge as broad or narrow abstractions? A broad abstraction, unlike the narrow form of abstraction, requires no additional mapping process in order to be applied in a novel vocabulary. In principle the Serial Reaction Time (SRT) paradigm (Nissen & Bullemer, 1987), could provide compelling evidence that sequential dependencies are a broad form of abstraction because the task does not permit a mapping process at test. In a typical SRT task a light appears in one of four (or more) locations along a horizontal axis on a computer screen. Participants are required to press a corresponding key on a keyboard as fast as possible. Unbeknownst to the participants the location of each light is determined by a set of rules. Typically reaction time to each stimulus presentation decreases over subsequent presentations of the sequence — participants learn to anticipate the location of each light — relative to participants trained on random sequences who do not. For example, Cleeremans and McClelland (1991) trained participants on an artificial grammar using the SRT paradigm, however, on 15% of trials the sequence was ungrammatical (random). With extensive training participants enjoyed significant reaction time savings on grammatical but not ungrammatical (random) trials. Subsequent analyses indicated that participants became incrementally sensitive to higher order dependencies (e.g. *T* can follow the fragment *PTV*?) between three consecutive elements. Verbal reports revealed substantial knowledge of salient dependencies such as adjacent repeating elements, but did not reveal knowledge of less salient dependencies even though participants had exhibited reaction time saving on them. Generally participants believed that the sequences were random. But clearly, participants learn about the sequential dependencies between vocabulary elements generated by a finite-state grammar. In principle, this paradigm could provide compelling evidence that *n*gram information is represented as a broad form of abstraction if these learning savings are preserved when the vocabulary is changed. To test this idea, Gomez (1997, Experiment 2) presented sequences of letters one at a time in a single location. As in the

Cleeremans and McClelland studies these sequences were generated by a finite-state grammar. During training each letter of a sequence appeared on screen and participants were required to press the corresponding key on a keyboard before the subsequent letter was presented (controls proceeded directly to the test phase). There was a direct and an indirect test. The indirect test consisted of one third grammatical sequences, one third ungrammatical sequences that contained an illegal second-order dependency (bigram) and one third that contained an illegal higher-order dependency (trigram). The dependent variable was the mean reaction time for each type of transition. Participants were presented with sequences in the either the same or a different vocabulary. The letters-same group did not differ from untrained controls in their sensitivity to illegal second-order dependencies but were significantly more sensitive to illegal higher-order dependencies. Gomez suggested the untrained controls had learned something of second-order dependencies early in the test phase because the mean reaction time for grammatical sequences was faster than for sequences that contained illegal bigrams, but not for illegal trigrams. The transfer group did not differ from untrained controls on either second- or higher-order dependencies. Thus, any learning that they exhibited must have occurred, as with controls, during the test phase rather than reflecting the transfer of information abstracted from exemplars. Clearly in this experiment grammatical knowledge was not represented in a vocabulary-independent way. One might suppose that transfer involves mapping between vocabulary elements at test, rather than abstraction at source (narrow rather than broad abstractions). Although this finding is not entirely conclusive because knowledge acquired via motor responses may be more perceptually bound than knowledge that is not.

Nonetheless, evidence from the SRT paradigm suggests that *n*gram information is not automatically available in a novel vocabulary and must consist primarily of narrow abstractions. The difference between the SRT and the classification task is that in the latter case participants have the opportunity to determine the frequency distributions of elements of which the test sets are composed and does not require speeded motor responses — the task has more in common with concept learning. So an additional mapping process is required to determine the correspondences between vocabularies that can only occur during a classification test. To what extent must participants have direct, explicit, access to the information abstracted from training exemplars in order to perform the mapping between vocabularies?

Gomez (1997, Experiment 1) investigated whether the ability to classify sequences in a novel vocabulary (an indirect test) is dependent upon participants being able to recall the relevant bigrams in the source vocabulary (a direct test). This is an important issue in resolving how fragmentary *n*gram knowledge can be mapped onto a novel vocabulary. Participants were trained on one of two vocabularies followed by a classification phase involving sequences presented in one of the original vocabularies (thus for one of the training groups this constituted a novel vocabulary). There were equal numbers of three types of test sequence. The first were grammatical, the second contained illegal bigrams and the third contained illegal trigrams. The direct test was a recognition-rating test of both legal (old) and illegal (new) fragments of exemplar sequences (although these did not indicate location the initial and terminal *n*grams were explicitly marked with a full stop). Participants tested in the source vocabulary were able to correctly classify substantially more sequences than participants tested in the novel vocabulary (this was because they were sensitive to more complex features, see later) and untrained controls. Participants were then partitioned into three groups according to their ability to recognise fragments of the training sequences (i.e. low, medium, high). Participants whose recognition of bigrams was no different than controls (low) were significantly better than controls in their ability to classify sequences that contained illegal bigrams, but not illegal trigrams in the source vocabulary. In contrast the medium and high recognition groups were much better at classifying sequences that contained both illegal bigrams and illegal trigrams. The medium and high recognition groups were also able to identify legal fragments in the novel vocabulary, but only the high recognition group were able to use that information to correctly classify sequences in the novel vocabulary, and only those that contained illegal bigrams. Clearly deliberate access to memory of fragments plays an important role in transfer. If participants are to map narrow abstractions from one vocabulary to another they must first be able to recall them. This study is consistent with the idea that knowledge acquired from exemplar sequences is a narrow form of abstraction that is initially bound to the vocabulary in which it was acquired. The mapping of knowledge from one vocabulary to another would seem to occur during the test phase and involve *n*gram knowledge.

An interesting feature of this study was that Gomez (1997) also found that participants were less sensitive to higher-order dependencies in the novel vocabulary than they were in the source vocabulary. So, if participants are able to apply knowledge of a second order dependency in the source vocabulary, they might only be able to apply knowledge of a first-order dependency in a novel vocabulary. Unfortunately, the distinction made earlier between first- and second-order dependencies is often confounded and can lead to erroneous conclusions. A study by Shanks, Johnstone and Staggs (1997)[3] provides an example of just such a confound. Shanks *et al.* claimed to demonstrate that participants were sensitive to bigrams in a novel vocabulary. In one subset of five pairs of items — one grammatical and the other ungrammatical — the ungrammatical version was constructed by changing the second letter in each of the five grammatical sequences, thus creating an 'illegal initial bigram.' Shanks *et al.* ensured that the resulting sequence-initial bigram (*VT, VR,* or *MT*) was permitted by the grammar, but *not* in that position. Participants could discriminate between the grammatical and ungrammatical versions of these sequences in the novel vocabulary, Shanks *et al.* concluded that participants 'have abstract knowledge either of legal triplets or of the restrictions on the positioning of bigrams' (Shanks *et al.*, 1997, p.228). However, participants need not have been responding on the basis of second-order dependency information at all — they may instead have been responding on the basis of a first-order dependency, how frequently an element occurred in the second position of each sequence. There were just three letters that could legally occur in this position (*M, V,* or *X*). The five ungrammatical sequences in this subset

[3] This study is considered, because it made appropriate partitions between different

introduced two new letters (*T* and *R*) in this position, and of the 60 sequences seen in all (including five other subsets of five pairs each), only these five sequences contained one or other of these two new letters. Thus, if participants learned how frequently an element could occur in the second position of the training exemplars, the ungrammatical sequences could be rejected on the basis that they contained low-frequency elements in position two. The other five subsets of test stimuli violated other aspects of the grammar, leaving the second position 'intact'. Moreover, the control group may have responded in a similar way, they tended (non-significantly) to endorse as grammatical the ungrammatical stimuli in this subset. Inspection of the scrambled sequences that they had memorised reveals that all five vocabulary elements could occur in position two and occurred there with equal frequency, but did not contain any second-order dependencies with other elements. An analysis of this particular study reveals that knowledge of fragments can be confused with knowledge of frequency, particularly since participants seem to be sensitive to less complex information in a novel vocabulary than they are in the source vocabulary. Frequency information is interesting because it could be used to map the component elements of a fragment acquired in one vocabulary onto novel vocabulary elements. This would provide a useful mechanism for the transfer of sequential dependencies.

Summary

The studies reviewed in this section confirm that fragmentary knowledge is important for the classification of sequences in the same vocabulary as learning. But they also indicate that fragmentary knowledge is a little more sophisticated than simple memory for fragments because participants are also sensitive to how often and where those fragments occur in both the source and a novel vocabulary. Clearly something of the sequential nature of the vocabulary elements of a language is abstracted from whole training exemplars and can be transferred to novel vocabularies. But it appears that

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types of ungrammatical sequences and lists the stimuli that were used.

this transfer of *n*gram structure is dependent upon mapping knowledge from one vocabulary to another at test. However, an alternative episodic account is not predicated on the representation of either rules or fragments of instances. The correct classification of new sequences can be accomplished by comparing them in terms of their overall similarity to stored exemplars that have not been analysed or decomposed in any way. This theory is discussed next.

1.2.4 Episodic memory for whole exemplars

Episodic memory for whole exemplars also provides a compelling account of artificial grammar. This class of model assumes that participants only encode exemplar sequences to the extent that they are instructed to, and have the capacity to store a large amount of unanalysed sequences in a discrete way. So, abstraction typically does not occur during training but at test when participants are asked to utilise information contained within the stored exemplars. Of course information concerning the structural relations amongst vocabulary elements will be tacit within an array of stored exemplars, but could be abstracted when required. These assumptions have a degree of plausibility because participants are generally *not* instructed to analyse sequences to determine how they were constructed, but *are* often instructed to memorise them. There are four main features of the episodic accounts of artificial grammar learning (Neal & Hesketh, 1997). First each sequence is stored as a separate memory trace or episode. Second, the memory trace encodes the particular operations that were carried out on a stimulus at the time of encoding. Third, the retrieval of a particular episode is dependent upon the reinstatement of a cue, for example a similar test sequence. Finally, test sequences are compared in terms of similarity on a *sequence-by-sequence* basis. This final feature is in contrast to the abstractionist and fragmentary accounts. But how could the similarity of a test sequence to an unanalysed exemplar be determined when they are instantiated with different vocabulary elements? Brooks and Vokey (1991) suggest that transfer occurs because of an *abstract analogy* (structural and relational similarities) between test sequences and stored training exemplars despite the changes in vocabulary. They suggested that the most readily available cue would be the patterns of repeating elements that are preserved in novel vocabularies. In fact the only features over which similarity could be computed on a sequence-by sequence basis in a novel vocabulary are patterns of repeating elements.

> "Experience with the distributional properties of the domain (e.g. frequency, characteristic features, privileges of occurrence) can remain distributed in the knowledge base and can be exploited by a process of forming immediate local models or analogies as the need arises." (Brooks & Vokey, 1991, p328).

So even the episodic account of artificial grammar learning is predicated, at least with respect to transfer, upon the representation of sequential dependencies between vocabulary elements – in this case dependencies between identical elements. This section begins by reviewing the evidence that rule-abstraction during training is neither an automatic nor a necessary process to account for the correct classification of sequences in the same vocabulary as training exemplars. This is followed by a discussion of the claim that similarity, rather than rule-adherence or chunk strength, is the dominant cue to the correct classification of test sequences. In addition, the evidence that the features over which similarity can be computed is, in the source vocabulary at least, dependent upon how participants are asked to study exemplars. Finally, we review the evidence that similarity in a novel vocabulary can only be computed, sequence-by-sequence, over patterns of repeating elements.

The first distinguishing feature of the exemplar based theory concerns whether or not abstraction occurs automatically during learning. Reber, Kassin, Lewis and Cantor (1980) argued that instructions to memorise or observe sequences leads to covert structural analysis — rule abstraction. In contrast Brooks (1978) argued that classification of sequences does not require covert abstraction at the point of learning. Rather the data can be accounted for by non-analytic memorisation of training exemplars.

To demonstrate this idea, Brooks (1978) asked participants to memorise sequences generated by two different grammars (A and B), that constrained the same set of vocabulary elements. Each sequence was paired with the name of either a city or an animal. However, the pairings were not obvious and reflected whether the city or animal was of new world or old world origin. Prior to testing some participants were informed of the old world-grammar A pairing and the new world-grammar B pairing and asked to classify new sequences as being either a new world entity, an old world entity, or neither. Overall 62% of sequences were correctly classified (relative to 33% chance). Another group of participants had not been informed of the old world-grammar A/new world-grammar B pairings but were instead told that they had studied two sets of sequences that differed in the way that they had been constructed. This group was also asked to classify the test sequences as belonging to either grammar A or Grammar B or neither. Unlike participants who had been informed of the pairings they were unable to do so. In contrast to the claims made by Reber and co-workers, there was no evidence for the automatic and passive abstraction of rules — the data were best explained by the *similarity* between the test sequences and the training exemplars that participants had memorised.

However, the use of a paired associate learning task could have made abstraction difficult. To investigate this issue Reber and Allen (1978) contrasted a partial replication of the paired-associate learning task that Brooks had used with the standard memorisation task. Verbal reports of both tasks highlighted the salience of bigrams in facilitating both the learning processes and for determining the similarity of test sequences and their well-formedness. The memorisation task led to slightly, but significantly, better classification performance than the paired associate learning did (.80 vs. .74 correct). Relative to memorisation, paired-associate learning led to an increase in accuracy for recognition, and a decrease in classification according to grammaticality. They conceded that paired-associate learning enhanced episodic learning rather than abstraction during training. This occurs because each exemplar is presented as a unique item and paired with a unique label rather than in the usual list form that highlights the different types of sequence, and the statistical distributions of the language.

McAndrews and Moscovitch (1985) reasoned that studying few exemplars would promote memory for instances, whilst studying many exemplars would promote rule abstraction during the study period. This seems plausible, since increasing the number of training exemplars would make remembering all of them more difficult because of capacity limitations, but might increase the salience of common features within the training exemplars thus promoting abstraction. McAndrews and Moscovitch used an incidental-orienting task that framed the acquisition phase as preference for computer brand names. To test the effects of similarity and rule-adherence, McAndrews and Moscovitch constructed test sequences, in the same vocabulary as learning, in which similarity was orthogonal to grammaticality: two types of grammatical and ungrammatical sequence were used that differed in how similar they were to the training exemplars. Similar (*near*) sequences differed from a training exemplar by only one element, whilst dissimilar sequences (*far*) differed by two or more elements. Each was presented twice as part of a two-alternative forced choice test, once with a *near* ungrammatical sequence and once with a *far* ungrammatical sequence. No test pair was derived from the same training exemplar. Overall the results suggested relatively equal and additive contributions of rule-adherence and similarity. Moreover, McAndrews and Moscovitch were able to identify two relatively distinct subgroups of participants, those for whom *grammaticality* was the dominant cue, and those for whom *similarity* was the dominant cue. The difference between this study, and results obtained by Reber and Lewis indicate that an orienting task is an important component to the form stimulus encoding takes during the acquisition phase. Unfortunately McAndrews and Moscovitch did not include a transfer condition. Vokey and Brooks (1992) reported a similar study that also used a set of test materials in which the similarity of the test sequences to the training exemplars was orthogonal to their grammatical status. In their Experiment 1, participants were asked to either classify new sequences as similar to the training exemplars, or classify them according to their rule-adherence. There were equivalent effect sizes for each condition, and in both conditions there were equivalent contributions of both the similarity of sequences and their grammatical status on both recognition and classification performance.

Overall the results of both the McAndrews and Moscovitch (1985) and the Vokey and Brooks (1992) studies suggested relatively equal and additive contributions of grammaticality and similarity to recognition and classification performance. These findings are not surprising because even if participants do abstract rules during exposure to training exemplars, they will of course remember both the learning episode and at least some of the training exemplars themselves. This might lead us to conclude that both processes could be responsible for artificial grammar learning phenomena in at least the same vocabulary as training exemplars. If so, the relative contributions of these two modes of classification could be determined by the way in which participants are asked to study and encode training exemplars.

A number of workers have investigated the effects of different orienting tasks to determine whether the processing episode is encoded along with the instances. For example, Whittlesea and Dorken (1993, Experiments 1-3) presented participants with sequences generated by two different grammars. One grammar was to be spelled and the other was to be pronounced. Participants were then required to either discriminate between new sequences generated by the two grammars, categorise sequences as belonging to either grammar or as ungrammatical, or finally to categorise the sequences as either new or old. Overall participants were able to complete these tasks with a reasonable degree of accuracy. Critically however, the two grammars contained an overlapping set of sequences that could be categorised as either. When participants were asked to discriminate between the two grammars they tended to classify the common sequences according to how they were processed during training. Whittlesea and Dorken suggested that the way that training exemplars are processed during training is encoded along with the memory for that instance.

An alternative procedure attempts to directly dissociate the effects of episodic and rule encoding at the point of acquisition using different orienting tasks. Mathews *et al.* (1989, Experiment 3) used two orienting tasks specifically designed to elicit either rule abstraction or instance memorisation strategies during learning. In order to elicit rule abstraction participants were presented with ungrammatical sequences and were asked to *edit* them in order to make them grammatical (despite not having seen any well-formed exemplars); participants were required to continue editing each sequence until they were correct. In order to elicit non-analytic memorisation participants were presented with an exemplar and asked to memorise it. Five seconds later participants were required to match the exemplar to one of several new sequences. Participants in both groups were able to correctly classify test sequences in the same vocabulary at roughly the same level of accuracy.

Shanks *et al.* (1997, Experiment 3) took this design to its logical conclusion within the same vocabulary. They investigated sensitivity to Brooks and Vokey's (1991, Experiment 3) similar (*near*) and dissimilar (*far*) test items following the *match* and *edit* orienting tasks that Mathews *et al.* had used. One might expect that the *match* group would be more likely to endorse the similar sequences (whether grammatical or not) whilst the *edit* group would be more likely to endorse grammatical sequences (whether similar or not). However, as in the Mathews *et al.* case Shanks *et al.* found no difference between groups with both groups classifying around 57% of sequences correctly. There were also significant effects of both grammaticality and similarity but these did not differ between groups, although these trends were in the expected directions. It would have been interesting to observe the effects of this procedure on a novel vocabulary. Shanks *et al.* acknowledged that these data might have been a consequence of overlapping chunk strength in the stimuli sets (c.f. Perruchet, 1994). It seems that in the source vocabulary it is difficult to distinguish between different modes of representation.

These studies, conducted in the same vocabulary as exemplars, have not provided conclusive evidence that the classification of previously unseen sequences is predicated on either memory for exemplars or rule-abstraction. These two theories can, however, be distinguished by the information that is available to classify sequences in a novel vocabulary. The exemplar-based model assumes that classification proceeds by determining, *sequence-by-sequence*, how similar test sequences are to stored exemplars. As Brooks and Vokey (1991) suggested, the only information that similarity could be computed over on a sequence-by-sequence basis in a novel vocabulary are dependencies between repeating elements. The following section reviews some evidence of similarity based classification in a novel vocabulary.

Exemplar based transfer

Brooks and Vokey (1991) demonstrated that test sequences presented in a novel vocabulary could be classified according to how similar they were to training exemplars. They used the same set of (*near* and *far*) test sequences that Vokey and Brooks (1992) had used, in which grammaticality and similarity to training exemplars was orthogonal. For the training phase, participants were required to memorise the sequences to criterion. The test phase consisted of a rating-recognition test for training exemplars and previously unseen sequences and a standard classification task in which participants were either asked to rate the sequences according to their grammatical rule adherence or their similarity to training exemplars. The order of test presentation was counterbalanced. As in the same vocabulary studies mentioned earlier, in both the recognition and the classification task, both grammaticality and similarity exerted significant effects. In both cases similarity was marginally, but not significantly, more influential, irrespective of whether participants were asked to classify sequences according to similarity or grammaticality. The grammaticality effect is in accordance with the rule-abstraction hypothesis that was outlined earlier. However, the significant effect of similarity suggests that the abstraction of rules might have occurred during testing and not during training. Brooks and Vokey suggested that transfer occurs because of an *abstract* between test sequences and stored training exemplars despite the changes in vocabulary.

This notion of abstract analogy refers simply to whether a test sequences contains patterns of repeating elements that were either present or absent in the exemplars. In this way the similarity of a test sequence in a novel vocabulary, to stored exemplars, can be determined on a sequence-by-sequence basis. Sensitivity to sequential dependencies between repeating elements in novel vocabularies has been observed by a number of workers. For example, Shanks *et al.* (1997, Experiments 1 & 2) used a two-alternative forced-choice test to compare sensitivity to specific grammatical violations in both the source and a novel vocabulary. In their Experiment 1, Shanks *et al.* found that although participants were sensitive to other features in the source vocabulary they were sensitive only to illegal repetitions in the novel vocabulary. In their Experiment 2, Shanks *et al.* found some sensitivity to other features in the novel vocabulary such as how frequently single elements and *n*grams occurred in particular positions. Indeed Perruchet (1994) noted that the stimuli used by Brooks and Vokey were confounded by the frequency of occurrence of old and new bigrams, and that the results obtained could be explained by the fragmentary account and not simply by a whole-exemplar account. For example the test sequences used by Brooks and Vokey contained varying proportions of initial and terminal trigrams that had been present in the exemplar sequences. The grammatical test sequences, whether *near* or *far*, contained substantially more initial and terminal bigrams present in the exemplar sequences than the ungrammatical test sequences, and in both cases the *far* items were dissimilar in terms of the frequency of occurrence of these initial and terminal trigrams. Participants may have learned the frequency of particularly salient bigrams and mapped those onto the new bigrams in the novel vocabulary. Ungrammatical sequences could then be rejected if they began with low frequency bigrams. In sum, Perruchet (1994) argued that memory for specific fragments, dependent upon their frequency of occurrence in the exemplar set (*chunk strength*) carried the effects seen in both these studies. See Knowlton and Squire (1994, 1996) for similar observations. The point here is that these studies have not been able to rule out the transfer of information based upon dependencies between non-repeating elements that can only be mapped onto a novel vocabulary by inducing distributional information *across* the test sequences.

One paradigm, the so-called randomly changing transfer paradigm, provides stronger evidence for participants' sensitivity to sequential dependencies between identical elements because it removes all other cues to the well-formedness of test sequences such as *n*gram frequency. Typically in transfer studies there is a one-to-one mapping between the elements of one vocabulary and the elements of the vocabulary, for example *M* is always replaced with *Q*. Manza and Reber (1997, Experiment 1) report a study where the elements of the source vocabulary were used at test but the mapping of the elements to the grammar was randomly transposed from trial to trial. For example, using the vocabulary *MTV*, on the first trial every *M* might be replaced with *T*, on the second trial *M* might be replaced with *V*, and so on. The importance of this procedure is that information, such as frequency of occurrence, about elements that do not repeat is lost and participants are unable to induce the correspondences of those elements and their dependencies between the source and novel vocabulary. The only information that is preserved when the mapping of grammar to vocabulary changes from trial to trial are the sequential dependencies between identical elements. The mapping of this form dependency between the source and randomly changing vocabulary can be induced on an item by item basis by virtue of the fact that they repeat within each sequence. Manza and Reber found participants' ability to classify sequences under these conditions was the same as another groups' ability to classify sequences in a novel vocabulary with a one-to-one mapping. However, participants who were tested on the sequences in the source vocabulary, that was not randomly transposed classified substantially more sequences correctly. This replicates the typical cost of changing vocabularies, but suggests that the information available to the two transfer conditions was the same – even in the fixed transfer condition participants classified sequences according to the dependencies between identical elements.

Redington and Chater (1996, 1997) reported a similar study. In this case however, a novel vocabulary was used, that allowed a fairer comparison between the random mapping condition and the one-to-one mapping condition. For example, in one test sequence the letter *M* might be substituted with the letter *Q*, but on another it might be substituted with *T* or *V*. However, those data were retracted when some errors in the experimental software were identified that meant some participants in the randomly changing transfer group were in fact tested on simple transfer. The participants who were actually run in the randomly changing transfer condition do show a transfer effect, but this did not reach the criterion for statistical significance (M. Redington, 1997, personal communication).

In their Experiment 5, Whittlesea and Dorken (1993) also used the randomly changing transfer procedure to assess participants' sensitivity to the patterns of repeating elements. Participants were asked to either process the repeating elements within each sequence (condition 5b) or to simply memorise the sequences (condition 5c). In the test phase participants were presented with sequences in a new vocabulary where the letter to letter mapping changed from trial to trial. Participants who had been asked to attend to patterns of repeats were similarly sensitive to the well-formedness of sequences in either the same or a different vocabulary. In contrast participants who were asked to memorise sequences were substantially more sensitive to the well-formedness of sequences in the same vocabulary than they were in the new one.

Unfortunately neither Whittlesea and Dorken nor Manza and Reber included appropriate controls and sensitivity was relatively low albeit above chance. Redington and Chater did include an appropriate control condition in which participants were trained on scrambled sequences, but their study suffered software errors. Control groups are important, because as Perruchet (1994) pointed out it is not inconceivable that learning can occur during the test phase. To draw the conclusion that the transfer of grammatical knowledge has occurred, trained participants must exceed the performance that might be expected if learning (other than the mapping between vocabularies) did occur during testing. However, the principle remains sound: although in each of these three experiments both the sequential dependencies between repeating and non-repeating elements were violated in the ungrammatical sequences, the only information that is preserved when the mapping between vocabularies changes from trial to trial are the patterns of repeating elements. Even if participants do possess narrow abstract knowledge (e.g. of sequential dependencies between non-repeating elements, and perhaps how frequently they occur) that knowledge could not be applied.

Only broad abstractions pertaining to patterns of repeating elements could be used to classify test sequences. To demonstrate this point Whittlesea and Dorken (1993) in their Experiment 4 created a "grammarless grammar" that generated a set of sequences that contained patterns of repeating elements using a bi-conditional rule. For example, an element on one half of a sequence would be repeated on the right half of the sequence (_ _ X _ → _ _ X _) while the intervening element (denoted by an underscore would be random). Participants were asked to study half of these sequences instantiated in one vocabulary, and asked to classify the remaining half and the exemplar sequences instantiated with a novel vocabulary. Participants classified 57% of these sequences correctly, a figure that is comparable with both the randomly changing and the standard transfer effects. However, Altmann *et al.* (1995) pointed out that the effect of repetition structures, rather than being predicated on the representation of whole training exemplars, could be accounted for by a simple rule. We return to this issue in Chapters 5 and 6. So abstract analogy as a means to transfer information concerning grammatical structure need not be predicated upon the representation of whole training exemplars. Irrespective of what information is learned from training exemplars the point here is that in a novel vocabulary knowledge of dependencies between repeating elements supports the correct classification of test sequences on a *sequence-by-sequence* basis but has not ruled out the transfer of other forms of information.

If sequences are classified in a novel vocabulary on the basis of similarity to stored exemplars (computed over patterns of repeating elements) then we would not expect participants to be able to transfer knowledge of a grammar that did not generate dependencies between repeating elements. Gomez, Gerken and Schvaneveldt (in press) investigated this issue. In one Experiment, the grammar generated dependencies between identical elements in a third of the sequences. They found that although participants were able to correctly classify sequences that contained only dependencies between non-identical elements in the source vocabulary, in the novel vocabulary only those sequences that contained legal and illegal dependencies between identical elements could be discriminated. In another experiment the grammar was modified so that it generated no dependencies between identical elements at all. In this case, participants were able to correctly classify sequences in the source vocabulary, but were unable to do so in the novel vocabulary. However, this language contained a larger vocabulary (10 elements) than is common in transfer studies, for example, the most frequently used language shown in Figure 1.2 contains only five elements, also the grammar contained more nodes (10) than the grammar in Figure 1.2 (6). In addition, the violations in the ungrammatical sequences used by Gomez *et al.* did not occur in salient locations (such as the start and endings of sequences) and so participants may have been less sensitive to them.

In contrast Altmann *et al.* (1995) performed a post-hoc partitioning of their data and found that participants *were* able to discriminate between a subset of grammatical and ungrammatical sequences (43% of the test set) that did not contain repeating elements in a novel vocabulary. Participants may have learned, on the basis of those sequences that did contain repeating elements, something of the mapping between vocabularies that then allowed them to transfer this knowledge, or they may have been sensitive to cues (such as frequency) that were not present in the Gomez *et al.* study. This final study, like the Shanks *et al.* study, finds evidence that participants are sensitive to more than just patterns of repeating elements in a novel vocabulary. This additional information could only be induced *across* test sequences. We return to exactly what this information might be in Chapter 3. In sum, memory for training exemplars does allow the transfer of knowledge to new situations and vocabularies; however, even theories based upon memory for whole sequences must resort to grammatical information such as sequential dependencies between repeating elements in order to account for the classification of sequences in a novel vocabulary. Indeed some of the studies described earlier (e.g. Gomez *et al.* in press; Manza & Reber, 1997; Whittlesea & Dorken, 1993) suggest that the transfer effect may be entirely predicated upon this form of sequential dependence. The important feature of this form of grammatical knowledge is that it can be applied on a *sequence-by-sequence* basis. Other studies, that do not discriminate between these two forms of sequential dependence, suggest that both forms of dependence may be transferred on the basis of information (such as frequency) induced across training and test sets (e.g. Altmann *et al.* 1995; Gomez & Schvaneveldt, 1994; Shanks *et al.* 1997).

1.2.5 Interim Summary

Do episodic- and rule-based forms of knowledge differ in how abstract they are? Thus far we have discussed a variety of different meanings of abstraction. Grammatical knowledge may be abstract in the sense that it reflects fragmentary rules that summarise the information contained in a set of training exemplars, alternatively it may be rather more concrete in the sense that it represents the training exemplars themselves. This latter mode of representation could not be described as abstract in the sense that it summarises information and could not be described as a representation of grammar in the linguistic sense. Both forms of knowledge appear to play a part in participants' ability to classify test sequences, especially in the source vocabulary. Another meaning of abstraction relates to whether those representations are tied to the perceptual features of the vocabulary in which they were acquired. Both episodic- and rule-based theories of artificial grammar learning are predicated upon the representation of sequential dependencies. In the episodic case the representation of sequential dependencies are latent in the stored exemplars until they are utilised as a means to classify test sequences. As mentioned earlier, the correspondence between repeating elements in one vocabulary and another can be induced at test on a sequence-by-sequence basis simply because those elements share a property (they repeat) that is readily apparent. This characteristic is consistent with broad abstraction but would seem dependent, in cases where the dependency spans whole sequence, upon the representation of whole sequences. In contrast, sequential dependencies between non-repeating elements appear to be induced by computing the relative frequencies with which individual elements occur and co-occur with other elements in the training exemplars. The transfer of this information can only proceed by inducing the same information *across* sequences in a novel vocabulary and mapping that information onto that derived from the training exemplars. These findings are important because they suggest that participants may possess abstract, as opposed to episodic, knowledge of sequential dependencies that can only be applied in a novel vocabulary by inducing the correspondences between the two vocabularies. This form of representation was earlier referred to as a narrow abstraction in contrast to a broad abstraction that is independent of surface form or vocabulary (c.f. Shanks, 1995).

Some empirical evidence suggests that participants might be able to transfer knowledge of dependencies between both repeating and non-repeating elements (e.g. Altmann *et al.* 1995; Shanks *et al.* 1997). Of course participants remember fragments of training exemplars, and indeed whole sequences, just as people (including infants) remember and can repeat the utterances of other people. Underlying that process is the ability to abstract and transfer the sequential dependencies contained within that information to new sequences and to new vocabulary elements. So the question remains, do people remember such sequences in a relatively unanalysed form to be recalled and processed as the task demands (e.g. Brooks & Vokey, 1991), or do they automatically induce the rules of the language (e.g. Reber, 1993)? These are issues that might never be entirely resolved by empirical enquiry alone. However, computational modelling provides an ideal test bed for theories of learning and of representation. It is to these studies that we turn next.

1.3 COMPUTATIONAL MODELS

Computational models of artificial grammar learning are formal implementations of the theoretical accounts that were outlined earlier. Because they model putative learning mechanisms devoid of the idiosyncrasies of human empirical data they provide ideal test beds for different theories of abstraction and representation.

1.3.1 Fragmentary Models

Competitive Chunking

Servan-Schreiber and Anderson (1990) describe a model of artificial grammar learning that acquires episodic fragments during exposure. What makes this model interesting is that knowledge is formed as a network of hierarchical chunks, which as a consequence of being abstracted from training exemplars, encodes both the grammatical constraints and the frequency of their implementation. As a consequence, this model ought to capture fine details observed in the empirical literature. For example, during training, the sequence TTXVPXVS may be initially chunked at the word-structure level (TTX) (VP) (XVS). Further chunking might encode 'phrases' such as ((TTX) (VP)), followed by a single superordinate chunk (((TTX) (VP)) (XVS)), encoding the whole exemplar. Classification would then proceed on the basis of *familiarity*, where familiarity is the total number of chunks in a given test sequence (this rubric corresponds to *chunk strength* discussed earlier). In order to test this model, Servan-Schreiber and Anderson (Experiment 1) compared participants presented with either well-structured, badly-structured, or unstructured training exemplars. Well-structured training exemplars preserved the constituent structure of the grammar (i.e. chunks tended to recur in other sequences), whilst badly-structured training exemplars violated it (i.e. chunks tended not to recur). Initially participants were required to memorise to criterion the training sequences, by reproducing five blocks of four. Significantly fewer errors to criterion were observed for the well-structured and unstructured conditions relative to the badly-structured condition. Note that participants who memorised unstructured training exemplars chunked 72.5% of the training exemplars when asked to reproduce them. At test, participants who had memorised either unstructured or well-structured training exemplars outperformed participants who had memorised badly-structured training exemplars. Morgan *et al.* (1987) in a similar experiment found that when participants were instructed to look for rules, memorisation of well-structured training exemplars led to better classification than memorisation of unstructured training exemplars (see also Reber *et al.*, 1980). It is unclear how the model *knows* where to place markers between the constituents (word-structure level) without them being presented in pre-chunked form, as was the case for human participants. Servan-Schreiber and Anderson reported a good fit between the simulation data and their empirical data; this was confirmed by Dienes (1992) and by Redington and Chater (submitted).

This model was specifically designed to model the classification of sequences presented in the same vocabulary as the training exemplars. Since this model represents fragments symbolically, transfer would have to be effected using an additional mechanism that would map the chunk strength of each stored fragment in one vocabulary onto a comparable fragment in another. If this could be effected, the model could account for cross-modal but not randomly changing transfer because the model does not encode the dependencies between repeating and non-repeating elements in different ways. Servan-Schreiber and Anderson did not consider this extension. Because knowledge of fragments is represented episodically they are available for conscious report (as the bulk of the empirical data suggests), however, the chunk strength is not available for conscious report. As a consequence this model does predict discrepancies between verbal report or direct recognition test performance (a measure of explicit knowledge) and indirect classification performance (a measure of implicit knowledge). However, the ability of any one computational model to classify sequences within the same vocabulary as training exemplars is of little interest, the majority of models do model the empirical data reasonably well. What is important is the notion of abstraction, both in terms of the representation of grammar, but more importantly how that knowledge can be applied to a novel vocabulary. Although this model is interesting in its ability to form a representation of the stimulus array that corresponds to the notions of sequentiality that were described earlier it fails to describe how episodic knowledge of fragments might be transferred to a new vocabulary. One such mechanism is reviewed next.

Toy models

Redington and Chater (1996) considered a class of fragment learning models, that they called 'Toy models', that operated in a much simpler fashion than the one described by Servan-Schreiber and Anderson (1990). As mentioned earlier these models are important because they describe how knowledge of fragments that are instantiated in one vocabulary (a narrow form of abstraction) can be mapped onto another.

The models learn permissible fragments from training sequences. In the same vocabulary as learning the models classify a new sequence as grammatical if it contains old fragments and ungrammatical if it contains new fragments. The models could clearly achieve a high degree of classification performance when tested in the same vocabulary as learning, and are consistent with Perruchet and Pacteau's (1990) account that was outlined earlier. In order to classify sequences in a novel vocabulary the models must map fragments from the source vocabulary onto fragments in the novel vocabulary. Thus this class of model abstracts episodic fragments during learning and induces the correspondences between vocabulary elements at test (Redington & Chater, 1996, p132).

The mapping of vocabulary elements must be consistent within each sequence (i.e. contain no novel fragments) otherwise that sequence would be rejected. For example, if only the bigrams *MS* and *SM* occurred in training, the test sequence *BTBT* (now in a new vocabulary) would be classified as grammatical the potential mapping that equates *B* with *M* and *T* with *S* is consistent. In contrast the sequence *BTTB* would be classified as ungrammatical because the bigram *TT* cannot be mapped onto either *MS* or

SM. Where the language contains more than two vocabulary elements the models must induce a mapping *across* sequences that is consistent in the proportion of the test set that is grammatical (i.e. it must be applicable in usually 50% of all sequences). For example, if every exemplar begins with *MS, MV,* and *VX,* and at test participants see sequences such as *JDHBHF, BFHHHH,* and *JBHFJ,* participants can induce that if J begins a sequence it can be followed by one of two letters. Hence *J* in the novel vocabulary corresponds to *M* in the source vocabulary, and *B* corresponds to *V* as it can begin a sequence and follow *M.* Similarly *F* must correspond to *X* because it is the only element that can follow *V,* and so on. The point here is that even in these simple toy models, knowledge of the sequential dependencies between non-repeating elements, can only be mapped onto sequences in a novel vocabulary by computing that mapping over a series of test sequences, rather than on a sequence-by-sequence basis. However, these models can transfer their knowledge to novel vocabularies but do not encode sequentiality or positional information other than initial and terminal fragments nor do they encode how frequently those fragments occur. This last feature is inconsistent with the empirical literature that suggests participants do encode positional information (e.g. Gomez & Schvaneveldt, 1994). Consequently these models fail to adequately simulate empirical data.

Redington and Chater (1996) found that these models could account for transfer across vocabularies using a variety of grammars and procedures, including cross-modal transfer and randomly changing transfer. However, the models differ widely in accuracy according to the length of fragments that they encode (e.g. bigrams or trigrams) and whether they encode initial and terminal fragments as distinct from internal fragments. Consider the case of Whittlesea and Dorken's (1993, Experiment 5) case of randomly changing transfer. In that experiment the best classification performance was found when participants were asked to study the patterns of repeating elements in each exemplar (57% correct) relative to memorisation (53%) and reading the sequences out loud (52%). Redington and Chater found that the best simulation of these data was given when the model encoded trigrams that also explicitly encoded either the initial (54%) and terminal fragments (58%).

Since Whittlesea and Dorken specifically designed this grammar to be classified by repetition structures, many of which could be present within trigrams, we can conclude that *this* feature was the basis of the models' performance. Also, Redington and Chater (1996) did not specify how the mechanism might 'know' to retain the mapping it induced over one sequence to the next, or whether it is supposed to discard that mapping from sequence to sequence in cases of randomly changing transfer. The main problem with this model is that Redington and Chater did not specify what information the models should encode and this leads to poor simulation results.

Because there are no criteria concerning *what* to encode (e.g. how long a fragment should be) these models are too strong and could plausibly be used to fit *any* data. Where the models match human source vocabulary performance, they underestimate novel vocabulary performance. For example, consider the cross modal effects observed by Altmann *et al.* in their Experiments 1 & 2; they observed that participants classified 58% of sequences correctly in the source vocabulary and 55% in the novel vocabulary. Only one of Redington and Chater's toy models could simulate source vocabulary performance exactly. In this simulation the model explicitly encoded the terminal bigram as position specific and the remaining bigrams (including the first) irrespective of position; transfer performance was underestimated at 50% correct. If the match is with transfer performance, the source vocabulary performance is overestimated. For example, one simulation that explicitly encoded the initial bigrams and the remainder irrespective of location classified 54% of test sequences in the novel vocabulary, but overestimated source vocabulary performance at 68% correct. If the model also explicitly encoded the terminal bigrams the model classified 56% of sequences in the novel vocabulary but 85% in the source vocabulary. All of the models described by Redington and Chater underestimated the 65% of sequences that participants classified in the novel vocabulary observed by Altmann *et al.* in their Experiment 4.

An even greater problem is that these models would not be able to reject sequences that contain misplaced legal bigrams, despite Gomez and Schvaneveldt's (1994) demonstration that human participants *are* able to correctly reject such sequences. In general this class of model provides a good description of how fragmentary information can be mapped across vocabularies, but do not provide a good fit of the human data. This problem could only be resolved if the assumption is made that classification in the two vocabularies relies upon mapping different information. One such mechanism is discussed next.

Classifier systems

Mathews *et al.* (1989; see also Roussel, Mathews, & Druhan, 1990; Mathews and Roussel (1997) describe a model that owes much to the traditional problem solving literature (e.g. Holland, Holyoak, Nisbett and Thagard, 1986). Various instantiations of their classifier system THIYOS (The Ideal Yoked Subject) explicitly encode rules as symbolic condition-action relationships. THIYOS has two learning mechanisms, the first simulates source vocabulary learning by learning fragments of the surface vocabulary, and the second simulates classification in a novel vocabulary by learning patterns of repeating elements.

Mathews *et al.* (1989) trained and tested participants on grammatical exemplar sequences successively over a four-week period. Prior to each classification test they asked for verbal reports in order to guide yoked participants. Whilst the performance of the yoked participants exceeded both chance and that of a control group, it remained below that of the original participants. This suggests some residual knowledge that was not, or could not be, articulated. THIYOS was designed to determine what knowledge was responsible for the discrepancy between the trained and yoked participants' classification performance. Classifier systems acquire symbolic representations of fragments that become *conditions.* An important difference between THIYOS and the other fragment learning models is that it learns using a forgetting algorithm. Upon presentation of an exemplar sequence THIYOS learns the position of each vocabulary element (or elements) forming a rule (*if 'M'* occurs in position 1, *then* respond 'good') that becomes weighted according to frequency of occurrence. At test each rule is either applicable or it is not. If a match occurs the rule is strengthened, and if it is not, the strength of the rule is weakened (i.e. forgotten). At test, a sequence is classified (the *action*) if it contains a known fragment (the *condition*). In this respect the model behaves much like the models described by Redington and Chater in the source vocabulary. It *is* different in the sense that it contains a working memory that allows different rules to compete; for example, when two condition-action rules are applicable to a single sequence. Competition occurs because each rule is weighted according to how often it has been applied on previous trials, if two rules are in agreement the probability of classification increases (again this feature is consistent with the earlier rubric of *chunk strength*). When THIYOS was run with equally weighted rules, its performance matched those of yoked participants who, of course, had no experience to tune the strength of each rule. When the rules were weighted according to their applicability (they matched the overall *chunk strength*) THIYOS classified at a level comparable with the original trained participants who did have the experience needed to weight the rules. However, the relative utility of each rule was not articulated and constitutes *implicit* knowledge. Hence, in this respect, the model is consistent, although relying upon a different architecture, with other fragmentary models.

Unlike the fragment learning models described by Redington and Chater (1996) THIYOS effects transfer on the basis of a parallel learning mechanism that encodes patterns of adjacent repeats. The elements in each exemplar are encoded according to whether they are the same or different to the previous element, elements that do not repeat or are not adjacent within a sequence are discarded. For example the sequence MTTVX would be encoded as - - r - - but the sequence MTVTX would be encoded as though it did not contain repeats (i.e. - - - - -). Test sequences are classified according to whether they contain familiar or unfamiliar patterns of repeating elements, using the same forgetting algorithm as the source vocabulary system. Roussel *et al.* (1990) report that THIYOS classifies sequences in novel vocabularies at a level comparable to the human participants the Mathews *et al.* study. In principle it could also classify above chance on randomly changing transfer. In addition, classification according to repetition structures can proceed on a sequence-by-sequence basis because no mapping between vocabularies need be computed at test. Each test sequences can be directly compared to either one or all stored training exemplars. Hence the first test sequence that contains repetition structure has the same probability of being correctly classified as the last. Another implication of this system is that participants ought not to be able to transfer knowledge of sequential dependencies between non-repeating elements or to be able to classify sequences that do not contain any repeating elements. In this model classification of sequences on the basis of repetition structures is not rule-like behaviour: If the assumption is that participants endorse all familiar and reject all unfamiliar repetition structures then the representations responsible for endorsing grammatical sequences cannot generalise to new repetition structures.

One problem identified by Dienes (1992) was that an element or fragment in one position is encoded as an entirely different entity than the same element or fragment in another (i.e. they become conditions in different rules). However, this is in fact an advantage of this model because it allows the rejection of ungrammatical sequences that contain misplaced legal bigrams. Perhaps a greater problem is that in novel vocabularies it is entirely dependent upon the grammar generating patterns of adjacent repeats. However, here the empirical data are inconsistent. For example in the Altmann *et al.* (1995) study participants could classify sequences generated by a grammar (Figure 1.2) that did not permit dependencies between repeating elements to be adjacent, although the model would require only a simple modification to extend the range of these dependencies. In addition, participants were also able to correctly classify a subset of sequences that did not contain any repeats (but see Gomez *et al.* 1997 discussed earlier). This issue is explored in subsequent chapters. Unfortunately, this model has not been directly compared to the performance of other models.

In conclusion, THIYOS can simulate the classification of sequences in both the source and a novel vocabulary, despite being restricted to learning fragments. THIYOS relies, in order to effect transfer, on the presence within a sequence of adjacent repeating elements; if the human data were to confirm that participants can transfer their knowledge to novel sequences that do not contain repeating elements, the model would not be tenable. This issue is discussed further in Chapters 3-5.

1.3.2 Exemplar Models

This class of model owes much to the traditional categorisation literature (e.g. Estes, 1986; Hintzman, 1986; Reed, 1972). Test sequences are classified according to their overall similarity to stored training exemplars (rather than fragments). There are two potential mechanisms for how this might occur. For example, a *Nearest Neighbour* model' (Reed, 1972) might compare each test item to each individual stored exemplar. Alternatively, the so-called *Chorus of Instances* model compares each test sequence with all the stored training exemplars at once (c.f. Hintzman, 1986; Nosofsky, 1992). Both models could support transfer if some features, particularly patterns of repeating elements, in the novel vocabulary are analogous to features in the source vocabulary. Memory for whole sequences is advantageous because all of the information that could potentially be used to classify sequences in any vocabulary is present. Dienes (1992) tested an exemplar model using the *Chorus of Instances* mechanism to compute similarity (c.f. Hintzman, 1986; Vokey & Brooks, 1992). The model assumes that participants faithfully store veridical representations of each and every training exemplar. When a test sequence is presented it activates every stored exemplar. The features common to all training exemplars are more active than features that are not shared by the training exemplars. The similarity of the test sequence can be computed according to the number of common features it shares with this rather abstract *chorus of instances.* This abstract *chorus of instances* in effect represents a fuzzy prototype, and so each test sequence is compared in terms of how similar it is to a prototypical sequence. Participants need not have explicit access to each exemplar.

Nearest Neighbour exemplar models (c.f. Brooks, 1978, Reed, 1972) compare each test item to each stored exemplar in turn. For each comparison a similarity index is computed as a function of the number of features common to the pair. The probability of endorsing a sequence as grammatical is a function of this similarity index.

There are two features of these models that are worth considering. First, they predict that participants should be homogenous with respect to which sequences they are able to classify. This is because, on the assumption that participants have the capacity to faithfully store each and every exemplar, similarity judgements should the same. In contrast the fragmentary accounts assume limited capacity, and idiosyncrasies in which fragments participants encode. Second, transfer occurs by a process of *abstract analogy* (c.f. Brooks & Vokey, 1991); in a novel vocabulary the only common features that remain between the chorus of instances, or each individual exemplar, and the test sequence would be the patterns of repeating elements. Finally, test sequences are compared on a sequence-by-sequence basis whatever the vocabulary. The models are simple in that to transfer knowledge there is no need to compute a mapping between each vocabulary element in each domain because those that repeat are readily available and allow above chance classification. Of course performance in the source vocabulary would be greater because features other than repetition structure could be compared in terms of their similarity to training exemplars. According to these models, transfer should not occur to sequences that do not contain repeating elements. In addition exemplar models also predict that in at least the same vocabulary as learning, participants should be sensitive to both misplaced legal fragments and to illegal fragments. In a novel vocabulary these models probably could not reject sequences that contain illegal bigrams or trigrams unless they were composed of repeating elements. However, neither Redington and Chater (submitted) nor Dienes (1992) found a good fit between these models and the empirical data.

1.3.3 Neural Networks

This class of models is becoming increasingly ubiquitous in cognitive science. From the point of view of artificial grammar learning they are interesting because they are powerful in describing other linguistic processes (e.g. Elman, 1990; Chater & Conkey, 1992; Rumelhart & McClelland, 1986). Connectionist models are also appealing because they behave in rule-like ways without explicitly representing anything that would correspond to a symbolic rule. It is safe to say that connectionism has become the dominant architecture for computational modelling in cognitive-experimental psychology, although the connectionist movement is not without its critics (e.g. Fodor & Pylyshyn, 1988; 1997). A number of different architectures have been proposed but in the main the most successful is the simple recurrent network (SRN, a modified version is shown in Figure 1.4).

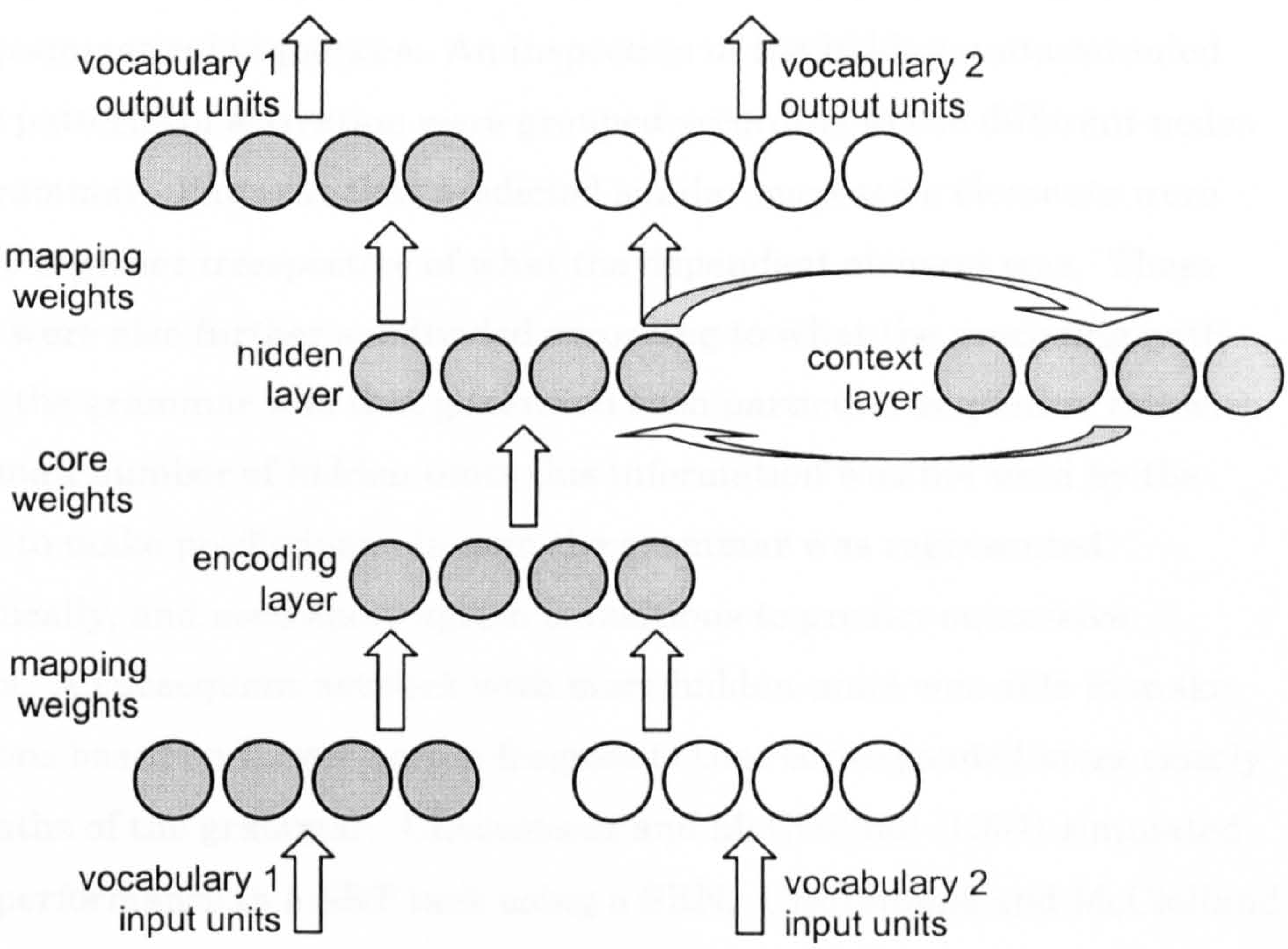

Figure 1.4: A modified Simple Recurrent Network (Dienes *et al.* 1999)

Elman (1990) described a training regime that appears well adapted to the task of encoding sequential structure in time. He trained a SRN to predict what its next input would be given a current or preceding input. Each element of a stimulus is presented to the input units and the resulting patterns of activation percolates up through to the output units. The hidden units feed back on themselves, thus the processing of the previous sequence influences the processing of the current sequence. The context units provide a kind of memory where the patterns of activation in the hidden units are copied – this constitutes the *representation* of the previous stimulus input. Consequently, the network's ability to identify the subsequent element is partially determined by its representation of the previous input. In sum, the SRN learns sequential dependencies. Cleeremans, Servan-Schreiber, and McClelland (1989; see also Servan-Schreiber, Cleeremans, & McClelland, 1991) presented just such a network with sequences generated by the finite-state grammar shown in Figure 1.1. They found that after training, the network performed exceptionally well at discriminating between grammatical and ungrammatical sequences. An inspection of the hidden units revealed that the patterns of activation were grouped according to the different nodes of the grammar. Patterns that predicted similar successive elements were clustered together irrespective of what the dependent element was. These clusters were also further subdivided according to what the preceding path through the grammar was that generated each particular sequence, but with only a small number of hidden units this information was not used by the network to make predictions. In sum the grammar was represented hierarchically, and used short *n*gram transitions to predict successive elements. A subsequent network with more hidden units was able to make predictions based on longer *n*gram fragments that corresponded more closely to the paths of the grammar. Cleeremans and McClelland (1991) simulated human performance in a SRT task using a SRN. Cleeremans and McClelland (discussed earlier) had found that participants learned to predict successive elements (light locations constrained by a grammar) that were dependent upon up to two preceding elements – trigrams. They found that a modified version of the network, in which the activation of a particular response was based on both the incoming pattern of activation and a decaying trace of the previous pattern of activation, explained 81% of the variance of the human data. Closer inspection of the network revealed that it became sensitive to successively higher order dependencies over successive trials in much the same way that humans do. When given a particular sequential stimulus the SRN will learn to predict the next stimulus element in that sequence. This is precisely what participants are able to do in the anagram task of Reber and Lewis (1977) and Sequential Letters Dependency task of Dienes *et al.* (1991). Indeed this is one of the fundamental principles of both generative grammar *and* associative learning. Berry and Dienes (1993) demonstrated that the predictive properties of this kind of network could accurately simulate the ability of humans to classify previously unseen sequences by calling a sequence grammatical if its component elements were accurately predicted, and ungrammatical if they were not. In the SRN the patterns of activation are intimately dependent upon the surface features of the vocabulary, if a network learns the dependencies between the vocabulary *ABCD* how could it possibly apply that information to an entirely novel vocabulary *PQRS?*

Since the vocabulary is represented by the network as a distributed pattern of activation across the input units, and the grammar is represented as a distributed pattern of activation throughout the rest of the network, it follows that it is only the peripheral layers that are dependent upon the perceptual features of the vocabulary. In much the same way that the retina and the cochlea process different percepts, the brain itself might processes those different percepts in much the same way. All a network need do is determine the correspondences between vocabularies and apply the same internal patterns of activation. Those patterns of activation must reflect the statistical distribution of vocabulary elements in the language and could, in principle, permit the mapping of dependencies seen in one vocabulary onto those seen in another. To do this Dienes, Altmann and Gao (1999; see also Dienes, Altmann, Gao, & Goode, 1995a) modified the SRN by adding an additional hidden layer of units that induces the mapping between the two vocabularies. Their modified network, shown in Figure 1.4, learns, and can classify sequences in the same vocabulary as training exemplars, in the same way that was described earlier. However, when presented with sequences in a new vocabulary the core weights are frozen. The second vocabulary mapping weights between the input and encoding layer are initially set at random values (as in the initial training phase), and the network begins to predict successive elements. But of course the internal patterns of activation will be constrained by the frozen weight changes experienced in the source vocabulary. Thus over a number of trials the network will induce the correspondences between the two vocabularies – in order to predict successive elements in the new vocabulary given the 'frozen grammar', the system must, in effect, induce such correspondences. Critically, knowledge is not mapped between vocabularies, but between the properties of vocabulary elements, such as what elements they depend upon and can predict.

Of course, as in the source vocabulary, the system cannot correctly predict which elements can follow which others in the first few novel vocabulary sequences. Just as the network's representation of the grammar is an approximation of the actual grammar, the mapping between the two vocabularies can only ever be an approximation of the actual mapping. Consequently the correct classification of sequences in the novel vocabulary will be lower than of those in the source vocabulary. This simulates the transfer deficit that was discussed at the beginning of this chapter. For example, Altmann *et al.* (Experiment 1) asked participants to classify sequences in either the same or a different modality than training exemplars. They observed that classification in a novel modality (55%) was 66% of that seen in the source modality (58%) relative to controls (49%). Just as the network matched human classification performance in the source modality, the 66% cost of switching modality at test was also accurately simulated. But what of the effects of similarity to stored training exemplars?

Brooks and Vokey (1991) had shown that, irrespective of their actual grammatical status, if a sequence was similar to an exemplar participants tended to call it grammatical, and if it was dissimilar participants tended to call it ungrammatical, but there remained a significant effect of actual grammatical status. They argued that sequences were classified by a comparison with unanalysed stored training exemplars, and transfer occurred because of analogies to common features such as the patterns of repeating elements. Dienes *et al.* (1999) found that their network could simulate the effects of grammaticality and similarity that Brooks and Vokey had observed. However, the networks' sensitivity to the similarity of test sequences was not due to an abstract analogy, not least because the network does not store whole sequences, but primarily because of the differences in the proportion of permissible and impermissible transitions in the subsets of sequences. As Perruchet (1994) noted, similar sequences will usually contain a greater proportion of legal fragments than will unfamiliar sequences.

The classification of sequences according to Brooks and Vokey's notion of abstract analogy requires the presence of repetition structures within a sequence. To test this idea Altmann *et al.* partitioned the data from their Experiment 3 and determined that participants were in fact able to correctly classify the 43% of sequences that did not contain any repeating elements. The network successfully simulated this effect, and in fact dependencies between repeating elements are processed no differently to dependencies between non-repeating elements. As in the case of the Brooks and Vokey data, this effect may have been due to imbalances in the frequency distribution of individual elements. This issue is discussed in Chapter 3. However, in the randomly changing transfer paradigm the patterns of repeating elements are the only way that participants can accurately discriminate between grammatical and ungrammatical sequences. Indeed Whittlesea and Dorken (1993, Experiment 5) had found that this kind of transfer was best when participants were instructed to study the patterns of repeating elements, rather than to simply memorise sequences. Dienes *et al.* also modelled these effects. To simulate instructions to learn repetition structures Dienes *et al.* replaced the input units with 'abstract feature units' that coded sequences in a similar way to THIYOS with the exception that the repeats did not need to be adjacent. The first unit coded whether the current element was a repeat of the previous element, the second coded whether the current element was a repeat of the element two elements previous, and so on. The memorisation task followed the same procedure outlined earlier. The behaviour of the network simulated the results obtained by Whittlesea and

Dorken where participants asked to study the patterns of repeats classified more sequences correctly than those who were asked to memorise the training exemplars.

How abstract is the information within this kind of network? The first and perhaps most important point is that the network learns to represent the rules of the grammar as sequential dependencies. That is, it encodes the abstract relationships between the vocabulary elements of training exemplars rather than stores the training exemplars themselves.[4] These abstractions can be applied to more than one previously unseen sequence. In fact this learning is statistical in nature – bigrams are learned because the component elements frequently co-occur. These dependencies increase in length (higher-order *n*grams) as learning proceeds. So although the vocabulary elements are encoded in the encoding and context layers (and might reflect narrow abstractions) it is the statistical relationships between those elements that are responsible for the correct classification of new sequences (and these might be referred to as broad abstractions). This blend of both broad and narrow abstraction is consistent with the literature. For example, Gomez (1997) had found that participants could only classify a sequential dependency in a novel vocabulary if they could remember it (as it should be) in the source vocabulary. Thus the contingent relationships between the component elements of a sequential dependency can only be transferred to a novel vocabulary if one has a representation of the original component elements themselves.

1.3.4 Conclusions

In general, all of the models are capable of correctly classifying the stimuli that they were designed to. However, the computational models reviewed support the distinctions between broad abstractions pertaining to dependencies between repeating elements applied on a sequence-by-sequence

[4] Whether or not a neural network stores whole exemplars or *n*gram is dependent

basis (THIYOS and exemplar accounts), and narrow abstractions pertaining to dependencies between non-repeating elements applied across sequences (Competitive Chunking, Toy Models, and the SRN). Clearly these distinctions provide empirical tests that could resolve the issue of how artificial languages are represented. Recently Gomez *et al.* (in press) have shown that if a language does not contain repeating elements then participants are able to classify them in the source but not the novel vocabulary. The SRN model described by Dienes *et al.* (1999) would, presumably, predict that participants *should* have been able to transfer that knowledge. Gomez *et al.*'s (in press) finding would however be consistent with both the THIYOS and the exemplar models that would not predict transfer in a language without repeating elements. The assumption shared by the fragment learning and neural network models that participants classify sequences in the novel vocabulary on the same basis as sequences in the source vocabulary is critical distinguishes them from the exemplar models and THIYOS. The implication of this assumption is that these models do not distinguish between sequential dependencies between repeating and non-repeating elements. However, although THIYOS and exemplar models predicate source vocabulary performance upon dependencies between repeating and non-repeating elements, transfer is predicated upon dependencies between repeating elements alone. This empirical issue is explored in Chapter 5.

1.4 DISCUSSION

This chapter has reviewed some of the empirical and computational data concerning artificial grammar learning. The specific focus has been on the transfer of grammatical knowledge to new vocabularies. This effect has typically been associated with abstract knowledge. Two meanings of abstract were discussed, abstraction of rules from training exemplars, and knowledge

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upon the parameters of the network.

independent of perceptual form. In fact, this transfer effect could be consistent with both exemplar *and* fragment memorisation, because on inspection they are both predicated upon the representation of sequential dependencies. The issue of whether that knowledge is independent or dependent of perceptual form is less clear, because of course both modes of representation are amenable to transfer to novel situations. Hahn and Chater (1997, 1998) have argued that whilst there might be a distinction between abstract- and instance-based representation in terms of computational modelling and artificial intelligence, they may not be so distinct in human cognition. Even if there were such a distinction, there are problems in defining rule-based and instance-based representation. Any category member must be defined in terms of its overall similarity to other training exemplars or some prototype. Similarly, if rules encompass defining features (in the case of artificial grammar learning a defining feature might be 'all sequences begin with *M* or *T*') they must also include graded similarity in terms of how many defining features they encode, how strong they are, and how applicable to a potential category member they might be. Thus we might always be faced with either a tautological or an intractable argument if we conceptualise representation in terms of these kinds of dichotomies. If there is to be a distinction between instance memorisation and rule abstraction classical architectures could only instantiate it in a non-unitary architecture. This is the same concern that Cleeremans (1997) voices concerning the implicit-explicit distinction. In contrast, connectionist architectures lie somewhere in the middle; they do not abstract rules nor do they memorise instances. They do, however, behave in ways that are consistent with both descriptions of representation. The SRN discussed earlier is capable of modelling many, if not all, of the effects that have been discussed in this review. Essentially, because representation in neural networks is sub-symbolic such models circumvent many of the traditional distinctions in human cognition. As Barsalou (1990) notes, both exemplar and abstractionist models of categorisation can be functionally indistinguishable. For example, Estes' (1986) exemplar model of categorisation stores veridical sequences and when required to categorise a new stimuli computes the relative frequencies of particular features. So although the model learns veridical exemplars its categorisation process performs the kind of abstraction associated with rule abstraction during learning (such a model has not been proposed to account for the transfer of grammatical knowledge). Similarly, a model that performs rule- or feature-abstraction at the point of learning can reconstruct, and perhaps generate, valid category exemplars. It seems then, theoretical and computational assumptions about modes of representation can only be distinguished by prior empirical observations about what units of information are learned and how that knowledge can be transferred. A suitable unit of knowledge is the sequential dependency.

Focus on mapping and classification processes

A number of workers have argued that the focus of implicit learning research should be on *what* and *how* information is learned and represented (e.g. Perruchet & Gallego, 1997; Shanks & St. John, 1994; Cleeremans 1993). Partly this is motivated by marked failures to objectively demonstrate that learning can proceed without awareness and problems in defining rule-based and episodic-based learning. The view that implicit knowledge is implied knowledge retains the notion that we do not necessarily have to be aware of what is being learned whilst we are learning it, but circumvents problems associated with demarcating implicit and explicit processes. Similarly, if we define the units of knowledge as the sequential dependency we can circumvent the distinctions between episodic- and rule-based representation. The transfer effect in artificial grammar learning is important for this line of research because it indicates what information is available on different tasks rather than how it is represented. For example, knowledge of sequential dependence in the source vocabulary is often freely available to verbal report; it is also available for fluent processing on SRT tasks. Grammatical knowledge is clearly composed of rule-like abstractions (even if represented in episodic form), but these abstractions are narrow in the sense that they do not transfer easily. This is true whether or not we regard those abstractions as being statistical or symbolic. Even if knowledge of grammatical rules, is predicated on statistical information (as in the SRN), that knowledge can only be transferred if participants are able to induce the correspondences, on the basis frequency of occurrence and of co-occurrence of individual elements, between vocabulary elements. Similarly, symbolic rules can only be mapped in similar ways; one can only determine that one token corresponds to another if the two share the same abstract properties – frequency of occurrence and of co-occurrence. However, the finding that narrow abstraction of sequential dependencies between non-repeating elements does not support fluent processing (i.e. reaction time savings) of sequences in a novel vocabulary (c.f. Gómez, 1997) is of little concern – after all the transfer effect is primarily a categorisation effect. The question becomes, how do we determine that a perceptually novel sequence *is* a category member? The distinction between sequential dependencies of repeating and of non-repeating elements is important because close inspection of different computational models reveal that they each treat these dependencies in different ways. Consider the episodic models. Instance-based classification in a novel vocabulary proceeds by comparing each stimulus to one or a multitude of stored training exemplars. This procedure allows a mapping to be computed only between elements that repeat. Sequential dependencies between identical elements can sbe induced on a sequence-by-sequence basis, simply because the property of repetition is readily apparent despite changes in perceptual form. In contrast, fragment- or rule-based classification (if the abstractions are narrow) proceeds in a novel vocabulary by first determining which vocabulary elements each rule is applicable to. Many of the computational models discussed earlier assume that this is effected by either determining the most consistent mapping or inducing element-to-element (or fragment-to-fragment) correspondences on the basis of frequency across both the training and the test sets. Thus this mapping procedure does not occur on a sequence-by- sequence basis, although subsequent classification does. Clearly it is the classification of sequences in a novel vocabulary that is the important and tractable issue in artificial grammar learning.

The following chapters investigate these issues in more detail. Chapter 2 begins by attempting to replicate the transfer of grammatical knowledge from one vocabulary to another. Chapter 3 investigates if participants are able to classify sequences in a novel vocabulary when there are no illegal sequential dependencies between repeating elements. If participants can classify sequences under these conditions, what information are they transferring – knowledge of first- or higher-order dependencies? Chapter 4 looks for evidence of the transfer of second-order sequential dependencies between non-repeating elements that cannot be identified on the basis of frequency. Chapter 5 considers the interactions between participants' sensitivity to dependencies of identical and of non-identical elements and finds that participants do treat them in functionally distinct ways. Chapter 6 reviews the empirical work presented in the preceding chapters, and concludes with a speculative discussion on the implications of the data for theories of artificial grammar learning.

CHAPTER TWO

REPLICATING THE BASIC EFFECT

2.1 INTRODUCTION & OVERVIEW

This chapter begins with an exploratory study of the limits of the transfer of grammatical knowledge acquired in one vocabulary to classification of previously unseen sequences in another vocabulary. Experiment 1 compared the ability of participants to classify sequences presented in two vocabularies. However, the basic transfer effect did not appear as ubiquitous as either the rule-abstraction or instance-memorisation accounts suggest. Subsequently, Chapter 1 proceeds by determining the degree of *difference* between test sequences and exemplar sequences that is required for those test sequences to be classified as grammatical or ungrammatical according to the grammar that generated the exemplars.

In Experiment 1 participants were asked to study sequences of syllables that unbeknownst to them were generated by an artificial grammar. This grammar had previously been used by Altmann *et al.* (1995). Later participants were told that the sequences obeyed some simple rules and were asked to classify new sequences as either obeying those rule or not, in both the same vocabulary as the exemplars (syllables) and a different one (symbols). Despite significant discrimination in the same vocabulary as learning (relative to untrained controls), there was no evidence that the trained participants were able to transfer that knowledge to the novel vocabulary. Experiment 2 successfully replicated a study by Altmann *et al.* (1995, Experiment 4) that had also previously used the same grammar as Experiment 1. This indicated that the failure to replicate any transfer effect in Experiment 1 might have been due to some property of the ungrammatical sequences that made them indiscriminable from the grammatical test sequences. Experiment 3 questioned whether participants could discriminate between the grammatical sequences that Altmann *et al.* had used and the ungrammatical sequences used in Experiment 1. In this experiment participants were unable to discriminate between sequences in either the same or a different vocabulary to the exemplar sequences. Experiment 4 used the same grammatical exemplar and test sequences as Experiment 2, but used a new set of ungrammatical sequences that were more dissimilar (in terms of the overall proportion of illegal bigrams) to the exemplars than the ones used in Experiments 1 and 3 (but not Experiment 2). Experiment 4 investigated whether participants were sensitive to the proportion of illegal bigrams independently of first element legality. That is, unlike Experiment 2, none of the ungrammatical sequences began with an illegal starting element, but they did contain a higher proportion of illegal bigrams than Experiments 1 and 3. Participants were able to classify sequences in the same vocabulary as the exemplars but were again unable to transfer that knowledge to a novel vocabulary.

2.2 EXPERIMENT 1 A FAILURE TO REPLICATE THE TRANSFER OF GRAMMATICAL KNOWLEDGE TO A NOVEL VOCABULARY.

2.2.1 Introduction

Participants are relatively more sensitive to the well-formedness of test sequences when they are presented in the same vocabulary as the exemplar sequences, than when they are presented in a novel vocabulary. For example Altmann *et al.* (1995) found in their Experiment 4, that trained participants were able to classify 71% of test sequences correctly in the same vocabulary as the exemplars, but only 65% in a novel one. If we take the control participants ability to classify sequences as the minimum possible (51% in the source vocabulary and 49% in the novel vocabulary), and trained participants' classification performance as the maximum possible, then the classification of sequences in the vocabulary was 76% of that in the source vocabulary — a 24% cost of changing vocabularies. Similarly, Shanks *et al.* (1997) observed that participants (on a two-alternative forced-choice version of the task) were able to classify 77% of sequences in the same vocabulary as exemplars but only 73% in a novel vocabulary — 12% drop of performance (relative to controls who classified 44% of sequences correctly). The source of this transfer deficit is an unresolved question. Clearly it reflects an inability to transfer all of the knowledge that is available in one vocabulary to another, but there are a number of potential explanations for this deficit. For example, episodic accounts suggest that only familiar or similar sequences are endorsed (c.f. Brooks & Vokey, 1991; Whittlesea & Dorken, 1993), whilst abstractionist accounts suggest that the first few sequences are classified at chance whilst a consistent mapping is being induced across vocabularies (Dienes, Altmann & Gao, 1999). Experiment 1 begins the investigation of this phenomena using the same language (grammar and vocabulary) that Altmann *et al.* had used in their Experiment 4.

2.2.2 Method

Participants

Twenty-four members of the University of York participated in this study for either course credit or payment. Care was taken to ensure that no volunteer had participated in any other artificial grammar learning experiment.

Stimuli

Training and test sequences were generated by the grammar shown in Figure 2.1 and which had previously been used by Altmann *et al.* (1995). Thirty, out of a possible seventy-two, sequences were selected for the exemplar set to represent the different lengths of possible sequences and preserve the first-order statistics of the grammar. These sequences were presented four times in pseudo-random order, and instantiated with syllables (14 point uppercase Times Roman) according to the mapping shown in Figure 2.2, and presented in a five-page booklet for study. The four sequences generated by the grammar that were composed of just two elements were omitted from both the training and test sets.

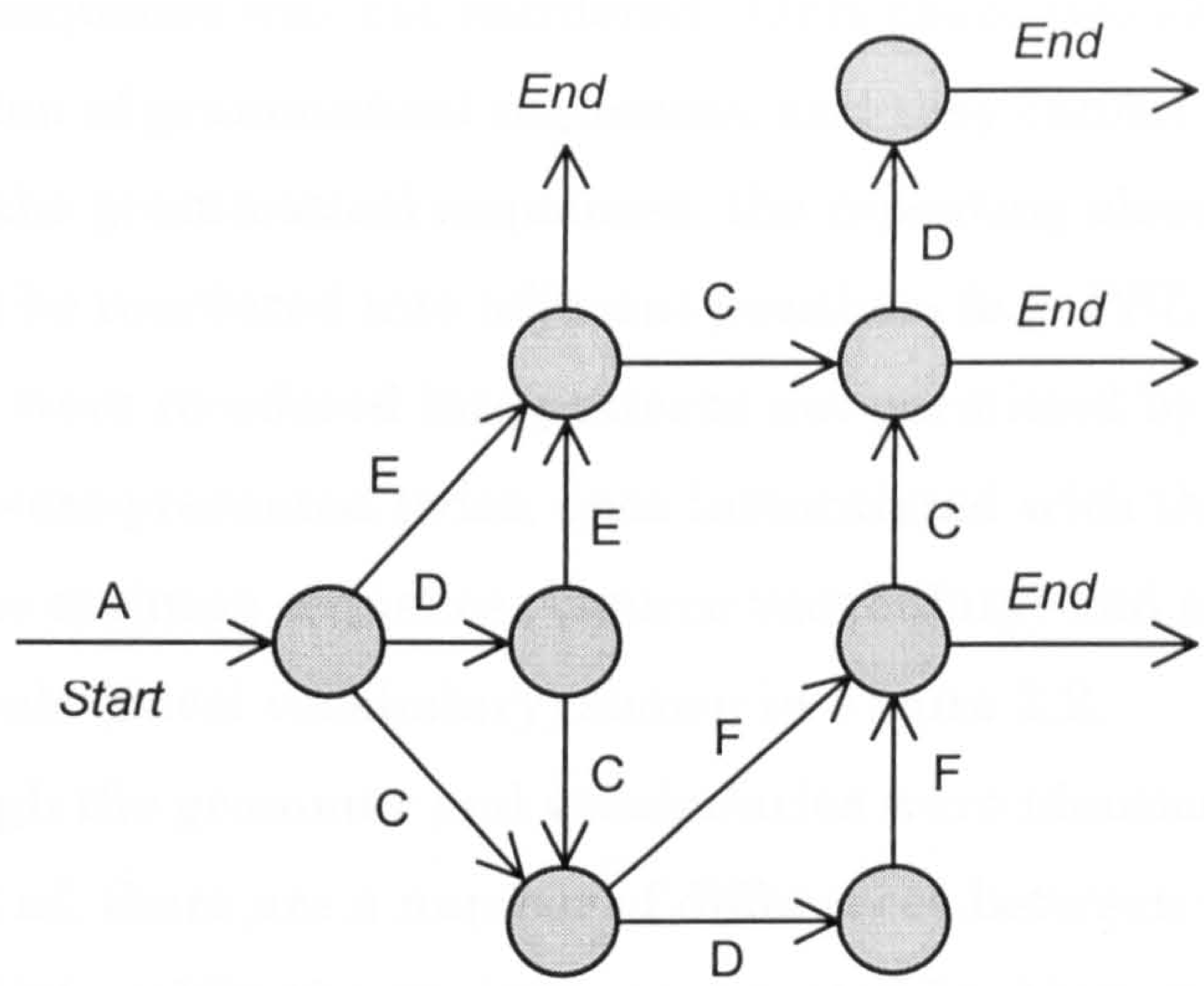
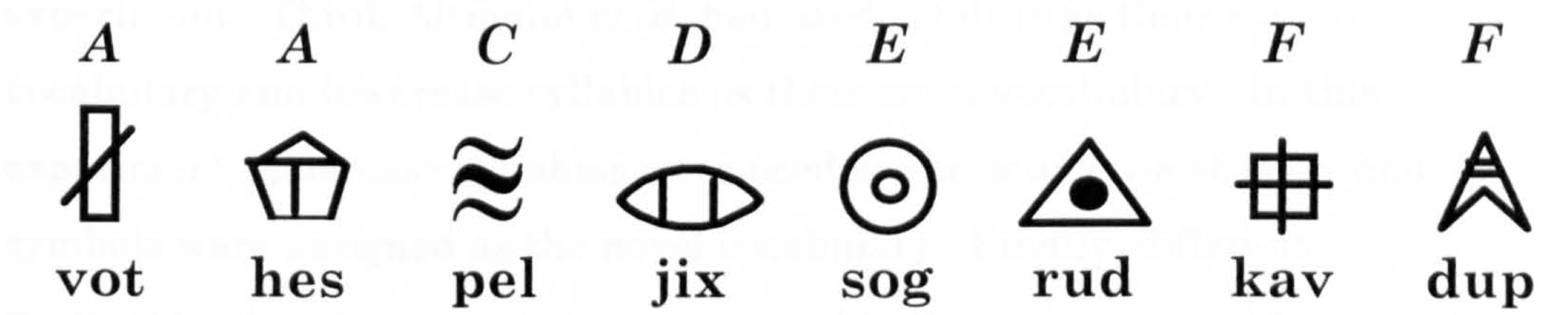


Figure 2.1: Finite-state grammar used in Experiments 1-4



Note that there are two elements that correspond to *A*, *E*, and *F*. This serves to increase the number of unique sequences that the grammar can generate.

Figure 2.2: The correspondence between the grammar and two vocabularies

The remaining thirty-eight grammatical sequences were assigned to the test set. The test set also included thirty-eight ungrammatical sequences. The thirty-eight ungrammatical test sequences were constructed by reordering two or more of the vocabulary elements of each grammatical test sequence. This preserved the first-order statistics of the grammatical sequences (that is each element occurred with an equal frequency in the grammatical and ungrammatical test sets). Two grammatical constraints were preserved in the ungrammatical sequences. The first element (*HES* or

VOT) of each sequence was not reordered. Only these two elements occur in the first position of grammatical sequences, and they cannot occur elsewhere. Second, as in the grammatical sequences, the repeating elements (*PEL* or *JIX*) could not be reordered into adjacent positions (e.g. *PEL PEL* or *JIX JIX*), although they were reordered into patterns not permitted by the grammar. The test sets were presented twice, once instantiated with the same set of syllables as the training sequences (source vocabulary) and once instantiated with the symbols (novel vocabulary) shown in Figure 2.2.

Although the grammar and vocabularies were identical to those used by Altmann *et al.* there are a number of differences between this experiment and theirs. First, unlike the experiment reported by Altmann *et al.* in which 35% of the ungrammatical sequences were constructed by reordering the starting element, the constraints upon which elements can begin a sequence were preserved. Nine ungrammatical sequences were identical to ones that Altmann *et al.* had used. Second, Altmann *et al.* had included sequences that contained less than three elements. These were omitted from this experiment. Third, Altmann *et al.* had used symbols as their source vocabulary and lowercase syllables as their novel vocabulary. In this experiment uppercase syllables were used as the source vocabulary and symbols were assigned as the novel vocabulary. Finally, different grammatical and sequences were assigned to the exemplar and test sets than the ones used by Altmann *et al.* The stimuli are presented in Appendix A.

Design

The design was identical to the one employed by Altmann *et al.* in their Experiment 4. This was a split-plot design with twelve participants assigned to each of the two training conditions (Trained and Untrained). The Untrained group, acting as controls, did not see the exemplar sequences and proceeded directly to the test phase, the Trained group was asked to study the exemplar sequences. Participants in each group were presented with the test sequences in both the same vocabulary as the exemplars (Source vocabulary) and a different one (Novel vocabulary). The order of test presentation was counterbalanced, half of all participants classified sequences in the source vocabulary first (syllables), and half classified sequences in the novel vocabulary first (symbols). The order in which sequences were presented in each vocabulary was different.

Procedure

Trained participants were presented with the exemplar sequences. They were instructed to learn as much about the sequences as they could:

> "In this experiment you will be given some sequences of syllables to look at. Please inspect them carefully. Try and learn as much about the sequences as you can, as you will be asked questions about them later. Once you have worked through all of the sheets, return to the first sheet and continue until the time is up."

After ten minutes the Trained participants were presented with a test booklet and informed that all of the sequences that they had just seen obeyed some simple rules of construction. They were asked to place a tick next to the sequences that they thought obeyed the same rules as the exemplars and to place a cross next to the ones that they thought disobeyed those rules. Untrained controls proceeded directly to the test phase without observing any exemplars. They were asked to indicate the sequences that they thought either obeyed or disobeyed some simple rules of construction.

2.2.3 Results

To determine whether participants were able to correctly classify grammatical and ungrammatical sequences two A' discrimination indices were calculated for each participant. One A' index as an estimate of discrimination acuity in the source vocabulary and another A' index as an estimate of discrimination acuity in the novel vocabulary. This index, like proportion correct, leaves the results on a 0-1.0 scale with a fixed chance point of .5, and can be interpreted as an estimate of what performance would have been in a two-alternative forced-choice version of the task (Donaldson & Good, 1996; Macmillan & Creelman, 1996). The percentages of correct classification scores were also calculated and are presented in Table 2.1, but these were not entered into statistical analyses.

		Test Vocabulary			
		Source (syllables)		Novel (symbols)	
		mean	*se*	mean	*se*
Training					
Untrained		51%	1.39	49%	2.39
	A'	.52	.03	.48	.04
	B'	-.12	.07	-.06	.07
	Hits	.54	.02	.51	.04
	False alarms	.52	.03	.53	.02
Trained		57%	2.22	52%	2.30
	A'	.61	.03	.54	.04
	B'	-.41	.11	-.46	.10
	Hits	.70	.04	.66	.04
	False alarms	.56	.04	.61	.04

Table 2.1: Mean Percentages of correct classification scores and discrimination indices by training condition and test vocabulary for Experiment 1

The *A'* data were entered into a split-plot ANOVA[1] with one between-subjects factor Training with two levels (Trained or Untrained), and one within-subjects factor Vocabulary with two levels (Source and Novel). There was no effect of Training ($F(1, 22) = 3.38$, $p = .08$, $MS_e = .02$, $\eta^2 = .13$)[2], no effect of Vocabulary ($F(1, 22) = 3.65$, $p = .07$, $MS_e = .01$, $\eta^2 = .14$), nor was there an interaction between the two ($F < 1.0$).

However, simple main effects revealed an effect of Training in the Source vocabulary ($F(1, 22) = 4.50$, $p < .05$, $MS_e = .01$, $\eta^2 = .17$), but did not reveal any effect of Training in the novel vocabulary ($F(1, 22) = 1.04$, $p = .32$,

[1] A two-tailed criterion was used for this and all subsequent experiments.
[2] Eta squared (η^2) can be interpreted as the proportion of the total variability in the dependent variable that is accounted for by variation in the independent variable. It is calculated as the ratio of the between-groups sum of squares to the total sum of squares. The effect size *f* can be readily found using the formula $\sqrt{(\eta^2/1-\eta^2)}$.

$MS_e = .01$, $\eta^2 = .05$). Subsequent analyses revealed no effects of, nor interactions with, order of test presentation (all Fs < 1.0).

2.2.4 Discussion

Experiment 1 failed to replicate the transfer of grammatical knowledge to a novel vocabulary under implicit learning conditions. This is in contrast to numerous published examples of the transfer effect (e.g. Altmann *et al.* 1995; Gomez & Schvaneveldt, 1994; Manza & Reber, 1997; Shanks *et al.* 1997). Participants clearly did learn something of the exemplars because they were able to discriminate between grammatical and ungrammatical sequences in the source vocabulary, but they were unable to apply that knowledge to the same sequences instantiated in the novel vocabulary. This experiment used the same grammar and vocabulary elements that Altmann *et al.* had used in their Experiments 3 and 4. In this Experiment however, the source vocabulary was uppercase syllables and the novel vocabulary was symbols. In their Experiment 4 Altmann *et al.* had used the symbols as their source vocabulary and lowercase syllables as their novel vocabulary. One possible explanation for this failure to replicate their result is that transfer between these vocabularies might be asymmetric, perhaps as a consequence of differential encoding of the two vocabularies. This conclusion is unlikely because Altmann *et al.* observed (in their Experiment 3) cross-modal transfer from vocal syllables to the visual symbols.

The trained participants exhibited some knowledge of the grammatical rules, as indexed by their ability to discriminate between grammatical and ungrammatical sequences in the source vocabulary. It seems likely that they failed to induce a mapping between the two vocabularies and were consequently unable to discriminate between grammatical and ungrammatical sequences. An alternative explanation might be that if participants use different knowledge to classify sequences in the two vocabularies, then that knowledge was unavailable in the novel vocabulary. For example, the THIYOS model (Mathews *et al.* 1989) assumes that participants are able to utilise knowledge of rules regarding non-identical vocabulary elements in the source vocabulary, but are only able to transfer knowledge of rules regarding patterns of adjacent repeating elements. Although this grammar did not permit adjacent repeats, Altmann *et al.* did observe significant transfer. These two explanations are not distinguished in this experiment. What seems clear is that the transfer effect in artificial grammar learning is not an altogether ubiquitous effect and cannot be entirely predicated, if at all, on knowledge that is abstract in the sense that it is independent of vocabulary. To recapitulate, Experiment 1 failed to replicate the transfer of grammatical knowledge to a novel vocabulary using a language that had previously been used by Altmann *et al.*, who did observe significant transfer. It seems appropriate that Experiment 2 should attempt to replicate the implicit learning condition of Altmann *et al*'s (1995) Experiment 4.

2.3 EXPERIMENT 2
A REPLICATION OF ALTMANN *ET AL.* (1995, EXPERIMENT 4).

2.3.1 Introduction

Contrary to expectations, Experiment 1 found no evidence of the transfer of grammatical knowledge under implicit learning conditions. Although the transfer effect in artificial grammar learning is a robust effect that has been observed many times, there has not been a previous attempt to replicate the effect using the exact stimuli that Altmann *et al.* had used in their Experiment 4. A replication of that study seems appropriate to explore whether the failure to demonstrate transfer in Experiment 1 was a consequence of some property of the grammar or vocabularies that were used.

The stimuli used in Experiment 2 here were identical to the ones used by Altmann *et al.* and differed from those used in Experiment 1 in a number of respects. First, participants were trained on a different set of exemplar sequences that were composed of symbols rather than syllables. Second, a different set of ungrammatical sequences was used, a proportion of these

(35%) violate the constraint that only the two elements (*hes* or *vot*) may begin a sequence and may not occur elsewhere in a sequence. A number of workers have observed that participants are relatively more sensitive to the initial and terminal portions of a sequence (e.g. Reber & Lewis, 1977; Perruchet & Pacteau, 1990). The four grammatical sequences that were composed of only two elements were included. Finally, a different set of grammatical test sequences was used. Procedurally however, Experiment 2 was identical to the Trained condition of Experiment 1.

2.3.2 Method

Design

This was a repeated-measures design. All ten participants undertook the same training phase before proceeding to the test phase. The order of test phase presentation was counterbalanced, half the participants classified sequences of symbols first followed by the syllable sequences, whilst the other half classified syllable sequences first followed by the sequences of symbols.

Participants

Ten participants took part in this study for either payment or course credit. Care was taken to ensure that no volunteer had previously taken part in any artificial grammar learning experiment.

Stimuli

The stimuli were identical to those used by Altmann *et al.* (1995; Experiment 4). Seventy sequences were generated from the grammar shown in Figure 2.1. The allocation of grammatical training and test sequences was identical to Altmann *et al*: The same thirty sequences were assigned to the training set and forty sequences were assigned to the test sets. The frequency of occurrence of sequences of individual elements, and of sequences of different lengths, were kept constant, proportionately across training and test sets.

For the test sets, the same forty ungrammatical sequences were used that Altmann *et al.* had used. These were generated by re-ordering each grammatical test sequence. This ensured that the frequency of occurrence of individual elements within sequences of different lengths matched those of the grammatical test sets. As mentioned earlier 35% of the ungrammatical sequences began with an element that the grammar does not permit at the beginning of a sequence. The sequences assigned to the training set were instantiated as sequences of symbols according to the mapping shown in Figure 2.2. The ungrammatical and grammatical sequences assigned to the test sets were instantiated as both sequences of symbols (source vocabulary) and sequences of lowercase syllables (novel vocabulary). These were presented in response booklets in different orders. The stimuli can be seen in Appendix A.

Procedure

The procedure was identical to the Trained condition used in Experiment 1 and the Symbols condition used by Altmann *et al.* (1995; Experiment 4). Participants were presented with the thirty training sequences repeated four times in a seven page booklet for study. Training sequences were instantiated with symbols according to the mapping shown in Figure 2.2. There were approximately eighteen sequences per sheet. Participants were allowed ten minutes to study the booklet, and asked to try and learn everything that they could about the sequences since they would be asked questions later.

In the test phase participants received two response booklets that each contained the eighty test sequences. Half the participants received symbols sequences first and half received syllable sequences first. Participants were informed that all the sequences that they had seen during the training phase were generated by a set of rules, and that they were now required to indicate which of the sequences in the response booklets either obeyed or disobeyed those rules.

2.3.3 Results

The percentages of correct classification scores are shown in Table 2.2, also shown are the same figures observed by Altmann *et al.* for comparison.[3]

The A' values revealed that discrimination in the source vocabulary was reliably greater than would be expected by chance (.5) alone ($t(9) = 7.50$, $p < .01$), discrimination in the novel vocabulary was also greater than chance ($t(9) = 4.25$, $p < .02$). In contrast to Altmann *et al.*, discrimination in the novel vocabulary was not reliably less than discrimination in the source vocabulary ($t(9) = 1.26$, $p = .24$). Subsequent analyses revealed no effect of order of test presentation ($F < 1.0$).

		Test Vocabulary			
		Source (symbols)		Novel (syllables)	
		mean	*se*	mean	*se*
Experiment 2					
Trained		**70%**	3.56	**63%**	3.35
	A'	.78	.04	.71	.05
	B'	-.42	.12	-.41	.17
	Hits	.80	.05	.74	.07
	False alarms	.40	.05	.47	.03
Altmann et al					
Symbols		**70%**	2.50	**65%**	3.02
	A'	.78	.03	.73	.04
	B'	-.03	.18	-.19	.18
	Hits	.69	.05	.68	.07
	False alarms	.31	.04	.41	.05

Table 2.2: Percentages of correct classification scores and discrimination indices for Experiment 2 and for Altmann *et al.* (1995, Experiment 4)

[3] Altmann *et al.* also reported data observed by a control group who did not reliably differ from chance performance.

2.3.4 Discussion

Experiment 2 successfully replicated the transfer of grammatical knowledge to a novel vocabulary under implicit learning conditions using the same stimuli that Altmann *et al.* had used in their Experiment 4. Clearly the failure to replicate this effect in Experiment 1 was not a peculiarity of either the grammar or the vocabularies that were used. For example, participants were able to transfer knowledge that could not have been predicated upon patterns of *adjacent* repeating elements (c.f. Mathews & Roussel, 1997). Is the difference between participants' discrimination acuity in Experiments 1 and 2 due to some property of the ungrammatical sequences? A proportion (35%) of the ungrammatical sequences used in Experiment 2 began with an illegal starting element, whilst those used in Experiment 1 did not. Alternatively, the difference in participants' discrimination acuity could have been due to the grammatical test sequences that were used. In Experiment 1, trained participants' hit rate was .61 in the source vocabulary and .54 in the novel vocabulary. In Experiment 2, trained participants' hit rates were substantially higher in both the source (.80) and in the novel vocabulary (.74). In Experiment 3 below participants were required to discriminate between the grammatical test sequences used in Experiment 2 and the ungrammatical sequences used in Experiment 1. If participants are unable to discriminate between these particular grammatical and ungrammatical test sequences, then the differences between Experiments 1 and 2 can only be attributed to some property of the ungrammatical sequences.

2.4 EXPERIMENT 3
A SECOND FAILURE TO REPLICATE THE TRANSFER OF GRAMMATICAL KNOWLEDGE

2.4.1 Introduction

In Experiment 3, the same grammatical sequences are allocated to the training set and test sets as Experiment 2. The ungrammatical sequences that were used in Experiment 1 replace those that were used in Experiment 2. If participants are unable to discriminate between these particular grammatical and ungrammatical test sequences, then the differences between Experiments 1 and 2 can only be attributed to some property of the ungrammatical sequences. In particular the proportion of illegal bigrams in the ungrammatical sequences used in Experiment 1 (.16) was lower than in the ungrammatical sequences used in Experiment 2 (.31). This difference was partly due to 35% of the ungrammatical sequences in Experiment 2 beginning with an illegal starting element, whilst none of the ungrammatical sequences used in Experiment 1 did so.

2.4.2 Method

Design

This was a split-plot design. Participants were trained on either sequences of symbols or received no training. All participants were tested on both sequences of symbols and syllables. The order of test presentation was counter balanced.

Participants

Twenty members of the University of York participated in this study for either course credit or payment. No volunteer had taken part in any previous artificial grammar learning experiment.

Stimuli

The thirty grammatical training and forty grammatical test sequences were identical to those used in Experiment 2. Thirty-eight of the ungrammatical test sequences were identical to those used in Experiment 1, with the addition of two sequences that contained only two elements to compensate for the inclusion of grammatical test sequences with only two elements. No ungrammatical sequences began with an element that was not permitted by the grammar. The exemplar sequences were instantiated with symbols and presented four times in a booklet for study. The eighty test sequences were presented twice, once instantiated with symbols and once instantiated with syllables. The stimuli are presented in Appendix A.

Procedure

Participants were assigned to one of two training conditions (Trained and Untrained). These training conditions were identical to the Trained and Untrained conditions of Experiment 1. Trained participants were presented with the exemplar sequences in a seven page booklet, and asked to learn as much about the sequences as they could. Participants in the Untrained group acted as controls and proceeded directly to the test phase. The test phase was identical to the conditions of Experiments 1 and 2. Trained participants were informed that all the sequences that they had just seen obeyed some simple rules, and that they would now see some new sequences that either obeyed or violated those rules. Untrained participants were asked to identify sequences that they thought either obeyed or disobeyed some simple rules. Half the participants classified symbols sequences first, whilst the other half classified syllable sequences first before proceeding to the symbol sequence.

2.4.3 Results

The percentages of correct classification scores and related indices of discrimination and bias are shown in Table 2.3.

		Test Vocabulary			
		Source (symbols)		Novel (syllables)	
		mean	*se*	mean	*se*
Training					
Untrained		53%	1.93	45%	1.83
	A'	.55	.04	.44	.03
	B'	.12	.16	.18	.18
	Hits	.49	.05	.41	.05
	False alarms	.44	.05	.47	.06
		54%	3.16	47%	1.47
Trained	*A'*	.56	.05	.44	.03
	B'	-.25	.20	-.16	.21
	Hits	.60	.08	.50	.07
	False alarms	.53	.05	.56	.07

Table 2.3: The percentages of correct classification scores and discrimination indices by training condition and test vocabulary for Experiment 3

The *A'* data were entered into a split-plot ANOVA with one between-subjects variable Training (Trained or Untrained) and one within-subjects variable Test Vocabulary (Symbols and Syllables). Whilst there was no overall effect of Training ($F < 1.0$), there was an effect of Test Vocabulary ($F(1, 18) = 6.51, p = .02, MS_e = .02, \eta^2 = .27$), but no interaction between the two ($F < 1.0$). Subsequent analyses revealed no effects of, nor interactions with order of test presentation (all *F's* < 1.0). Planned comparisons revealed that Trained participants did not discriminate between grammatical and ungrammatical sequences any better than untrained controls in either the Source ($F < 1.0$), or the Novel vocabulary ($F < 1.0$).

2.4.4 Discussion

Experiments 2 and 3 only differed in the ungrammatical test sequences that were used. In Experiment 2 trained participants were able to discriminate between grammatical and ungrammatical sequences in the same vocabulary as the exemplars and were also able to transfer that knowledge to a different one (70% and 63% correct respectively). Experiment 3 used the same exemplars and grammatical test sequences as Experiment 2, but included the ungrammatical sequences that participants were unable to correctly reject in Experiment 1. Participants in Experiment 3 were unable to discriminate between the two sets of test sequences in either the same vocabulary as the exemplars or the novel one (54% and 47% respectively). Since in all other respects, Experiment 3 was identical to Experiment 2, the difference in participants ability to discriminate between the grammatical and ungrammatical sequences can only be attributed to some property of the ungrammatical sequences.

Clearly this set of ungrammatical sequences is not discriminable from the grammatical test sequences used in Experiment 2 in either the source or the novel vocabulary. This set of ungrammatical sequences differed from the set used in Experiment 2 in one important respect: each ungrammatical sequence began with one of the two grammatical starting elements (*hes* or *vot*), whereas 35% of those in Experiment 2 began with one of four other elements. Because no ungrammatical sequence in Experiment 3 began with an illegal starting element the overall proportion of illegal bigrams in the ungrammatical sequences was reduced relative to that in Experiment 2 (.16 vs. .31 respectively). However, the issue of chunk strength (the frequency with which bigrams occurring in the exemplars also occurred in a test set) is separable from the issue of the first element. A number of workers have claimed that chunk strength is a major determinant of whether those sequences are likely to be classified as grammatical or not (e.g. Perruchet, 1994; Redington & Chater, 1996; Shanks *et al.* 1997). Typically, measures of chunk strength consider the initial and terminal fragments of a sequence as being particularly salient (e.g. Higham, 1997a). However, participants may reject sequences that begin with an illegal starting element on that basis alone (i.e. an illegal first-order dependency), and not because it forms an illegal bigram with other sequences (i.e. an illegal second-order dependency).

If participants are sensitive to the frequency with which elements occur in the first position irrespective of what elements can follow, then that would *not* constitute grammatical knowledge of sequential dependence. If however, participants were shown to be sensitive to what elements can follow that element (or any other) then that would imply knowledge of grammar in the sense of second- or higher-order dependencies (chunk strength). The final experiment of Chapter 2 is concerned with chunk strength as a cue to ungrammaticality whilst keeping the first element grammatical and constant. In Experiment 4 the proportion of illegal bigrams was increased as much as was possible without violating the first element constraint to determine whether chunk strength alone can mediate transfer.

2.5 EXPERIMENT 4
THE PROPORTION OF ILLEGAL BIGRAMS INFLUENCES CLASSIFICATION IN THE SOURCE BUT NOT THE NOVEL VOCABULARY

2.5.1 Introduction

This experiment was identical to Experiment 3 with the exception that a new set of ungrammatical sequences was constructed. These differed from the ungrammatical sequences used in Experiment 3 in that they contained a greater proportion of illegal bigrams but did not violate the constraint that only two elements (*hes* or *vot*) may begin a sequence, and may not occur elsewhere in a sequence. If participants are sensitive to the overall proportion of legal and illegal bigrams independently of the first element constraint, then participants should be able to discriminate between grammatical and ungrammatical sequences. If, however, participants are unable to discriminate between sequences in the novel vocabulary then the transfer effect observed in Experiment 2 can almost certainly be attributable to the frequency with which different elements occur in the first position.

2.5.2 Method

Design

The design was identical to that used Experiment 3.

Participants

Twenty members of the University of York participated in this study for either course credit or payment. No volunteer had taken part in any previous artificial grammar learning experiment.

Stimuli

The thirty grammatical training and forty grammatical test sequences were identical to those used in the Experiments 2 and 3. A new set of forty ungrammatical sequences was constructed that contained a greater proportion of illegal bigrams (.21) than in Experiment 3 (.16). It was not possible to raise the proportion of illegal bigrams to the level of the ungrammatical sequences used in Experiment 2 (.31) without violating the constraints that the first element of each ungrammatical sequence must be grammatical and no repeating elements may be adjacent. The stimuli are given in Appendix A.

Procedure

The procedure was identical to that used in Experiment 3.

2.5.3 Results

The percentages of correct classification scores and related indices of discrimination and bias are given in Table 2.4.

		Test Vocabulary			
		Source (symbols)		Novel (syllables)	
		mean	*se*	mean	*se*
Training					
Untrained		49%	1.78	47%	2.78
	A'	.50	.03	.45	.05
	B'	-.02	.18	.16	.22
	Hits	.49	.06	.42	.09
	False alarms	.51	.05	.47	.06
		62%	3.64	49%	1.95
Trained	*A'*	.69	.05	.48	.04
	B'	-.49	.10	-.30	.12
	Hits	.74	.05	.56	.04
	False alarms	.50	.05	.59	.04

Table 2.4: The percentages of correct classification scores and discrimination indices by training condition and test vocabulary for Experiment 4

The *A'* data were entered into a split-plot ANOVA with one between-subjects variable Training (Trained or Untrained) and one within-subjects variable Test Vocabulary (Symbols and Syllables). There was an effect of Training ($F(1, 18) = 8.37, p = .01, MS_e = .02, \eta^2 = .32$), an effect of Test Vocabulary ($F(1, 18) = 7.65, p = .01, MS_e = .02, \eta^2 = .30$), and a marginal interaction between the two ($F(1, 18) = 3.90, p = .06, MS_e = .02, \eta^2 = .18$). Subsequent analyses revealed no effects of, nor interactions with order of test presentation (all Fs < 1.0). Simple main effects revealed that Trained participants were able to discriminate between grammatical and ungrammatical sequences better than untrained controls in the Source Vocabulary ($F(1, 18) = 13.54, p < .01, MS_e = .02, \eta^2 = .42$), but were unable to discriminate between grammatical and ungrammatical sequences any better than untrained controls in the Novel Vocabulary ($F < 1.0$). An increase in the proportion of illegal bigrams alone did enhance participants' ability to correctly reject ungrammatical sequences in the source vocabulary but did not enhance discrimination in the novel vocabulary.

2.5.4 Discussion

Experiment 4 reinstated the implicit learning effect in the source vocabulary that was absent in Experiment 3, but again failed to replicate the transfer of that knowledge to a novel vocabulary. In the source vocabulary this effect can be attributed to the increased proportion of illegal bigrams in the ungrammatical sequences relative to the ungrammatical sequences used in Experiments 1 and 3. This failure to demonstrate transfer cannot be attributed to a floor effect. If the 13% advantage enjoyed by trained participants' classification performance over controls in the source vocabulary is taken as the ceiling against which to compare magnitude of the transfer effect, the cost of changing test vocabularies is 85%, substantially greater than the 24% cost that Altmann *et al.* (1995) observed in their Experiment 4. Participants may not be sensitive to the proportion of illegal bigrams in a novel vocabulary independently of how frequently an element begins a sequence. Experiment 2 did demonstrate reliable transfer to an ungrammatical set of sequences that contained a high proportion illegal bigrams as a consequence of sequences that began low frequency starting elements in Experiment 2. Chapter 3 investigates participants' sensitivity to the legality and frequency of elements that begin sequences.

2.6 CHAPTER SUMMARY

The experiments described in this chapter, with the notable exception of Experiment 2, consistently failed to demonstrate the transfer of grammatical knowledge to a vocabulary that is different to the one where that knowledge was acquired. Experiment 1 replicated the finding that following exposure to exemplars participants can classify previously unseen sequences as being grammatical (according to the exemplars) or not. However, participants were only able to do this when the test sequences were instantiated with the same vocabulary elements as the exemplars (57% correct); they were not able to transfer that knowledge to a novel vocabulary (52%). Experiment 2 was a successful replication of Altmann *et al.* (1995, Experiment 4) and provided a base against which to compare subsequent experiments, because in this case there was evidence for the transfer of grammatical knowledge to a novel vocabulary (63% correct). Experiment 3 used the same grammatical exemplar and test sequences that were used in Experiment 2, but replaced the ungrammatical sequences with the ones that had been used in Experiment 1. Under implicit learning conditions participants were unable to discriminate between these sequences in either the same vocabulary as the exemplars (54%) or the novel one (47%), relative to untrained controls (53% source and 45% novel). The only difference between Experiment 2 (that showed a learning effect in both vocabularies) and Experiment 3 (that did not show a learning effect in either vocabulary) was the ungrammatical sequences. In Experiment 2 some sequences (35%) began with an illegal starting element, but none did so in Experiment 3. Reordering the vocabulary elements of each grammatical test sequence created the ungrammatical sequences in Experiment 2. In 35% of these sequences the two elements that occurred in the first position were reordered. Experiment 4 aimed to determine whether the difference in discrimination acuity between the two sets of sequences could be attributed to the difference in the overall proportion of illegal bigrams between the two experiments independently of the difference in the violations to the first element constraint. The ungrammatical sequences in Experiment 4 contained a greater proportion of bigram violations than in Experiment 3. Participants were now able to discriminate between ungrammatical and grammatical test sequences in the same vocabulary as the exemplars but were still unable to transfer that knowledge to the novel vocabulary.

Chapter 1 argued that *n*gram knowledge should be considered grammatical knowledge. That is, as representations of the sequential dependencies between vocabulary elements abstracted from exemplar sequences during the training phase. How might episodic accounts of artificial grammar learning account for the experiments described in this chapter? Episodic accounts are typically predicated upon the similarity of the two sets of test sequences, both grammatical and ungrammatical, to the exemplar sequences. Although proponents of the episodic processing account of artificial grammar learning generally endorse the 'chorus of instances' mechanism, in practise similarity is generally calculated by nearest neighbour comparisons. For example, Vokey and Brooks (1992; see also McAndrews & Moscovitch,1985) called a test sequence similar (*near*) if it differed from an exemplar sequence by only one element in a particular location, and dissimilar (*far*) if it differed by two or more elements irrespective of whether those elements repeat or not. They have demonstrated that similar sequences tend to be endorsed as well-formed and dissimilar sequences tend to be rejected as ill-formed, in both the same and a different vocabulary to the exemplars. However, others have observed that there is usually a significant residual effect of chunk strength (Higham, 1997a, Knowlton & Squire, 1996). Table 2.5 shows the proportion of similar and dissimilar sequences within each of the two types of test sequences for each experiment. The reader will notice that these proportions do not differ to a degree that is likely to account for the differences in participants' discrimination acuity across Experiments 1-4.

Whittlesea and Dorken (1993, see also Brooks & Vokey 1991) suggested that sequences in one vocabulary could be classified as category members by comparison to exemplars in another vocabulary solely on the basis of patterns of repeating elements. Patterns of repeating elements (repetition structures) are preserved despite a change in (or a randomly changing) vocabulary, and do not require rule-abstraction. In order to determine whether a sequence contains familiar or unfamiliar repetition structure, elements that do not repeat within that sequence are discarded (even if they do repeat in other sequences) and their locations marked. For example the sequence *hes <u>pel</u> dup <u>pel</u> jix* might be encoded as -1-1- . If a test sequence contains the same pattern of repeats as an exemplar sequence it is classed as *familiar*, if it does not it is classed as *unfamiliar*. The differences between the proportions of familiar and unfamiliar repetition structures across Experiments 1-4 are shown in Table 2.5.

		Experiment 1		*Experiment 2*		*Experiment 3*		*Experiment 4*	
Dimension	*Test sets*	Gr	Ug	Gr	Ug	Gr	Ug	Gr	Ug
Nearest neighbour	near	.89	.26	.83	.25	.83	.30	.83	.22
	far	.11	.74	.17	.75	.17	.70	.17	.78
Repetition structures	familiar	1.0	.55	.81	.20	.81	.40	.81	.40
	unfamiliar	.00	.45	.19	.80	.19	.60	.19	.60
Proportion Bigrams	legal	1.0	.84	1.0	.69	1.0	.84	1.0	.78
	illegal	.00	.16	.00	.31	.00	.16	.00	.21

Table 2.5: The similarity of each test set to the exemplar sequences used in each experiment in Chapter 2 along three dimensions

For example, in Experiment 4 forty-one test sequences contained repetition structures. Of those twenty-one appeared in the grammatical sequences and twenty in the ungrammatical sequences. However, eight ungrammatical sequences (40% of those that contained repeats) contained identical repetition structures to the exemplar sequences and were thus *familiar*, twelve (60%) did not and were thus *unfamiliar*. Similarly, seventeen of the twenty-one grammatical test sequences (81%) contained *familiar* repetition structures and four contained *unfamiliar* ones (19%). In the novel vocabulary trained participants classified 58% of the forty-one test sequences that contained repetition structure in accordance with this measure of familiarity (ignoring actual grammatical status), whilst untrained participants classified only 53%. Of course, this analysis cannot be considered independent of nearest neighbour similarity and is likely to interact with the proportion of illegal/legal bigrams and so cannot be taken as definitive. However, as argued in Chapter 1, sensitivity to sequential dependencies between repeating elements would allow test sequences to be correctly classified in a novel vocabulary on an *item-by-item* basis. This issue is investigated in more detail in Chapter 5.

Chapter 3 investigates whether participants are able to transfer knowledge of sequential dependencies between non-repeating elements that can only be mapped *across* sequences. This chapter found little or no evidence that participants were sensitive to ungrammatical sequential dependencies in a novel vocabulary. Although Experiment 2 did find evidence of transfer, it is unclear whether that effect reflected the transfer of first-order dependency information pertaining to which elements can begin a sequence, or whether it reflected also the transfer of second-order dependency information pertaining to the proportion of illegal bigrams. Experiment 4 indicated that participants were unable to correctly reject sequences in the novel vocabulary that contained a relatively high proportion of illegal second-order dependencies independently of the constraint upon what elements can begin a sequence. Chapter 3 investigates in more detail whether participants are sensitive to which elements can begin a sequence (a first-order dependency) and whether they are also able to transfer knowledge of second-order dependencies.

CHAPTER THREE

TRANSFERRING THE IDENTITY OF THE FIRST ELEMENT

3.1 INTRODUCTION

Three out of the four experiments described in Chapter 2 found no evidence that participants are able to utilise knowledge abstracted from exemplar sequences in one vocabulary to classify new sequences in another vocabulary. Perruchet (1994) argued that where demonstrations of the transfer of grammatical knowledge had been reported in the literature, they were predicated on small effects that differed against chance (usually 50%) rather than against the discrimination of appropriate controls groups. However, Altmann *et al.* (1995) and other workers (e.g. Gomez & Schvaneveldt, 1994; Shanks *et al.*, 1997) have reported large transfer effects relative to appropriate control groups. For example, in their Experiment 4 Altmann *et al.* observed that participants trained in one vocabulary were able to classify 65% of test sequences correctly in a novel vocabulary relative to untrained controls who correctly classified a mere 49% of sequences. The results of that experiment were replicated in Experiment 2 of this thesis (63%). On what basis were participants able to discriminate between those particular ungrammatical and grammatical test sequences in the novel vocabulary in Experiment 2 and in the Altmann *et al.* example? One possible explanation involves two particular features of the ungrammatical sequences used by Altmann *et al.* which were also present in Experiment 2, but not present in Experiment 1: A proportion of the ungrammatical sequences began with a low frequency element (relative to the exemplars) in the first position of each sequence. As a consequence, the ungrammatical sequences in Experiment 2 also had a larger proportion of illegal bigrams than was present in Experiment 1. However, participants could in principle, have been sensitive to these features independently. This is an important issue; if participants are only able to reject sequences that begin with a low frequency element irrespective of the proportion of illegal bigrams, that would constitute evidence of the transfer of first- but not second-order sequential dependence. Such a finding would not support the view that grammatical knowledge is, in any theoretically important sense, responsible for the transfer effect in artificial grammar learning. However, Experiment 4 found that participants were unable to correctly reject a set of ungrammatical sequences in the novel vocabulary that contained a larger proportion of illegal bigrams than Experiments 1 and 3, although they were able to correctly reject such sequences in the source vocabulary. This does not, however, confirm the view that the transfer effect in artificial grammar learning is not predicated on the transfer of grammatical information. If participants' knowledge of sequential dependence consists of "narrow" abstractions that are tied to the source vocabulary, such knowledge might only be mapped onto a novel vocabulary by inducing the frequency of occurrence and co-occurrence of individual elements. Thus, if participants can induce the identity of the first element of a sequence on the basis of frequency information, that knowledge ought to assist in the mapping of other elements and their dependencies. This issue is investigated in this chapter. In the following experiments, half of the ungrammatical sequences begin with low frequency elements that are not permitted by the grammar to occur in that position, whilst the other half contain only illegal second-order dependencies whilst preserving the legality of the first element. Altmann *et al.* reported that participants were able to discriminate between ungrammatical and grammatical sequences when those ungrammatical sequences that began with an illegal first element were omitted from their analysis; this may have been because participants were able to reject sequences that contained unfamiliar repetition structures. Their procedure is essentially repeated in the subsequent experiments. However, as argued in Chapter 1, it is important to distinguish between dependencies between repeating and non-repeating elements. Experiment 2 and the experiment reported by Altmann *et al.* confounded these two forms of sequential dependence. This issue is discussed next.

3.1.1 The transfer of two forms of sequential dependence

Chapter 1 drew a distinction between two kinds of sequential dependencies that finite-state, and other grammars are capable of generating. These differ in the ways that knowledge of them could be mapped between two vocabularies. The first kind, sequential dependencies between repeating elements, forms the repetition structures of a grammar. This kind of knowledge may be abstract in the sense that it is independent of vocabularies, but could be transferred to a novel vocabulary either on the basis of memory for exemplars or on the basis of rule like knowledge. Only the latter case would necessarily be predicated on abstract grammatical knowledge. The second kind, sequential dependencies between non-repeating vocabulary elements are abstract in the sense that they are predicated on sequential rule-like knowledge, but appear not to be abstract in the sense that they are independent of vocabularies. If participants are to classify sequences on the basis of this latter form of dependency, they must first induce the correspondences between elements in the two vocabularies. Sensitivity to the first kind of sequential dependency does not imply sensitivity to the second.

The relevance of the dependency between identical elements is that the 'repetition patterns' that they form could in principle form the basis for distinguishing between sequences generated by the grammar (which obey the pattern), and ungrammatical sequences, which often do obey the pattern. Brooks and Vokey (1991), pointed out that there is a certain similarity between sequences such as *vot pel sog pel rud* and sequences such as *vot pel sog pel rud* which is not shared with sequences such as *vot pel sog rud pel* — the former can be characterised as sharing the repetition pattern _ X _ X _, which is distinct from the pattern associated with the third sequence _ X _ _ X. If no sequence in the grammar conformed to this last pattern, it could be classified as 'ungrammatical' on the basis of this difference. The task of inducing a mapping between a pattern of repeating elements in one vocabulary and another is relatively trivial. As the above example illustrates, the well-formedness of any sequence that contains repeating elements can be determined on the basis of whether that pattern had been previously seen in a training sequence. Hence, classification could proceed on a *sequence-by-sequence* basis and need not be based on knowledge induced *across* the test sequences. As mentioned earlier, if the assumption is that participants endorse *all* familiar and reject *all* unfamiliar repetition structures such knowledge is not rule-like because the representations responsible for endorsing grammatical sequences cannot generalise to new repetition structures.

Contrast this with the considerable problem of inducing a mapping between several elements that do not repeat within any sequence onto their counterparts in another vocabulary. The mapping of non-repeating elements across vocabularies, and subsequent classification of sequences containing them, can only be accomplished on the basis of knowledge induced *across* whole training and test sets. For example, if one *n*gram is highly frequent and another highly infrequent in the training set, and in the test set (presented in a novel vocabulary), one *n*gram is highly frequent and another highly infrequent, then the two pairs can be mapped onto each other on the basis of their statistical distributions across the training and test sets. Alternatively, the system could attempt to compute all possible mappings that allowed each successive sequence at test to be classified as grammatical (thus, it might compute all possible mappings that enabled sequence one to be classed as grammatical, and then compute which of these enabled sequence two to be classed as grammatical, and so on). Of course, such a mechanism would seem better adapted to situations that did not include ungrammatical stimuli. Nonetheless, in this case also, a mapping could only be effected by computing *across* test sequences.

Studies using randomly changing vocabularies (e.g. Manza & Reber, 1997; Whittlesea & Dorken, 1993) would seem to support the idea that participants are able to discriminate between grammatical and ungrammatical sequences solely on the basis of repetition structure. If each test sequence is instantiated with a different mapping between grammar and vocabulary, participants are nonetheless able to discriminate between grammatical and ungrammatical, when the only cue to grammaticality is whether or not the repetition pattern of each sequence was present in the exemplar sequences. Recently Gomez *et al.* (in press) have found that transfer (with fixed mapping between vocabularies) does not occur with an artificial language that does not contain repeating elements, despite participants being able to discriminate between grammatical and ungrammatical sequences in the source vocabulary.

In order to rule out the possibility that grammaticality judgements in the Altmann *et al.* (1995) study were being based simply on differences in repetition structure, Altmann *et al.* performed a *post-hoc* partitioning of their stimulus set and excluded all test sequences containing repeating elements. They calculated logistic *d'* measures on the remaining sequences (where *d'* is an index of discrimination acuity) and reported that discrimination remained reliable, relative to untrained controls, on the subset of sequences that did not contain repeating elements (amounting to 57% and 52% of sequences, for their Experiments 3 & 4 respectively). Altmann *et al.* argued, therefore, that participants in their experiments were able to apply knowledge that did not pertain only to repetition structure to sequences presented in a novel vocabulary. This of course does not rule out the possibility that such structure may be important in enabling the process that maps information derived from one vocabulary onto structure experienced in another. However, Gomez *et al.* (in press) reported a second experiment using a grammar that generated repetition structures in only one third of the grammatical sequences. Contrary to the findings that Altmann *et al.* reported in their post-hoc analyses, Gomez *et al.* did not find that transfer extended beyond those sequences that contained repetition structure. Clearly the experiments reported by Altmann *et al.*, and the replication of their Experiment 4 described in Chapter 2 (Experiment 2) are at odds with Experiments 1, 3 and 4, *and* the results that Gomez *et al.* observed. What is the difference between these experiments that could account for these discrepancies?

Altmann *et al.* (1995) pointed out that a subset of their ungrammatical stimuli started with symbols or syllables that were not 'legal' starting elements. That is, all the grammatical syllable sequences, started with the syllables *hes* or *vot*, but a proportion of the ungrammatical stimuli (25% and

35% in Experiments 3 and 4 respectively) started with one of *kav*, *dup*, *jix*, or *pel*. These two starting elements could not legitimately occur anywhere else in a sequence, nor could any other element replace them. This grammatical constraint was not violated when the ungrammatical sequences were constructed for the experiments in Chapter 2 (except Experiment 2). In the experiments reported there, excluding Experiment 2, each ungrammatical sequence began with one of the two legal starting elements, and these elements did not occur elsewhere in any sequence. It was for this reason in Experiment 4, that the overall proportion of illegal bigrams did not reach the proportion in Experiment 2. In principle, participants in Experiment 2 and in Altmann *et al.*'s experiments could have rejected any sequence that started with an element that, across all the test sequences, occurred only infrequently in initial position. To test this hypothesis, Altmann *et al.* (1995) carried out a second *post-hoc* partitioning of their stimulus sets and excluded all ungrammatical sequences containing an illegal starting element. Logistic *d'* values revealed, again, that participants could nonetheless discriminate, relative to controls, between grammatical and ungrammatical sequences when all the sequences started with legal starting elements. However, half of these sequences also contained illegal repetition structures. However, they could not assess these two factors independently; a proportion of sequences without repeating elements did contain illegal starting elements, and a proportion of sequences with legal starting elements violated repetition structure. The studies reported in Chapter 2 and by Gomez *et al.* (in press) did not use ungrammatical sequences that violated the grammatical constraints involving the first element of a sequence.

To recapitulate, Altmann *et al.* (1995) claimed, on the basis of their post-hoc partitioning of the data, that participants were able to discriminate between grammatical and ungrammatical sequences even when these sequences contained legal repetition structure and legal starting elements. There was, however, a degree of overlap between their partitions. Thus, whilst 65% of the ungrammatical sequences in their Experiment 4 begin with a legal element, half of these contained illegal repetition structure. Similarly, of the 48% of their ungrammatical sequences that did not contain repeats, 37% of these began with an illegal starting element (and hence, low frequency for that position). Altmann *et al.*'s partitions did not, after all, exclude the confounds that they sought to eliminate. Consequently, it is conceivable that the effects found by Altmann *et al.* and observed also in Experiment 2 were based on unfamiliar repetition structures and/or violations of the frequency distributions of elements in first position. Although sensitivity to the latter violation requires knowledge induced across the test set, it is possible that the transfer effects observed by Altmann *et al.* were not predicated on the transfer of knowledge pertaining to sequential dependencies between non-repeating elements. The relevance of the legality of the first element is that it is evidence of first-order dependency information that is abstract in the sense that it encodes frequency information, but it is not evidence of abstract knowledge of the sequentiality of second- or higher-order dependency information.

3.1.2 Overview of Experiments

In the experiments reported below, the design employed in Chapter 2 (and by Altmann *et al.*, 1995, Experiment 4) is repeated, but using substantially different ungrammatical items that allow the stimuli sets to be partitioned. In each experiment all the ungrammatical sequences preserve repetition structure – the sequential dependencies between identical elements generated by the grammar – to ensure that participants' classifications could not be based only, or at all, on an abstract analogy with exemplars. In Experiment 5, half the ungrammatical sequences began with an illegal starting element, whilst the other half violated dependencies other than those that involved the first element. As in Experiment 2 (and in Altmann *et al.*'s Experiment 4), a proportion of the sequences that were rendered ungrammatical by reordering the starting elements began with an element that can potentially repeat in other sequences but did not do so within those particular sequences. In order to establish whether participants rejected these sequences on the basis of the illegal placement of a potentially repeating element in position 1, Experiment 6 removed this particular cue.

In Experiment 6, the sequences that began with an illegal starting element did not begin with either of the two elements that formed repetition structures. However, sequences that begin with an illegal starting element could also be rejected because the legitimate starting elements were moved to positions not permitted by the grammar (only these two elements could begin a sequence, and could not occur elsewhere in a sequence). In order to determine whether participants were sensitive to starting element legality (or frequency) or to the constraint that the two starting elements could not occur elsewhere in a sequence, in Experiment 7 the first element in each sequence was masked to conceal its identity. In this case half the ungrammatical sequences could be rejected on the basis that they contain a misplaced starting element, whilst the other half could only be rejected on the basis of the illegal sequential dependencies between non-repeating elements other than those that involved the starting elements. Of course if participants identify a salient cue such as the first element they might neglect other less salient cues. In Experiment 8 all of the ungrammatical sequences began with a legal starting element, and did not contain any misplaced legitimate starting elements, as was the case in Experiments 2, 5 and 6. Neither could they be rejected on the basis that they contained unfamiliar repetition structures (as in Experiments 5 & 6). Hence, in Experiment 8, participants could only discriminate between ungrammatical and grammatical test sequences on the basis of sequential dependencies between non-repeating elements other than those that involve the starting elements. Finally, in Experiment 9 all the test sequences were essentially grammatical, but the frequency with which the sequence started with one element or another was manipulated. This demonstrated how the first element (at least) of each sequence could be mapped between vocabularies on the basis of frequency information abstracted from exemplars and induced across test sequences alone.

3.2 EXPERIMENT 5 SENSITIVITY TO FIRST ELEMENT ILLEGALITY CARRIES THE TRANSFER EFFECT IN THE ABSENCE OF ILLEGAL REPETITION STRUCTURE.

3.2.1 Introduction

Experiment 5 was similar to Experiment 2 (and Altmann *et al.*'s 1995, Experiment 4) with the exception that: sequences with fewer than three elements were omitted, and the remaining thirty-eight ungrammatical sequences contained two discrete subsets of ungrammatical sequences (nineteen each). In both subsets, each ungrammatical sequence was created by reordering one of the grammatical test sequences. The first subset, *Illegal Starters* were rendered ungrammatical by placing an illegal element in the first position. This had the inevitable consequence of moving the legitimate first element to elsewhere in the sequence. The second subset, *Legal Starters* were rendered ungrammatical by violating *n*grams other than those containing elements in the first position. Over both subsets, 21 (55%) of the ungrammatical sequences contained one or two elements that repeated elsewhere within the sequence (but they could never be adjacent). The location of repeating elements relative to the non-repeated elements conformed to the repetition structure generated by the grammar. This excluded the possibility that any of the ungrammatical stimuli could be classified as ungrammatical on the basis that they contained *unfamiliar* repetition structures.

Statistically reliable classification of test sequences (whether presented in the same or novel vocabularies) could in principle be achieved simply by rejecting *Illegal Starters*. However, if participants can reliably reject *Legal Starters* in the novel vocabulary condition, then some form of abstract knowledge other than repetition structure is being applied across items and across the vocabularies. These effects are investigated by calculating discrimination indices for each participant.

3.2.2 Method

Participants

Twenty-four members of the University of York participated in the study for either course credit or payment. Care was taken to ensure that volunteers had not previously taken part in any artificial grammar learning experiment.

Stimuli

Thirty sequences generated by the grammar shown in Figure 2.1 were assigned to the learning set. These were the same thirty sequences that had been employed in Experiment 2. These learning sequences were instantiated with symbols according to the mapping shown in Figure 2.2. These were repeated four times in varying order, and presented in a seven-page booklet for study (see Appendix A). The same thirty-eight grammatical test sequences that were used in Experiment 2 were used as the grammatical test set, with the exception that sequences composed of only two elements were omitted. The frequency of occurrence of individual vocabulary elements and the frequency of occurrence of sequences of different lengths were kept constant, proportionally, across the learning and test sets.

Thirty-eight ungrammatical sequences were generated by reordering the elements of each grammatical test sequence, preserving the frequency of occurrence of individual elements. There were two types of ungrammatical sequence (nineteen of each), those with illegal starting elements and those without illegal starting elements. The first type, *Illegal Starters,* were generated by interchanging the legitimate first element of each sequence with another element elsewhere in that sequence. In grammatical sequences, the legitimate first element (*hes* or *vot*) can only occur in the first position and no other element can legitimately begin a sequence. Reordering elements other than the starting elements generated the second type of ungrammatical sequences – *Legal Starters.* Across both types of ungrammatical sequence there were 21 sequences that contained one or more repeating elements. The grammar permits two elements to repeat within a sequence (*jix* and *pel*). When these elements did repeat in an ungrammatical sequence, they were not reordered. Thus, the repetition structure within these ungrammatical sequences was identical to those of the grammatical sequences. In sequences where the repeating elements *jix* or *pel* occurred once but did not repeat, they could be reordered into positions where they did not occur in grammatical sequences. It follows, therefore, that sequences with violations of this type did in fact violate one aspect of the repetition structure of grammar. That is, an element that could be identified as a potentially repeating element now occurred in a position in which no such repeating elements occurred in the training exemplars. This occurred in 13 of the 19 (68%) Illegal Starters and in none of the Legal Starters. However, sensitivity to this violation would be predicated on applying information that was derived *across* the test sequences. Thus, for all sequences the underlying repetition structure remained grammatical. In the novel vocabulary, ungrammatical sequences could only be rejected on the basis of information that could be induced across the test set. Stimuli are given in Appendix A.

Design

The design was identical to Experiment 4. This was a split-plot design with twelve participants assigned to each of two groups. Trained participants were presented with the training set (symbols) before being presented with response sheets in the test phase (symbols and syllables). Untrained participants, acting as controls, were presented with the response sheets without prior exposure to the training set. The order in which the test sets were presented was counterbalanced. Half of all participants classified symbols sequences first, and half classified syllable sequences first. In addition, the syllable sequences were presented in an order that was different from the one that had been used for the symbol sequences.

Procedure

Trained participants were presented with the training set for ten minutes, and instructed to learn as much about the sequences as they could, as they would be asked questions later. Prior to the test phase, participants in the experimental group were informed that, 'all of the sequences that they had just seen were created using a complex set of rules.' They were then asked to indicate which sequences on the response sheets they thought were 'constructed using the same set of rules.' Untrained participants received no training but were presented with the same response sheets and asked to identify those sequences that they thought were constructed using an unspecified set of rules.

3.2.3 Results

Classification performance

The percentages of correct classification scores, and associated *A'* values are shown in Table 3.1.

		Test Vocabulary			
		Source (Symbols)		Novel (Syllables)	
		mean	*se*	mean	*se*
Training					
Untrained		53%	0.89	52%	1.57
	A'	.55	.02	.54	.03
	B'	-.18	.12	-.23	.10
	Hits	.58	.04	.59	.04
	False alarms	.53	.04	.55	.03
		77%	2.51	70%	3.05
Trained	*A'*	.85	.02	.78	.04
	B'	-.41	.13	-.59	.11
	Hits	.84	.05	.83	.05
	False alarms	.29	.02	.42	.02

Table 3.1: Percentages of correct classification and discrimination indices for Experiment 5

The *A'* data were entered into a split-plot ANOVA with the within-subjects variable Vocabulary (Source and Novel), and the between-subjects variable learning Training (Trained or Untrained). There was an effect of

Training ($F(1, 22) = 70.85$, $p < .01$, $MS_e = 01$, $\eta^2 = .76$), a marginal effect of Vocabulary ($F(1, 22) = 3.52$, $p = .07$, $MS_e = .01$, $\eta^2 = .14$), but no interaction between the two ($F(1, 22) = 1.63$, $p = .22$, $MS_e = .01$, $\eta^2 = .07$). A repeated measures ANOVA on Trained participants A' values clarified the marginal effect of Vocabulary: trained participants were significantly better at discrimination in the source than in the novel vocabulary ($F(1, 11) = 5.69$, $p = .04$, $MS_e = .02$, $\eta^2 = .34$). Subsequent analyses revealed no effects of, nor interactions with, order of test presentation (all Fs < 1.0).

Planned comparisons revealed that correct classification of symbol sequences was reliably greater with exposure to the exemplar symbol sequences relative to no such exposure ($F(1, 22) = 27.98$, $p < .01$, $MS_e = .01$, $\eta^2 = .56$); classification of syllable sequences was also reliably enhanced by exposure to exemplar symbol sequences ($F(1, 22) = 112.39$, $p < .01$, $MS_e = .01$, $\eta^2 = .84$).

Sensitivity within partitions

To determine whether discrimination between grammatical and ungrammatical sequences was localised in the ungrammatical sequences with illegal starting elements, or whether ungrammatical sequences with legal starting elements could also be discriminated from grammatical sequences two further discrimination indices (A') were calculated for each participant for each vocabulary. The first to assess discriminability of grammatical sequences from ungrammatical sequences with illegal starting elements, and the second to assess discriminability of grammatical sequences from ungrammatical ones with legal starting elements. Each discrimination index A', was compared to the equivalent values derived for the control participants. These values are given in Table 3.2.

		Test Vocabulary			
		Source (Symbols)		Novel (Syllables)	
		legal starters	illegal starters	legal starters	illegal starters
Training					
Untrained	*A'*	.51 (.04)	.56 (.02)	.54 (.02)	.51 (.04)
	B'	-.21 (.13)	-.15 (.12)	-.20 (.10)	-.26 (.10)
	False alarms	.55 (.05)	.51 (.04)	.54 (.04)	.56 (.04)
Trained	*A'*	.74 (.04)	.93 (.03)	.61 (.04)	.89 (.04)
	B'	-.66 (.12)	.44 (.13)	-.76 (.10)	.26 (.07)
	False alarms	.54 (.04)	.04 (.03)	.74 (.04)	.12 (.04)

*Standard errors appear in parenthesis.

Table 3.2: Discrimination and bias for sequences with and without a legal starting elements, by vocabulary

Discrimination of sequences with illegal starting elements

When tested in the source vocabulary (symbols), planned comparisons revealed that discrimination between grammatical sequences and ungrammatical sequences with illegal starting elements was reliably better with prior-exposure than with no prior exposure (A' = .93 and .56 respectively: $F(1, 23) = 127.46$, $p < .01$, $MS_e = .86$, $\eta^2 = .85$). When tested in the novel vocabulary (syllables), discrimination between grammatical sequences and ungrammatical sequences with illegal starting elements was also better following prior-exposure than with no prior exposure (A' = .89 and .51 respectively: $F(1, 23) = 42.22$, $p < .01$, $MS_e = .82$, $\eta^2 = .66$).

Discrimination of sequences with legal starting elements

In the source vocabulary discrimination between grammatical sequences and ungrammatical sequences with legal starting elements was also reliably better following prior exposure (A' = .74 and .51 respectively: $F(1, 23) = 20.03$, $p < .01$, $MS_e = .27$, $\eta^2 = .48$). Critically, however, in the novel vocabulary trained participants were unable to discriminate between grammatical sequences and ungrammatical sequences with legal starting elements any better than untrained controls (A' = .61 and .54 respectively: $F(1, 23) = 2.59$, $p = .14$, $MS_e = .03$, $\eta^2 = .11$).

Table 3.2 provides a picture of the distribution of participants' responses to each of the three types of sequence. When tested in the source vocabulary (symbols), the overall percentage correct performance (77%) is largely due to increased hits relative to controls (84% vs. 58%), and increased rejection of illegal starters (97% vs. 49%). There is little difference in the proportion of correctly rejected legal starters (46% vs. 45%). When tested in the novel vocabulary (syllables), the overall percentage correct performance (70%) is due to an increase in the proportion of hits (83% vs. 59%), an increase in correctly rejected illegal starters (88% vs. 44%), and a *decrease* in the number of correctly rejected legal starters (26% vs. 46%). These data raise the question: Could the patterns of responses be due solely to correct rejection of illegal starters?

3.3.3 Discussion

Experiment 5 demonstrated that participants were able to discriminate between grammatical and ungrammatical sequences in the same vocabulary as the exemplars despite the repetition structure of those ungrammatical sequences being entirely grammatical. Similarly, participants were able to discriminate between grammatical and ungrammatical sequences when the first element in the sequence was legitimate. Clearly, in the source vocabulary participants are applying knowledge of sequential dependencies between non-identical elements. In the novel vocabulary however, there is little evidence of sensitivity to such sequential dependencies. The pattern of responses is largely accounted for by participants' rejection of sequences starting with illegal starting elements. These data strongly suggest that in Experiment 5, participants were unable to transfer, to any theoretically significant extent, knowledge of sequential dependencies between non-identical elements across vocabularies.

Were participants responding to the frequency with which different vocabulary elements occurred in the first position? A proportion (68%) of the ungrammatical sequences that began with an illegal starting element did so with one of the two elements (*pel* or *jix*) that formed part of the repetition structure of the grammar. For example the grammatical sequence *vot pel jix sog dup jix*, would be rendered ungrammatical as *pel vot jix sog dup jix*, but *pel* and *jix* could repeat in other sequences, for example *vot pel sog dup pel*, although they did not do so within those particular sequences. This proportion was too large to allow a further partitioning of these data, so Experiment 6 determined whether trained participants rejected ungrammatical sequences that began with such elements by replacing them with two new vocabulary elements, *sab* and *lak*.

3.3 EXPERIMENT 6
SENSITIVITY TO FIRST ELEMENT ILLEGALITY IS NOT PREDICATED ON SENSITIVITY TO MISPLACED REPEATING ELEMENTS

3.3.1 Introduction

In Experiment 5 there was no evidence that participants were able to discriminate between grammatical and ungrammatical sequences that started with a legal starting element in the novel vocabulary. Participants were, however, able to reject sequences that began with an illegal starting element. However, it may have been that participants' successful rejection of sequences that began with an illegal starting element was predicated on the detection of misplaced repeating elements in some (68%) of those sequences. On the assumption that elements which can recur are particularly salient, it is conceivable that participants are particularly sensitive to misplaced potentially recurring elements. In Experiment 6 every illegal starter that began with one of the two potentially recurring syllables (*pel* or *jix*) was replaced with one of two new syllables (*sab* or *lak*) when they occurred in that position. This was the only change that was made to the stimuli that were used in Experiment 5. Unlike the stimuli used in that experiment, these two new illegal starting elements shared two properties of the legal starting elements; they did not occur in any other position than the first, nor did they occur more than once in any sequence.

3.3.2 Method

Participants

Twenty-four members of the University of York participated in the study for either course credit or payment. No volunteer had taken part in any other experiment.

Stimuli

The stimuli were identical to that used in the previous experiment, with the exception that whenever a repeating item (*jix* or *pel*) occurred in the first position, it was converted to a new syllable, either *sab* or *lak* (see Appendix A). For example an ungrammatical sequence such as <u>*pel*</u> *sog vot* would become <u>*sab*</u> *sog vot*, but a sequence such as *kav* <u>*pel*</u> *vot* <u>*pel*</u> would remain unchanged. Hence there were the same thirty exemplar symbol sequences, and the same thirty-eight grammatical syllables sequences as the previous experiment. Only thirteen of the thirty-eight ungrammatical differed from Experiment 5 in that they began with one of the two new starting elements. Only the transfer (novel vocabulary) condition was investigated.

Design

This was a between-subjects design with twelve participants randomly assigned to each of the two groups. Trained participants were presented with the training set (symbols), before being presented with the response sheets (syllables), in the test phase. Untrained participants proceeded directly to the test phase.

Procedure

This was the same as the procedure employed in the previous experiment, with the exception that only the transfer vocabulary was presented at test.

3.3.3 Results

The percentages of correct classification scores are shown in Table 3.3.

		Novel Vocabulary (Syllables)	
		Mean	*se*
Training			
Untrained		50%	1.26
	A'	.50	.03
	B'	-.04	.15
	Hits	.49	.05
	False alarms	.49	.04
Trained		66%	3.01
	A'	.73	.04
	B'	-.42	.10
	Hits	.76	.04
	False alarms	.44	.04

Table 3.3: The Percentages of Correct Classification Scores and Discrimination Acuity (*A'*) Observed in Experiment 6

A one-way ANOVA (Trained vs. Untrained) on *A'* values revealed that overall discrimination between grammatical and ungrammatical syllable sequences was reliably greater with prior exposure to exemplar symbol sequences than with no such exposure ($F(1, 23) = 27.38$, $p < .01$, $MS_e = .33$, $\eta^2 = .55$).

Sensitivity within partitions

As in the previous experiment two further *A'* values were calculated for each participant, one for each partition (see Table 3.4). One-way ANOVAs revealed that discrimination of ungrammatical syllable sequences with illegal starting elements was significantly enhanced with prior exposure relative to controls (.85 and .65, respectively: $F(1, 23) = 21.81, p < .01, MS_e = .24, \eta^2 = .50$). On the other hand, discrimination of ungrammatical sequences with legal starting elements was not improved following prior exposure relative to controls (0.55 and 0.55 respectively: $F < 1.0$).

		Novel Vocabulary (Syllables)	
		legal starters	illegal starters
Training			
Untrained	*A'*	.55 (.02)	.65 (.02)
	B'	.07 (.17)	-.08 (.15)
	False alarms	.45 (.06)	.54 (.05)
Trained	*A'*	.55 (.05)	.85 (.04)
	B'	-.69 (.08)	.34 (.15)
	False alarms	.70 (.06)	.18 (.06)

*Standard errors appear in parenthesis.

Table 3.4: Discrimination and bias for sequences with and without a legal starting element

3.3.4 Discussion

Experiment 6 confirmed that participants are able to correctly reject ungrammatical sequences on the basis of a first-order dependency, namely, how often an element begins a sequence, rather than whether that element has the property that it can repeat within other sequences.

Comparisons with Experiment 5

Comparisons between trained participants' novel vocabulary *A'* values for this and the previous experiment indicated that there was no reliable difference in overall discrimination ($F < 1.0$) or discrimination within the two partitions (both Fs < 1.0). Trained participants in these two experiments appear to have responded in the same way to sequences of syllables and were only able to discriminate grammatical from ungrammatical if the ungrammatical sequences began with an illegal starting element.

However, sequences that began with an illegal starting element contain another cue to their grammaticality. In all the previous experiments in this chapter, ungrammatical sequences were generated by reordering the elements of a grammatical sequence. When ungrammatical sequences that began with an illegal starting element were created, the legitimate starting element (*hes* or *vot*) would be swapped with another element in the same sequence. For example a sequence such as *hes pel dup jix* might become *dup hes pel jix*. So sequences that began with an illegal starting element would also contain a misplaced legitimate starting element. Recall that the legitimate starting elements do not occur in any other position than the first in any grammatical sequence. In principle, sequences that began with an illegal starting element could have been rejected on the basis that participants were sensitive to the novelty of *misplaced starting elements*.

A further possibility is that the participants were sensitive to the addition of the two new vocabulary elements (*sab* and *lak*) in Experiment 6 that altered the frequency distribution of the stimulus set. That is, in the exemplar sequences, there were only eight vocabulary elements, in the test set, the *illegal starters*, contained ten. Participants may have also been sensitive to this particular difference. The following experiment attempted to resolve these issues of whether participants could reject such sequences because they were sensitive the frequency of occurrence in a specific location, or whether they were sensitive to novelty irrespective of location. Specifically, when participants encode how frequently an element begins an exemplar sequence, do they also encode the property that that element may not occur elsewhere in a sequence?

3.4 EXPERIMENT 7
MASKING THE FIRST ELEMENT

3.4.1 Introduction

Experiment 7 used the same grammatical and ungrammatical sequences that were used in Experiments 5 and 6. However in this experiment the first element of each test sequence was masked and consequently the frequency with which any given sequence begins with an illegal first element could not be used as a cue to the grammaticality of that sequence. However those ungrammatical sequences that began with an illegal starting element could be rejected on the basis that they contained a *misplaced* (and visible) legitimate starting element (*hes* or *vot*). That is, an element that occurs in that location in none of the grammatical training or test sequences. Those ungrammatical sequences that did not begin with an illegal starting element, and did not contain a misplaced legitimate starting element could be rejected on the basis that they contained violations to sequential dependencies other than those that involved the starting element or repeating elements. If participants are able to correctly reject sequences that contain a misplaced starting element, then clearly in addition to encoding how frequently they begin exemplar sequences participants must also encode the property that these two elements do not occur elsewhere in grammatical sequences.

3.4.2 Method

Participants

Twenty-four member of the University of York participated in this experiment for either course credit or payment. No volunteer had taken part in any previous artificial grammar learning study.

Stimuli

The sequences were identical to those used in Experiments 5 and 6, with the exception that the first element in each test sequence was masked. Thus, the test sequence *vot dup jix pel* became ■ *dup jix pel.* The thirty-eight ungrammatical sequences may now be partitioned into those that contain a *misplaced* legitimate starting element (19) and those that contain a *well-placed* (albeit masked) starting element (19). For example, ■ *pel dup jix* and ■ *pel hes jix,* specifically where *hes* is a misplaced starting element.

Design

The design was identical to that of Experiment 5. One group of participants was trained on grammatical sequences in one vocabulary (symbols), and another received no training. Both groups were required to classify different grammatical and ungrammatical sequences in both the same vocabulary and in another novel one.

Procedure

The procedure was identical to Experiment 5. Participants who enquired about the black mask were informed that there had been a printing error but were asked to continue anyway.

3.4.3 Results

The percentages of correct classification scores and discrimination indices are shown in Table 3.5.

		Test Vocabulary			
		Source (Symbols)		Novel (Syllables)	
		mean	*se*	mean	*se*
Training					
Untrained		54%	1.72	51%	1.73
	A'	.57	.03	.51	.03
	B'	-.19	.14	-.16	.13
	Hits	.59	.04	.56	.04
	False alarms	.52	.05	.54	.04
		73%	3.88	58%	2.76
Trained	*A'*	.80	.05	.62	.04
	B'	-.33	.14	-.16	.16
	Hits	.78	.06	.61	.06
	False alarms	.32	.03	.46	.06

Table 3.5: Percentages of correct classification scores and associated *A'* and *B'* indices for Experiment 7

The *A'* values were entered into a split-plot ANOVA with the within-subjects variable Test Vocabulary (symbols and syllables), and the between-subjects variable Training (Trained or Untrained). There was an effect of Training ($F(1, 22) = 17.16$, $p < .01$, $MS_e = .02$, $\eta^2 = .44$), an effect of Test Vocabulary ($F(1, 22) = 11.34$, $p < .01.$, $MS_e = .01$, $\eta^2 = .34$), and a marginal interaction between the two ($F(1, 22) = 3.30$, $p = .08$, $MS_e = .01$, $\eta^2 = .13$).

Planned comparisons revealed that correct classification of symbol sequences was reliably greater with exposure to the exemplar symbol sequences relative to no such exposure ($F(1, 22) = 16.86$, $p < .01$, $MS_e = .02$, $\eta^2 = .96$); classification of syllable sequences was also reliably enhanced by exposure to exemplar symbol sequences ($F(1, 22) = 4.36$, $p < .05$, $MS_e = .01$, $\eta^2 = .17$). In addition, a repeated measures ANOVA on Trained participants' *A'* values revealed that those participants were significantly better at discriminating between grammatical and (both sets of) ungrammatical sequences in the source than they were in the novel vocabulary ($F(1, 11) = 11.40$, $p < .01$, $MS_e = .02$, $\eta^2 = .51$). A similar ANOVA on Untrained participants' *A'* scores revealed no such effect ($F(1, 11) = 1.46$, $p = .25$, $MS_e =$

.01, $\eta^2 = .12$). Subsequent analyses revealed no effects of, nor interactions with, order of test presentation (all *F*s < 1.0).

Sensitivity within partitions

Two further discrimination indices (*A'*) were calculated for each participant for each vocabulary (see Table 3.6). These determined whether the discrimination between grammatical and ungrammatical sequences was localised in the ungrammatical sequences that contained misplaced legitimate starting elements, or whether ungrammatical sequences that did not contain this novel feature could be discriminated from grammatical sequences. Each discrimination index *A'*, was compared to the equivalent values derived for the control participants.

		Test Vocabulary			
		Source (Symbols)		Novel (Syllables)	
		well-placed starters	misplaced starters	well-placed starters	misplaced starters
Training					
Untrained	*A'*	.50 (.08)	.57 (.03)	.49 (.04)	.48 (.06)
	B'	-.19 (.15)	-.19 (.13)	-.17 (.13)	-.14 (.13)
False alarms		.52 (.06)	.51 (.04)	.54 (.05)	.53 (.05)
Trained	*A'*	.72 (.06)	.85 (.05)	.58 (.06)	.62 (.05)
	B'	-.50 (.15)	.10 (.11)	-.22 (.18)	-.08 (.16)
False alarms		.47 (.05)	.17 (.04)	.50 (.08)	.43 (.06)

*Standard errors appear in parenthesis.

Table 3.6: Discrimination and bias for sequences with and without a legal starting element, by vocabulary for Experiment 7

Discrimination of sequences with misplaced starting elements

When tested in the source vocabulary (symbols), planned comparisons revealed that discrimination between grammatical sequences and ungrammatical sequences with *misplaced* starting elements was reliably better with prior-exposure than with no prior exposure (*A'* = .85 and .57 respectively: $F(1, 23) = 21.91$, $p < .01$, $MS_e = .46$, $\eta^2 = .50$). When tested in the novel vocabulary (syllables), discrimination between grammatical sequences and ungrammatical sequences with *misplaced* starting elements was numerically but not statistically reliably better following prior-exposure than with no prior exposure (A' = .62 and .48 respectively: $F(1, 23) = 2.70$, $p = .11$, $MS_e < .11$, $\eta^2 < .11$).

Discrimination of sequences with well-placed starting elements

In the source vocabulary discrimination between grammatical sequences and ungrammatical sequences with *well-placed* starting elements was also reliably better following prior exposure (A' = .72 and .50 respectively: $F(1, 23) = 4.68$, $p = .04$, $MS_e = .28$, $\eta^2 = .18$). Critically, however, in the novel vocabulary trained participants were unable to discriminate between grammatical sequences and ungrammatical sequences with *well-placed* starting elements any better than untrained controls (A' = .58 and .49 respectively: $F(1, 23) = 1.37$, $p = .26$, $MS_e = .05$, $\eta^2 = .06$).

Comparisons with Experiment 5

Comparisons between trained participants in this experiment and Experiment 5 revealed no reliable difference overall in discrimination between grammatical and ungrammatical sequences in the source vocabulary (A' = .80 and .85 respectively: $F(1, 23) = 1.03$, $p = .32$, $MS_e = .02$, $\eta^2 = .05$), but did reveal a reliable difference in discrimination in the Novel vocabulary (A' = .62 and .78 respectively: $F(1, 23) = 9.19$, $p < .01$, $MS_e = .17$, $\eta^2 = .30$).

In the source vocabulary there was a no difference in discrimination between grammatical and ungrammatical sequences with *illegal* starting elements (A' = .85 and .93 respectively: $F(1, 23) = 2.00$, $p = .17$, $MS_e = .04$, $\eta^2 = .09$), nor between grammatical and ungrammatical sequences with *legal* starting elements (A' = .72 and .74 respectively: $F < 1.0$).

In the novel vocabulary there was no reliable difference in the discrimination of sequences that contained *legal starting elements* (A' = .58 and .61 respectively: $F < 1.0$). However, there was a reliable difference in discrimination of sequences that contained *illegal starting elements* (A' = .62 and .89 respectively: $F(1, 23) = 15.99$, $p < .01$, $MS_e = .44$, $\eta^2 = .42$).

3.4.4 Discussion

The results obtained in Experiment 7 indicate that the patterns of responses observed in the novel vocabularies of Experiments 5 and 6 might be due primarily to the rejection of sequences that began with illegal starting elements. In the exemplar sequences only two elements could begin a sequence (*hes* or *vot*) and these did not occur anywhere else in a sequence. In the test sets 75% of the test sequences began with one of two elements that also did not occur anywhere else in a sequence and 25% began with one of the remaining six elements in the vocabulary, it follows that participant could induce that the high frequency starters in the novel vocabulary map onto the two starting elements in the exemplars. When, in Experiment 7, the first element of each sequence was masked and participants could not utilise this frequency information, the only remaining cues available to discriminate between grammatical and ungrammatical sequences were illegal sequential dependencies between non-repeating elements, and the occurrence of the misplaced starting element. Participants were unable to discriminate between sequences on the basis of these cues. Experiment 7, in comparison to Experiments 5 and 6, found that participants were relatively more sensitive to the frequency with which different elements occurred in the first position than they were to the novelty of misplaced legitimate starting elements alone. Of course the greater discrimination acuity observed in Experiments 5 and 6 relative to Experiment 7 could be because in those experiments *both* ungrammatical features (illegal starters and misplaced starters) are present and visible. The critical findings of the previous experiments are, however, that participants do not reject ungrammatical sequences that do not contain violations involving those starting elements or repetition structures in a novel vocabulary.

Are participants sensitive to sequential dependencies between non-repeating elements in a novel vocabulary? Thus far the data suggest that they are not. However, the salience of such novel features may mask any sensitivity to other internal features of the sequences. Experiment 8 removes any of the cues to the grammaticality of a sequence that involves the first element.

3.5 EXPERIMENT 8
NO EVIDENCE OF TRANSFER WHEN THE FIRST ELEMENT AND REPETITION STRUCTURES ARE HELD CONSTANT.

3.5.1 Introduction

Experiment 8 followed the same design and procedure as the previous experiments. In those experiments however, half of the ungrammatical sequences contained violations that in some way involved the legality of the first element of each sequence, and in Experiment 5 one aspect of the repetition structure of the grammar. Participants were also sensitive to violations in the other set of ungrammatical sequences that did not involve the two legitimate starting elements. However, participants were unable to transfer this information to the novel vocabulary. It is possible that the violations involving the first element are so salient that they occluded any sensitivity that participants might have had to violations not involving the first element of a sequence. Experiment 8 determined whether or not this was the case. In Experiment 8 ungrammatical sequences contained *only* violations to sequential dependencies between non-repeating elements, and those violations did not involve the first element. That is, each ungrammatical sequence is comparable to the *legal starters* subset of ungrammatical sequences that were used in the previous experiments in this chapter. This Experiment is different however, from Experiment 4 (in the preceding chapter) in that the ungrammatical sequences used here do not contain any unfamiliar repetition structures.

3.5.2 Method

Participants

Twenty-four members of the University of York participated in the study for either course credit or payment. Care was taken to ensure that volunteers had not previously taken part in any artificial grammar learning experiment.

Stimuli

The thirty training and thirty-eight grammatical test sequences were identical to those used in Experiments 5-7. As before, each sequence could only start with one of two legitimate starting elements (*hes* or *vot*). The ungrammatical sequences that began with a legitimate starting element in those experiments (19 sequences) were duplicated to create two identical sets of ungrammatical stimuli (38 in total). The first element (*hes* or *vot*) of each duplicated sequence was replaced by the alternative starting element: if the original ungrammatical sequence began with the legal starter *hes*, its duplicate began with the legitimate *vot*. If it began with *vot*, it was changed to *hes*. This ensured that each ungrammatical sequence was unique. As before there were no violations of repetition structure. The ungrammatical sequences were created by re-ordering the grammatical test sequences in such a way that elements that repeated within the sequence remained in the same positions. Elements that could in principle repeat but which did not do so within that particular sequence were only moved, if at all, to positions permitted by the grammar. This experiment is different from Experiment 4. In that Experiment a proportion of ungrammatical and grammatical test sequences contained unfamiliar repetition structures. In this Experiment, no test sequence contains a pattern of repeating elements that did not occur in the exemplars. As a consequence, the overall proportion of illegal bigrams in the ungrammatical sequences was less (.17) than the sequences in Experiment 4 (.21), but was comparable to the proportions in Experiment 1 and 3 (both .16).

Design and Procedure

The design and procedure were identical to those of Experiments 5 and 6. Participants were either trained on sequences of symbols, or received no training. Later participants were informed that those sequences obeyed some simple rules and asked to classify new sequences as being either well- or ill-formed according to those rules.

3.5.3 Results

Classification Performance

The percentages of correct classification scores are shown in Table 3.7. Unlike the previous experiments in this chapter, the only partitions to be made are between the grammatical and the ungrammatical test sequences.

		Test Vocabulary			
		Source (Symbols)		Novel (Syllables)	
		mean	*se*	mean	*se*
Training					
Untrained		50%	0.81	48%	1.76
	A'	.51	.02	.47	.03
	B'	-.15	.08	.06	.10
	Hits	.54	.02	.46	.03
	False alarms	.54	.02	.50	.03
		57%	1.71	51%	1.64
Trained	*A'*	.62	.03	.52	.03
	B'	-.29	.08	-.13	.12
	Hits	.65	.03	.54	.03
	False alarms	.51	.02	.53	.04

Table 3.7: Percentages of correct classification scores and associated *A'* and *B'* indices for Experiment 8

The *A'* scores were entered into a split-plot ANOVA the within-subjects variable being Test Vocabulary (symbols and syllables), and the between-subjects variable being Training (Trained or Untrained). There was an effect of Test Vocabulary ($F(1, 22) = 5.89$, $p = .02$, $MS_e = .01$, $\eta^2 = .21$), an effect of Training ($F(1, 22) = 9.44$, $p < .01$, $MS_e = .01$, $\eta^2 = .30$), but no interaction between the two ($F(1, 22) = 1.40$, $p = .25$, $MS_e = .01$, $\eta^2 = .06$). Planned comparisons revealed that participants given prior-exposure to grammatical exemplars had higher A' values than control participants when tested in the source vocabulary (symbols): .62 vs. .51 ($F(1,22) = 11.67$, $p < .01$, $MS_e = .01$, $\eta^2 = .35$). In the novel vocabulary (syllables), there was no difference: .52 vs. .47 ($F(1,22) = 1.13$, $p = .30$, $MS_e = 01$, $\eta^2 = .05$). A repeated measures ANOVA on Trained participants' A' values revealed that they were significantly better at discriminating between grammatical and ungrammatical sequences when tested in the source vocabulary compared to when they were tested in the novel vocabulary ($F(1, 11) = 6.39$, $p = .03$, $MS_e = .01$, $\eta^2 = .37$). Subsequent analyses revealed no effects, nor interactions with order of test presentation (all Fs < 1.0).

Comparisons with Experiment 5

Comparisons between Trained participants sensitivity to sequences with *legal starting elements* in Experiment 5 revealed only a marginal difference in the source vocabulary ($A' = .62$ and .73 respectively: $F(1, 23) = 3.77$, $p = .06$, $MS_e = .06$, $\eta^2 = .15$), and a significant difference in the novel vocabulary ($A' = .52$ and .62 respectively: $F(1, 23) = 7.05$, $p = .01$, $MS_e = .09$, $\eta^2 = .24$). Clearly, when repetition structure and information concerning the first element cannot be used to discriminate between grammatical and ungrammatical sequences, participants are still able to discriminate between the two in the source vocabulary but are unable to transfer that knowledge to a novel vocabulary.

3.5.4 Discussion

Experiment 8 found that participants were able to discriminate between sequential dependencies between non-repeating elements that did not involve the first element of a sequence in the same vocabulary as the training exemplars. However, participants were unable to transfer that knowledge to a novel vocabulary. Unlike the previous experiments described in this chapter, this was the only basis on which grammatical and ungrammatical sequences could be discriminated. Hence, Experiment 8 has failed to demonstrate the transfer of this particular form of grammatical knowledge across vocabularies.

Do these data provide the statistical power to accept this failure to replicate the transfer effect at face value? Post-hoc power analyses were performed using procedures outlined by Erdfelder, Faul, and Buchner (1996). Post-hoc power analyses require the effect size of an exemplar experiment to be calculated – the drop in discrimination acuity from the source to the novel vocabulary in Experiment 5, that did demonstrate an effect, was calculated. Overall discrimination in the source vocabulary may be taken as the ceiling against which to compare the magnitude of the transfer effect (A' = .85 for trained participants, .55 for untrained). The difference between trained (.78) and untrained (.54) participants' discrimination acuity in the novel vocabulary is 82% of the equivalent difference in the source vocabulary. The power of detecting 82% of the effect size seen between trained and untrained participants in the source vocabulary of Experiment 8, that we could expect to see transferred to the novel vocabulary was $1 - \beta = .8043$. That is, if there was an effect of the magnitude seen in Experiment 5 present in Experiment 8, the probability of detecting such an effect was .8. This calculation is somewhat conservative given that there was in fact no statistically significant drop in discrimination accuracy from the source to the novel vocabulary in Experiment 5.

As in Experiment 5, participants are unable to transfer knowledge of sequential dependencies between non-identical elements to the novel vocabulary. Interestingly, the absolute percentage correct classification in the source vocabulary was considerably reduced in Experiment 8 (57%) compared with the equivalent percentage from Experiment 5 (77%). The decrease in performance in the source vocabulary between Experiments 5 and 8 could only be attributed to the observation that, in Experiment 5, participants' response patterns were heavily influenced by their sensitivity to illegal starters. In the novel vocabulary, participants appeared to rely on this cue to ungrammaticality to the detriment of others cues that were also present.

On what basis are participants sensitive to the legality of the starting element? When tested in the source vocabulary (when no mapping is required between the tokens used in the learning and test sets), the identity of starting elements is clearly available. In the novel vocabulary, however, this identity is lost, and some other cue must be used. In Experiments 5 and 6, 75% of test sequences started with a legal starting element (as determined by the assignment of syllables to transitions within the grammar used to generate the grammatical stimuli). The remaining 25% started with a variety of illegal starting elements (in fact, four different illegal starters were employed). So 75% of test sequences started with either *hes* or *vot* (both legal); 17% with either *jix* or *pel* (or *sab* and *lak* in Experiment 6 so as not to violate repetition structure: all illegal); and 8% with either *kav* or *dup* (both illegal). In principle, participants could have used frequency of occurrence as a cue, rejecting as ungrammatical any sequence that had a starting element that occurred relatively infrequently in the first position. In Experiment 9, these frequencies were manipulated, holding all other variables constant, in order to determine directly whether participants are indeed sensitive to the frequency with which items occur in first position.

3.6 EXPERIMENT 9
TRANSFERRING THE IDENTITY OF THE FIRST ELEMENT

3.6.1 Introduction

In Experiment 9 the thirty-eight grammatical test sequences from the previous experiments in this chapter were duplicated to create seventy-six test sequences. The majority of these items were then modified so that their (legal) starting elements were replaced with one of the two syllables *sab* and *lak*. In all other respects, these sequences were 'grammatical', and participants could only discriminate between these sequences and the unmodified ones on the basis of the starting element. If participants are sensitive to the frequency of occurrence of the starting element *independently* of any prior exposure, then both trained *and* untrained participants would endorse as grammatical test sequences that contained the higher frequency starting elements, and would reject the sequences that contained the lower frequency starting elements as ungrammatical. If, on the other hand, sensitivity to the frequency of occurrence of the starting element develops only in response to the frequency characteristics of the training exemplars, then only participants who received prior exposure to this set would discriminate between test sequences according to their frequency characteristics. Recall that in the learning phase, all stimuli start with just one of two symbols. In the test phase employed in this experiment, the stimuli start with one of four syllables, but two of them occur with high frequency (approximately 70% of stimuli contain these two) and two with low frequency (30%). Participants must determine which of these syllables correspond to, and should be mapped onto, the initial symbols seen in the learning phase.

3.6.2 Method

Participants

Twenty-four members of the University of York participated in the study for either course credit or payment. Care was taken to ensure that volunteers had not previously taken part in any artificial grammar learning experiment.

Stimuli

The thirty exemplars (symbol sequences) were identical to those used in the previous experiments (see Appendix A). The thirty-eight grammatical test sequences that had been used in previous experiments were duplicated to create seventy-six test sequences, of which twenty-two were left intact, and fifty-four were modified by replacing the starting elements (*hes* or *vot*) with two new syllables (*sab* and *lak*). In all other respects, these fifty-four sequences were entirely grammatical. The test stimuli are given in Appendix

A. As in previous experiments, each exemplar was presented to Trained participants four times in a seven page booklet for study. Both Trained and Untrained participants were required to classify the syllable sequences according to whether they obeyed a set of rules or not.

Design and Procedure

The design and procedure was identical to Experiment 6—as in that experiment, test sequences were only presented in the novel vocabulary.

3.6.3 Results

Classification Performance

Sequences were scored as 'correct' if sequences with the high-frequency starters (*sab* and *lak*) were endorsed as grammatical, and sequences with low-frequency starters (*hes* and *vot*) were rejected as ungrammatical. The percentages of correct classification scores and discrimination indices are shown in Table 3.8.

		Novel vocabulary (Syllables)	
		Mean	*se*
Training			
Untrained		51%	2.82
	A'	.49	.04
	B'	-.08	.16
	Hits	.51	.05
	False alarms	.50	.06
Trained		78%	5.93
	A'	.80	.08
	B'	.27	.19
	Hits	.78	.05
	False alarms	.20	.08

Table 3.8: Percentages of correct classification scores and associated *A'* and *B'* indices for Experiment 9

The untrained controls classified 51% of the test sequences correctly. Trained participants classified 78% of the test sequences correctly — that is, they endorsed as grammatical sequences with high-frequency starters and rejected as ungrammatical sequences with low-frequency starters. A one-way ANOVA on A' values revealed that these response patterns were reliably different, $F(1,23) = 11.99$, $p < .01$, $MS_e = .56$. $\eta^2 = .35$. Despite this difference in discrimination, there was no difference in bias (B') between groups $F(1,23) = 1.95$, $p = .18$, $MS_e = .73$. $\eta^2 = .08$.

3.6.4 Discussion

Experiment 9 demonstrated how participants are sensitive to the frequency of elements occurring in the first position of a sequence. Crucially, the control participants who received no prior-exposure to the grammar did not respond according to the frequency of the first element at all. It would appear that sensitivity to this feature develops as a result of prior exposure to grammatical exemplars, and the requirement to induce a mapping between the vocabulary elements experienced in the two vocabularies. This is an important finding, because it demonstrates that participants encoded how many and how often elements begin a sequence. This first-order dependency information was abstract in the sense that it could be transferred to the novel vocabulary.

3.7 CHAPTER SUMMARY

The experiments reported in this chapter demonstrate that the transfer effect observed by Altmann *et al.* (1995, Experiment 4), that was replicated in Experiment 2 (in the preceding chapter), can be attributed to the correct rejection of sequences that began with an illegal starting element. The experiments that did not demonstrate a significant transfer effect in Chapter 2 used ungrammatical sequences in which the first element was not reordered. Of course, in the experiments described in Chapter 2, participants could have responded to other features, such as sequential dependencies between repeating elements, and between non-repeating elements. In Chapter 3, no ungrammatical sequence contained repetition structures that were not permitted by the grammar; hence discrimination between grammatical and ungrammatical sequences in a novel vocabulary could not proceed on the basis of abstract analogies between such structures. In Experiment 5-7, two types of ungrammatical sequences were created. The first type contained illegal sequential dependencies between non-repeating elements alone. The second type contained, in addition, violations involving the two vocabulary elements that the grammar dictates must begin each grammatical sequence.

To recapitulate, Experiment 5 determined that at least with respect to the grammar used in Chapters 2 and 3 (and by Altmann *et al.* 1995), above chance classification of stimuli in a novel vocabulary appears to be dependent almost entirely on the legality of the first element in each test sequence. Experiment 6 excluded the possibility that participants were rejecting ungrammatical sequences in Experiment 2 and 5 on the basis that a large proportion of them began with an element (*pel* or *jix*) that could potentially form repetition structures in other sequences. These sequences were, it seems, rejected on the basis that they began with elements that occurred only infrequently in the first position, and so could not be mapped onto the high frequency starting elements in the exemplar sequences. In Experiment 7 the first element of each test sequence was masked to determine whether participants were sensitive to misplaced starting elements independently of which elements occurred in first position. In this experiment ungrammatical sequences that contained an illegal starting element could only be rejected because the legitimate starting elements (*hes* or *vot*) were misplaced elsewhere in each of those sequences. In the event, participants were insensitive to this feature alone (c.f. Experiment 5). Critically, in Experiments 5-7, participants were unable to discriminate between grammatical and ungrammatical sequences that did not violate grammatical constraints on the first element. Experiment 8 determined that this was not because the salience of this feature occluded sensitivity to ungrammatical sequences that did not involve such violations. Finally, Experiment 9 demonstrated how sensitivity to the frequency with which vocabulary elements begin exemplar sequences allows the grammatical and ungrammatical test sequences to be correctly classified even in a novel vocabulary. So participants do encode at least some abstract knowledge from exemplars: how often elements occur, and where they might occur. Such knowledge reflects first- but not second-order dependency information.

This chapter has demonstrated how sensitivity to the frequency with which an element occurs in a specific position (in this case the first) enables participants to discriminate between grammatical and ungrammatical sequences in a novel vocabulary. These findings confirm that participants encode first-order dependency information from exemplar sequences, and that this information is used to map the correspondences between elements in the source and novel vocabularies. There was no evidence that participants were able to transfer second-order dependency information regarding which elements can co-occur elsewhere in a sequence. Since there was no evidence that participants were able to transfer knowledge of sequential dependencies between non-repeating elements to a novel vocabulary, there is no evidence that such knowledge is abstract in the sense that it is independent of the vocabulary in which it was acquired. Experiment 9 demonstrated how knowledge that is not vocabulary independent and does not form repetition structures could nonetheless be mapped between vocabularies on the basis of the frequency distributions across exemplars and test sets. Even in the experiments reported here, participants' knowledge of the artificial grammar was abstract in the sense that it reflected something of the grammatical rules underlying sequence construction (hence the notion of 'legality' as applied to the starting elements). However, it was not the case that *all* aspects of the grammatical dependencies constituting the grammar could be transferred to a novel vocabulary.

In Altmann *et al.*'s study, discrimination between sequences that did not contain repeating elements can be attributed to sensitivity to starting element legality. Altmann *et al.* did not claim that the mapping between the two vocabularies could necessarily be induced in the absence of either repetition structure or illegal starting elements. Rather, they suggested that the mapping between the vocabularies be represented in a way that allowed that mapping to be applied to sequences that contained no repeating elements or no illegal starting elements. Indeed, information about repetition structure or illegal starters could doubtless play an important role in the induction of that mapping, and Altmann *et al.* did not rule out this possibility. As such, the findings described here are entirely consistent with other findings in the literature. A number of workers have found that participants are relatively more sensitive to the initial portions of a sequence (relative to elsewhere in a sequence) in the same vocabulary as exemplar sequences (e.g. Reber & Lewis, 1977; Perruchet & Pacteau, 1990; Higham, 1997a) and a different vocabulary (e.g. Shanks *et al.* 1997, Experiment 2).

The same conclusion can be drawn concerning the discrepancies between the recent findings of Gomez *et al.* (in press) and those of Altmann *et al.* (1995). Gomez *et al.* found no evidence that participants could classify sequences in a language that did not contain repetition structures, whereas Altmann *et al.* (Experiment 4) found that participants could discriminate between grammatical and ungrammatical versions of such sequences, although their language does permit repetition structures in other sequences. The question remains, therefore, whether there are any data that directly test whether knowledge pertaining to sequential dependencies between non-identical elements can be transferred across vocabularies. No single study that directly manipulates such dependencies has done so without also manipulating other sources of potentially discriminating information that are not sequential in nature. That is, the evidence to date concerning the transfer of second-order dependency information has confounded simple first-order (frequency-by-location) information. In the strongest sense 'sequential' refers to the notion that the identity of an element at one position within a sequence is determined by the identity of an element at another position within the sequence. Demonstrations to-date of vocabulary-independent structure have either included ungrammatical sequences that violated repetition structure (e.g. Altmann *et al.*, 1995; Brooks & Vokey, 1991; Dienes & Altmann, 1997; Gomez, 1997; Gomez & Schvaneveldt, 1994; Mathews *et* *al.*, 1989; Redington & Chater, 1996; Whittlesea & Dorken, 1993; Whittlesea & Wright, 1997; Shanks *et al.* 1997), or have confounded *n*gram violations with differences in position-specific frequency distributions which, although sensitive to where in the sequence a particular distribution is located, do not necessitate, for their encoding, information about dependencies between items in different positions (e.g. Altmann *et al.*, 1995; Gomez & Schvaneveldt, 1994; Shanks *et al.*, 1997). Other studies have not listed the stimuli and so it is not possible to determine the exact nature of the ungrammatical sequences (e.g. Knowlton & Squire, 1996; Manza & Reber, 1997). This last observation, that there is no definitive evidence to-date of transfer of knowledge pertaining to sequential dependencies between non-repeating elements, should not be taken to imply that any demonstrations of this transfer effect should be discounted — they do demonstrate, after all, that *some* aspects of grammatical knowledge *can* be transferred across vocabularies. At issue is *which* aspects, and consequently, the nature of the mechanisms that are postulated to underlie such transfer. In Experiment 5, participants were clearly sensitive to the positional frequency of at least some vocabulary elements. Previous demonstrations of the transfer effect appear to be carried by sensitivity to sequentially dependent positional frequency of either individual or two or more identical elements. In Chapter 3 there was no evidence that participants can apply knowledge of sequentially dependent positional frequency of two or more non-identical elements, evident in the source vocabulary, to a novel one. The challenge of Chapter 4 is to determine whether participants can learn such information and apply it in a novel vocabulary independently of any frequency cues. This research is important because both the rule-abstraction and fragment memorisation theories on artificial grammar learning are predicated upon knowledge of dependencies between vocabulary elements, rather than feature-frequency or repetition structures. Two important features of the Experiments used in Chapter 4 control for these extraneous cues to grammaticality. First, the grammar used does not generate any repetition structures. Second, the unit of sequential dependence to be investigated is the second-order sequential dependency. Bigram information reflects fragmentary grammatical transitions between two vocabulary elements. Chapters 2 and 3, have found no evidence that participants are able to transfer this kind of information, but the stimuli used in the studies reported there contained other potential cues to grammaticality such as repetition structures and first-order frequency-by-location information. If participants are unable to transfer bigram information, then clearly the transfer effect in artificial grammar learning cannot be explained by theories involving either rule abstraction or fragment memorisation.

CHAPTER FOUR

THE TRANSFER OF SECOND-ORDER SEQUENTIAL DEPENDENCE

4.1 INTRODUCTION

Chapter 3 found evidence that participants were sensitive to first-order dependencies, but found no evidence that participants were sensitive to second-order dependencies. Second-order dependencies are often referred to as bigrams. Bigrams themselves are assumed to be episodic fragments of exemplars. Fragment based accounts of artificial grammar learning are extremely persuasive: they can account for data that is consistent with other theoretical descriptions, and often with a greater degree of parsimony (see Johnstone & Shanks, 1999; Perruchet, 1994). Indeed bigram fragments are often referred to as *the* unit of knowledge that is abstracted from exemplars and used to determine the well- or ill-formedness of new sequences. Participants remember fragments because memorisation of whole sequences is beyond the capacity of working memory (Perruchet & Gallego, 1997; c.f. Miller, 1956). Memory for bigrams permits the correct classification of test sequences because grammatical sequences *tend* to contain more familiar bigrams, that is bigrams that were present in exemplars, than ungrammatical sequences that *tend* to contain fewer familiar bigrams. But in principle the bigram is a little more sophisticated than an episodic fragment of an exemplar because it encodes the contingency between two adjacent vocabulary elements. As such the bigram can be regarded as a second-order sequential dependency, either between two repeating vocabulary elements (e.g. *MM*) or between two non-repeating vocabulary elements (e.g. *MT*).

Bigram knowledge can be regarded as rule-like in two senses. First memory for bigrams provides a degree of generative capacity in that such fragments can be recombined into new sequences and permit the correct classification of previously unseen sequences. For example, remembering that

MT had appeared in an exemplar would permit previously unseen sequences such as *MTXRV* or *RXMTV* to be accepted as grammatical but not one such as *TMXRV* (c.f. Perruchet & Pacteau, 1990; Mathews & Roussel, 1997). This degree of generative capacity makes it particularly difficult to distinguish between different theoretical accounts of artificial grammar learning, particularly between the fragment- and rule-based theories (e.g. Higham, 1997b; Johnstone & Shanks, 1999). In contrast, exemplar based classification would only permit previously unseen sequences to be classified as category members if they were perceptually similar to stored exemplars. One source of confusion is that bigram knowledge is often considered to be episodic in nature (that is people remember fragments), rather than an abstract encoding of the sequential rules of the grammar. However, the encoding of such sequential rules is incidental to memory for fragments because each bigram represents the transition from one vocabulary element to another. Since participants are sensitive to specific locations and can indicate where in a particular sequence a bigram can and cannot occur, we can assume that participants also encode positional information of those fragments (e.g. Dulany *et al.* 1984; Shanks *et al.*, 1997). In this case the bigram represents the transition from one grammatical node to another. So the *n*gram can in principle be regarded as a representation of the contingency or sequential dependency between two or more vocabulary elements. For example, if all that was abstracted from an exemplar such as *MTMRX* was bigram fragments such as *MT* or *MR*, those representations would permit the prediction of either *T* or *R* if participants were asked what letter could follow *M*. However, if participants are asked what letter can follow *MTM* they may only respond with *R* (c.f. Dienes, Broadbent & Berry, 1991; Reber & Lewis, 1977). Of course knowledge of higher-order dependencies, such as trigrams, provides stronger evidence that participants abstract some of the sequential rules of a grammar from exemplar sequences. Cleeremans and McClelland (1991) have demonstrated that knowledge of second-order dependencies (e.g. *MT*) precedes knowledge of higher-order dependencies (e.g. *MTV*) that only become apparent after extensive training. And this is reflected in a number of different computational models of artificial grammar learning, in particular the Competitive Chunking (Servan-Schreiber & Anderson, 1990) and Simple Recurrent Network models (Cleeremans, 1993; Dienes, Altmann & Gao, 1999). Bigram knowledge then is important because it seems to reflect rule-like grammatical, rather than purely episodic, knowledge. Of course the grammar that participants develop from partial fragmentary knowledge (i.e. remembering only some highly frequent transitions) can only ever be a correlated representation of the grammar that generated the exemplars (c.f. Dulany *et al.*, 1984), rather than a veridical representation of the finite-state transitions (c.f. Reber, 1967).

As we shall see in the following sections these rule-like properties of fragmentary knowledge are best observed in the transfer of that knowledge to a novel vocabulary. However, there is some debate concerning the extent to which the mind is capable of representing rules (e.g. Bates & Elman, 1993; Fodor & Pylyshyn, 1988; Marcus, 1998). To some extent, this debate is driven by computational modelling. For example, connectionist networks behave in rule like ways and given grammatical input do encode sequential dependencies (e.g. Cleeremans, 1993; Dienes, Altmann & Gao, 1999). However, because such sub-symbolic networks represent knowledge as patterns of activation between processing units they do not possess explicit representations of rules in the sense that symbolic computational models do. For purposes of theoretical neutrality *n*gram knowledge is referred to here as rule-like knowledge without making any assumptions as to how that information might be represented.

4.1.1 Can fragmentary knowledge account for transfer across vocabularies?

Whether *n*gram knowledge abstracted from exemplars can support the correct classification of test sequences in a novel vocabulary has been the source of some controversy. Chapter 3 found no evidence that participants were able to transfer knowledge of sequential dependencies between non-repeating elements across vocabularies. Perruchet (1994) argued that bigram knowledge could not support the classification of sequences in a novel vocabulary because with the exception of the first and last bigram participants do not encode the positions where a bigram might occur. For example, Perruchet and Pacteau (1990) showed that when participants were asked to memorise bigrams, they could subsequently classify test sequences (in the same vocabulary as exemplars) at a level comparable with that of participants who had memorised exemplars (from which the bigrams were derived). However, Gomez and Schvaneveldt (1994) demonstrated that participants do encode the positions where legal bigrams might occur in a sequence, and use that knowledge to classify sequences presented in a novel vocabulary. They asked participants to either memorise whole sequences or the bigrams that composed those sequences. Subsequently participants were asked to classify three sets of test sequences in either the same or a different vocabulary than the exemplars. The first set consisted of previously unseen grammatical sequences, the second of ungrammatical sequences that contained an illegal bigram, and the third contained misplaced legal bigrams. Participants who had memorised whole exemplars were able to correctly discriminate between grammatical sequences and both the ungrammatical sequences that contained illegal bigrams, and those that contained misplaced legal bigrams in both the same vocabulary as the exemplars and a different one. Importantly the participants who had memorised fragments were unable to discriminate between grammatical sequences and these two types of ungrammatical sequence in the novel vocabulary. If, after studying exemplars, participants had not been able to discriminate between grammatical and the ungrammatical sequences that contained misplaced legal bigrams, we would have to conclude that participants did not encode information about where bigrams can occur in a sequence as Perruchet and Pacteau (1990) had argued. Clearly, *n*gram information when it is abstracted from whole exemplars does encode positional information that can subsequently be mapped onto *n*grams presented in a novel vocabulary. However, although Chapter 3 found evidence that participants were sensitive to sequential dependencies between non-repeating elements in the same vocabulary as learning, there was no evidence that such knowledge could be transferred to a novel vocabulary. What did allow participants to classify *some* sequences as being ungrammatical was knowledge of a first-order dependency (the frequency of with which elements occur independently of other elements in the language) – in the first position of a sequence. Such knowledge is relatively trivial and need not be predicated on rule-like knowledge of grammar. How can we be sure that participants trained on whole exemplars classify sequences in a new vocabulary on the basis of second- or higher-order dependencies rather than simple frequency-by-location information such as that observed in Chapter 3?

4.1.2 On sensitivity to first- and second order dependencies

A first order dependency determines the frequency with which an element occurs in a specific location in a sequence. For example the occurrence of *M* in the first position of a sequence is dependent upon there being a grammar that predicts *M* with some probability in position x of a sequence. A second-order dependency might determine that *M* in position x predicts *T* in position $x+1$ with some probability, hence the bigram *MT*. If participants are asked to memorise a set of sequences that all begin with *MT* and are subsequently asked to decide which of the three sequences *MTVX, MVTX,* and *MTXV* conform to those exemplars, they ought to endorse the first and last sequences, but reject the second. We might conclude that participants had learned that *T* was sequentially dependent upon *M* (a second-order dependency). Alternatively we could conclude that the sequence *MVTX* had been rejected because participants had never seen a *V* in second position in the exemplar sequences (a first-order dependency). Now if that classification test had been in a different vocabulary than the exemplars, *AEOU AOEU* and *AEUO,* we could draw two analogous conclusions. Either participants had rejected the second sequence because the second-order dependency *MT* could be mapped onto the sequences 1 and 3, but not onto sequence 2. Or more parsimoniously we could conclude that participants had rejected sequence 2 because *O* occurs only infrequently in that position whereas *T* had occurred in position 2 of every exemplar. That is, knowledge of a first-order and not a second-order dependency had been transferred. Although some of the correspondences between the two vocabularies have been mapped, we cannot necessarily conclude that the contingent relationships between individual elements have also been mapped.

Consider a study by Shanks, Johnstone and Staggs (1997) that claimed to show cross-domain transfer of grammatical knowledge pertaining to *n*gram structure. Using a two alternative forced choice paradigm, Shanks *et al.* demonstrated that participants were sensitive to a wide range of features in an artificial language, and could transfer that knowledge to a different vocabulary. For each grammatical sequence an ungrammatical sequence was generated by substituting one element for another that could not legitimately occur in that position. For their Experiment 2, they included a subset of five pairs of items — one grammatical and the other ungrammatical — in which the ungrammatical version was constructed by changing the second letter in each of the five grammatical sequences, thus creating an 'illegal initial bigram.' Shanks *et al.* ensured that the resulting sequence-initial bigram (*VT*, *VR*, or *MT*) was permitted by the grammar, but *not* in that position. They showed that participants could discriminate between the grammatical and ungrammatical sequences in the novel vocabulary (a different letter-set), on which basis they concluded that participants 'have abstract knowledge either of legal triplets or of the restrictions on the positioning of bigrams' (Shanks *et al.*, 1997, p.228). However, participants in their experiment need not have been responding on the basis of bigram information at all — they may instead have been responding on the basis of the frequency of occurrence of items in (in this case) position 2 of each sequence. There were just three letters that could legally occur in this position (*M*, *V*, or *X*). The five ungrammatical sequences in the subset under consideration introduced two new letters (*T* and *R*) in this position, and of the 60 sequences seen in all (including five other subsets of five pairs each), only these five sequences contained one or other of these two new letters. Thus, participants could have induced frequency-based knowledge on the basis of prior training with grammatical exemplars, and then rejected as ungrammatical any sequence which contained low-frequency elements in position two (the other five subsets of test stimuli violated other aspects of the stimuli, leaving position two 'intact', but violating either initial starting element or repetition structure). Interestingly, the control group performed (non-significantly) below chance on this subset — they tended to endorse as grammatical the ungrammatical stimuli in this subset, and inspection of the 'grammar' these control subjects had been trained on reveals that all five vocabulary items could occur in position two and occurred there with equal frequency.

The important point about knowledge concerning sequential dependencies between non-repeating elements, whether it be a first–order dependency such as that discussed in Chapter 3, or higher-order dependencies; is that such knowledge is abstracted *across* exemplars and can only be mapped onto a novel vocabulary by processing relevant information *across* the test sequences. This Chapter aims to determine whether knowledge of second-order dependencies that are abstracted from exemplars in one vocabulary can indeed be transferred to sequences instantiated in another vocabulary unconfounded with first-order dependency information. In this way knowledge of the dependencies within an episodic representation of a bigram can be revealed. Recently Johnstone and Shanks (1999) requested that a debate be opened concerning the use of finite-state grammars in artificial grammar learning research. They discussed the problems of confounding the distributional statistics of the sequences that finite-state grammars generate with particular reference to what can be concluded about how participants represent fragments of such sequences. They suggested that grammars be constructed that do not confound the distributional statistics of *n*grams with rule structure. Although the following experiments were conducted prior to this debate it is reassuring to note that this research is entirely consistent with their point.

4.1.3 Is fragmentary knowledge implicit?

In addition to asking whether participants can learn these contingent relationships, we can also ask to what extent participants are aware of those relationships. Fragmentary knowledge has often been described as 'explicit' because bigram knowledge is available on direct tests and can be articulated (e.g. Dulany *et al.* 1984; Perruchet & Pacteau, 1990). Gomez (1997) found that participants were unable to transfer *n*gram knowledge to a novel vocabulary unless they were also able to recognise illegal and legal *n*grams in the same vocabulary as exemplars. A good deal of research has indicated that people do not learn about the contingent relationships between events or objects without being *aware* of those relationships (e.g. Boakes, 1989; Brewer, 1974; Davey, 1994; Dawson & Schell, 1987). In contrast Dienes and Altmann (1997) observed that participants could successfully discriminate between grammatical and ungrammatical sequences in a novel vocabulary but their confidence ratings revealed that they often believed that they were guessing, and that their confidence did not predict how accurate they were. The experiments described below also question whether participants' knowledge of the contingent relationships between vocabulary elements is implicit in the sense that participants seem to lack metaknowledge of the knowledge used to discriminate between the two sets of test sequences. The stimuli used in this chapter were designed so that knowledge of the contingent relations between vocabulary elements is the only knowledge that would permit discrimination between the two types of test sequence. Thus this measure of awareness meets the *Information* and *Sensitivity criteria* that Shanks and St. John (1994) argued must be met before the learning of artificial grammars can be considered in any way implicit.

4.1.4 Overview of experiments

The experiments described below use the same two vocabularies (symbols and syllables) that were used in Chapters 2 and 3. However, a rather simpler grammar than the ones that are normally used in artificial grammar learning was designed to generate only two sequential dependencies (between *A* and *B*, and between *C* and *D*), only one of which could occur in any one sequence. Ungrammatical dependencies were constructed by switching the elements that co-occurred in the grammatical sequences (*A* co-occurred with *C*, and *B* co-occurred with *D*). These occurred in the same locations and with the same frequency as in the grammatical sequences (see Figure 4.1, p140). In effect, the design is analogous to the two-grammar design that Redington and Chater (1996) proposed. However, in this case ungrammatical sequences could not be discriminated from the grammatical sequences on the basis of differences in first-order dependency information, nor on the frequency with which the legal and illegal bigrams occurred at test. Also, because the grammar did not permit any repeating elements, test sequences could not be classified on the basis of an abstract analogy. Finally, both grammatical and ungrammatical sequences were equally similar to exemplars – neither could be endorsed nor rejected on the basis of similarity. The sequential dependencies can only be correctly classified at test if participants learn that the two dependencies that they saw in the exemplar sequences are mutually exclusive. Hence, although participants may learn information about the surface features of the sequences, the transfer of that knowledge requires that they also learn the abstract relationships between the component elements of each sequential dependency.

Experiment 10 questioned whether participants could transfer knowledge of bigrams to a novel vocabulary where the stimuli are unconfounded with frequency cues. On the whole, participants were able to perform this task, but a closer inspection revealed that about half the participants were extremely successful, whilst the other half did not differ from controls. Confidence ratings indicated that those participants who had correctly classified test sequences were aware of the knowledge that guided their responses. The nature of the contingencies involved meant that, at test, participants could endorse either, but not both sets of sequences. Perhaps participants who did not demonstrate a learning effect were unable to decide which set of sequences to endorse. To exclude this possibility Experiment 11 changed the ungrammatical sequences that had been used in Experiment 10 so that there was only one possible mapping between the two vocabularies. Although overall classification performance did not reach statistical significance, inspection of the pattern of the results for this experiment revealed the same kind of distribution seen in Experiment 10. Clearly the distribution of responses observed in Experiment 10 could not be attributed to a failure to settle on a consistent mapping. Alternatively participants who did not demonstrate a learning effect either did not encode or were unable to recall the relevant information from the exemplars. Experiment 12 was identical to Experiment 10 with the exception that a series of post-experiment direct tests were administered to determine whether the distributions observed in the previous experiments could be attributed to differences in the information that participants abstracted from the exemplars. In this experiment the bimodal pattern of responses observed in the previous two experiments was replicated. The post-task direct tests revealed that participants who could accurately discriminate between the two sets of test sequences, but not those who were inaccurate, could freely recall the target bigrams embedded in the exemplar sequences. Experiment 13 used identical stimuli to Experiment 10 and 12, but used an incidental-orienting task (it did not refer to the target bigrams) to ensure that all participants studied the exemplars to a similar degree. Inspection of the pattern of results revealed, in this case, a more normal (non-bimodal) distribution of responses. Clearly participants can abstract knowledge of sequential dependencies between non-repeating elements, and transfer that knowledge to a novel vocabulary. Simple observation does not guarantee abstraction, but an incidental-orienting task does.

4.2 EXPERIMENT 10
THE TRANSFER OF SECOND-ORDER SEQUENTIAL DEPENDENCE

4.2.1 Introduction

This experiment sought to determine whether participants could discriminate between grammatical and ungrammatical sequences that contain no repeating elements in the absence of any frequency cues. If so the implication is that participants encode the contingent relationship between the component elements of a bigram when it is abstracted from an exemplar sequence. To test this a partial-grammar was constructed. The function of any grammar is to constrain the sequential ordering of vocabulary elements within a language. This grammar generated ninety-six sequences that each contained six different elements, from a vocabulary of eight elements (*A* through to *H*), but imposed constraints only upon the sequential ordering of half the vocabulary (*A* to *D*, hence it is a 'partial' grammar). Each sequence that the partial-grammar generated contained one of two pairs of contingent elements, for example *A* and *B* always co-occurred, and *C* and *D* always co-occurred (see Figure 4.1, p140). These contingent pairs were embedded in positions 3 and 4 of sequences that contained no other dependencies (*E-H* occurred randomly in positions 1 and 2, and 5 and 6).

The ability of participants to classify sequences in the source vocabulary was not tested in this experiment because it would be a relatively trivial recognition test—transfer is the best procedure to reveal abstract knowledge. Confidence ratings were taken for each decision that allowed participants' metaknowledge to be assessed. That is whether or not participants were aware of the knowledge that was used to classify test sequences (Dienes, Altmann, Kwan, & Goode, 1995b; Dienes & Altmann 1997; Dienes & Perner, 1996).

4.2.2 Method

Participants

Twenty members of the University of York participated in this study for either course credit or payment. No volunteers had taken part in any previous artificial grammar learning experiment.

Stimuli

A partial grammar was constructed that constrained four (*A-D*) of the eight vocabulary elements (*A-H*) that had been used in Chapters 2 and 3 (see Figure 4.2). Each sequence was six elements in length. Positions one and two, and five and six contained the non-contingent elements (*E-H*). These occurred randomly in each position with an equal frequency and so did not form sequential dependencies but served to embed the dependencies between the contingent elements (*A-D*). Each sequence contained one of two sequential dependencies between two different vocabulary elements (*AB, BA, CD* and *DC*) in positions three and four of each sequence. For example, where *A* occurs in position three, *B* always follows in position four. However, *B* may occur in position three, in which case it will always be followed by *A* in position four. The same rule also applies to another two elements, *C* and *D*. These sequences (Set 1) can be seen in Figure 4.1 with the non-contingent elements omitted for clarity.

Exemplars &Test Set 1	n	Test Set 2	n
– – *A B* – –	24	– – *C A* – –	24
– – *B A* – –	24	– – *A C* – –	24
– – *C D* – –	24	– – *D B* – –	24
– – *D C* – –	24	– – *B D* – –	24

Set 1 sequences were presented during the experimental training phase. For the test phase all participants were presented with both Set 1 and Set 2 sequences instantiated in a novel vocabulary. Control participants were presented with scrambled versions of Set 1 and 2 sequences. The stimuli can be seen in Appendix A.

Figure 4.1: Stimuli used in Experiment 10

For the exemplar sequences the ninety-six unique sequences (Set 1) that the grammar generated were instantiated with the symbols (source vocabulary) shown in Figure 4.2. Each contingent pair (*AB, BA, CD, DC*) occurred twenty-four times, each time in a unique sequence. These were presented twice in random order, and presented in a seven-page booklet for study. The sequences used as exemplars (Set 1) were also in the test phase, but this time were instantiated with syllables. A second set of sequences was also constructed for the test phase (Set 2). These were identical to the Set 1 sequences with the exception that the dependencies (or mapping) between the contingent vocabulary elements was reversed relative to Set 1 sequences (e.g. *AC, CA, DB,* and *BD*). The consequence of presenting two sets of sequences, one with reversed contingent pairings in a different vocabulary to the exemplars, is that either one, but not both, may be endorsed as "grammatical" according to the exemplars. That is, the mapping or identity of any element may not be induced by the frequency with which it either occurs or co-occurs with another element. The grammatical knowledge to be transferred across domains is knowledge of the relationship between the elements composing the contingent pairs, exhibited by participants consistently endorsing one, but not both sets of items. All one hundred and ninety-two test sequences were presented in one of two random orders, and instantiated with syllables according to the mapping in Figure 4.2. Stimuli are presented in Appendix A.

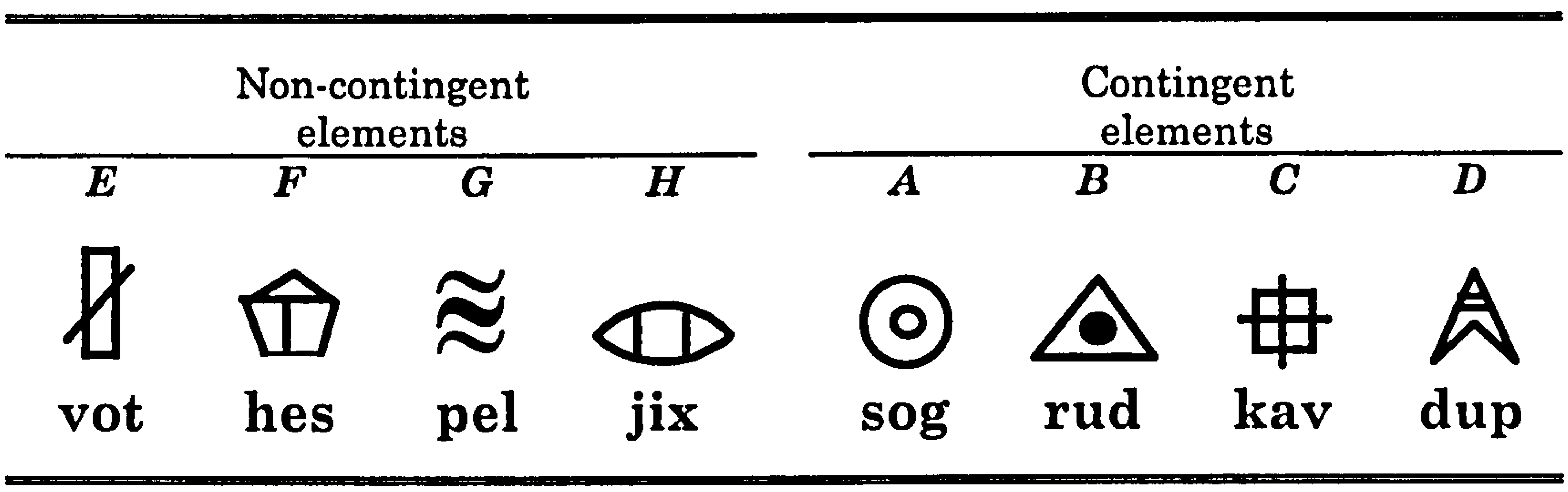


Figure 4.2: The mapping between vocabulary elements used in Chapter 4

A second training set of one hundred and ninety-two unique sequences was generated for Control participants. This set consisted of scrambled versions of the test sequences so that Control participants could not learn that contingent elements occurred in the same sequences. As such these scrambled sequences did not contain any sequential dependencies – each element occurred in each position of each sequence with an equal frequency. These were instantiated with symbols.

Design

This was a between-subjects design. Participants were trained on either scrambled (n = 10) or Set 1 grammatical sequences (n = 10), both were instantiated with symbols (source vocabulary) according to the mapping shown in Figure 4.2. All participants were tested on Set 1 and 2 test sequences instantiated as syllables (novel vocabulary). The order of test presentation was counterbalanced: half classified test sequences in one order, and half classified test sequences in another order.

Procedure

Superficially this might appear to be a relatively easy task. However, this experiment retains the conventional format of an artificial grammar learning experiment, and as in Chapters 2 and 3, participants were not informed that the sequences contained any rules nor were they explicitly instructed how to process the training sequences.

Participants were presented with either the scrambled or Set 1 training booklets to study for fifteen minutes, and instructed to 'learn as much about the sequences as they could, as they would be asked questions later.' Prior to the test phase participants were informed that 'all of the sequences they just saw were created using a complex set of rules' and that they would now see some sequences of syllables, of which some obeyed and some disobeyed the same set of rules as the sequences of symbols. Following each decision, participants were asked to rate how sure they were for each decision on a scale of 50-100% (where 50% is a complete guess, and 100% is absolutely sure).

4.2.3 Results

Scoring

The extent to which participants were able to discriminate between the syllable sequences was assessed by the number of "yes" responses for Set 1 and Set 2 sequences, for each participant. Each participant could legitimately endorse either set of sequences as grammatical, but not both. Consequently, each participants' hit rate was determined by the set of sequences that received the greatest number of "yes" responses, and their false alarm rate was determined by the set that received the lowest number of "yes" responses. For example, if a participant endorsed Set 1 sequences more than Set 2 sequences, the number of Set 1 sequences would be transformed into the proportion of consistent hits whilst the number of Set 2 sequences that were rejected would be transformed into the proportion of consistent correct rejections. If however, a participant had endorsed Set 2 sequences more than Set 1 sequences, the converse would be true—the number of endorsements to Set 2 sequences would be transformed into the proportion of consistent hits whilst rejections of Set 1 sequences would be transformed into the proportion of consistent correct rejections. The consequence of this procedure for calculating the overall discrimination acuity is that no individual false alarm rate can exceed a hit rate. Consequently no discrimination index (e.g. A' or d') that is based on signal-detection theory could be used, because such an index if applied to these data would theoretically describe only half the ROC curve (see Macmillan & Creelman, 1991). Instead the *percentage of consistent responses* was calculated using the formula $((h + cr)/2)*100$. This measure can be read as the percentage of correct classification scores.

Classification performance

The proportions of consistent responses are shown in Table 4.1. Six Control and six Experimental participants endorsed Set 1 sequences as grammatical. The remaining eight participants all endorsed Set 2 sequences as grammatical.

	Proportion of consistent responses Mean	*se*
Control	52%	(0.56)
hits	.59	(.04)
correct rejections	.45	(.04)
Experimental	70%	(7.31)
hits	.70	(.07)
correct rejections	.69	(.08)

Table 4.1: The proportions of consistent responses observed in Experiment 10

A one-way ANOVA revealed that Experimental participants were more consistent in classifying the two sets of sequences than control participants: $F(1, 19) = 6.01$, $p < .03$, $MS_e = 1614.38$, $\eta^2 = .25$. Overall, the Experimental participants were able to discriminate between the two sets of sequences in the absence of frequency cues. However, close inspection of the pattern of responses revealed that four of the experimental participants were extremely accurate in discriminating between the two sets of sequence, whilst six appeared not to differ from controls. The six Experimental participants whose classification performance fell below the median (53.65%) for that group (*non-learners*) did not classify reliably more sequences correctly than Control participants (52.08%, $se = .05$, and 51.88%, $se = 0.56$ respectively: $t(14) = 0.25$, $se = 8.33$, $p = .81$). Whilst those whose classification performance that fell above the median (*learners*) classified significantly more sequences correctly (96.48%, $se = 2.35$) than the *non-learners* and Controls combined (52%, $se = 0.39$): $t(8) = 33.30$, $se = 1.34$, $p < .01$. The frequency distributions of each group are shown in Appendix B.

Confidence & Accuracy

Participants were asked to report how confident they were for each of their classification decisions on a percentage scale of 50-100%, where 50 is a complete guess, and 100 is absolutely sure. Table 4.2 shows the mean confidence for correct and incorrect decisions, by group and by the partition between *learners* and *non-learners.*

Two measures of awareness are possible using confidence ratings. The *Guessing criterion* (Cheesman & Merikle, 1984) determines whether participants are accurate when they believe that they are literally guessing (50% sure). Unfortunately these data were not amenable to this analysis because the Experimental participants made very few guesses. Table 4.2. indicates that this was because the bimodal distribution in classification performance was reflected also in participants' confidence ratings.

		Mean confidence		
		correct decisions	*incorrect decisions*	*difference*
Control		55 (2.45)	55 (2.24)	0.06 (0.30)
	***upper*			0.74
	lower			-0.61
Experimental		67 (8.95)	65 (4.31)	2.18 (1.84)
	upper			6.34
	lower			-1.98
	learners	75 (8.94)	70 (10.29)	5.47 (4.23)
	upper			18.92
	lower			-7.98
	non-learners	62 (3.52)	62 (2.92)	-0.01 (0.76)
	upper			1.95
	lower			-1.98

*Standard errors appear in parenthesis.
**Upper and lower boundaries of the 95% confidence intervals.

Table 4.2: The mean confidence for correct and incorrect decisions, by group and by partition between *learners* and *non-learners* for Experiment 10

These data are however amenable to alternative analysis devised by Chan (1992). The *Zero-correlation criterion* determines whether confidence predicts accuracy. Chan (1992) found that when participants were informed of the rules of the grammar used to generate sequences their subsequent confidence in classifying test sequences predicted how accurate those classification decisions were. In contrast when participants studied exemplars under implicit learning conditions, their confidence in classifying test sequences did not predict how accurate those classification decisions were. Dienes and Altmann (1997) found that transfer could be implicit according to this measure. This correlation, or lack of, is best expressed as the difference between the mean confidence for correct decision and the mean confidence for incorrect decisions. If there is no difference, participants' knowledge is said to be implicit and explicit where a difference is found.[1] The

[1] Of course this measure is problematic because demonstrating implicit knowledge relies upon a null effect of awareness, whereas the guessing criterion does not.

interesting feature of this criterion is that the difference between implicit and explicit knowledge is reflected as a graded dimension, rather than as a binary dimension such as the distinction between direct and indirect tests. Thus, what is important is not strictly whether the difference between confidence for correct and incorrect decisions is zero, but whether it differs from controls whose ability to discriminate between sequences can only be a consequence of learning during the test phase.

A comparison between Control and Experimental participants did not reveal a significant difference ($t(18) = 1.14$, $se = 1.86$, $p = .27$) which might suggest that the overall difference between the groups' classification performance was due to the application of 'implicit knowledge'. However when the *non-learners* were grouped with Controls, and compared with *Learners* a reliable difference was found ($t(18) = 2.66$, $se = 2.04$, $p < .02$). The knowledge that was responsible for *learners'* ability to classify test sequences was largely explicit, but an implicit component cannot be ruled out because the lower bounds of the confidence intervals for the *learners* were below zero. However, this may have been due to the high discrimination acuity of *learners* – participants were sometimes as confident in the few errors that they made, as they were in the correct decisions.

4.2.4 Discussion

This experiment confirmed that *some* participants abstracted the second-order contingent relationship between the vocabulary elements in the exemplar sequences and were subsequently able to discriminate between the two sets of test sequences in a different vocabulary. Participants who could discriminate between the two sets of test sequences, *learners*, were clearly aware of that knowledge. This finding is inconsistent with the results obtained by Dienes and Altmann (1997). They observed that transfer could be implicit (according to the *zero-correlation criterion*). Could their results be attributed to implicit knowledge of first-order dependency information? In their Experiment 2 the sequences were generated by a finite-state grammar that could have confounded first- and second-order dependency information.

Chapter 3 demonstrated for this grammar that participants were only able to transfer knowledge of first-order dependency information. The difference between *learners* (97%) and *non-learners* (52%) performance in this experiment indicates that the application of this kind of knowledge is not graded. What is the difference between these two sub-groups of experimental participants? If knowledge of sequential dependencies is learned incrementally, as is suggested by SRN models of artificial grammar learning, then non-learners may only have acquired knowledge of first-, rather than second-order dependencies. The stimuli were designed so that knowledge of first-order dependencies would not allow the two sets of test sequences to be correctly classified. Perhaps the *non-learners* had in fact learned something of first-order information but were unable to apply that knowledge. An alternative possibility stems from the nature of the test sequences themselves, rather than participants representations of the exemplars. Both sets of test sequences preserved the contingency between elements appearing in positions 3 and 4, so that either set, but not both, could be endorsed as grammatical. Perhaps the *non-learners* could not decide which set to endorse and which to reject. This possibility is explored in Experiment 11.

4.3 EXPERIMENT 11 TRANSFER OF SECOND ORDER SEQUENTIAL DEPENDENCE WITH ONLY ONE POSSIBLE MAPPING BETWEEN VOCABULARIES

4.3.1 Introduction

Experiment 10 found that only some participants were able to classify test sequences according to the second-order contingent relationships between vocabulary elements, and that this knowledge was explicit. However, in that Experiment knowledge of the contingent relationship between vocabulary elements could be mapped onto either set of test sequences. About half the participants were able to discriminate between the two sets of test sequences, whilst the remainder did not discriminate any better than controls. Experiment 11 investigates whether *all* trained participants transfer their knowledge of the second-order dependencies if only one of the two sets of test sequences can be correctly endorsed as grammatical. The same exemplar and Set 1 test sequences that were used in Experiment 10 are again used. However, the Set 2 sequences were modified so that although they did not differ in terms of the frequency distribution of elements (and so could only be rejected on the basis of second-order dependency information) they could not be endorsed as grammatical. Consequently, unlike in Experiment 10, there is only one possible mapping between the dependencies in the exemplar sequences and the dependencies in the test sequences.

4.3.2 Method

Participants

Twenty members of the University of York participated in this experiment either for course credit or payment. Care was taken to ensure that volunteers had not previously taken part in any artificial grammar learning experiment.

Stimuli

The grammatical training and test sequences were identical to the Set 1 sequences that were used in Experiment 10. Control participants were also trained on the same set of scrambled sequences. The Set 2 sequences were modified so that they could now be correctly rejected as ungrammatical on the basis that the four non-contingent elements now occurred in position 4 and were not dependent upon the elements that occurred in position 3. Elements that occurred in position 4 of each sequence were simply swapped with those that occurred in position 5 (see Figure 4.3).

Exemplars & Test Set 1	*n*	Ungrammatical Test Set 2	*n*
– – *A B* – –	24	– – *C* – *A* –	24
– – *A B* – –	24	– – *A* – *C* –	24
– – *C D* – –	24	– – *B* – *D* –	24
– – *C D* – –	24	– – *D* – *B* –	24

Set 1 sequences were presented during the experimental training phase. For the test phase all participants were presented with both Set 1 and Set 2 sequences instantiated in a novel vocabulary. Control participants were presented with scrambled versions of Set 1 and 2 sequences.

Figure 4.3: Stimuli used in Experiment 11

Although overall the elements *E-H* now occurred in position 4 with the same frequency as the contingent elements in the grammatical set, they co-occurred with each of the contingent elements that occurred in position 3. Whereas, in the Set 2 sequences used in Experiment 10 each element that occurred in position 4 could only co-occur with one other element in position 3, hence the contingency was preserved. In this set of ungrammatical sequences each contingent element (*A-D*) in position 3 would be followed by *E, F, G,* or *H,* and so the contingency was not preserved. The sequences can be seen in Appendix A.

Design

This was a between-subjects design. Participants were trained on either scrambled (Control n = 10) or Set 1 grammatical sequences (Experimental n = 10), instantiated as syllable sequences. All participants were tested on the same test sequence instantiated as symbols. Unlike Experiment 10, participants' responses do not determine which set of sequences is grammatical and the hit rate can exceed the false alarm rate. Otherwise, the design of this experiment was identical to that of Experiment 10.

Procedure

The procedure was identical to that employed in Experiment 10.

4.3.3 Results

Classification performance

The percentages of correct classification scores are shown in Table 4.3. Unlike Experiment 10, participants may only endorse Set 1 sequences as grammatical. Consequently the false alarm rate may exceed the hit rate. A one-way ANOVA on the percentages of correct classification scores revealed that the Experimental participants did not correctly classify reliably more sequences than Controls ($F(1, 19) = 2.70, p = .12, MS_e = 644.59, \eta^2 = .13$).

	Proportion of consistent responses Mean	*se*
Control	45%	(3.15)
hits	.49	(.04)
correct rejections	.41	(.03)
Experimental	56%	(6.15)
hits	.64	(.06)
correct rejections	.48	(.08)

Table 4.3: The proportions of consistent responses observed in Experiment 11.

Overall, the Experimental participants were unable to discriminate between the two sets of sequences. However, inspection of the distribution of responses revealed a similar pattern to the one observed in the previous experiment. Two Experimental participants had high scores (91.15%, *se* = 8.85) and were classed as *learners*, the remaining eight Experimental participants (47.46%, *se* = 1.84) did not appear to differ from controls and were classed as *non-learners*. As in Experiment 10, *t*-tests revealed that the *non-learners* did not differ reliably from Controls ($t(16) = 0.67$, *se* = 3.89, $p = .51$) and without performing an analysis (that would not be appropriate with only two participants) *learners* were clearly more accurate than both the *non-learners* and *Controls*. The distributions are given in Appendix B.

Confidence & Accuracy

The lack of overall difference between the classification scores of the groups does not warrant an analysis of confidence by the *zero-correlation criterion*. The mean confidence ratings are given in Table 4.4.

		Mean confidence		
		correct decisions	*incorrect decisions*	*difference*
Control		59 (3.07)	60 (3.29)	-0.52 (0.34)
	***upper*			0.25
	lower			-1.28
Experimental		64 (4.77)	53 (6.52)	10.62 (9.57)
	upper			32.27
	lower			-11.04
	learners	84 (10.95)	36 (36.18)	47.87
	non-learners	59 (3.78)	58 (2.90)	1.30 (2.44)
	upper			7.08
	lower			-4.48

*Standard errors appear in parenthesis.
**Upper and lower boundaries of the 95% confidence intervals.

Table 4.4: The mean confidence for correct and incorrect decisions, by group and by the partition between *learners* and *non-learners* for Experiment 11

Of the two learners, one scored 100% ($h = 1.0$, $cr = 1.0$) correct with a mean confidence of 95%, whilst the other classified 82.29% ($h = .97$, $cr = .68$) of the sequences correctly with a mean confidence in correct decision of 73.10% and 72.35% for incorrect decisions. Thus for one of the two learners the knowledge was clearly explicit whilst the other was only marginally more confident in correct decision than for incorrect decisions. Unfortunately, it is impossible to examine how these two participants might have differed.

4.3.4 Discussion

Despite the differences in stimuli, the pattern of results in this Experiment was similar to the distributions observed Experiment 10: A few participants were able to classify a substantial number of sequences correctly whilst others classified no more than controls. It seems that the type of discrimination required in this Experiment (with only one possible set of grammatical sequences) was not easier than the type of discrimination in Experiment 10 (where either test set could be endorsed a grammatical). However, the pattern of responses reflects the same kind of distribution observed in Experiment 10. Thus the bimodal distribution of responses seen in Experiment 10 could not have been due to some participants failing to decide between possible mappings. An alternative explanation for why some participants failed to discriminate between the two sets of sequences is that they may not encode the dependency during training, perhaps because they do not occur in the initial portions of sequences where participants seem to be more sensitive (c.f. Chapter 3). Experiment 12 replicates Experiment 10 but includes a series of direct tests to determine what information participants learn from the exemplar sequences and are able to map onto the test sequences.

4.4 EXPERIMENT 12 DIRECT AND INDIRECT TRANSFER OF SECOND-ORDER SEQUENTIAL DEPENDENCE

4.4.1 Introduction

One possible explanation for why, in the previous two experiments, some participants classified sequences near perfectly and others did not differ from controls might be that participants are unable to recall the target dependencies from the exemplars, and are consequently unable to map them onto the novel vocabulary. This Experiment was a replication of Experiment

10, with the exception that three direct tests were administered following the test phase. This allowed a comparison of performance on both a direct and the indirect (classification) task. As mentioned earlier bigram knowledge is best described as episodic, and the contingencies within that bigram cannot be transferred (indirect test) unless one can remember the bigram itself (direct test). Recall that Gomez (1997) had found that participants were only able to classify sequential dependencies in a novel vocabulary if they were able to recognise them in the source vocabulary. In principle, this may be because participants are relatively less sensitive to the internal portions of the sequences as Chapter 3 suggested. To increase power this experiment also included a slightly larger sample size (n = 24).

4.4.2 Method

Participants

Twenty-four members of the University of York participated in this experiment either for course credit or payment. Care was taken to ensure that volunteers had not previously taken part in any artificial grammar learning experiment.

Stimuli

The stimuli used in this experiment were identical to those used in Experiment 10.

Direct Test

This experiment included three direct tests of participants' knowledge of the contingent symbols seen during training, of which syllables were contingent during test, and of which syllables corresponded to which symbols. Each task took the same format where participants were required to indicate on an 8 by 8 matrix (with each vocabulary elements arrayed along the top and left side) which elements were paired. Each test gives scores in the range of 0 – 4, where 0 = no direct or explicit knowledge of the contingencies, and 1 = direct knowledge of one of the contingencies. These tasks are shown in Appendix A.

Design

The design was identical to that employed in Experiment 10 with the exception that direct tests were administered after the classification phase.

Procedure

The procedure was identical to that used in Experiment 10 with the addition of the three direct tests. The direct tests were administered immediately following the test phase.

4.4.3 Results

Classification performance

The percentages of correct classification scores are shown in Table 4.5. Note that as in Experiment 10, participants may endorse either set of sequences as grammatical, but not both. Consequently the false alarms rate may not exceed the hit rate. Five Experimental and five Control participants endorsed Set 1 sequences as grammatical, the remaining fourteen participants endorsed Set 2 sequences.

	Proportion of consistent responses	
	Mean	*se*
Control	52%	(0.58)
hits	.58	(.02)
correct rejections	.47	(.02)
Experimental	72%	(6.77)
hits	.77	(.06)
correct rejections	.68	(.08)

Table 4.5: The proportions of consistent responses observed in Experiment 12.

Overall, Experimental participants discriminated between the two sets of sequences with a greater degree of accuracy than Control participants ($F(1, 23) = 8.41$, $p < .01$, $MS_e = 2329.69$, $\eta^2 = .28$). Inspection of the distribution of Experimental participants' classification performance again revealed a bimodal distribution. The six participants who fell below the median (55.99%) were classed as *non-learners* and the six who exceeded the median were classed as *learners*. The *non-learners* did not classify reliably more sequences than controls ($t(16) = 0.14$, $se = 0.93$, $p = .89$). However, the *learners* did classify reliably more sequences correctly (92%, $se = 6.93$) than did the *non-learners* (53%, $se = .59$) and Controls combined (52%, $se = .42$): $t(22) = 10.09$, $se = 3.89$, $p < .01$.

Confidence & Accuracy

Mean confidence for correct and incorrect decision can be seen in Table 4.6.

		Mean confidence		
		correct decisions	*incorrect decisions*	*difference*
Control		54 (1.66)	54 (1.69)	0.04 (0.14)
	***upper*			0.33
	lower			-0.26
Experimental		62 (4.49)	60 (3.21)	1.99 (1.54)
	upper			5.47
	lower			-1.48
	learners	72 (8.75)	67 (5.89)	7.42 (3.77)
	upper			23.64
	lower			-8.80
	non-learners	55 (2.26)	55 (2.43)	-0.33 (0.15)
	upper			0.04
	lower			-0.70

*Standard errors appear in parenthesis.
**Upper and lower boundaries of the 95% confidence intervals.

Table 4.6: The mean confidence for correct and incorrect decisions, by group and by the partition between *learners* and *non-learners* for Experiment 12

Overall, there was no reliable difference between Experimental and Control participants in terms of the *zero-correlation criterion* ($t(20) = 1.40$, $se = 1.40$, $p = .18$) which might indicate that the discriminations were implicit. However there was a reliable difference when the *learners* are compared to the *non-learners* and Controls combined ($t(20) = 3.826$, $se = 1.44$, $p < .01$). This indicates that *learners'* knowledge was explicit (see Table 4.6), although an implicit component cannot be ruled out because the lower bounds of the 95% confidence intervals was below zero.

Direct test performance

The direct tests were essentially diagnostic tools used to determine the source of the bimodal distribution observed in the responses of the Experimental participants. The first direct test assessed whether participants had direct knowledge of which elements were contingent during the training phase. This score ranged from 0-4. Although there were only two pairs of contingent elements (*AB* and *CD*), a score of 4 was achieved by indicating that they could occur in either order. Six experimental participants scored the maximum of four, whilst the remaining six participants scored less than two. This indicates that these participants did not encode the contingent relationship between elements. The second test was similar to the first, but examined which syllables seen during testing participants thought formed pairs. The third test was again similar, and examined which syllables (test) corresponded to which symbols (training) — as in the classification phase participants could choose between two possible mappings. The three test scores were in perfect agreement. That is, if a participant scored 4 on the first test they also scored 4 on the remaining two tests. Similarly, participants who scored less than 4 on the first test also scored less than 4 on the second and third tests. This allowed a partition to be made between Experimental participants with (*aware*, $n = 6$) and without direct knowledge (*unaware*, $n = 6$) of the contingencies between vocabulary elements and the mapping between domains. This partition corresponded exactly with the partition made between *learners* ($n = 6$) and *non-learners* ($n = 6$). The scores can be seen in Appendix B. It seems that bigram knowledge is only available for transfer if it is amenable to recollection, and demonstrably explicit.

4.4.4 Discussion

Why were some participants unable to recall the target bigrams within the exemplars that they had studied? There are at least two explanations. First, one could speculate about whether there were differences in the amount of attention that participants apportioned to the exemplars during the training phase. Second, a good deal of research (including data reported here) has found that participants are relatively more sensitive to the initial and terminal portions of a sequence than they are to the central positions. Perhaps *non-learners* are more sensitive to these areas and do not notice the target bigrams. Both of these issues are circumvented in Experiment 13 by using an incidental orienting to ensure that all participants studied the exemplars to at least the same extent and would be aware of the target bigrams.

4.5 EXPERIMENT 13
AN INCIDENTAL ORIENTING TASK FACILITATES THE TRANSFER OF SECOND ORDER SEQUENTIAL DEPENDENCE

4.5.1 Introduction

Experiment 13 was identical to Experiments 10 and 12, with the exception that an incidental-orienting task was devised to ensure participants studied the exemplars to at least the same degree, and would be aware of the target bigram. A number of different incidental orienting tasks have been used in artificial grammar learning. For example, Whittlesea and Dorken (1993) compared the classification performance of participants who had been asked to read out sequences as a distractor task for another experiment with participants who were asked to indicate whether a particular element was a repeating element, and with participants who simply memorised sequences. Although they found only small differences in the source vocabulary, in the transfer condition participants who had been asked to identify repeats were significantly better than the other conditions. Of course that particular manipulation highlighted the relative salience of patterns of repeating elements that may have been critical in novel vocabulary classification. Mathews *et al.* (1989) used an alternative manipulation that attempted to induce different learning strategies during training. To induce episodic-based processing Mathews *et al.* (1989) asked participants to match an exemplar with an array of other exemplars. To induce rule-abstraction participants were asked to edit ungrammatical sequences to render them grammatical. In both cases participants were given feedback. These manipulations have revealed relatively small differences in subsequent response patterns using finite-state grammars (although see Shanks *et al.*, 1997 for results using a bi-conditional grammar)

The orienting task used in this experiment was not designed to induce different learning strategies or subsequent response patterns. Rather it was designed to ensure that all of the trained participants attended to the exemplars to at least the same degree and were aware of the target bigrams that occurred in the central positions of each exemplar sequence. The partial grammar generates sequences that contain six different vocabulary elements out of a possible eight (*A-H*). Four of those elements are always contingent upon one another (*AB* & *CD*), but the two dependencies could never be present in the same sequence, consequently each exemplar contained all of the non-contingent elements (*E-H*) but was always missing one of the contingent pairs. Participants were asked to indicate which symbols were missing from each exemplar.

4.4.2 Method

Participants

Twenty-four members of the University of York participated in this study for either course credit or payment. Care was taken to ensure that no volunteer had previously taken part in any artificial grammar learning experiment.

Stimuli

The stimuli were identical to those used in Experiments 10 and 12 with the exception that an incidental-orienting task was applied during the training phase. The same direct tests used in Experiment 12 were also administered after the classification test.

Design

The design was identical to the one employed in Experiments 10 and 12.

Procedure

The procedure was identical to the one employed in Experiment 12, with exception that the incidental orienting task replaced the study period that had been used previously. The way in which the sequences were constructed meant that within any one sequence two of the four contingent symbols did not occur. Participants were asked to indicate, by either reproducing or assigning each a number) which symbols were missing from each exemplar sequence.

4.5.3 Results

Classification performance

The proportions of consistent correct classification scores are shown in Table 4.7. Four Experimental and three Control participants endorsed Set 1 sequences as grammatical, the remainder endorsed Set 2.

	Proportion of consistent responses Mean	*se*
Control	53%	(0.86)
hits	.65	(.06)
correct rejections	.41	(.06)
Experimental	87%	(5.78)
hits	.87	(.05)
correct rejections	.87	(.06)

Table 4.7: The proportions of consistent responses observed in Experiment 13

Overall Experimental participants discriminated between Set 1 and Set 2 sequences with a greater degree of accuracy than Control participants, $F(1, 23) = 33.82, p < .01, MS_e = 6929.63, \eta^2 = .61$. Inspection of the distribution of Experimental participants' responses revealed that only three participants classified sequences at a level comparable to that of Controls (because the median, 97%, was greater than the mean, 87%, a median split would be inappropriate for these data). These three *non-learners* classified 54% (*se* = 1.42) of the test sequences correctly compared to the 98% (*se* = 0.55) that the nine *learners* classified correctly. These distributions are given in Appendix B.

Confidence & Accuracy

The mean confidence ratings for correct and incorrect decisions are given in Table 4.8. There was a marginal difference between Experimental and Control participants' in terms of the *zero-correlation criterion* ($t(22) = 2.01$, $se = 11.53$, $p < .06$). This indicates that the discriminations made by Experimental participants were largely explicit. When the three *non-learners* were combined with the Control group this difference reached statistical significance ($t(22) = 2.71$, $se = 11.25$, $p < .01$). From the results of the previous experiments and an inspection of the differences in confidence ratings across the cells of Table 4.8 it is clear that knowledge of the contingent relationship between the four target vocabulary elements was explicit. Was this pattern in participants confidence also present (as in Experiment 12) in the direct tests?

		Mean confidence		
		correct decisions	*incorrect decisions*	*difference*
Control		66 (4.76)	66 (4.95)	-0.02 (0.57)
	***upper*			1.24
	lower			-1.27
Experimental		77 (5.02)	54 (10.50)	23.16 (11.51)
	upper			48.50
	lower			-2.18
	learners	83 (4.87)	53 (14.03)	30.57 (14.68)
	upper			64.42
	lower			-3.27
non-learners		59 (6.94)	58 (7.48)	0.94 (1.10)
	upper			5.68
	lower			-3.81

*Standard errors appear in parenthesis.
**Upper and lower boundaries of the 95% confidence intervals.

Table 4.8: The mean confidence for correct and incorrect decisions, by group and by the partition between *learners* and *non-learners* for Experiment 12

Knowledge available on direct tests

The direct tests were identical to those used in the Experiment 12. The first direct test assessed whether participants had direct knowledge of which elements were contingent during the training phase. This score ranged from 0-4. Although there were only two pairs of contingent elements (*AB* and *CD*), a score of 4 was achieved by indicating that they could occur in either order. Nine experimental participants scored the maximum of 4, whilst the remaining three experimental participants scored less than two. The second test was similar to the first, but examined which syllables seen during testing participants thought formed pairs. The third test was again similar, and examined which syllables (test) corresponded to which symbols (training) final test. The three test scores were in perfect agreement. That is, the nine participants who could remember the contingent pairings of elements in the exemplars and scored 4 on the first test also scored 4 on the remaining two tests. Similarly, the three participants who scored less than 4 on the first test also scored less than 4 on the second and third tests. As in Experiment 12 a partition was made between Experimental participants with (*aware*, $n = 9$) and without direct knowledge (*unaware*, $n = 3$) of the contingencies between vocabulary elements and the mapping between domains. This partition corresponded exactly with the partition made between *learners* ($n = 9$) and *non-learners* ($n = 3$). The scores can be seen in Appendix B. This confirms the findings of Experiment 12, and of Gomez (1997) that knowledge is only available for transfer if it is amenable to recollection, and demonstrably explicit.

4.5.4 Discussion

The results of Experiment 13 revealed that the majority of trained participants were able to learn the contingent relationship between vocabulary elements, and transfer that knowledge to the novel vocabulary. This is in contrast to Experiments 10-12 where substantial proportions of participants were unable to do so. Clearly the inclusion of the incidental orienting task ensured that participants attended each exemplar to the same extent, and did not attend more to the initial and terminal portions of the exemplars (as may have been the case in Chapter 3).

4.6 CHAPTER SUMMARY

Chapter 4 demonstrated that participants could learn and transfer second-order dependency information, in a language that contains no-repetition structures, and could transfer that knowledge to a novel vocabulary. This knowledge was largely explicit.

Because finite-state grammars often confound first- and second-order dependency information (relative to ungrammatical sequences) a rather simpler partial grammar was designed to generate only two sequential dependencies, only one of which could occur in any one sequence. 'Ungrammatical' sequences were constructed by switching the dependent elements and so could not be rejected on the basis of first-order dependency information (except Experiment 11), nor on the frequency with which the 'legal' and 'illegal' bigrams occurred at test. Also, because the partial-grammar did not permit any repeating elements, test sequences could not be classified on the basis of an abstract analogy. The sequential dependencies can only be correctly classified at test if participants learn that the two dependencies that they saw in the exemplar sequences were mutually exclusive and could map that knowledge onto dependencies in the novel vocabulary. Experiment 10 determined that *some* participants could transfer knowledge of bigrams to a novel vocabulary but close inspection revealed that about half the participants were extremely successful, whilst the other half did not differ from controls. This knowledge was explicit according to the *zero-correlation criterion.* Experiment 11 excluded the possibility that this distribution of responses was a consequence of the *non-learner* participants being unable to decide which set of sequences to endorse and which to reject. Again confidence ratings indicated that those participants who had correctly classified test sequences were aware of the knowledge that guided their responses. Although overall classification performance did not reach statistical significance, inspection of the pattern of the results for this experiment revealed the same kind of distribution seen in Experiment 10. Experiment 12 was identical to Experiment 10 with the exception that a series of post-experiment direct tests were administered to determine whether the distributions observed in the previous experiments could be attributed to differences in the information that participants abstracted from the exemplars. In this experiment the bimodal pattern of responses observed in the previous two experiments was replicated. The direct tests revealed that participants who could accurately discriminate between the two sets of test sequences, but not those who were inaccurate, could freely recall the target bigrams embedded in the exemplar sequences. There are a number of possible reasons why some participants did not learn the target bigrams. Participants may not study the sequences to the same degree, or they may focus on the initial and terminal portions of each sequence that were variant (*E-H* occurred randomly) and erroneously concluded that the target positions were also variant (*A-D* were not random). To circumvent these possible differences Experiment 13 used identical stimuli to Experiment 10 and 12, but used an incidental-orienting task (it did not directly refer to the target bigrams) to ensure that all participants studied the exemplars to a similar degree. Inspection of the pattern of results did not reveal a strongly bimodal distribution of responses. Clearly participants can abstract knowledge of sequential dependencies between non-repeating elements, and transfer that knowledge to a novel vocabulary. Simple observation does not guarantee abstraction of the sequential dependencies between non-repeating elements, but an incidental-orienting task does. One of the interesting features of these data is that this knowledge of sequential dependencies seems to be *all-or-none* rather than graded – the high discrimination acuity indicted that each sequence was as likely to be correctly classified as any other was.

How abstract was the knowledge of the target bigram?

The issue of abstraction is a complex one and Chapter 1 discusses a variety of ways in which knowledge can be described as abstract. The knowledge that participants learned from the target bigram in exemplar sequences was abstract in the sense that it encoded the contingent relationships between the vocabulary elements. It was also abstract in the sense that it could be transferred, or at least mapped, onto sequences in a novel vocabulary. It was rule-like in two senses. First previously unseen sequences were correctly classified, and the rule was applied equally to each new category member (hence the *all-or-none* nature of the distributions). This interpretation is weakened if we assume that participants learned that the non-contingent elements were highly variant. In which case the test sequences were not, strictly speaking, new category members. Importantly, the knowledge was rule-like in the sense that it encoded the contingent relationship between vocabulary elements. The rules were learned incidentally, and under 'implicit learning' conditions. Of course, calling a behaviour rule-like does not imply that those rules are represented in symbolic form. Connectionist networks are often described as exhibiting rule-like behaviour, for example they can learn the *–ed* past rule for the past tense of verbs following exposure to exemplars, but do not explicitly encode that information in explicit form (Rumelhart & McClelland, 1986; Plunkett & Marchman, 1994). This is a theoretical issue that is discussed in Chapter 6.

Conclusions

Participants are able to abstract knowledge of second-order dependency information under implicit learning conditions, and subsequently use that knowledge to determine the well- or ill-formedness of previously unseen sequences presented in a novel vocabulary. The sequences that contained these second-order dependencies could be neither endorsed nor rejected on the basis of their similarity to exemplars, but only on the basis that particular elements predicted other vocabulary elements. Chapter 3 determined that knowledge of first-order dependency information was responsible for much of the discrimination between grammatical and ungrammatical sequences presented in a novel vocabulary that had been reported by Altmann *et al.* (1995). How do the experiments in Chapters 3 and 4 differ? First the Experiments in Chapter 4 used stimuli that could not be discriminated on the basis of first-order dependency information, those in Chapter 3 did. Such knowledge need not be represented in a rule-like form, and may or not have been 'implicit'. A second difference between the experiments in Chapters 3 and 4 concerns the simplicity of the grammar. Chapter 3 used a relatively complex grammar, where each sequence contained multiple dependencies. In Chapter 4, each sequence contained only one dependency that could only occur in a restricted location within any sequence. The ease with which these dependencies could be mapped was reflected in the all-or-none pattern of responses and high levels of explicit knowledge that were observed. However, complex and simple knowledge are orthogonal to implicit and explicit knowledge. For example, simple conditioning does not occur in humans unless it is accompanied by an awareness of the contingencies involved (e.g. Brewer, 1974; Boakes, 1989). In contrast one extremely simple learning paradigm – invariant learning – frequently finds that participants are sensitive to invariant features without knowledge of those features being revealed by direct tests (e.g. Bright & Burton, 1998; Cock, Berry & Gaffan, 1994). In principle, Experiments 12 and 13 could have provided strong evidence of a dissociation between implicit and explicit knowledge if participants were able to classify tests sequences in Experiments 12 and 13 without that knowledge being revealed on the direct tests. If such a dissociation had occurred it would have satisfied both the *information* and *sensitivity criteria* (the target bigrams were the only means to classify test sequences, c.f. Shanks & St. John, 1994) for demarcating implicit and explicit knowledge. However, this was not found to be the case. This issue is discussed further in Chapter 6.

A third difference concerns the exclusion of repeating elements. A number of workers have demonstrated that sequences in a novel vocabulary can be discriminated on the basis of an abstract analogy between patterns of repeating elements and stored exemplars (e.g. Brooks & Vokey, 1991; Whittlesea & Dorken, 1993), whilst others have argued that such patterns are encoded automatically during training (e.g. Mathews & Roussel, 1997). The mapping of patterns of repeating elements is relatively trivial in comparison to determining a mapping between non-repeating elements. For example, the grammar-used in Chapters 2 and 3 generated repetition structures with only two out of eight possible vocabulary elements. At test, participants need only map the patterns formed by just those two elements on an sequence-by-sequence basis. The simplest mechanism that has been proposed to map knowledge of non-repeating elements is more complex. Redington and Chater (1996) discussed a number of simple 'toy' models that encode *n*gram information from exemplars in one vocabulary, and incrementally determine a mapping between the elements that compose those *n*grams onto elements in the novel vocabulary. For example, if every exemplar begins with *MS, MV,* and *VX,* and at test participants see sequences such as *JDHBHF, BFHHHH,* and *JBHFJ,* participants can deduce that if J begins a sequence it can be followed by one of two letters. Hence *J* in the novel vocabulary corresponds to *M* in the source vocabulary, and *B* corresponds to *V* as it can begin a sequence and follow *M*. Similarly *F* must correspond to *X* because it is the only element that can follow *V*, and so on. The point here is that even in these simple toy models, knowledge of the sequential dependencies between non-repeating elements, can only be mapped onto sequences in a novel vocabulary by computing that mapping over a series of test sequences, rather than on a sequence-by-sequence basis. But in Chapter 3 there was no evidence that participants mapped knowledge of sequential dependencies between non-repeating elements, that was present in the source vocabulary, onto the novel vocabulary. Perhaps the patterns of repeating elements are considered the easiest way to classify sequences, even though such a strategy would have been unsuccessful in the experiments reported in Chapter 3. In contrast Gomez, Gerken & Schvaneveldt (in press) found no discrimination between grammatical and ungrammatical sequences in a language that did not contain repeating elements. That study was carefully controlled in that the initial and terminal portions of each sequence could not cue an accurate grammaticality decision. This finding is not problematic because Experiment 11 also found no convincing evidence that participants were sensitive to difference in first-order dependency information in the internal portions of each sequence. Perhaps this was also true for the Gomez *et al.* study although their studies used a typing task during acquisition from which participants ought to have learned the relevant information rather than the traditional passive observation procedure during the learning phase that this Chapter has found does not guarantee learning. The question remains whether or not participants are able to learn and transfer second-order sequential dependencies between non-repeating elements in languages that also contain repetition structures. Chapter 3 found no evidence for knowledge of this kind, but this Chapter has demonstrated that it is possible for participants to learn and transfer this kind of information. In Chapter 5 the relationship between participants' sensitivity to dependencies between repeating and non-repeating elements is explored.

CHAPTER FIVE

A FUNCTIONAL DISSOCIATION BETWEEN THE CLASSIFICATION OF TWO FORMS OF SEQUENTIAL DEPENDENCE

5.1 INTRODUCTION

Chapter 5 begins by extending the finding that participants are able to abstract knowledge of second-order sequential dependencies between non-repeating elements and subsequently transfer that knowledge to a novel vocabulary. This was neither a new finding (e.g. Gomez & Schvaneveldt, 1994; Shanks et al, 1997), nor a new claim: a number of workers have stressed the importance of such knowledge in the representation of both artificial and natural languages (e.g. Braine, 1966; Smith, 1966). However, the partial-grammar used in Experiments 10-14 was relatively simple in comparison to the finite-state grammars that are typically used in artificial grammar learning research, not least because that language did not contain dependencies between repeating elements. This simplicity has confirmed the view that bigram knowledge is rule-like knowledge (c.f. Reber, 1993). That is not to say that such knowledge is *represented* as rules, simply that the behaviour is characteristically rule-like in a number of respects. First, fragments of exemplar sequences encode the sequential dependencies between vocabulary elements. Second, such knowledge is abstract in the sense that it can be mapped onto dependencies between novel vocabulary elements. It is also important to note that verbal report and direct probing can reveal this knowledge. This rule-like knowledge of the language is important, but what is the influence of episodic knowledge in artificial languages?

5.1.1 Similarity and rule-based processing are predicated upon different forms of sequential dependence.

One aim of this chapter is to investigate the relationship between similarity and rule-based classification in novel vocabularies. The classification of test sequences according to their similarity to stored exemplars could in principle play a greater role when those sequences are presented in a novel vocabulary than when they are presented in the same vocabulary the exemplars. In the source vocabulary, a number of workers have found that even when the similarity of test sequences to exemplars is orthogonal to their grammatical status, there remains a significant residual effect of either grammatical status (e.g. Knowlton & Squire, 1992; Vokey & Brooks, 1992) or of chunk strength (e.g. Higham, 1997b; Johnstone & Shanks, 1999). In contrast, Brooks and Vokey (1991, see also Whittlesea & Dorken, 1993) argued that the classification of test sequences in a novel vocabulary proceeds on the basis of the similarity of the dependencies between repeating elements to repetition structures in the exemplar sequences. Dependencies of non-repeating elements, other than position and frequency of occurrence are discarded because in a novel vocabulary similarity can only be computed over repeating elements (c.f. Chapter 3). This claim is supported by the finding that when test sequences are presented in randomly changing vocabularies their correct classification can be attributed solely to the patterns of repeating elements alone (e.g. Whittlesea & Dorken, 1993; Manza & Reber, 1997; Redington & Chater, 1996). Hence, according to the episodic-exemplar based theories the similarity of repetition structures is the basis of classification in a novel vocabulary. In contrast, both the fragmentary and abstractionist accounts of artificial grammar learning are predicated not only upon dependencies between repeating elements, but also upon those between non-repeating elements. These two positions can be distinguished on the basis of how those two forms of dependence are then mapped between vocabularies. Of course, the representation of different forms of sequential dependency can only be inferred on the basis of differences in classification and perhaps phenomenology. So rather than assume that the representation of grammatical knowledge is predicated upon either episodic- or rule-based forms of representation, this chapter will assess the relative contributions of the two forms of sequential dependence associated with these two modes of representation. The following section discusses how the mapping of these two forms of dependence might differ.

5.1.2 How might knowledge of two forms of sequential dependence be dissociated?

If different processes do underlie the mapping and subsequent classification of sequences that contain dependencies between either repeating or non-repeating vocabulary elements then the two should in principle be amenable to dissociation. A dissociation between these types of dependency would be best observed in a novel vocabulary, where the two forms of dependency are suited to different mapping processes. For example, if participants encode or remember the sequential dependence between two-repeating elements (e.g. A - - A - -) it is a relatively trivial task to map that pattern onto patterns seen in a new vocabulary (e.g. X - - X - -) and it can be accomplished on an *sequence-by-sequence* basis. The properties and positions of dependencies between repeating elements are readily apparent in each sequence even in a novel vocabulary. In this case participants can judge the well- or ill-formedness of a test sequence by a comparison with one or more stored exemplars that contain dependencies between repeating elements. If a proportion of those exemplars contain an alternative structure where A might not always predict another A (e.g. some contain A - - A - -, but others contain A - - B - -), a match between the test sequence (e.g. X - - X - -) can still be made. The existence of the alternative exemplar structure should not have a detrimental effect on the ability of participants to recall the appropriate stored exemplar (although there could conceivably be some interference between the two, or some limited memory resource that adversely affects the ability to recall the appropriate exemplar). In contrast the task of mapping a sequential dependency between non-repeating elements is a substantially more complex process because it can only be accomplished by inducing information *across* sequences. If a proportion of exemplar sequences seen during the learning phase contain sequential dependencies between non-repeating elements that cannot be used to judge the well- or ill-formedness of test sequences, this might impair participants ability to map that knowledge onto dependencies in novel vocabulary. For example, if participants can recall that each exemplar began with one of two bigrams *AB* or *CD* it would be possible to induce information (e.g. frequency of occurrence) across the test sets that would allow the mapping of those bigrams onto bigrams in another vocabulary such as *XY* and *ZW*. However, if the dependencies within the exemplars are not absolute, *A* might not always predict *B*, then the problem of computing the mapping between *AB* and *XY* is problematic. The bigram *AB* could also be mapped onto an illegal bigram such as *XZ* because the element that *Z* in the novel vocabulary corresponds to in the source vocabulary might be indeterminate. This problem should not arise when participants are asked to classify test sequences in the same vocabulary as the exemplars where those bigrams can be recognised, but only when participants are required to map that knowledge onto dependencies in another. Thus the introduction of irrelevant exemplars (those that do not contain target dependencies) should provide a functional dissociation between the mapping and subsequent classification of dependencies between repeating elements and those between non-repeating elements.

5.1.3 Overview of experiments

The primary aim of this chapter is to determine the relationship between the encoding and subsequent classification of sequential dependencies of repeating and of non-repeating elements, in a vocabulary that differs from the exemplar vocabulary. Rule-based classification has been associated with sequential dependencies between non-repeating elements, and can be transferred to a novel vocabulary by mapping those dependencies *across* sequences. Similarity-based classification has been associated with sequential dependencies between repeating elements, and can be transferred to a novel vocabulary by mapping those dependencies on a *sequence-by-sequence* basis.

In principle, these two forms of sequential dependence might be differentially affected by varying the strength of those dependencies in exemplar sequences. If true, this would explain the varying sensitivity to different grammatical features in novel vocabularies.

Experiment 14 extends the finding that participants can transfer knowledge of sequential dependencies between non-repeating elements across vocabularies, in sequences that contain no repetition structures. Chapter 4 found that the transfer of this kind of dependency had a rule-like *all-or-none* characteristic, but this was predicated upon a very simple partial-grammar where the target dependencies occurred in the same positions of each sequence. In Experiment 14 a slightly more complex partial-grammar was used where these dependencies occur in three different locations. The bigram *CD* occurs in both positions 1 and 2, and positions 3 and 4 (i.e. *CD* - - - -, - - *CD* - -), while the bigram *AB* occurs in both positions 3 and 4, and positions 5 and 6 (i.e. - - *AB* - - , - - - - *AB*). The two experimental conditions differ in the strength of the target dependencies in the exemplar sequences. If the strength of the dependency (i.e. how frequently the component elements co-occur) is critical to the transfer of knowledge, participants who observe exemplars that contain irrelevant dependencies (i.e. *A* does not always predict *B*) should not be able to transfer knowledge to the novel vocabulary. Participants should, however, be able to apply the relevant information in the source vocabulary. Experiment 15 introduces sequential dependencies between repeating elements into the same training and test sequences that participants learned in Experiment 14. The elements *A* and *D* form repetition structures (e.g. *CD - D-* - - , - - *CD - D*, *A - AB* - - , - - *A - AB*). As in Chapter 3, participants cannot reject ungrammatical sequences on the basis of unfamiliar repetition structure, but only on the basis of ungrammatical second-order dependencies between non-repeating elements. As in Experiment 14, one group of participants learned 'noisy' exemplars that contained neither dependencies between repeating or non-repeating elements, but these were also unlike the ungrammatical sequences. If there is a distinction between the way dependencies of and non-repeating elements are mapped onto new vocabulary elements, participants in the experiment should not be able to correctly classify sequences in a novel vocabulary after studying a proportion of irrelevant exemplars. If however, they cannot, in this case, apply the relevant knowledge in the source vocabulary then clearly repetition has an effect on the *application* of dependencies between non-repeating elements. To determine whether the presence of repetition structures has an effect on the *encoding* of dependencies between non-repeating elements, Experiment 16 trained participants on sequences that contained dependencies between both repeating and non-repeating elements, but tested participants on sequences that contain only dependencies between non-repeating elements. Finally, Experiment 17 uses the exemplar and grammatical test sequences that were used in Experiment 15, but includes a different set of ungrammatical sequences that violate the sequential dependencies between repeating, and not the non-repeating elements. In contrast to Experiment 14, if there is a distinction between the way dependencies between and non-repeating elements are mapped onto new vocabulary elements, participants in the experiment should be able to correctly classify sequences after studying a proportion of irrelevant exemplars. Confidence ratings were taken throughout to assess participants' awareness of the knowledge that was used to discriminate between grammatical and ungrammatical sequences.

5.2 EXPERIMENT 14 NOISE INHIBITS THE TRANSFER BUT NOT THE ACQUISITION OF SEQUENTIAL DEPENDENCIES BETWEEN NON-REPEATING ELEMENTS

5.2.1 Introduction

The principle aim of Experiment 14 is to determine the effect that a proportion of irrelevant exemplars has on participants' ability to transfer knowledge of dependencies between non-repeating elements to a novel vocabulary. Earlier it was suggested that irrelevant or 'noisy' exemplars might impair the process that maps dependencies between vocabularies by computing information across sequences. For example, if *B* does not always follow *A* in a small proportion of exemplars participants could erroneously map more than one vocabulary element onto *B*. That is, participants might fail to determine that one element *always* corresponds to one other, alternatively they might erroneously induce that one elements can map onto more than one other element. In both cases participants should not be able to classify sequences in the novel vocabulary. This manipulation should not affect classification in the same vocabulary as learning because in that case grammatical sequences can be endorsed if participants *recognise* the target bigram in each sequence. This is one reason why bigram knowledge has been associated with episodic knowledge. But in a novel vocabulary that knowledge can only be applied if participants encode the contingent relations of the component elements of a bigram. Ungrammatical sequences should not be endorsed because the irrelevant exemplars were constructed to be dissimilar to them.

A new partial grammar was constructed to constrain the same four vocabulary elements that were also constrained in the partial grammar used in Chapter 4. This grammar is similar to the one used in Chapter 4 in that it does not generate sequences that contain dependencies between repeating elements. However, each of the two dependencies between the contingent elements *A* to *D* can occur in two locations and are uni-directional (*B* cannot precede *A*, and *D* cannot precede *C*, in Chapter 4, the grammar permitted *AB* and *BA*, as well as *CD* and *DC*). Thus this partial grammar has more in common with both finite-state and natural grammars than the one used in Chapter 4. For example, in natural languages dependencies are not restricted to the same specific location of each sentence and are not usually bi-directional.

5.2.2 Method

Participants

Thirty-six members of the University of York participated in this experiment for either course credit or payment. Care was taken to ensure that no volunteer had taken part in any previous artificial grammar learning study.

Stimuli

A new partial-grammar was constructed. This partial grammar constrained four vocabulary elements (*A-D*), but did not constrain the remaining four vocabulary elements (*E-H*). However unlike the Chapter 4 grammar, two of the sequential dependencies occurred in positions other than positions three and four of each sequence (see Figure 5.1). For example, the bigram *AB* occurs in positions five and six of each sequence as often as it did in positions three and four. Similarly, the bigram *CD* occurred in positions one and two of each sequence as often as in positions three and four. Unlike the Chapter 4 grammar the dependency between the component elements of each bigram was uni-directional (i.e. *B* never preceded *A*, and *D* could not precede *C*). The grammar generated ninety-six unique grammatical sequences. Forty-eight grammatical sequences were assigned to the exemplar set, and the remaining forty-eight were assigned to the test set. These two sets were matched for the first-order dependency distribution of the non-contingent vocabulary elements (*E-H*, e.g. *F* would occur in each location with the same frequency across both sets). Participants in the No-noise condition studied the forty-eight exemplars. These were presented twice in random order and instantiated with the symbols shown in Figure 5.2.

For the Noise condition 75% (72) of the grammatical exemplars used in the No-noise condition were selected and a further set of irrelevant or 'noisy' exemplars were constructed to make 25% (12) of the exemplar set for this condition. These contained two of the four contingent elements, but not those that were contingent upon one another. For example, *A* and *D* might occur in the same sequence. An important feature of these 'noisy' exemplars is that they were unlike the ungrammatical test sequences. Consequently ungrammatical sequences could be neither endorsed nor rejected in either the source or novel vocabulary simply because they contained the same features as the 'noisy' exemplars.

Grammatical	*n*	*Ungrammatical*	*n*
– – – – *A B*	24	– – *C* – *A* –	24
– – *A B* – –	24	*C* – *A* – – –	24
– – *C D* – –	24	– – – *D* – *B*	24
C D – – – –	24	– *D* – *B* – –	24

Half the grammatical sequences were used as training exemplars, and the remaining half were presented during test.

Figure 5.1: Stimuli used in Experiment 14

Because each sequence was six elements in length and no element could repeat, there were always at least two elements that were missing.

The forty-eight exemplars that were not selected for the training exemplars were used as grammatical test sequences. Forty-eight ungrammatical sequences were constructed by re-ordering the contingent elements of each grammatical test sequence. However, this was performed in a systematic way that did not allow the ungrammatical sequences to be rejected on the basis that any contingent vocabulary element occurred in a position or frequency that differs from the grammatical sequences. That is, ungrammatical sequences could not be rejected on the basis of first-order dependency information, but only on the basis of second-order dependency information. The ninety-six test sequences were presented twice, once instantiated with symbols (source vocabulary) and once instantiated with syllables (novel vocabulary) according to the mappings shown in Figure 5.2.

Control participants were trained on scrambled versions of the symbol test sequences. This ensured that although Control participants might learn something of the source vocabulary they would not be able to discriminate between grammatical and ungrammatical test sequences on the basis of information learned from the exemplars.

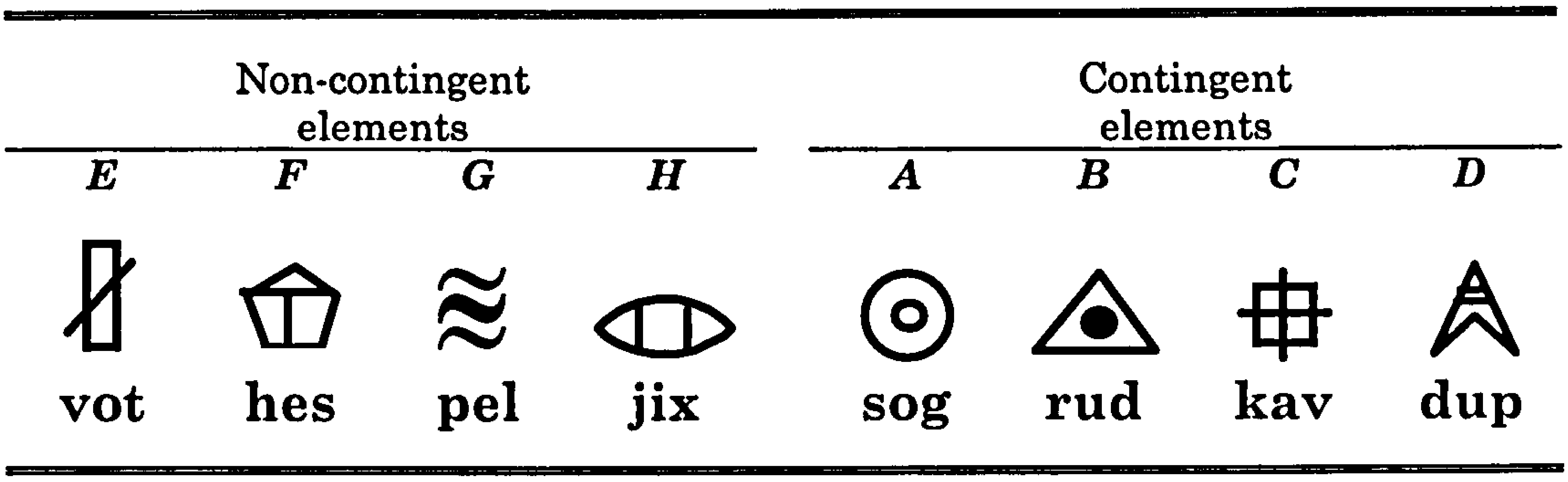


Figure 5.2: The mapping between vocabulary elements used in Chapter 5.

Design

This was a split-plot design with one between-subjects factor Training (Control, Noise or No-noise), and one within-subjects factor Test Vocabulary (Source or Novel). Participants were randomly assigned to one of the three training groups before proceeding to the same test sets. Order of test vocabulary presentation was counterbalanced, half classified syllables (novel vocabulary) first and half classified symbols first (source vocabulary).

Procedure

Initially participants were presented with one of three training booklets. The training procedure consisted of the incidental-orienting task developed in Chapter 4. This was designed to ensure that participants studied each sequence to the same degree. Participants were asked to indicate which symbols out of the possible eight they thought were missing from each sequence (there were always at least two). Prior to the test phase all participants were informed that the sequences that they had just seen obeyed some simple rules of construction. For the test phase participants were required to indicate those sequences that they thought obeyed the same rules as the sequences they had seen during training, and those that did not. In addition, participants were required to indicate how confident they were about each decision on a scale of 50% to 100%, where 50% indicates a complete guess and 100% indicates absolute certainty.

5.2.3 Results

Classification performance

The percentages of correct classification scores and related discrimination indices are shown in Table 5.1. Because there is only one possible mapping between the two vocabularies the false alarm rate can exceed the hit rate. This makes the data amenable to analyses using the discrimination index *A'*.

		Test Vocabulary			
		Source (syllables)		Novel (symbols)	
		mean	*se*	mean	*se*
Training					
Control		46%	3.85	50%	2.51
	A'	.45	.05	.51	.04
	B'	-.39	.10	-.34	.11
	Hits	.58	.05	.61	.04
	False alarms	.65	.06	.60	.06
Noise		75%	6.47	51%	3.22
	A'	.78	.07	.52	.05
	B'	-.09	.14	-.13	.13
	Hits	.79	.06	.55	.03
	False alarms	.28	.07	.52	.06
No-noise		95%	3.66	86%	5.66
	A'	.96	.03	.88	.05
	B'	.57	.16	.44	.15
	Hits	.95	.03	.85	.06
	False alarms	.04	.04	.13	.06

Table 5.1: The percentages of correct classification scores and related discrimination indices for Experiment 14

The *A'* data were entered into a split-plot ANOVA with the within-subjects factor Vocabulary (Source and Novel) and the between-subjects factor Training (Control, Noise or No-noise). There was an effect of Training ($F(2, 33) = 28.31$, $p < .01$, $MS_e = .04$, $\eta^2 = .63$), and effect of Vocabulary ($F(1, 33) = 7.47$, $p < .01$, $MS_e = .02$, $\eta^2 = .19$), and an interaction between the two ($F(2, 33) = 7.26$, $p < .01$, $MS_e = .02$, $\eta^2 = .31$). There were no effects of order (all *F's* < 1.0).

Simple main effects revealed no effect of Vocabulary for either the No-noise group ($F(1, 33) = 1.95$, $p = .17$, $MS_e = .02$, $\eta^2 = .06$), or for the Control group ($F(1, 33) = 1.04$, $p = .32$, $MS_e = .02$, $\eta^2 = .03$). However, there was a substantial effect of Vocabulary for the Noise group ($F(1, 33) = 19.01$, $p < .01$, $MS_e = .02$, $\eta^2 = .37$). Participants in the No-noise condition were equally adept at discriminating between grammatical and ungrammatical sequences in both vocabularies. However, participants in the Noise condition were more accurate in the same vocabulary as exemplars than they were in the Novel vocabulary.

Simple main effects revealed that there was an effect of Training within each vocabulary (Source: $F(2, 33) = 22.06$, $p < .01$, $MS_e = .04$, $\eta^2 = .57$, Novel: $F(2, 33) = 20.61$, $p < .01$, $MS_e = .03$, $\eta^2 = .56$). Planned comparisons revealed that in the Source vocabulary, the No-noise condition were significantly more accurate than the Noise condition ($F = 5.89$, $p = .02$), and the Noise condition were significantly more accurate than Controls ($F = 17.16$, $p < .01$). In the Novel vocabulary the pattern was different, the No-noise condition was significantly more accurate than the Noise condition ($F = 30.11$, $p < .01$), but the Noise condition was no more accurate than Controls ($F < 1.0$).

Clearly participants who had studied the exemplar set that contained 25% noise were able to classify between test sequences but were unable to transfer that knowledge to the novel vocabulary. Whereas participants in the No-noise condition were. We can conclude that participants in the No-noise condition formed representations of sequential dependencies that could be mapped onto a novel vocabulary, whilst the representations induced in the Noise condition differed because they could not be transferred.

Metaknowledge

Was the difference between participants' representation of sequential dependencies reflected in their confidence? Table 5.2. shows the mean confidence for correct and incorrect decisions. Participants in the experimental groups (Noise and No-noise) made too few guesses to use the *guessing criterion* to assess implicit knowledge.

Training	Source Vocabulary correct	incorrect	difference	Novel Vocabulary correct	incorrect	difference
Control	58%	58%	0.36	56%	56%	-0.46
	(2.74)	(2.31)	(0.76)	(1.49)	(1.61)	(0.68)
n	12	12	12	12	12	12
***upper*			2.03			1.04
lower			-1.30			-1.95
Noise	74%	68%	6.36	61%	61%	0.37
	(4.12)	(4.60)	(3.96)	(3.35)	(3.22)	(0.41)
n	12	11	11	12	12	12
upper			15.18			1.28
lower			-2.46			-0.54
No-noise	94%	90%	1.82	78%	76%	1.54
	(2.58)	(5.57)	(4.41)	(5.51)	(5.37)	(1.97)
n	12	8	8	12	9	9
upper			12.24			6.07
lower			-8.61			-2.99

*Standard errors appear in parenthesis.
**Upper and lower bounds of 95% confidence intervals.

Table 5.2: Mean confidence for correct and incorrect decisions by group and vocabulary for Experiment 14.

Inspection of the difference between confidence for correct and incorrect decisions reveals only small numeric differences between groups but large differences in the upper limits of the 95% confidence intervals of participants between the Noise and No-noise conditions and the Control conditions. These indicate that participants may have possessed a good deal of explicit knowledge of the target dependencies, although perhaps more so in the source than in the novel vocabulary. A similar (but significant) finding was reported by Dienes and Altmann (1997) who found that classification in a novel vocabulary was more 'implicit' on this measure than in the source vocabulary. Since there were too few incorrect responses in some cells to take an accurate difference (*zero-correlation criterion*) score, an analysis of participants' mean confidence for correct decisions was performed. A split-plot ANOVA with one between-subjects factor Training (Control, Noise or No-

noise) and one within-subjects factor Vocabulary (Source and Novel) revealed a significant effect of Training ($F(2, 33) = 24.61$, $p < .01$, $MS_e = 209.30$, $\eta^2 = .60$), and effect of Vocabulary ($F(1, 33) = 21.26$, $p < .01$, $MS_e = 90.24$, $\eta^2 = .39$), and a marginal interaction between the two ($F(2, 33) = 3.01$, $p = .06$, $MS_e = 90.24$, $\eta^2 = .15$).

Simple main effects of Vocabulary revealed that participants in the No-noise condition were more confident when correct in the Source vocabulary than in the Novel vocabulary ($F(1, 33) = 15.44$, $p < .01$, $MS_e = 90.24$, $\eta^2 = .32$). This pattern was also present in the No-noise condition ($F(1, 33) = 11.38$, $p < .01$, $MS_e = 90.24$, $\eta^2 = .26$), but not in the Control condition ($F < 1.0$).

Simple main effects of Training revealed a significant trend within both the Source ($F(2, 33) = 30.00$, $p < .01$, $MS_e = 124.58$, $\eta^2 = .98$) and the Novel vocabulary ($F(2, 33) = 9.63$, $p < .01$, $MS_e = 174.97$, $\eta^2 = .96$). In the source vocabulary linear contrasts confirmed that participants in the No-noise condition were significantly more confident when correct than participants in the Noise condition ($F = 18.34$, $p < .01$) who were themselves significantly more confident than Controls ($F = 11.89$, $p < .01$). This pattern was different in the Novel Vocabulary: participants in the No-noise condition were significantly more confident when correct than the Noise case ($F = 10.34$, $p < .01$), however the Noise condition was no different than Controls ($F < 1.0$).

The patterns of discrimination performance are reflected in participants' confidence for correct decisions. These analyses indicate that, although there may have been an implicit component (the lower bound of the confidence intervals was below zero) the knowledge that participants used to discriminate between grammatical and ungrammatical sequences was largely explicit. The data do not however rule out an implicit component. However, the experiments in Chapter 4 consistently found no evidence that participants were able to classify sequential dependencies between non-repeating elements similar to these in the novel vocabulary, unless that knowledge was subjectively explicit and available for direct probing.

5.2.4 Discussion

Experiment 14 confirmed that participants are able to learn sequential dependencies in a language that does not contain repetition structures. Unlike the Experiments reported in Chapter 4, these dependencies were uni-directional, and occurred in different locations. Importantly, these results indicate that the process by which this form of dependency is mapped between vocabularies requires those dependencies to be absolute – a small proportion of irrelevant exemplars disrupted this process. This result provides an explanation of why Gomez *et al.* (in press) found no evidence of transfer in a finite-state language that did not contain repetition structures. Their grammar contained substantially more vocabulary elements than is common in artificial grammar learning research. The consequence is that an element in one position might predict several elements in the next. If those dependencies are not equifrequent, then lower frequency dependencies might have the same effect that noise did in this experiment and inhibit participants' ability to induce the correspondences between vocabularies. Although there may have been an implicit component to the classification of these sequences (in both vocabularies) the knowledge was largely explicit. However, the sequences used in artificial grammar learning typically include dependencies between *both* non-repeating and repeating elements. To what extent are participants able to learn and transfer knowledge of dependencies between non-repeating elements when those sequences also contain dependencies between repeating elements? Chapter 3 found no evidence that participants could transfer knowledge of dependencies between non-repeating elements in the presence of dependencies between repeating elements. As mentioned earlier, the episodic account of transfer has suggested that when sequences contain dependencies between repeating elements, these form the basis for classification in a novel vocabulary (e.g. Brooks & Vokey, 1991; Whittlesea & Dorken, 1993; Whittlesea & Wright, 1997). The following Experiment asks whether participants can transfer second-order dependency information in a language that also contains repetition structures.

5.3 EXPERIMENT 15
PARTICIPANTS DO NOT APPLY DEPENDENCIES BETWEEN NON-REPEATING ELEMENTS WHEN THE LANGUAGE ALSO CONTAINS REPEATING ELEMENTS.

5.3.1 Introduction

The simple partial grammars used in Experiment 14 and Chapter 4 have revealed that participants are able to learn the abstract contingent relationships between non-repeating vocabulary elements and to transfer that knowledge to a novel vocabulary. Experiment 14 demonstrated that 'noisy' exemplars that do not contain the target dependencies disrupt this transfer process. Those partial grammars were specifically designed not to generate repetition structures. Chapter 3 found no evidence that participants are able to classify sequences according to the dependencies between non-repeating elements when the language also contained repetition structures, although participants were sensitive to first-order dependency differences. However those experiments used a rather more complex finite-state grammar. Experiment 15 questions whether participants are sensitive to dependencies between non-repeating elements in a modified version of the partial grammar that also generates dependencies repeating elements. As in Chapter 3 those repetition structures cannot be used to classify test sequences. As in Experiment 14 half the experimental participants studied exemplars of which a small proportion were irrelevant.

5.3.2 Method

Participants

Thirty members of the University of York participated in this experiment for either course credit or payment. Care was taken to ensure that no volunteer had taken part in any previous artificial grammar learning study.

Stimuli

The stimuli used in this experiment were identical to those used in the previous experiment with the exception that an additional contingent vocabulary element (*A* or *B*) was added to each sequence. This created sequential dependencies between repeating elements in addition to the existing dependencies between the non-repeating elements. For example, the occurrence of *A* in a sequence now predicts both another *A*, and the occurrence of the element *B* (e.g. - - *A* - *AB, A* - *AB*). The same principle applies to the elements *D* and *C* (e.g. *CD* - *D* - - , - - *CD* - *D*). Each dependency was equifrequent in each location. Each sequential dependency was embedded into 24 unique sequences by inserting the remaining (non-contingent) vocabulary elements (*E-H*) into the remaining positions. In 12.5% of all sequences one contingent element (*A* through *D*) would occur in a sequence and not be predictive of its associated structure. For example, a sequence such as *C D* – *D* – *A* would be possible. This ensured that at test participants could not reject any sequence on the basis that an element occurred in a novel location. Forty-eight of these grammatical sequences were selected for the exemplars used in the No-noise condition.

A set of forty-eight exemplars was constructed for the *Noise* condition. This set contained 75% (n = 36) of the exemplars used in the No-noise condition (they were representative), and 25% (n = 12) sequences that contained no sequential dependencies at all. Each one of these sequences contained two contingent elements in random positions, but not elements that were contingent upon each other. For example, *A* and *B* are contingent upon each other, in one of the 25% of sequences that contained no dependencies the elements *C* and *A* might co-occur, but they would not do so in a position where they occurred in either the grammatical or ungrammatical sequences. Thus no ungrammatical sequence could be endorsed because it contained similar structures to one of these 'noisy' exemplars. Both sets of exemplars were presented twice in random order and instantiated with the symbols shown in Figure 5.2.

There was a single set of forty-eight grammatical and forty-eight ungrammatical test sequences. However, the ungrammatical sequences retained the illegal bigrams that participants were able to correctly classify in the previous experiment, and did not violate the repetition structures (see Figure 5.3). The grammatical sequences were the forty-eight sequences that were not selected as exemplars. The ungrammatical sequences were identical to the ones used in the Experiment 14 with the exception that they now contained an additional element that formed the same (i.e. grammatical) repetition structures as the grammatical test sequences. Consequently the grammatical and ungrammatical test sequences could only be rejected on the basis of the sequential dependencies between non-repeating elements. Test sequences were presented twice, once instantiated with symbols (source vocabulary) and once instantiated with syllables (novel vocabulary).

Control participants were trained on scrambled versions of the grammatical and ungrammatical symbol sequences. This ensured that although they might learn something of the vocabulary elements, and their frequency of occurrence, they could not learn anything of the contingent nature of those elements in the grammatical exemplars.

Grammatical exemplars & test	*n*	*Ungrammatical (illegal bigram)*	*n*
– – A – A B	24	*B – A – A –*	12
A – A B – –	24	*A – A – – B*	12
– – C D – D	24	*C – – D – D*	12
C D – D – –	24	*– D – D – C*	12

Half the grammatical sequences were assigned to the training set and the remaining half were assigned to the test set.

<u>Figure 5.3: Stimuli used in Experiment 15</u>

Procedure

Initially participants were presented with one of three training booklets. The training procedure consisted of an incidental-orienting task, designed to ensure that participants studied each sequence to the same extent. Participants were asked to indicate which symbols out of the possible eight they thought were missing from each sequence (there were always at least three in each condition). Prior to the test phase all participants were informed that the sequences that they had just seen obeyed some simple rules of construction. For the test phase participants were required to indicate those sequences that they thought obeyed the same rules as the sequences they had seen during training, and those that did not. In addition, participants were required to indicate how confident they were about each decision on a scale of 50% to 100%, where 50% indicates a complete guess and 100% indicates absolute certainty.

5.3.3 Results

Classification performance

The percentages of correct classification scores and related discrimination indices are shown in Table 5.3. As before two discrimination indices (*A'*) were calculated for each participant, one for each vocabulary.

		Test Vocabulary			
		Source (syllables)		Novel (symbols)	
		mean	*se*	mean	*se*
Training					
Control		**53%**	1.60	**54%**	1.53
	A'	.56	.03	.58	.03
	B'	-.33	.07	-.34	.09
	Hits	.62	.03	.64	.34
	False alarms	.55	.03	.55	.02
Noise		**49%**	1.39	**49%**	1.77
	A'	.49	.03	.48	.03
	B'	.01	.08	-.10	.05
	Hits	.49	.03	.52	.02
	False alarms	.51	.02	.54	.02
No-noise		**50%**	1.22	**50%**	1.72
	A'	.50	.02	.49	.03
	B'	-.09	.08	-.16	.06
	Hits	.53	.03	.54	.02
	False alarms	.52	.02	.55	.03

Table 5.3: The percentages of correct classification scores and related discrimination indices for Experiment 15.

The *A'* data were entered into a split-plot ANOVA the within-subjects factor was Vocabulary (Source and Novel) and the between-subjects factor was Training (No-noise, Noise, or Control). There was an effect of Training ($F(2, 27) = 5.58$, $p < .01$, $MS_e = .01$, $\eta^2 = .29$), but no effect of Vocabulary ($F(< 1.0$), and no interaction between the two $F(< 1.0)$. There were no effects of order (all *F's* < 1.0).

Simple main effects revealed no effect of Training within the Source vocabulary ($F(2, 27) = 2.08$, $p = .15$, $MS_e = .01$, $\eta^2 = .13$), there was however a marginal effect of Training within the Novel Vocabulary ($F(2, 27) = 3.23$, $p = .06$, $MS_e = .01$, $\eta^2 = .19$). In the Source vocabulary planned comparisons revealed that participants in the No-noise condition were no more accurate than participants in the Noise condition ($F = 0.18$, $p = 65$), but that Control participants were marginally more accurate than participants in the Noise condition ($F = 3.80$, $p = .06$). Similarly in the Novel vocabulary participants in the Noise condition were as accurate as those in the No-noise condition ($F = 0.02$, $p = .90$) but Controls were more accurate than participants in the Noise condition ($F = 5.13$, $p = .03$).

This experiment used a slightly smaller sample size ($n = 30$) than the last ($n = 36$). To determine whether the null effect of Vocabulary and interaction between Vocabulary and Training in this experiment could be attributed to the smaller sample size than the previous one, post-hoc power analyses were computed for the omnibus ANOVA using the effect sizes observed in Experiment 14[1]. For $n = 30$ the power of detecting the same effect size that was observed in Experiment 14 ($n = 36$) for Vocabulary ($f^2 = .24$) was $1- \beta = .91$, and for the interaction between Vocabulary and Training ($f^2 = .45$) $1-\beta = .97$. Clearly these null effects are real and cannot be attributed to the change in sample size. Since there was a significant effect of Training (albeit in an unexpected direction) this analysis was not strictly necessary.

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[1] This analysis is different to the one performed in Chapter 3. In this case the difference between groups is less important than the omnibus interaction between

Metaknowledge

Since experimental participants were unable to discriminate between test sequences in either vocabulary confidence for correct and incorrect decisions are not reported.

5.3.4 Discussion

In this experiment prior exposure to exemplars, whether noisy or not, did not enable participants to discriminate between grammatical and ungrammatical test sequences, in either the novel or even the source vocabulary. In fact participants who had seen exemplar sequences performed significantly worse than control participants who saw scrambled test sequences during training. Two questions are in need of some discussion. Why were the two groups of Experimental participants unable to discriminate between test sequences, and why were controls significantly better than both groups of trained participants (see above) and chance: Source $t(9) = 19.38$, $se = .03$, $p < .01$, and Novel $t(9) = 21.37$, $se = .03$, $p < .01$?

Control groups often perform significantly above chance whether due to learning during the test phase or some inherent bias in the stimuli. Bias is an unlikely explanation since these test sequences could not be discriminated on the basis of simple frequency-by-location information. Perhaps Control participants were able to learn something of the two types of sequence that allowed them to be classified. This was, after all, part of Perruchet's (1994) criticism of previous transfer studies. In some respects this is an irrelevant issue, but what is important is that exposure to exemplars constrained the experimental participants in that they were unable to learn whatever the control participants had learned during the test phase.

Why were participants who were trained on exemplar sequences unable to discriminate between grammatical and ungrammatical sequences in either the source or the novel vocabulary? The sequences differed from those in Experiment 14, only in that they contained an additional (contingent) vocabulary element that formed a sequential dependency with

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vocabulary and training. The effect size f^2 is given by $\eta^2/1- \eta^2$

another identical element. Perhaps the mere presence of repetition structures inhibits participants' sensitivity to the sequential dependencies between non-repeating elements. In Chapter 3 there was strong evidence that participants were sensitive to first-order dependencies, but no evidence that participants were at all sensitive to second-order dependencies in the novel vocabulary. Those sequences contained repetition structures. A common theme throughout this thesis has maintained that the classification of sequences according to sequential dependencies between repeating elements involves simpler cognitive processes than the classification of sequential dependencies between non-repeating elements. First, the mapping of dependencies between repeating elements from one vocabulary to another can be performed on a sequence-by-sequence basis. Subsequent classification of those sequences might be most economically[2] explained by a comparison to the repetition structures encoded within stored exemplars – that is by *abstract analogy*. In contrast the mapping of sequential dependencies between non-repeating elements must proceed by induction *across* sequences. In this case the subsequent classification of sequences, seems inextricably bound with that mapping process. For example, most abstractionist and fragmentary models assume that the correct classification of the first few sequences is at chance as that mapping is being induced (Dienes, Altmann & Gao, 1995a; Redington & Chater, 1996). Perhaps then the system favours the most economic option when faced with a language that contains patterns of repeating elements. This may also explain why the knowledge used to classify the sequential dependencies in Chapter 4 seemed explicit. But this interpretation fails to explain why participants were unable to classify sequences in the source vocabulary where no mapping process is necessary. Perhaps participants failed to even encode the dependencies between non-repeating elements. To determine whether this was the case Experiment 16 uses the same exemplar sequences used in Experiment 15 that contain dependencies between both repeating and non-repeating elements. However, the test sequences are identical to those that were used in Experiment 14

[2] By economically I mean in terms of both parsimony and perhaps also cognitive economy.

(where participants were able to make the discrimination) that contained only dependencies between non-repeating elements.

5.4 EXPERIMENT 16
PARTICIPANTS DO NOT ENCODE DEPENDENCIES BETWEEN NON-REPEATING ELEMENTS WHEN THE LANGUAGE ALSO CONTAINS REPEATING ELEMENTS.

5.4.1 Introduction

Experiment 14, and those reported in Chapter 4, demonstrated that participants are able to encode and transfer knowledge of the abstract contingent relationships between vocabulary elements in simple languages that do not contain dependencies between identical elements. However, in Experiment 15 participants exhibited no apparent sensitivity to dependencies between non-identical elements when each sequence in the language also contained dependencies between identical elements. This raises the question, did participants fail to *encode* the dependencies between non-identical elements from the exemplars, or did they fail to *apply* these dependencies at test? Experiment 16 resolves this issue. Experiment 14 demonstrated that dependencies between identical elements can only be transferred to a novel vocabulary by inducing across both training and test sets the correspondences between vocabulary elements. In contrast, the correspondences between dependencies of identical elements can, in principle, be induced on a sequence-by-sequence basis. Clearly this latter process is relatively less complex. Thus we might assume that participants did learn the dependencies between non-identical elements, but failed to apply them because dependencies between identical elements could proceed on a sequence-by-sequence basis, even though it did not lead to the test sequences being correctly classified. Alternatively, participants when asked to study the exemplar sequences might encode the relatively more salient repetition structures within exemplars to the detriment of the information pertaining to elements that did not repeat. In Experiment 16, participants were asked to study the same exemplars used in Experiment 15 that contained dependencies between both identical and non-identical elements. However, the test sequences were the same ones that were used in Experiment 14 that contained only dependencies between non-identical elements. If participants are able to classify these sequences correctly then clearly they must have encoded the dependencies between non-identical elements as well as those between identical elements from the exemplars. However, if participants are unable to classify these sequences correctly, then they could not have learned about the dependencies between non-identical elements.

5.4.2 Method

Participants

Thirty members of the University of York participated in this experiment for either course credit or payment. Care was taken to ensure that no volunteer had taken part in any previous artificial grammar learning study.

Stimuli

The exemplar sequences were identical to those used in Experiment 15, including the set that contained irrelevant exemplars. These contained dependencies between both repeating and non-repeating elements. The test sequences were identical to the ones used in Experiment 14, these contained only legal and illegal dependencies between non-repeating elements – no test sequence contained any repetition structure and could only classified on the basis of the dependencies between non-repeating elements. See Figure 5.4.

Grammatical exemplars	*n*
– – *A* – *A B*	24
A – *A B* – –	24
– – *C D* – *D*	24
C D – *D* – –	24

Grammatical test (legal bigram)	*n*
– – – – *A B*	24
– – *A B* – –	24
– – *C D* – –	24
C D – – – –	24

Ungrammatical (illegal bigram)	*n*
– – *C* – *A* –	24
C – *A* – – –	24
– – – *D* – *B*	24
– *D* – *B* – –	24

Figure 5.4: Stimuli used in Experiment 16.

The forty-eight exemplars were presented twice and instantiated with the symbols shown in Figure 5.2. The same scrambled and noisy exemplars used in Experiment 15 were also used. Test sequences were presented twice, once instantiated with symbols (source vocabulary) and once instantiated with syllables (novel vocabulary).

Procedure

The procedure was identical to the one used in the previous two experiments. Initially participants were presented with one of three training booklets. The training procedure consisted of the same incidental-orienting task previously. Participants were asked to indicate which symbols out of the possible eight they thought were missing from each sequence (there were always at least three in each condition). Prior to the test phase all participants were informed that the sequences that they had just seen obeyed some simple rules of construction. For the test phase participants were required to indicate those sequences that they thought obeyed the same rules as the sequences they had seen during training, and those that did not. In addition, participants were required to indicate how confident they were about each decision on a scale of 50% to 100%, where 50% indicates a complete guess and 100% indicates absolute certainty. Participants were given no information regarding the dependencies between identical elements that appeared in the exemplars but not in the test sequences.

5.4.3 Results

Classification performance

The percentages of correct classification scores and related discrimination indices are shown in Table 5.4. As before two discrimination indices (*A'*) were calculated for each participant, one for each vocabulary.

		Test Vocabulary			
		Source (syllables)		Novel (symbols)	
		mean	*se*	mean	*se*
Training					
Control		50%	1.96	51%	2.78
	A'	.50	.03	.52	.05
	B'	-.08	.09	-.02	.08
	Hits	.52	.03	.52	.03
	False alarms	.52	.03	.50	.04
Noise		54%	1.84	53%	1.62
	A'	.56	.03	.56	.03
	B'	-.07	.05	-.11	.04
	Hits	.55	.02	.56	.02
	False alarms	.48	.02	.49	.02
No-noise		55%	3.95	49%	0.67
	A'	.57	.05	.48	.01
	B'	-.01	.08	.01	.07
	Hits	.57	.05	.49	.02
	False alarms	.47	.04	.51	.02

Table 5.4: The percentages of correct classification scores and related discrimination indices for Experiment 16.

The *A'* data were entered into a split-plot ANOVA the within-subjects factor was Vocabulary (Source and Novel) and the between-subjects factor was Training (No-noise, Noise, or Control). There was no effect of Training

($F(2, 27) = 1.65$, $MS_e = .01$, $p = .21$, $\eta^2 = .11$), no effect of Vocabulary ($F < 1.0$), and no interaction between the two ($F < 1.0$).). There were no effects of order (all F's < 1.0).

Simple main effects revealed no effect of Training within the Source vocabulary ($F < 1.0$), or within the Novel Vocabulary ($F(2, 27) = 1.48$, $MS_e = .01$, $p = .25$, $\eta^2 = .10$). In the Source vocabulary planned comparisons revealed that participants in the No-noise condition were no more accurate than participants in the Noise condition ($F < 1.0$), nor were participants in the control condition any less accurate than participants in the Noise condition ($F = 1.05$, $p = .28$). Similarly in the Novel vocabulary participants in the Noise condition were as accurate as those in the No-noise condition ($F = 1.31$, $p = .10$) and Controls were no less accurate than participants in the Noise condition ($F < 1.0$).

Do we have the statistical power to accept these null results at face value? The analysis to determine statistical power is identical to the one that was performed in Experiment 15. The power of detecting an effect the size of the one observed in Experiment 14 for Vocabulary ($f^2 = .24$) was $1\text{-}\beta = .91$, for Training ($f^2 = 1.72$) was .99, and for the interaction the two ($f^2 = .45$) $1\text{-}\beta = .97$. Clearly these null effects are reliable and, as in Experiment 15, cannot be attributed to the change in sample size.

Metaknowledge

Since experimental participants were unable to discriminate between test sequences in either vocabulary confidence for correct and incorrect decisions are not reported.

5.4.4 Discussion

As in Experiment 15 prior exposure to exemplars that contained dependencies between both repeating and non-repeating elements, whether noisy or not, did not enable participants to discriminate between grammatical and ungrammatical test sequences, in either the novel or even the source vocabulary. Unlike Experiment 15, in this experiment the test sequences contained only legal and illegal dependencies between non-identical elements. It seems then that participants did not encode these dependencies from the exemplars, in either this, or the previous experiment.

The mere presence of repetition structures in exemplars inhibits participants' ability to learn the sequential dependencies between non-repeating elements under implicit learning conditions. Of course, if participants had been given explicit instructions to learn information about elements that did not repeat, they probably would have been able to classify the test sequences. But even the incidental orienting task that ensured that participants attended to each vocabulary element did not ensure that participants encoded the dependencies between the relevant vocabulary elements. Chapter 3 found strong evidence that participants were sensitive to first-order dependencies, but no evidence that participants were at all sensitive to second-order dependencies. Those sequences contained repetition structures. In this experiment second-order dependencies between non-repeating elements were the only basis upon which test sequences could be classified. The classification of sequences according to sequential dependencies between repeating elements involves simpler cognitive processes than the classification of sequential dependencies between non-repeating elements. For example, the mapping of dependencies between repeating elements from one vocabulary to another can be performed on a sequence-by-sequence basis. Subsequent classification of those sequences might be best explained by a comparison to the repetition structures encoded within stored exemplars – that is by *abstract analogy*. In contrast the mapping of sequential dependencies between non-repeating elements must proceed by induction *across* sequences. Perhaps then the system favours the most economic option when faced with a language that contains patterns of repeating elements. This may also explain why the knowledge used to classify the sequential dependencies in Chapter 4 seemed explicit. But as Experiment 16 demonstrates this economy extends beyond both the source and novel vocabularies to the exemplars themselves. This latter effect is not predicted by the exemplar based accounts, because the relevant information would have been available in an episodic trace. Experiment 17 determines whether or not participants are more sensitive to the more economic option of encoding only the dependencies between repeating elements. In addition, Experiment 17 questions whether the processes used to classify the two forms of sequential dependence can indeed be dissociated (by being differentially sensitive to noise) and finally whether this similarity based process can be described as implicit.

5.5 EXPERIMENT 17 THE ENCODING AND SUBSEQUENT CLASSIFICATION OF REPETITION STRUCTURES IS UNAFFECTED BY NOISE

5.5.1 Introduction

Experiment 17 uses the same grammatical training (including irrelevant exemplars) and test sequences that were used in Experiment 15. These sequences contained dependencies between both repeating and non-repeating elements. However in this experiment a new set of ungrammatical sequences were constructed that violated only the sequential dependencies between repeating elements. The prediction is that irrelevant exemplars will not impair the classification of test sequences in the novel vocabulary because each test sequences can be compared to the repetition structures of stored exemplars. The mapping process should not be impaired because patterns of repeating elements can be mapped, even in a novel vocabulary, on a sequence-by-sequence basis if participants can recall at least one relevant exemplar.

5.5.2 Method

Participants

Thirty members of the University of York participated in this experiment for either course credit or payment. Care was taken to ensure that no volunteer had taken part in any previous artificial grammar learning study.

Stimuli

The grammatical training (including the irrelevant exemplars) and test sequences were identical to those used in Experiment 15. However, in this experiment the ungrammatical sequences were constructed by reordering one of the repeating elements in each of the grammatical sequences. The element that was reordered was the one that was not adjacent to a non-repeating contingent element so that the ungrammatical sequences did not contain any illegal bigrams. As before the ungrammatical sequences could not be rejected because they contained any elements in locations where they did not occur in the exemplar sequences, nor could the ungrammatical sequence be endorsed because they were similar to the irrelevant exemplars.

Grammatical	*n*	*Ungrammatical (illegal repetition)*	*n*
– – *A* – *A* *B*	12	*A* – – – *A* *B*	12
A – *A* *B* – –	12	– – *A* *B* – *A*	12
– – *C* *D* – *D*	12	*D* – *C* *D* – –	12
C *D* – *D* – –	12	*C* *D* – – – *D*	12

Half the grammatical sequences were assigned to the training set and the remaining half to the test set.

Figure 5.5: Stimuli used in Experiment 17.

Design

This was a split-plot design with one between-subjects factor Training (Control, Noise or No-noise), and one within-subjects factor Test Vocabulary (Source or Novel). Participants were randomly assigned to one of the three training groups before proceeding to complete the same test sets. Order of test vocabulary presentation was counterbalanced, half classified syllables (novel vocabulary) first and half classified symbols first (source vocabulary).

Procedure

Initially participants were presented with one of three training booklets. The training procedure consisted of an incidental-orienting task, designed to ensure that participants attended to each sequence. Participants were asked to indicate which symbols out of the possible eight they thought were missing from each sequence (there were always at least three). Prior to the test phase all participants were informed that the sequences that they had just seen obeyed some simple rules of construction. For the test phase participants were required to indicate those sequences that they thought obeyed the same rules as the sequences they had seen during training, and those that did not. In addition, participants were required to indicate how confident they were about each decision on a scale of 50% to 100%, where 50% indicates a complete guess and 100% indicates absolute certainty.

5.5.3 Results

Classification performance

The percentages of correct classification scores and related indices of discrimination are given in Table 5.5. The A' data were entered into a split-plot ANOVA with one between-subjects factor Training (Control, Noise or No-noise) and one within-subjects factor Vocabulary (Source and Novel). There was an effect of Training ($F(2, 27) = 9.11, p < .01, MS_e = .06, \eta^2 = .40$), and an effect of Vocabulary ($F(1, 27) = 4.18, p < .05, MS_e = .01, \eta^2 = .13$), but no interaction between the two ($F < 1.0$).). There were no effects of order (all F's < 1.0).

Simple main effects revealed no effect of Vocabulary for either the No-noise group ($F < 1.0$), or for the Noise group ($F < 1.0$), and only a marginal effect for the Control group ($F(1, 27) = 3.77, MS_e = .01, p = .06, \eta^2 = .12$). Clearly participants within each condition were able to classify sequences equally well within each vocabulary. Simple main effects revealed a substantial effect of Training within the source Vocabulary ($F(2, 27) = 8.53, p < .01, MS_e = .04, \eta^2 = .91$). Moreover planned comparisons revealed that there was no difference between participants in the Noise and No-noise conditions

($F = 2.93$, $p = .10$), and participants in the Noise condition were better than Controls ($F = 5.76$, $p = .02$). This pattern was also reflected in the Novel vocabulary ($F(2, 27) = 6.61$, $p < .01$, $MS_e = .04$, $\eta^2 = .33$), planned comparisons again revealed no difference between participants in the Noise and No-noise conditions ($F = 3.28$, $p = .08$), whilst participants in the Noise condition were marginally better than Controls ($F = 4.31$, $p = .05$).

		Test Vocabulary			
		Source (syllables)		Novel (symbols)	
		mean	*se*	mean	*se*
Training					
Control		46%	2.20	52%	3.51
	A'	.43	.04	.53	.05
	B'	-.21	.11	-.34	.08
	Hits	.53	.06	.60	.05
	False alarms	.62	.05	.56	.03
Noise		61%	5.22	64%	4.64
	A'	.64	.07	.69	.06
	B'	-.31	.15	-.25	.09
	Hits	.69	.07	.69	.05
	False alarms	.46	.06	.42	.05
No-noise		78%	7.54	83%	7.23
	A'	.80	.08	.84	.07
	B'	.13	.13	.18	.09
	Hits	.78	.07	.82	.07
	False alarms	.21	.09	.15	.07

Table 5.5: The percentages of correct classification scores and related discrimination indices for Experiment 17

Metaknowledge

To what extent were participants aware of the knowledge used to discriminate between sequences in each of the two vocabularies? Table 5.6 shows the mean confidence for correct and incorrect decision for each training condition and each vocabulary. As in Experiment 14 these data were not amenable to analysis by either the *zero correlation* or the *guessing criterion*.

Training	Source Vocabulary *correct*	*incorrect*	*difference*	Novel Vocabulary *correct*	*incorrect*	*difference*
Control	59%	58%	0.61	57%	57%	0.23
	(2.04)	(1.72)	(0.73)	(1.77)	(1.70)	(0.58)
n	10	10	9	10	10	10
***upper*			2.30			1.53
lower			-1.08			-1.07
Noise	68%	66%	1.89	65%	62%	3.75
	(4.40)	(3.44)	(1.35)	(4.15)	(2.73)	(2.02)
n	10	10	10	10	10	10
upper			4.94			8.32
lower			-1.16			-0.83
No-noise	80%	66%	7.00	80%	60%	3.40
	(5.36)	(7.33)	(3.30)	(5.71)	(7.21)	(4.29)
n	10	6	6	10	4	4
upper			15.49			17.07
lower			-1.49			-10.27

*Standard errors appear in parenthesis.
**Upper and lower bounds of 95% confidence intervals.

Table 5.6: Mean confidence for correct and incorrect decisions by group and vocabulary for Experiment 17

Inspection of Table 5.6 reveals that in the No-noise condition four participants made no errors in the source vocabulary and six made none in the novel vocabulary. The 95% confidence intervals indicate a substantial range in difference scores. Although every participant made errors in the Noise condition inspection of the 95% confidence intervals of the difference scores reveals a substantial range in confidence. Consequently the mean confidence for correct decisions was analysed. These data were entered into a split-plot ANOVA with the within-subjects factor as Test Vocabulary (Source and Novel) and the between-subjects variable being Training (Control, Noise or No-noise). This revealed an effect of Training ($F(2, 26) = 5.96$, $p < .01$, $MS_e = 353.74$, $\eta^2 = .31$), an effect of Vocabulary ($F(1, 26) = 5.59$, $p < .03$, $MS_e = 5.10$, $\eta^2 = .18$), but no interaction between the two ($F(2, 26) = 0.33$, $p = .72$, $MS_e = 5.10$, $\eta^2 = .03$).

Simple main effects of Vocabulary revealed no difference in the No-noise ($F(1, 26) = 1.32$, $p = .26$, $MS_e = 5.10$, $\eta^2 = .05$) or the Control condition ($F < 1.0$), but did reveal a significant difference in the Noise condition ($F(1, 26) = 5.59$, $p < .05$, $MS_e = 4.21$, $\eta^2 = .14$).

Simple main effects of Training revealed a significant trend within both the source ($F(1, 26) = 5.92$, $p < .01$, $MS_e = 178.08$, $\eta^2 = .97$) and the Novel ($F(1, 26) = 5.84$, $p < .01$, $MS_e = 180.76$, $\eta^2 = .31$) vocabularies. In the Source vocabulary linear contrasts revealed that participants in the No-noise condition were significantly more confident when correct than participants in the Noise condition ($F = 4.31$, $p < .05$) however, participants in the No-noise condition were not significantly more confident than Control participants ($F = 1.92$, $p = .18$). This pattern was also reflected in the Novel vocabulary with the No-noise condition being more confident than the Noise condition ($F = 5.03$, $p = .03$), whilst Noise condition was no more confident than Controls ($F = 1.90$, $p = .18$). Consequently the pattern of discrimination was not reflected in phenomenology. Although, the *zero-correlation criterion* could not be applied the lack of difference between participants in the Noise and Control conditions mean confidence may indicate the application of implicit knowledge by participants who studied noisy exemplars.

5.5.4 Discussion

Experiment 17 has demonstrated that participants are able to learn and transfer knowledge of dependencies between repeating elements to a novel vocabulary even when some of the exemplars were irrelevant. This is in contrast to Experiment 14 where irrelevant exemplars impaired participants' ability to map the dependencies between non-repeating elements. Also, this Experiment contrasts with Experiments 15 and 16, where the inclusion of a more salient form dependency (between repeating elements) inhibited participants ability to learn and transfer the target dependency (between non-repeating elements).

Comparisons with Experiment 14

Is the difference in the classification of the two forms of sequential dependence reflected in participants' self-reported phenomenology? Comparisons with participants' confidence in classifying the illegal bigrams revealed few differences. In the Noise conditions there was no effect of Experiment in either the same ($t(20) = 1.10$, $se = 6.05$, $p = .28$) or the novel vocabulary ($t(20) = 0.83$, $se = 5.27$, $p = .42$). In the No-noise conditions there was a significant effect of Experiment in the source vocabulary ($t(20) = 2.45$, $se = 5.63$, $p = .02$) but no effect in the novel vocabulary ($t(20) = 0.04$, $se = 7.97$, $p = .97$). Participants were more confident when they correctly classified sequences that contained illegal bigrams (94% sure) than those who had classified illegal repetitions (80% sure), but only in the source vocabulary. As in Experiment 14, the confidence ratings suggest that although there may have been an implicit component (the lower bounds of the confidence intervals were below zero) the knowledge used to classify test sequences in both vocabularies was predominantly explicit. Thus the difference in the classification of the two forms of sequential dependence does not appear to be reflected in phenomenology.

Clearly the distinction between the way dependencies between repeating and non-repeating elements is psychologically real and can be functionally dissociated by the presence of irrelevant exemplars. However, the effect of noise did lead to numerically less discrimination in both the source and novel vocabularies, but this did not approach statistical significance. In Experiment 14, noise did not impair participants' ability to classify sequences in the source vocabulary, but did impair participants' ability to transfer that information – noise disrupted the mapping process. In Experiment 17, noise did not effect participants ability to map the target dependencies. The slight drop in discrimination can, probably, be attributed to a small decrease in the ability to recall the target dependencies from stored exemplars that might have been obscured by the recall of noisy exemplars. Of course a larger proportion of noisy exemplars might impair discrimination further, but discrimination would remain equivalent in the two vocabularies. This could not be said for dependencies between non-repeating elements.

These two forms of sequential dependence have been associated with different modes of representation. The Chapter Summary discusses to what extent this attribution is a valid one.

5.5 CHAPTER SUMMARY

The data presented in Chapter 5 provide evidence of a functional dissociation across four experiments between the transfer of knowledge of sequential dependencies between repeating and non-repeating elements. This dissociation was also reflected in participants' confidence in their responses. A small proportion of irrelevant exemplars (that do not contain the target dependencies) impairs the mapping and subsequent classification of dependencies between non-repeating elements (Experiment 14) but does not impair the mapping and classification of dependencies between repeating elements (Experiment 17). Importantly Experiment 15 found that participants were unable to classify sequences according to dependencies between non-repeating elements when those sequences also contained dependencies between repeating elements in either the same or a different vocabulary to the exemplars. Moreover Experiment 16, found that participants do not even encode the dependencies between non-repeating elements if each exemplar also contains dependencies between repeating elements. This is in contrast to what we might have expected if participants stored veridical representations of each exemplar – this finding was true even if all of those exemplars were relevant.

Where lies the dissociation?

The distinction between sequential dependencies of repeating and non-repeating elements can be used to distinguish between different aspect of the system that learns artificial grammars. This chapter has introduced a manipulation, noise, which successfully dissociates these aspects. Consider Experiment 17. When participants study sequences that contain patterns of repeating elements (e.g. *A - - A - -*) that information can be used to determine the *similarity* of new sequence, even in a novel vocabulary, on a sequence-by-sequence basis. For example, the sequence *X - - X - -* is similar to the previous example in that it conforms to the pattern of repeating elements and could be endorsed as grammatical, the pattern *- X - X - -* is dissimilar in that it does not conform to that pattern and could be rejected as ungrammatical. If a proportion of the training exemplars contain an alternative structure where *A* does not always predict another *A* (e.g. some contain *A - - A - -*, but others contain *A - - B - -*), a match between a test sequence (e.g. X - - X - -) and a relevant exemplar can still be made. That is, noisy exemplars do not inhibit the ability of participants to classify test sequences in a novel vocabulary on the basis of dependencies between repeating elements. However, the transfer of information concerning patterns of non-repeating elements is disrupted by noisy exemplars. For example, in Experiment 14, participants were asked to study exemplars that contained only two dependencies between non-repeating elements: *AB* and *CD*. That information could be mapped onto structures, *XY* and *ZW*, in a novel vocabulary that share the same properties such as where and how often they occur (such information can only be induced *across* sequences). However, when *A* did not always predict *B*, participants were able to learn the bigram (as indexed by their ability to classify sequences in the source vocabulary) but were unable to map that information onto structures in the novel vocabulary because in this case, the bigram *AB* could also be mapped onto an illegal bigram such as *XZ* because the element that *Z* in the novel vocabulary corresponds to in the source vocabulary is indeterminate. Thus the introduction of irrelevant exemplars (those that do not contain target dependencies) dissociates two aspects of the grammar learning system, but between what aspect of the grammar learning system?

The classification of sequences according to *abstract analogy* of repetition structures has been associated with memory for whole exemplars (Brooks & Vokey, 1991). In contrast, the transfer of bigram information, whether composed of repeating elements or not, has been associated with both the abstractionist and fragmentary theories of artificial grammar learning (e.g. Manza & Reber, 1997; Redington & Chater, 1996). Of course, artificial languages can be encoded by either an episodic memory system or a statistical abstraction system and, in principle, these data could be taken to support this distinction between these representational systems.

However, when defined in terms of the information available to transfer, these putative representational systems differ solely in the way that they map information across vocabularies. In principle, Experiments 15 and 16 could have supported an exemplar-based account. In those experiments participants were asked to study sequences that contained dependencies between both repeating and non-repeating elements (e.g. *AB* - *B* - -). In Experiment 15 the ungrammatical sequences contained legal dependencies between repeating elements and illegal dependencies of non-repeating elements (e.g. - *B* - *B* - *A*). The ungrammatical sequences used in Experiment 16 contained only illegal dependencies of non-repeating elements (e.g. - *B* - - - *A*). If participants memorised whole exemplars they should have been able to correctly reject these ungrammatical sequences in the source vocabulary. This was not the case—it seems participants preferred to encode the dependencies between repeating elements. However, simply because dependencies between repeating and non-repeating elements are treated differently does not necessarily imply that they are predicated upon discrete modes of representation. Dependencies between repeating elements can also be encoded as rules in the same way that dependencies between non-repeating elements can. It might be more parsimonious, in terms of capacity limitations, to encode dependencies between repeating elements as rules. This issue is discussed further in Chapter 6.

In sum, the dissociation reported in this chapter could reflect a distinction between memory for whole instances and memory/abstraction of *n*gram information if it is assumed that in a novel vocabulary the exemplar information that is retrieved concerns the repetition structure of exemplars. The transfer of such information was unaffected by the presence of irrelevant exemplars. An indistinguishable alternative is that patterns of both repeating and non-repeating elements are encoded as sequential rules. Experiment 16 supports this latter case. Thus the dissociation lies between the mapping of dependencies between repeating and non-repeating elements and not their mode of representation *per se*. The mapping and classification of dependencies between repeating elements can, consistent with broad abstraction, proceed on a sequence-by-sequence basis, whilst the mapping of dependencies between non-repeating elements can, consistent with narrow abstraction, only proceed by inducing information across sequences. However, the issue of this mapping process is orthogonal to the representation of the two forms of sequential dependence and again may only be revealed by computational modelling. Parsimoniously then, the differences between the way that dependencies between repeating and non-repeating elements are treated is best viewed as being between the mapping of dependencies between repeating and non-repeating elements. In the General Discussion the relevance of these findings for computational models of artificial grammar learning and theories of representation are discussed.

CHAPTER SIX

GENERAL DISCUSSION

The information that people learn when they are asked to study exemplar sequences of an artificial language has often been described as abstract (e.g. Reber, 1993; Manza & Reber, 1997). Theories of abstraction in artificial language learning are explicitly contrasted with theories of memory based learning (e.g. Knowlton & Squire, 1996; Shanks & St, John, 1994). The abstractionist theory of artificial language learning shares many characteristics with theories of natural language, in particular that it is predicated on the representation of rules that are automatically learned from exemplars and can be applied to objects or elements that differ in perceptual form and modality. These rules can be described as sequential dependencies between vocabulary elements. The transfer effect in artificial grammar learning – the ability of participants to classify sequences in a different vocabulary than exemplars as being grammatical or not – has been taken as *prima facie* evidence that grammatical knowledge is abstract. However, episodic-fragment and exemplar based theories of the transfer of learning are also predicated, in part, upon knowledge of grammatical rules also in the form of sequential dependencies. The episodic-fragment based theory (e.g. Perruchet & Pacteau, 1990; Redington & Chater, 1996) shares much in common with the abstractionist account (e.g. Reber & Lewis, 1977) – both involve the acquisition of fragmented sequential dependencies between both repeating and non-repeating vocabulary elements. However, on inspection memory for permissible fragments alone cannot account for participants' ability to identify illegally placed fragments within a sequence or to map knowledge from one vocabulary to another (e.g. Gomez & Schvaneveldt, 1994; Gomez, 1997). Knowledge of fragments can support the correct classification of sequences in a novel vocabulary if participants also encode other information about those fragments. For example, participants might learn how often elements occur and co-occur with other elements in specific locations, such information would allow those structures to be mapped onto similar structures in a novel vocabulary. The important feature of a mechanism such as this is that the correspondences between two vocabularies must be induced *across* sequences. For example, in order to determine that a permissible bigram such as *MT* in one vocabulary corresponds to *XY* in another, one must first determine whether the frequency with which *M* and *T* co-occur in the training exemplars is proportional to the frequency with which *X* and *Y* co-occur in the test sequences. In which case the fragmentary account becomes indistinct from the abstractionist account which assumes that the rules that participants learn reflect the statistical distribution of vocabulary elements with the language (see Cleeremans & Jiménez, 1998). In contrast, exemplar based accounts of artificial grammar learning are predicated upon the representation of whole exemplars rather than fragments of exemplars. In the same vocabulary as learning, new sequences can be both endorsed and rejected on a sequence-by-sequence basis according to whether or not they are similar to one or more stored exemplars. Similarly, classification in a novel vocabulary also proceeds on a sequence-by-sequence basis. However, the only information that any rubric of similarity can be computed over on a sequence-by-sequence basis in a novel vocabulary is the patterns of repeating elements. Patterns of repeating elements are of course a form of sequential dependence. It follows that determining which forms of sequential dependency participants are able to classify in a novel vocabulary indicates in what form participants represent artificial languages.

The first part of this chapter reviews the theoretical constructs relating to the transfer of grammatical information that can demarcate different forms of abstract knowledge. Next the empirical findings of this thesis are related to those theoretical constructs. This chapter concludes with a discussion of how this thesis might impact on theories of artificial grammar learning.

6.1 The representation of artificial languages: Summary of empirical work

Theoretical constructs

Chapter 1 discussed a number of ways in which knowledge could be described as abstract. For example, knowledge can be abstract in the sense that it summarises information from a series of exemplars that allows the category membership of new stimuli to be determined (e.g. Smith *et al.* 1992). This form of knowledge is often referred to as a necessary (but not sufficient) property of rule-like knowledge (Shanks, 1995). Memory for fragments of exemplars is one description of this kind of knowledge. Fragments can form the conditional basis of decision rules that participants might use to determine whether a previously unseen sequence is a category member or not (c.f. Dulany *et al.* 1984; Perruchet & Pacteau, 1990). The transfer of this kind of knowledge to novel vocabularies reveals another rule-like property of fragmentary information – an encoding of the sequential dependencies between vocabulary elements. However this kind of knowledge appears not to be abstract in the sense that it is independent of the vocabulary in which it was acquired, but can support transfer by being mapped (but not it seems automatically) onto the vocabulary elements of a novel vocabulary. This form of knowledge was referred to as a *narrow abstraction* (c.f. Shanks, 1995). In contrast a *broad abstraction* can be readily applied, without any mapping process, onto the elements of a novel vocabulary. In an artificial language a broad abstraction could apply to knowledge of sequential dependencies between repeating elements. Chapter 1 argued that these two forms of abstraction could be distinguished behaviourally by the way in which they can be applied in a novel vocabulary. The narrow abstraction can only be applied by inducing the correspondences, *across* sequences, between the source and novel vocabularies, whereas the broad abstraction can be applied on a *sequence-by-sequence* basis. This behavioural distinction corresponds to different theories of artificial grammar learning. Indeed conceptualising exemplar based and rule-based theories in terms of sequential dependencies between vocabulary elements reveals a psychologically real distinction between the way dependencies of identical and of non-identical elements are mapped across vocabularies. Theories predicated upon learning episodic fragments of sequences, or fragmentary rules, and the classification of new sequences according to whether those fragments can be applied to those sequences, or conform to those rules, correspond to the narrow abstraction of dependencies between both repeating and non-repeating elements. This mode of representing an artificial language can only be applied in a novel vocabulary by inducing information *across* sequences. Theories predicated upon learning whole training exemplars and the classification of new sequences on the basis of their similarity to stored exemplars on a sequence-by-sequence basis correspond to the broad abstraction of sequential dependencies of repeating elements. It is important to note that the classification of sequential dependencies between repeating elements (abstract analogy) is in fact orthogonal to the representation of whole exemplars. That is, if participants classify sequences according to dependencies between repeating elements — abstract analogy — it does not necessarily imply that participants encoded other features of a training exemplar.

The following section relates these theoretical constructs to the empirical observations described in Chapters 2-5.

Empirical observations

Chapter 2 found that the transfer of grammatical knowledge was by no means an automatic or ubiquitous process. In three of the four Experiments reported in Chapter 2 participants were able to classify test sequences in the same vocabulary as the training exemplars but were unable to transfer that knowledge to a novel vocabulary. Experiment 2, however, successfully replicated the transfer effect using identical stimuli previously used by Altmann *et al.* (1995; Experiment 4). The difference between these experiments lay in the grammatical constraints that were violated in the ungrammatical sequences.

The ungrammatical sequences used in the experiments that did not demonstrate a transfer effect contained a smaller proportion of illegal bigrams[1] than the ungrammatical sequences used in Experiment 2. This difference was due primarily to the breaking of one particular grammatical constraint in a proportion of the ungrammatical sequences used in Experiment 2 but not in the other Experiments: that only two elements (*hes* or *vot*) may begin a sequence and they may not occur elsewhere in a sequence.

Chapter 3 demonstrated that the transfer effect observed in Experiment 2 and by Altmann *et al.* was almost entirely due to the frequency with which an element could begin a sequence. A number of workers have found that in the same vocabulary as learning participants are relatively more sensitive to the beginning portions of a sequence (e.g. Perruchet & Pacteau, 1990; Reber & Lewis 1977). In Experiment 2 and in Altmann *et al.* (1995, Experiment 4), all of the grammatical test sequences, and 65% of the ungrammatical sequences began with one of two starting elements (*hes* or *vot*). Altmann *et al.* reported that in the novel vocabulary participants were able to discriminate between grammatical test sequences and the subset of ungrammatical sequences that did not begin with an illegal starting element. They concluded that participants were able to apply knowledge of the sequential dependencies between vocabulary elements. However, that finding may have been due to participants being sensitive to another cue – whether or not those sequences contained patterns of repeating elements that were not present in the training exemplars. Although patterns of repeating elements are a form of sequential dependence, the aim of Chapter 3 was to investigate whether participants are sensitive to second- or higher-order sequential dependencies between non-repeating elements, as Altmann *et al.* concluded, or whether the effect was due solely to the transfer of first-order dependency information—specifically how frequently an element can begin a sequence. When participants were prevented from applying knowledge of which element began each sequence they were unable to effect transfer. It seems that participants learn where and how frequently an element occurs in a sequence

[1] The proportion of illegal bigrams is defined as the proportion of bigrams in the test set that did not occur in the exemplars.

irrespective of whether it co-occurs with other elements or not. This feature of frequency based classification is however characteristic of narrow abstraction. That is, it can only be accomplished by encoding how frequently elements occur in specific locations within training exemplars, and inducing, across sequences, how frequently elements in a novel vocabulary occur in the same positions of new sequences. These findings confirmed that the distinction between mapping narrow abstractions across sequences is a valid distinction and can form the basis for a mechanism that effects the transfer of information across vocabularies.

This sensitivity to which elements can begin a sequence is important because it indicates that even if participants are sensitive to second- or higher-order dependencies in the source vocabulary they seem to transfer knowledge of only first-order dependencies to a novel vocabulary.

Because previous demonstrations of transfer have confounded different forms of grammatical information, Chapter 4 specifically addressed the question of whether participants were able to transfer information regarding second-order sequential dependencies (bigrams) between non-identical vocabulary elements. For this series of experiments a partial grammar was constructed that removed all other confounding cues to the grammaticality of the test sequences. This partial grammar generated only two dependencies that always occurred in the same positions and were embedded within larger sequences. Previous experiments that have claimed to demonstrate the transfer of bigram information (second-order dependencies) have been confounded with frequency-by-location information (first-order dependencies; e.g. Shanks *et al.* 1997). The important feature of the dependencies that this grammar generated was that they could not be mapped onto sequences in a novel vocabulary on the basis of frequency information, and did not contain any repetition structures at all. Only knowledge of the co-occurrence of individual elements with other elements could be used to discriminate between sequences.

Chapters 2 and 3 found that participants could learn some abstract properties of vocabulary elements that are amenable to transfer, such as how frequently they occur and where they can occur. Chapter 4 adds to these findings by showing that participants also encode, and can transfer, the sequential dependencies between different vocabulary elements. This represents the first conclusive evidence for the transfer of *grammatical* knowledge (defined as knowledge of sequential dependence) across vocabularies, but does not obviate prior claims of the transfer of *implicit* knowledge (the relationship between these issues is explored later). The difference between the stimuli used in Chapters 2 and 3 that found evidence of the transfer of only first-order dependencies, and Chapter 4 that found evidence of the transfer of second-order dependencies, was that the language used in the latter Experiments did not contain patterns of repeating elements. Chapter 5 investigated the relationship between participants' sensitivity to dependencies of identical and of non-identical vocabulary elements.

Sequential dependencies between identical elements form the repetition structures of a language. A number of workers have suggested that the correct classification of sequences in a novel vocabulary can proceed by an abstract analogy with the repetition structures of stored exemplars (e.g. Brooks &Vokey, 1991). Even in randomly changing vocabularies, the patterns of repeating elements are preserved, and can be compared, in terms of similarity, to stored exemplars on a sequence-by-sequence basis (e.g. Whittlesea & Dorken, 1993). The distinction between the classification of test sequences according to non-repeating and repeating elements corresponds to the distinction between narrow and broad abstract knowledge. The primary contribution of Chapter 5 was the introduction of a manipulation—noisy training exemplars—that dissociated these two modes of classification, and by inference of representation. In the case of classification according to repetition structures; if the component elements of a dependency between repeating elements do not always co-occur in the exemplars (noise), so long as a match can be found with the repetition structure in a test sequence, that test sequence should be correctly endorsed. In contrast, the task of inducing a mapping between the dependencies between non-identical elements is dependent upon learning how frequently different elements co-occur. If the component elements of this form of sequential dependency do not always co-occur, participants' ability to induce the correspondences between two different vocabularies should be impaired. That is, the frequency distribution of *n*gram information abstracted from the training exemplars might not correspond to the frequency distribution of *n*gram information in the test sequences. The experiments in Chapter 5 tested these predictions.

Chapter 5 confirmed the findings of the previous empirical chapters and the theoretical arguments advanced in Chapter 1. Whether or not the elements of a sequential dependency always co-occur in training exemplars demarcates narrow and broad forms of abstractions. When the language contained only dependencies between non-repeating elements, noise did not impair participants' ability to learn that dependency (as indexed by participants' ability to apply that knowledge in the source vocabulary) but severely impaired participants' ability to map that information onto a novel vocabulary. However, noise did not impair the transfer of sequential dependencies between repeating elements. Finally, when the language contained dependencies between both repeating and non-repeating elements, and the latter was the only cue to grammaticality, participants were unable to classify test sequences in either the source or the novel vocabulary. Clearly participants did not encode the dependencies between non-repeating elements. This finding indicates that participants do not memorise whole training exemplars, and that classification according to abstract analogy reflects the encoding of patterns of repeating elements as sequential dependencies during acquisition and not as un-abstracted patterns in stored exemplars. That is not to say, however, that participants cannot learn first-order dependency information from sequences that also contain repetition structure, as evident in Chapter 3, but in this case such information could not be used to discriminate between sequences.

Summary

People can learn grammatical information from exemplar sequences and represent it in ways that conform to the two definitions of abstract that were discussed in Chapter 1. First, the knowledge represents fragments of the sequential rules of the grammar that allow previously unseen sequences to be correctly categorised. Second, that knowledge can be transferred to novel vocabularies. This implies that participants also encode other properties of vocabulary elements that allow them to be mapped onto elements in another vocabulary, such as whether they repeat or not, and how frequently they occur and co-occur with other elements. The features of sequences that participants are able to classify in the source vocabulary may be taken as the sum of the knowledge that participants encode and are able to apply following exposure to exemplars. The features that participants are able to classify in a novel vocabulary may be taken as the subset of the knowledge available in the source vocabulary that is independent of the exemplar vocabulary. The residual difference between the two can only reflect vocabulary dependent knowledge. Clearly participants are sensitive to a wide range of grammatical constraints and the similarity of test sequences in the source vocabulary, but even when the correspondences between the two vocabularies are transparent (Dienes & Altmann, 1997) participants are unable to transfer all of their knowledge. This thesis has determined what knowledge learned from exemplars can be characterised as grammatical knowledge. For these purposes grammatical knowledge is abstract knowledge in the sense that it is an encoding of rule-like sequential dependencies and can be transferred to novel vocabulary elements. However, participants are not equally sensitive to different forms of sequential dependence and this informs us about how we should characterise the representation of artificial languages. For example, when in Chapter 5 participants studied training exemplars that contained dependencies between both repeating and non-repeating elements they failed to encode the dependencies non-repeating elements. However, Chapter 3 demonstrated that some information concerning particularly salient non-repeating elements is encoded such as how frequently they occur and where they might occur. This information allows participants to map the identity of a non-repeating element in one vocabulary onto an element in another. When, in Chapter 4 (and the first experiment of Chapter 5), the language (exemplars and test sequences) did not contain any dependencies between repeating elements participants can learn and transfer dependencies between non-repeating elements. In these cases participants learn, presumably also on the basis of frequency, which elements can co-occur, and subsequently map that information onto similar structures in another vocabulary. Of course whether elements and structures in a novel vocabulary share frequency distributions with those seen in the source vocabulary can only be induced *across* sequences. When the frequency distributions of the training test sets do not correspond, the process that maps dependencies of non-repeating elements across vocabularies is disrupted. However, when the language contained dependencies between repeating elements, participants can learn and subsequently classify sequences on that basis – that is by abstract analogy. Unlike the mapping of non-repeating elements, the process that maps repeating elements is not disrupted when the frequency distributions of the training and test sets do not correspond in a similar way.

The classification of test sequences in a novel vocabulary according to patterns of repeating elements has been taken to support theories of artificial grammar learning based upon episodic memory for exemplars (e.g. Brooks & Vokey, 1991; Whittlesea & Dorken, 1993). In contrast the classification of test sequences according to *n*gram information has been taken to support theories based upon both rule abstraction and memory for fragments (e.g. Altmann *et al.* 1995; Redington & Chater, 1996; Reber, 1993; Manza & Reber, 1997). But simply because the mapping processes involved in the transfer of these two forms of sequential dependence differs does not necessarily imply that these two forms of sequential dependence are predicated upon different modes of representation. The following sections explore the implications of these findings for theories of artificial grammar learning.

6.2 Implications for theories of artificial grammar learning

Chunk strength and the transfer of narrow abstractions

Since Miller (1956) first described the process of 'chunking' as a means to learning and remembering increasingly large and complex information, the 'chunk' has had a privileged place in theories of cognitive processes. This is especially true of artificial grammar learning where the chunk is often referred to in terms of *n*gram information. This section discusses the implication of the notion of chunking in artificial grammar learning and concludes that the process that maps *n*gram information learned in one vocabulary onto another operates over individual elements and not chunks.

A number of workers have found that chunk strength, how frequently bigrams that occurred in the training exemplars occur in test sequences, is a major determinant of whether participants endorse or reject a test sequence in at least the same vocabulary as the training exemplars. Some workers have argued that this measure of chunk strength is a measure of similarity rather than of grammaticality (e.g. Meulemans & Van der Linden, 1997). However, that assumption holds only if participants do not encode where those bigrams can occur. Johnstone and Shanks (1999) have demonstrated using regression techniques that, consistent with the empirical evidence, participants do encode positional information and that chunk strength should be regarded as a measure of the grammaticality of a sequence. Of course similarity, as defined by chunk strength, and grammaticality are not truly orthogonal in an artificial language. Rather participants' sensitivity to chunk strength should be taken as an index of participants' knowledge of the distributional properties of the language. Moreover, the research presented in this thesis has a number of implications for our conception of a chunk or *n*gram. In particular that the transfer of *n*gram information operates over the distributional properties of the component elements of a chunk, rather than the distributional properties of the chunk itself.

Chunk strength was, in the studies reported here, a relatively unimportant cue to the grammaticality of a sequence in a novel vocabulary. In Chapter 2 decreasing the chunk strength of the ungrammatical sequences relative to the grammatical sequence facilitated the classification of sequences in the source vocabulary but not the novel vocabulary. In Chapters 4 and 5, chunk strength could not be used as a cue to discriminate between sequences, yet participants were able to correctly classify sequences in a novel vocabulary. More important, in the studies reported here, was the distinction between sequential dependencies between repeating and non-repeating elements. The following section considers this distinction further.

On the representation of different forms of sequential dependence

Participants encode and apply dependencies between repeating and non-repeating elements in different ways. It is this feature of the empirical observations that impact upon computational models of artificial grammar learning. This section begins by reviewing this evidence and how different computational models might explain these data. This section concludes with some brief speculations about what this might mean in terms of general cognitive processes.

In Chapter 3, participants learned but were unable to transfer information concerning second- or higher-order dependencies between non-repeating elements, in a language that also contained dependencies between repeating elements. Participants were however able to transfer knowledge of at least one first-order dependency in the first position of each sequence in the language. In Chapter 4 (and the first experiment of Chapter 5), participants were able to learn and transfer information concerning second-order dependency information, but in this case the language did not contain dependencies between repeating elements. In Chapter 5, when the language (training exemplars and test sequences) contained dependencies between both repeating and non-repeating elements, participants were unable to apply information concerning second-order dependencies between non-repeating elements, in either the same or a different vocabulary. Similarly, when the training exemplars contained dependencies between both repeating and non-repeating elements but the test sequences contained only dependencies between non-repeating elements, participants were still unable to apply the relevant information, in either the same or a different vocabulary. These two studies strongly suggest that participants did not encode any information concerning dependencies between non-repeating elements. In contrast, when dependencies between repeating elements was the cue to the grammaticality of the test sequences participants were able to apply the relevant information in both the same and a different vocabulary, even when some of the exemplars were irrelevant. Only when neither the training nor test sequences contained repeating elements, could participants transfer knowledge of second-order dependencies across vocabularies.

Of the different computational models of artificial grammar learning only THIYOS (Mathews *et al.* 1989) encodes dependencies between repeating and non-repeating elements differently. The classification of sequences in the source vocabulary proceeds on the basis of dependencies between both repeating and non-repeating elements. However, transfer is effected solely on the basis of patterns of adjacent repeating elements. But this model could not account for any of the effects observed in this thesis. For example, if transfer was effected by humans solely on the basis of repeating elements (whether adjacent or not) participants would not have been sensitive to the frequency with which a non-repeating element began the sequences in Chapter 3. Nor would participants have been able to correctly classify sequences according to second-order dependencies between elements in the partial grammars that contained no repetitions at all. Because THIYOS does not utilise frequency information abstracted from exemplars to induce the correspondences between vocabularies across sequences it cannot transfer information concerning elements that do not repeat.

Exemplar-based models (e.g. Brooks & Vokey, 1991; Whittlesea & Dorken, 1993) also encode dependencies between both repeating and non-repeating elements (although these are tacit within the representation of whole-exemplars). Classification of test sequences proceeds, sequence-by-sequence, on the basis of the similarity of those test sequences to stored exemplars. In the source vocabulary similarity can be computed over dependencies between both repeating and non-repeating elements. In a novel vocabulary, similarity can only be computed, sequence-by-sequence, on the basis of dependencies between repeating elements. Like THIYOS, this class of model cannot account for many of the effects reported here; for example, in Chapter 4 (and the first experiment of Chapter 5) where the language did not contain any repeating elements. Even if exemplar-based models *could* compute similarity over non-repeating elements in a novel vocabulary they still could not account for these data because each test sequence, both grammatical and ungrammatical, was equally similar to the exemplars. Those sequences could only be correctly classified if participants had induced rules, on the basis of statistical regularity in the source vocabulary, which governed the co-occurrence of different elements. Moreover, the data reported in Chapter 5 are also problematic for exemplar based accounts of source vocabulary performance. In Chapter 5 participants were asked to study exemplars that contained dependencies between both repeating and non-repeating elements. However, in two of the Experiments reported there the test sequences could only be classified on the basis of dependencies between the non-repeating elements. If participants had access to veridical representations of whole exemplars they should have been able to classify the test sequences in the source vocabulary. This was not the case. In principle, exemplar-based models might predict the effects observed in Chapter 3 if it is assumed that participants were able to detect the *novelty* of the low frequency starting elements. However, this would forsake a fundamental principle of exemplar-based models – that similarity is computed on a sequence-by-sequence basis.

Both THIYOS and exemplar-based models of classification are interesting because they assume that dependencies between repeating and non-repeating elements have different roles in artificial grammar learning. Despite this they are unable to account for many of the effects reported in this thesis. Chapter 1 concluded that although the Simple Recurrent Network (SRN) model does not distinguish between different forms of sequential dependency it provides by far the best fit with the effects observed in the existing empirical literature (see Cleeremans, 1993; Dienes *et al.* 1999). It is unclear however, whether the modified SRN used by Dienes *et al.* (1999) to simulate transfer could account for all of the effects observed in this thesis. As with other classes of model the SRN acquires information about the dependencies between both repeating and non-repeating elements. However, consistent with the findings of Chapter 4 and in contrast with other models, the SRN can transfer dependencies between non-repeating elements as well as dependencies between repeating elements. The SRN acquires dependencies incrementally, beginning with first-order frequency by location information and subsequently second- and higher-order dependency information. This model predicts the effects seen in Chapter 3 where participants were sensitive to how frequently an element began a sequence, even in a language that also contained repetition structures.

However, if the network were also to simulate the transfer of second-order dependencies reported in Chapter 4 (and the first experiment of Chapter 5) then it should be able to discriminate between the sequences that began with legal starting elements in Chapter 3. In which case the model would be too powerful. On the other hand, if the network simulated the behaviour of participants in Chapter 3 and could not discriminate between the sequences that began with a legal starting element, it should not simulate the transfer of second-order dependencies observed in Chapter 4 (and the first experiment of Chapter 5). In which case the model would be too weak. This paradox arises because the SRN does not distinguish between dependencies of repeating and of non-repeating elements. Of course, these findings were observed using partial grammars, that constrained only half of the vocabulary elements, rather than finite-state grammars that constrain all of the vocabulary elements. The differences in the types of grammar may have some relevance to this issue, but these are issues that cannot be resolved without testing the model itself, and remain open to enquiry. What is important is that the distinction between dependencies between repeating and non-repeating elements, and the ways in which they can be transferred to a novel vocabulary are psychologically real and provide good tests of formal models of artificial grammar learning. Of course, these formal models are instantiations of theories of representation. The following section discusses the relevance of these data to the distinction between episodic- and rule-based representation of knowledge.

On rules vs. episodes

It is often noted that psychologists love dichotomies (e.g. Cleeremans, 1997; Reber, 1993; Whittlesea & Dorken, 1997) because they aid our understanding of seemingly different constructs. A classic example, with particular relevance to artificial grammar learning, is the distinction between implicit and explicit cognition (we return to this issue in the next section). But this dichotomy has been largely superseded by the distinction between episodic- and rule-based processing. The data reported here couched the distinction between episodic and rule-based processing in terms of the information that is available to those putatively discrete systems in a novel vocabulary. But as we have seen, the difference between the information available is a consequence of the processes that map that information across domains rather than the representation of that information itself. Of course, learning and memory are inextricably linked, but differ, at least theoretically, in terms of the classification of new category members. But if those classification processes are a function of the mapping processes why should we retain a distinction between episodic and rule based processing at all?

Exemplar based accounts of artificial grammar learning are predicated upon the similarity of test sequences, calculated on a sequence-by-sequence basis, to stored exemplars. These theories are generally vague concerning the features over which similarity is determined, with some mechanisms computing similarity over bigram information (in which case the model is fragmentary; e.g. Meulemans & Van der Linden, 1997) and others computing similarity over individual elements (e.g. Vokey & Brooks, 1992). In a novel vocabulary, however, the only basis upon which exemplar-based accounts can compute similarity is sequential dependencies between repeating elements — a process often referred to as *abstract analogy* (Brooks & Vokey, 1991; Whittlesea & Dorken, 1993). Chapter 5 indicated that participants prefer to encode during training, and apply during testing, information regarding patterns of repeating elements to the detriment of dependencies between non-repeating elements. This latter finding is inconsistent with a model based upon the veridical representation of whole exemplars that would predict correct classification in at least the source vocabulary. The final Experiment of Chapter 5 demonstrated that the classification of dependencies between repeating elements was unaffected by noise, indicating that participants could recall a relevant pattern from stored exemplars and consequently apply that knowledge on a sequence-by-sequence basis (because no information need be computed *across* sequences at all). This classification of test sequences by abstract analogy (Brooks & Vokey, 1991) is associated with memory for whole exemplars (Vokey & Brooks, 1992). However the two are in fact orthogonal to one another.

During learning, participants may encode dependencies between repeating elements rather than those of non-repeating elements because they might be more salient. But should we regard the encoding and subsequent classification of patterns of repeating elements as being predicated upon episodic memory for whole exemplars or upon rule-abstraction? It could be argued that information concerning repeating elements is encoded and subsequently applied on the basis of salience and cognitive economy. Such an argument would be inconsistent with a model that stores veridical representations of whole exemplars – itself a rather uneconomic approach. It is equally, and perhaps more plausible that dependencies between repeating elements could be encoded as rules rather than as incidental properties of stored exemplars. For example, Brooks and Vokey (1991) argued that to classify a sequence in a novel vocabulary a participant might compare that sequence to an array of stored exemplars. Since in Chapter 5, participants appeared not to encode any features other than repetition structures it seems reasonable, again on the basis of economy, that rather than storing all ninety-six exemplars, participants could simply form a single representation for each of the four dependencies between repeating elements (– – A – A –, A – A – – –, – – – D – D, – D – D – –). Representations of dependencies such as these could be described as templates and could be applied on a sequence-by-sequence basis. Such template-like representations would remain consistent with an episodic-similarity-based framework and could not generalise to previously unseen patterns of repeating elements. In contrast, such patterns could, in principle, also be captured by broad rule abstraction. In fact all four dependencies between repeating elements used in Chapter 5 could be captured by a single rule of the form *"An element in position n predicts an identical element in position n+2"*. Such a rule would permit the correct classification of test sequences according to patterns of repeating elements, and would also *generalise* to previously unseen repetition structures (e.g. – A – A – –), but would not generalise to sequences that violated the rule (e.g. A – – – A –). Indeed a rule involving repeating elements would seem little different from a rule involving non-repeating elements, and would only differ in the way that such information is mapped during test. Whether participants encode dependencies between repeating elements as features in an array of whole exemplars, templates, or rules is an empirical issue yet to be resolved. However, on inspection it seems that rule abstraction could, in principle, provide a parsimonious account of classification according to repetition structures. This would be consistent with the SRN model of transfer (Dienes *et al.* 1999). The dissociation between the application of those two forms of sequential dependency when drawn from a set noisy of exemplars (Chapter 5) is a consequence of the information that is available to induce a mapping between vocabularies. It is not necessarily due to encoding differences – different mapping processes do not imply different modes of representation. Although, the distinction between these two forms of sequential dependency has been associated with the distinction between broad and narrow abstraction this is a consequence of the stimuli and not encoding. That is, when participants were asked to study exemplars they were not informed that they would be later asked to transfer any relevant information to a novel vocabulary, and so could not have differentially encoded information relevant to the task. Rather the distinction between broad and narrow abstraction can only reflect the incidental properties of the stimuli, repetition structures form broad abstractions solely because they have a property (they repeat) that is readily apparent in a novel vocabulary. This property is not shared with dependencies between non-repeating elements. However, participants' preference to encode dependencies between repeating rather than non-repeating elements might reflect a more general principle whereby people prefer to encode broad abstractions that are more salient and perhaps more readily generalized than narrow abstractions. To recapitulate, had participants learned the sequences used in Chapter 5 by memorizing whole exemplars then they should have been able to classify sequences that contained illegal dependencies between non-repeating elements in the source vocabulary correctly. This was clearly not the case, even when the training exemplars did not contain noise.

In contrast with exemplar-based accounts of transfer, abstract analogy of repetition structures is not the only means to effect transfer. The findings of Chapter 4 provide a clear demonstration that participants can learn and transfer knowledge of second order dependencies between non-repeating elements in a language that contained no repetition structures at all. Moreover, the classification of those sequences could not have been based upon an entirely episodic system at all, first because the language did not contain patterns of repeating elements, and more importantly because each test sequence, whether grammatical or ungrammatical was equally similar to the exemplars. The correct classification of these sequences could only have occurred on the basis that participants had induced a rule regarding which elements could co-occur. At first glance these data seem to contradict the findings of Gomez *et al.* (in press) who found that participants were able to learn a language that did not contain repeats (as evident by classification in the source vocabulary), but were unable to effect transfer. In a second experiment Gomez *et al.* introduced dependencies between repeating elements into a third of the sequences. Participants were subsequently able to correctly classify the sequences that contained repeats (both grammatical and ungrammatical) but remained at chance on those sequences that did not. This last finding is consistent with the data presented in Chapter 5 – participants learn about dependencies between repeating elements to the detriment of those between non-repeating elements. Gomez *et al.*'s first finding that participants could not effect transfer when the language did not contain repeats, is on face value more problematic. However, the language used by Gomez *et al.* contained substantially more vocabulary elements than is common in artificial languages, primarily because there were no repeats. It is possible that there is an upper limit on the number of dependencies that participants may learn and be able to transfer in the relatively short learning episodes that are common in artificial grammar research. Second, participants may have had problems in inducing a mapping between the two vocabularies because the large grammar used by Gomez *et al.* meant that an element in one position might predict two or more elements in the next. If those dependent elements were not equifrequent then they could have impaired participants' ability to determine the correspondences between vocabularies. That is, participants may have responded in the Gomez *et al.* study in the same way that participants did in Chapter 5 when asked to study a language that did not contain repeats, where low frequency dependencies in training exemplars inhibited participants' ability to map high frequency dependencies across vocabularies.

Why should we retain a distinction between episodic and rule based processing at all? After all both classes of theory are predicted upon the representation, in one form or another, of sequential dependencies. Although the simple recurrent networks described by Cleeremans (1993) and by Dienes *et al.* (1999) encode dependencies between both repeating and non-repeating elements, neither corresponds to an exemplar based memory system. Neural networks can of course instantiate exemplar based episodic classification mechanisms (e.g. Nosofsky, Kruschke, & McKinley, 1992) but these are also predicated upon the abstraction of statistical regularities. In practice neural networks that simulate classification according to similarity do not adequately simulate participants' performance in artificial grammar learning (see Dienes, 1992) whereas those that simulate classification according to the statistical regularities themselves – sequential dependencies – do (Dienes, 1992; Dienes *et al.* 1999; Redington & Chater, 1999). So the differences in the way that participants treat sequential dependencies between repeating and non-repeating elements could, in principle, be accounted for within a unitary framework rather than relying upon discrete modes of representation. The following section discusses the extent to which the data reported in Chapters 4 and 5 contribute to the literature concerning the other dichotomy in artificial grammar learning – the implicit-explicit distinction?

Is transfer implicit?

Artificial grammar learning has been regarded by some workers as an ideal paradigm for the study of both implicit learning and the representation of tacit knowledge (e.g. Reber, 1967). An intuitive motivation for this use of artificial grammars is that generally we cannot introspect or verbally report our knowledge of natural grammars (or in fact exactly how we control our muscles, play chess, or control complex systems; hence serial reaction time learning, problem solving, and complex instrumental learning studies). A number of workers have argued that implicit learning phenomena are the consequence of a distinct learning process that is ancillary to the processes underlying explicit learning. Not least because implicit learning often shows dissociations between verbal report, direct probes and the presence of amnesia and other cognitive insult (e.g. Abrams & Reber, 1988; Cohen & Curren, 1993; Knowlton & Squire, 1994). Unfortunately, these dissociations often fail to be replicated in normal populations (e.g. Perruchet & Gallego, 1993; Perruchet & Pacteau, 1990). Other workers have argued that rather than searching for objective task dissociations, dissociations between subjective confidence and task performance can be psychologically meaningful (e.g. Chan, 1992; Cheesman & Merikle, 1984; Dienes *et al.* 1995b; Dienes & Altmann, 1997).

It is common to conclude that knowledge of artificial languages involves explicit memory for bigrams (e.g. Perruchet & Pacteau, 1990). Wherever dissociations *are* observed critics claim that the direct tests were insensitive or did not identify the correct units of knowledge (see Shanks & St. John, 1994, for a review). But memory for bigrams or exemplars may not in fact be directly responsible for classification at all. In Chapter 4 the knowledge responsible for indirect test performance were rules that specified the co-occurrence of particular vocabulary elements. Two Experiments in Chapter 4 also included a direct test of participants' memory for target bigrams. These two experiments add to a number of studies demonstrating associations between indirect test performance and the ability to recall or recognise bigrams. It seems that participants could only transfer knowledge of these rules to the novel vocabulary if they could explicitly recall at least some of the target bigrams from exemplars. This finding is consistent with a study reported by Gomez (1997) who found bigram information could not be applied in a novel vocabulary unless participants were able to recognise the target bigrams in the source vocabulary. So of course participants will be able to remember bigram and other information from a set of exemplars that they saw maybe fifteen minutes prior to being asked whether they recognised them. But this does not imply that the knowledge used to *map* bigram information onto a novel vocabulary is also explicit. Consider the experiments in Chapter 3 where participants were able to map the identity of the starting element of sequences in one vocabulary onto starting elements in another on the basis of frequency information. Clearly participants, had they been asked, would have been able to recognise the starting elements in the source vocabulary but that does not imply that the frequency information that was used to map those elements was explicitly represented. This argument is embodied into a variety of computational models of artificial grammar learning. For example, how frequently fragments occur (chunk strength) is regarded as implicit in Servan-Schreiber and Anderson's (1990) Competitive Chunking model although the fragments themselves are available for free report. Similarly, the strength of association between distributed processors in neural networks is assumed to be implicit although the system must explicitly represent vocabulary elements in the input and output units (Dienes & Perner, 1992). Finally THIYOS explicitly encodes fragmentary rules of co-occurrence as bigram information, but the strength of those rules and how frequently they can be applied is represented only tacitly (Mathews & Roussel, 1997). Nonetheless, in Chapter 4 associations were observed between direct-recognition and indirect-classification performance. But as Dienes *et al.* (1995b) point out that does not imply that the knowledge used to classify sequences is *subjectively* implicit.

Dienes and Altmann (1997) asked participants to rate how confident they were for each classification decision they made. This permitted Dienes and Altmann to apply two subjective criteria, the *Guessing criterion* (c.f. Cheesman & Merikle, 1984), and the *Zero-correlation criterion* (c.f. Chan, 1992) to determine whether the knowledge used to classify sequences in a novel vocabulary was implicit. They concluded that because in the novel vocabulary participants made reliably more accurate than inaccurate guesses and confidence did not predict accuracy the knowledge used to classify sequences was implicit. Chapters 4 and 5 asked whether participants were aware of the incidentally acquired information that they later used to classify sequences. We can be sure that the measures of tacit knowledge were exhaustive because the information participants were provided with or were able to apply was specifically controlled. In each of the experiments in Chapters 4 and 5 subjective measures of participants' knowledge were taken. These revealed that participants' confidence predicted how accurate they were, suggesting a high degree of explicit knowledge. However, an implicit component was not ruled out even though the subjective measures ensured that this was possible. For example, although trained participants were reliably more confident in their decisions than were untrained participants, in each experiment the lower bound of participants' difference score (between correct and incorrect decisions) was below zero. This suggests an implicit component to participants' knowledge that could have been ruled out had this lower bound exceeded zero. Dienes and Altmann (1997) found that transfer *was* implicit according to subjective criteria. In their studies confidence did not predict accuracy, but as in Chapter 3 this may have been due to the mapping of information regarding the frequency-by-location of individual elements. In Chapters 4 and 5 the stimuli were specifically designed to prevent the transfer of this kind of knowledge. In sum, the experiments reported in Chapters 4 and 5 indicated that knowledge of both forms of sequential dependence was explicit according to subjective criteria, but could not exclude the possibility that an implicit component was involved.

However, as mentioned earlier, the partial-grammars used in Chapters 4 and 5 have properties that differ from the finite-state grammars typically used in artificial grammar learning. Although these partial-grammars were specifically designed to examine a number of hypotheses relating to finite-state grammar learning, these differences could, in principle, have led to participants using mechanisms of learning and transfer that might differ form those typically observed in finite-state and natural grammar learning. In sum, the data reported in this thesis did not determine conclusively whether transfer was subjectively implicit, in part because the simplicity of the languages led to extremely good performance.

6.5 Concluding remarks

Artificial grammar learning research has primarily been used to investigate phenomenological issues in learning (e.g. Reber, 1967). However, a number of workers have argued that the focus should be upon the representational issues involved in acquisition and categorisation (e.g. Shanks & St. John, 1994). This thesis has contributed to this latter area. It may be that both episodic and rule-abstraction play a significant role in transfer, because both are predicated upon the classification of sequences according to sequential dependencies. Indeed a number of workers regard artificial grammar learning, under the rubric of implicit learning, as a form of implicit memory (e.g. Neal & Hesketh, 1997; Shanks & St. John, 1994; Whittlesea & Wright, 1997), whilst others regard artificial grammar learning as primarily an exercise in concept formation (e.g. Berry & Dienes, 1991; 1993; Chan, 1992; Stanley *et al.* 1989), and a few as a proxy for natural language acquisition (e.g. Mathews & Cochran, 1998; McLaughlin, 1980; Winter & Reber, 1992).

Redefining episodic and fragmentary based models of the transfer effect in artificial grammar learning in terms of what forms of grammatical knowledge they support has revealed hitherto unknown aspects of the acquisition and representation of grammatical information, particularly in how information can be transferred to novel domains. These findings inform theories of both knowledge representation and natural language acquisition. As with most laboratory paradigms, how one interprets artificial grammar learning depends upon the nature of the grammar. For example, finite-state grammars have been used in a substantial number of experiments to demonstrate that complex sequential information can be learned, represented and applied implicitly. They have also been used to demonstrate that knowledge can be abstract in the sense of being represented as rules, and in the sense of being represented independently of vocabulary. Unfortunately, the history of artificial grammar learning has been littered with problems in identifying the units of knowledge that people learn and later apply, let alone defining operational terms such as 'abstract' and 'implicit'. These problems have hindered attempts to resolve very real and important issues in human cognition. For example, we cannot determine whether or not people are aware of a unit of knowledge, or whether that representation is abstract, unless we first know what that item of knowledge is. Psycholinguists using artificial languages have followed a rather different approach in their research than better known experimental psychologists (see for example the work of Morgan, Meier & Newport, 1986; Braine, 1966; McLaughlin, 1980). Rather than using an 'off the shelf' grammar (usually a finite-state grammar) psycholinguists generate grammars specifically tailored to test empirical questions about what can be learned rather than seemingly intractable questions about consciousness. Of course research should, where necessary, retain particular stimuli in the interests of generalisation and replication. Where specific questions about what is learned have been raised, psychologists have proceeded by generating new grammars, but often these retain the characteristics of finite-state grammars whereby vocabulary elements are dependent upon their position in a sequence as well as preceding elements. The grammars used in the experiments reported here represent both approaches; where replication was important a finite-state grammar was used, and where an investigation of what was actually learned and could be transferred was required new grammars were generated. The partial-grammars developed in Chapters 4 and 5 were vitally important in determining that the transfer effect, observed using finite-state grammars, is predicated on grammatical information in the form of sequential dependencies. As with other paradigms in experimental psychology, the big picture is in the details.

APPENDIX A

STIMULI

The stimuli used in Chapters 2 and 3 were generated by the finite-state grammar shown in Figure 1.2. Ungrammatical sequences were constructed by reordering the vocabulary elements of grammatical test sequences.

Experiment 1: Grammatical exemplars

Each sequence was presented four times in random orders and instantiated with syllables (14pt uppercase Times New Roman) according to the mapping shown in Figure 1.3.

HES JIX PEL DUP PEL
HES JIX PEL DUP PEL JIX
HES JIX PEL JIX DUP PEL JIX
HES JIX PEL JIX KAV
HES JIX PEL JIX KAV PEL
HES JIX PEL KAV
HES JIX PEL KAV
HES JIX RUD
HES JIX SOG PEL
HES JIX SOG PEL JIX
HES PEL DUP
HES PEL DUP PEL JIX
HES PEL JIX DUP
HES PEL JIX KAV PEL JIX
HES PEL KAV PEL
HES RUD PEL JIX
VOT JIX PEL DUP
VOT JIX PEL JIX DUP
VOT JIX PEL JIX KAV PEL JIX
VOT JIX PEL KAV PEL
VOT JIX PEL KAV PEL JIX
VOT JIX RUD PEL JIX
VOT JIX SOG
VOT PEL DUP PEL
VOT PEL JIX DUP PEL
VOT PEL JIX DUP PEL JIX

VOT PEL JIX KAV
VOT PEL KAV
VOT PEL KAV PEL JIX
VOT RUD PEL
VOT SOG PEL JIX

Experiments 2-9: Grammatical exemplars

Grammatical exemplar sequences used for training were presented four times in different orders. These sequences were instantiated with symbols according to the mapping shown in figure 1.3.

hes	jix	pel	jix	dup		
hes	jix	pel	jix	dup	pel	
hes	jix	pel	jix	dup	pel	jix
hes	jix	pel	kav	pel	jix	
hes	jix	rud	pel			
hes	jix	rud	pel	jix		
hes	jix	sog				
hes	pel	dup				
hes	pel	dup	pel			
hes	pel	dup	pel	jix		
hes	pel	jix	dup			
hes	pel	jix	kav	pel		
hes	pel	jix	kav	pel	jix	
hes	pel	kav	pel			
hes	rud					
hes	sog					
vot	jix	pel	dup	pel	jix	
vot	jix	pel	jix	kav		
vot	jix	pel	jix	kav	pel	
vot	jix	rud				
vot	jix	sog	pel			
vot	jix	sog	pel	jix		
vot	pel	jix	dup	pel		
vot	pel	jix	dup	pel	jix	
vot	pel	jix	kav			
vot	pel	kav				
vot	pel	kav	pel			
vot	pel	kav	pel	jix		
vot	rud	pel	jix			
vot	sog	pel				

Experiment 1: Grammatical test sequences

Test sequences were intermingled with ungrammatical sequences (see below) and presented twice, once instantiated with syllable and once instantiated with symbols according to the mapping given in Figure 1.3. For each presentation the sequences were presented in different orders.

VOT JIX PEL DUP PEL
VOT PEL DUP PEL JIX
HES JIX PEL KAV PEL
VOT PEL KAV PEL
VOT JIX PEL DUP PEL JIX
VOT JIX PEL JIX DUP PEL
VOT PEL JIX KAV PEL
HES PEL JIX KAV PEL
VOT JIX PEL JIX DUP PEL JIX
HES PEL JIX DUP PEL JIX
HES PEL DUP PEL
HES JIX RUD PEL JIX
VOT JIX PEL JIX KAV
HES PEL KAV PEL JIX
VOT JIX PEL JIX KAV PEL
HES JIX PEL JIX DUP
HES JIX PEL KAV PEL JIX
HES JIX PEL JIX KAV PEL JIX
VOT JIX SOG PEL JIX
HES PEL JIX DUP PEL
VOT PEL JIX KAV PEL JIX
HES JIX PEL JIX DUP PEL
VOT PEL DUP
HES SOG PEL JIX
HES SOG PEL
VOT JIX RUD PEL
HES JIX PEL DUP
HES RUD PEL
HES PEL JIX KAV
VOT SOG PEL
HES JIX RUD PEL
VOT RUD PEL JIX
VOT JIX RUD
VOT JIX SOG PEL
VOT JIX PEL KAV
HES JIX SOG
VOT PEL JIX DUP
HES PEL KAV

Experiments 2-8: Grammatical test sequences

In Experiments 5-8 the two sequences that contained only two elements were omitted.

vot	jix	sog				
hes	sog	pel	jix			
vot	pel	dup	pel	jix		
hes	jix	pel	jix	kav		
vot	jix	pel	dup			
hes	rud					
vot	jix	pel	kav	pel		
vot	sog					
vot	pel	jix	kav	pel		
hes	rud	pel	jix			
vot	pel	jix	dup			
hes	jix	pel	kav			
hes	pel	kav				
hes	sog	pel				
vot	jix	rud	pel	jix		
hes	jix	pel	dup	pel	jix	
hes	jix	pel	kav	pel		
hes	pel	kav	pel	jix		
vot	jix	pel	jix	dup	pel	
vot	jix	pel	jix	dup	pel	jix
vot	jix	pel	jix	dup		
vot	pel	jix	kav	pel	jix	
vot	pel	dup	pel			
hes	jix	pel	jix	kav	pel	jix
vot	sog	pel	jix			
hes	jix	pel	jix	kav	pel	
vot	jix	rud	pel			
hes	jix	rud				
hes	jix	sog	pel	jix		
hes	jix	pel	dup			
vot	jix	pel	kav	pel	jix	
hes	jix	pel	dup	pel		
hes	pel	jix	dup	pel		
hes	jix	sog	pel			
vot	rud	pel				
hes	pel	jix	dup	pel	jix	
vot	jix	pel	dup	pel		
hes	pel	jix	kav			
vot	jix	pel	kav			
vot	pel	dup				

Experiments 1 & 3: Ungrammatical test sequences

In Experiment 1 these sequences were intermingled with the grammatical sequences for Experiment 1, and for Experiment 3 they were intermingled with the grammatical sequences used in Experiment 2. In Experiment 3 the syllables were in lowercase.

VOT	JIX	PEL	JIX	PEL	DUP	
HES	PEL	DUP	JIX	PEL	JIX	
HES	JIX	DUP	JIX	PEL		
VOT	JIX	KAV	JIX	PEL		
VOT	PEL	JIX	PEL	DUP		
HES	PEL	JIX	PEL	KAV		
VOT	KAV	JIX	PEL	JIX	PEL	
HES	KAV	JIX	PEL	JIX	PEL	JIX
VOT	DUP	JIX	PEL	JIX	PEL	
HES	DUP	PEL	JIX	PEL		
VOT	PEL	SOG	PEL	JIX		
HES	PEL	RUD	PEL	JIX		
HES	DUP	JIX	PEL	JIX	PEL	
VOT	PEL	JIX	PEL	KAV		
VOT	KAV	PEL	JIX	PEL		
HES	PEL	JIX	PEL	DUP		
HES	PEL	JIX	PEL	JIX	KAV	
VOT	JIX	PEL	DUP	JIX	PEL	JIX
HES	KAV	PEL	JIX	PEL		
VOT	PEL	KAV	JIX	PEL	JIX	
VOT	JIX	KAV	PEL			
HES	PEL	RUD				
VOT	PEL	SOG				
HES	SOG	JIX				
HES	PEL	DUP	JIX			
HES	PEL	SOG	JIX			
VOT	DUP	PEL				
VOT	SOG	JIX	PEL			
VOT	RUD	JIX				
VOT	PEL	KAV	JIX			
HES	PEL	SOG				
VOT	RUD	JIX	PEL			
HES	KAV	PEL				
HES	JIX	DUP	PEL			
HES	RUD	JIX	PEL			
HES	JIX	KAV	PEL			
VOT	JIX	DUP	PEL			
VOT	PEL	RUD	JIX			

Experiment 4: Ungrammatical sequences

In Experiment 4 these ungrammatical sequences were intermingled with the grammatical test sequences used in Experiments 2-9.

hes	pel	jix	pel	dup		
hes	jix	dup	pel			
hes	pel	jix	pel	jix	kav	
hes	dup	jix	pel	jix	pel	
hes	kav	pel	jix	pel		
hes	pel	sog	jix			
hes	kav	pel				
hes	kav	jix	pel	jix	pel	jix
hes	dup	pel	jix	pel		
hes	pel	jix	pel	kav		
hes	jix	kav	pel			
hes	pel	rud	pel	jix		
hes	sog	jix				
hes	rud	jix	pel			
hes	pel	dup	jix	pel	jix	
hes	pel	dup	jix			
hes	kav					
hes	jix	rud	jix	pel		
hes	pel	rud	jix			
vot	kav	pel	jix	pel		
vot	jix	kav	jix	pel		
vot	dup					
vot	pel	jix	pel	dup		
vot	rud	jix				
vot	pel	jix	pel	kav		
vot	sog	jix	pel			
vot	rud	jix	pel			
vot	pel	sog	pel	jix		
vot	kav	jix	pel	jix	pel	
vot	dup	pel	jix	pel		
vot	pel	rud				
vot	pel	kav	jix			
vot	jix	pel	dup	jix	pel	jix
vot	sog	jix				
vot	jix	dup	pel			
vot	jix	kav	pel			
vot	pel	rud	jix			
vot	dup	pel				
vot	jix	pel	jix	pel	dup	
vot	pel	kav	jix	pel	jix	

Experiments 5-7: Ungrammatical test sequences

In experiment 6, the elements *pel* and *jix* were replaced with the elements *sab* and *lak* whenever they occurred in the first position of the illegal starters. For example the sequence *pel jix dup vot* would become *sab jix dup vot*. In Experiment 7 the first element of each sequences was replaced with a black mask. For example the sequence *pel jix dup vot* would become ■ *jix dup vot*.

Illegal starters

pel	jix	dup	vot			
pel	jix	sog	hes	jix		
pel	rud	vot				
pel	jix	hes	rud			
pel	sog	hes				
pel	vot	dup				
kav	pel	jix	vot	pel	jix	
kav	jix	pel	jix	vot	pel	jix
kav	jix	pel	jix	hes	pel	
jix	pel	hes	sog			
jix	pel	hes	kav			
jix	pel	hes	dup	pel		
jix	kav	pel	vot	pel		
jix	kav	vot	pel			
jix	dup	pel	vot	pel		
jix	pel	kav	pel	hes		
dup	pel	vot	pel			
dup	jix	pel	jix	vot		
dup	jix	pel	hes	pel	jix	

Legal starters

vot	jix	pel	jix	sog	pel	
vot	sog	jix				
vot	pel	jix	pel	dup		
vot	jix	pel	dup	jix		
hes	kav	jix	pel			
vot	pel	dup	jix			
vot	kav	pel	jix			
hes	kav	pel				
hes	dup	jix	pel			
vot	pel	kav	jix	pel		
hes	jix	pel	rud	pel		
hes	kav	jix				
hes	pel	jix	sog	pel	jix	
hes	pel	dup	jix			
hes	jix	pel	jix	rud	pel	jix
vot	jix	pel	rud	pel	jix	

vot	dup	jix	pel			
hes	jix	kav	jix	pel		
hes	kav	pel	jix	pel		

Experiment 8: Ungrammatical test sequences

vot	jix	pel	jix	sog	pel	
vot	pel	dup	jix			
vot	pel	jix	sog	pel	jix	
hes	kav	jix	pel			
vot	jix	pel	rud	pel		
hes	jix	pel	rud	pel		
vot	kav	jix				
hes	pel	jix	pel	dup		
hes	dup	jix	pel			
vot	kav	pel	jix	pel		
hes	sog	jix				
vot	pel	dup	jix			
vot	kav	pel	jix			
hes	pel	dup	jix			
hes	dup	pel	jix			
hes	pel	dup	jix			
hes	kav	pel				
hes	kav	pel	jix	pel		
hes	kav	pel	jix			
hes	pel	jix	sog	pel	jix	
hes	jix	pel	jix	sog	pel	
hes	jix	pel	jix	rud	pel	jix
hes	kav	jix	pel			
vot	sog	jix				
vot	jix	kav	jix	pel		
vot	jix	pel	jix	rud	pel	jix
vot	dup	jix	pel			
hes	pel	kav	jix	pel		
hes	kav	jix				
vot	dup	jix	pel			
vot	pel	jix	pel	dup		
vot	kav	pel				
vot	jix	pel	dup	jix		
vot	jix	pel	rud	pel	jix	
hes	jix	pel	rud	pel	jix	
hes	jix	kav	jix	pel		
vot	pel	kav	jix	pel		
hes	jix	pel	dup	jix		

<u>Experiment 9: Test sequences</u>

Sequences with an asterisk contain only one violation, an illegal starting element (70%). All other sequences are grammatical (30%).

Low frequency starters

hes	jix	pel	kav	pel		
hes	sog	pel	jix			
hes	jix	rud				
hes	jix	pel	dup	pel		
hes	rud	pel				
vot	pel	dup	pel			
vot	jix	pel	jix	kav	pel	
vot	sog	pel				
vot	jix	sog				
hes	pel	kav	pel	jix		
vot	jix	pel	dup			
hes	jix	sog	pel	jix		
vot	sog	pel	jix			
hes	jix	pel	kav			
vot	jix	pel	jix	dup		
hes	jix	pel	jix	kav	pel	jix
vot	jix	pel	kav			
vot	jix	pel	kav	pel		
vot	pel	jix	kav	pel	jix	
hes	pel	jix	dup	pel	jix	
hes	jix	pel	dup			
vot	jix	pel	dup	pel		

High frequency starters

lak	rud	pel				
sab	pel	kav				
lak	rud	pel	jix			
sab	jix	pel	kav	pel	jix	
lak	jix	pel	jix	kav	pel	jix
sab	jix	pel	jix	kav		
lak	jix	pel	dup			
sab	pel	jix	kav	pel		
lak	jix	pel	kav	pel		
lak	pel	kav				
sab	pel	jix	kav			
sab	jix	rud	pel			
lak	pel	jix	dup			
sab	jix	pel	dup	pel	jix	
lak	jix	pel	jix	kav		
lak	pel	dup	pel	jix		
sab	pel	dup	pel	jix		
lak	jix	rud	pel			

sab	jix	sog	pel			
sab	jix	sog	pel	jix		
sab	sog	pel				
sab	pel	jix	dup	pel	jix	
lak	jix	pel	dup	pel	jix	
sab	jix	pel	kav	pel		
lak	pel	jix	kav	pel		
lak	pel	jix	dup	pel		
sab	jix	rud	pel	jix		
lak	jix	pel	jix	dup	pel	
sab	jix	pel	jix	dup	pel	jix
lak	pel	dup				
sab	pel	jix	dup	pel		
lak	pel	kav	pel	jix		
sab	jix	pel	jix	kav	pel	
sab	rud	pel	jix			
lak	sog	pel	jix			
lak	pel	jix	kav			
sab	jix	sog				
lak	jix	pel	kav			
lak	jix	pel	kav	pel	jix	
sab	jix	pel	dup			
lak	pel	jix	kav	pel	jix	
lak	jix	rud				
lak	jix	pel	jix	dup	pel	jix
sab	jix	pel	dup	pel		
sab	sog	pel	jix			
lak	jix	sog	pel			
sab	jix	pel	jix	dup		
lak	jix	rud	pel	jix		
sab	pel	dup				
lak	pel	dup	pel			
sab	jix	pel	jix	dup	pel	
sab	pel	jix	dup			
lak	jix	pel	dup	pel		
sab	jix	pel	kav			

Experiments 10-13: Grammatical exemplars and test sequences

jix	vot	dup	kav	hes	pel
jix	pel	kav	dup	vot	hes
vot	hes	kav	dup	pel	jix
pel	vot	dup	kav	hes	jix
jix	vot	kav	dup	pel	hes
pel	vot	dup	kav	jix	hes
hes	pel	dup	kav	jix	vot
vot	pel	dup	kav	jix	hes
pel	jix	kav	dup	vot	hes
jix	pel	kav	dup	hes	vot
hes	pel	kav	dup	vot	jix
pel	hes	dup	kav	vot	jix
vot	jix	kav	dup	pel	hes
hes	pel	kav	dup	jix	vot
hes	vot	dup	kav	pel	jix
jix	vot	dup	kav	pel	hes
pel	hes	dup	kav	jix	vot
vot	jix	kav	dup	hes	pel
jix	hes	kav	dup	pel	vot
jix	hes	dup	kav	pel	vot
jix	hes	dup	kav	vot	pel
vot	hes	dup	kav	pel	jix
pel	jix	kav	dup	hes	vot
vot	jix	dup	kav	hes	pel
hes	jix	kav	dup	pel	vot
pel	hes	kav	dup	vot	jix
pel	hes	kav	dup	jix	vot
hes	jix	dup	kav	vot	pel
hes	pel	dup	kav	vot	jix
pel	jix	dup	kav	hes	vot
hes	vot	dup	kav	jix	pel
vot	pel	kav	dup	jix	hes
vot	jix	dup	kav	pel	hes
jix	pel	dup	kav	hes	vot
vot	pel	dup	kav	hes	jix
pel	vot	kav	dup	jix	hes
hes	jix	dup	kav	pel	vot
pel	jix	dup	kav	vot	hes
hes	vot	kav	dup	pel	jix
pel	vot	kav	dup	hes	jix
hes	vot	kav	dup	jix	pel
hes	jix	kav	dup	vot	pel
vot	hes	dup	kav	jix	pel
jix	pel	dup	kav	vot	hes
jix	hes	kav	dup	vot	pel
vot	pel	kav	dup	hes	jix
vot	hes	kav	dup	jix	pel

These sequences were instantiated with symbols and presented in random order as exemplars for Experiments 10-13. They also formed one of the two test sets for Experiments 10-13 but were instantiated with syllables according to the mapping shown in Figure 4.2.

pel	jix	rud	sog	hes	vot
jix	pel	rud	sog	hes	vot
jix	hes	sog	rud	vot	pel
hes	jix	rud	sog	pel	vot
pel	vot	sog	rud	hes	jix
hes	vot	rud	sog	jix	pel
pel	vot	sog	rud	jix	hes
hes	pel	sog	rud	jix	vot
vot	jix	rud	sog	hes	pel
pel	hes	rud	sog	jix	vot
jix	hes	rud	sog	vot	pel
pel	vot	rud	sog	jix	hes
vot	jix	rud	sog	pel	hes
pel	jix	rud	sog	vot	hes
pel	hes	sog	rud	vot	jix
hes	vot	sog	rud	pel	jix
hes	pel	sog	rud	vot	jix
vot	pel	rud	sog	jix	hes
jix	pel	sog	rud	hes	vot
jix	pel	sog	rud	vot	hes
vot	pel	rud	sog	hes	jix
jix	hes	sog	rud	pel	vot
vot	jix	sog	rud	pel	hes
jix	pel	rud	sog	vot	hes
jix	vot	rud	sog	hes	pel
jix	vot	rud	sog	pel	hes
pel	jix	sog	rud	vot	hes
pel	vot	rud	sog	hes	jix
pel	hes	rud	sog	vot	jix
hes	jix	sog	rud	vot	pel
hes	pel	rud	sog	vot	jix
vot	pel	sog	rud	hes	jix
hes	jix	sog	rud	pel	vot
jix	vot	sog	rud	pel	hes
vot	hes	sog	rud	pel	jix
vot	pel	sog	rud	jix	hes
hes	vot	rud	sog	pel	jix
vot	hes	sog	rud	jix	pel
hes	pel	rud	sog	jix	vot
hes	jix	rud	sog	vot	pel
jix	vot	kav	dup	hes	pel
vot	hes	rud	sog	pel	jix
vot	jix	sog	rud	hes	pel
pel	jix	sog	rud	hes	vot
pel	hes	sog	rud	jix	vot
hes	vot	sog	rud	jix	pel
vot	hes	rud	sog	jix	pel
jix	hes	rud	sog	pel	vot
jix	vot	sog	rud	hes	pel

Experiments 10-14: Scrambled exemplars

kav	vot	sog	pel	jix	hes
vot	kav	dup	hes	jix	pel
hes	rud	sog	jix	pel	vot
dup	kav	jix	vot	pel	hes
dup	rud	jix	hes	vot	pel
pel	vot	hes	dup	kav	jix
pel	hes	dup	vot	jix	rud
pel	vot	jix	hes	sog	rud
jix	sog	pel	rud	vot	hes
jix	vot	rud	pel	hes	dup
vot	sog	hes	jix	rud	pel
vot	jix	pel	kav	hes	sog
kav	jix	sog	pel	vot	hes
hes	pel	dup	jix	vot	kav
kav	dup	vot	pel	jix	hes
jix	sog	kav	vot	pel	hes
sog	pel	kav	jix	vot	hes
pel	hes	rud	vot	jix	dup
jix	dup	pel	hes	vot	rud
pel	hes	jix	kav	dup	vot
pel	kav	vot	jix	dup	hes
dup	hes	vot	pel	kav	jix
vot	sog	pel	hes	kav	jix
jix	hes	vot	sog	rud	pel
dup	vot	rud	hes	jix	pel
pel	hes	vot	kav	sog	jix
pel	rud	dup	hes	jix	vot
hes	sog	rud	vot	pel	jix
pel	vot	jix	dup	hes	kav
vot	dup	pel	kav	jix	hes
sog	hes	jix	kav	vot	pel
dup	hes	kav	jix	pel	vot
dup	pel	vot	jix	hes	rud
sog	jix	hes	vot	pel	rud
hes	vot	kav	jix	dup	pel
dup	vot	kav	hes	pel	jix
hes	rud	jix	dup	pel	vot
hes	sog	kav	vot	jix	pel
rud	hes	dup	pel	vot	jix
jix	dup	hes	kav	pel	vot
hes	rud	dup	vot	jix	pel
rud	jix	hes	vot	dup	pel
hes	rud	sog	jix	vot	pel
vot	pel	rud	hes	jix	sog
sog	pel	hes	jix	vot	rud
rud	jix	hes	sog	vot	pel
pel	hes	dup	jix	vot	kav

vot	hes	pel	sog	kav	jix
vot	rud	pel	dup	hes	jix
hes	vot	dup	pel	jix	rud
kav	jix	sog	vot	hes	pel
pel	jix	hes	vot	rud	dup
hes	dup	rud	jix	pel	vot
vot	jix	pel	hes	kav	dup
dup	jix	vot	pel	rud	hes
kav	jix	pel	dup	vot	hes
kav	jix	hes	pel	vot	dup
kav	vot	jix	sog	pel	hes
vot	kav	sog	pel	jix	hes
jix	pel	vot	dup	rud	hes
vot	dup	pel	rud	hes	jix
kav	jix	hes	vot	pel	dup
kav	hes	sog	jix	pel	vot
jix	pel	hes	sog	kav	vot
vot	pel	jix	kav	sog	hes
vot	jix	hes	dup	pel	kav
vot	jix	hes	dup	rud	pel
rud	hes	pel	sog	vot	jix
jix	kav	hes	vot	dup	pel
dup	pel	jix	kav	vot	hes
vot	pel	hes	jix	kav	dup
pel	jix	kav	vot	hes	dup
pel	jix	hes	sog	rud	vot
jix	vot	dup	hes	pel	rud
dup	hes	vot	rud	pel	jix
sog	jix	rud	hes	pel	vot
pel	kav	hes	jix	vot	sog
vot	hes	pel	rud	jix	dup
vot	dup	pel	rud	hes	jix
rud	jix	sog	vot	hes	pel
hes	jix	pel	sog	rud	vot
rud	vot	pel	jix	sog	hes
kav	pel	jix	sog	hes	vot
pel	kav	jix	vot	dup	hes
vot	pel	hes	jix	sog	kav
kav	jix	sog	pel	hes	vot
pel	rud	hes	vot	sog	jix
hes	sog	rud	jix	vot	pel
hes	vot	kav	pel	jix	dup
hes	rud	pel	jix	sog	vot
kav	jix	dup	pel	hes	vot
rud	hes	pel	vot	jix	sog
pel	hes	vot	jix	rud	dup
jix	hes	vot	pel	sog	kav
pel	hes	jix	vot	sog	rud
vot	jix	hes	rud	dup	pel
hes	sog	vot	kav	pel	jix

hes	pel	dup	jix	vot	kav
pel	vot	jix	dup	hes	kav
pel	kav	vot	sog	jix	hes
jix	sog	hes	pel	rud	vot
sog	pel	vot	kav	hes	jix
jix	vot	rud	pel	hes	dup
rud	jix	hes	vot	dup	pel
hes	kav	jix	pel	dup	vot
vot	kav	pel	hes	dup	jix
kav	vot	sog	jix	pel	hes
hes	vot	jix	rud	pel	sog
vot	jix	kav	hes	pel	dup
pel	dup	hes	rud	jix	vot
pel	vot	hes	rud	dup	jix
sog	kav	hes	pel	jix	vot
kav	vot	jix	dup	pel	hes
sog	rud	hes	pel	jix	vot
hes	pel	vot	sog	rud	jix
dup	jix	hes	vot	rud	pel
hes	pel	rud	jix	vot	sog
rud	hes	vot	dup	jix	pel
pel	vot	kav	jix	hes	sog
vot	pel	sog	jix	hes	rud
jix	hes	vot	pel	sog	kav
jix	rud	pel	vot	dup	hes
vot	hes	jix	sog	pel	kav
pel	jix	dup	hes	vot	kav
rud	vot	pel	dup	hes	jix
jix	pel	dup	hes	vot	rud
jix	dup	hes	pel	vot	rud
jix	pel	vot	kav	hes	dup
rud	pel	vot	sog	hes	jix
hes	vot	jix	rud	sog	pel
vot	jix	pel	hes	rud	sog
jix	rud	sog	vot	pel	hes
sog	pel	hes	jix	kav	vot
dup	vot	kav	jix	pel	hes
pel	rud	vot	sog	jix	hes
rud	pel	hes	jix	sog	vot
vot	sog	kav	pel	hes	jix
rud	vot	jix	hes	dup	pel
vot	sog	pel	rud	hes	jix
sog	kav	jix	pel	hes	vot
kav	jix	hes	vot	dup	pel
hes	kav	pel	jix	vot	sog
dup	jix	hes	vot	kav	pel
vot	jix	rud	pel	hes	dup
jix	pel	hes	rud	vot	sog
sog	pel	jix	hes	vot	kav
dup	hes	jix	vot	rud	pel

vot	hes	rud	pel	jix	dup
vot	jix	hes	pel	rud	sog
jix	hes	pel	vot	sog	kav
pel	hes	sog	vot	jix	rud
pel	dup	vot	kav	jix	hes
vot	sog	jix	pel	hes	rud
dup	vot	rud	hes	jix	pel
vot	rud	pel	jix	dup	hes
sog	vot	kav	hes	pel	jix
pel	jix	hes	rud	dup	vot
pel	rud	vot	dup	jix	hes
pel	vot	jix	sog	kav	hes
sog	vot	pel	rud	jix	hes
sog	pel	hes	rud	jix	vot
pel	vot	rud	jix	hes	sog
hes	kav	sog	vot	pel	jix
rud	vot	pel	dup	hes	jix
pel	kav	vot	dup	hes	jix
vot	kav	jix	hes	sog	pel
vot	jix	sog	pel	hes	rud
kav	dup	hes	pel	vot	jix
hes	jix	kav	vot	pel	sog
hes	pel	vot	kav	sog	jix
vot	pel	hes	jix	kav	sog
rud	vot	hes	pel	sog	jix
pel	dup	jix	kav	vot	hes
jix	vot	dup	pel	kav	hes
jix	hes	vot	pel	kav	sog
jix	dup	kav	hes	pel	vot
pel	vot	dup	hes	jix	kav
pel	dup	jix	vot	rud	hes
pel	jix	hes	sog	vot	kav
hes	rud	sog	jix	pel	vot
jix	sog	hes	vot	kav	pel
pel	dup	hes	kav	vot	jix
vot	sog	jix	pel	hes	kav
dup	vot	jix	hes	kav	pel
vot	hes	kav	jix	pel	sog
vot	sog	jix	kav	pel	hes
rud	dup	jix	pel	hes	vot
jix	sog	pel	rud	vot	hes
sog	jix	rud	vot	pel	hes
hes	pel	vot	dup	kav	jix
sog	pel	rud	vot	jix	hes
vot	hes	dup	pel	jix	rud

Experiments 10, 12 & 13: Test sequences with reversed contingencies

hes	pel	sog	dup	jix	vot
pel	jix	dup	sog	vot	hes
hes	vot	sog	dup	jix	pel
hes	jix	dup	sog	vot	pel
pel	vot	dup	sog	jix	hes
hes	pel	sog	dup	vot	jix
vot	jix	dup	sog	hes	pel
hes	jix	sog	dup	vot	pel
jix	pel	sog	dup	hes	vot
vot	hes	dup	sog	pel	jix
pel	vot	dup	sog	hes	jix
pel	jix	sog	dup	vot	hes
jix	hes	dup	sog	vot	pel
jix	vot	sog	dup	hes	pel
hes	vot	dup	sog	jix	pel
pel	hes	dup	sog	vot	jix
vot	hes	sog	dup	jix	pel
vot	pel	sog	dup	jix	hes
jix	pel	dup	sog	vot	hes
hes	pel	dup	sog	jix	vot
vot	pel	dup	sog	jix	hes
hes	vot	dup	sog	pel	jix
hes	jix	sog	dup	pel	vot
jix	pel	dup	sog	hes	vot
jix	pel	sog	dup	vot	hes
vot	jix	sog	dup	pel	hes
hes	vot	sog	dup	pel	jix
pel	hes	sog	dup	vot	jix
vot	pel	dup	sog	hes	jix
pel	jix	sog	dup	hes	vot
pel	vot	sog	dup	jix	hes
vot	hes	dup	sog	jix	pel
pel	vot	sog	dup	hes	jix
vot	jix	dup	sog	pel	hes
jix	hes	sog	dup	vot	pel
jix	vot	dup	sog	hes	pel
jix	vot	sog	dup	pel	hes
vot	pel	sog	dup	hes	jix
jix	hes	dup	sog	pel	vot
pel	jix	dup	sog	hes	vot
jix	hes	sog	dup	pel	vot
hes	jix	dup	sog	pel	vot
vot	jix	sog	dup	hes	pel
pel	hes	dup	sog	jix	vot
vot	hes	sog	dup	pel	jix
jix	vot	dup	sog	pel	hes
pel	hes	sog	dup	jix	vot

These sequences were instantiated with syllables according to the mapping shown in Figure 4.2, and intermingled with the test sequences shown above for Experiments 10, 12 and 13.

hes	pel	dup	sog	vot	jix
pel	hes	rud	kav	jix	vot
hes	vot	kav	rud	jix	pel
vot	hes	rud	kav	jix	pel
jix	pel	kav	rud	vot	hes
hes	pel	kav	rud	vot	jix
hes	pel	rud	kav	vot	jix
hes	vot	rud	kav	jix	pel
pel	jix	kav	rud	vot	hes
vot	hes	rud	kav	pel	jix
jix	vot	rud	kav	hes	pel
jix	hes	rud	kav	pel	vot
pel	vot	kav	rud	jix	hes
pel	vot	rud	kav	jix	hes
hes	jix	kav	rud	pel	vot
hes	jix	rud	kav	vot	pel
vot	pel	kav	rud	hes	jix
vot	pel	rud	kav	jix	hes
pel	jix	rud	kav	vot	hes
vot	jix	rud	kav	pel	hes
jix	hes	kav	rud	pel	vot
pel	vot	rud	kav	hes	jix
jix	pel	kav	rud	hes	vot
jix	vot	kav	rud	pel	hes
pel	vot	kav	rud	hes	jix
pel	hes	kav	rud	jix	vot
vot	pel	kav	rud	jix	hes
jix	pel	rud	kav	hes	vot
jix	vot	kav	rud	hes	pel
vot	jix	kav	rud	pel	hes
vot	pel	rud	kav	hes	jix
hes	pel	rud	kav	jix	vot
jix	hes	rud	kav	vot	pel
pel	hes	kav	rud	vot	jix
hes	pel	kav	rud	jix	vot
pel	jix	kav	rud	hes	vot
hes	vot	kav	rud	pel	jix
vot	jix	rud	kav	hes	pel
hes	jix	kav	rud	vot	pel
vot	hes	kav	rud	pel	jix
vot	jix	kav	rud	hes	pel
jix	vot	rud	kav	pel	hes
pel	hes	rud	kav	vot	jix
vot	hes	kav	rud	jix	pel
hes	jix	rud	kav	pel	vot
jix	hes	kav	rud	vot	pel
hes	vot	rud	kav	pel	jix
pel	jix	rud	kav	hes	vot
jix	pel	rud	kav	vot	hes

Experiment 11: Ungrammatical test sequences

pel	hes	rud	jix	kav	vot
hes	vot	kav	jix	rud	pel
vot	hes	rud	jix	kav	pel
jix	pel	kav	vot	rud	hes
hes	pel	kav	vot	rud	jix
hes	pel	rud	vot	kav	jix
hes	vot	rud	jix	kav	pel
pel	jix	kav	vot	rud	hes
vot	hes	rud	pel	kav	jix
jix	vot	rud	hes	kav	pel
jix	hes	rud	pel	kav	vot
pel	vot	kav	jix	rud	hes
pel	vot	rud	jix	kav	hes
hes	jix	kav	pel	rud	vot
hes	jix	rud	vot	kav	pel
vot	pel	kav	hes	rud	jix
vot	pel	rud	jix	kav	hes
pel	jix	rud	vot	kav	hes
vot	jix	rud	pel	kav	hes
jix	hes	kav	pel	rud	vot
pel	vot	rud	hes	kav	jix
jix	pel	kav	hes	rud	vot
jix	vot	kav	pel	rud	hes
pel	vot	kav	hes	rud	jix
pel	hes	kav	jix	rud	vot
vot	pel	kav	jix	rud	hes
jix	pel	rud	hes	kav	vot
jix	vot	kav	hes	rud	pel
vot	jix	kav	pel	rud	hes
vot	pel	rud	hes	kav	jix
hes	pel	rud	jix	kav	vot
jix	hes	rud	vot	kav	pel
pel	hes	kav	vot	rud	jix
hes	pel	kav	jix	rud	vot
pel	jix	kav	hes	rud	vot
hes	vot	kav	pel	rud	jix
vot	jix	rud	hes	kav	pel
hes	jix	kav	vot	rud	pel
vot	hes	kav	pel	rud	jix
vot	jix	kav	hes	rud	pel
jix	vot	rud	pel	kav	hes
pel	hes	rud	vot	kav	jix
vot	hes	kav	jix	rud	pel
hes	jix	rud	pel	kav	vot
jix	hes	kav	vot	rud	pel
hes	vot	rud	pel	kav	jix
pel	jix	rud	hes	kav	vot

These sequences were instantiated with syllables according to the mapping shown in Figure 4.2, and intermingled with the same test sequences shown above for Experiments 10-13.

jix	pel	rud	vot	kav	hes
hes	pel	sog	jix	dup	vot
pel	jix	dup	vot	sog	hes
hes	vot	sog	jix	dup	pel
hes	jix	dup	vot	sog	pel
pel	vot	dup	jix	sog	hes
hes	pel	sog	vot	dup	jix
vot	jix	dup	hes	sog	pel
hes	jix	sog	vot	dup	pel
jix	pel	sog	hes	dup	vot
vot	hes	dup	pel	sog	jix
pel	vot	dup	hes	sog	jix
pel	jix	sog	vot	dup	hes
jix	hes	dup	vot	sog	pel
jix	vot	sog	hes	dup	pel
hes	vot	dup	jix	sog	pel
pel	hes	dup	vot	sog	jix
vot	hes	sog	jix	dup	pel
vot	pel	sog	jix	dup	hes
jix	pel	dup	vot	sog	hes
hes	pel	dup	jix	sog	vot
vot	pel	dup	jix	sog	hes
hes	vot	dup	pel	sog	jix
hes	jix	sog	pel	dup	vot
jix	pel	dup	hes	sog	vot
jix	pel	sog	vot	dup	hes
vot	jix	sog	pel	dup	hes
hes	vot	sog	pel	dup	jix
pel	hes	sog	vot	dup	jix
vot	pel	dup	hes	sog	jix
pel	jix	sog	hes	dup	vot
pel	vot	sog	jix	dup	hes
vot	hes	dup	jix	sog	pel
pel	vot	sog	hes	dup	jix
vot	jix	dup	pel	sog	hes
jix	hes	sog	vot	dup	pel
jix	vot	dup	hes	sog	pel
jix	vot	sog	pel	dup	hes
vot	pel	sog	hes	dup	jix
jix	hes	dup	pel	sog	vot
pel	jix	dup	hes	sog	vot
jix	hes	sog	pel	dup	vot
hes	jix	dup	pel	sog	vot
vot	jix	sog	hes	dup	pel
pel	hes	dup	jix	sog	vot
vot	hes	sog	pel	dup	jix
jix	vot	dup	pel	sog	hes
pel	hes	sog	jix	dup	vot
hes	pel	dup	vot	sog	jix

Experiment 14: No-noise grammatical exemplars

kav	dup	vot	jix	hes	pel
kav	dup	pel	hes	jix	vot
kav	dup	jix	pel	vot	hes
pel	jix	hes	vot	rud	sog
hes	vot	jix	pel	rud	sog
kav	dup	vot	pel	jix	hes
kav	dup	jix	vot	hes	pel
kav	dup	vot	jix	hes	pel
kav	dup	pel	jix	vot	hes
jix	pel	rud	sog	vot	hes
hes	jix	rud	sog	pel	vot
kav	dup	vot	pel	jix	hes
hes	jix	vot	pel	rud	sog
kav	dup	pel	jix	hes	vot
vot	jix	hes	pel	rud	sog
hes	vot	pel	jix	rud	sog
kav	dup	jix	hes	pel	vot
hes	pel	jix	vot	rud	sog
vot	jix	kav	dup	hes	pel
pel	jix	rud	sog	vot	hes
hes	jix	vot	pel	rud	sog
hes	jix	pel	vot	rud	sog
vot	pel	kav	dup	hes	jix
kav	dup	hes	jix	pel	vot
vot	jix	kav	dup	hes	pel
pel	jix	kav	dup	hes	vot
pel	jix	rud	sog	vot	hes
kav	dup	pel	vot	jix	hes
kav	dup	pel	jix	hes	vot
kav	dup	jix	vot	hes	pel
pel	hes	rud	sog	vot	jix
jix	vot	hes	pel	rud	sog
hes	jix	kav	dup	vot	pel
pel	hes	vot	jix	rud	sog
jix	vot	rud	sog	hes	pel
jix	hes	rud	sog	vot	pel
vot	hes	rud	sog	pel	jix
vot	hes	kav	dup	jix	pel
vot	pel	kav	dup	hes	jix
kav	dup	pel	vot	jix	hes
hes	jix	kav	dup	vot	pel
hes	vot	rud	sog	jix	pel
pel	hes	kav	dup	jix	vot
kav	dup	hes	jix	pel	vot
jix	hes	kav	dup	vot	pel
pel	hes	vot	jix	rud	sog
hes	vot	rud	sog	jix	pel

kav	dup	jix	hes	pel	vot
jix	vot	kav	dup	hes	pel
pel	vot	rud	sog	jix	hes
pel	vot	rud	sog	jix	hes
vot	pel	jix	hes	rud	sog
jix	hes	kav	dup	vot	pel
vot	hes	rud	sog	pel	jix
jix	pel	rud	sog	hes	vot
hes	vot	pel	jix	rud	sog
kav	dup	pel	jix	vot	hes
pel	hes	kav	dup	jix	vot
hes	vot	rud	sog	pel	jix
jix	hes	rud	sog	vot	pel
vot	hes	rud	sog	jix	pel
hes	vot	kav	dup	jix	pel
kav	dup	vot	hes	pel	jix
jix	hes	vot	pel	rud	sog
jix	pel	rud	sog	vot	hes
pel	jix	kav	dup	hes	vot
hes	vot	rud	sog	pel	jix
jix	pel	rud	sog	hes	vot
pel	jix	hes	vot	rud	sog
jix	hes	kav	dup	pel	vot
vot	jix	hes	pel	rud	sog
jix	vot	kav	dup	pel	hes
hes	pel	kav	dup	vot	jix
jix	vot	kav	dup	hes	pel
kav	dup	pel	hes	jix	vot
kav	dup	vot	hes	pel	jix
hes	pel	kav	dup	vot	jix
vot	hes	jix	pel	rud	sog
vot	hes	kav	dup	jix	pel
jix	vot	hes	pel	rud	sog
kav	dup	hes	vot	pel	jix
jix	vot	kav	dup	pel	hes
kav	dup	hes	vot	pel	jix
hes	pel	jix	vot	rud	sog
vot	hes	jix	pel	rud	sog
pel	hes	rud	sog	vot	jix
vot	pel	jix	hes	rud	sog
hes	vot	jix	pel	rud	sog
kav	dup	jix	pel	vot	hes
jix	hes	vot	pel	rud	sog
hes	jix	rud	sog	pel	vot
hes	jix	pel	vot	rud	sog
jix	vot	rud	sog	hes	pel
hes	vot	kav	dup	jix	pel
vot	hes	rud	sog	jix	pel
jix	hes	kav	dup	pel	vot

Experiment 14: Noisy exemplars

hes	jix	kav	vot	pel	sog
kav	vot	pel	jix	hes	sog
pel	hes	kav	jix	vot	sog
jix	dup	hes	vot	rud	pel
jix	vot	pel	dup	rud	hes
hes	dup	jix	vot	rud	pel
kav	hes	pel	sog	vot	jix
vot	pel	kav	sog	hes	jix
kav	hes	vot	sog	pel	jix
pel	vot	rud	dup	hes	jix
jix	dup	rud	hes	pel	vot
hes	pel	rud	dup	vot	jix
pel	vot	kav	hes	jix	sog
pel	vot	kav	sog	jix	hes
jix	hes	kav	vot	pel	sog
vot	jix	rud	dup	pel	hes
vot	jix	rud	dup	hes	pel
jix	pel	vot	dup	rud	hes
pel	dup	rud	hes	jix	vot
jix	dup	vot	hes	rud	vot
hes	dup	pel	vot	rud	jix
kav	pel	vot	jix	hes	sog
kav	hes	pel	sog	vot	jix
kav	jix	hes	pel	vot	sog

Experiments 15-17: Noisy exemplars

dup	vot	hes	jix	pel	sog
dup	vot	jix	hes	rud	pel
dup	jix	rud	pel	vot	hes
dup	pel	jix	hes	vot	sog
dup	jix	pel	hes	rud	vot
dup	pel	rud	vot	jix	hes
rud	jix	hes	pel	vot	kav
hes	pel	rud	jix	vot	kav
jix	vot	hes	sog	pel	kav
rud	pel	hes	vot	jix	kav
vot	jix	rud	pel	hes	kav
vot	pel	hes	sog	jix	kav
sog	pel	hes	jix	vot	dup
sog	vot	pel	dup	jix	hes
sog	jix	kav	vot	pel	hes
sog	jix	hes	vot	pel	dup
sog	hes	vot	dup	pel	jix
sog	pel	kav	vot	hes	jix

kav	pel	jix	vot	hes	rud
vot	dup	hes	pel	jix	rud
hes	vot	jix	dup	pel	rud
kav	hes	pel	vot	jix	rud
hes	dup	jix	pel	vot	rud
hes	jix	vot	dup	pel	rud

Experiment 14 & 16: Grammatical test sequences

pel	vot	kav	dup	jix	hes
vot	jix	pel	hes	rud	sog
kav	dup	hes	jix	vot	pel
kav	dup	vot	hes	jix	pel
jix	pel	kav	dup	hes	vot
kav	dup	pel	hes	vot	jix
vot	hes	kav	dup	pel	jix
kav	dup	jix	hes	vot	pel
jix	pel	kav	dup	vot	hes
vot	pel	hes	jix	rud	sog
kav	dup	jix	vot	pel	hes
hes	pel	kav	dup	jix	vot
vot	pel	rud	sog	jix	hes
vot	pel	rud	sog	hes	jix
hes	jix	rud	sog	vot	pel
kav	dup	hes	vot	jix	pel
kav	dup	vot	pel	hes	jix
pel	vot	jix	hes	rud	sog
vot	jix	rud	sog	hes	pel
hes	vot	kav	dup	pel	jix
hes	pel	rud	sog	vot	jix
jix	pel	vot	hes	rud	sog
pel	vot	kav	dup	hes	jix
vot	jix	kav	dup	pel	hes
kav	dup	hes	pel	jix	vot
kav	dup	hes	pel	vot	jix
jix	hes	rud	sog	pel	vot
pel	jix	rud	sog	hes	vot
pel	vot	rud	sog	hes	jix
jix	hes	pel	vot	rud	sog
kav	dup	jix	pel	hes	vot
hes	pel	vot	jix	rud	sog
vot	pel	kav	dup	jix	hes
jix	pel	hes	vot	rud	sog
hes	jix	kav	dup	pel	vot
pel	jix	vot	hes	rud	sog
jix	vot	rud	sog	pel	hes
hes	pel	rud	sog	jix	vot
kav	dup	pel	vot	hes	jix

vot	hes	pel	jix	rud	sog
vot	jix	rud	sog	pel	hes
pel	hes	jix	vot	rud	sog
pel	jix	kav	dup	vot	hes
jix	vot	pel	hes	rud	sog
kav	dup	vot	jix	pel	hes
pel	hes	rud	sog	jix	vot
pel	hes	kav	dup	vot	jix
pel	vot	hes	jix	rud	sog

Experiment 14 & 16: Ungrammatical test sequences

hes	pel	kav	jix	rud	vot
kav	jix	rud	pel	vot	hes
pel	vot	kav	jix	rud	hes
hes	dup	vot	sog	jix	pel
jix	pel	kav	vot	rud	hes
kav	vot	rud	pel	hes	jix
jix	dup	pel	sog	hes	vot
vot	hes	jix	dup	pel	sog
jix	dup	pel	sog	vot	hes
kav	jix	rud	hes	pel	vot
kav	pel	rud	hes	vot	jix
jix	vot	hes	dup	pel	sog
kav	vot	rud	hes	pel	jix
jix	dup	vot	sog	hes	pel
vot	jix	hes	dup	pel	sog
kav	vot	rud	pel	jix	hes
hes	jix	kav	vot	rud	pel
jix	hes	kav	pel	rud	vot
jix	pel	vot	dup	hes	sog
jix	vot	kav	pel	rud	hes
pel	hes	kav	jix	rud	vot
pel	hes	jix	dup	vot	sog
kav	hes	rud	jix	vot	pel
vot	dup	hes	sog	jix	pel
vot	pel	jix	dup	hes	sog
pel	vot	hes	dup	jix	sog
jix	dup	hes	sog	pel	vot
kav	hes	rud	pel	jix	vot
jix	vot	kav	hes	rud	pel
kav	hes	rud	vot	jix	pel
kav	pel	rud	vot	jix	hes
pel	hes	kav	vot	rud	jix
pel	dup	jix	sog	vot	hes
pel	hes	vot	dup	jix	sog
hes	dup	jix	sog	pel	vot
pel	jix	hes	dup	vot	sog

kav	jix	rud	hes	vot	pel
hes	dup	jix	sog	vot	pel
vot	hes	pel	dup	jix	sog
vot	jix	kav	pel	rud	hes
pel	dup	vot	sog	jix	hes
pel	jix	kav	vot	rud	hes
vot	dup	jix	sog	pel	hes
jix	dup	vot	sog	pel	hes
pel	jix	vot	dup	hes	sog
kav	vot	rud	jix	hes	pel
vot	pel	hes	dup	jix	sog
hes	vot	kav	jix	rud	pel

<u>Experiment 15-17: No-noise exemplars</u>

rud	pel	rud	sog	vot	hes
sog	vot	kav	dup	pel	dup
kav	dup	vot	dup	jix	rud
dup	jix	rud	hes	rud	sog
pel	jix	rud	hes	rud	sog
dup	vot	rud	hes	rud	sog
kav	dup	jix	dup	vot	hes
vot	jix	rud	hes	rud	sog
hes	jix	kav	dup	pel	dup
sog	pel	kav	dup	vot	dup
kav	dup	hes	dup	pel	rud
dup	jix	rud	vot	rud	sog
hes	pel	kav	dup	vot	dup
kav	dup	pel	dup	vot	hes
sog	jix	kav	dup	vot	dup
hes	pel	kav	dup	jix	dup
dup	hes	rud	pel	rud	sog
kav	dup	hes	dup	pel	jix
sog	jix	kav	dup	hes	dup
rud	jix	rud	sog	vot	hes
pel	hes	rud	vot	rud	sog
sog	vot	kav	dup	jix	dup
vot	jix	kav	dup	hes	dup
rud	hes	rud	sog	jix	kav
kav	dup	jix	dup	vot	rud
kav	dup	pel	dup	hes	vot
dup	pel	rud	jix	rud	sog
kav	dup	pel	dup	jix	rud
vot	pel	kav	dup	jix	dup
hes	pel	rud	vot	rud	sog
rud	jix	rud	sog	vot	kav
kav	dup	vot	dup	pel	jix
hes	vot	kav	dup	pel	dup

rud	hes	rud	sog	vot	pel
jix	vot	rud	hes	rud	sog
rud	pel	rud	sog	jix	hes
kav	dup	pel	dup	vot	rud
sog	hes	kav	dup	pel	dup
kav	dup	hes	dup	vot	rud
rud	pel	rud	sog	jix	kav
rud	pel	rud	sog	hes	vot
kav	dup	vot	dup	pel	rud
sog	pel	kav	dup	hes	dup
kav	dup	hes	dup	jix	rud
rud	pel	rud	sog	hes	kav
dup	hes	rud	vot	rud	sog
kav	dup	pel	dup	jix	hes
jix	hes	kav	dup	pel	dup
kav	dup	pel	dup	hes	rud
hes	jix	rud	pel	rud	sog
pel	vot	kav	dup	hes	dup
kav	dup	vot	dup	hes	rud
rud	hes	rud	sog	vot	kav
kav	dup	hes	dup	vot	pel
sog	hes	kav	dup	jix	dup
dup	pel	rud	hes	rud	sog
rud	pel	rud	sog	vot	jix
hes	vot	rud	pel	rud	sog
rud	hes	rud	sog	pel	jix
rud	jix	rud	sog	hes	kav
sog	hes	kav	dup	vot	dup
vot	pel	rud	jix	rud	sog
rud	vot	rud	sog	jix	hes
rud	pel	rud	sog	vot	kav
kav	dup	jix	dup	pel	rud
kav	dup	hes	dup	jix	pel
jix	vot	kav	dup	hes	dup
rud	hes	rud	sog	pel	vot
rud	vot	rud	sog	pel	jix
dup	hes	rud	jix	rud	sog
dup	vot	rud	jix	rud	sog
rud	vot	rud	sog	pel	kav
kav	dup	vot	dup	jix	hes
sog	vot	kav	dup	hes	dup
dup	jix	rud	pel	rud	sog
kav	dup	pel	dup	vot	jix
rud	jix	rud	sog	hes	pel
pel	vot	kav	dup	jix	dup
hes	pel	rud	jix	rud	sog
kav	dup	hes	dup	pel	vot
sog	jix	kav	dup	pel	dup
rud	vot	rud	sog	jix	kav
pel	vot	rud	hes	rud	sog

rud	vot	rud	sog	hes	kav
rud	hes	rud	sog	pel	kav
pel	hes	kav	dup	vot	dup
jix	hes	rud	pel	rud	sog
rud	jix	rud	sog	pel	kav
dup	pel	rud	vot	rud	sog
kav	dup	jix	dup	hes	pel
sog	pel	kav	dup	jix	dup
pel	vot	rud	jix	rud	sog
rud	hes	rud	sog	jix	pel
dup	vot	rud	pel	rud	sog
pel	jix	kav	dup	hes	dup
kav	dup	jix	dup	hes	rud

<u>Experiments 15-17: Scrambled exemplars</u>

dup	pel	vot	rud	hes	rud
dup	jix	rud	vot	pel	rud
hes	rud	pel	dup	rud	vot
vot	rud	hes	dup	jix	rud
rud	hes	rud	rud	vot	dup
jix	rud	rud	vot	pel	dup
hes	jix	rud	dup	pel	rud
rud	jix	pel	dup	vot	rud
rud	jix	dup	hes	vot	rud
rud	hes	dup	jix	rud	vot
rud	rud	hes	jix	dup	pel
jix	rud	pel	rud	dup	vot
rud	dup	pel	vot	rud	jix
jix	dup	rud	pel	rud	vot
jix	rud	vot	rud	pel	dup
rud	vot	jix	rud	hes	dup
kav	kav	vot	jix	hes	sog
kav	jix	kav	vot	pel	sog
hes	sog	pel	kav	kav	vot
vot	sog	hes	kav	jix	kav
pel	hes	sog	kav	vot	kav
jix	kav	sog	vot	pel	kav
hes	jix	kav	kav	pel	sog
kav	jix	pel	kav	vot	sog
sog	jix	kav	hes	kav	pel
sog	hes	kav	jix	pel	kav
vot	sog	hes	kav	kav	pel
jix	sog	kav	hes	kav	vot
kav	kav	pel	vot	sog	jix
jix	kav	hes	kav	sog	vot
jix	hes	vot	sog	kav	kav
pel	vot	kav	sog	hes	kav

rud	pel	vot	rud	hes	kav
rud	jix	hes	vot	rud	kav
rud	kav	pel	rud	jix	vot
vot	kav	hes	rud	jix	rud
pel	rud	kav	jix	vot	rud
rud	hes	kav	vot	pel	rud
hes	jix	vot	rud	rud	kav
rud	jix	pel	rud	vot	kav
kav	jix	rud	hes	rud	pel
kav	hes	rud	jix	pel	rud
vot	kav	hes	jix	rud	pel
jix	kav	rud	hes	rud	vot
hes	rud	pel	vot	kav	jix
jix	rud	hes	pel	kav	rud
rud	hes	vot	kav	pel	rud
pel	rud	jix	kav	hes	rud
sog	pel	sog	jix	hes	rud
sog	rud	hes	vot	pel	sog
hes	sog	pel	sog	rud	vot
vot	rud	hes	sog	jix	sog
pel	rud	sog	jix	vot	sog
rud	hes	sog	vot	pel	sog
hes	jix	vot	sog	rud	sog
hes	rud	pel	sog	vot	sog
sog	jix	sog	hes	rud	pel
rud	hes	sog	jix	pel	sog
vot	sog	rud	jix	sog	pel
sog	rud	pel	hes	sog	vot
rud	sog	pel	vot	sog	jix
jix	sog	sog	pel	rud	vot
rud	hes	vot	sog	pel	sog
pel	sog	jix	rud	hes	sog
dup	dup	vot	jix	hes	kav
dup	jix	dup	vot	pel	kav
hes	kav	pel	dup	dup	vot
vot	kav	hes	dup	jix	dup
pel	hes	kav	dup	vot	dup
dup	hes	kav	vot	pel	dup
hes	dup	vot	dup	pel	kav
hes	jix	dup	dup	vot	kav
kav	jix	dup	dup	vot	pel
kav	hes	dup	jix	dup	vot
vot	kav	hes	jix	dup	dup
jix	kav	pel	dup	dup	vot
hes	dup	dup	vot	kav	jix
dup	dup	hes	pel	kav	vot
jix	dup	vot	kav	pel	dup
pel	vot	jix	kav	dup	dup
sog	dup	vot	jix	hes	dup
sog	jix	hes	dup	pel	dup

hes	dup	pel	sog	dup	vot
vot	dup	dup	sog	jix	pel
pel	hes	dup	jix	dup	sog
dup	hes	dup	vot	pel	sog
hes	jix	vot	sog	dup	dup
dup	jix	pel	sog	vot	dup
dup	jix	sog	hes	vot	dup
dup	hes	sog	jix	dup	vot
vot	dup	hes	jix	sog	dup
jix	dup	pel	dup	sog	vot
hes	sog	dup	vot	dup	jix
dup	sog	hes	pel	dup	vot
jix	dup	vot	dup	pel	sog
pel	vot	dup	dup	hes	sog

Experiments 15-17: Grammatical test sequences

vot	hes	rud	pel	rud	sog
vot	pel	rud	hes	rud	sog
vot	jix	rud	pel	rud	sog
jix	pel	rud	vot	rud	sog
hes	jix	rud	vot	rud	sog
jix	vot	rud	pel	rud	sog
vot	hes	rud	jix	rud	sog
pel	vot	rud	hes	rud	sog
pel	hes	rud	jix	rud	sog
hes	vot	rud	pel	rud	sog
pel	jix	rud	hes	rud	sog
pel	jix	rud	vot	rud	sog
rud	pel	rud	sog	vot	hes
rud	jix	rud	sog	pel	vot
rud	vot	rud	sog	hes	jix
rud	jix	rud	sog	vot	pel
rud	hes	rud	sog	vot	pel
rud	vot	rud	sog	pel	hes
rud	pel	rud	sog	jix	vot
rud	vot	rud	sog	jix	pel
rud	vot	rud	sog	hes	pel
rud	hes	rud	sog	jix	vot
rud	pel	rud	sog	hes	jix
rud	pel	rud	sog	jix	hes
hes	vot	kav	dup	pel	dup
vot	jix	kav	dup	pel	dup
pel	jix	kav	dup	hes	dup
vot	hes	kav	dup	jix	dup
vot	pel	kav	dup	hes	dup
pel	hes	kav	dup	jix	dup
jix	vot	kav	dup	pel	dup

hes	jix	kav	dup	vot	dup
jix	pel	kav	dup	vot	dup
pel	vot	kav	dup	hes	dup
vot	hes	kav	dup	pel	dup
pel	jix	kav	dup	vot	dup
kav	dup	hes	dup	vot	pel
kav	dup	pel	dup	jix	hes
kav	dup	pel	dup	hes	jix
kav	dup	pel	dup	jix	vot
kav	dup	hes	dup	jix	vot
kav	dup	vot	dup	hes	jix
kav	dup	jix	dup	pel	vot
kav	dup	pel	dup	vot	hes
kav	dup	jix	dup	vot	pel
kav	dup	vot	dup	pel	hes
kav	dup	vot	dup	jix	pel
kav	dup	vot	dup	hes	pel

Experiment 15: Ungrammatical sequences (illegal bigrams with repeats)

sog	vot	rud	hes	rud	jix
sog	jix	rud	pel	rud	vot
sog	pel	rud	vot	rud	hes
sog	vot	rud	hes	rud	pel
sog	vot	rud	jix	rud	pel
sog	jix	rud	vot	rud	pel
sog	pel	rud	hes	rud	jix
sog	vot	rud	pel	rud	hes
sog	pel	rud	jix	rud	vot
sog	hes	rud	jix	rud	vot
sog	hes	rud	vot	rud	pel
sog	pel	rud	jix	rud	hes
rud	vot	rud	jix	pel	sog
rud	pel	rud	jix	vot	sog
rud	pel	rud	vot	hes	sog
rud	jix	rud	pel	vot	sog
rud	pel	rud	hes	jix	sog
rud	hes	rud	jix	vot	sog
rud	jix	rud	vot	pel	sog
rud	hes	rud	vot	pel	sog
rud	vot	rud	pel	hes	sog
rud	vot	rud	hes	jix	sog
rud	vot	rud	hes	pel	sog
rud	pel	rud	jix	hes	sog
kav	vot	pel	dup	hes	dup
kav	pel	hes	dup	jix	dup
kav	hes	vot	dup	pel	dup
kav	vot	hes	dup	jix	dup

kav	pel	jix	dup	vot	dup
kav	jix	vot	dup	pel	dup
kav	pel	jix	dup	hes	dup
kav	pel	vot	dup	hes	dup
kav	hes	jix	dup	vot	dup
kav	jix	pel	dup	vot	dup
kav	vot	jix	dup	pel	dup
kav	vot	hes	dup	pel	dup
jix	dup	pel	dup	vot	kav
hes	dup	vot	dup	pel	kav
hes	dup	jix	dup	vot	kav
pel	dup	hes	dup	jix	kav
vot	dup	pel	dup	hes	kav
vot	dup	hes	dup	pel	kav
vot	dup	jix	dup	pel	kav
pel	dup	jix	dup	vot	kav
pel	dup	jix	dup	hes	kav
jix	dup	vot	dup	pel	kav
vot	dup	hes	dup	jix	kav
pel	dup	vot	dup	hes	kav

<u>Experiment 17: Ungrammatical sequences (illegal repeats)</u>

rud	vot	hes	jix	rud	sog
rud	jix	pel	vot	rud	sog
rud	pel	vot	hes	rud	sog
rud	vot	hes	pel	rud	sog
rud	vot	jix	pel	rud	sog
rud	jix	vot	pel	rud	sog
rud	pel	hes	jix	rud	sog
rud	vot	pel	hes	rud	sog
rud	pel	jix	vot	rud	sog
rud	hes	jix	vot	rud	sog
rud	hes	vot	pel	rud	sog
rud	pel	jix	hes	rud	sog
vot	jix	rud	sog	pel	rud
pel	jix	rud	sog	vot	rud
pel	vot	rud	sog	hes	rud
jix	pel	rud	sog	vot	rud
pel	hes	rud	sog	jix	rud
hes	jix	rud	sog	vot	rud
jix	vot	rud	sog	pel	rud
hes	vot	rud	sog	pel	rud
vot	pel	rud	sog	hes	rud
vot	hes	rud	sog	jix	rud
vot	hes	rud	sog	pel	rud
pel	jix	rud	sog	hes	rud
dup	vot	kav	dup	pel	hes

dup	pel	kav	dup	hes	jix
dup	hes	kav	dup	vot	pel
dup	vot	kav	dup	hes	jix
dup	pel	kav	dup	jix	vot
dup	jix	kav	dup	vot	pel
dup	pel	kav	dup	jix	hes
dup	pel	kav	dup	vot	hes
dup	hes	kav	dup	jix	vot
dup	jix	kav	dup	pel	vot
dup	vot	kav	dup	jix	pel
dup	vot	kav	dup	hes	pel
kav	dup	jix	pel	vot	dup
kav	dup	hes	vot	pel	dup
kav	dup	hes	jix	vot	dup
kav	dup	pel	hes	jix	dup
kav	dup	vot	pel	hes	dup
kav	dup	vot	hes	pel	dup
kav	dup	vot	jix	pel	dup
kav	dup	pel	jix	vot	dup
kav	dup	pel	jix	hes	dup
kav	dup	jix	vot	pel	dup
kav	dup	vot	hes	jix	dup
kav	dup	pel	vot	hes	dup

DIRECT TESTS USED IN CHAPTER 4

We are interested in what you might have learned during this experiment. For these questions reflect for a moment upon what you might know or recall about the different types of sequence. Try and answer the questions as you would have during the experiment, rather than what you might think now.

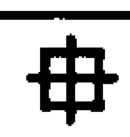
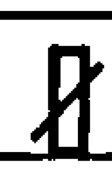
1. You may have noticed that some symbols tended to occur together. Please tick the boxes below to indicate which symbols you think went together with which others. For example, if you thought that ᚱ≋ tended to occur, in that order, then tick the box marked 1 below. If you thought they went together but in the reverse order (≋ᚱ), then tick the box marked 2 below.

	⌗	ᚱ	⬠	≋	◎	Ⱥ	◬	⊂⊃
⬠								
ᚱ				2				
◎								
≋		1						
◬								
Ⱥ								
⌗								
⊂⊃								

2. Which **syllables** do you think form pairs? As with the symbols, the syllables may have occurred in different orders. The syllables along the top happen first, and those along the side follow. Place a tick in the appropriate box below.

	hes	sog	kav	pel	vot	dup	jix	rud
hes								
kav								
rud								
dup								
sog								
vot								
pel								
jix								

3. Which **syllables** do you think correspond to which **symbols**? Place a tick in the appropriate box.

				≈	◎			⊂⊃
hes								
vot								
kav								
dup								
sog								
rud								
pel								
jix								

APPENDIX B

DISTRIBUTIONS OBSERVED IN CHAPTER 4

FREQUENCY DISTRIBUTIONS FOR EXPERIMENT 10

Percent Correct	Frequency	Percent	Valid Percent	Cumulative Percent
51.04	3	30.0	30.0	30.0
52.08	1	10.0	10.0	40.0
53.65	2	20.0	20.0	60.0
89.58	1	10.0	10.0	70.0
97.40	1	10.0	10.0	80.0
99.48	2	20.0	20.0	100.0
Total	10	100.0	100.0	

Experiment 10: Distribution of Experimental group classification scores.

Percent Correct	Frequency	Percent	Valid Percent	Cumulative Percent
50.00	2	20.0	20.0	20.0
50.52	1	10.0	10.0	30.0
51.56	4	40.0	40.0	70.0
52.08	1	10.0	10.0	80.0
54.69	1	10.0	10.0	90.0
55.21	1	10.0	10.0	100.0
Total	10	100.0	100.0	

Experiment 10: Distribution of Control group classification scores.

FREQUENCY DISTRIBUTIONS FOR EXPERIMENT 11

Percent Correct	Frequency	Percent	Valid Percent	Cumulative Percent
36.46	1	10.0	10.0	10.0
45.83	1	10.0	10.0	20.0
46.35	1	10.0	10.0	30.0
47.40	1	10.0	10.0	40.0
48.44	1	10.0	10.0	50.0
49.48	1	10.0	10.0	60.0
52.08	1	10.0	10.0	70.0
53.65	1	10.0	10.0	80.0
82.29	1	10.0	10.0	90.0
100.00	1	10.0	10.0	100.0
Total	10	100.0	100.0	

Experiment 11: Distribution of Experimental group classification scores.

Percent Correct	Frequency	Percent	Valid Percent	Cumulative Percent
19.79	1	10.0	10.0	10.0
39.58	1	10.0	10.0	20.0
43.75	1	10.0	10.0	30.0
44.79	1	10.0	10.0	40.0
46.35	1	10.0	10.0	50.0
47.92	1	10.0	10.0	60.0
48.44	2	20.0	20.0	80.0
53.65	1	10.0	10.0	90.0
55.73	1	10.0	10.0	100.0
Total	10	100.0	100.0	

Experiment 11: Distribution of Control group classification scores.

FREQUENCY DISTRIBUTIONS FOR EXPERIMENT 12

direct test scores*			Percent Correct	Frequency	Percent	Valid Percent	Cumulative Percent
1	2	3					
0	0	0	51.04	2	16.7	16.7	16.7
0	0	4	52.60	1	8.3	8.3	25.0
2	2	2	53.13	1	8.3	8.3	33.3
3	0	1	53.65	1	8.3	8.3	41.7
0	2	2	54.69	1	8.3	8.3	50.0
0	2	0	57.29	1	8.3	8.3	58.3
4	4	4	96.35	1	8.3	8.3	66.7
4	4	4	98.44	1	8.3	8.3	75.0
4	4	4	98.96	1	8.3	8.3	83.3
4	4	4	100.00	2	16.7	16.7	100.0
			Total	12	100.0	100.0	

*Mean direct test scores per cell. Test 1 assessed memory for bigrams seen during training. Test 2 assessed knowledge of bigrams during testing. Test 3 assessed knowledge of the mapping between vocabularies.

Experiment 12: Distribution of Experimental group classification scores.

Percent Correct	Frequency	Percent	Valid Percent	Cumulative Percent
50.00	2	16.7	16.7	16.7
50.52	1	8.3	8.3	25.0
51.56	2	16.7	16.7	41.7
52.60	3	25.0	25.0	66.7
53.13	1	8.3	8.3	75.0
54.69	1	8.3	8.3	83.3
55.73	2	16.7	16.7	100.0
Total	12	100.0	100.0	

Experiment 12: Distribution of Control group classification scores.

FREQUENCY DISTRIBUTIONS FOR EXPERIMENT 13

direct test scores*			Percent Correct	Frequency	Percent	Valid Percent	Cumulative Percent
1	2	3					
0	2	4	52.08	1	8.3	8.3	8.3
2	0	0	53.13	1	8.3	8.3	16.7
3	0	0	56.77	1	8.3	8.3	25.0
4	4	4	95.31	1	8.3	8.3	33.3
4	4	4	96.88	1	8.3	8.3	41.7
4	4	4	97.40	3	25.0	25.0	66.7
4	4	4	98.44	1	8.3	8.3	75.0
4	4	4	100.00	3	25.0	25.0	100.0
			Total	12	100.0	100.0	

*Mean direct test scores per cell. Test 1 assessed memory for bigrams seen during training. Test 2 assessed knowledge of bigrams during testing. Test 3 assessed knowledge of the mapping between vocabularies.

Experiment 13: Distribution of Experimental group classification scores.

Percent Correct	Frequency	Percent	Valid Percent	Cumulative Percent
50.00	3	25.0	25.0	25.0
52.08	3	25.0	25.0	50.0
52.60	1	8.3	8.3	58.3
53.13	1	8.3	8.3	66.7
53.65	1	8.3	8.3	75.0
54.69	1	8.3	8.3	83.3
56.25	1	8.3	8.3	91.7
60.42	1	8.3	8.3	100.0
Total	12	100.0	100.0	

Experiment 13: Distribution of Control group classification scores.

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