Canadian Journal of Experimental Psychology
xx, 2004

0.9.0

Abstract Analogies and Positive Transfer in Artificial Grammar Learning

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Following Brooks and Vokey (1991), we show that positive transfer to new items generated from an artificial grammar in which the vocabulary has been changed from training to test can be based on abstract analogy to specific training items (specific similarity) rather than abstraction of a grammar and symbol re-mapping rules, even with re-mapping unique to each test item. Ironically, we also show that the effect of specific similarity does not depend on surface characteristics of the items, but the effect of grammaticality does.

Brooks (1978) introduced the then radical idea that positive transfer in structural learning situations can be accomplished without experimental participants' directly representing any aspect of that structure. Partly in response to Reber's (1967, 1969, 1976) earlier claims for abstraction of structure in artificial grammar learning, Brooks (1978) proposed instead that participants could be responding at test by analogy to specific, remembered training exemplars, a judgment of "sort of like old". Because such a distributed knowledge base preserves many of the statistical properties of the domain, responding on the basis of similarity to prior instances enables relatively accurate classification of novel items because within-domain items generally will be more similar to studied items from the domain than will items from other domains (a property exploited by all subsequent instance and distributed memory models, e.g., Estes, 1986; Hintzman, 1986, 1988; Logan, 1988; Medin & Schaffer, 1978; Nosofsky, 1986). With the elaboration that it is the memory for specific processing *episodes* (e.g., Jacoby & Brooks, 1984), this view has since become a dominant explanation of performance in structural learning situations, and particularly in artificial grammar learning (e.g., Brooks & Vokey, 1991; Higham, 1997a, 1997b; Higham & Brooks, 1997; Vokey & Brooks, 1992, 1994; Whittlesea, 1987; Whittlesea & Dorken, 1993; Whittlesea & Wright, 1997).

The current idea is that classification in artificial grammar learning experiments is based on similarity to prior processing episodes, but that application of this single episodic memory system can have multiple, dissociable effects. Consider dissociations that are obtained if the grammaticality of test items is manipulated orthogonally to their specific similarity (i.e., the similarity of a given test item to a specific training instance; e.g., see Vokey & Brooks, 1992). Higham (1997b), to take just one example, found that dividing attention at test reduced the specific similarity effect, but left the grammaticality effect intact. One explanation for this dissociation might be that the effect of grammaticality is based on an implicit memory system that abstracts the rules of the grammar, and which is relatively impervious to limited attentional resources at test (Reber & Allen, 1978). Conversely, the effect of specific similarity might be based on an explicit memory system (i.e., explicit retrieval of specific training items) whose operation is impaired by a secondary task.

However, Brooks and his students have advocated a more parsimonious explanation: both the grammaticality and specific similarity effects derive from similarity to prior instances stored in an episodic database (e.g., Brooks & Vokey, 1991; Higham, 1997a, 1997b; Higham, Vokey, & Pritchard, 2000; Higham & Vokey, 2000; Vokey & Brooks, 1992, 1994; Vokey & Higham, 1999; Whittlesea & Dorken, 1993). The dissociations between the two effects occur not because of two separate systems, but because sometimes test strings are matched to single (or a few identifiable) prior episodes (specific similarity), but at other times to the ganged retrieval of multiple episodes, or a "chorus" of instances. If specific, single instances are used in the comparison, the results of the matching process are controllable by the participant, and so such factors as limited attentional resources affect them. On the other hand, the results of comparisons with a chorus of instances, that drive the grammaticality effect, are much more ill-defined, are less controllable, and so are not as affected by attentional and other item-specific manipulations. The important point, however, is that both effects derive from exactly the same, unitary memory system.

One situation for which this distributed memory perspective seems particularly appropriate concerns positive transfer to new items generated from an artificial grammar in which the symbols have been changed from training to test. Such results often have been interpreted as evidence for abstraction of the underlying generative structure of the items (e.g., Altmann, Dienes, & Goode, 1995; Gómez & Schvaneveldt, 1994; Manza & Reber, 1997; Mathews et al., 1989; Reber, 1969; Shanks, Johnstone, & Staggs, 1997). However, Brooks

This work was supported by a Natural Sciences and Engineering Research Council of Canada operating grant to JRV and a British Academy research grant to PAH. We thank Lee Brooks for many things, but limit our thanks here for his comments on an earlier draft of the manuscript. We also thank Janine Weir, Lorna Moore, Lisa Bjorn and Scott Glover for their assistance in collecting the data.

and Vokey (1991) argued that this conclusion was confusing the two different meanings of "abstraction": abstraction in the sense of representations beyond the surface details of the items and abstraction in the sense of collapsed across items. Only this latter sense of abstraction is unique to the generative structure. Brooks and Vokey (1991) suggested that the analogical process of memory for prior episodes need not be not restricted to literal, surface detail analogies (i.e., of the type "MVTTTT is like MXTTTT because they differ by only one letter") that would be disrupted by a change in the symbols; rather, it could also operate on the basis of abstract analogies, analogies that transcend the literal symbols or surface details (i.e., of the type "MMVVXX is like BBCCDD", or "MMVVXX is an item with a 112233 pattern", depending on encoding), and hence any changes in them (cf. Redington & Chater, 1996). Moreover, it is not necessary to assume that all or even any of the relevant encoding is necessarily either deliberate or explicit. Such other relatively automatic transcendent perceptual and encoding details as the rhythm or pattern of repetitions are likely to accompany encoding the items in any letter-set or in many other symbol translations and thereby afford a basis for a "sort of like old" judgment whether or not the participant is aware of the pattern.

To demonstrate that such transcendent, analogical mechanisms could account at least in part for the positive transfer in artificial grammar experiments with changed symbols, Brooks and Vokey (1991) manipulated specific similarity orthogonally to grammaticality, but with the vocabulary of letters changed from training to test. Participants received the test items in the same, standard vocabulary (MRTVX), but each was trained with the training items recoded using a different, randomly-chosen vocabulary that excluded the standard letters. Although attenuated relative to the standard condition, it was still the case that there were substantial effects of both grammaticality and specific similarity with changed letters. Subsequent research has shown that, as long as within-item patterning is maintained, positive transfer is still possible even though the vocabulary has changed uniquely for each test item (see Tunney & Altmann, 1999, for review), eliminating the abstraction or application of a consistent symbol-change mapping as a necessary component.

A common alternative to specific similarity to account for exemplar effects has been that participants accumulate knowledge not of whole items, but rather of fragments or "chunks" of them, such as bigrams and trigrams (e.g., Perruchet & Pacteau, 1990), weighted by their frequencies of occurrence over the training set (e.g., Knowlton & Squire, 1994; Perruchet, 1994). Test items with high average "chunk strength" (i.e., composed of frequently occurring bigrams and trigrams) are more likely to be endorsed as grammatical (Knowlton & Squire, 1994). As with specific similarity, this chunk effect has been found to occur independently of grammaticality, and, in at least one experiment, to eliminate the effect of specific similarity when specifically similar and non-similar items were balanced for chunk strength (Knowlton & Squire, 1994). However, other research has found effects of specific similarity even with materials balanced for chunk strength (see Higham, 1997a; Meulemans & Van der Linden, 1997). Furthermore, as Perruchet (1994) acknowledged, by itself the fragment proposal cannot account for the effects of specific similarity with changed symbols. Indeed, Chang and Knowlton (2004) manipulated chunk strength orthogonally to grammaticality in an experiment in which the case and font face of the items were changed from training to test. Participants tested with items in the same format as training replicated the usual results of effects of both grammaticality and chunk strength; however, despite the relatively minimal changes to the items, participants tested with changed items showed an effect only of grammaticality—the effect of chunk strength was eliminated, suggesting that its influence, unlike that of specific similarity in Brooks and Vokey (1991), was tightly tied to the literal, superficial characteristics of the items.

None of the research subsequent to Brooks and Vokey (1991) has investigated the effect of changing symbols as a function of both grammaticality and specify similarity, so it is not clear whether specific similarity effects would still occur when the vocabulary is changed uniquely for each test item, nor whether the effect of grammaticality would remain unchanged under those circumstances once the effects of specific similarity have been controlled. The results bear on the issue of whether item-specific analogy, or "ganged retrieval", or both is responsible at least in part for the positive transfer with changed symbols. The current research was designed to explore these possibilities.

Three experimental groups and two control groups were tested. The first two experimental groups replicated the standard and generic letter-change conditions of Brooks and Vokey (1991). The third was an item-specific letter-change condition in which each test item for each participant was spelled with a unique (although consistent throughout the item) random set of 5 letters (from those not used in the training set, and unique for each participant). Concerns have been raised about whether the effects of structure in many artificial grammar experiments reflect knowledge gained from the training experience, or simply a priori biases among the test items (e.g., Dienes & Altmann, 2003; Perruchet, 1994; Perruchet & Reber, 2003; Reber & Perruchet, 2003; Redington & Chater, 1996). Accordingly, two control conditions were run. The first was a standard control condition in which participants received only the standard transfer set identical to that received by participants in the standard and generic letter-change conditions. The second was an item-specific control condition in which participants received only the item-specific transfer sets of the participants in the item-specific letter-change condition.

Method

Participants. 102 University of Lethbridge undergraduate students participated in exchange for partial credit in a course in introductory psychology; they were distributed roughly equally over the 5 conditions. One-half of the participants in each of the three conditions that included training were trained with list 1 items, and the remainder with list 2 items.

Materials and Procedure. The materials and procedure were the same as those used in in Brooks and Vokey (1991), except for the changes necessary to implement the item-specific letter changes in the experimental and control conditions with item-specific letter-changes. In brief, there were two, 16-item training lists generated from the artificial grammar, each item chosen so that it was 3 to 7 letters in length, and at least two letters in position different from every item in the alternate list. Associated with each training item was a matched, grammatical (G) test item, also generated from the grammar, that differed from its matched training item by a single letter in an initial, middle, or terminal letter position, but by at least two letters from every item in the alternate training list. For every training and G test item pair, there was a matching nongrammatical (NG) test item that differed from the training item by one letter in the same position as the matched G test item, but by at least two letters in position from every item in the alternate training list, for a total of 64 transfer items. Thus, for participants trained with list 1, each corresponding list 1 test item was specifically similar (S) to its matched training item, whereas those in list 2 were specifically nonsimilar (NS) to the training items (i.e., different from all) by the one-letter different criterion. For participants trained with list 2 items, the specific similarity of the test items was reversed. Hence, there were four types of test item: grammatical similar (G-S), grammatical nonsimilar (G-NS), nongrammatical similar (NG-S) and nongrammatical nonsimilar (NG-NS), with the specific similarity of individual items counterbalanced across participants by varying the training list.

Both the training and test phases were presented via computer. For training, the specific training list was divided at random for each participant into four sublists, and each sublist was presented four times successively, each time in a different randomly-selected order. For each presentation of a sublist, participants were instructed to study the four items, and then to press the space-bar on the computer keyboard to clear the screen to begin an attempt at recall in a booklet provided. Participants were instructed that after recalling as much as possible about the four items, they were then to turn over the recall page in the booklet and press the space-bar to receive the next study list. Between sub-lists, participants were informed on the computer screen that a new list of four items would be presented. Participants in the standard and item-specific letter-change conditions received the training items in the standard letter-set (i.e., `MVXRT`). Participants in the generic letter-change condition each received a different, randomly-selected recoding of the training items into a new letter set from the remaining 15 consonants, as in Brooks and Vokey (1991). Training was completely self-paced. Following completion of training, participants were informed on the computer screen that the first phase of the experiment was completed and that they were to await instructions from the experimenter. Participants in the two, no-training control conditions began their participation at this point.

At test, participants were first informed about the existence of the generative grammar as "a complex set of rules", but not the grammar itself. Trained participants were also informed that all the items they had received during training had been generated from the same grammar, whereas untrained participants were told that they were to attempt to determine the basis for grammaticality from the test items themselves. Participants were told that their task would be to rate new items for grammaticality, some generated from the same grammar as had been used to generate the training items and others generated specifically to violate the grammar. They were instructed to respond using a four-point scale: "1-sure it doesn't obey the rules", "2-guess it doesn't obey the rules", "3-guess it obeys the rules" and "4-sure it obeys the rules". Participants in the changed-letter test conditions were further informed about the letter change. Those in the generic letter-change condition received the same transfer test as in the standard transfer condition. Participants in the item-specific letter change condition were informed that each test item had been constructed with a new, randomly-selected set of consonants applied consistently throughout the item, but that nonetheless some of the items obeyed the rules used to generate the training items and some did not. These items were constructed online at test by selecting 5 letters at random for each item from the 15 consonants remaining after the standard set (i.e., `MVXRT`) had been removed to form a specific letter remapping of each test item. Thus, for example, for a given participant, the item `VXTRRR` might be remapped as `QZBKKK`, whereas `VMTRRRX` might appear as `WKDSSSQ`, and so on. For another participant, items `VXTRRR` and `VMTRRRX` might appear as `KBZHHH` and `GLCWWWP`, respectively.

The 64 test items were divided at random, twice, independently for each participant into 16 sublists of 8 items each, with each item appearing twice, as in Brooks and Vokey (1991). All 64 test items were presented once before any item was repeated. Each sublist was presented on the screen with the 8 items arranged vertically. Beside each item was a blank in which participants were to type the numerical rating for that item. Across the top of the screen was a legend depicting the four scale responses. Participants could move freely among the items to insert and to edit their ratings. Upon assigning a rating to every item of a sublist, a message occurred at the bottom of the screen informing participants that they could press the space-bar to advance to the next sublist of items. Participants could not move to the next sublist until every item had received a rating, nor could they move back to a sublist after they had advanced to the next one, but they were free to continue to edit their responses to a given sublist even once every item had received a rating.

Results and Discussion

Table 1 shows the results of the test phase for all five groups as the mean proportion of items endorsed as grammatical (i.e., scale responses > 2) as a function of group and the grammatical status and specific similarity of the test item. Because specific similarity is a function of training list, it is not possible to obtain endorsement proportions as a function of specific similarity for the two, non-training control conditions; for those conditions, only the endorsement proportions as a function of grammatical status are reported under "Non-

Table 1
Mean proportion of items endorsed as "grammatical" as a function of study/test condition and the grammatical status (G vs. NG) and specific similarity (Similar vs. Nonsimilar) of the transfer items. Mean discrimination index, A', for both grammaticality and specific similarity as a function of study condition is shown as the last two columns.

	Similar		Nonsimilar		Discrimination A'	
Study/test Condition	G	NG	G	NG	Grammaticality	Similarity
Standard Condition	0.61	0.42	0.50	0.35	0.65	0.58
Generic Letter-change	0.55	0.47	0.49	0.44	0.56	0.54
Item-specific Letter-change	0.58	0.52	0.51	0.44	0.56	0.57
Standard Control			0.55	0.53	0.51	
Item-specific Control			0.44	0.43	0.51	

similar". The endorsement proportions for the three groups that received training were subjected to a three-way ANOVA with participants nested within letter-change condition (standard, generic letter-change, and item-specific letter-change) but crossing grammaticality and specific similarity as the random variate.

On average, a proportion of .49 of the transfer items were endorsed as grammatical by the three groups that received training, a value that did not vary significantly as a function of letter-change condition ($F < 1$). A significantly greater mean proportion of S (.52) than NS (.45) items were endorsed as grammatical [$F(1,64) = 36.54; MSe = .009$], and this effect did not differ significantly as a function of the three training conditions [$F(2,64) = 1.21; MSe = .009$]. Contrasting the effect of specific similarity for the standard condition with that of the two changed-letter conditions combined confirmed that there was no significant effect of changing letters between training and test on the effect of specific similarity [$F(1,64) = 1.51; MSe = .009$], nor, in a separate orthogonal contrast did the two changed letter conditions differ significantly from one another in this regard [$F(1,64) = 1.01; MSe = .009$].

In contrast, not only on average were a significantly greater proportion of G (.54) than NG (.44) items endorsed as grammatical [$F(1,64) = 89.09; MSe = .008$], this effect of grammaticality varied significantly as a function of letter change [$F(2,64) = 10.62; MSe = .008$]. Contrasting this effect for the standard condition with that of the two changed-letter conditions combined, revealed that there was a significant reduction in the effect of grammaticality of changing letters between training and test [$F(1,64) = 21.11; MSe = .008$], although, as revealed in a separate orthogonal contrast, the reduction in the effect of grammaticality relative to the standard condition did not differ significantly over the two changed letter conditions ($F < 1$). There was no interaction of grammaticality with specific similarity [$F(1,64) = 1.59; MSe = .004$], nor was there a significant three-way interaction with letter-change condition ($F < 1$).

The results for each training condition were compared with their corresponding no training control conditions to establish that the grammaticality effect was above the no-training baseline. For the standard and generic letter-change groups, for which the same test items were used, the appropriate comparison for each is with the standard control condition. The effect of grammaticality was significantly larger than that of the no training, standard control for both the standard [$F(1,38) = 44.04; MSe = .003$] and the generic letter-change [$F(1,37) = 4.25; MSe = .015$] training conditions. Similarly, for the item-specific letter change condition, the effect of grammaticality was significantly greater than that of its corresponding item-specific letter change control [$F(1,39) = 4.94; MSe = .003$]. Thus, for all three groups that received training, the effect of grammaticality cannot be attributed solely to some a priori bias or clustering among the test items, but must reflect some transfer from the training experience.

A summary of the effects of grammaticality and specific similarity in terms of the discrimination index, A', as a function of condition is provided as the last two columns of Table 1. Neither of the two, no training control conditions discriminated grammaticality at a level significantly different from the chance expectation of .50 [maximum $t(20) = 1.03$]. As with the raw, endorsement proportions, changing the letters from training to test, as given by the contrast of the standard, same letter condition with the two different letter conditions combined, revealed a significant reduction in the discrimination of grammaticality [$F(1,64) = 22.35; MSe = .006$], but the two different letter conditions did not differ significantly from one another in this regard ($F < 1$). However, there was no effect of changing letters from training to test on the discrimination of specific similarity, whether the standard condition is contrasted with the two different letter conditions combined [$F(1,64) = 1.51; MSe = .007$], or the two different letter conditions are contrasted with one another ($F < 1$).

The effect of grammaticality was reduced by changing the letters from training to test, but the effect of specific similarity, although otherwise significant, was not. The principal result of the current experiment is that this pattern of results is not limited to a generic letter-change mapping from training to test; rather, the same pattern occurs even with random, test item by test item, letter-changes. To get an effect of specific similarity with item-specific letter changes that doesn't apparently differ from that of generic letter changes or no changes at all suggests that the access or use of specific similarity in these tasks is or at least can be far removed from the surface details of the items; that, indeed, the access or use of this information must be at a level that transcends not only a consistent letter-mapping from training to test, of the type that one could imagine participants computing after studying a few transfer items, especially with the specifically-similar items available to guide the encoding, but also completely random, item-specific letter mappings, for which what mappings had been worked out for other transfer items would be of little use for any given item. But, as this effect is independent of the effect of grammaticality, these remappings, although clearly abstract in one sense of the term, are equally clearly not abstractions of the underlying grammatical structure of the materials; they must be truly abstract analogies.

These results suggest that the effect of specific similarity, even in the same letter condition, is mediated by access to or routine use of analogies at a level far more abstract than the matching of literal surface characteristics or simple, consistent remappings of same. We investigated this possibility by reanalysing the transfer data of the current experiment as a function of letter change condition, and the specific similarity and the degree of repetition "patterning" of the test items, analogous to the direct manipulation of such patterning in Tunney and Altmann (1999). For this purpose, patterning was defined as the number of letter repetitions of each test item: no repetitions—the kind of items for which item-specific letter changes cannot provide a basis for the discrimination of specific similarity, one repetition, and many repetitions, the last two of which all three letter change conditions support. An item such as MMVRT would be coded as having one repetition, whereas items such as MMMVRT (two repetitions of M) and MMVVRT would both count as having many repetitions.

The results are shown in Table 2. There were significant main effects of both specific similarity [$F(1,64) = 5.21; MSe = .041$] and repetition-type [$F(2,128) = 24.00; MSe = .070$], with the effect of specific similarity significantly moderated by repetition-type [$F(2,128) = 8.53; MSe = .045$]. Specifically, the rate of endorsement of similar items significantly exceeded that of nonsimilar items for transfer items containing repetitions [$F(1,128) = 16.27; MSe = .045$, for the contrast with one and many repetitions combined], but not for items without repetitions [$F(1,128) = 3.71; MSe = .045$; in fact, if anything, it is of marginal significance, $p = .06$, in the opposite direction]. Importantly, none of these patterns of results varied significantly as a function of letter-change condition (all Fs < 1); that is, all three letter change conditions, including no change at all, evinced the same pattern for specific similarity: the rate of endorsement of similar items exceeded that of nonsimilar items only for "patterned" test items. Hence, although participants in both the same letter and generic letter change conditions could use the specific similarity of transfer items without repetitions, they apparently do not, responding instead in a manner indistinguishable from that of participants in the item-specific letter change condition for whom the specific similarity of items lacking repetitive patterns could not be detected. Thus, with respect to the effect of specific similarity, we have evidence that regardless of the letter-change condition, not only do participants apparently routinely access or use abstract analogies, the level of abstraction of these analogies is at a level well beyond that of superficial, literal characteristics of the items. Note that we are not suggesting that these abstract analogies are precomputed at training, only that participants generally can and do discriminate or can be and are affected by the specific similarity of the transfer items at a level well beyond that of literal surface characteristics of the items, and that the effect of specific similarity appears generally to be mediated by such abstract analogies, even when more superficial mappings would be adequate (cf. Whittlesea & Dorken, 1993).

In this regard, the specific similarity of the test items appears to dissociate from the grammaticality of them: unlike specific similarity, grammaticality was significantly reduced by changing the letters from training to test. This result was shown regardless of how the change in letters was effected: item-specific letter changes were no more but also no less effective in reducing the effect of grammaticality than were consistent letter changes: both reduced the effect of grammaticality to the same degree relative to the same letter condition. Thus, the effect of grammaticality, unlike that of specific similarity, but like that of chunk strength in the research of Chang and Knowlton (2004), appears in part to be dependent on low-level, superficial, literal characteristics of the items. This pattern of results suggests that the effect of grammaticality in artificial grammar experiments that change the letters or domain between training and test is in fact quite tied to the literal or surface characteristics of the items, and that this effect occurs independently of the specific similarity of the items.

The results could not be more ironic. The effect of specific similarity, manipulated by varying surface details of the items, turns out to be insensitive to related surface variations in the symbols as long as abstract patterning is maintained, whereas the effect of grammaticality, often thought of as the embodiment of deep structure in the sense of transcendent rules turns out to be quite sensitive to variations in what should be irrelevant surface details of the items. It could be argued that the effect of grammaticality remaining after changing the symbols, albeit much reduced, is evidence for the abstract knowledge of structure of the type envisioned by e.g., Reber (1989) and Mathews et al. (1989). But this argument is suspect, if only because it could easily be the case that with further control over other details of common elements of encoding (e.g., rhythmic patterning, or even specific encoding instructions as in Vokey & Brooks, 1992; Whittlesea & Dorken, 1993) even this residual effect of grammaticality would disappear. That is, given the current results there is every reason to believe that after the analogical effects of specific similarity have been removed, the effects of structure in these tasks may be mediated by little more than a relatively automatic, low-level process of general familiarity (i.e., ganged retrieval) induced by any overlap in encoding between test items and the contextually-defined set of training items (see Whittlesea & Dorken, 1993), or contradicted by some salient difference from all training items (Higham et al., 2000; Higham & Vokey, 2000; Vokey & Higham, 1999; Wright & Burton, 1995).

Table 2
Mean proportion of items endorsed as "grammatical" as a function specific similarity (Similar vs. Nonsimilar), and "patterning" or number of repetitions per item (None, One, or Many) for each of the three letter-change conditions.

	Patterning					
	None		One		Many	
Letter Change	Similar	Nonsimilar	Similar	Nonsimilar	Similar	Nonsimilar
Same	0.49	0.53	0.38	0.27	0.54	0.44
Generic	0.55	0.63	0.39	0.27	0.53	0.48
Item-Specific	0.51	0.59	0.46	0.27	0.56	0.50

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