NONHUMANS HAVE NOT YET SHOWN STIMULUS EQUIVALENCE

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Recently, two published articles have reported finding stimulus equivalence in nonhumans. One suggested that equivalence was due to the mediation of names. The procedure used trained all components of all tested relations. Because nothing was derived, the defining characteristics of equivalence were not achieved. In the second study a definition of equivalence was proposed that fails to distinguish functional stimulus classes from equivalence classes. The resulting data are not clearly relevant to stimulus equivalence in Sidman's sense of the term. Stimulus equivalence has not yet been shown in nonhumans.

Key words: stimulus equivalence, naming, homogeneous chains, response mediation, equivalence in nonhumans

If a verbally competent human subject is trained to match Stimulus A to Stimulus B, and B to C, the subject will now show derived symmetry (match B to A, C to B), derived transitivity (A to C), and reflexivity (A to A, B to B, and C to C). These relations are derived in the sense that they occur without direct training (at least with the specific stimuli involved). Together these derived relations have been used as the defining characteristics of a phenomenon known as *stimulus equivalence* (Sidman, 1971; Sidman & Tailby, 1982).

The term stimulus equivalence has had a long history in psychology, and many of its uses do not relate directly to equivalence as defined by derived symmetry, transitivity, and reflexivity. These other uses of the term may be perfectly legitimate in their own right. Thus, to avoid confusion in the present article I will use the term *Sidman equivalence* to refer to stimulus equivalence as defined by these three relations.

Psychologists have studied similar phenomena from the beginning of the discipline, but Sidman's procedure has notable advantages. It can be applied as readily to the experimental study of nonhuman behavior as to human behavior. It allows for repeated acquisitions. It allows for sensitive measurement of the emergence of derived performances, without the all-or-none quality of many earlier preparations.

In the nearly 20 years since Sidman's original demonstration, Sidman equivalence has been shown in a variety of human populations. It occurs readily in normal children (Lazar, Davis-Lang, & Sanchez, 1984; Sidman & Tailby, 1982) even as young as 2 years old (Devany, Hayes, & Nelson, 1986). It occurs in retarded children and adolescents (Sidman, Cresson, & Willson-Morris, 1974; Spradlin, Cotter, & Baxley, 1973) and normal adults (Lazar, 1977; Wulfert & Hayes, 1988). To date, only humans without any spontaneous use of symbols have failed to reveal Sidman equivalence (Devany et al., 1986).

In contrast, until 1987, Sidman equivalence had not been said to have been shown in any nonhuman organism. Unsuccessful attempts had been made with pigeons (D'Amato, Salmon, Loukas, & Tomie, 1985; Lipkens, Kop, & Matthijs, 1988), and monkeys, baboons, and other primates (D'Amato et al., 1985; Sidman et al., 1982). Recently, however, two published articles (McIntire, Cleary, & Thompson, 1987; Vaughan, 1988) have claimed to have demonstrated stimulus equivalence in Sidman's sense of the term. The grounds for these claims distort the defining properties of Sidman equivalence.

MCINTIRE, CLEARY, AND THOMPSON (1987)

The rationale underlying this study was as follows. Humans can readily engage in behavior of a characteristic topography in the presence of stimuli in a matching-to-sample task (i.e., emit verbal names). "It is possible

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that the discriminated emission of differential response topographies is itself of functional significance in the establishment of stimulus equivalence" (McIntire et al., 1987, p. 280). Perhaps two stimuli can be related because the same characteristic response is emitted in the presence of each. McIntire et al. generated an experimental procedure said to be based on this model. Monkeys trained in the procedure apparently showed all the defining characteristics of equivalence.

The most straightforward problem with McIntire et al.'s (1987) demonstration is one of method. A detailed examination of their procedure shows that they directly trained all components of all tested relations. Because no relations were derived, there are no grounds to conclude that these animals showed Sidman equivalence.

Problems with McIntire et al.'s Method

McIntire et al. (1987) trained the monkeys to engage in a characteristic response (a name) to one set of stimuli (different colors were used as stimuli) and a topographically distinct response to another set. For expository purposes they called one set *odd* and labeled the stimulus elements 1, 3, and 5; the other set was *even* (2, 4, and 6). An odd response ("R(odd)") was trained to the odd set, an even response ("R(even)") to the even set.

In a training trial, the animal was required to emit the appropriate response topography to the sample and then again to the correct comparison. For example, if the sample was 1 and the comparisons were 3 and 4, a reinforced performance could be characterized as follows: 1-R(odd)-3-R(odd). Any other combination of stimulus selections or of response topographies would not be reinforced.

The primary set of relations McIntire et al. (1987) trained is shown in the Appendix, using the above notation. The tests for equivalence are shown at the bottom of the table. For ease of reference, I have labeled each problem with a letter. Letter names below refer to those in the Appendix. The six relations representing the names for each stimulus—1-R(odd), 3-R(odd), 5-R(odd), 2-R(even), 4-R(even), and 6-R(even)—were of course directly trained (see section on trained components in the Appendix). Animals were directly reinforced for engaging in a characteristic response to each stimulus set. This training is required by any

naming model of equivalence. However, the following relations were also directly trained: R(odd)-1, R(odd)-3, R(odd)-5, R(even)-2, R(even)-4, R(even)-6. This type of training is not required by a naming model and its inclusion eliminates the relevance of the results to Sidman equivalence.

Consider Problems A and D (Appendix). In these problems either Stimulus 1 or 2 is the sample and 3 and 4 are the comparisons. Assume that the animal always selects each stimulus with its appropriate name as it has been trained to do. If so, only four sequences are likely:

Problem A correct: 1-R(odd)-3-R(odd)

Problem D correct: 2-R(even)-4-R(even)

Problem A incorrect: 1-R(odd)-4-R(even)

Problem D incorrect: 2-R(even)-3-R(odd).

Note that selecting Stimulus 3 after R(odd) is directly reinforced (Problem A correct), whereas doing so after R(even) is not (Problem D incorrect); selecting 4 after R(even) is reinforced (Problem D correct), whereas doing so after R(odd) is not (Problem A incorrect).

In McIntire et al.'s (1987) procedure, emitting the characteristic response to the sample turns off the sample and turns on the comparisons. The characteristic response always immediately precedes selection of the correct comparisons. Thus, the animal has been directly trained in name-stimulus relations as well as stimulus-name relations.

It seems certain that name-stimulus relations were learned in addition to any stimulus-stimulus relations that were acquired. Research with pigeons has shown that in a normal matching-to-sample preparation, the relation between the *response* given to the sample and the selected comparison *stimulus* is *more* readily learned than the sample-comparison relation per se (Cohen, Brady, & Lowry, 1981).

Had McIntire et al. (1987) simply used their procedure to train the conditional relations typically trained in equivalence research (Problems A, B, D, and E), their findings would indeed have constituted evidence for derived relations, although not necessarily of the same kind observed in humans. Unfortunately, two problems were added: 1-R(odd)-1-R(odd) and 2-R(even)-2-R(even) (Problems C and F, respectively). In a footnote (p. 281), they explained that these "reflexivity problems" were added so that Stimuli 1 and 2 "would be pre-

sented as comparison stimuli during training" (p. 281). This is legitimate, but by using the same stimuli also as *samples* in these trials (rather than, for example, novel samples with no characteristic response required) they ensured that *all* name-stimulus relations were *directly* trained (see Appendix).

For example, in Problems A and B the animals were directly trained in the sequences 1-R(odd)-3-R(odd) and 3-R(odd)-5-R(odd). A crucial test trial in this case is Problem O: 5 (sample), 1 and 2 (comparisons). Both 5 and 1 share the same name. McIntire et al. (1987) argue that "stimulus events related to the emission of the first naming response set the occasion for a subsequent naming response of the same type" (p. 285). Because Stimulus 5 leads to the emission of an R(odd) response (through direct training) it should also set the occasion for another R(odd) response. This response is only likely to be emitted given Comparison 1 and not 2 (again, because of direct training), and thus the animal should choose 1. The expected terminal performance is 5-R(odd)-1-R(odd).

Adding Problem C—1-R(odd)-1-R(odd)—directly trains the only link in this terminal performance not directly trained: R(odd)-1. The subject sees Sample 5 and engages in a directly trained R(odd) response (acquired in Problem B). Immediately after engaging in the R(odd) response, two comparisons are presented, 1 and 2. The animal has been reinforced for selecting 1 after engaging in an R(odd) response and has been extinguished for selecting 2 after such a response. Both relations were learned in Problem C. So the animal selects 1, as has been trained. The animal does so with an R(odd) response emitted to 1, as has been trained in Problems A and C.

Because a name has been directly trained to both precede and follow all stimuli of the same set, moving from one to another in any possible test trial can occur as a series of simple discriminations. Nothing is derived about this performance except the relatively unimportant derivation of specific response sequences.

Logical Problems With Naming Models of Equivalence

McIntire et al. (1987) are vague about the precise theoretical process they suspect gives rise to equivalence, but some consideration of their theoretical position seems warranted. The

authors seem to be appealing primarily to the generalized effects of a greater history of reinforcement for topographically homogeneous response chains than topographically heterogeneous response chains. If homogeneity of response topographies is reinforced across a variety of stimulus sets and particular response forms, then eventually, when presented with a sample that evokes a response of a given topography, the organism may now choose a comparison that evokes a response of the same topography.

There is inadequate space to deal with this model in detail, but it seems worthwhile to point to a few of its difficulties. First, if it is to be applied to equivalence situations we must suppose that the subject often self-generates the necessary topographically distinct responses. If the subject did this in all situations, then a homogeneous chain model would predict that symmetry would always result whenever a conditional discrimination was learned. But symmetry may be harmful at times in the natural environment. Time flows in one direction, not two, and reinforcement will not necessarily result if the functions of conditional and discriminative stimuli are simply reversed. A chimp learning to orient toward and approach a thicket upon seeing a lion dares not thereby orient toward and approach a lion upon seeing a thicket. Thus, a homogeneous chain model would need to describe how the self-generation of homogeneous responses is appropriately restricted.

Further, consider stimuli coming from different functional classes, for which topographically *distinct* response functions were *directly* trained. Because common objects (each with directly learned response functions) can surely enter into equivalence classes with each other, a homogeneous chain model would have to explain how self-generated common responses somehow override directly trained distinct response functions.

Third, if it is the case that derived symmetry can be harmful in at least some instances, then subjects would need to learn to avoid deriving symmetry even when there is a common response function. Otherwise, all functional classes would yield equivalence classes, even when it would be detrimental to do so.

The homogeneous chain model also becomes very awkward when applied to some important instances of equivalence. As McIntire et

al. (1987) point out, "many studies of the establishment of equivalence classes in human subjects used verbal stimuli (spoken or heard by the subjects) . . ." (p. 280). They go on to note that even when so-called "verbal stimuli" (cf. Hayes & Hayes, in press) are absent, "subjects may have derived their own linguistic codes" (p. 280). That is, McIntire et al. attempt to invoke verbal naming in cases where verbal names were not used. But how does a naming model explain Sidman equivalence when verbal stimuli *were* used?

Consider an example. A person is exposed to a written word as a sample (A), and to an array of objects. Selection of one of the objects (B) is reinforced. The A-B relation has been trained. Typically, A is the written name for the object, so we may say that the subject has learned a written name-object relation. Suppose the subject now chooses the written word A when shown the object B. In order to explain this symmetry McIntire et al. (1987) must suppose that a common response is emitted to both stimuli. Because no such response was directly trained, we have to suppose that the subject emitted a topographically distinct common response on his or her own. Perhaps when the subject saw the written name as a sample she said Y. She also said Y when she saw the object as a comparison. When later presented with the object as a sample, she once again says Y and now selects the written name as a comparison because she also said Y in its presence.

The awkwardness is revealed as the class is elaborated. Suppose the subject is again exposed to the written word as a sample and to a series of spoken names as comparisons. Selection of one of the auditory names (C) is reinforced. The A-C relation is trained. Suppose the subject now selects A (written word) given C (spoken word), B (object) given C (spoken word), and C (spoken word) given B (object). This result with such stimuli is a completely usual instance of Sidman equivalence (e.g., see Dixon & Spradlin, 1976). To the extent that McIntire et al.'s (1987) model is meant to explain Sidman equivalence, it must presumably explain what they themselves say is the typical case.

To apply a homogeneous chain model to the derived symmetry between the spoken name and written name we must suppose that the subject names the written name Y and then

names the spoken name Y as well. The same name must be used in this situation as before or, according to McIntire et al.'s (1987) model, derived relations between B and C could not occur. The directly trained spoken name C cannot be the common response that relates the members of the class because the A-B relation was learned (and symmetry derived) before C was heard. Thus, the subject must say Y when she hears C or C could not be related to A and B. Further, a person now asked to read A (the written name) or to name B (the object) would have to say Y (but not aloud as it would be incorrect) and then say C because the name C is also named Y. The awkwardness of this example only increases as additional relations are added to the class. None of these problems are necessarily lethal, but clearly a much more elaborated homogeneous chain model is needed before its logical integrity can be examined.

More work is also needed on the empirical side. To test a model of Sidman equivalence based on a generalized tendency to emit homogeneous response chains, it would be necessary to leave out Problems C and F from McIntire et al.'s (1987) procedure. An even more rigorous test would be as follows. Reinforce only homogeneous response chains in pretraining with several stimulus sets. Then train a common response to each stimulus in a novel stimulus set X (to avoid training of name-stimulus relations, this common response should not be acquired in a matching-to-sample format). Now when shown any element from X as a sample in a matching-to-sample format the subject should be able to select any other stimulus from Set X when used as a comparison without direct training to do so. A homogeneous chain model of equivalence should require only a history of homogeneous chains and the presence of samples and comparisons that evoke the same response (i.e., have the same name).

VAUGHAN (1988)

A study by Vaughan (1988) is the second to report finding stimulus equivalence with nonhumans. The term *stimulus equivalence* has been given a variety of meanings in the history of psychology. Vaughan points out that "there is another way to view an equivalence relation, termed a *partition*" (p. 36, emphasis in the original). A partition is a type of subset: "Two

triangles are said to be similar if corresponding angles are equal. [This relation] partitions a given set of triangles in disjoint subsets" (p. 36). Vaughan views partitioning as synonymous with equivalence relations.

Vaughan (1988) divided a set of slides randomly into two sets (call them A and B). One set was defined arbitrarily as an S+ set, and the other was an S- set. Using pigeons, Vaughan established a discrimination between the sets. The S+ set then became an S- set; the S- set became an S+ set. After performance stabilized, the functions were reversed again. These reversals were repeated over and over. Eventually, when the conditions were reversed, exposure to a few slides in a set led to appropriate performances with all the remaining members of the set.

This clever study shows that functional stimulus classes can be formed on totally arbitrary grounds. The history that established the class, however, is direct. The same contingencies and same reinforced responses were always involved for all members of the set. Based on this direct history, each subset of stimuli came to operate as a functional class. The transfer between slides in a set when reversals were made emerged over time as multiple reversals took place.

It might be argued that although Sets A and B were directly trained functional stimulus classes, the timing of the reversal was derived. If derived means without direct training, this too must be rejected. The contingencies for the first A stimulus in a set were always the same as for all A stimuli; the same was true with the B stimuli. Suppose Set A shifted from an S+ to an S- set. The bird would peck the first member of Set A, but reinforcement would not follow. The bird had a long history of these kinds of reversals. Failure of reinforcement to follow pecking any A stimulus was reliably associated with reinforcement for pecking any B stimulus, but not any other A stimulus. In the terminal performance, when Stimulus A1 changes from an S- to an S+ the bird may reverse responding to Stimuli A2, A3, and so on, without making contact with the new contingency. But this emerged only after a direct history in which reversals occurred *after* extensive contact with the changed contingency. Thus, initial contact with a reversal could serve as a directly trained discriminative cue. The birds' acquisition curve over repeated reversals

can be conceptualized as acquisition of this directly trained discriminative function.

The primary problem with Vaughan's (1988) study is not empirical or procedural, however; it is definitional. Vaughan has a right to define stimulus equivalence in any way he wishes, though we may bemoan the confusion that results. Vaughan claims as a *definitional* matter that "any partition implies an equivalence relation" (Vaughan, 1988, p. 36). This type of "equivalence" requires only a stimulus set or partition, and Vaughan's procedure clearly yielded a stimulus set. Any functional class of stimuli is a partition, and if that alone defines equivalence, then any demonstration of a discriminated operant shows it. But Vaughan does not have grounds to state, as he does, that he has shown Sidman equivalence.

Vaughan's (1988) procedure does not allow testing for the characteristics of Sidman equivalence. Symmetry requires that the functions of sample and comparison be substitutable, without direct training. It is not clear how these concepts, developed in a matching-to-sample preparation, apply to the reversal procedure used by Vaughan. Perhaps it may be said that derived symmetry in a more generic sense involves responding to one stimulus (e.g., a comparison) in terms of its relation to another stimulus (e.g., a sample) based on direct training, and vice versa without training. The Vaughan procedure showed that functions given to one member of a functional stimulus class transferred throughout the class, but this required a direct history with the transfer itself. The birds required several dozen reversals before the repetition of a previously experienced change in stimulus function (i.e., changing from S+ to S- or vice versa) would transfer throughout the class.

Transitivity in a generic sense requires responding to one stimulus in terms of its symmetrical relation to a second, via a symmetrical relation to yet another stimulus. Transitivity requires, at a minimum, relations among three stimuli, all of which are class members. In Vaughan's (1988) procedure each stimulus was related to all other stimuli in the same subset in the same way: as fellow members of a directly trained partition or class. There seems to be no reasonable possibility of transitivity in this case, because all stimuli are related to each other directly, not through a common relation to a third stimulus that is also a class

member. If it is argued that food could be the third stimulus, then it would have to be shown that the bird responds symmetrically to the stimulus–food relation.

One of the primary justifications Vaughan (1988) gives for viewing stimulus partitions as identical to Sidman equivalence is the following quote: “After an equivalence class (ABC) is formed, if one member (A) becomes discriminative for a response, then B and C will also become discriminative for the same response” (Devany et al., 1986, p. 255). Vaughan goes on to note, “In the present experiment, when an arbitrary subset of slides in Set 1 became discriminative for pecking . . . responses to most of the rest of the slides [in Set 1] changed appropriately. On the basis of these data, the inference may be drawn that the pigeons had formed equivalence classes” (1988, p. 41). The context of the Devany et al. quote makes it clear that we did not mean to substitute a transfer of functions through equivalence classes for the defining criteria of Sidman equivalence, namely, reflexivity, symmetry, and transitivity. When reflexivity, transitivity, and symmetry occur, such a transfer is likely. Other psychological processes can produce transfer of stimulus functions, however (e.g., classical conditioning, stimulus generalization). Vaughan’s logic would allow the use of the term stimulus equivalence on the basis of any transfer of stimulus functions, including, as with his triangle example, those having their sources in formal properties of the stimuli. In addition, we were speaking of derived transfer of functions. In Vaughan’s procedure, changes in function were themselves directly trained.

The distinctions between functional stimulus classes and equivalence classes seem important. Equivalence classes are defined by the derived relations among members. Functional classes are defined by common response functions—nothing need be derived. Although it deserves more investigation, there are empirical reasons to suppose that the two kinds of classes are quite distinct. It is known, for example, that discriminative, reinforcement, or other functions given to an element of an equivalence class will transfer to other members of the class without direct training (e.g., Hayes, Brownstein, Devany, Kohlenberg, & Shelby, in press; Wulfert & Hayes, 1988).

Functional classes, however, are formed in terms of common functions and are distinguished from other classes by differing functions. Suppose Stimuli A and B are members of functional Class X because of a shared Response Function Y. If Stimulus A acquires a new Response Function P then it may also have membership in another functional Class Q. This does not mean that Classes Q and X will now be the same class. If new functions given to one member of a functional stimulus class readily transferred to all other members as they do in equivalence classes, eventually such a process would lead *all* stimuli to be in one functional class. In equivalence, this transfer of functions is controlled both by the context establishing class membership and the context selecting relevant functions (Hayes, in press; Wulfert & Hayes, 1988). It is not clear what the parallel process could be in the case of functional classes, because class membership is defined by the very processes (i.e., response functions) that would be transferred.

Untangling these issues empirically will be difficult, all the more so because humans seem to use virtually any cue to bring to bear a history of arbitrarily applicable relational responding on a current setting. Thus, humans will almost certainly—under some conditions—derive equivalence classes from functional classes. The very fact that psychologists can talk about and identify functional classes (e.g., “food-related settings”) suggests as much, because doing so presumably requires an equivalence class for the psychologist. But this derivation may not itself be reducible to an issue of functional class formation. Rather, processes of equivalence class formation may act upon the products of a wide variety of other behavioral processes.

CONCLUSIONS

My criticism of the applicability of the studies by Vaughan (1988) and McIntire et al. (1987) to Sidman equivalence is not by way of saying that Sidman’s definition of stimulus equivalence draws the proper distinctions between phenomena of this kind and other phenomena. Sidman’s definition seems to have been driven too much by mathematics and not enough by psychology. Reflexivity, for example, can always be based on formal characteristics of the stimuli involved, whereas

symmetry and transitivity by definition must not be based on a priori formal properties of the stimuli. It is not obvious why two such different kinds of psychological relations should be combined into the definition of one phenomenon. Similarly, as Sidman defined it, transitivity need only be "one way" (i.e., given A-B and B-C, then A-C). Such unidirectional performances can readily be derived from simpler conditioning processes (perhaps one reason this kind of transitivity is obtainable in nonhumans; see D'Amato et al., 1985), whereas mutual transitivity (i.e., both A-C and C-A derived performances) and symmetry cannot. Nevertheless, alternative formulations should be discussed and defended directly (see Hayes, in press, and Hayes & Hayes, in press, for examples of such an attempt). It can only create confusion to take phenomena that *do not* meet the requirements for Sidman equivalence and attempt to alter Sidman's definition *de facto* by claiming that they *do*.

The problems with these two studies do not mean, of course, that Sidman equivalence cannot be shown in nonhumans. One possible approach may be to provide an extensive reinforced history with symmetrical relations. After hundreds or thousands of directly reinforced symmetrical relations, equivalence may emerge (Hayes, in press). Language-trained chimpanzees probably have as extensive a history of reinforced symmetry as any nonhuman. These animals should be most likely to show equivalence if a history with symmetry or transitivity can lead to stimulus equivalence in nonhumans.

Conversely, we must be open to the possibility that stimulus equivalence may be an example of complex human behavior that cannot be derived solely from processes known in nonhumans. Although such a prospect presents a challenge to some strategic assumptions of behavior analysis (Hayes, 1987), it deserves careful empirical study by behavior analysts. An adequate understanding of stimulus equivalence is too important to the analysis of human functioning to define the problem away. Behavior analysts must be careful not to claim that equivalence has been shown in nonhumans when the defining characteristics of equivalence have not been met. As of this moment, stimulus equivalence has not been shown in nonhumans.

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APPENDIX

The problems used by McIntire et al. (1987) for training and testing, and the component performances that could have produced their results.

Training problems:		
Problem	Trained sequence	
A	1-R(odd)-3-R(odd)	
B	3-R(odd)-5-R(odd)	
C	1-R(odd)-1-R(odd)	
D	2-R(even)-4-R(even)	
E	4-R(even)-6-R(even)	
F	2-R(even)-2-R(even)	
Trained components:		
Stimulus-name relations:	Trained in problem	
1-R(odd)	A and C	
3-R(odd)	A and B	
5-R(odd)	B	
2-R(even)	D and F	
4-R(even)	D and E	
6-R(even)	E	
Name-stimulus relations:		
R(odd)-1	C	
R(odd)-3	A	
R(odd)-5	B	
R(even)-2	F	
R(even)-4	D	
R(even)-6	E	
Testing problems:		
		Components
Problem type	Problem	trained in problems
Symmetry	G	3-R(odd)-1-R(odd) A, B, C
	H	5-R(odd)-3-R(odd) A, B
	I	4-R(even)-2-R(even) D, E, F
	J	6-R(even)-4-R(even) D, E
Reflexivity	K	3-R(odd)-3-R(odd) A, B
	L	4-R(even)-4-R(even) D, E
Transitivity	M	1-R(odd)-5-R(odd) A, B, C
	N	2-R(even)-6-R(even) D, E, F
Sym × Trans	O	5-R(odd)-1-R(odd) A, B, C
	P	6-R(even)-2-R(even) D, E, F