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## SELECTIVE ASSOCIATIONS: A METHODOLOGICAL CRITIQUE

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A crossover design must be used and a crossover data pattern must result if selective associations are to be inferred. In addition, three other methodological criteria must be met. (1) Only the nature of the reinforcer should be varied, (2) nonassociative explanations must be ruled out, and (3) the differences in behavior controlled by the conditioned stimuli (CSs) must be caused by a learning difference and not a performance difference. Experimental evidence typically cited as demonstrating the existence of selective associations was reviewed here and found to fall short of meeting the criteria stated herein. I conclude that interpretations invoking selective associations have been premature.

Thorndike (1898) found that when cats were required to scratch or lick themselves to escape from a box, they learned it slower than other responses such as pulling a ring or scratching the side of the box. Also the vigor of scratching and licking decreased unlike the other escape responses. Speculating about these results, Thorndike (1932, p. 72) stated: "At the beginning there was presumably only a very low degree of *belonging* between the impulse to scratch and the situation of being confined in that cage (or in any cage), the act of scratching *belonging* chiefly to some bodily stimuli. This low degree of *belonging* [italics added] was indeed probably one main reason why the learning of so simple a connection was so slow."

Thorndike's notion of "belongingness" was not confined to a response belonging to some stimulus. Rather, in his work on human learning (Thorndike, 1932), he discussed how some stimuli belong with other stimuli. For example, in one experiment, a paragraph was read to

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subjects 10 times. The subjects were then asked to answer a set of questions that required them to tell the experimenter what word had followed another word within the paragraph. Two of the questions gave the first name of a character in the paragraph and were included to test the belongingness between the first and last name. When the questions dealt with pairs of words that had little belongingness, subjects scored at *chance level*, but when given the two questions with the first name, subjects scored above chance level.

Empirically, belongingness is a result that shows that certain stimulus-stimulus or response-stimulus combinations produce the result of interest more easily than other combinations (Testa, 1974). It should be recognized, however, that all empirical observations are theoretically influenced (Kuhn, 1962; Skinner, 1950). Over the years the empirical notion of belongingness, in whole or in part, has appeared and reappeared in different theoretical guises. For example, instinctual drift (Breland & Breland, 1961), stimulus-relevance (Capretta, 1961), species-specific defense reactions (Bolles, 1970, 1972), preparedness (Seligman, 1970; Seligman & Hager, 1972), stimulus-response and stimulus-reinforcer specificity (Shettleworth, 1972a, 1972b), and selective associations (LoLordo, 1979a, 1979b; LoLordo & Droungas, 1989) all have incorporated the notion of belongingness (see also Krane & Wagner, 1975; Revusky, 1971; Rozin & Kalat, 1971; Logue, 1979).

### The Definition

This report is concerned with only one of the theoretical interpretations of belongingness. This theoretical interpretation, which has received much experimental attention, focuses only on those examples of belongingness between a stimulus and reinforcer. This interpretation, termed selective association, has been defined by LoLordo (1979b, pp. 369-370) as follows: "connections between certain antecedent and consequent events (CS1-US1, CS2-US2) within a set of events (CS1, CS2, US1, US2) may be formed very easily, whereas connections between other antecedents and consequents (CS1-US2, CS2-US1) may not be formed at all, or only with great difficulty."

Discussed subsequently are the methodological details required by this definition to successfully infer selective associations.

### The Crossover Design

A crossover design (Schwartz, 1974) must be used if selective associations are to be inferred. In such a design, each CS type (CS1 and CS2) is paired with each US type (US1 and US2). The definition of selective associations requires this design in order to eliminate some of the nonassociative interpretations of the results. For example, suppose CS1 is paired with US1 for Group 1 and that CS2 is paired with US1 for Group 2. Suppose further that conditioning proceeds faster or more

thoroughly to CS1. Is CS1 simply more salient? If so, then it should condition more easily when paired with a second US, US2 (Rescorla & Wagner, 1972). If, however, CS2 conditions more readily than CS1 when each is separately paired with US2, then clearly we cannot argue that CS1 is more salient. Rather, we would argue that CS1 must have conditioned more rapidly when paired with US1 because it was selectively associated with US1. Likewise CS2 must have been selectively associated with US2.

What conclusions can be drawn if CS1 conditions better than CS2 when paired with US1, but neither conditions when paired with US2? Such a data pattern is inconclusive: Perhaps CS1 is more salient than CS2 but the conditioning parameters (e.g., US2 intensity) are not suitable for conditioning to either CS. Or perhaps CS1 was selectively associated with US1 and neither CS associated readily with US2. Similarly, what conclusions should be drawn if CS1 conditions better than CS2 with US1 but both condition well and equally with US2? Again, the data are inconclusive. Perhaps CS1 is more salient, but US2 is so effective that ceiling effects prohibit detecting a difference between CS1 and CS2. Or perhaps CS1 associates selectively with US1 but both CSs associate well with US2. In short, only when CS1 is better associated with US1 and CS2 is better associated with US2 can a conclusion about the existence of selective association be made. I shall refer to this outcome as the "crossover data pattern."

Within the selective association literature, the methodological requirement of a crossover design has typically been satisfied by the use of the single-cue procedure (used in the previous example) or the compound-cue procedure (LoLordo, 1979b; LoLordo & Jacobs, 1983). In the compound-cue procedure, one group receives a compound CS paired with US1 (e.g., CS1CS2-US1). A second group receives the same compound; however, it is paired with US2 (CS1CS2-US2). Subsequent procedures, such as an element test in which each CS is presented alone, are used to assess the associative strength acquired by each CS. As with the single-cue design, selective associations can be inferred only if there is a crossover data pattern; that is, if CS1 is more readily conditioned when the compound is paired with US1 and CS2 is more readily conditioned when the compound is paired with US2.

### Other Methodological Criteria

Besides the required crossover design, several other methodological criteria for inferring the existence of selective associations are suggested by the definition. First, when the reinforcer is varied, all other procedural details must be held constant (Shettleworth, 1972a). If other procedural details are allowed to vary along with the reinforcer, then any difference in behavior could be as justifiably attributed to these other procedural differences as to the differences in the reinforcers (Shettleworth, 1972a, 1972b).

Before continuing, I must clarify the meaning of procedural detail. Parameters such as CS duration and intensity, session length, intertrial interval, interstimulus interval (ISI), and number and distribution of CS-US pairings are a few examples of what I mean by procedural detail. Where the meaning of procedural detail becomes confusing is with respect to the reinforcer. For example, suppose that when US1 is presented, it has an immediate effect upon the animal (e.g., shock), but when US2 is presented, it has a delayed effect (e.g., toxin). These intrinsic properties of a reinforcer are *not* what I mean by procedural details. By definition, they are the very thing that is manipulated when the nature of the reinforcer is varied.

From this discussion, a distinction can be made between two types of confounds in a selective association experiment. When procedural details like number of CS-US pairings are confounded with variations in US, then the confounds can be said to be avoidable. They are avoidable in that they can be eliminated without altering the nature of the US. In contrast, if the latency of the unconditioned response (UR) is confounded with variations in US quality, then the confound can be said to be unavoidable. To eliminate the confound, one would have to alter the nature of the reinforcer (i.e., the very thing of interest). Unavoidable confounds such as this may complicate our efforts to infer selective associations, but we may have to tolerate them. There is no reason, however, why we should tolerate avoidable confounds.

A second methodological necessity for inferring selective associations is the use of appropriate nonassociative control groups (Linwick, Patterson, & Overmier 1981; LoLordo, 1979b; Mitchell, Scott, & Mitchell, 1977; Rescorla & Holland, 1976). The term *selective association* implies that an experimentally observed stimulus-reinforcer interaction must reflect differences in association. Nonassociative accounts must thus be precluded as plausible explanations. For example, Holland (1977) noted that different CSs evoked different unconditioned or orienting responses (e.g., startle to tone and rearing to light). Therefore the measured responses may not be conditioned responses, but instead may be URs to the CSs. Other nonassociative interpretations include sensitization or pseudoconditioning. In a pseudoconditioning control, the US is presented alone and later the CS is tested. This control procedure is designed to see if mere presentation of an US is enough to produce the appearance of conditioning to a CS. Other nonassociative controls include the explicitly unpaired control and the truly random control (Rescorla, 1967).

A third necessity, related to the second, is implied by the idea that selective association is defined in terms of differences in connection strength. Thus, it must be demonstrated that the stimulus that controlled the behavior of interest to a lesser degree (S1) actually acquired less associative strength than the stimulus that controlled behavior to a greater degree (S2). That is, it must be demonstrated that learning differed and not just performance (LoLordo, 1979b; LoLordo & Jacobs,

1983; Shapiro & LoLordo, 1982). Procedures such as blocking (Kamin, 1969) or second-order conditioning may be used to demonstrate differences in associative strength (see Holland, 1977). The importance of multiple measures in assessing the associative strength acquired by a stimulus has recently been discussed by Spear, Miller, and Jagielo (1990). Note, however, that the definition does not preclude acquisition of associative strength to S1, but instead it states that conditioned value may be more difficult to acquire. If asymptotically equivalent associative strength is demonstrated, then it must be shown that acquisition of associative strength is slower to S1 as compared to S2 (LoLordo & Jacobs, 1983).

Selective associations are often thought to be specific instances of "biological constraints" on learning. Some of the purported evidence for selective associations has been discussed in recent reviews of the biological constraint issue (Damianopoulos, 1989; Domjan & Galef, 1983). The following review, in contrast, focuses exclusively and hopefully more exhaustively on the experimental evidence for selective associations per se. Its goal is to determine whether that evidence meets the methodological criteria required by the definition.

### Experimental Evidence

Typically Garcia and Koelling's (1966) results are referenced as the classic demonstration of selective association. Using water-deprived rats and the compound-stimulus crossover design, they paired a compound CS with illness (induced by X ray or lithium chloride, LiCl) or with grid shock. The compound CS was a gustatory stimulus (sweet or salty water) together with an audiovisual stimulus (clicker and light). Each lick at the taste CS caused a brief presentation of the audiovisual CS. The compound-X-ray group received a 20-min presentation of the compound followed by exposure to X rays. Three such pairings were administered. The compound-LiCl group also received a 20-min exposure to the compound in which the toxic LiCl was mixed with the solution and provided the salty taste component of the compound. Five such trials were given. Reinforced compound-illness trials occurred every 3 days. The compound-shock condition involved a 2-min trial in which a brief shock occurred 2 sec after the first lick at the compound gustatory/audiovisual CS. Two reinforced and two nonreinforced trials were given each day for 4 consecutive days. All groups received a subsequent elements test. The dependent measure was the amount of solution consumed per minute. Garcia and Koelling found that the gustatory element for the compound-illness groups controlled avoidance of the fluid whereas the audiovisual element did not. They found the opposite result in the compound-shock group. From this crossover data pattern it was concluded that "The cues, which the animal selects from the welter of stimuli in the learning situation, appear to be related to the consequences of the subsequent reinforcer" (Garcia & Koelling, 1966, p. 124).

From this brief description of Garcia and Koelling's procedure, it is clear that there are some obvious avoidable confounds that make hazardous the conclusion that the crossover data pattern was caused exclusively by a difference in the reinforcers. Thus no inference about selective associations can be drawn. The X-ray group received three pairings, the LiCl group received five pairings, and the shock group received eight pairings. Perhaps the relative effectiveness of an element in a compound CS depends on the number of pairings with the US. There is precedent in the literature for this suggestion. For example, when Thomas, Berman, Serednesky, and Lyons (1968) held the reinforcer constant in a serial compound conditioning procedure, they found that the relative effectiveness of CS1 vs. CS2 did change with training. Additionally, in the Garcia and Koelling procedure, duration of the compound and the ISI varied between reinforcer conditions. X rays were administered following 20-min exposure to the compound, LiCl was consumed throughout the 20-min exposure to the compound, and shock was delivered 2 sec after the first lick in a 2-min compound-CS trial. Thus each US type was programmed in a different temporal relation with the compound. Note also that compound exposure for the shock group was shorter than that for the illness groups. Similar avoidable confounds are found in later taste-aversion research and thus these studies too fail to provide the empirical support needed to infer selective associations (e.g., Garcia, Kovner, & Green, 1970; Garcia, McGowan, Ervin, & Koelling, 1968; Gemberling & Domjan, 1982; Green, Holmstrom, & Wollman, 1974; Miller, 1984).

Other authors have pointed out that Garcia and Koelling's study lacked appropriate control groups (Bitterman, 1976; Delprato, 1980). Bitterman (1976) noted the lack of pseudoconditioning controls. As discussed earlier, appropriate nonassociative controls are needed to eliminate nonassociative accounts before any inferences can be made as to the existence of selective associations. Many other studies in the taste-aversion paradigm have also failed to include appropriate controls for nonassociative effects (e.g., Domjan & Wilson, 1972; Garcia et al., 1968; Green et al., 1974; Miller & Domjan, 1981; Wilcoxon, Dragoin, & Kral, 1971).

Testa and Ternes (1977) suggested another plausible interpretation of Garcia and Koelling's results. They proposed that the elements of a compound differ in novelty attributable to normal methods of rearing laboratory rats. Typically, rats are bombarded with a wide range of visual and auditory stimuli but are usually exposed to only one variety of food and tap water. This may make gustatory stimuli such as salt or saccharine highly novel as compared to auditory and visual CSs such as clicker and light. Therefore it may be more difficult to associate illness with auditory and visual stimuli because of the wide range of preexposure to auditory and visual stimuli.

Gemberling and Domjan (1982) tested this novelty hypothesis. By using 1-day-old rats, they decreased preexposure to auditory and visual

stimuli to a level similar to that of the gustatory stimuli. Briefly, for the LiCl groups, 10-min exposure to .5% infused saccharine solution or 10-min exposure to a smooth cardboard floor was followed by an injection of LiCl. For the shock groups, the same CSs were given, but a 1-s shock occurred every 1 min during CS exposure. When they paired a tactile stimulus or saccharine separately with a shock or with an injection of LiCl, they found less intake of saccharine by the group that received saccharine-LiCl pairings than by the group that received saccharine-shock pairings. Contact time with the tactile stimulus for the tactile-shock group was lower than that for the tactile-LiCl group. The use of 1-day-old rats seems to eliminate an account based on the rats' preexperimental history. However, because of avoidable confounds, the crossover data pattern does not force a selective association interpretation. The temporal pattern and number of stimulus-reinforcer pairings were allowed to vary along with the quality of the reinforcer. The LiCl groups received one CS-US pairing, whereas the shock groups received 10 pairings (10 shocks distributed throughout a single CS).

Interestingly, in a second experiment looking at the effects of US delay, Gemberling and Domjan (1982) eliminated the temporal confound just described by having shocks immediately follow the tactile stimulus just as the LiCl immediately followed saccharine. Under this new arrangement, withdrawal behavior evoked by the tactile stimulus was similar to that of a no-shock control group. In other words, when one of the avoidable confounds was eliminated, the crossover data pattern necessary for inferring selective associations (CS1 better associated with US1 and CS2 better associated with US2) disappeared, making an interpretation of the results impossible.

Some of the subsequent research on taste aversion has focused on extending the generality of the phenomenon. For example, Garcia et al. (1968) used size of pellet (large or small) or flavor of pellet (sugar coated or flour coated) as CSs and X ray or shock as USs. They found conditioning to flavor in the X-ray groups and to size in the shock groups. Once again, however, differences in the reinforcer were confounded with differences in procedure. Specifically, the number and temporal pattern of stimulus-reinforcer pairings differed in the two US conditions. The shock US was delivered each time the pellet was picked up by the rat and placed in its mouth within a 60-min session. In contrast, only one X-ray US exposure was given following 60-min access to the CS. The number of times the rats picked up a pellet and received a shock was not reported. Additionally, nonassociative controls were not employed. Another criticism of this study, noted by Bitterman (1975), was that the existence of a stimulus-reinforcer interaction was based on two different dependent measures. When the *amount of food consumed* was the dependent measure in the X-ray condition, flavor was better conditioned than size. When *latency to seize a pellet* was the dependent measure in the shock condition, size was better conditioned than flavor. When this avoidable confound between US type and



dependent measure was removed by using latency rather than amount for the X-ray condition, the crossover data pattern disappeared. The same was true if consumption was used for the shock group. Because the crossover data pattern necessary to infer selective associations was not obtained when the confound between dependent measure and reinforcer type was eliminated, a conclusion about selective associations may not be drawn.

Before leaving taste aversion, I should mention one more thing. A reader not fully familiar with the taste-aversion literature may gain the impression from this review that the exteroceptive stimuli present during conditioning with illness gain no control over subsequent behavior. This is not the case. For example, work by Archer and colleagues has convincingly shown the importance of exteroceptive stimuli (e.g., odors and bottle type) in taste aversion (Archer, Sjöden, & Nilsson, 1985; Archer, Sjöden, Nilsson, & Carter, 1980; Sjöden & Archer, 1988; see also Testa, 1974, 1975).

A possible example of selective association in Pavlovian fear conditioning was presented by Welker and Wheatley (1977). They paired visual stimuli with shock while rats barpressed for food. The stimuli consisted of an increase in light or a decrease in light from a mid-value. More conditioned barpress suppression was found to the increase in light than to the decrease. The same stimuli, however, supported equal rates of discrimination learning when used in an appetitive discrimination task. The authors interpreted these results to mean that "the effectiveness of the specific increments and decrements in luminance levels used in these experiments as discriminative stimuli depends upon the nature of the US or reinforcer, aversive or appetitive" (Welker & Wheatley, 1977, p. 260). Before this conclusion can be made one must show that the procedural details of the aversive and appetitive tasks are identical except for the nature of the reinforcer. This was not the case. The aversive procedure was Pavlovian; the appetitive procedure was operant. Additionally, the visual stimuli occupied half the session in the operant task and much less than that in the Pavlovian task. Thus severe avoidable confounds preclude the conclusion that selective association was demonstrated. Also, the crossover data pattern necessary to infer selective association was not obtained.

Selective associations have also been inferred from experiments using only operant procedures. For example, Foree and LoLordo (1973), using a light + tone compound as a discriminative stimulus in operant appetitive and avoidance procedures with pigeons, found that whether the light or tone element controlled subsequent behavior depended upon the nature of the reinforcer. In the final appetitive training procedure, white houselights were on during the intertrial interval (ITI). The ITI duration was 15 s if no treadle presses occurred, but every response during the ITI extended it 15 s. Following the ITI, a 5-sec trial began in which a red houselight and a tone served as the compound stimulus. If a response occurred, the trial was immediately terminated and grain was

delivered. If no response occurred during the trial, the ITI conditions were reinstated. Many details of the avoidance procedure were the same as those for the appetitive procedure. The ITI stimulus, the compound stimulus, and the minimal ITI duration were identical. If a response occurred during the ITI, the ITI duration was reset to 15 sec. Following the ITI, a trial of at least 5 sec began in which the compound stimulus was presented. If a response occurred within 5 sec of the onset of the compound, the trial was terminated (avoidance) and the ITI was reinstated. However, if a response did not occur, then a brief shock was delivered every 5 sec in the presence of the compound until a response occurred. Given a response, the ITI conditions were reinstated (escape). After training, conditioning to the compound and elements was tested. For the appetitive task, the light element controlled more responding than the tone element. For the avoidance task, the tone element controlled more responding than the light element. Given this crossover data pattern, the authors concluded that "the nature of the reinforcer for the pigeon's treadle-pressing response determines whether the auditory or the visual element of a compound  $S^D$  will exert more control over that response" (Foree & LoLordo, 1973, p. 556).

Before this conclusion can be drawn, it must be shown that everything was held constant except the nature of the reinforcer. A close look at the procedural details reveals several avoidable confounds (i.e., more than the nature of the reinforcer was varied). For example, a response in the ITI had different consequences. In the appetitive procedure, an extension of the ITI may have functioned as a punisher for responding by delaying the food trial by 15 sec, but in the avoidance procedure, resetting the ITI timer to 15 sec may have acted as a reinforcer for responding by delaying the shock trial. Another avoidable confound was that in the avoidance procedure a response was required on 100% of the trials. This was not the case in the appetitive procedure where the ITI was reinstated if a response did not occur during the trial. Varying this factor created other methodological problems for inferring selective associations. For example, the temporal pattern and number of compound-reinforcer pairings varied between conditions. (Avoidable confounds similar to those just described were also present in a study using rats by Schindler and Weiss, 1982.)

LoLordo (1979b), noting that Foree and LoLordo (1973) lacked nonassociative controls, stated that nonassociative explanations of the results seemed unlikely. However, Linwick et al. (1981) examined the implications of not having appropriate controls in a selective association study and empirically demonstrated that a proposed selective association was caused by a difference in the nonassociative effects of two CSs. These authors first trained dogs on an unsignaled shuttlebox avoidance task in which a shock was delivered every 5 sec. If an avoidance response occurred, shock was delayed 30 sec. The dogs were then placed in one of four groups. CS+ groups received pairings of shock with either a flashing light or tone. The dog was restrained and

shock was delivered to the paw. The other two groups served as controls. They received either a flashing light or a tone independent of shock. Following this training, the dogs were then returned to the shuttlebox and rate of responding during the CS was measured. Using a percentage of change in baseline responding as the dependent measure, Linwick et al. found that shock avoidance was facilitated more to tone than to light. However, when the increase in responding in each experimental group was compared to that in its respective control group, the increase in responding in the experimental groups was similar. Linwick et al. (1981) concluded that the difference in responding to the CSs was because of nonassociative effects. From this result, it seems that nonassociative effects can be one likely account of putative demonstrations of selective associations and that proper controls are crucial for an appropriate interpretation. Many studies fail to control for nonassociative interpretations. For a recent example, see Cook and Mineka (1990).

Recognizing the importance of holding all procedural details constant if selective associations are to be inferred, LoLordo and his colleagues in subsequent work focused on eliminating avoidable confounds in appetitive-aversive tasks using pigeons (LoLordo & Furrow, 1976; LoLordo, Jacobs, & Foree, 1982; Shapiro, Jacobs, & LoLordo, 1980; Shapiro & LoLordo, 1982). A similar progression of attempts to remove avoidable confounds can be seen using a wheelturn preparation with rats (Jacobs & LoLordo, 1977, 1980).

However, even this later work failed to meet the three methodological criteria for inferring selective association. For example, using Pavlovian procedures for the appetitive and aversive task, Shapiro et al. (1980) reported a possible example of selective association. In the first experiment, a compound-cue design was used in which the compound was the offset of a white houselight and the onset of a red houselight and a tone. This compound, 5 sec long, was presented 90 times within each of the five 30-min sessions. Compound presentations were separated by 15-sec intervals in which only the houselight was on. For half the pigeons, the compound was followed by a brief shock, whereas for the other half, the compound was followed by food magazine illumination and brief access to grain. Subsequently, testing was conducted to assess the associative strength acquired by the red light and tone alone. Percentage of trials with a peck near or in the food magazine was the dependent measure in the appetitive task. Head raising and side-to-side movements were the dependent measures in the aversive task. In the appetitive task, more pecking near the food magazine was obtained during the red light than during the tone. In the aversive task, more head raising and side-to-side movements were obtained to the tone than to the red light. With some changes in the procedure, a second experiment basically replicated the results using a single-cue design and, in addition, eliminated nonassociative accounts.

A close look at the procedural details, however, reveals avoidable

confounds that call into question the interpretation of the crossover data pattern in terms of selective associations. In the appetitive task, the magazine light was lit upon presentation of grain. This light, followed by response-independent food, established an autoshaping procedure (Brown & Jenkins, 1968; Hearst & Jenkins, 1974) that was present only within the appetitive task. More than the nature of the reinforcer was thus varied between the appetitive and aversive tasks. It is also possible that generalization from the food magazine light to the red light influenced the results (Balsam & Gibbon, 1988). Also, before the results of Shapiro et al. (1980) can be interpreted as demonstrating selective associations, it must be shown that the difference in the behaviors controlled by red light and tone, in both the appetitive and aversive task, reflects a difference in learning and not performance. This was not done.

Finally, a study by Rescorla and Furrow (1977) has been cited as a conclusive demonstration of selective associations (LoLordo, 1979b). In a series of experiments, Rescorla and Furrow simulated both the crossover design and data pattern using, for example, previously conditioned tone and light CSs as reinforcers in a second-order conditioning procedure. They found that a second tone CS became better conditioned when paired with the tone reinforcer than when paired with the light reinforcer. Similarly, a second light CS became better conditioned when paired with the light reinforcer than when paired with the tone reinforcer. Rescorla and Furrow's results clearly demonstrated that the similarity between CS and reinforcer is a factor in second-order conditioning (see also Rescorla & Gillan, 1980). By extension, they render plausible the hypothesis that CS-US similarity may also be a factor in first-order conditioning. Indeed they enhance the plausibility of the idea that CS-US similarity could contribute to belongingness or selective associations in first-order conditioning. They do not, however, constitute a demonstration of selective associations as defined here. Such a demonstration requires conditioning based on different first-order, not second order, reinforcers.

### Concluding Comments

In addition to the required crossover design and data pattern, selective association as defined by LoLordo (1979b) implies several things: First, the behavioral control must be shown to be associative, thus implying the use of appropriate nonassociative control groups. Second, the differences in the behaviors evoked by the target CSs must be shown to reflect differences in learning as opposed to differences in performance. Third, if a change in relative effectiveness of the two target CSs is to be attributed solely to a change in reinforcer, then all other procedural details must be held constant when the reinforcer is changed. If this condition is not met, then the changes in the relative effectiveness of the target CSs could be attributed to the changes in procedure and not to the changes in the reinforcer. The literature abounds with many

fascinating examples of differential effectiveness of stimulus-reinforcer combinations. However, to interpret these results in terms of selective associations may be premature because no study, to my knowledge, has met the three methodological criteria required by the definition of selective association.

Perhaps it seems extremely difficult or even impossible to meet the criteria just previously discussed. I shall therefore describe a hypothetical experiment that I believe will do so. The study is designed to eliminate avoidable confounds, minimize unavoidable confounds, control for nonassociative effects, and test to see if the obtained results are caused by a learning or a performance difference. Because much of the evidence for selective association has come from the taste aversion literature, I have chosen an example that relies on the taste aversion procedure.

Preparation, adaptation, and water deprivation would be exactly the same for all rats. The rats would have surgically implanted an oral cannula for later infusion of fluids (e.g., see Domjan & Wilson, 1972) and an intravenous cannula for later delivery of LiCl (e.g., Dougherty & Pickens, 1973). Following preliminary preparation and adaptation, conditioning would begin. The experiment would use a compound-cue crossover design. Half the rats would receive a compound of tone plus saccharine paired with shock (Group Sh). The other half would receive the same compound paired with toxic intravenous LiCl delivery (Group Li).

More specifically, all rats would receive 2 days of conditioning. On Day 1, all rats would receive an explicitly unpaired US at the same time within the session. For the rats in Group Sh, that unpaired US would be a toxic 4-sec LiCl presentation delivered through the intravenous cannula. For the rats in Group Li, that unpaired US would be a 4-sec 1-mA shock. Rats in both groups would be removed from the chambers after a similar amount of time had elapsed since presentation of the explicitly unpaired US. After the rats in Group Sh recovered from the illness induced by the unpaired LiCl, both groups would then receive the compound paired with the remaining US (i.e., shock for Group Sh and LiCl for Group Li). Following an equal amount of time, the rats in both groups would receive, for instance, a 20-sec simultaneous presentation of the tone and the infused saccharine. Upon termination of the compound, Group Li would receive a 4-sec delivery of LiCl, and Group Sh would receive a 4-sec 1-mA shock. Again, the rats would be removed from the chambers after the same amount of time had elapsed since the US.

Presenting both the explicitly unpaired and paired USs to each group provides a within-groups control for nonassociative accounts of subsequent results (Rescorla & Holland, 1976). The above design also removes typical avoidable confounds. For example, the intravenous presentation of LiCl permits the ISI for both groups to be equated. However, the CS-UR interval will still be unavoidably confounded. Also, the experimenter does not have to remove the rat from the chamber to administer the LiCl, thereby eliminating the typical between-groups

confound of US presentation in different contexts. Other procedural details such as the number and temporal distribution of CS-US pairings are also equated.

Conditioning would be assessed by giving half of the rats a standard two-bottle test. Evidence for conditioning only to tone in Group Sh and only to saccharine in Group Li would constitute necessary but not sufficient evidence for selective associations.

Before an interpretation in terms of selective associations could be drawn, it would be necessary to show that the results were caused by a difference in learning rather than a difference in performance. To this end, the other half of the rats that did not receive the two-bottle test would receive a simultaneous second-order conditioning procedure. For both groups, half the rats would receive a novel light simultaneously paired with the tone. The other half would receive the novel light simultaneously paired with infusion of saccharine. A group of rats (Group C), surgically prepared in the same manner as all other rats, would be added as controls for second-order conditioning. Before this phase, the rats in Group C would receive the CSs (tone and saccharine) and USs (shock and LiCl) explicitly unpaired. Half of Group C would then receive simultaneous light-tone pairings and the other half would receive simultaneous light-saccharine pairings. Procedural details in the second-order conditioning phase would be the same for all groups.

Subsequently, the associative strength acquired by the second-order light would be assessed by giving all rats a two-bottle test. Both bottles would contain tap water, but a lick to one of the bottles would turn on a brief light. A selective association would be inferred only if the crossover data pattern was obtained during first-order conditioning and only if the tone-light rats of Group Sh and the saccharine-light rats of Group Li showed evidence of conditioning following the second-order procedure. That is, the tone-light rats of Group Sh would have to drink less from the bottle for which licks produced the light than their controls in Group C and less than the rats in Group Sh that had light paired with saccharine. Additionally, the saccharine-light rats of Group Li would have to drink less from the bottle for which licks produced the light than their controls in Group C and less than the rats in Group Li that had light paired with tone.

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