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An Evaluation of Conditioned Inhibition as Defined by Rescorla's Two-Test Strategy

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Rescorla's (1969) two-test strategy for diagnosing conditioned inhibition has been widely accepted by researchers interested in animal learning and memory. Recently, however, several reports have questioned the necessity of the two-test strategy for identifying conditioned inhibition. Moreover, Papini and Bitterman (1993) have criticized previous demonstrations of conditioned inhibition that used the two-test strategy as being inadequate to rule out alternative explanations of behavior often interpreted as evidence of inhibition. The present research addresses some of the problems identified by Papini and Bitterman in previous demonstrations of conditioned inhibition and provides a demonstration of empirical conditioned inhibition using the two-test strategy. Implications for arguments concerning the necessity and sufficiency of the two-test strategy are discussed. © 1997 Academic Press

Since the seminal work of Pavlov (1927), researchers in the field of animal learning and memory have been interested in the nature of conditioned inhibi-

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tory processes. One practical concern that has been generated by this interest is by what means based on a subject's behavior we can infer the existence of a conditioned inhibition process. Consider Pavlov's *operational* definition of conditioned inhibition, in which Stimulus A is reinforced when presented alone (i.e., A+), but is not reinforced when presented in compound with a second stimulus, X (i.e., AX−). Following such Pavlovian inhibition training, if one were to test for behavioral evidence of conditioned inhibition by presenting the putative inhibitor (X) alone and observe little conditioned responding, one could not conclude that the observed lack of responding was indicative of conditioned inhibition. The lack of observed responding could just as reasonably reflect a failure on the subject's part to have learned any relationship between X and the US. In either case (X activating an inhibitory process or a failure to learn about X), excitatory behavioral control by X might be expected to be weak or absent. Because of this difficulty in directly assessing the inhibitory response potential of stimulus, special tests were developed to assess conditioned inhibition.

Rescorla (1969; see also Hearst, 1972) proposed a two-test strategy for behaviorally diagnosing the inhibitory potential of a CS. The two-test strategy requires the passage of both a summation test (initially used by Pavlov to assess inhibition) and a retardation test to identify a stimulus as a conditioned inhibitor. A summation test for inhibition consists of compounding the intended inhibitor (e.g., X in the above example) with a known conditioned excitor. Use of the summation test was based on the expectation that the inhibitory response potential (conceptualized as negative associative value) of a conditioned inhibitor would algebraically summate with the excitatory response potential (conceptualized as positive associative value) of the training excitor (i.e., Stimulus A from our earlier example) or with that of an independently trained excitor (e.g., a transfer excitor such as B). Conditioned inhibition is often inferred when the putative inhibitor (i.e., X) attenuates responding to the excitatory stimulus relative to responding to the excitor when it is presented alone. An advantage to a summation test with a transfer excitor as opposed to the training excitor is that passage of such a test precludes noninhibitory explanations predicated on the subjects learning that A is reinforced and AX, processed as a configured stimulus, is not reinforced (e.g., Williams, 1995). If X's apparent inhibitory control over responding to A was due to AX being configured, then X should not support inhibitory control over B when BX is presented. This is because X and B were not presented together during training and therefore the subject never had the opportunity to configure them. The retardation test consists of pairing X with the US (i.e., X+) following inhibitory training. Excitatory conditioned responding to X by subjects that had received prior inhibitory training with X is then compared to that of subjects that had received no inhibitory training with X. Retardation of acquisition is inferred when X is slower to acquire excitatory control of behavior following inhibition training relative to a situation in which X had not been previously trained as a signal for US omission. Such retardation of acquisition in turn is often assumed to reflect X having accrued inhibition properties. When a stimulus satisfies both the summation test and the retardation test, it is said to be a conditioned inhibitor.

Rescorla's rationale for the two-test strategy of certifying a stimulus to be a conditioned inhibitor is that *both* tests are necessary to preclude alternative explanations of the observed behavior that hinge on changes in attention rather than any sort of inhibitory mechanism. The requirement that both tests for conditioned inhibition (summation and retardation) be passed rests on the logic that each test can rule out alternative attentional explanations that are applicable to the results of the other test. Specifically, the attentional (i.e., noninhibitory) explanation of a stimulus passing a summation test for conditioned inhibition is that the stimulus was not an inhibitor but instead commanded increased attention at the cost of attention to the accompanying test excitor. That is, the target stimulus (i.e., the putative inhibitor) distracted the subject from the excitor, thereby reducing responding to the excitor. In contrast, the attentional explanation of a putative conditioned inhibitor passing a retardation test is that the subject was deficient in attention to the target stimulus. If attention to the target inhibitory stimulus was deficient, learning about that stimulus is apt to have been impaired. Thus, deficient behavioral control by the putative inhibitor is explained without recourse to an inhibitory mechanism. This argument rests on the assumption that attention is necessary for learning to occur. With the further assumption that attention is a unitary variable, it would be impossible for a treatment to both increase and decrease attention to a given stimulus. Hence, Rescorla argued that attentional explanations could be discounted if both tests are passed. The inference was that the elimination of attentional explanations left inhibitory mechanisms as the most plausible explanation of the behaviors observed on both tests.

Until recently, Rescorla's two-test strategy for diagnosing conditioned inhibition has been met with widespread acceptance among researchers working in the associative tradition [but cognitive researchers using human subjects have tended to settle for summation data alone (e.g., Hasher, Stoltzfus, Zacks, & Rypma, 1991; Neill & Westbury, 1987; Tipper, 1985)]. However, recent reviews of the conditioned inhibition literature by Williams, Overmier, and LoLordo (1992) and Papini and Bitterman (1993) have questioned the adequacy of the two-test strategy.

Papini and Bitterman (1993) criticized the two-test strategy in particular, and existing evidence for conditioned inhibition in general. They contend that there are few if any *adequate* demonstrations of conditioned inhibition based on both summation and retardation tests. They suggest that experiments yielding results that have been interpreted as evidence for conditioned inhibition may be more parsimoniously explained in terms of other well-established (and sometimes not so well-established) psychological processes. A second related criticism levied by Papini and Bitterman is directed at the methodological adequacy of the control groups that have been used in the conditioned inhibition literature. Papini and Bitterman correctly note that many published studies of conditioned inhibition have failed to use the same control treatments in summation tests as in retardation tests, and have failed to counterbalance critical stimuli. These methodological failures, they argue, allow interpretations of summation and retardation test performance that do not invoke inhibitory mechanisms.

The present paper focuses on criticisms of prior demonstrations of (empirical) conditioned inhibition levied by Papini and Bitterman (1993). Most of the issues raised by Papini and Bitterman are directed at the control procedures used in such demonstrations. Their criticisms are important, but in fairness it should be noted that some of the studies they criticize as inadequately controlled to demonstrate conditioned inhibition were performed for reasons other than to demonstrate conditioned inhibition (e.g., Miller, Hallam, Hong, & Dufore, 1991). Hence, there is little reason to expect the control conditions they demand of those studies.

As a first concern with existing demonstrations of conditioned inhibition, Papini and Bitterman (1993) point out the necessity for studies of conditioned inhibition that employ the two-test strategy to provide the control groups in summation and retardation test experiments with identical treatments. Although this is a seemingly obvious point, many studies have failed to use identical control treatments in retardation and summation experiments (e.g., Wessells, 1973). One reason for this failure is that, in a summation test, control subjects often receive nonreinforced, pretraining exposure to X (i.e., the putative inhibitor) to ensure that X will not diminish responding to the test-trial excitor as a result of external inhibition. [External inhibition is a nonassociative short-term inhibition of responding that is distinct from conditioned inhibition because it produces temporarily decreased responding to any eliciting stimulus; that is, it is neither CS nor US specific, and depends merely on the novelty and salience of the external inhibitor (Pavlov, 1927).] However, in a retardation test, control subjects usually receive no pretraining exposure to X because such preexposure may result in the retardation of X's acquiring excitatory control of responding on X+ trials due to latent inhibition (Lubow & Moore, 1959). [Latent inhibition is often viewed as a loss of attention to X that is distinct from conditioned inhibition because a stimulus presented during latent inhibition treatment usually fails a summation test for conditioned inhibition (e.g., Rescorla, 1971).]. Such retardation might mistakenly be attributed to the presence of conditioned inhibition. Despite these very real problems, Papini and Bitterman are quite correct in their view that the logic of the two-test strategy for diagnosing conditioned inhibition demands that the control groups in the two tests be given identical treatment to equate experience prior to testing. Identical treatment of the control groups ensures that attention to X will be equivalent at the initiation of testing.

The present research obviated these concerns by (*a*) administering a small number of nonreinforced pretraining presentations of X (and the control stimulus, Y) to all subjects in both the summation and the retardation test experiments and (*b*) using the same control treatment in summation (Experiment 1) and retardation (Experiment 2) tests. The intent of the small number of pretraining presentations was to give enough nonreinforced pretraining exposure to X (and control stimulus Y) to minimize unconditioned external inhibition by X (and Y) during the summation test (Experiment 1). At the same time, this preexposure to X (and Y) was selected to be insufficient to yield latent inhibition when X (and Y) was later paired with the US in the retardation test (Experiment 2). Notably, one of the control groups in the summation study received exactly the same treatment as the control group in the retardation study.

A second criticism of existing demonstrations of empirical conditioned inhibition (defined by the two-test strategy) advanced by Papini and Bitterman (1993) is that these demonstrations have frequently failed to counterbalance critical stimuli. A lack of counterbalancing allows alternatives to inhibitory accounts of data obtained from summation and retardation tests. For example, in the experiments of Miller *et al.* (1991; also see Wessells, 1973), the critical stimuli [the target stimulus and the control stimulus (given to the control group in place of the putative inhibitor)] were not counterbalanced. This allowed the possibility that the experimental group and control group may have differed in the degree to which conditioned excitation generalized to the putative inhibitor. Specifically, the absence of counterbalancing allowed the possibility that the degree of excitation from the excitatory CS of training that generalized to the putative inhibitor may have been smaller in the experimental group than in the control group. This in turn might account for the lower excitatory responding witnessed in the experimental group relative to the control group during the retardation test. The present experiments, in addition to using control subjects that received identical treatment in both summation and retardation tests, fully counterbalanced the physical identity of the critical cues, thereby addressing two of the major complaints of Papini and Bitterman concerning previous reports.

EXPERIMENT 1—SUMMATION TEST

In Experiment 1, a summation test was administered in accord with Rescorla's two-test strategy for determining the inhibitory properties of a stimulus. All animals received Pavlovian conditioned inhibition training with the target CS (i.e., A+/AX−), as well as training with a transfer excitor (i.e., B+) that was later used in summation testing. Three fully counterbalanced auditory stimuli served as the putative conditioned inhibitor (X), the control stimulus (Y) administered during testing of the novel-stimulus control group in the place of X, and the transfer excitor (B). Following conditioned inhibition training with X and transfer excitor training with B, a summation test was administered to assess the inhibitory potential of X relative to Y. Subjects were tested in the following manner. Group TRAN was presented with the transfer excitor alone (B); Group EXP was presented with the transfer excitor in compound with the putative trained inhibitor (BX); and Group CON was presented with the transfer excitor in compound with a neutral stimulus (BY). The task used was conditioned lick suppression. To the extent that X gained inhibitory potential, experimental animals presented with the transfer excitor in compound with the trained inhibitor X (BX) at the time of testing were expected to exhibit less suppression to the compound than the control animals presented with either the transfer excitor alone (B) or the transfer excitor in compound with the neutral stimulus Y (BY).

Method

Subjects

The subjects were 24 male and 24 female experimentally naive, Sprague-Dawley-descended rats raised in our own breeding colony. Body weight ranges were 243–410 g for males and 180–278 g for females. The animals were individually housed in standard hanging, stainless-steel, wire-mesh cages in a vivarium maintained on a 16-h light/8-h dark cycle. Experimental manipulations occurred near the midpoint of the 16-h light cycle. The animals were allowed free access to Purina Laboratory Chow in their home cages. Starting one week prior to the initiation of the study, all animals were progressively deprived of water. By Day 1 of the study, access to water in the home cage was limited to 10 min per day, which was thereafter provided 18–22 h prior to any treatment scheduled for the following day. All subjects were handled three times per week, for 30 s, from the time of weaning (age = 22 days) until the initiation of the study (age = 85–110 days). Subjects were randomly assigned to one of three groups (n = 16) counterbalanced for sex.

Apparatus

Two types of experimental chambers were used. Chamber R was rectangular in shape and measured 30.30 × 8.25 × 12.30 cm (l × w × h). The walls and ceiling were constructed of clear Plexiglas and the floor consisted of stainless-steel rods. The rods of the floor measured 0.48 cm in diameter and were spaced 1.5 cm apart, center to center. The rods were connected through NE-2 neon bulbs, which allowed constant-current footshock to be delivered by means of a high-voltage AC circuit in series with a 1.0-MΩ resistor. Each of six copies of Chamber R was housed in a separate light- and sound-attenuating environmental isolation chest. Chamber R was dimly illuminated by a 2-W (nominal at 120 VAC) houselight driven at 56 VAC. The houselight was mounted on an inside wall of the environmental chest approximately 30 cm from the center of the experimental chamber. Background noise, primarily from a ventilation fan, was 78 dB(C-scale) SPL.

Chamber V was a 22.30-cm-long box in the shape of a vertical truncated-V. The chamber was 26.2 cm high and 21 cm wide at the top, and narrowed to 5.25 cm wide at the bottom. The ceiling was constructed of clear Plexiglas, the end-walls of black Plexiglas, and the sloping sidewalls of stainless steel. The floor consisted of two 25.5-cm-long parallel stainless-steel plates, each 2 cm wide and separated by a 1.25-cm gap. A constant-current footshock could be delivered through the sloping sidewalls and floor of the chamber. Each of six copies of Chamber V was housed in a light- and sound-attenuating environmental isolation chest. Chamber V was illuminated by a 7.5-W (nominal at 120 VAC) houselight driven at 56 VAC. The houselight was mounted outside the experimental chamber on an inside wall of the environmental chest approximately 30 cm from the center of the experimental chamber. Light entered the experimental chamber primarily by reflection from the roof of the environmental chest. The light intensities inside Chamber V roughly matched those inside Chamber R, due to differences in the opaqueness of the walls of Chamber R and Chamber V, which compensated for differences in the intensities of the houselights. These houselights were illuminated throughout the experiment. Background noise, primarily from a ventilation fan, was 78 dB(C-scale) SPL.

Within each chamber, a visual stimulus consisting of a flashing light (0.20 s on/ 0.20 s off) could be presented. In Chamber R, this light was provided by a 25-W bulb; in Chamber V the light was provided by a 100-W bulb. These bulbs were mounted on an inside wall of each experimental chest and were located approximately 30 cm from the center of the chamber. Due to differences in the opaqueness of the walls of Chambers R and V, these two light cues produced roughly equivalent illumination in the two types of chambers. Chambers R and V could be equipped with water-filled lick tubes. When inserted, the lick tube extended about 1 cm forward into a cylindrical drinking recess that was set into one of the narrow Plexiglas walls of the chamber with its axis perpendicular to the wall. Each drinking recess was left–right centered on the chamber wall, with its bottom 1.75 cm above the floor of the apparatus. The recess was 4.5 cm in diameter and 5.0 cm deep. An infrared photobeam was projected across the recess approximately 1 cm in front of the lick tube. In order to drink from the lick tube, subjects were required to insert their heads into the recess, thereby breaking the photobeam. Thus, the duration of intervals during which subjects accessed the lick tube could be monitored. All chambers were equipped with three speakers mounted on the interior walls of the environmental chest. Each speaker could deliver a different auditory stimulus, specifically a 6/s click train, a white noise, or a buzzer. When presented, the auditory stimuli were approximately 8 dB above the ambient background noise of 78 dB(C-scale). All CSs used in the

TABLE 1
Design Summary

Group	Treatment			Tests	
	Preexposure	Conditioned inhibition training	Transfer excitor training	Experiment 1	Experiment 2
EXP	X−, Y−, B−	A+/AX−	B+	BX	X+
CON	X−, Y−, B−	A+/AX−	B+	BY	Y+
TRAN	X−, Y−, B−	A+/AX−	B+	B	

Note. Stimuli X, Y, and B were counterbalanced auditory cues. Stimulus A was a flashing light. ‘‘+’’ indicates reinforcement with footshock. ‘‘−’’ indicates nonreinforcement. ‘‘/’’ indicates interspersion of the trial types preceding and following the slash.

study were 30 s in duration and the US was a 0.5-s, 1.0-mA footshock. On reinforced trials, US onset always occurred at CS termination.

Procedure

The critical aspects of Experiment 1 are summarized in Table 1 and further explained below. For half of the animals in each group, the training context (Context Train) was Chamber R and the testing context (Context Test) was Chamber V. For the remaining animals in each group the training and testing contexts were Chambers V and R, respectively. Use of different contexts for inhibition training and testing assured that the test stimuli had been presented to each group equally often in the test context. This minimized differences between groups in the novelty of the test stimulus with respect to the test context, and thereby reduced the potential impact of such differences upon behavior during testing.

Acclimation. Acclimation to the chambers was conducted on Days 1 and 2. On Day 1, subjects were exposed to Context Train in a 60-min session during which animals were allowed free access to the water-filled lick tubes. On Day 2, subjects received a similar 60-min session in Context Test. During acclimation sessions on both days, all subjects received three nonreinforced presentations of each of the auditory stimuli designated X, Y, and B, which later served as the target stimulus, the nontarget stimulus used for control purposes, and the transfer excitor, respectively. Stimuli X, Y, and B were the click, white noise, and buzzer, counterbalanced within groups. Presentations of the three stimuli were interspersed, with intertrial intervals of approximately 6.5 min. The purpose of this modest number of pretraining exposures was to minimize the novelty of the stimuli, thereby reducing any possible effects of external inhibition without creating (as a result of excessive pretraining exposures) a latent inhibition effect on the retardation test to be administered in Experiment 2.

Conditioned inhibition training. Following acclimation, the lick tubes were removed from all chambers. Conditioned inhibition training was conducted in Context Train on Days 3–7 and Days 10–11. Each session was 60 min in duration and consisted of four reinforced presentations of the flashing light, which served as the training excitor (i.e., A+). Pseudorandomly interspersed among these 4A+ trials were eight nonreinforced presentations of the training excitor in simultaneous compound with an auditory CS (i.e., AX−). Intertrial intervals were approximately 5 min.

Transfer excitor training. All subjects received transfer excitor training in Context Test on Days 8 and 9. On each day there was a 60-min session consisting of four reinforced presentations of the transfer excitor (i.e., B+). Intertrial intervals were approximately 15 min. Lick tubes were not available to subjects during these sessions.

Reacclimation. On Days 12 and 13, the water-filled lick tubes were reinserted and daily 60-min reacclimation sessions were administered in Context Test. The rationale underlying these sessions was to reestablish a stable rate of licking for all subjects prior to testing. During these sessions no discrete stimuli were presented.

Summation test with transfer excitor. On Day 14, the potential of X to pass a negative summation test for inhibition was assessed in Context Test. Use of Context Test assured that each subject had identical experience with B, X, and Y in the text context prior to the test itself. Groups EXP and CON were tested for conditioned suppression of ongoing licking in the presence of a compound stimulus consisting of the transfer excitor and the putative inhibitor or the transfer excitor and the control stimulus, respectively (i.e., BX or BY). Group TRAN was tested with the transfer excitor alone (i.e., B). The duration of the testing session was 11 min. Following each subjest's completion of its first 5 cumulative s of licking after placement in the chamber, the test CS (either BX, BY, or B) was presented. Pre-CS times to complete this first 5 cumulative s of licking and times to complete an additional 5 cumulative s of licking in the presence of the test CS were recorded. A ceiling of 10 min was imposed on times to complete 5 cumulative s of licking in the presence of the test stimulus.

Reacclimation. In preparation for a second summation test to be conducted on Day 16, animals were administered a reacclimation session in Context Train on Day 15. This session was identical to the previously identified reacclimation sessions except that it occurred in Context Train rather than Context Test.

Summation test with conditioned inhibition training excitor. On Day 16, a summation test was administered in Context Train with the flashing light (i.e., A) serving as the excitatory CS. That is, on Day 16, a summation test was conducted using the excitor that had been employed during conditioned inhibition training. A randomly selected half of the subjects in Groups EXP, CON, and TRAN was tested for suppression of ongoing licking in the presence of the training excitor alone (i.e., A), whereas the remaining animals were tested in the presence of the training excitor in compound with the intended inhibitor (i.e., AX). Thus, these summation test conditions were counterbalanced with respect to the test conditions that prevailed on Day 14. Testing was procedurally the same as the prior summation test except for the stimuli presented and the fact that the test occurred in Context Train. The rationale for administering the test with Stimulus A in Context Train rather than Context Test was that unpublished data from our laboratory had determined that behavioral control by light CSs (e.g., A) did not transfer well between contexts, whereas behavioral control by auditory CSs did.

Prior to statistical analysis, all lick suppression data were converted to log (base 10) times to improve the normalcy of the within-group data, thereby better meeting the assumptions of inferential parametric statistics. An alpha level of .05 was adopted for all tests of statistical significance.

Results and Discussion

Conditioned suppression to both the transfer excitor and the training excitor was attenuated by the presence of the putative inhibitor (Group EXP), but not by the presence of the relatively neutral cue (Group CON). Thus, Pavlovian conditioned inhibition training imbued Stimulus X with the potential to pass a summation test for conditioned inhibition.

An equipment failure on Day 14 resulted in the elimination of four animals, three from Group TRAN and one from Group CON, from the following analyses. On neither Day 14 nor Day 16 was any difference between groups observed in times to complete the first 5 cumulative s of licking (i.e., prior to CS onset), $F\text{s} < 1$. This indicates that the three groups did not differ in their propensities to lick within each context during the tests. A one-way analysis of variance performed on the Day 14 summation test times to complete 5 cumulative s of licking in the presence of the test stimuli revealed an overall effect of test condition, $F(2,41) = 6.87$. Planned comparisons using the overall error term from the analysis of variance were conducted. As suggested by the left side of Fig. 1, Group EXP showed less suppression of ongoing licking than Group TRAN, $F(1,41) = 4.27$, or Group CON, $F(1,41) = 13.11$. Although of less consequence, Group CON's suppression to BY did not differ significantly from Group TRAN's suppression to B, $F(1,41) = 2.53$. Notably, full counterbalancing of X, Y, and B resulted in Groups EXP and CON being tested with the same physical set of auditory cues. Consequently, the difference between Groups EXP and CON was a result of the different training histories with respect to X and Y.

Analysis of the Day 16 summation test data showed that subjects tested with Stimulus A alone exhibited greater suppression of licking than those subjects tested with the AX compound, $F(1,42) = 4.60$ (see the right side of

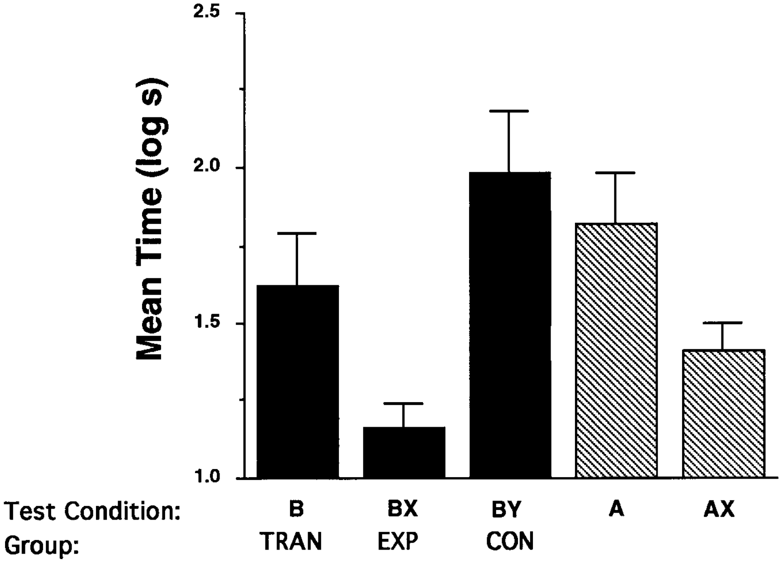

FIG. 1. Summation test data from Experiment 1. Solid bars represent data from the summation test with transfer excitor (B). Depicted are mean time to complete five cumulative seconding of licking in the presence of the transfer excitor alone (i.e., Group TRAN), the transfer excitor in compound with the putative inhibitor (i.e., Group EXP), or the transfer excitor in compound with a neutral stimulus (i.e., Group CON). Striped bars represent pooled data from Groups EXP, CON, and TRAN on the summation test with the training excitor (A). Depicted are mean delays to complete 5 cumulative s of licking in the presence of either the training excitor alone (i.e., A) or the training excitor in compound with the putative inhibitor (i.e., AX). Half of the subjects in each group were tested on A alone and the remaining subjects were tested on AX. Brackets represent standard errors.

Fig. 1). Thus, Stimulus X reduced suppression to both the conditioned inhibition training excitor (A) and the transfer excitor (B) in Group EXP.

EXPERIMENT 2—RETARDATION TEST

Experiment 2 was conducted in order to assess the potential of a stimulus subjected to Pavlovian conditioned inhibition training to pass a retardation test for conditioned inhibition (see Table 1). Experiment 2 was procedurally identical to Experiment 1 from Day 1 through Day 11, except that there was no Group TRAN. The difference in treatment between Experiments 1 and 2 was that, following conditioned inhibition training in Experiment 2, subjects received a retardation test. This consisted of excitatory training with the putative inhibitor (i.e., X) or an initially neutral control stimulus (Y), followed by testing for excitatory suppression to X or Y to determine if Group EXP was retarded (relative to Group CON) in exhibiting conditioned suppression.

Consistent with the logic underlying the retardation test for conditioned inhibition, less conditioned suppression to X by Group EXP than to Y by Group CON would be indicative of Pavlovian inhibition training having made X an effective inhibitor as defined by the two-test strategy.

Method

Subjects

The subjects were 24 male and 24 female experimentally naive, Sprague-Dawley-descended rats. Body weight ranges were 248–503 g for males and 211–302 g for females. Animals were assigned to one of two groups (n = 24), counterbalanced for sex and experimental chamber. The animals were housed and maintained as in Experiment 1.

Apparatus and Procedure

The apparatus was as described in Experiment 1, and the critical aspects of the procedure are summarized below and in Table 1. Apparatus acclimation with CS preexposure (Days 1 and 2), conditioned inhibition training (Days 3–7 and 10–11), and transfer excitor training (Days 8 and 9) were identical to the procedures used for Groups EXP and CON in Experiment 1. The transfer excitor (B) was not an integral part of Experiment 2, but B–US pairings were administered on Days 8 and 9 in order to equate pretest experience between Experiment 1 and Experiment 2.

Retardation training. On Day 12, Group EXP received 3 X–US retardation test pairings, whereas Group CON received 3 Y–US pairings. This training occurred during a 60-min session conducted in Context Test. Although inhibition training had not been administered in Context Test because the two groups had received different inhibition treatments and we did not want groups to have different histories within the test context, retardation training was given in Context Test because all subjects received the identical treatment during this phase. Trial onsets occurred at 10, 25, and 40 min into the session. Lick tubes were not available during retardation training.

Reacclimation. Reacclimation occurred in Context Test on Days 13 and 14 and was identical to the reacclimation treatment of Days 12 and 13 in Experiment 1.

Retardation test. Retardation testing on Day 15 was identical to that of Day 14 in Experiment 1 except that the test CS for Group EXP Stimulus X and for Group CON was Stimulus Y. All animals were tested in Context Test for suppression of ongoing licking in the presence of Stimulus X or Y.

Results and Discussion

Pavlovian conditioned inhibition training with X prior to X–US pairings resulted in retarded conditioned responding to X by Group EXP, relative to conditioned responding to Y by Group CON.

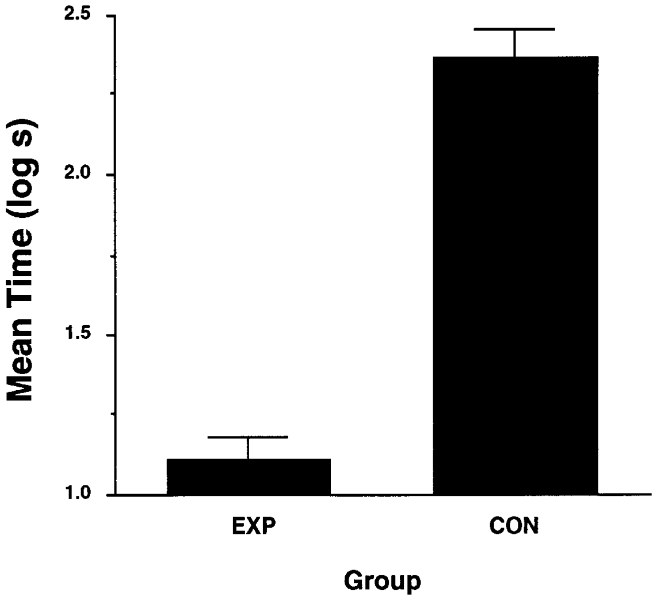

FIG. 2. Retardation test data from Experiment 2. Mean time to complete 5 cumulative s of licking in the presence of Stimulus X after X–US pairings (Group EXP) or in the presence of Stimulus Y after Y–US pairings (Group CON). Stimulus X had been previously subjected to Pavlovian inhibition training (i.e., A+/AX−), whereas Stimulus Y was a neutral stimulus. Brackets represent standard errors.

During the course of Experiment 2, four animals became ill (one from Group EXP and three from Group CON) and were thus excluded from the following analyses. No difference was observed in Day 15 times to complete the first 5 cumulative s of licking (i.e., prior to CS onset), $F(1,42) = 2.58$. This indicates that the two groups did not differ in propensity to lick in Context Test. A one-way analysis of variance conducted on Day 15 times to drink in the presence of the CS (i.e., retardation test times) revealed that animals that received inhibitory training with the CS used in later testing (i.e., Group EXP) showed less suppression of ongoing licking than animals that had not received inhibitory training with the stimulus used in the subsequent test (i.e., Group CON), $F(1,42) = 117.30$ (see Fig. 2). Full counterbalancing of X, Y, and B precludes this difference being a product of physical differences between X and Y. Consequently, the difference in responding arose from Stimulus X, but not Y, having undergone Pavlovian conditioned inhibition training.

GENERAL DISCUSSION

Both the summation and retardation tests yielded results consistent with the existence of some inhibition-like process. In Experiment 1, following conditioned inhibition training with Stimulus X, subjects exhibited less conditioned suppression to the BX compound (i.e., Group EXP) than to B alone (i.e., Group TRAN) or the BY compound (i.e., Group CON). In Experiment 2, following conditioned inhibition training with Stimulus X, retardation test pairings of X and footshock (or Y and footshock) resulted in retarded responding to X (i.e., Group EXP) relative to responding to Y (i.e., Group CON). Thus, Pavlovian conditioned inhibition training resulted in X passing both summation and retardation tests for conditioned inhibition. These results were obtained despite the physical stimuli serving as cues being counterbalanced and the same treatment being administered to control (CON) groups in the summation and retardation experiments. Consequently, this demonstration of empirical conditioned inhibition is not the result of these factors that have plagued many prior studies (Papini & Bitterman, 1993). We interpret these results as a clear demonstration of *empirical* conditioned inhibition as defined by the two-test strategy using Pavlov's A+/AX− procedure.

In the following discussion, we focus on the efficacy of using the two-test strategy as a means of diagnosing *empirical* conditioned inhibition. Although many (not all) researchers would agree that meeting the criteria of the two-test strategy for diagnosing conditioned inhibition is sufficient to label a stimulus an empirical conditioned inhibitor, some would reject the necessity of the two-test strategy as an empirical definition of conditioned inhibition. These objections typically hinge on the supposition that summation tests are unnecessary with respect to diagnosing empirical conditioned inhibition, because the noninhibitory explanations that summation tests are designed to assess can supposedly be rejected on purely logical grounds. We now consider one such argument.

Papini and Bitterman (1993) rejected summation tests as evidence of inhibitory processes in part because they believe all inhibitory summation data can be readily explained by the putative inhibitor distracting subjects in the experimental group from the excitor used on the summation test. Presumably, inhibition training increases a cue's potential to command attention. Thus, on a test with the BX compound, distraction from B by X is greater in the experimental group than the control group, which does not receive inhibition training with the target stimulus. Papini and Bitterman go on to argue that the only compelling published evidence of inhibitory processes comes from retardation tests. However, a reliance on retardation data for evidence of conditioned inhibition requires some basis for rejecting the attentional interpretation of retardation test performance. The standard attentional interpretation of a retardation test is that attention to the target stimulus in the experimental group was decreased, prior to the retardation pairings by the inhibition treatment, more than in the control group. (This is an interpretation that summation tests are commonly used to preclude.) In order to support their view that properly controlled retardation tests alone can detect inhibition, Papini and Bitterman cite studies that used inhibition training procedures that they believe were unlikely to have reduced attention to the putative inhibitor (e.g., A+/AX−).

However, this preference for retardation test data depends strongly on accepting their assumption that the conditioned inhibition treatment in question did not reduce attention to the putative inhibitor. We hesitate to accept this assumption because it is based entirely on subjective views concerning the plausibility of arguments that the inhibition treatment could not have decreased attention to X. Summation tests, which Papini and Bitterman reject as being unilluminating, in some instances (including the present research) may provide evidence for rejecting a reduced attention explanation of retardation test data. Hence, it appears that the two-test strategy is not as easily replaced by retardation tests alone as Papini and Bitterman suggest.

As noted previously, Papini and Bitterman (1993) find fault with many prior demonstrations of (empirical) conditioned inhibition defined by the two-test strategy because the control groups used in those summation and retardation tests have frequently received different treatments, and because there was often a failure to counterbalance critical stimuli (i.e., trained inhibitors, control stimuli that are not subject to conditioned inhibition training, and excitors). We believe that if one is to use a single control group, a good conservative treatment would be one identical to that of the experimental group except for a ''nontarget'' stimulus being substituted for the putative inhibitor during the inhibition training phase of the study. Such a control group treatment ensures equivalency with the experimental group in terms of factors other than those directly involving the target CS. Of course, it does not differentiate associative and nonassociative factors directly involving the CS, but doing that is a central rationale for the two-test strategy.

In the present research, Groups EXP and CON received identical treatment during training, and identical treatment in testing except that during testing for Group CON Stimulus Y was substituted for X in both Experiments 1 (summation test) and 2 (retardation test). The fact that Group CON received exactly the same treatment in Experiment 1 (summation test) and Experiment 2 (retardation test) obviates Papini and Bitterman's (1993) well-founded concern about prior studies using different control treatments for retardation and summation tests. With respect to the issue of counterbalancing, the critical stimuli (i.e., X, Y, and B) were fully counterbalanced in the current study, thereby discounting the view that differences in generalized excitation from one physical stimuli to another contributed to either summation test performance or retardation of acquisition.

Although the present research squarely confronts two concerns raised by Papini and Bitterman (1993; equating treatment for control groups and counterbalancing of cues), other points that they discuss are not so easily addressed. In the spirit of Papini and Bitterman's thesis, the current summation and retardation data can be explained without invoking inhibitory constructs. Their analysis suggests that summation test performance could be attributed in part or in total to greater distraction from B by X by Group EXP than by Y by Group CON. However, such distraction could not explain retardation test performance because greater attention to X in Group EXP than to Y in Group CON should have either facilitated or not affected acquisition of behavioral control by X, but should not have retarded acquisition. Papini and Bitterman's analysis further suggests that external inhibition by X of baseline licking, and generalization of excitation from A and B to X (both of which they believe would be reduced relative Stimulus Y, due to exposure to X but not Y during conditioned inhibition training) could have contributed to both the summation and the retardation test performance indicative of conditioned inhibition.

First, we examine the plausibility of external inhibition as an explanation of the observed performance on the present summation (Experiment 1) and retardation (Experiment 2) tests. Papini and Bitterman (1993) contend that external inhibition by a putative inhibitor can suppress baseline responding (licking in the current experiments) in control subjects and is less apt to do so in conditioned inhibition-trained subjects because, during conditioned inhibition training, the putative inhibitor loses its novelty and hence presumably loses its potential to externally inhibit ongoing behavior. This can explain why in Experiment 1 (see Fig. 1) there was more suppression to the BY stimulus compound by Group CON than to B alone by Group TRAN or to BX by Group EXP (i.e., more external inhibition of licking by a relatively novel Stimulus Y than a Stimulus X that had been presented in conditioned inhibition treatment). However, external inhibition induced by Y in Group CON does not explain why there was less suppression to BX in Group EXP than to B alone in Group TRAN.

Another of Papini and Bitterman's (1993) explanations of the current data would rely on what they refer to as reduced generalization of excitation. They might contend that, after inhibition training, Group EXP showed less suppression to the BX compound than did Group CON to the BY compound (i.e., X passed a summation test), and slower acquisition of excitatory behavioral control by an X–US association than by a Y–US association (i.e., X passed a retardation test), due to nonreinforced exposure to X during inhibition training. This argument rests on the assumption that inhibition training with X reduces generalization of excitation from A and B to X, but not from A and B to Y. That is, exposure to a stimulus (such as that which occurs during conditioned inhibition training) is assumed to narrow the generalization gradient around it. On a summation test, generalized excitation to X presumably results in more responding to BY by Group CON than to BX by Group EXP. On a retardation test, generalized excitation to Y presumably gives Y a ''head start'' in acquiring behavioral control relative to X. However, the viability of explanations of summation and retardation that are dependent uniquely on differences in generalization of excitation to X and Y are rarely plausible in light of an additional control group often included in studies of conditioned inhibition, but not emphasized by Papini and Bitterman. Specifically, most studies of conditioned inhibition that use a summation test include a group tested with the transfer excitor alone (e.g., Group TRAN in Experiment 1) as well as groups tested on BX and BY. Explanations predicated on X possessing less generalized excitation training following inhibition training, although they potentially can explain why responding to BX (i.e., Group EXP) is less than responding to BY (i.e., Group CON), cannot explain why responding to the BX compound is also less than responding to B alone (i.e., Group TRAN; see Fig. 1). Explanations of differences in responding to BX and BY based on inhibition training with X reducing or eliminating generalization of excitation to X relative to Y predict stronger or at least equal suppression to BX in Group EXP than to B alone in Group TRAN because whatever generalization of excitation to X in Group EXP remains after inhibition training might be expected to summate with the excitation evoked by B.

Papini and Bitterman (1993) argue reasonably that the concept of conditioned inhibition is superfluous if all the behavior that it explains can also be explained through other mechanisms or combinations of mechanisms that we are inclined to accept as existing because the latter mechanisms are necessary to explain various well-established noninhibitory behavioral phenomenon. Among these alternative explanations are: (*a*) increases and decreases in attention to the putative inhibitor, (*b*) latent inhibition to the putative inhibitor, (*c*) habituation to the US during inhibition training, (*d*) blocking of the putative inhibitor by contextual cues on the inhibitor–US pairings of a retardation test, (*e*) generalization decrement on summation tests between transfer excitors and compounds of the transfer excitors and the putative inhibitor, (*f*) differences in generalization of excitation from the inhibition training excitor and the transfer excitor to the putative inhibitor, (*g*) configural learning, (*h*) differential external inhibition (which presumably is not conditioned inhibition) as a function of prior exposure to the putative inhibitor (which could either depress the conditioned response or depress the baseline behavior in a suppression experiment, creating the illusion of increased conditioned responding), and (*i*) comparator processes (e.g., Miller & Matzel, 1988). [Notably, (*a*) and (*b*) collapse into a single factor if latent inhibition is viewed as a decrease in attention to the target stimulus, a popular but not universally held position.] Each of the factors (*a*) through (*i*), depending on procedures and parameters, can contribute to behavior often interpreted as evidence of inhibition without recourse to any inhibitory constructs (i.e., negative associations, CS–no US associations, elevated response thresholds).

The combined explanatory power of this cluster of phenomena is considerable, and the necessary control groups (or conditions) to reject all of these alternatives is so large that no published study has encompassed them all. The question is whether a single inhibitory process provides a more parsimonious explanation of behaviors often interpreted as evidence of inhibition than any such cluster of alternative processes. In terms of Rescorla's data alone, the answer to this question is undoubtedly yes, inhibition provides a more parsimonious explanation. But if the complete literature concerning Pavlovian conditioning is considered, accepting inhibition as a fundamental theoretical process in its own right fails to eliminate the need for selective attention, external inhibition, habituation to the US, and the other previously described noninhibitory processes to explain various reliable phenomena that are outside the domain of inhibition. Thus, despite our demonstration of empirical inhibition as defined by the two-test strategy, it remains questionable whether such a demonstration necessitates the acceptance of the theoretical construct of conditioned inhibition.

Papini and Bitterman's (1993) preference for parsimony of concepts and processes is commendable, but many demonstrations of conditioned inhibition cannot be explained by any single one of these alternative concepts. Often two or more alternative processes must be invoked, especially when both summation and retardation tests have been performed (see Papini & Bitterman). As the number of alternative processes that must be assumed to act in concert grows large, parsimony is lost. Our present data can be explained by either an inhibitory mechanism or by a combination of differences in generalized excitation to the putative inhibitor *and* external inhibition, but not by either of these latter two concepts alone. Are such hybrid explanations of summation and retardation test performance sufficiently satisfactory to avoid invoking a conditioned inhibition mechanism? Or are alternative theoretical viewpoints (e.g., comparator theories, negative occasion setting) to conditioned inhibition more compelling explanations of inhibition-like behavior than is theoretical conditioned inhibition per se? The answer to these questions in part hinges on whether any theoretical alternative to conditioned inhibition can withstand the rigor of empirical evaluation and provide a more viable account of behaviors often viewed as evidence of inhibition than that provided by an inhibitory mechanism per se.

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