PAVLOVIAN CONDITIONING AND THE RANDOM CONTROL PROCEDURE

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by

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This thesis is concerned with control procedures employed in Pavlovian conditioning, in particular, the "truly random" control. Pilot work indicated that this procedure employed in a CER paradigm resulted in response suppression. Our first objective then was to examine this finding in a more formal experiment, as well as attempting to determine the cause of the observed suppression. In a second experiment, a variable predicted to affect the amount of conditioning within traditional conditioning theory was employed to determine the possible role of this variable in suppression produced by the random control procedure.

It was found, in the first experiment, that two variations of the random procedure resulted in suppression in testing. The results of a third group indicated that the suppression was not due to a between-session discriminative function of the CS. A fourth group demonstrated, within contingency logic, inhibitory control, but this group, like the others, showed suppression.

In the second experiment, variations of stimulus conditions, none of which made the US contingent on the CS, resulted in marked differences in suppression in testing. Further, significantly different recovery rates of the operant baseline were noted both as a function of the recovery condition and of the CS employed in training.

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CHAPTER ONE

Historical Introduction

The problem with which this thesis is primarily concerned is the "random control" procedure for classical conditioning, as described by Rescorla (1967). The classical conditioning procedure as conceived by Pavlov (1927) may be thought of as providing an index of association. Pavlov's experiments were so arranged as to have a neutral stimulus presented in close temporal contiguity with an unconditioned stimulus. "After several repetitions of the combined stimulation", the previously neutral stimulus acquired approximately the same response-evoking properties as the unconditioned stimulus. The acquired ability to evoke a response evidences the formation of an association between the previously neutral stimulus and the unconditioned stimulus. Response probability, or quantitative variation of some aspect of the response, serves as an index of associative strength.

It is of critical importance to note at the outset that response probability may be altered by non-associative factors inherent within the Pavlovian paradigm. Presentation of the to-be-conditioned stimulus, or of the unconditioned stimulus, either singly or in some non-contiguous manner, say in itself lead to a subsequent inhibition of facilitation of the criterion response. "Therefore, measures of response strength are an index of strength of conditioning only when those measures of response strength are unencumbered by nonassociative factors." (Harris, 1943).

Presentation of the to-be-conditioned stimulus before conditioning (the "CS alone" procedure) may have either of two consequences. For example, the first presentation of the to-be-conditioned stimulus often evokes an unconditioned response (UR). In most cases, the initial presentation of the stimulus in the experimental session is the subject's first encounter with that stimulus. This response, often termed an orienting response, has been noted by many investigators (Berlyne, 1960). After a few presentations of the stimulus, the subject habituates, and subsequent presentations fail to evoke an orienting response. The effect on subsequent conditioning depends upon whether the response the stimulus evokes is compatible with or incompatible with the conditioned response which will subsequently be developed through the Pavlovian paradigm. If the orienting response is incompatible, conditioning will now proceed at an increased rate, as the habituated response can not interfere with the developing conditioned response (CR). On the other hand, if the orienting response is compatible with the to-be-developed CR, then conditioning will proceed at a decreased rate as a result of habituation (Berlyne, 1960). In situations where the orienting response and the CR are similar, and no habituation is provided prior to conditioning, discriminating the nonassociative orienting response from the associative response (CR) is impossible.

Prior experience with the unconditioned stimulus (the "US alone" procedure) may also markedly alter responding, and again in two directions. For example, MacDonald (1946) and Dufort and Kimble (1958) have demonstrated an adaptation effect in human eyelid conditioning when the subject receives prior experience with the US. That is, conditioning proceeds more slowly as a result of this experience.

Prior experience with the US may also have facilitative effects. Sears (1934), conditioning goldfish with a shock US, found that after a number of presentations of the US alone, a previously neutral stimulus would elicit a response similar to the UR to shock. This phenomenon is usually referred to as sensitization. It is obvious that, in the absence of some appropriate control procedure, a sensitized response to a CS cannot be distinguished from a CR. Grether (1938), replicating Sears' results in a different experimental situation, termed the phenomenon pseudoconditioning. Employing monkeys as subjects, Grether found that the effect would be produced with a number of stimuli, and by either of two procedures, ten backward pairings of the CS and the US, or ten presentations of the US alone. Further, Grether found the nonassociatively produced responding to persist for several days.

The essential fact about sensitized or pseudoconditioned responses is that, though in appearance they may be identical to the CR, they cannot serve as an index of the associative process assumed to underlie Pavlovian conditioning. This process depends sensitively upon the temporal relations between CS and US (cf. Kimble, 1961, pp. 155-160), and it is obvious that the responses in question can not be a consequence of contiguous pairings of CS and US.

The presence of nonassociative factors which can facilitate responding presents the researcher with the task of identifying what proportion of the CR, in a given Pavlovian conditioning experiment, is a reflection of associative factors, and what proportion is due to nonassociative factors. To ascertain the contribution of nonassociative factors, the experimenter might attempt to obtain a response measure when <u>only</u> these factors are operating. Response strength in excess of this nonassociative or control level might then be attributed to associative factors. We might note that under some experimental arrangements responding could be <u>less</u> than that observed within the nonassociative control procedure. That is, associative factors might serve to <u>inhibit</u> responding.

Rescorla (1967) has recently reviewed a number of control procedures commonly employed in connection with Pavlovian conditioning, and has pointed to the inadequacy of each of the cited procedures. The "CS alone" and "US alone" procedures have sometimes been employed to control for adaptation and sensitization. Both of these procedures are subject to the same criticism in that they do not afford experience with the remaining stimulus of the pairing. The "novel CS" control is a procedure in which a control group of naive animals is presented with the CS for the first time. Their responding can be compared to that of animals which, after Pavlovian pairings of CS and US, are presented with a test trial of CS alone. However, as Rescorla indicates, this procedure scarcely seems relevant to the assessment of nonassociative effects in the Pavlovian procedure.

There are also a number of procedures in which both CS and US are presented, but in an allegedly "non-associative" sequence. The "explicitly unpaired" control presents the US systematically separated in time from the CS. The "backward conditioning" control reverses the Pavlovian temporal order of stimulus presentation; that is, the US precedes the CS in time. The "discriminative conditioning" control procedure presents a CS-US pairing, and also a second previously neutral stimulus systematically displaced in time from the pairing. Thus, the neutral stimulus displaced from the US might be regarded as an appropriate test for nonassociative factors. All three of these procedures share the same criticism. While they do provide control subjects with experience both with CS and US, they also alter the contigency between the stimuli from a positive to a negative one. That is, the stimuli are so ordered that presentation of the previously neutral stimulus indicates that a US will not occur. To the degree that inhibitory conditioning can occur, the "control" animals might respond less to the CS than would animals presented with a more appropriate nonassociative control procedure. We may conclude as Rescorla (1967) does "that each of the proposed control procedures either confounds some important nonassociative change with the disruption of the CS-US contingency or changes the contingency from a positive to a negative one."[1]

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[1] Rescorla does not discuss, as a control procedure, the possibility of conditioning different groups of subjects with different temporal intervals between CS and US - the traditional "CS-US interval" studies. The demonstration that responding depends sensitively on this interval is perhaps the hallmark of Pavlovian conditioning. It is true, however, that examination of a CS-US interval function cannot tell us at what point responding is equal to that produced by non-associative factors. It is conceivable that, at long CS-US intervals, the CS is inhibitory.

Rescorla (1967), in suggesting an appropriate control procedure, has extended and refined Prokasy's earlier suggestions. According to Prokasy (1965) and Rescorla (1967) the roots of the control problem lie in the traditional "contiguity" view of the conditioning process. The contiguity notion proposes that temporal pairing of CS and US is a sufficient condition for Pavlovian conditioning. This would suggest a control procedure including experience with the CS and the US, but not with their temporal pairing. As was noted previously, these "explicitly unpaired" control procedures, rather than removing the CS-US relationship, establish a negative relationship.

Rescorla suggests that it is primarily because of the neglect of inhibitory relationships that researchers have failed to arrive at an adequate control procedure. Focussing only on excitatory processes, the relevant distinction between "learning that CS is not followed by the US" and "not learning that CS is followed by the US" has not been drawn. Rescorla proposes that an alternative theoretical view of Pavlovian conditioning may be fruitful. The crucial event may not be the temporal contiguity of CS and US, but the contingency between these stimulus events. In place of primary regard for what is occurring in the presence of the CS, the contingency view notes equally as well what is occurring in the absence of the CS. Pavlovian conditioning may then be defined by these two event classes: what events occur during time intervals of CS occurrence, and what events occur during time intervals other than CS occurrence. More explicitly, what is involved is a dependency notion which may be stated in terms of the probability of US given CS, and the probability of US given non-CS. The two probability statements allow for both excitatory and inhibitory properties to be acquired by a CS, and they provide as well a zero point at which no conditioning can take place. It is this zero point, the point where a zero contingency or dependency exists between the CS and the US, that offers the proper control procedure. This condition is achieved by a completely random presentation of the stimulus events. Thus, probability of US given CS is equal to probability of US given non-CS; occurrence of the CS in no way informs the subject of occurrence of the US, and vice versa. Rescorla points out quite explicitly that the response tendencies developed by a subject exposed to such a "truly random" control procedure define the baseline from which either excitatory or inhibitory effects of Pavlovian conditioning must be measured.

Thus, for example, we might compare three animals, each of which has been exposed to a CS 100 times and to a US 100 times. The "truly random" subject would have received CS and US randomly scattered during the experimental sessions. The "excitatory" subject would have received the same distribution of US's, but each US would have been immediately preceded by a CS. The "inhibitory" subject, again with the same distribution of US's, would have received 100 CSs systematically separated in time from the US. If we now test each animal with the CS, and if nonassociative factors are at all involved in producing the criterion response, Rescorla's logic clearly predicts the ordering, from most to least responding, of excitatory, followed by random, followed by inhibitory. Note that the logic does not tell us how much responding, if any, will be observed in the random subject. This is an empirical matter, and it is precisely deviations from this empirical baseline which will allow us to state that associative conditioning - positive or negative - has taken place.

CHAPTER TWO

Experimental Introduction

We turn now to a consideration of the empirical results of recent experiments designed to test the theoretical notions involved in Rescorla's arguments. These experiments characteristically serve a double purpose. First, they are designed to contrast the "contingency" view of conditioning to the "pairings" view. That is, within the truly random procedure, a number of "incidental" forward pairings of CS and US inevitably occur; the contingency view is explicit in stating that, despite the fact that the number of such pairings is equal to the number of pairings sufficient to establish a strong CR within the traditional Pavlovian procedure, no conditioning should occur in the random procedure. Within the random procedure, of course, such pairings are imbedded within a large number of unrelated presentations of CS and US.[2] The experiments are also designed to demonstrate that, using the random procedure as a baseline, both excitatory and inhibitory associative effects can be produced by appropriately manipulating the relation between CS and US.

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[2] The random control procedure obviously makes sense only if a "large" number of CSs and USs are delivered, so that fortuitous associations (or non-associations) are neutralized.

The first such experiment was reported by Rescorla in 1966. Three groups of dogs were employed as subjects in a shuttlebox apparatus. The subjects in all groups were first trained to a stable rate of responding in a hurdle-jumping Sidman avoidance task. The subjects were then confined, for a total of 5 daily sessions, to one side of the shuttlebox, and treated according to group assignment. For all groups, 24, 5-sec. tones were presented randomly during the daily 1-hr. session, such that tone onset was equiprobable at any time in the session. Group R (random) received during each session 24, 5-sec. shocks, programmed on a variable interval schedule with a mean of 2.5 min. Thus, for Group R, there was no systematic relationship between occurrence of the CS and occurrence of the US, though the procedure guaranteed that some fortuitous pairings of the two would occur. Group P (positive prediction) received only those shocks programmed to occur during the 30 sec. following each tone onset. Thus, on the assumption that occurrence of the US within 30 sec. of CS onset constitutes a pairing, all USs received by Group P were paired. (Most of the CSs received by this group were not, of course, paired with a US; in this sense, the group resembles a partial reinforcement procedure.) Group N (negative prediction) received only those shocks programmed to occur at times other than within 30 sec. following tone onset. Thus, within the Group N procedure, occurrence of the CS guaranteed that no shock will occur for at least the next 30 sec.

Pavlovian conditioning and Sidman avoidance training days were alternated until all groups had received a total of seven avoidance (three prior to the first day of Pavlovian conditioning) and five conditioning sessions. Then a single test session was given, during which 24, 5-sec. tones were superimposed on the avoidance baseline with a mean intertrial interval of 2.5 min. The focus of interest is on possible differences among groups in the effect of CS presentation on the rate of Sidman responding. The assumption is made that Sidman avoidance responding is motivated by fear, and that a Pavlovian CS excitatory of fear will increase, while a CS inhibitory of fear will decrease, rate of Sidman avoidance responding.

Prior to presentations of the CS on the test day, all groups were responding at approximately the same rate. In the presence of the CS, the mean response rate increased markedly for Group P, decreased markedly for Group N, and remained unchanged for Group R. In comparing Groups P and R, it should be noted that Group R had experienced at <u>least</u> as many "pairings" as Group P. Nevertheless, it is Group P which responds to presentation of the CS. The Rescorla interpretation is, of course, that in Group P there is no contingency; it is contingency, not mere number of pairings, which is said to produce conditioning. Within Group N, there is a negative contingency between CS and US, and the effect of CS presentation on avoidance responding appears to reflect this appropriately.

We should note that, although empirically the CS had no effect on the avoidance rate of Group R, logically this is not a necessary outcome for regarding Group R as an appropriate control group for associative effects. The present study, however, is not well designed with respect to the "random control" problem. The three groups differ in number of USs received during the conditioning sessions, as well as in the contingency between CS and US. We do not know what effect, if any, number of experiences with the US has on the tendency to jump when presented with an extraneous stimulus in the shuttlebox. Peculiarly, this study, seeking to demonstrate both excitatory and inhibitory associative effects, violates a central tenet of the logic underlying the random control procedure; "All CS and US occurrences for the control group are the same as for the experimental group except that the regular temporal contingency between CS and US is eliminated." (Rescorla, 1967, p. 64). Logic aside, however, it is of interest that the experimental outcomes conform so well to the author's predictions; in fact, a positive contingency produced an increase, a negative contingency, a decrease, and no contingency, no effect, on avoidance responding.

Very recently, after the inception of the present thesis, Rescorla (1968) has reported two studies which replicate and extend these results. The subjects were rats, in a modified Estes-Skinner conditioned emotional response (CER) procedure. The food-deprived subjects first received lever-press training for five days. On the next day, Pavlovian conditioning trials were begun. During this phase of the experiment, the lever was retracted and an aluminum false wall prevented access to the food cup. Conditioning was carried on for five daily, 2-hr. sessions. Group R-1 (random) received on each day 12, 2-min. tone CSs with a mean intertone interval of 8 min. Twelve, 0.5-sec., 0.9-ma. electric shocks were presented on a random schedule throughout each session, such that shock presentation was equiprobable at any time during the session. Group G (gated) received treatment similar to Group R-1 except that all shocks programmed to occur in the absence of the CS were omitted. Thus, both groups received the same number of "pairings",[3] but as Group G received only those shocks continguous with the CS, it received fewer total shocks. Group R-2 was treated exactly as Group R-1 except that it received the same average number of shocks as Group G (approximately 2.4 per session). These shocks were programmed randomly throughout the session, independently of the tone presentation. Thus, Group G was equated with Group R-2 as far as number of shocks was concerned, and with Group R-1 as far as number of pairings was concerned. Following the conditioning sessions, two, 2-hr. bar-pressing sessions were given to assure recovery of a stable rate of responding. (The experience of shock in the apparatus characteristically reduces - sometimes drastically - rate of bar-pressing.) Subsequently, ten, 2-hr. test sessions were given, within each of which four, 2-min. tone CSs, with a mean intertrial interval of 30 min., were superimposed on the lever-pressing base line. No shocks were presented during any of these sessions.

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[3] Note that "pairing" here refers to delivery of a US <u>at any time</u> during the continued action of a 2-min. CS. This is not precisely the type of "pairing" characteristically employed in Pavlovian conditioning, within which there is normally a specific temporal interval between CS onset and US. Further, insofar as Group G is receiving Pavlovian conditioning, the procedure is that of partial reinforcement.

The results were expressed in terms of suppression ratios, comparing response rate during the CS period to the response rate for the same unit of time just prior to CS onset. CS presentation had virtually no effect on Groups R-1 and R-2. In Group G, however, CS presentation produced a marked decrement in responding. The suppression of food-motivated bar-pressing by a fear-eliciting CS is, of course, the characteristic outcome of the CER procedure.

These results seem to strongly support Rescorla's theoretical views. Group R-2 is an appropriate random control for fear-eliciting associative effects operative within Group G. The suppression of bar-pressing produced by the CS in Group G, coupled with the absence of any effect of the CS on Group R-2, allow us to deduce that such associative effects did occur in Group G. Further, though Groups R-1 and R-2 differed substantially in number of shocks received, the CS was similarly ineffective in each of these groups. This lends considerable weight to the observation that, though Groups G and R-1 each had the same number of pairings, only Group G showed evidence of conditioning. It is as if the "extra" shocks received by Group R-1 in some way counteracted the effects of the pairings. More precisely, conditioning was observed only when the probability of the US in the presence of the CS was greater than the probability of the US in the absence of the CS.

This study clearly supported the contingency view, by contrasting the effect of a high degree of contingency with the effect of no contingency whatsoever. Rescorla next proceeded to a parametrically designed study, examining the effects of various degrees of contingency. The basic CER procedure was identical to that just described.

Following lever-press training, the rats were divided into ten experimental groups, each given five daily 2-hr. conditioning sessions. Conditioning was given in a chamber similar to the bar-pressing chamber except that the chamber did not contain a lever or food cup. For all groups, twelve, 2-min. tones were delivered with a mean intertone interval of 8 min. The US was electric shock, 0.9-ma. in intensity and 0.5-sec. in duration. The probabilities of receiving a US per two minute interval for the experimental groups were varied both during CS and non-CS intervals as follows: .4-.4, .2-.2, .1-.1, 0-0, .4-.2, .4-.1, .4-0, .2-.1, .2-0, .1-0.[4] The first number represents the probability of a US during a CS period; the second number represents the probability of a US during a non-CS period. After the conditioning phase of the experiment, two, 2-hr. VI sessions were given to ensure a stable bar pressing rate during the test phase. Six daily, 2-hr. test sessions were then given with four, 2-min. CSs superimposed on the operant baseline. No shocks were delivered during the testing period.

Again suppression ratios during the test sessions were used as an index of conditioning. When the probability of shock was of the same value for CS and non-CS periods, there was little or no suppression. This was true despite the fact that the four groups in which these probabilities were equal differed vastly in number of shocks received -

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[4] It is important to note that a difference in probabilities is correlated with a difference in the number of shocks presented. That is, over the five conditioning sessions, group .4-.4 received 120 shocks while group .2-.2 received only 60 and group .1-.1, only 30.

from 0 to 120! As the probability of shock in the absence of the CS decreased relative to the probability of shock during the CS, a systematic and significant increase in the degree of suppression was noted. There was considerable suppression even when the probability of shock in the presence of the CS was very low, so long as the probability of shock in the absence of the CS was even lower. However, a high probability of shock in the presence of the CS produced no suppression if the probability of shock in the absence of the CS was also high. The families of curves presented by Rescorla strongly support the conception that degree of contingency, rather than number of pairings, is the critical variable.

This study, unfortunately, provides no evidence relevant to inhibitory conditioning, which presumably would occur if the probability of shock in the absence of the CS were higher than in its presence. It is, of course, by no means clear that a fear-inhibitory CS should influence the rate of food-motivated bar-pressing.

The inhibitory aspect of Pavlovian conditioning, in the context of a random control procedure, has been examined by Hammond (1967). The basic procedure, with rats as subjects, was the CER; however, the inhibitory effect of a CS was assessed by examining the degree to which joint presentation of the inhibitory CS with an excitatory CS attenuated the effect of the excitatory CS.

The experimental procedure was as follows. Two groups of rats were first trained, during daily 2-hr. sessions, to bar-press on a VI-1 min. schedule for water reward. The CER training, employing a 3-min. tone, a 3-min. light, and a 0.5-sec., 0.72 ma. shock was then superimposed on operant bar-pressing. There were, for each group on each day, three tones, three lights, and three shocks. The tone, during CER training, was always terminated with a shock.

The experimental groups differed in the relationship of the light stimulus to the tone-shock pairings. For one group (group I), the light stimulus never occurred during a tone-shock trial or during the 3-mins. prior to each tone-shock pairing. For the second experimental group (group R), the light stimulus was presented on a random schedule, independently of the tone-shock pairings. The light stimulus could occur at any time during the 2-hr. session, and presumably occasionally overlapped the tone-shock pairings.

Finally, to test for presumed inhibitory properties acquired by the tone in group I, the light and tone were presented together, without shock reinforcement. If the light (CS^-) was an active inhibitor of fear, then one would expect an attenuation of the suppression which had been conditioned to the tone (CS^+). However, such an attenuation might result from the superimposing of any stimulus over the tone. To control for this possibility of external inhibition or generalization decrement, group R, a random control group, was included. The light, for group R, is assumed to be associatively neutral.

The testing phase was continued for 5 days, on each of which three stimulus compounds of the light and tone were presented without shock.

By the end of the 10 conditioning days, both groups showed almost complete suppression to the tone, and no suppression to the light stimulus. During the testing phase, simultaneous presentation of the tone and light had differential effects for the experimental groups. Throughout, group R was significantly more suppressed than was group I.

Hammond interprets these results as a reflection of the active inhibitory properties of the light in group I, which counteracted the excitatory properties of the tone. The performance of group R during the test phase is assumed to provide an associatively neutral baseline against which to assess inhibitory effects in group I. However, this study appears to contain a very grave, even fatal, methodological flaw. The random programming of lights during conditioning made it highly probable that subjects within group R experienced a few occasions on which joint presentation of light plus tone was paired with shock. This experience could never occur within group I. The CER characteristically develops in a very few trials. Thus, it scarcely seems surprising that, during the test, a group (R) presented with a stimulus complex identical to that which had previously been reinforced should show greater suppression. The difference in performance between the two groups, taken by Hammond to indicate the presence of inhibition in one group, can be plausibly interpreted, instead, as reflecting the presence of excitation in the other group!

Most recently, Jacobs (1968) has attempted to extend the contingency view of conditioning to a situation based upon an appetitive, rather than aversive, US. The Jacobs study, in fact, tests the operant secondary reinforcing properties of a CS which has been paired with food, rather than its response-eliciting properties. However, since the operations involved in establishing a secondary reinforcer are identical to those employed in establishing a CR, this seems a reasonable extension of Rescorla's notion.

Food-deprived rats served as subjects. The experiment consisted of two phases. During Phase 1, three groups each received a total of 3 hrs. of secondary reinforcement training, with procedure varying among groups. During phase 2, all groups received an identical 1-hr. test of secondary reinforcement strength. During Phase 1, the bar was removed from the chamber. Group C (random control group) received 180, 45-mg. food pellets on a VI-1 min. schedule. An equal number of 2-sec. CS (light off) presentations were programmed to occur on a VI-1 min. schedule, independently of the pellet delivery. Group E received treatment similar to Group C except that they received only those pellets programmed to occur within 12 sec. of the CS onset. They thus averaged 36 US deliveries.[5] Group CRF received the same treatment as Group E except that if a pellet had not been delivered by the end of the 12 sec. period following CS onset, one was presented automatically at that time. Thus, for Group CRF, every CS presentation was reinforced. This latter group was included to control for the possibility that a difference in secondary reinforcement strength between Groups E and C might be due to a partial reinforcement effect.

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[5] Feeding procedures were such as to keep hunger drive constant among groups during the test session.

During Phase 2 of this study, the bar was made available for the first time, and each bar-press produced the CS, without further food reinforcement. The number of bar presses emitted by Group E during the 1-hr. test was significantly greater than the number emitted by Group C, and did not differ from the number emitted by Group CRF. The author notes that, in keeping with Rescorla, Group C received at least as many pairings as did Group E: yet it is Group E which demonstrated the secondary reinforcing effect. This finding, of course, supports the contingency view, as opposed to the number of pairings view, of conditioning. Further, Group C appears to be an appropriate random control group for the excitatory associative effects produced within Group CRF; these two groups received, in Phase 1, equal numbers of CSs and USs. The author unfortunately did not include a group in which the CS was deliberately presented temporally separated from food. If the analogy between the appetitive and aversive cases is complete, such a CS should have functioned as a secondary negative reinforcer; animals given such a treatment should have bar pressed less often than did Group C.

The preceding review of the small number of studies which have thus far been conducted within the framework of the random control procedure indicates that they agree in several important particulars. First, in both the aversive and appetitive cases, it appears that a partially reinforced group, with a clear positive contingency between CS and US, exhibits more conditioning than does a random control group, for which "extra" USs destroy the contingency between CS and US, without reducing the number of CS-US pairings. Second, with both the CER and Sidman avoidance procedures, the random control procedure appears to result in a CS which has no effect on the criterion response. Third, in Rescorla's most recent CER study, this is shown to be the case despite wide variation in both total number and probability of shocks delivered within the random control procedure. Fourth, again in the most recent Rescorla study, amount of excitatory conditioning appears to be very smoothly related to the difference between the probabilities of US granted CS and of US granted non-CS. We ought finally to note that despite the fact that the major impetus for the conception of the random control procedure came from a general lack of conceptual ability to handle inhibitory conditioning, evidence focussed on this aspect of conditioning is far less compelling than the evidence for excitatory conditioning. As already indicated, Rescorla's study employing the Sidman procedure does not include an appropriate random control group; and the Hammond study is beset with its own methodological difficulties.

The experiments to be reported in this thesis employ the CER procedure in further studies of the random control procedure, excitatory conditioning and inhibitory conditioning. There appears to be adequate reason (Kamin, 1965) to regard the CER procedure as an especially sensitive index of Pavlovian conditioning, and the procedure is already prominent in studies of the random control. Our interest was particularly aroused by pilot observations in our laboratory which indicated that, at least under some conditions, the random control procedure might produce a CS which <u>did</u> suppress operant bar-pressing.

This finding, as will later be seen, has a number of theoretically relevant consequences.

CHAPTER THREE

Experiment 1 - Introduction

Pilot observations in our laboratory had indicated some moderate suppression in groups treated with a random control procedure. This, as indicated earlier, is quite unlike Rescorla's empirical reports, and our first experiment was designed both to explore the conditions under which such suppression in a random control might be obtained, and to investigate its significance.

The empirical difference may have resulted from a slight difference between our "random" procedure and that employed by Rescorla. In our pilot work, we divided the 2-hr. experimental session into 60, 2-min. intervals, some of which were "filled" with CSs and some of which were not. In distributing USs throughout the session, one and only one shock was allowed to occur in any 2-min. interval. This is a restriction not imposed by Rescorla's procedure, within which any number of USs could occur in a 2-min. interval. In order to determine if the discrepancy in empirical results was a product of this difference in procedures, two groups were included in the present study. For one group, USs were delivered on the restricted basis, only one US being possible in any

2-min. interval. The second group received a "truly random" distribution of USs, with any number of shocks possible in a 2-min. interval.

According to Rescorla's argument, suppression resulting from the "truly random" procedure would have nothing to do with an association between CS and US. Presumably, such suppression would be reflecting a nonassociative process, perhaps sensitization produced by experience with the US. However, the behavioral consequences of the random procedure would be viewed as providing an empirically defined neutral baseline. It is precisely this virtue of the random procedure that allows one to assess an inhibitory associative effect in a CER procedure. A group given the same CS distribution as the random group, but with the US systematically associated with <u>non-CS</u>, should show <u>less</u> suppression than the random group. Such an inhibitory group, with the CS and US "explicitly unpaired", was also included in our design. It should be noted that Rescorla's logic does not tell us whether an explicitly unpaired group should accelerate, decelerate, or maintain its response rate when presented with the CS. The logic simply asserts that, if the random procedure does produce deceleration to the CS, the explicitly unpaired group should not decelerate as much.

An alternative explanation of the observed suppression produced by a random procedure, one based on a kind of associative relationship between the CS and US, may be offered. During the preliminary training neither the CS nor the US is presented. Then, on conditioning days, both the CS and US are presented.

This situation suggests the possibility of an association between CS and US, not based on controlled temporal relations within a session, but depending on a discrimination between sessions. Prior to testing, shock is delivered only in those sessions during which the CS is also presented. Thus we have the possibility of the CS serving as a cue for sessions during which the US is delivered. To assess this possible interpretation, we included a group for which the CS was a continuous background noise acting throughout the 2-hr. session, but present only on days when shock was delivered. If, in fact, this discriminative mechanism is exclusively responsible for the observed suppression in the random procedure, all our remaining groups - random and inhibitory - might be expected to show equivalent suppression.

Experiment I

Method

Subjects

The subjects were 32 experimentally naive male hooded rats between 270 and 340 grams at the beginning of the experiment, supplied by Quebec Breeding Farms.

Apparatus

The apparatus consisted of eight standard operant conditioning chambers. Each chamber was individually housed in a sound-insulating chest. The ceiling, front and rear walls of the operant chamber were constructed of clear acrylic plastic. The two side walls were constructed of aluminum sheeting. One of the aluminum walls contained a bar weighted such that at least 40 gr. of pressure was necessary for depression. A food cup was located to the left of and below the bar. The floor of the chamber was made of stainless grid bars which were connected to a Grason-Stadler shock generator (E1064GS). The US was 0.5-sec. in duration and 1-ma. (nominal) in intensity.

A white noise CS, generated by a Grason-Stadler noise generator (Model 901B), was delivered through a loudspeaker which was located directly below the bar. Noise levels were measured using a General Radio sound level meter (Type 1551-C). An exhaust fan at the side of the isolated chamber provided an ambient level of noise of approximately 55 dbs. The CS increased the total measured noise level to 80 dbs. The CS was 2-mins. in duration.

The output of a two-minute re-cycling clock pulsed two steppers, which were programmed to present the CS and/or the US during a given 2-min. period. The CS, when it occurred, always "filled" a given 2-min. interval. The time of delivery of the US within the 2-min. interval was controlled by a tape timer activated at the beginning of the 2-min. interval.

All programming equipment was located in a room adjacent to the one containing the experimental chambers. Standard Grason-Stadler relays, timers, and counters were programmed to operate and record automatically.

Procedure

Preliminary Training. - After the animals were received from the breeder, they were allowed free access to food and water for three days. On the fourth day, all Ss were deprived for 24 hours. On the following day, a 24-hour feeding rhythm was initiated. This regimen remained in force until the animals were reduced to 75% of their day 4 weight. The animals were stabilized at the 75% weight for two to three days before magazine training was begun.

During magazine training, each animal was assigned to a particular operant chamber. Subsequently, all experimental treatment of that animal was conducted in the assigned chamber only. During magazine training the animals were placed in the operant chamber with free access to the bar. Food pellets (45 mg. Noyes pellets) were programmed to be delivered automatically on a VI-1 min. schedule. Concurrently, the animal could bar-press on a continuous reinforcement schedule. After a maximum of 40 free pellets, the VI schedule of "free" pellets was disconnected, leaving only the bar-pressing contingency in force. The animal then was permitted to bar-press for a maximum of 80 additional pellets. In most cases, animals would bar-press quite efficiently before the entire 40 free pellets were delivered. In such cases, the VI-1 minute schedule of pellet delivery was eliminated, and the animal allowed to bar press for the balance of the 120 pellets. Shaping was provided when necessary.

Subsequent to magazine training, all experimental sessions lasted for 2 hours. On the day following magazine training, the animals were placed in the operant chambers and allowed to bar press on a VI-2.5 minute schedule. This VI training phase is, in our laboratory, normally conducted for five days, providing a reasonably stable rate of responding.[6]

Conditioning. - The conditioning phase, begun on the day following preliminary training, was conducted with the bar covered with a black acrylic cup. There was thus no possibility of bar pressing, or of food reinforcement.

The treatment of groups differed during the conditioning phase. With three groups, the animal was placed into the operant chamber and presented with 20, 2-min. CSs and 20 USs in some quasi-random sequence. Independent quasi-random distributions for the presentation of the CS and the US were generated by a PDP8/S computer. The 20 CSs were, for each of the three groups, distributed throughout 60, 2-min. intervals, with the restriction that no two CS's could be immediately adjacent in time. Thus, a minimum interval of 2-min. between the end of one CS and the onset of another was insured. The distribution of the CSs was different for the five days of conditioning, it was the same for all three groups receiving a discrete CS.

The distribution of the USs differed according to the experimental design. The "Random-Restricted" group was conditioned with the same procedure employed in our pilot work. The computer-generated distributions placed 6 2/3 shocks[7] within the 20, 2-min. CS periods

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[6] Due to an apparatus failure, it was necessary to run three groups in VI for a sixth day. The "Background Noise" group received only 5 days of VI, as it was run at a later date than the other three groups.

[7] In practice, of course, this meant that on some days six of the 20 CS periods contained a shock, on other days seven of the CS periods contained a shock.

of a conditioning session, and 13 1/3 shocks within the 40, 2-min. non-CS periods. The restriction, it will be recalled, provided that only one shock could occur within one of the 2-min. periods. The placement of the shock within the 2-min. period was randomized by still another computer-generated distribution.

The 'Truly Random' group was to receive essentially the same procedure that Rescorla had employed in his research. For this group, another computer-generated distribution randomized presentation of the 20 USs over the 7200 seconds of the 2-hr. session.

The "Explicitly Unpaired" group was to receive an inhibitory contingency, so another distribution was used to place USs randomly in 20 of the 40 non-CS periods. There was again the restriction that only one US could occur in a given 2-min. non-CS period. Its time of occurrence within the period was again randomized.

The fourth experimental group, the "Background Noise" group, received <u>exactly</u> the same distribution of USs as did the truly random group. Except for these two groups, no distribution of USs was ever used more than once. Distributions differed both among the groups and among the five days of conditioning.

These various arrangements thus provided for the following conditions. Three experimental groups were matched in terms of delivery of a discrete CS. The 2-min. CS occurred irregularly 20 times within each of the five conditioning sessions. Further, each of these three groups received 20 USs on each conditioning day. However, the relation of USs to CSs differed among groups. For the Truly Random group, the USs could occur at *any* time; the structure of the design is such that approximately one-third of the USs should occur while a CS is acting, but the CS in no sense predicts the occurrence of the US. For the Random-Restricted group, precisely one-third of the CS periods, and precisely one-third of the non-CS periods, contain a single shock. Again, the probability of shock within each type of period is equated. For the Explicitly Unpaired group, *none* of the CS periods, but one-half of the non-CS periods, contained a single shock. Thus, for these *Ss*, the CS signals a period safe from shock. For the Background Noise group, of course, the constantly present "CS" can provide no information about the occurrence of shocks *within* the conditioning sessions.

Baseline Recovery. - The conditioning phase of the experiment was followed by two days of operant recovery. During these sessions, *Ss* were placed in the operant chambers and allowed to bar-press under the same conditions that prevailed in preliminary training. The operant recovery phase thus assures a stable rate of responding during testing. As was noted previously, it is often the case that experience with a number of shocks reduces - sometimes to a considerable degree - the baseline of responding.

Testing. - During the test phase of the experiment, the 2-min. CS was superimposed on the operant baseline. The CS was presented four times during each test session at 18, 56.5, 95, and 112.5 mins. into the 2-hr. session. Testing was carried out for two days. Shock was not presented at any time during this testing phase.

Measure. - During the testing phase of the experiment, a suppression ratio was used to assess the effect of the CS on ongoing responding. The ratio, described by Annau and Kamin (1961), compares responding during the CS interval with responding during an equal interval of time immediately prior to the CS. The ratio is A/A+B, where A is the number of responses during the CS, and B, the number of responses during an equal interval of time immediately preceding the CS. This suppression ratio may yield values ranging from .00 to 1.00. Complete response suppression during the CS yields a ratio of .00. Alternatively, responding during the CS only and complete absence of responding during the period preceding the CS yields a ratio of 1.00. A ratio of .50 indicates no change in responding during the CS relative to the period preceding CS onset.

Ratios were computed for each animal for each of the eight test trials. Pooled daily ratios were also computed by adding the number of responses during the 4 pre-periods in a test session and comparing this total with the total number of responses made during the 4 CS periods.

Results

Operant Baseline

Table 1 presents the median number of bar presses for all groups on the last day of VI training and on the recovery days. The Kruskal-Wallis H test on the bar presses for the last day of VI yielded no significant effect. Following this last day of VI training the conditioning phase was begun, with the bar out for all groups. Operant recovery followed conditioning. On the first day of operant recovery, the median response rate for all groups was markedly suppressed. An H test on the first recovery day, however, indicated no significant differences between groups. On the second day, all groups showed some recovery of baseline but all groups remained suppressed relative to the last day of VI training. Again, using an H test, no differences were found among the four groups on the second day of recovery. Thus, though recovery was not complete for any group, at no time in the recovery phase (nor on the last day of VI training) were there any significant differences in bar press rates among the experimental groups.

Suppression to CS

Although some groups were markedly suppressed on the first trial, what suppression that did occur, rapidly extinguished. By the second day of testing, excepting for rather slight spontaneous recovery, all the groups were well extinguished. Data from the second test day, therefore, will not be included in the following discussion.

Figure 1 presents the results of the 4 test trials of the first day of testing. Each point represents the median suppression ratio for a particular group as a function of trial of testing. Examining Figure 1, the Truly Random group was clearly suppressed on the first trial. In fact, all of the groups show some degree of suppression, but no group was as suppressed as the Truly Random group. As noted previously, the suppression was

Table 1

Median Number of Bar Presses Per Session, for Selected Days

Group	Day		
	Last Day, VI	First Day Recovery	Second Day Recovery
Random-Restricted	2623	1628	2039
Truly-Random	2423	1381	1637
Explicitly-Unpaired	2277	1226	1991
Background-Noise	2311	1467	1708

transitory and had essentially extinguished for all groups by the fourth trial of the session. The rate of extinction, however, seemed more rapid for some groups than for others. Particularly, by the second trial the Random-Restricted group and the Explicitly Unpaired group had almost completely extinguished suppression.

In reporting the results of his experiments, it has been Rescorla's practice to report only a single suppression ratio. This ratio is a composite of the 4 test trials. That is, in the ratio A/A+B, B represents the total number of responses that occurred during the 4, 2-min. periods immediately preceding CS onset, and A represents cumulative responding during the 8 minutes of CS periods.

We shall look at suppression in two ways. Initially, ratios based only on the first trial will be considered. Then, following Rescorla, data for the first four trials pooled will be considered. Table 2 presents summary data on suppression ratios for the four groups on the first trial, as well as summary data on ratios pooled over the 4 trials of the session.

An H test on the suppression ratios for the first trial was significant ($H = 10.15$, $p = .025$). Further analysis was carried out to clarify the results of the H test. Multiple U tests on the first trial ratios found the Explicitly Unpaired group significantly <u>less</u> suppressed than the Truly Random and the Random-Restricted group (Random-Restricted - Explicitly Unpaired, $U = 8.5$, $p < .02$; Truly Random - Explicitly Unpaired, $U = 2$, $p < .001$). The two control groups, Random-Restricted and Truly Random,

Figure 1. Median suppression ratio, by group, as a function of test trial.

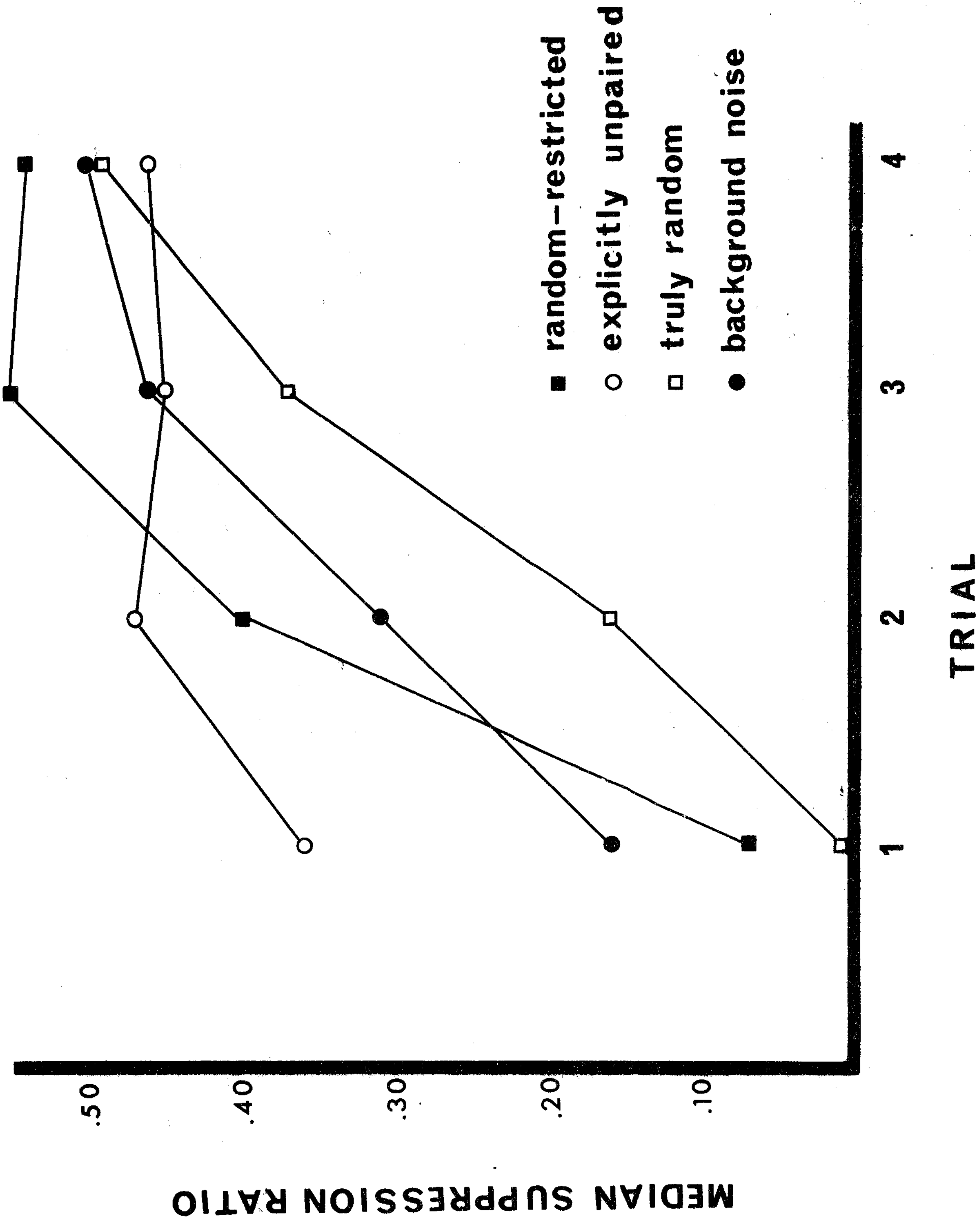

Table 2

Suppression Ratios, by Group, for Trial 1

	Random-Restricted	Truly Random	Explicitly Unpaired	Background Noise
Median	.07	.01	.35	.16
Mean	.11	.03	.29	.16
Range	.00-.29	.00-.09	.06-.42	.00-.26

Suppression Ratios, by Group, for Trials 1-4 Pooled

	Random-Restricted	Truly Random	Explicitly Unpaired	Background Noise
Median	.46	.29	.44	.39
Mean	.44	.26	.41	.38
Range	.36-.48	.02-.36	.21-.64	.26-.51

did not differ. The Background Noise group differed both from the Truly Random group (U = 12, $p < .05$) and the Explicitly Unpaired group (U = 11, $p < .05$).

An analysis of the pooled suppression ratios found a non-significant but suggestive result (H = 6.55, $p \cong .09$). An examination of the data in the lower half of Table 2 further suggests differences among some of the groups. Multiple U tests were used to compare the groups. This analysis indicated that the Truly Random group was significantly different from both the Random-Restricted and the Background Noise groups (Random-Restricted - Truly Random, U = 0.5, $p < .001$; Background Noise - Truly Random, U = 11.5, $p \cong .04$). None of the other comparisons were significant.

The two control groups appear to have shown some spontaneous recovery on the second day. They were slightly suppressed on the first trial of that session (median suppression ratios: Truly Random .28, Random-Restricted .36). As was noted previously however, no group on any other trial of the second testing day displayed suppression; median trial ratios varied between .42 and .57.

Discussion

The first objective of this experiment was to determine if our pilot observations were an artifact of our restricted random procedure. Over both days of the experiment, the Truly Random group was at <u>least</u> as suppressed as the Random-Restricted group. This suppression is obvious both with first trial data and the pooled ratios. The first non-reinforced CS presentation yielded a median ratio of .01 for the Truly Random group. The median pooled ratio for the first day was .29. On the other hand, the Random-Restricted group had a median suppression ratio of .07 on the first trial and a median pooled ratio of .46. While the two control groups did not differ significantly on the first trial, the difference over the first test session, as reflected in the pooled ratios, was in fact significant. The Truly Random group was <u>more</u> suppressed than the Random-Restricted group.

How to account for such a difference is rather perplexing. The difference may be due to the spacing of shocks in the two groups, or to the fact that the number occurring during the CS differed[8], or to yet other variables. As the Truly Random group meets all of Rescorla's strictures regarding the proper control, it shall be the focus of our attention. The question of primary concern, of course, is, <u>why</u> does it show any suppression at all?

We had suggested the possibility of a between-day discrimination, that is, the noise stimulus serving as a discriminative cue for days on which shock would occur. The Background-Noise group was included to assess this possibility. If this, in fact, was the sole function of the CS, then one would expect all groups to suppress equally. The Background-Noise group and the Truly Random

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[8] Although the expected number of shocks occurring during the CS was the same for both groups, viz. 33, the actual number of shock pairings is controlled, not by the experimenter, but by the distribution of random numbers generated by the computer. The Random-Restricted group received 34 "incidental" pairings over the 5 days of conditioning while the Truly Random group had 42.

group had the same distribution of shocks throughout the experiment. On first trial measures, as well as with the pooled suppression ratios, the groups differed significantly. The Truly Random group was more suppressed than the Background Noise group but, the Background Noise group was suppressed.

Why did the Background-Noise group suppress at all? The fact that noise was present during the conditioning days seems to be crucial. Brimer and Kamin (1963), and Brimer (1963) have shown that unsignalled shock depresses the operant baseline, and that if an 80 db noise is then presented, the subject will accelerate responding in the presence of the noise - a disinhibition effect. We thus assume that another group, given the same US distribution as the Background Noise group, but without noise, would not have suppressed to noise in testing. However, the Background Noise group does not appear to have formed a discrimination in the sense we had proposed. On operant recovery days, their baseline of responding was just as suppressed as that of any other group. Thus it appears as though the occurrence of the US in the presence of noise is responsible for this group's suppression to the test stimulus, despite the fact that noise bore no within-session contingency to shock. Further, there is an absence of any evidence of a discrimination between noise days and silence days. That is, presentation of noise seemed to elicit fear on the test day, though the absence of noise did not seem to function as a "safety signal" on the recovery days.

Why does the Truly Random group suppress more than the Background Noise group, though matched to it for US distribution? For Rescorla, the Truly Random group by definition presents a neutral point - any suppression it shows has to be the result of "nonassociative" factors. The Background Noise group being less suppressed than the Truly Random group suggests either that the noise in the Background Noise group had acquired associative inhibitory properties (which seems nonsensical) or that, for some reason, nonassociative factors in the Background Noise group were less potent in producing suppression than in the Truly Random group. One could argue, post hoc, that the Background Noise group's long exposure to the noise reduced the noise's capacity to elicit (nonassociative) suppression. However, one could just as well argue that, due to a number of "incidental" pairings of CS onset and US in the Truly Random group, the CS does acquire some excitatory properties - more so than in the Background Noise group (e.g., the number of times that a US occurs "shortly after" onset of a CS could remain an important determiner of Pavlovian conditioning, even within a Truly Random procedure.)

Turning to the Explicitly Unpaired group, its data can be reconciled with Rescorla's logic. This group was not as suppressed as either the Truly Random or the Random-Restricted groups, although its median suppression ratio on the first trial was below .50. That is, all of the Explicitly Unpaired animals were making fewer responses during the CS than during an equal interval of time just prior to CS onset. The range of suppression ratios on the first trial for this group was .06 to .42. Thus, none of the animals in this group showed an acceleration of responding specific to the CS, but this is not a logical necessity for Rescorla's logic to apply. Rescorla would argue that the Truly Random group reflects the nonassociative factors inherent in the conditioning procedure. Suppression in this group may have its source in pseudoconditioning, sensitization, etc. The suppression in the Explicitly Unpaired group is not as great as the suppression in the Truly Random group, and as such the group reflects an attenuation of suppression. That is, the Explicitly Unpaired group's performance can be viewed as the superimposition of inhibitory associative effects over those nonassociative effects which make for suppression! This, of course, is entirely contingent upon whether or not one accepts the Truly Random group as a neutral point. It is possible that Rescorla's theorizing is incorrect and that the Explicitly Unpaired group is in fact suppressing as a result of weak excitatory associative conditioning to the CS! In an experiment where one group receives 2 CER trials and a second group receives 8 such trials, one would expect a difference in suppression between groups. This difference would be attributed to a difference in excitatory strength, and not to the antagonism of an inhibitory process in the group showing poorer suppression. It is plausible that the same thing is at work in comparing the Explicitly Unpaired group with the Truly Random group. If we assume that the Truly Random group's performance reflects excitatory conditioning, we could assume that the Explicitly Unpaired group's performance also does so, but to a lesser degree. We could view the Explicitly Unpaired group as a form of a trace-conditioning procedure. Unlike the Truly Random group, no shock could occur within 2 minutes of CS onset but some shocks did occur shortly after CS termination. Finally, we should note that Rescorla's logic implies not only that the CS for the Explicitly Unpaired group should be inhibitory of fear, but also that the non-CS should be excitatory of fear, as the probability of US is greatest during non-CS. Again, recovery data does not seem to support this - the Explicitly Unpaired group's baseline is not any more depressed than that of other groups. This fact makes it difficult to regard performance of the Explicitly Unpaired group as supporting Rescorla's views.

Published experiments to date which employ the random control are extremely fortunate in that the CS does not, in fact, have any effect on the criterion response. The Rescorla logic seems to accommodate the data quite straightforwardly. We have just seen that, if the CS does effect the criterion response;[9] interpretation becomes very ambiguous, unless we assert dogmatically that the Truly Random group <u>does</u> present <u>the</u> neutral baseline - and we then, post facto, interpret differences in other groups which go counter to our associative predictions in terms of circularly appropriate nonassociative effects. This is scarcely satisfactory. We have ultimately to answer the question of whether or not the suppression shown by the Truly Random group is associative in origin. The empirical outcome of a random procedure does indeed, make a difference. In the concluding experiment, we shall

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[9] In the final chapter of the thesis, we shall suggest some of the possible reasons why our CER procedure, unlike Rescorla's, does produce suppression within a random control group.

attempt to demonstrate that two random control procedures can produce very different outcomes. We shall attempt both to indicate the problems which this creates for Rescorla's viewpoint, and to interpret the data in terms of Pavlovian conditioning occurring within a random procedure.

CHAPTER FOUR

Experiment II - Introduction

The account of Pavlovian conditioning proposed by Rescorla may be paraphrased to assert that the formation of an "associative connection" between some "neutral" stimulating event (CS) and the US occurs only if, and to the degree that, the probability of occurrence of the US at some particular temporal relation to the CS <u>differs</u> from the probability of occurrence of the US at other temporal relations to the CS. Thus, if a CS is presented regularly at intervals during an experimental session, and if occurrence of the US is scattered randomly throughout the session, conditioning to the CS cannot in principle occur. There is, of course, the possibility that the animal may in fact come to respond in some way to such a CS. The Rescorla account says two things about any such responding. First, such responding is by definition the result of non-associative factors. Second, the amount of such responding provides an appropriate control level for the assessment of Pavlovian conditioning in animals which have received the same amount of experience with the CS and with the US, but for whom presentation of the US was contingent upon presentation of the CS.

The present experiment was designed in such a way that none of the groups to be investigated can, under Rescorla's assumptions, acquire any conditioning to a CS. This is so because, for each of the groups, the same random schedule of delivery of the US will be employed. While for two major groups the turning-on and the turning-off of a white-noise "CS" will occur regularly throughout experimental sessions, occurrence of the US will be equiprobable at all points in the CS on-off cycles.

The two major groups, however, will differ with respect to the duration of the CS on-off cycle. For each group, one-half of the experimental session will be spent in the presence of noise, and one-half of the session will be spent in silence. Thus, within limits of chance, one-half of the shocks received by each group will occur in the presence of noise, and one-half in silence. However, for one group the noise will occur on a regular cycle of 2 mins. on, and 2 mins. off. For the other group, the noise will occur on a regular cycle of 15 mins. on, and 15 mins. off. Within each of these two groups, half of the subjects will eventually be tested to presentation of noise against a background of silence, and half will be tested to presentation of silence against a background of noise. This counterbalancing is necessary since there is no a priori reason why, if an association between delivery of the US and a change in background stimulating conditions should occur, the association should be made to one rather than another change of background state.

The most readily interpretable outcome, from the Rescorla point of view, would be if no suppression is displayed on the test trial by any group. The results of the first experiment suggest that this is an improbable outcome. There would be no real difficulty for the Rescorla conception if the two major groups displayed suppression on the test trial, and if the suppression were equal. The suppression would be viewed as the result of non-associative factors inherent within the training procedure. The problem for the Rescorla conception occurs if the two major groups _differ_ in the amount of suppression on the test. They have had exactly equal experience with the US, and, in at least one important sense, equal experience with the CS. Thus, whatever differences are observed between groups will have, post facto and circularly, to be attributed to differences between groups in the influence of non-associative factors related to the CS. It is not obvious in advance that groups should differ in this respect. However, we will be testing the animals' response to onset of the CS. The 2-min. cycle animals will have experienced many more CS onsets than the 15 min. cycle animals; what we know about habituation thus suggests that, if anything, non-associative factors producing suppression in the 2-min. cycle should be _less_ than those in the 15 min. cycle.

The predictions that seem to flow from more traditional views of Pavlovian conditioning run counter to those of Rescorla. The major variable determining the degree of conditioning is usually stated to be the number of occasions on which CS and US occur "in close temporal contiguity". The 2-min. and 15-min. cycle animals clearly differ in the mean interval between CS onset and a subsequent shock; at the extremes, this interval cannot possibly exceed four minutes in the 2-min. cycle, while in theory it could be 30 minutes in the 15-min. cycle. The structure of the experimental treatments is such that, no matter how we define "close temporal contiguity the number of "close pairings" between CS onset and US will be approximately 7.5 times greater for the 2-min. cycle animals than for the 15-min. cycle animals. Thus, if Pavlovian conditioning does occur despite the total absence of contingency between CS and US, it should clearly occur more strongly in the 2-min. than in the 15-min. cycle.

There is, in previous work (Kamin, 1965), considerable evidence to indicate that rats acquire a CER more readily to noise-onset than to noise-offset. Thus, if conditioning does occur in this experiment, we might expect it to occur primarily to the noise as a CS, not to silence as a CS. Once again, available evidence (Hilton, 1964) indicates that unconditioned suppression (non-associative) is greater and more persistent to noise offset than to noise onset. Thus, in general, the predictions stemming from a "pairings" point of view are quite opposite to those stemming from the view that any suppression to a CS observed in this experiment is non-associative in origin.

Method

Subjects

Thirty-two experimentally naive hooded rats between 285 and 340 grams at the beginning of the experiment served as subjects. The rats were obtained from Quebec Breeding Farms, St. Eustache, Quebec.

Apparatus

The operant chambers and programming equipment employed in the first experiment also were used in this experiment, with one exception. In the first experiment, a tape timer and stepper were used to program the US presentation. In this experiment, the USs were again programmed on the stepper, but presented by a Western Union tape transmitter (Type 29-A). This device provided a greater degree of accuracy in placing the shocks on the time base. By using the tape transmitter, shocks could be presented to within a second of the time specified by the random distribution.

The CS was again a 80 db white noise that varied in duration according to the experimental group. The US was of the same intensity and duration as that used in the first experiment.

Procedure

Preliminary Training. - Treatment during this phase of the experiment was similar to the preliminary training phase in Experiment 1. All experimental sessions, except the initial magazine training session, were 2 hrs. in duration. After magazine training, the animals were allowed to bar-press on a VI 2.5-min. schedule for 5 daily 2-hr. sessions. On the day following VI training, the conditioning phase was initiated. Throughout the experiment, the animals were run in squads, each squad comprised of 4, 2-min. cycle animals and 4, 15-min. cycle animals.

Conditioning. - Conditioning was conducted as in Experiment 1. The animals were placed into the operant chamber with the bar covered and presented a number of CSs and USs in some predetermined sequence. Twenty USs were randomly distributed over the 7200 seconds of the 2-hr. session. On a given training day, all groups received the same distribution of shocks. Five different distributions were used for each of the five days of conditioning. The "CS", of course, consisted of the regular on-off cycle of noise presentations; 2 mins. for half the animals, 15 mins. for the other half.

Baseline Recovery. - Following conditioning, five days of operant recovery were given. The animals were allowed to bar press on a VI 2.5-min. schedule. Half the animals in each group were recovered with an 80 db noise on continuously. The other half of the animals recovered without a noise background. This procedure allowed for half the animals ultimately to be tested for suppression to presentation of silence, and for half to be tested with a presentation of noise.

Testing. - As the animals were recovered in different stimulus situations, the logical testing procedure would be to present the opposing stimulus as the test. That is, if the animal recovered with a noise background, a 2-min. cessation of the background noise (silence), unreinforced, would constitute one test trial. Alternatively, if the animal recovered in silence, then a 2-min. noise presentation, unreinforced, constituted a test trial. Four trials were given in each 2-hr. session, the test stimulus occurring at the same times as in Experiment 1. Testing was carried out for 2 days with shock being presented at no time during the testing phase.

The experimental groups were as follows:

Group S-2 During each conditioning day, the noise stimulus was presented 30 times in alternation with 30 silent periods. That is, the animals in this group received, a 2-minute noise signal followed by 2 minutes of silence which in turn was followed by another 2-minute noise signal, and so on for the 2-hour session. Twenty USs were randomly distributed throughout the noise-silence cycle such that $p(US|CS) = .33$ and $p(US|\overline{CS}) = .33$.

These animals were then allowed to recover operant rate for 5 days during which no noise was presented. They were ultimately tested to noise.

Group S-15 The noise stimulus for this group was 15 minutes in duration and alternated with 15-minute periods of silence. The same distribution of shock presentations was used for this group as group S-2. These animals then recovered operant rate for 5 days under the same conditions as group S-2, and, like them, were tested with noise.

Group N-2 This group received exactly the same treatment as group S-2 except that during recovery a constant noise background was present, and the test was to silence.

Group N-15 This group received the same treatment as group S-15 except that during recovery a constant noise background was present and the test was to silence.

The design of the experiment is thus a 2 x 2 factorial. During conditioning, there are only two treatments; half the animals receive the 2-min. cycle and half receive the 15-min. cycle. Half of each of these groups, however, are given baseline recovery days in continuous noise and tested to silence; the other half are given baseline recovery days in silence, and tested to noise. The "S" and the "N" in the notation refer to the condition during recovery days, not to the stimulus to which the animal is tested.

Results

Operant Baseline

Table 3 presents the median number of responses for the 4 groups for the last day of VI training, and for the recovery days. An analysis of variance of baseline responding for the last day of VI training showed no significant differences among the groups.

After conditioning, the recovery phase of the experiment was initiated. As in the first experiment, this phase was originally intended to last for two days. It soon became evident, however, that bar pressing had suffered a greater decrement than in the first experiment and was recovering at a slower rate. A number of animals failed to initiate bar pressing at all during the first recovery session. It seemed likely that testing with such low baselines would introduce variables other than the ones of interest, thereby confounding any interpretation of the results. In an attempt to rectify this situation, recovery was continued for three additional days for a total of five days. As will be seen later, this attempt was only partially successful.

Figure 2 presents the course of recovery for the four experimental groups over the 5 days. Percentage scores were calculated by dividing the response total for each animal for each day of recovery by that same animal's total on the last day of VI training.

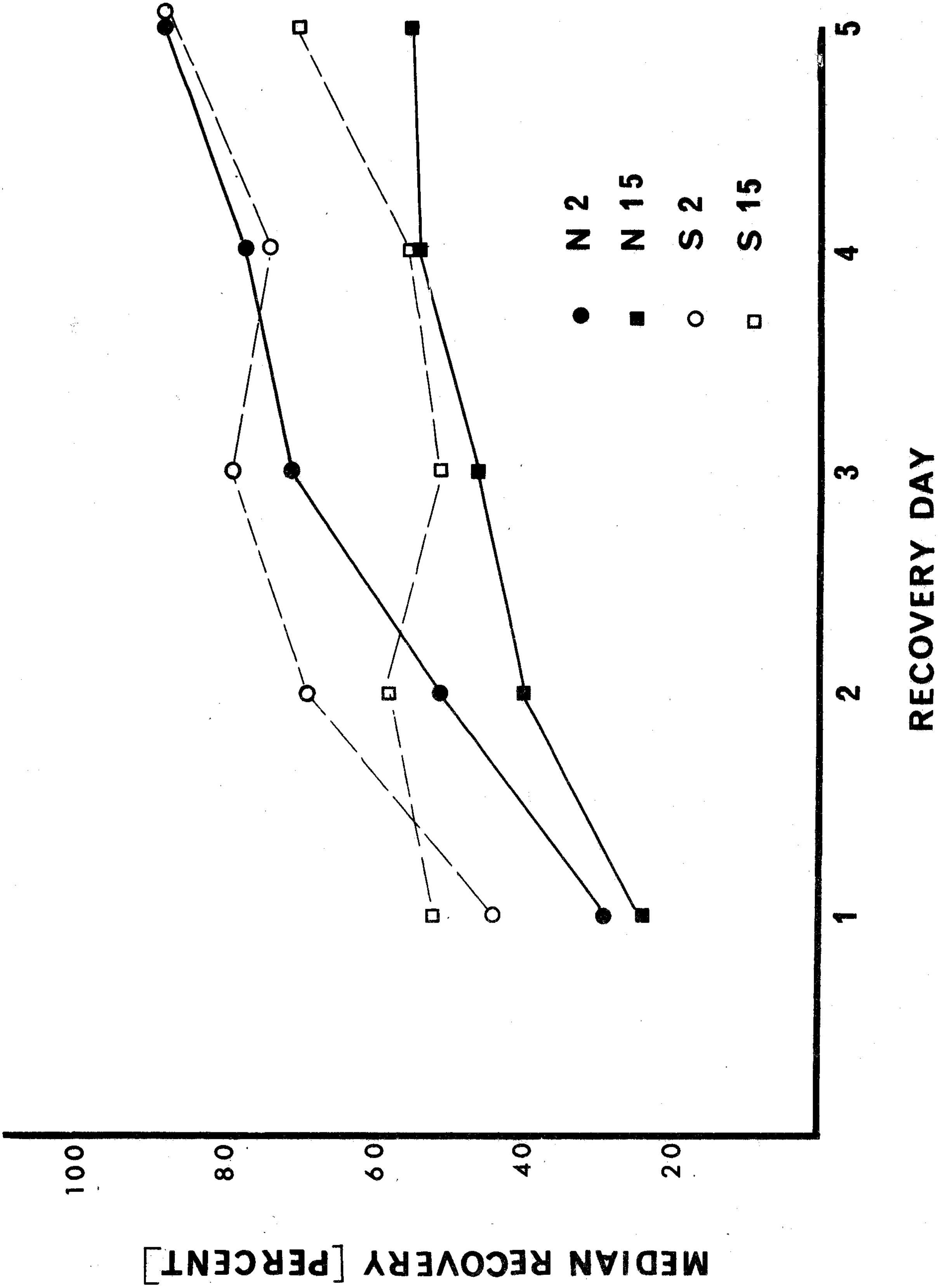
A 2 x 2 x 5 mixed analysis of variance of these percentage scores yielded two significant main effects,

Table 3

Median Number of Bar Presses Per Session for Selected Days

	Group			
Day	S-2	S-15	N-2	N-15
Last Day of VI	2462	1916	2881	2034
Recovery 1	1162	1073	626	571
Recovery 2	1433	1186	1293	851
Recovery 3	1547	916	1712	1236
Recovery 4	1561	1068	2077	1262
Recovery 5	2421	1412	2561	1522

Figure 2. Median recovery of operant rate (per cent) as a function of recovery day.



with no significant interactions. The effect of cycles (2 or 15 min. stimulus duration) was significant ($F = 6.52$, $p < .025$). The 15 min. cycle showed, overall, less recovery. The effect of days was also significant ($F = 15.57$, $p < .01$), indicating simply that recovery did occur. However, as Figure 2 makes clear, recovery was not complete for any group.

Figure 2 suggests the possibility of a difference between the recovery conditions (noise vs. silence) early in recovery training. While no effect involving recovery conditions was significant in analysis of variance, a U test yielded a significant difference between noise and silence on the first day of recovery ($p < .05$). The difference was no longer significant on Day 2. There is thus some suggestion that animals recovered in noise show, at the outset, a greater decrement in response rate than do animals recovered in silence.

A 2 x 2 x 5 mixed analysis of variance performed on the raw response scores for recovery days yielded results quite similar to the analysis of percentage scores. However, in this case, the Days x Cycles interaction was significant. There was, as can be detected in Figure 2, a tendency for the 2-min. cycle to recover more rapidly than did the 15 min. cycle.

Suppression to CS

Following recovery, the test phase was initiated. Suppression ratios were computed for each animal for each of the 4 test trials on the two days of testing. The second day of testing, as in Experiment 1, yielded no differences among the groups, extinction being almost complete. We shall again focus our attention on the results of the first test day.

Table 4 presents a summary of first trial data. The major effect indicated by an analysis of variance was a highly significant interaction between test stimulus (recovery condition) and cycle ($F = 9.69$, $p < .01$). The main effect of test stimulus was at the borderline of significance ($F = 4.14$, $p \leqq .05$). Examination of Table 4 makes it clear that, while all groups showed some suppression on the first test trial, the 2-min. cycle group tested with noise stood out from all others, with a median ratio of .03.

Figure 3 presents the median suppression ratio by groups as a function of trial in testing. Extinction of suppression, as in Experiment 1, was again quite rapid. By the third trial of testing, Group S-2 (tested with noise) was no longer distinguishable from the others.

Pooled ratios were calculated in the same manner as in Experiment 1. These ratios reflect responding over all four test trials of the session. Table 5 presents a summary of the pooled ratios for the first test session. A 2 x 2 analysis of variance on these ratios found neither the main effects nor their interaction significant. This lack of significance indicates that the first trial effects extinguished quite rapidly as can clearly be seen in Figure 3.

Discussion

The first point of interest is that, indeed, there

Table 4

Suppression Ratios, by Group, for Trial 1

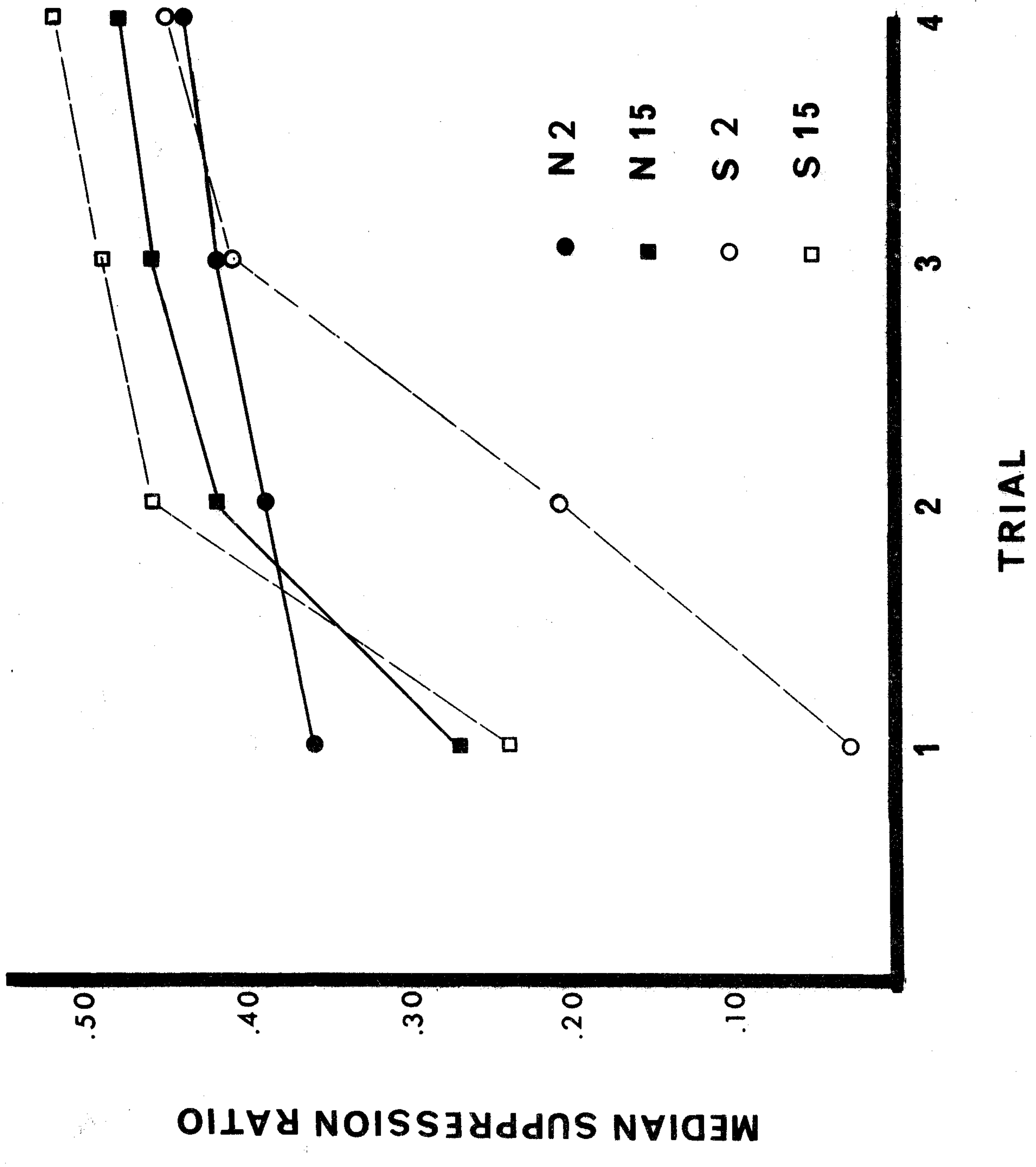
	Group			
	S-2	S-15	N-2	N-15
Median	.03	.24	.36	.27
Mean	.04	.29	.37	.23
Range	.00-.20	.00-.57	.26-.50	.00-.45

Table 5

Suppression Ratios, by Group, for Trials 1-4 Pooled

	Group			
	S-2	S-15	N-2	N-15
Median	.33	.44	.41	.39
Mean	.26	.40	.40	.37
Range	.01-.37	.03-.68	.35-.49	.19-.49

Figure 3. Median suppression as a function of test trial.



are differences among the experimental treatments during testing. While all groups show some suppression, Group S-2 shows by far the most.

Rescorla's theory in no way predicts such differences. By definition, as a Truly Random schedule of US presentation was employed, conditioning could not have occurred in any of the four groups. The observed suppression then _must_ be nonassociative in origin; and, post facto, the nonassociative factors resulting in suppression must be greater in Group S-2 than in the other groups. This kind of post facto circular "interpretation" is obviously unsatisfactory, and indicates a fundamental weakness in Rescorla's theory. The theory has so far escaped this criticism because the empirical outcomes of random controls have, in the past, indicated no responsiveness to the CS. However, the potential difficulty was already clear in Rescorla's early assertion that, "in some conditioning situations, Ss treated with the Truly Random control procedure..........(may)......... show strong changes in behavior when the CS is presented." (Rescorla, 1967).

These particular results seem especially embarrassing to Rescorla, as the most obvious prediction about nonassociative factors would have been that they were _minimal_ in Group S-2. This group had had a great amount of habituation to CS onset (150 presentations), and sensitization should have been equal across groups, as all groups had the same experience with shock. There is, further, nothing to suggest that what we are observing in testing is a difference in the simple unconditioned effects of noise and silence. Hilton (1964) presents evidence supporting the contention that in fact the unconditioned effects of noise-onset and noise-offset do differ in their effect on bar-pressing. Employing the CER with rats, Hilton investigated the influence of amount and direction of change of CS intensity on conditioning. During pretest, a white noise stimulus was increased or decreased relative to background intensity to test for the unconditioned (nonassociative) effects of stimulus presentation. Hilton found that for both increases and decreases, responding suffered a slight decrement on the first trial. However, on subsequent trials, an increase in noise intensity resulted in an increase in bar-pressing while a decrease in noise intensity resulted in a decrement in responding.

Similarly, Brimer (1963), investigating disinhibition, found the response tendency to noise-onset following previous experience with shock, to be an increase in rate of responding. Thus both of these investigators present evidence which indicates that noise-onset should <u>not</u> produce suppression. Thus it seems especially strained in the present experiment to attribute Group S-2's suppression, as Rescorla must do, to unspecified "nonassociative factors".

At first glance, the data seem quite consistent with the traditional view that conditioning will vary directly with the number of contiguous occurrences of CS-onset and US. Group S-2 had many more such contiguous occurrences than either of the 15-min. groups. On the other hand, even in Group S-2, conditioning was not very great. The suppression that was clearly evident on the first trial extinguished quite rapidly. However, Rescorla's previous work has indicated the "extra" un-signalled shocks reduce conditioning - sometimes to the vanishing point. The importance of the present data lies in the suggestion that, even under conditions where there is <u>no</u> contingency between CS and US, conditioning can occur, and, further, seems to vary with the number of CS-US pairings.

We have also to deal with the fact that Group N-2 did not display, upon testing, the suppression shown by Group S-2. This finding in fact fits in neatly with prior work on the CER, which has consistently shown (Kamin, 1965) that, at least in rats, conditioning occurs more readily to noise onset than to noise offset. There is, within our cycling procedure, no <u>logical</u> reason for the animal to treat either the noise or the silent period as a "CS". However, it does appear that the animal is much more likely to associate the US with noise, rather than with silence. The animal appears to associate the US with a "salient" stimulus change.

There is, in Rescorla's formulation, no explicit provision for the salience of a cue to affect the probability of the animal's associating a US with it. Rescorla's account seems to regard the animal as a kind of logical computer, calculating the probabilities of shock over two gross periods - periods spent in noise and periods spent in silence. There is little in what we know about conditioning to encourage the notion that the animal will follow the experimenter in logically dividing the experimental session into two symmetrical and equivalent classes of time.

An interpretation based on differences in cue salience may explain not only the differences between groups in the present experiment, but also Rescorla's failure to obtain suppression within the same procedure which resulted in substantial suppression in our laboratory. Rescorla (1968) uses a 720 cps pure tone as CS, to which acquisition is relatively slow; 80 db white noise was used in our experiments. As the use of a less "salient" CS results in less strong conditioning, one might suppose that, in Rescorla's studies, the "extra" shocks were more effective in destroying all conditioning. Alternatively, if the CS is sufficiently salient, sheer number of pairings is, as the traditional view suggests, a very important variable, contingency aside.

We turn now to some difficult problems posed by the recovery data. Rescorla has never reported any differences among his experimental groups in baseline responding during recovery days. This is not totally surprising as, due to the great amount of between-subject variability, such differences are difficult to detect. The analysis of the recovery data in the present experiment, however, clearly indicates that conditioning and recovery treatments resulted in substantial differences among the groups during the recovery phase. We have still to discuss these, and also to indicate ways in which these differences might have exerted effects on the test data.

There was a strong suggestion in our data that recovering animals with a constant noise background resulted in greater suppression of baseline responding during recovery days. Analysis of the baselines for the first day of recovery show this effect to be significant. This is quite consistent with the view that noise, being the more salient stimulus, had been associated with shock. The differences obtained on the test day in this sense agree well with the differences observed during recovery. It is reasonable to suppose that recovering Group S-2 in silence "preserved" the fear acquired to noise. On the other hand, recovering Group N-2 in a constant noise background should result in extinction of any fear acquired to noise. If fear had been acquired selectively to noise rather than silence, as we suggested earlier, then one would expect Group S-2 to suppress to noise-onset during the test, and Group N-2 not to suppress to noise-offset. In fact, our experimental results agree perfectly with these predictions.

Results of the analysis of variance on the recovery data also yielded a significant cycle difference. The 2-min. animals recovered more rapidly than did the 15-min animals. This may simply be due to generalization decrement. For the 2-min. groups, noise and silence during conditioning alternated every two minutes. Thus, for these Ss during the recovery days it would soon become evident that the background stimulus was not changing at its former rate, and that things were different. On the other hand, for the 15-min. groups, who had experienced long stimulus durations, the stimulus conditions during recovery were more like those in effect during conditioning.

Whatever their origins, such differences in recovery of baseline responding present problems of interpretation of the test data. The suppression observed on the test day may well depend on the amount of recovery that a group achieved by the time of test. However, if amount of recovery is affecting suppression on the test day, just how it is doing so is not readily apparent. Groups S-2 and N-2 recovered to the greatest degree (88% of the level of responding on the last day of VI training for both groups), yet, Group S-2 showed the most suppression while Group N-2 showed the least. Groups N-15 and S-15 were well below the 2-min. groups, 55 and 70% responding respectively. The suppression observed in these groups was moderate and fell between the 2-min. groups. Further, these groups differ to a fair degree in amount of recovery, but show very little difference in amount of suppression.

Such problems of interpretation also affect Rescorla's experiments. In his research, Rescorla employed a pure tone CS and the absence of the tone (silence) as non-CS. During recovery days in silence, then, the animal is presented with a constant "non-CS". Although Rescorla has never commented on this aspect of his procedure, the complications which might arise are obvious. Take, for example, a group trained with an inhibitory contingency. The probability of shock occurring during the non-CS is greater than the probability of shock occurring during the CS. Thus while the CS is explicitly intended to be inhibitory, the non-CS is, by Rescorla's logic, excitatory. One would thus expect suppression of the baseline during recovery when this excitatory "non-CS" was constantly present. This suppression should extinguish with continued experience with the non-CS, and how this might effect reactivity to the CS is not entirely clear. The same kind of problem applies to the Truly Random procedure, wherein fear should be equal to both CS and non-CS. Logically, one might expect fear of the non-CS to extinguish during the recovery phase. This would result in leaving fear of the CS intact during testing, but it might also, through secondary extinction, eliminate previously acquired fear of the CS!

Finally, to complicate the recovery problem even further, it should be remembered that in Experiment 1 differences between groups did not occur where they might logically have been expected. We cannot pretend to any deep understanding of the variables controlling responding during recovery, nor of the detailed relations between recovery and the test data. It seems clear that future work on the problems posed by the Random Control procedure demands a more satisfactory resolution of these questions.

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APPENDIX A

Raw Data: Total number of responses

For the last day of VI training and for each day of recovery in Experiment 1 and Experiment II.

Total Number of Responses

Experiment I

Group		Last Day of VI	Recovery Day	
			1	2
Random-Restricted	1	3818	2486	2562
	2	3488	1147	1961
	3	2581	1614	985
	4	2665	2197	3006
	5	1877	1642	2117
	6	1566	1546	1384
	7	1785	1225	1542
	8	3785	3114	2234
Explicitly-Unpaired	1	2336	1435	1963
	2	3210	1005	2029
	3	1656	303	935
	4	5767	2264	4083
	5	2218	2696	2018
	6	1028	879	816
	7	2456	2018	2868
	8	2121	1017	1432
Truly-Random	1	3009	1117	1812
	2	2929	1652	2418
	3	1585	523	1085
	4	1233	789	1120
	5	2778	1645	864
	6	2970	2259	2900
	7	2067	1940	1824
	8	1867	950	1462
Background-Noise	1	4690	2913	3567
	2	2027	1535	2123
	3	2224	1398	1292
	4	2600	2842	3393
	5	5041	3172	5303
	6	2080	842	1077
	7	2397	682	1184
	8	1770	1225	1051

Experiment II

Group		Last Day of VI	Recovery Day				
			1	2	3	4	5
N-2	1	1979	543	1745	1762	2056	2303
	2	3490	609	841	955	1400	2996
	3	3157	1209	2113	2654	2478	2628
	4	4221	1285	3073	2828	3789	3722
	5	1982	643	685	743	575	657
	6	2848	1455	1914	2183	2098	2494
	7	2204	0	422	1662	2246	2411
	8	2913	0	7	1100	1369	2838
N-15	1	1504	610	970	1172	1313	1843
	2	3347	0	254	1572	811	920
	3	1479	0	455	483	531	571
	4	1921	713	918	773	1210	1575
	5	1857	531	831	1017	907	1031
	6	4943	940	1864	2158	2234	2608
	7	2146	1217	871	1451	1448	1647
	8	2992	405	800	1300	1779	1468
S-2	1	1602	557	1109	1453	1242	1155
	2	2469	159	1696	1641	1699	2228
	3	866	1	0	0	381	342
	4	2898	1790	2015	2907	3412	4353
	5	2988	2347	2982	4389	5130	7195
	6	1935	928	930	951	799	1656
	7	2454	1547	1903	2580	3396	4322
	8	3415	1396	1169	948	1422	2614
S-15	1	1940	1162	1501	1610	2278	2446
	2	1761	1036	1034	837	942	1212
	3	1699	291	121	231	966	1197
	4	2678	807	595	564	965	1198
	5	2791	1237	1550	1841	1205	1621
	6	1891	1109	1338	995	1169	1612
	7	1660	2	168	309	716	1023
	8	3086	1981	1919	2140	2636	2259

APPENDIX B

Raw Data: Recovery Ratios

For all days of operant recovery in both Experiment I and Experiment II.

Experiment I

Recovery Ratios

		Random-Restricted	Explicitly-Unpaired	Truly-Random	Background-Noise
Recovery Day 1	1	.65	.61	.37	.62
	2	.33	.31	.56	.76
	3	.62	.18	.33	.63
	4	.82	.39	.64	1.09
	5	.87	1.22	.59	.63
	6	.99	.86	.76	.40
	7	.69	.82	.94	.28
	8	.82	.48	.51	.73
Recovery Day 2	1	.67	.84	.60	.76
	2	.56	.63	.83	1.05
	3	.38	.56	.68	.58
	4	1.13	.71	.91	1.31
	5	1.13	.91	.31	1.05
	6	.88	.79	.98	.52
	7	.86	1.17	.88	.49
	8	.59	.68	.78	.59

Experiment II

Recovery Ratios

		Day in Recovery				
		1	2	3	4	5
N-2	1	.27	.88	.89	1.04	1.16
	2	.17	.24	.27	.40	.86
	3	.38	.67	.84	.78	.83
	4	.30	.73	.67	.90	.88
	5	.32	.35	.37	.29	.33
	6	.51	.67	.77	.74	.88
	7	.00	.19	.75	1.02	1.09
	8	.00	.002	.38	.47	.97
N-15	1	.41	.64	.78	.87	1.23
	2	.00	.08	.47	.24	.27
	3	.00	.31	.33	.36	.39
	4	.37	.48	.40	.63	.82
	5	.29	.45	.55	.49	.56
	6	.19	.38	.44	.45	.53
	7	.57	.41	.68	.67	.77
	8	.14	.27	.43	.59	.49
S-2	1	.35	.69	.91	.78	.72
	2	.06	.69	.66	.69	.90
	3	.001	.00	.00	.44	.39
	4	.62	.70	1.003	1.18	1.50
	5	.79	1.00	1.47	1.72	2.41
	6	.48	.48	.49	.41	.86
	7	.63	.78	1.05	1.38	1.76
	8	.41	.34	.28	.42	.77
S-15	1	.60	.77	.83	1.17	1.26
	2	.59	.59	.48	.53	.69
	3	.17	.07	.14	.57	.70
	4	.30	.22	.21	.36	.45
	5	.44	.56	.66	.43	.58
	6	.59	.71	.53	.62	.85
	7	.001	.10	.19	.43	.62
	8	.64	.62	.69	.85	.73

APPENDIX C

Raw Data: Suppression Ratios

For the four test trials on the first day of testing for both Experiment I and Experiment II.

Experiment I

Suppression Ratios

		Test Trial			
		1	2	3	4
Random-Restricted	1	.07	.56	.47	.53
	2	.05	.36	.51	.55
	3	.26	.43	.54	.78
	4	.29	.47	.62	.47
	5	.00	.05	.55	.53
	6	.00	.38	.67	.62
	7	.06	.35	.68	.63
	8	.16	.42	.49	.51
Explicitly-Unpaired	1	.20	.26	.19	.18
	2	.34	.49	.48	.40
	3	.42	.61	1.00	.80
	4	.36	.51	.63	.47
	5	.36	.70	.59	.68
	6	.06	.20	.32	.45
	7	.24	.15	.33	.32
	8	.35	.45	.42	.85
Truly-Random	1	.00	.18	.34	.34
	2	.02	.11	.06	.01
	3	.00	.00	.00	.77
	4	.08	.18	.45	.52
	5	.09	.21	.57	.67
	6	.02	.44	.38	.41
	7	.00	.13	.43	.53
	8	.00	.11	.36	.46
Background-Noise	1	.10	.42	.51	.54
	2	.00	.32	.41	.29
	3	.25	.14	.49	.29
	4	.00	.02	.31	.45
	5	.15	.29	.42	.46
	6	.11	.33	.37	.56
	7	.26	.27	.52	.60
	8	.21	.60	.48	.68

Experiment II

Suppression Ratios

		Test Trial			
		1	2	3	4
N-2	1	.37	.24	.41	.50
	2	.48	.36	.42	.39
	3	.30	.32	.59	.50
	4	.34	.42	.29	.44
	5	.50	.57	.47	.43
	6	.28	.46	.48	.43
	7	.26	.48	.38	.44
	8	.40	.28	.38	.36
N-15	1	.09	.37	.53	.44
	2	.44	.45	.13	.48
	3	.00	.14	.42	.39
	4	.00	.44	.00	.16
	5	.26	.55	.74	.64
	6	.45	.51	.51	.48
	7	.28	.39	.49	.48
	8	.28	.48	.37	.14
S-2	1	.00	.00	.00	.04
	2	.07	.35	.40	.46
	3	.20	.20	.30	.42
	4	.03	.50	.42	.43
	5	.00	.22	.65	.73
	6	.00	.22	.53	.48
	7	.03	.08	.45	.51
	8	.02	.00	.05	.15
S-15	1	.18	.48	.45	.48
	2	.00	.00	.80	.63
	3	.04	.00	.00	.04
	4	.73	.55	.57	.83
	5	.52	.56	.76	.55
	6	.00	.21	.40	.40
	7	.30	.64	.53	.52
	8	.57	.47	.33	.51

APPENDIX D

Raw Data: Pooled Suppression Ratios

For the first day of testing in both Experiment I and Experiment II.

Experiment I

Pooled Suppression Ratios

		Random-Restricted	Explicitly-Unpaired	Truly-Random	Background-Noise
First Day of Testing	1	.47	.21	.27	.44
	2	.38	.43	.02	.32
	3	.48	.64	.20	.26
	4	.47	.47	.31	.28
	5	.36	.59	.36	.38
	6	.46	.25	.33	.39
	7	.45	.27	.30	.46
	8	.44	.45	.27	.51

Experiment II

		N-2	N-15	S-2	S-15
First Day of Testing	1	.38	.41	.01	.43
	2	.41	.37	.35	.30
	3	.44	.29	.29	.03
	4	.35	.19	.37	.68
	5	.49	.47	.34	.58
	6	.41	.49	.34	.29
	7	.40	.43	.31	.47
	8	.35	.32	.03	.45