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Negative Priming and Occasion Setting in an Appetitive Pavlovian Procedure.

Esther Mondragón*, Charlotte Bonardi, and Geoffrey Hall
University of York

Short title: Negative priming and occasion setting

Address for correspondence:

Charlotte Bonardi
Department of Psychology
University of York
York YO10 5DD
United Kingdom

Telephone: 0044 1904 434357

Email: C.Bonardi@psych.york.ac.uk Fax: 0044 1904 433181

* Esther Mondragón is now at Department of Psychology, University of Hertfordshire,
College Lane, Hatfield, Hertfordshire AL10 9AB.

Abstract

Rats received training in which two auditory target stimuli, x and y, were signaled by two visual stimuli, A and B, and followed by food (i.e. $A \rightarrow x+$, $B \rightarrow y+$). The test consisted of presentations of x and y, preceded either by the same signal as during training (*same trials*: $A \rightarrow x$, $B \rightarrow y$) or by the alternative signal (*different trials*: $A \rightarrow y$, $B \rightarrow x$). After eight training sessions, animals responded less on same trials than on different trials; this effect was significantly reduced after 24 training sessions. In two further experiments animals that had also experienced presentations of A and B alone, either before or during training, showed the opposite pattern of results, responding more on same trials than on different trials. These results are interpreted as being due to the interaction between the effects of occasion setting and *negative priming* (cf. Wagner, 1981).

In a standard positive occasion-setting discrimination, a target conditioned stimulus (CS) is reinforced when it is signaled by a second, *feature* stimulus, but not when it is presented alone; animals learn to respond more to the target on trials when the feature is present. This effect is not due to the associative strength of the feature (e.g., Holland, 1983; Rescorla, 1985; Ross & Holland, 1981; 1982); for example, when animals are trained on two such discriminations, responding to the target stimuli is usually greater when each is signaled by the feature with which it was trained than by the alternative feature (e.g., Bonardi, 1996; Davidson & Rescorla, 1986; Holland, 1986; 1989a; 1989b; Rescorla, 1985). Instead it has been argued that the feature, or *occasion setter*, “sets the occasion” for reinforcement of the target CS, perhaps by retrieving information about the target-reinforcer association (e.g., Bonardi, 1989; Bouton, 1990; Holland, 1983; cf. Skinner, 1938. (There are alternative explanations of these effects in terms of configural learning; consideration of these theories will be taken up in the General Discussion).

There is controversy over the conditions required to establish occasion setting. Some argue that trials on which the target is presented without the feature are necessary for the feature to become an occasion setter (e.g., Bouton, 1993; Rescorla, 1985; 1988; see also Lovibond, Preston, & Mackintosh, 1984; Swartzentruber, 1995). For example, Rescorla (e.g., 1988) has argued that in order for a stimulus to become an occasion setter it must accompany reinforcement of a target CS that has a component of inhibitory strength -- in other words, that the target must be nonreinforced during the course of occasion-setting training. A related proposal was made by Bouton and Nelson (e.g., 1994), who argued that the role of an occasion setter is to resolve the ambiguity of a

target CS that has both excitatory and inhibitory associations with the US. It follows that if the target does not possess both types of association, the occasion setter can have no effect, and so the target must be nonreinforced at some point during training for the feature to become an occasion setter. Others, however, maintain that target ambiguity is not required, and that a stimulus will acquire occasion-setting properties merely by signaling reinforcement of a target CS -- *nonexplicit* training. For example, Bonardi (e.g., 1989) has argued that occasion setters form by a process of Pavlovian conditioning, in which the occasion setter becomes associated with a representation of the pairing of CS and US (cf. Honey, 2000; Mackintosh, 1983). This account predicts that occasion setting, like Pavlovian conditioning, can be produced by pairings of the to-be-associated events -- in this case, the feature, and the target-US pairing; it is thus constrained to predict that nonexplicit training is sufficient to produce occasion setting.

Evidence in favor of the position that occasion setting can result from nonexplicit training comes from experiments in which contextual cues serve as the occasion setters. If animals are trained with two CSs, each being reinforced in one of two distinctive contexts, then, at least in some training paradigms, they will respond more to those CSs when they are experienced in the contexts in which they were trained rather than in the alternative context (Hall & Honey, 1989; 1990). This effect has also been demonstrated when the feature is a discrete and manipulable stimulus such as a tone, which maintains the same temporal relation to the target CS as a contextual cue by being presented for a long period, and having a number of brief target CS presentations embedded within it (Bonardi, 1992).

The proposal that nonexplicit training is sufficient to produce occasion setting faces a challenge, however, from studies of simple serial conditioning. If all that is necessary for a feature to become an occasion setter is that it should signal reinforcement of the target CS, then a standard serial conditioning procedure, in which one brief feature stimulus signals another, reinforced, CS, should be able to establish occasion setting -- but there is evidence that this is not the case. Honey, Hall, and Bonardi (1993) demonstrated that when rats were trained with two reinforced serial compounds, $A \rightarrow x+$ and $B \rightarrow y+$, and then tested with x and y either preceded by the training features (*same* trials: $A \rightarrow x$ and $B \rightarrow y$), or by the alternative feature (*different* trials: $A \rightarrow y$ and $B \rightarrow x$), then the animals responded *less* on same trials than on different trials -- the exact opposite to what one would expect if A and B had been acting as occasion setters. At face value these findings suggest that nonexplicit training does not produce occasion setting, and hence support the idea that a CS must have an ambiguous history in order to be the target of an occasion setter. However, Honey et al. (1993) succeeded in explaining their results in terms of Wagner's (1981) theory, without abandoning the idea that nonexplicit training can produce occasion setting. According to Wagner's (1981) theory, a stimulus that is predicted is less well processed than one that is surprising -- an effect that has been termed *negative priming* (without necessarily implying a direct parallel with phenomena in human information processing that have received the same name; see e.g., Allport, Tipper, & Chmiel, 1985). Thus stimulus x should elicit less responding on trials when it is well predicted, being preceded by A , than on those when it is preceded by B -- just as Honey et al. found.

As well as being able to explain Honey et al.'s (1993) results, this account might also explain how nonexplicit training could produce occasion setting in experiments in which a long feature stimulus, or context, has the target CSs *embedded* within it, but not in this serial conditioning preparation. It is possible that serial conditioning *is* capable of producing occasion setting, but that this effect is not evident because it is masked by a negative priming effect. The question then arises as to why negative priming should be strong enough to mask occasion setting after serial conditioning, yet fail to do so in animals trained with the embedded procedure. According to Honey et al., this may be because negative priming relies on the integrity of the stimulus-stimulus association; the stronger the association, the greater the negative priming effect. Serial conditioning should produce a stronger feature-target association than the embedded procedure, because in serial conditioning the feature is *always* followed by the target CS, whereas in the embedded procedure the feature is presented for much of the time in the target's absence. Thus, in contrast to the case of serial conditioning, in the embedded procedure the feature-target association will be constantly weakened by what are effectively feature alone trials, and so will reach a lower asymptote. The negative priming effect will be thus relatively weak, and consequently unable to mask the underlying occasion setting effect.

To sustain this analysis, and hence those theories that predict that nonexplicit training can produce occasion setting, it is necessary to provide direct evidence to show that occasion setting is produced in this nonexplicit serial conditioning procedure. The present experiments attempted to provide such evidence, and hence to allow us to decide between these various accounts.

Experiment 1

The design of the first experiment is shown in Table 1. Animals in Group 8, received eight training sessions with two serial compounds, composed of visual stimuli, A and B, and auditory stimuli, x and y. Animals received some trials in which A was followed by a reinforced presentation of x, and an equal number in which B was followed by a reinforced presentation of y. They then received a test in which the two auditory cues were presented, signaled either by the same stimulus as during training (*same* trials: $A \rightarrow x$ and $B \rightarrow y$) or by the alternative stimulus (*different* trials: $A \rightarrow y$ and $B \rightarrow x$). It was anticipated that the negative priming effect reported by Honey et al. (1993) would be replicated, namely that there would be more responding on different trials than on same trials.

A second group of animals received equivalent but more extensive training; in Experiment 1a animals in this group received 16 training sessions, and in Experiment 1b 24 (Groups 16 and 24 respectively). If nonexplicit training can produce occasion setting, then this manipulation, of simply increasing the amount of training, could allow us to see the effect in this training situation. This is because, according to the account outlined above, negative priming depends on the integrity of a simple association between the feature and the target cue; associations of this type between two neutral stimuli require relatively few trials to be established, and may indeed be almost fully formed after only one trial (e.g., Hoffeld, Kendall, Thompson, and Brogden, 1960; Rescorla & Durlach, 1981). One would therefore predict that negative priming should be at a maximum after relatively little training. In contrast, occasion setting seems to require relatively many training trials to be established, and to increase in magnitude with overtraining (e.g., Ross

& Holland, 1981). One might therefore expect that occasion setting would increase in magnitude with extended training, whereas the negative priming effect would not. Thus the prediction is that a negative priming effect would be observed in Group 8, but that with increasing numbers of training sessions this effect would become masked by the developing occasion setting, until eventually a net occasion-setting effect would be revealed. If, on the other hand, occasion setting cannot be produced by nonexplicit training, then no emergent occasion setting effect would be predicted to develop, meaning that there would be no reason to expect a difference between the two groups.

Experiment 1a Method

Subjects

The subjects were 16 male hooded Lister rats with a mean *ad lib.* weight of 454 g (range = 430-525 g), housed in pairs in plastic tub cages with sawdust bedding. They had previously participated in an experiment on flavor aversion learning, but were naïve to the stimuli and procedures employed here. The colony rooms were lit from 8 am to 8 pm; the subjects were tested during the light portion of the cycle. Before the start of training they were reduced to 80% of their *ad lib.* weights, and were maintained at this level for the rest of the experiment by being fed a restricted amount of food at the end of each session.

Apparatus

A set of four standard Skinner Boxes (supplied by Campden Instruments Ltd.) was used. Each box had three walls of sheet aluminum, a transparent plastic door as the fourth wall, a grid floor and a white translucent plastic ceiling. One of the walls adjacent to the door contained a recessed food tray covered by a transparent plastic flap, 6 cm high

x 5 cm wide, that was hinged to the top of the opening to the food tray. Pushing this flap inward from its vertical resting position allowed subjects to gain access to the food tray. An inward movement of the flap actuated a microswitch, and each closing of the switch was recorded as a single response. The flap automatically returned to its resting position when the subject removed its snout from the food tray. Mixed-composition, 45-mg food pellets (Noyes, New Hampshire) could be delivered to the food tray. Retractable levers fitted alongside the food tray remained withdrawn throughout the experiment. Each box was housed in a sound- and light-attenuating shell. There were two visual stimuli, A and B, and two auditory stimuli, x and y. One visual stimulus was provided by illuminating a 2.8-W jewel light, which was situated on the front wall to the right of the food tray, and a second by illuminating a 2.8- bulb situated inside the food tray. The two auditory stimuli, an 81-dB white noise and a 2-kHz 79-dB tone, were provided by Campden instruments noise and tone generators respectively, and delivered through a speaker mounted on the wall of the chamber. The boxes were controlled by a BBC microcomputer programmed in a version of BASIC.

Procedure

Initially all animals were trained to retrieve pellets from the food tray; pellets were delivered according to a variable time (VT) 30-s schedule during the first of these three sessions, and a VT 60-s schedule in the remaining two. All animals were tested at the same time, but for Group 16 serial conditioning training 8 days before that in Group 8; thus magazine training for Group 8 took place on the same days as training sessions 9, 10 and 11 for Group 16.

All serial conditioning sessions comprised four training trials, two reinforced presentations of the serial compound $A \rightarrow x$, and two of the serial compound $B \rightarrow y$. These different types of trials were presented in a random order that varied from day to day. For half the animals in each group the jewel light signaled the noise, and the magazine light signaled the tone, and for the remainder the opposite was true, such that the jewel light signaled the tone, and the magazine light the noise. All stimulus presentations were of 30-s duration, and each trial was preceded by a 30-s pretrial interval, during which responding was recorded separately. The reinforcer was the delivery of a single food pellet. The intertrial interval (ITI -- measured from auditory stimulus offset to onset of the next pretrial period) was fixed at 408 s, such that the total session duration was 40 min.

The animals then received four test sessions. Each session comprised a single presentation of each of the four possible trial types, $A \rightarrow x$ and $B \rightarrow y$ (same) and $A \rightarrow y$ and $B \rightarrow x$ (different); all trial types were reinforced just as in the training phase. These different trial types were presented in a random order that varied across days.

Data treatment.

In all the experiments reported below flap-pressing was recorded separately during presentations of A and B, and of x and y, and also during the 30-s preCS period that immediately preceded the onset of the CS, and during the entire session. The measure of conditioned responding employed during the test was a corrected score, derived from the number of responses made during the auditory stimuli on all trials of the same type (same or different) in a session, after subtraction of the number of responses made during the corresponding pre-trial periods. Due to the small number of trials in

each test session there was considerable variability in responding in any one session; accordingly the response rates for same and different trials were pooled over all four test sessions. Finally, a significance level of $p < 0.05$ was adopted in this and all subsequent experiments.

Results and Discussion

Acquisition of responding to the auditory cues proceeded over the course of training; on the last training session the mean corrected rate of responding (in responses per minute -- rpm) to these stimuli was 12.69 in Group 16 and 11.56 in Group 8; these scores did not differ, $F < 1$. Responding to the visual cues was, in this and the subsequent experiments, relatively low, due presumably both to the suppressive nature of visual stimuli *per se*, and to the fact that these cues were temporally distant from reinforcer delivery. The corrected response rates to the visual stimuli in the present experiment were 2.62 rpm for Group 8 and 2.94 rpm for Group 16; these rates did not differ, $F < 1$. The corresponding rates of pretrial responding were 2.44 and .88 rpm respectively; these rates did not differ, $F(1,14)=2.6$.

The results of the test are shown in Figure 1. It is clear that animals in both groups responded somewhat less on same trials than on different trials, although this effect was rather less marked in Group 16 than in Group 8. This description of the data was partially supported by the results of an analysis of variance with Group (8 or 16) and trial type (same or different) as variables. This revealed a main effect of trial type, $F(1,14)=5.68$; neither the main effect of group nor the Group x Trial type interaction were significant, largest $F(1,14)=1.46$.

Although, in the absence of a significant interaction, performing further analyses is arguably inappropriate, it was encouraging to note that simple main effects analysis using the MSW error term revealed that the difference in responding on same and different trials was significant in Group 8, $F(1,14)=6.45$, but not in Group 16, $F<1$.

The results of Experiment 1a replicated the results reported by Honey et al. (1993). After 8 sessions of training animals responded significantly more on different trials than same trials -- a negative priming effect (cf. Honey et al., 1993). There was also a suggestion that this negative priming effect was less marked in the animals that had received the greater amount of training, those in Group 16. This provided preliminary support for our assertion that the nonexplicit training procedure used in these experiments might be capable of producing occasion setting, and suggested the possibility that if Group 16 had received still more training, an occasion setting effect might have been revealed in these animals. Thus in Experiment 1b the amount of training in this group was increased, by giving 24 training sessions rather than 16.

Experiment 1b Method

Subjects and apparatus

The subjects were 16 naïve male hooded Lister rats with a mean *ad lib.* weight of 370 g (range = 340-400 g). The apparatus was the same as that used in Experiment 1.

Procedure

Magazine training was identical to that in the previous experiment, except for when it occurred. In the present experiment all animals received two sessions per day during the serial conditioning phase, the first in the morning and the second in the

afternoon. Thus Group 8 received its three magazine training sessions on the same days as training sessions 13-16 of Group 24.

Both serial conditioning and test phases were identical to those of the previous experiment except for the fact that, as mentioned above, animals received two sessions per day rather than one during the serial conditioning phase.

Results and Discussion

Acquisition of responding to the auditory cues proceeded much as in the previous experiment; in the last training session the mean corrected rate of responding to these stimuli was 9.50 rpm in Group 24 and 12.56 rpm in Group 8; these scores did not differ, $F(1,14)=1.28$. The corrected rates of responding to the visual stimuli were 2.44 rpm for Group 8 and 3.44 rpm for Group 16; these rates did not differ, $F<1$. The corresponding rates of pre-trial responding were 1.31 and 1.19 rpm respectively; these rates did not differ, $F<1$.

The results of the test are shown in Figure 2. As in the previous experiment, animals in Group 8 responded more on different trials than same trials; however, there was no sign of such a difference in Group 24. This description of the data was supported by the results of an analysis of variance with group (8 or 24) and trial type (same or different) as variables. This revealed no main effect of group or of trial type, largest $F(1,14)=3.83$, but there was a significant interaction between these two variables, $F(1,14)=5.35$. Exploration of this interaction with simple main effects analysis, using the MSW error term, revealed that responding on same and different trials differed in Group 8, $F(1,14)=9.11$, but not in Group 24, $F<1$.

The results of this experiment confirm the suggestion made on the basis of the results of Experiment 1a, that overtraining will reduce the size of the negative priming effect. It did not, however, provide direct evidence for occasion setting, as we had hoped, in that overtraining never resulted in *more* responding on same than on different trials. It is possible that we simply did not give enough training to obtain an occasion-setting effect; but it is also possible that occasion setting is not produced in this serial conditioning procedure, and that the results we observed arose only because the degree to which negative priming may be observed is reduced by overtraining, rather than because occasion setting is increased. For example, perhaps overtraining renders responding less sensitive to change, so that in Group 24 the effect of negative priming was less easily observed. At present, then, we have no direct evidence that the negative priming effect is masking some second, opposing tendency for animals to respond more on same than on different trials, produced because initial training converted A and B into occasion setters. More direct evidence is required to confirm such a hypothesis. The purpose of Experiments 2 and 3 was to provide such evidence.

Experiment 2

Experiments 2 and 3 attempted to minimize the presence of stimulus-stimulus associations, and hence the magnitude of the negative priming effect, in order that any underlying occasion setting effect might be revealed. This was achieved by exploiting the fact that the rules governing formation of simple associations and of occasion setters are likely to differ in certain respects. For example, if an animal receives multiple nonreinforced preexposures to a stimulus before it is paired with reinforcement, learning about the stimulus will be slow, an effect called latent inhibition (e.g., Lubow & Moore,

1959). Although this retarding effect of preexposure on subsequent learning has been found in many standard conditioning preparations (e.g., Rescorla & Durlach, 1981), there are good reasons to believe that preexposing the feature will *not* necessarily retard conversion of that stimulus into an occasion setter. This is because when a stimulus signals reinforcement of a target CS, it *not only* has the opportunity to acquire occasion-setting properties, but *also* to enter into a direct association with the US – and formation of this association has been found to severely retard the acquisition of occasion setting (Rescorla, 1986b). The reason for this is simple: according to the standard competitive principles of associative learning, the more associative strength is acquired by the feature, the less will be available to be acquired by the target, and so the weaker will be the target-US on which the occasion setter acts. According to this analysis, feature preexposure will thus have two effects: first, to the extent that occasion setting is the product of Pavlovian conditioning (Bonardi, 1989) it will *reduce* the ability of the feature to acquire occasion-setting properties (i.e. it will reduce the ability of the feature to become associated with the target-US association) and second it will retard formation of any feature-US association – which will *indirectly increase* the degree to which occasion setting may be observed. It is an empirical question as to which of these effects will be greatest, but in this respect it is interesting to note that a recent report by Oberling, Gunther, and Miller (1999) found no evidence that nonreinforced preexposure to a stimulus retarded subsequent conversion of that stimulus into an occasion setter.

The logic behind the present experiment was based on these suppositions – first, that nonexplicit training produces occasion setting, and second that latent inhibition training might not affect the acquisition of this occasion setting, while still retarding the

formation of simple associations, such as the feature-target association that we have postulated to be responsible for the priming effect. Accordingly all subjects were trained as in the previous experiment, but before this training phase animals in Group E (experimental) were preexposed to stimuli A and B (see Table 2). Group C (control) were simply placed in the experimental chambers for an equivalent period of time. This preexposure phase would allow A and B to suffer latent inhibition in Group E, so that it would be difficult for these stimuli to enter into simple associations. This would have the effect of reducing the strength of the within-compound associations between A and x, and between B and y, in this group and so might be expected to reduce the size of the negative priming effect. But if we are correct in the assertion that such preexposure would not affect the acquisition of occasion setting, then one would predict that if the negative priming effect were sufficiently diminished, then an occasion setting effect should be revealed.

If, on the other hand, nonexplicit training is not able to produce occasion setting, then, although feature preexposure might be expected to reduce the size of the priming effect in Group E (compared to that shown in Group C), it should not reverse it, as there would be no occasion setting to unmask. Thus this account would predict a greater level of responding on different trials than on same trials in *both* groups, but that this effect might be smaller in Group E than in Group C.

Method

Subjects and Apparatus

The subjects were 16 naïve male hooded Lister rats with a mean *ad lib.* weight of 319 g (range = 300-345 g); these were deprived to 80% of their *ad lib.* weight before the

start of the experiment, and were housed and maintained exactly as in Experiment 1. The apparatus was the same as in Experiment 1.

Procedure

In the initial, preexposure phase there were 12, daily 40-min sessions. For Group E these comprised 4, 30-s presentations of each of the two visual stimuli. The two types of stimulus presentation were presented in a semi-random order that varied from day to day. Responding was not recorded during these sessions, so there was no pretrial period. The ITI (measured from stimulus offset to stimulus onset) was 226 s, such that the total session length was 40 min. Animals in Group C were placed in the chamber for the same amount of time, but received no stimulus presentations.

The animals then received three magazine training sessions, identical to those given in Experiment 1.

The serial conditioning training procedure was the same as that used in Experiment 1, except that both groups received 12 sessions of training; pilot work suggested that this would be enough to produce a negative priming effect in Group C, but not as large as that observed in Group 8 of Experiments 1 and 2, as this might prove too difficult to abolish with the latent inhibition manipulation.

The test sessions were identical to those of Experiment 1. There were five sessions in this stage.

Results and Discussion

As in the previous experiment, animals learned to respond during the auditory stimuli, and this was equally true in Groups E and C; the corrected mean rates of response during the last session of this stage were 21.56 rpm for Group E and 16.31 rpm

for Group C, and these rates did not differ, $F < 1$. The corrected rates for the visual stimuli were 11.81 rpm for Group E and 7.31 rpm for Group C; these rates did not differ, $F(1,14) = 1.31$. The corresponding pre-trial response rates were 4.25 and 5.44 rpm for groups E and C respectively, and these rates did not differ either, $F < 1$.

It is notable that the levels of responding during the visual and auditory stimuli seemed rather higher than in the previous two experiments. Presumably this was due to the fact that both groups of animals experienced extensive exposure to the context during the initial preexposure phase. To the extent that this treatment was able to diminish the degree to which the context could overshadow conditioning to the discrete stimuli during serial compound training, then this might be expected to elevate responding to these stimuli in just the manner that we observed.

The results of the test phase are shown in Figure 3. As in the previous experiment, Group C showed a small negative priming effect, but this effect was reversed in Group E, who responded more on same trials than on different trials. An analysis of variance with group (E or C) and trial type (same or different) as variables confirmed this interpretation, revealing neither a main effect of group nor of trial type, $F < 1$ and $F(1,14) = 1.09$ respectively, but a significant interaction between these two variables, $F(1,14) = 6.4$. Exploration of this interaction with simple main effects analysis, using the MSW error term, revealed a significant difference in responding during the two types of trial in Group E, $F(1,14) = 6.39$, but not in Group C, $F(1,14) = 1.10$.

In this experiment animals in Group C responded more on different trials than on same trials, as they did in the previous two experiments. Although here the effect did not attain statistical significance, the results from Experiments 1a and 1b, and from Honey et

al. (1993), strongly suggest that this difference was due to a negative priming effect. But more important are the results from Group E. These animals, for whom both features had been preexposed before training commenced, showed the opposite pattern of results, and responded *more* on same than on different trials. Those theories according to which nonexplicit training cannot produce occasion setting would have difficulty explaining these results, as they cannot predict that occasion setting should be observed in either of the two groups. This finding is, however, consistent with our original hypothesis: if association formation suffers from latent inhibition training whereas occasion setting does not, then preexposure to the features might be expected to limit the formation of stimulus-stimulus associations in this procedure, and hence the development of negative priming, while leaving the acquisition of occasion setting unaffected. Thus feature preexposure should replace the negative priming effect by occasion setting. This was exactly the effect that was observed.

Experiment 3

The previous experiment provided the first direct evidence that occasion setting can result from this serial conditioning procedure. The final experiment aimed to provide more such evidence, using a slightly different technique. Once more two groups of animals were employed, Group E (experimental) and Group C (control). Group C received training just as in Experiment 1; animals in Group E, however, were given presentations of A and B alone, just as in the previous experiment, except that here these feature presentations were interspersed with the training trials (see Table 3). The logic behind this manipulation relies on the observation that classical conditioning and occasion setting are differentially sensitive to extinction; presenting a CS alone will

reduce its ability to evoke a conditioned response, whereas it is well established that presenting an occasion setter alone has no effect on its occasion setting power (Holland, 1989; 1991; Macrae & Kehoe, 1993; Rescorla, 1985; 1986a). It was therefore anticipated that the treatment given to Group E would selectively reduce the strength of the stimulus-stimulus association responsible for the negative priming effect, but not affect the degree to which the features could act as occasion setters. Thus, as before, in Group C a negative priming effect was anticipated, demonstrated by animals responding more on different than on same trials; but in Group E this negative priming effect should have been eliminated, and occasion setting should be observed with animals responding more on same than on different trials. In contrast, those theories predicting that nonexplicit training cannot produce occasion setting would be constrained to predict a negative priming effect in both groups, the magnitude of which would be smaller in Group E than Group C.

Method

Subjects and Apparatus

The subjects were 16 naïve male hooded Lister rats with a mean *ad lib.* weight of 316 g (range = 290-345 g). They were housed and maintained exactly as in Experiment 1. The apparatus was the same as that used in Experiment 1.

Procedure

All subjects received two 40-minute sessions of VT-60 food delivery, as in Experiment 1.

Serial conditioning training was identical to that of Experiment 1, except in the following respects. First, both groups received ten sessions of training; we judged that

this would be enough to produce a negative priming effect in Group C, but not as large as that observed in Group 8 of Experiments 1 and 2, as this might prove too difficult to abolish with the extinction manipulation. Group E also received in each session two, 30-s presentations of A and two of B. The four different types of trial were presented in a random order that varied across sessions. These stimulus presentations were preceded by a 30-s pretrial period, and followed by a 30-s period when the auditory stimulus would normally have been presented. The ITI was 186 s, making the total session length 40 min.

The test sessions were identical to those in the previous experiment. There were five sessions in this phase.

Results and Discussion

As in the previous two experiments, animals learned to respond during the auditory stimuli, and the level of responding was not greatly affected by presentations of A and B alone in Group E. The corrected response rates for the last session of this phase were 8.69 rpm for Group E and 10.5 rpm for Group C, and these rates did not differ, $F < 1$. The corrected rates of responding for the visual stimuli in the last session of this stage were 6.38 for Group C and 4.91 for Group E, and they did not differ, $F < 1$. Thus although one would predict that, due to the nonreinforced feature presentations given to Group E, the rates of responding to the feature should be lower than in Group C, this effect was not statistically significant, probably because overall response rates were rather low and extremely variable. The corresponding pre-trial response rates were 2.0 and 1.81 rpm for Groups E and C respectively, and these rates did not differ, $F < 1$.

The results of the test phase are shown in Figure 4. The results from Group C are similar to those of Group 8 from Experiments 1 and 2, in that animals responded more on different than on same trials, although, as was anticipated, the increased number of training sessions slightly reduced the size of this effect. More important are the results of Group E, for whom A and B were presented alone during the training phase. In this group the reverse pattern of results was observed, and animals responded substantially more on same trials than on different trials. This description of the data was supported by the results of an analysis of variance with group (E or C) and trial type (same or different) as variables. This revealed neither a main effect of group nor of trial type, $F(1,14)=1.76$ and 2.96 respectively; however the interaction between these two factors was significant, $F(1,14)=6.54$. Exploration of this interaction with simple main effects analysis, using the MSW error term, revealed that there was a difference in responding to the two types of trial in Group E, $F(1,14)=9.15$, but not in Group C, $F<1$. As in Experiment 2, presenting the feature stimuli alone during training, a manipulation designed to selectively reduce the size of the negative priming effect, allowed an occasion setting effect to be revealed. Any account predicting that occasion setting cannot result from nonexplicit training would have difficulty explaining this result.

Although it was not significant, in the present experiment there did appear to be a tendency for animals in Group E to respond at an overall lower level than animals in Group C, especially on *different* trials, an effect which perhaps deserves comment. One likely explanation for this trend lies in the possibility that a contributor to the level of responding to the target CS is summation with the associative strength of the feature. Extinction of the feature, by reducing its associative strength, will reduce the magnitude

of this summation effect, and hence produce lower levels of responding. It should be noted, however, that this between-group effect does not influence the comparison of interest, the difference in responding on same and different trials, as this comparison is conducted within subjects, and so controls for any such summation effects.

One further point that deserves comment is that these findings are at face value inconsistent with those of some studies reported by Holland (1989b), in which he compared the ability of features from feature positive discriminations (A-- >x+, x-) and positive patterning discriminations (A-- > x+, x-, A-) to elevate the level of responding to their own and other occasion-set targets. He found that presentation of the feature alone in the patterning task did not affect responding to the target with which it was trained (equivalent to our same trials), but *enhanced* the ability of the feature to elevate responding to a transfer target (equivalent to our different trials). This suggests that the occasion setting effect was *reduced* by feature extinction, rather than increased, as in the present studies. There are reasons to question a direct analogy between the two studies, however. First, the strength of the feature-target association in the studies reported by Holland (1989b) would necessarily be weaker than in our procedure, as in both discriminations that he reports the target stimulus was *also presented alone*. Thus there would be relatively little negative priming contributing to his results, and consequently little to remove by feature extinction. Second, the stimuli he used were of considerably shorter duration than ours (5s as opposed to 30s) meaning that other factors might become more important. For example, the feature from the positive patterning discrimination was presented alone throughout training, whereas that from the feature-positive discrimination was not. The surprise resulting from experiencing the feature

with a new target might therefore be greater with the feature-positive feature than with the feature from the patterning task, resulting in more generalization decrement on trials with the transfer target in the former condition. This could disrupt responding on these transfer trials, and so effectively reduce the size of the transfer effect. Making the assumption that such a generalization decrement effect would probably be shortlived, then it might well affect responding during a 5-sec CS rather more than responding to the 30-sec CSs used in the present studies, and hence be more apparent in Holland's experiments than in our own. Whether or not this interpretation is correct, it seems clear that there are enough procedural differences between the two studies to make the apparent inconsistency between their results less surprising than it initially seems.

General Discussion

The present series of experiments sought evidence for the assertion that serial conditioning of form $A \rightarrow x^+$ and $B \rightarrow y^+$ established A and B as occasion setters for x and y respectively, such that animals respond more to x when signaled by A than by B, and more to y when signaled by B than by A. Previous work had suggested that in fact with serial conditioning the opposite result is obtained (Honey et al., 1993); the suggestion was that the formation of direct associations between A and x, and between B and y, result in a negative priming effect that masks the occasion setting. If this analysis is correct, then if it were possible to reduce the relative contribution of the negative priming effect, one should then reveal the occasion setting that underlies it.

The present experiments attempted to achieve this in three different ways. Experiment 1 examined the effects of increasing amounts of training on negative

priming. It was argued that formation of associations between two neutral stimuli requires fewer trials to be established than does occasion setting, so that although relatively little training should yield a maximal negative priming effect, with increasing training the occasion setting effect should increase, and thus eventually be revealed. The results of this experiment were suggestive: the negative priming effect was replicated, and also significantly reduced if sufficient training was given; however, at no stage was there evidence of the reverse, occasion setting effect. Occasion setting was, however, observed in Experiments 2 and 3. In Experiment 2 one group of animals was preexposed to the two stimuli that were to be the features, A and B. As it has been argued that association formation, upon which negative priming depends, is retarded after latent inhibition training, whereas acquisition of occasion setting is not (Oberling et al., 1999; cf. Rescorla, 1986b), then this preexposure should selectively reduce the negative priming effect, and so allow the occasion setting effect to emerge. The results supported this prediction; animals in Group E, unlike those in Group C, showed evidence of occasion setting. A similar strategy was used in Experiment 3, except that here Group E received feature presentations during, rather than before, the training phase. As it is well established that simple associations are weakened by such extinction of the CS, whereas occasion setting is not, then again one would predict that this treatment should weaken the association upon which the negative priming depends, while leaving the occasion setting intact. Once more this prediction was confirmed: animals in Group E again responded as though A and B had become occasion setters. Theories predicting that occasion setting cannot result from nonexplicit training have trouble explaining these findings.

One issue that perhaps requires further comment is our measure of occasion setting. There has always been considerable debate in the occasion-setting literature as to what constitutes an appropriate test of occasion setting and what does not. In the present series of experiments we have argued that if animals respond more to a target stimulus (x) in the presence of the modulator (A) with which it was trained, than to the same target in the presence of another modulator with the same associative strength, then this is evidence that A is modulating x , because responding to x in the presence of A is not to be explained in terms of simple Pavlovian conditioning. Although this test has been used quite widely (e.g. de Brugada, Garcia-Hoz, Bonardi, & Hall, 1995; Hall & Honey, 1989; 1990) it has (along with most others) been subject to a number of criticisms. First, it relies on the assumption that occasion setting is CS-specific – that an occasion setter will be more effective at elevating responding to a target with which it has been trained than to some other target stimulus (one would expect *some* transfer to different CS/modulator combinations even if modulators were CS specific, given that there is likely to be some degree of generalization between the different target CSs). Although the data supporting this claim were initially contradictory, now the bulk of evidence supports the idea that occasion setters are CS-specific (for review see Swartzentruber, 1995) – and indeed some authors who initially argued that occasion setters were not CS-specific have been forced to revise this claim (e.g., Rescorla, 1991a; 1991b). The second potential problem is that of generalization decrement -- a lower level of responding to untrained modulator/target combinations may reflect not the absence of occasion setting but the presence of generalization decrement. Although this must be treated as a serious possibility, it should be noted that in one study that explicitly controlled for this factor, no evidence for

generalization decrement was obtained (Bonardi, 1996; see also, e.g., Hall & Honey, 1989). Finally, converging evidence that we have indeed obtained occasion setting may be obtained from the test of Experiment 3, in which nonreinforced feature presentations were intermixed with occasion setting training in one of the groups. As the distinguishing characteristic of occasion setting is that it is not the result of Pavlovian conditioning, the prediction is that simple extinction of an occasion setter should not affect its ability to promote conditioned responding. To the extent that responding on *same* trials was similar in Group E, for whom the feature was extinguished, and Group C, for which it was not, support the proposal that in the present experiments the features were acting as occasion setters.

These data provide further support for the proposal that occasion setters are established simply by virtue of signaling the reinforcement of the target stimulus; there is no necessity for the target to be presented in the absence of the feature and of reinforcement for it to acquire occasion-setting properties. This observation may not be accommodated by those theories according to which the target must also be nonreinforced for the feature to acquire occasion-setting properties (e.g., Bouton & Nelson, 1994; Rescorla, 1988). It is, however, consistent with the proposal made by Bonardi (e.g., 1989) that an occasion setter's properties derive from the formation of an association between it and a representation of the CS-US pairing, that gives it hierarchical control over the information inherent in the CS-US association. (cf. Honey, 2000).

These data are also inconsistent with most types of configural account. According to this class of theory, performance on an occasion setting discrimination arises because a configural cue, composed of a combination of the feature and the target,

is an important predictor of reinforcement, so that animals respond most when feature and target are presented together. There are several versions of such an account. For example, Wagner and Rescorla (1972) argued that when two cues are presented together, a configural element is activated in addition to the stimulus elements activated by presentation of either cue alone. This cue is not salient, and normally would not acquire considerable associative strength from simple reinforcement of a compound stimulus. But if the two cues comprising the compound are only reinforced when presented together, and nonreinforced when presented alone, then this configural cue will acquire most of the available associative strength. In a standard occasion setting discrimination the feature is usually temporally removed from US presentation, and so only poorly associated with that US, and the target is explicitly nonreinforced; this means that the conditions required for the configural cue to acquire associative strength are satisfied. But according to this account occasion setting will not occur after nonexplicit training, in which the target is never presented alone without reinforcement. In this case the feature and target cues will acquire associative strength at the expense of the configural cue, and when the feature is presented with another target, as in the test phase of the present experiments, the only process operating to determine conditioned responding would be summation, from which one would predict no difference in responding on same and different trials. In addition to not being able to explain the observation of occasion setting after nonexplicit training, this theory has no way of accounting for the negative priming effect. If the configural cue has no associative strength, then there should be no difference in responding on same and different trials. But if it does possess some strength, then presenting a new feature/target combination can only replace the original

configural cue with some novel one, which would necessarily *reduce* the level of responding on different trials. There is therefore no way that such a theory could predict an increase in responding on different trials. It seems, therefore, that this theory cannot explain any aspect of the present pattern of results.

A different type of configural theory has been developed by Pearce (1987). According to his account, presentations of a compound stimulus activate the representation of a configural cue unique to that compound, rather than activating the representations of the elements that comprise it. This means that no special training is required to ensure that the configural cue acquires associative strength. The representations of the stimulus elements that constitute that compound are partially, but not completely, activated by compound presentation, and vice versa, so that generalization between elements and compound is incomplete -- *generalization decrement*. Accordingly when the feature is presented with the alternative target, as in our experiments, the absence of the configural cues present during training, and the failure to obtain complete generalization from the stimuli from the two feature-target compounds, ensure that less responding is observed on different than on same trials. But, like the configural theory invoking unique cues, this model fails in that it cannot explain the negative priming effect: the basis of the model is that stimulus configurations that are reinforced acquire associative strength, and any change from those configurations will induce generalization decrement, and hence a loss of responding. It cannot, therefore, explain why in the present experiments presenting feature-target combinations that had *not* been reinforced during training should ever elicit *more* responding than those that had.

One configural account that can explain both aspects of these data, however, is that recently proposed by Wagner and Brandon (2001). The full scope of the model is too complex from the present paper, but one key feature is that it retains the assumption of Wagner and Rescorla (1972) that presentation of a stimulus compound will result in the activation of a node corresponding to the conjunction of the stimulus elements that is not activated by presentation of the elements alone. As with Pearce's model no special training is required to ensure that this stimulus acquires associative strength, and so like Pearce's model it can explain why animals show generalization decrement with novel feature-target combinations -- and hence why occasion setting can be observed after nonexplicit training. The critical feature of this model that discriminates it from Pearce's is that it incorporates the same rules about stimulus processing as did Wagner's 1981 model, and so can also accommodate the fact that when the feature-target association is strong, processing of the target is attenuated. Thus it can easily accommodate the negative priming effects observed in Experiments 1a and 1b -- that when the feature-target association is strong, animals will respond more when the target is presented with another feature. The present data do not allow us to decide between this configural account and the proposal made by Bonardi (1989). But they do provide convincing evidence that nonexplicit training is sufficient to produce occasion setting.

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Figure captions

Figure 1: Group mean corrected scores during the auditory stimuli x and y on same and different trials, during the test sessions of Experiment 1a. The bars show standard errors.

Figure 2: Group mean corrected scores during the auditory stimuli x and y on same and different trials, during the test sessions of Experiment 1b. The bars show standard errors.

Figure 3: Group mean corrected scores during the auditory stimuli x and y on same and different trials, during the test sessions of Experiment 2. The bars show standard errors.

Figure 4: Group mean corrected scores during the auditory stimuli x and y on same and different trials, during the test sessions of Experiment 3. The bars show standard errors.

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Table 1

Design of Experiments 1a and 1b.

Training	Test: SAME	DIFFERENT
A → x+	A → x+	A → y+
B → y+	B → y+	B → x+

Note: A and B refer to the illumination of the jewel and magazine lights, and were counterbalanced across x and y, which refer a white noise and a tone. + refers to the delivery of a food pellet. The two groups in each experiment differed in receiving different numbers of sessions in the training phase. In both experiments one group received 8 sessions; in Experiment 1a the second group received 16 sessions, and in Experiment 1b it received 24.

Table 2

Design of Experiment 2.

	Preexposure	Training	Test: SAME	DIFFERENT
Group E	A-, B-	A → x+, B → y+	A → x+	A → y+
Group C	nothing	A → x+ B → y+	B → y+	B → x+

Note: A and B refer to the illumination of the jewel and magazine lights, and were counterbalanced across x and y, which refer a white noise and a tone. + refers to the delivery of a food pellet.

Table 3

Design of Experiment 3.

	Training	Test: SAME	DIFFERENT
Group E	A → x+, A- B → y+, B-	A → x+	A → y+
Group C	A → x+ B → y+	B → y+	B → x+

Note: A and B refer to the illumination of the jewel and magazine lights, and were counterbalanced across x and y, which refer a white noise and a tone. + refers to the delivery of a food pellet.







