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The Role of Awareness in Pavlovian Conditioning: Empirical Evidence and Theoretical Implications

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This article reviews research over the past decade concerning the relationship between Pavlovian conditioning and conscious awareness. The review covers autonomic conditioning, conditioning with subliminal stimuli, eyeblink conditioning, conditioning in amnesia, evaluative conditioning, and conditioning under anesthesia. The bulk of the evidence is consistent with the position that awareness is necessary but not sufficient for conditioned performance, although studies suggestive of conditioning without awareness are identified as worthy of further investigation. Many studies have used inadequate measures of awareness, and strategies for increasing validity and sensitivity are discussed. It is concluded that conditioning may depend on the operation of a propositional system associated with consciousness rather than a separate, lower level system.

Despite its apparent simplicity, Pavlovian or classical conditioning has proved stubbornly resistant to complete theoretical analysis. In a conditioning procedure a neutral conditioned stimulus (CS; e.g., a tone) is temporally paired with a motivationally significant unconditioned stimulus (US; e.g., an electric shock) and as a consequence comes to evoke a new pattern of behavior (the conditioned response, CR; e.g., a change in heart rate). Much is now known about the laws or principles of animal conditioning (Mackintosh, 1983; Pearce & Bouton, 2001), and these laws appear to apply equally well to human conditioning. However, a major unresolved issue is the extent to which conditioning is a higher order cognitive activity in the sense of requiring or being mediated by beliefs, expectations, and other cognitive states that in humans are characterized by their accessibility to consciousness. In this article we review evidence concerning the relationship between conditioning and awareness. Is conditioning independent of awareness, as some have argued (e.g., Squire, 1994), or is it instead fundamentally dependent on conscious cognition?

The answer to this question is important to basic researchers for several reasons. First, it provides information on the mapping between animal conditioning and human cognitive processes by linking conditioning to symbolic, attentional processes on the one hand or to automatic, procedural processes on the other. Such translations allow advances in each experimental field to inform the other and also clarify how conditioning can best be applied in practical domains, such as clinical disorders. Second, a better understanding of the relationship between conditioning and conscious, symbolic processes helps constrain the search for the neural bases of conditioning by indicating whether we are searching for a mechanism that can be thought of in terms of excitatory and inhibitory links or a more complex representational system that may require a network of linkages. Increasingly, advances in neuroscience are coming from studies of brain damage and imaging in humans, and this knowledge needs to be articulated with knowledge derived from animal research. Finally, regardless of how conditioning is construed, the study of awareness speaks to the question of how many processes are involved in conditioning. A correspondence between CRs and conscious knowledge would suggest a single learning process, whereas dissociations between these measures would suggest multiple learning processes, each potentially with its own laws, neural mechanisms, and applications.

We focus in this review on the literature post 1990, although we will refer to studies prior to that date where they are of particular significance. Several reviews of the earlier literature are already available (Boakes, 1989; Brewer, 1974; Dawson & Schell, 1985). For present purposes, the most important feature of these reviews is that despite the very different backgrounds of the authors, all three concluded that conditioning in the absence of conscious awareness had not yet been convincingly demonstrated. Since 1990, not only has additional empirical evidence been collected in traditional procedures but a variety of new techniques have been developed, such as methods for studying conditioning with subliminal stimuli and conditioning of affective responses. In addition, advances have been made in the theoretical analysis of related phenomena, such as implicit learning. It is therefore worth stand-

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ing back and asking what general conclusions can be drawn from this extensive body of new research and analysis. We begin by considering what exactly is at issue in this field of research and then discuss some of the major methodological and conceptual principles that need to be considered in assessing the relationship between conditioning and awareness.

Three Models of the Role of Awareness in Conditioning

It is generally agreed that conditioning is an associative process that is initiated by exposure to a contingency in the environment between the CS and US (Dickinson, 1980; Rescorla, 1988). In humans, there are two outcomes of such exposure that indicate learning has taken place. One is that, like animals, humans show CRs to the CS. The other is that they are able to report the nature of the contingency between the CS and US. Conscious awareness of the contingency has generally been taken as evidence that humans are capable of forming an internal representation of the environmental contingency in a symbolic or propositional form. But what is the nature of the relationship between such conscious knowledge and performance of the CR?

One class of model assumes that there is only a single learning process and that this process is propositional. In a "strong" version of the single-process model, awareness is given causal status: The learning process is assumed to lead to conscious awareness, which then gives rise to the CR (Figure 1a). In a "weak" version of the single-process model, the learning process gives rise to both conscious awareness and production of the CR (Figure 1b). This version could be classed as an epiphenomenalist model because awareness is granted no causal role in eliciting the CR. A second class of model (Figure 1c) postulates two independent learning processes. The first process is assumed to be propositional in nature and to lead to conscious awareness. The second process is assumed to be a lower level, nonpropositional process that activates CRs by a direct mechanism such as formation of an excitatory link between the CS and US nodes.

The critical task for the single-process models is to explain how propositional knowledge could lead to elicitation of a CR. The most obvious possibility is to assume that presentation of the CS retrieves the CS–US contingency, which leads to activation of a representation or expectancy of the US, which in turn elicits an appropriate anticipatory CR. Prima facie evidence for such a process is that participants in a conditioning task report that they are thinking about or expecting the US during presentation of the CS. In the strong version of the model the conscious US representation or expectancy itself triggers the CR, whereas in the weak version activation of the underlying US representation or expectancy leads to both conscious awareness and CR performance. Dual-process models can give a similar account of the translation of contingency knowledge into US expectancy but of course deny that such expectancies (or the underlying symbolic process that generate them) have any causal role in CR production.

It is important to note that if conscious expectancies do play a causal role in producing CRs, as claimed by the single-process models, that role is likely to be restricted to triggering the CR rather than the low-level task of actually generating the CR and determining the details of its topography. For example, CRs often involve activation of emotional response systems whose details depend on the prior organization of that system. Furthermore,

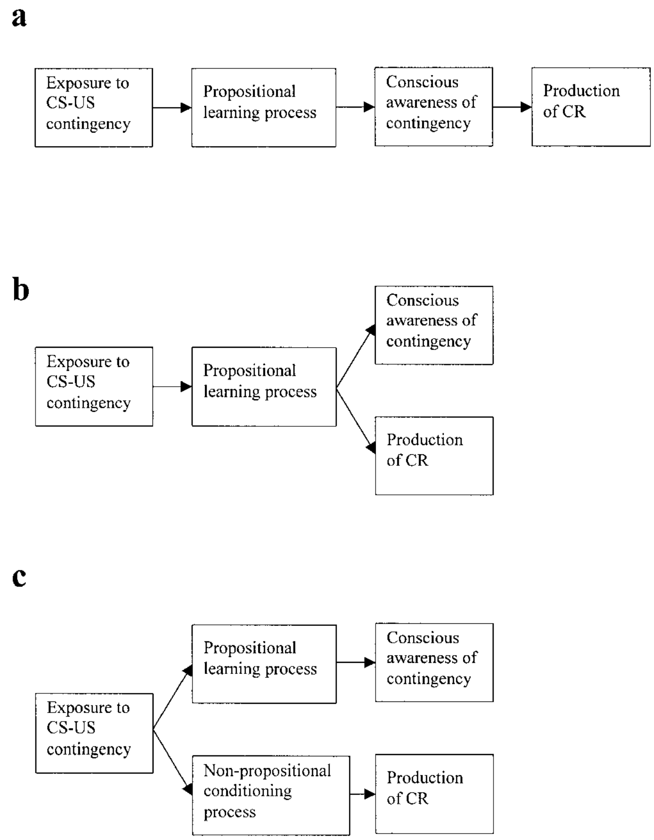

Figure 1. Panel a: Strong single-process model of the relationship between condition and awareness; Panel b: weak single-process model; Panel c: dual-process model. CS = conditioned stimulus; US = unconditioned stimulus; CR = conditioned response.

research with omission schedules has shown that animals often cannot suppress CRs even when it is advantageous for them to do so (see Dickinson, 1980), suggesting that CR production is automatic rather than voluntary. This finding is obviously compatible with dual-process models, which attribute CR production to a separate system that is automatic from beginning to end. However, it can also be accommodated by single-process models by assuming that activation of a representation or expectancy of the US automatically triggers anticipatory CRs appropriate to the US. Some support for the idea that conscious processes can trigger anticipatory responses comes from research in which expectancies established by a verbal warning of electric shock yield similar anticipatory responses to those elicited by a CS paired with shock (e.g., Epstein & Roupenian, 1970).

The primary differences between the three models, therefore, concern the role of contingency awareness and US expectancy in triggering CR production. By *contingency awareness* we mean knowledge that a specific CS predicts a specific US ("the tone predicts shock"), whereas by *US expectancy* we refer to the fact that presentation of a particular CS may trigger awareness of the imminent delivery of a specific US. The difference between these constructs from a conceptual point of view relates to justification. If the participant merely expects the US but cannot say why, then he or she would be classified as expectancy aware but not as contingency aware. On the other hand, participants who can justify their US expectancy by reporting the contingency ("I expect the shock because the tone predicts shock and the tone just occurred") would be classified as both expectancy aware and contingency aware. We will consider measures of both contingency knowledge and US expectancy and will discuss the question of whether they reflect the same processes in the General Discussion.

All three models in Figure 1 predict that in many learning situations both CRs and awareness will be observed. However, the models differ in their predictions about how closely these two learning outcomes will be related. The strong single-process model predicts a very close correspondence between CRs and awareness because the former is assumed to depend entirely on the latter. The weak single-process model predicts a close correspondence between CRs and awareness because both share a common cause, but it can allow for some dissociations. For example, suppose that the transfer of fine temporal information from the learning system to awareness is not very good; it would be possible to find participants who could not accurately report CS–US order, no matter how sensitive the test, but who still showed appropriate CRs. In the extreme case, it would be possible for a situation to arise whereby awareness is completely blocked (e.g., by brain damage or anesthesia) but not CR performance. Finally, on the dual-process model, it should be possible and even common to observe CRs without awareness and awareness without CRs because these are assumed to be regulated by two independent processes. Because the unique feature of the dual-process models is the postulation of a separate, noncognitive conditioning process, the crucial test of such models has usually been taken to be a demonstration of CR performance in the absence of awareness.

It is interesting to note that the models distinguished above have rarely been made explicit in the literature. The single-process model has been championed by Brewer (1974), but contemporary authors rarely articulate a clear version of this model. Similarly, the dual-process model has been advocated by Razran (1955) and more recently by Squire (1994), but it is a widely endorsed model that is implicit in the writings of many conditioning researchers. We consider that the models in Figure 1 represent the primary positions taken in the literature, although variants of these basic models are of course possible. For example, Dawson and Furedy (1976) proposed a "necessary gate" version of the dual-process model in which conscious awareness was considered necessary for initiation of the lower level conditioning process. We will consider variations of the three models in Figure 1 where relevant to the discussion of particular results.

Assessing the Relationship Between Awareness and Conditioning

Reliability of Measures

It is important to distinguish between measured variables and the theoretical constructs they are assumed to index. Awareness is always measured by self-report, such as a questionnaire, an interview, or a rating task. CRs are usually measured by an objective procedure such as psychophysiological responding or behavioral recording. All of these measures are subject to influence and constraint by a range of causal factors above and beyond the primary factor of theoretical interest. These extraneous factors will add "error" variance to the measures and reduce their reliability. For example, CRs are influenced not only by central associative strength but also by performance factors such as habituation, individual differences such as variations in responsivity, and extraneous factors such as external stimuli. These factors would not be expected to influence measures of awareness. Correspondingly, awareness measures may be influenced by demand characteristics and individual differences in reporting thresholds or in the use of rating scales that would not affect CR measures. Both measures may additionally be subject to floor and ceiling effects in particular contexts. All of these measurement artifacts will add extraneous variance to the measures and, importantly, will act to systematically reduce the size of the observed relationship between them. In the extreme case, a complete dissociation could be observed due to artifact alone: For example, a completely unreliable measure of awareness would by definition be unrelated to CR performance.

One implication of the foregoing is that *correlational* analyses of the relationship between conditioning and awareness are likely to be relatively uninformative. Several studies (e.g., Baeyens, Hermans, & Eelen, 1993) have reported low and nonsignificant correlations between conditioning and awareness measures without reporting whether awareness is absent in groups or subgroups of participants who show reliable conditioning. If the relationship between awareness and CR magnitude is nonlinear (e.g., a step function), if performance on either measure is at floor or ceiling or is restricted in range, or if there are performance variables that affect CR magnitude but not awareness, then low correlations would be expected even by single-process models.

In general, the range of factors that can interfere with the expression of CRs is greater than is the case for self-report measures, and CR measures are correspondingly less reliable (i.e., contain more error variance). Therefore, a dissociation in which participants show significant awareness but not significant CR performance may plausibly be due to performance or measurement effects alone. Even the strong single-process model (Figure 1a) allows for such a dissociation at the level of observed variables. However, a demonstration of the converse dissociation, in which participants show significant CR performance in the absence of awareness, would be less likely to be due to measurement issues. As argued earlier, this type of dissociation is also the one of most theoretical interest in distinguishing dual-process models from single-process models. Therefore, we will place particular emphasis on possible demonstrations of conditioning in the absence of awareness.

Statistically, the foregoing considerations highlight the importance of two tests of the relationship between conditioning and awareness. First, a comparison between participants classified as aware and unaware in terms of their CR performance provides a direct test of the relationship between awareness and conditioning. Second, a test of conditioning in unaware participants alone provides a test of the necessity of awareness for conditioning. Evidence for a relationship between conditioning and awareness is provided by a significant outcome of the first test and a null result on the second. Conversely, evidence for unconscious learning is provided by a null outcome on the first test and a significant outcome on the second. Null results are subject to well-known limitations, so presentation of the results of both tests allows for a clearer discrimination between these two patterns.

Validity of Measures

In addition to being reliable, measures should also be sensitive and valid indicators of their presumed theoretical construct. In contrast to the discussion of reliability above, it is the measurement of awareness that raises the most concerns with respect to validity. We summarize below four principles that we consider important for the valid assessment of awareness.

Relevance. The primary requirement is that the measures used should tap those aspects of conscious awareness that are relevant to the associative learning task. In particular, in order to provide an adequate test of the single-process models, they should measure all conscious knowledge that is relevant to production of the CR. For example, participants in conditioning experiments sometimes report hypotheses such as “all the bright colors lead to shock” or “no more than two trials of one type will occur in a row.” If these hypotheses are correlated with the true contingency, the participant may achieve above-chance conditioning performance but still be classified as unaware if the awareness measure relates only to the nominal CS–US relationships. Shanks and St. John (1994) refer to the need to measure these correlated hypotheses as the *information criterion*.

At the same time, awareness measures should not address material that is irrelevant to the question being investigated. For example, some experimenters have asked participants to report on the nature of their CRs or URs, but as argued earlier, these responses often involve activation of complex preorganized systems that the participant can hardly be expected to report fully. For example, a CS paired with an aversive US such as shock may trigger the anxiety system, leading to the coordinated activation of multiple neural, endocrine, and behavioral responses, many of which will be unavailable to awareness. Furthermore, there is good reason to believe that anticipatory CRs are triggered automatically rather than voluntarily, and such a mechanism of CR production is compatible with all three models in Figure 1. The primary point at issue is whether awareness plays a role in triggering CRs, and if the goal is to address this issue, measures of awareness should focus on contingency awareness and US expectancy.

Immediacy. It is well established that reports of conscious content are more complete the sooner they are recorded after a target event. The greater the passage of time, the greater the opportunity for forgetting, interference, and intrusion of invalid material (Ericsson & Simon, 1984). This principle suggests that concurrent measures, taken during the learning process, will be more sensitive than postexperimental measures. US expectancy measures lend themselves to concurrent recording, but contingency knowledge can also be recorded concurrently by asking participants to verbalize their hypotheses during the intertrial interval. When postexperimental measures are taken, they should occur as soon as possible after acquisition. It is even important to keep the duration of postexperimental testing itself to a minimum to reduce loss of information. Dawson and Reardon (1973), in the only direct experimental test of different methods of assessing awareness after conditioning, demonstrated that short questionnaire measures were more sensitive than were long ones.

The interpolation of additional phases or tasks prior to postexperimental reporting will also increase the possibility of forgetting. For example, Fulcher and Cocks (1997) gave one of their groups an affective rating task prior to measuring awareness in an evaluative conditioning procedure (described in more detail later). In this group, 29% of CS–US word pairs were recalled, compared to 56% of CS–US pairs in a group that went straight from the conditioning phase to the awareness test. The interpolation of an extinction phase prior to postexperimental reporting, a common feature of conditioning experiments, is even more problematic. It not only introduces a delay but also involves establishment of a new contingency that directly interferes with recall of the training contingency.

Sensitivity. Assessment of awareness should occur under optimal retrieval conditions. Ideally, conscious knowledge should be measured in the physical presence of the target stimuli, as is the case with expectancy ratings recorded during the CS. When this is not possible, testing conditions should approximate those of learning as closely as possible. For example, recognition tests are known to be more sensitive than free recall tests, and this superiority has been specifically demonstrated for contingency knowledge (Dawson & Reardon, 1973). Finally, the measurement instrument for awareness should be sensitive and psychometrically sound. Ratings that are continuous, such as visual analogue scales, or that have multiple response options, such as Likert scales, are generally more sensitive than binary true–false ratings and allow the participant to report low confidence or partial knowledge.

Reactivity. At first glance, the most straightforward way to determine the relationship between conditioning and awareness would be to require participants to report their state of awareness on presentation of the CS (for instance, by rating their US expectancy). However, asking the participant at the moment the CS is presented whether he or she expects the US is very likely to direct attention to that relationship. As an illustration, in a study by Baeyens, Eelen, and Van den Bergh (1990) that will be discussed in more detail later, the proportion of CS–US relationships nominated on a postconditioning recognition test increased from 18% to 77% when the participants also gave concurrent US expectancy ratings during the learning stage. Clearly, the concurrent index of awareness directed participants’ attention to the relationship and affected the very entity it was designed to measure (this is termed *reactivity*). Various strategies exist to circumvent this problem such as the use of masking tasks to divert attention and reduce the extent of contingency awareness.

Although concurrent awareness measures may increase awareness, it must be borne in mind that the alternative is to risk having a measure of awareness that is less sensitive. The mere passage of time between conditioning and a postconditioning recall or recognition test may cause forgetting. If concern about avoiding reactivity compels an experimenter to use a postconditioning test of awareness, then one rarely exploited way to check the magnitude of forgetting is to have an additional test of conditioning after the awareness test; participants thus receive a conditioning phase, an awareness test, and then a final test in which conditioned responding to the CSs is measured. If this final test yields evidence of conditioning whereas the awareness test suggests that contingency awareness is absent, then the viability of a pure forgetting account can be discounted.

With these theoretical and methodological points in mind, we now turn to our review of the recent research literature. We discuss autonomic conditioning, conditioning with subliminal stimuli, eyeblink conditioning, conditioning in amnesia, evaluative conditioning, and conditioning under anesthesia.

Autonomic Conditioning

Historically, the bulk of research concerned with awareness and conditioning has made use of an autonomic conditioning procedure. In this procedure, the US is a mildly aversive stimulus such as an electric shock or a loud noise, and the CR measured is an index of autonomic arousal, usually an electrodermal measure such as skin conductance responses (SCRs). CSs are typically simple visual or auditory stimuli, but sometimes pictures, presented for 5–10 s. Reviews of the early literature have consistently concluded that differential autonomic conditioning is strongly associated with verbal measures of contingency knowledge and in particular that differential conditioning does not occur in the absence of such knowledge (e.g., Davey, 1987; Dawson & Schell, 1985; Öhman, 1979). In contrast to other conditioning preparations, it is relatively uncommon for participants to be classified as contingency aware yet fail to show differential autonomic responding. Thus, although some reviewers have emphasized that contingency awareness is necessary but not sufficient for autonomic conditioning (Dawson & Furedy, 1976), others have argued that it is both necessary and sufficient (Brewer, 1974).

Perhaps because of the relatively clear pattern of existing data, relatively few articles have been published over the review period that focus directly on the role of awareness within a conventional autonomic procedure. However, several researchers have investigated other factors or measures that may influence the relationship between awareness and conditioned responding. Marinkovic, Schell, and Dawson (1989) tested the effect of using odors as CSs and found the same pattern as previously reported for visual and auditory stimuli: Differential skin conductance CRs were strongly related to concurrent US expectancy ratings and to postexperimental contingency knowledge, and no CR differentiation was found in participants classified as unaware. This result is interesting because it suggests that the strong relationship between awareness and autonomic conditioning is due to the aversive US or the autonomic measure rather than characteristics of the CS (such as modality or ease of naming). For example, the weaker role of awareness observed in evaluative conditioning with tastes and odors (see below) must be due to the US (gustatory) or measure (sweetness or preference) rather than to the use of an odor CS.

Lovibond (1992; Experiment 1) used much longer CS durations than are usual in autonomic conditioning (20–50 s) and recorded tonic skin conductance over the duration of the CS rather than the more conventional phasic response to CS onset. Despite these differences, the typical pattern was observed of a strong relationship between contingency awareness and differential skin conductance responding and no differentiation in unaware participants. Additional analyses were conducted to examine the relationship between verbalizable knowledge and magnitude of the skin conductance response. Previous attempts to relate these two variables have examined correlations between participants, thus allowing the wide individual differences in skin conductance reactivity to attenuate the observed association. In this study correlations were instead calculated across acquisition trials (A+, B−) separately for each participant. The mean within-subjects correlation between on-line US expectancy ratings and the tonic skin conductance measure was .44 in participants classified as aware by postexperimental interview and .17 in those classified as unaware. The highly significant association in aware participants contradicts the view that once awareness has been achieved, there is no further relationship between cognitive variables and conditioning outcomes (Dawson & Furedy, 1976; Furedy & Kristjansson, 1996).

Hamm and Vaitl (1996) concurrently measured two autonomic CRs, skin conductance and heart rate, and also CS modulation of the blink startle reflex to a loud auditory probe. The US was electric shock in Experiment 1 and an innocuous reaction time cue in Experiment 2. Analysis of the combined data showed the typical pattern of association between contingency awareness and differential SCRs. Systematic differences between aware and unaware participants were also observed on the heart rate measure, although these differences are hard to interpret because of the fact that differential conditioning did not reach significance on this measure. Of most interest is the finding that awareness was not associated with degree of differential startle modulation. The authors interpret these results as showing that whereas skin conductance and heart rate measures are sensitive to cognitive factors such as stimulus significance, startle modulation measures conditioned fear and may not depend on conscious knowledge. However, there are two factors that reduce the strength of this conclusion. First, awareness was assessed by postexperimental interview after an extended period of extinction that may have interfered with or allowed forgetting of contingency knowledge. Second, no separate analysis was carried out to assess whether significant differential startle modulation occurred in unaware participants.

More significant, however, is the fact that a subsequent study by Purkis and Lipp (2001) found a very strong relationship between blink startle modulation and contingency awareness. Purkis and Lipp used a masking task in which participants were presented with a series of four pictures each shown for 5 s and were requested to report back immediately the sequence of pictures (e.g., “flower, bird, dolphin, mountain”). In some sets, a particular picture (the CS+) was consistently paired with a shock US across 16 trials, whereas in 16 other trials, a control picture (CS−) was presented without the US. There was no extinction phase. Autonomic CRs and startle modulation to a loud probe were measured concurrently as in Hamm and Vaitl’s (1996) experiment (affective ratings were also measured—we describe these in a later section). Awareness was measured both concurrently using an expectancy dial and also using postexperimental questionnaire (these measures were in perfect agreement). Purkis and Lipp found differential startle modulation and skin conductance CRs to CS+ and CS− in aware participants but no differentiation at all in unaware participants. Moreover, they also found that these learning effects only emerged after the point at which participants began to show contingency awareness using the concurrent measure. These findings provide strong evidence that modulation of the startle reflex, like other autonomic measures, is closely associated with awareness.

A final factor that has attracted considerable attention in the autonomic conditioning literature is the role of the “fear-relevance” of the CS. This interest was stimulated by Seligman’s (1971) suggestion that so-called prepared associations between biologically fear-relevant stimuli (potentially phobic objects such as insects) and aversive outcomes may involve a powerful but primitive, nonconscious form of conditioning. Öhman (1979) initiated a program of research to test this hypothesis in an autonomic conditioning procedure with pictures of fear-relevant stimuli (typically snakes and spiders) as CSs. This research has provided some support for Seligman's suggestion in that differential responding to fear-relevant CSs has shown greater resistance to extinction than responding to fear-irrelevant CSs (e.g., Öhman, Erixon, & Lofberg, 1975). In addition, some experiments have supported the claim that conditioning to fear-relevant CSs may be less sensitive to cognitive factors such as verbal instruction and US expectancy (e.g., Hugdahl & Öhman, 1977).

More recent research has questioned each of these conclusions. Dawson, Schell, and Banis (1986) replicated the Öhman procedure but added a masking task to slow down learning and included on-line assessment of US expectancy. They found (a) no evidence of differential conditioning in unaware participants, regardless of fear-relevance; (b) rapid extinction after instruction about US omission, regardless of fear-relevance; and (c) greater resistance to extinction in the fear-relevant group on *both* skin conductance and US expectancy. In a subsequent study, Schell, Dawson, and Marinkovic (1991) investigated whether conditioning with fear-relevant stimuli might be differentially affected by the use of a very short interstimulus interval (0.5 s vs. 8 s) or a forgetting period of 1 or 6 months between acquisition and test. They found no effect of CS fear-relevance on acquisition, retention, or reconditioning but did replicate the finding of greater resistance to extinction to fear-relevant CSs on both expectancy and skin conductance. Dawson and his colleagues concluded that fear-relevant CSs do not induce a dissociation between verbal measures and CRs; rather they lead to persistent performance under extinction on *both* of these measures. Davey (1992) also found in several experiments that skin conductance differences between fear-relevant and fear-irrelevant stimuli (including resistance to extinction) were accompanied by parallel differences on shock expectancy and proposed a general expectancy-based model of fear relevance effects.

In the Schell et al. (1991) study, those participants who had forgotten the experimental contingencies over the 1- or 6-month delay period failed to show differential SCRs, confirming the typical finding of lack of autonomic discrimination in unaware participants. However, a different type of dissociation was observed between autonomic responding and conscious knowledge. Over the course of extinction, differential SCRs were still observed after the point at which differential shock expectancies had completely disappeared. A similar pattern was observed by Dawson et al. (1986). This type of dissociation is intrinsically a weak one because it could be due to different sensitivities or performance thresholds for the two measures. For example, expectancy was recorded using only seven buttons to cover the range from *absolutely certain no shock* to *absolutely certain shock*, making it difficult for participants to express small residual shock expectancies. Nonetheless, the dissociation observed qualifies as an instance of CR production in the absence of measured awareness and warrants further investigation using a more sensitive expectancy measure such as a continuous rating scale and controlling for other potential suppressive influences on expectancy ratings such as demand characteristics.

Although fear-relevant stimuli do not appear to induce a dissociation between autonomic CRs and conscious knowledge, the question arises as to why they differ from fear-irrelevant stimuli on both of these measures. McNally (1987) has reviewed a number of nonassociative explanations for the primary resistance to extinction phenomenon. He concluded that prior fear, CS salience, CS prepotency, CS significance, and selective sensitization could account for some of the observed effects of fear-relevance but that these factors were reasonably well controlled by the design used in most fear-relevance conditioning experiments, in which CS+ and CS− were both fear-relevant, or in a control group, in which CS+ and CS− were both fear-irrelevant. The reasoning here is that any nonassociative attributes of fear-relevant stimuli should be common to both CS+ and CS− and thus could not contribute to differential responding to the CSs.

However, Lovibond, Siddle, and Bond (1993) have questioned whether the between-group design does successfully control for one of these factors, selective sensitization. In this context, selective sensitization refers to the finding that SCRs to fear-relevant CSs are enhanced (sensitized) by a general threat of electric shock to a greater degree (i.e., selectively) than is the case for fear-irrelevant stimuli (e.g., Ohman, Eriksson, Fredrikson, Hugdahl, & Olofsson, 1974). This effect occurs in the absence of any pairings or instructions involving specific CSs and therefore must reflect some prior property of fear-relevant stimuli. In the course of a differential conditioning experiment, the CS+ comes to threaten shock, whereas the CS− comes to predict no shock. It is therefore possible that associative learning could lead to selective sensitization of a fear-relevant CS+ but an absence or suppression of selective sensitization of a fear-relevant CS−. Such an account does not require that associative learning to fear-relevant CSs is any different than learning to fear-irrelevant CSs. Lovibond et al. (1993) provided experimental support for this account and concluded that apparent resistance to extinction of fear-relevant stimuli could be due to selective sensitization rather than superior associative learning. It is also possible that some of the fear-relevance effects observed by Öhman in backwardly masked conditioning could be due to selective sensitization rather than to an unconscious associative process (see following section).

In summary, recent research on autonomic conditioning has confirmed that autonomic CRs are strongly associated with contingency knowledge and US expectancy. An exception to this pattern that is worthy of further investigation is the finding that over the course of extinction, differential CRs can still be observed after expectancy discrimination has disappeared. The use of fear-relevant stimuli as CSs can lead to different conditioning outcomes such as greater resistance to extinction, but these outcomes may be in part due to nonassociative factors and the effects are typically observed on both CR and self-report measures.

Conditioning With Subliminal Stimuli

Several researchers have studied conditioned responding to stimuli putatively below the threshold of conscious recognition. These studies at first sight constitute a powerful body of evidence for conditioning without awareness. However, as with research on autonomic conditioning, closer inspection suggests that the case is far from proven.

Soares and Öhman (1993) conditioned fearful and nonfearful participants to either fear-relevant (snakes, spiders, rats) or fear-irrelevant (flowers and mushrooms) CSs with shock as the US. After differential conditioning developed, the CSs were extinguished under masked conditions in which each slide appeared for 30 ms, followed immediately by a 100-ms presentation of a masking slide that was made from cutting and reassembling a CS slide. Soares and Öhman found that differential SCR responding to the CS+ and CS− persisted during extinction for the fear-relevant but not for the fear-irrelevant CSs in both the fearful and nonfearful participants and concluded that this continued responding must have been mediated by an automatic and unconscious processing mechanism (see Esteves, Dimberg, & Öhman, 1994, for similar results with faces as CSs).

The key evidence that the masked stimuli in Soares and Öhman's (1993) study were not consciously recognized came from a postextinction test in which CSs were presented under both masked and unmasked conditions and participants had to quantify on a scale from 1 (*never*) to 9 (*always*) how often each slide had been followed by shock. Soares and Öhman reported that participants gave reliably higher ratings for the nonmasked CS+ compared with the CS− but did not distinguish the CSs under masked conditions. Soares and Öhman did not provide any details of ratings in the latter case and so it is unclear whether there was a trend toward differential ratings, but in any case this test does not prove that the masked slides were not consciously perceived: Because both the CS+ and CS− slides predicted shock absence *when presented under masked conditions* in the extinction phase, participants were actually correct to give them ratings close to 1 in the shock association test. The fact that the same slides had different shock associations when presented unmasked in the acquisition stage is irrelevant for a participant who interprets the test as asking about the predictiveness of the slide under the precise conditions of its presentation in the test and who does not generalize strongly from a masked to an unmasked CS. As Öhman and Soares (1994) themselves noted in a replication of this finding, "the subjects *correctly* [italics added] identified that neither the masked presentations of the CS+ and the CS− . . . had been followed by shock during the experiment" (p. 126).

Öhman and Soares (1993, Experiment 1) conducted a series of different tests aimed at determining whether masked slides could be consciously recognized or not. Participants were presented with slides (snake, spider, flower, or mushroom) at various stimulus onset asynchronies (SOAs) prior to the mask, which was always presented for 100 ms. The participants' task was to identify each slide. When the SOA was 30 ms, identification accuracy was no better than chance (25% correct), and Öhman and Soares demonstrated that performance was still at chance in a repetition of the test given after administration of a series of shock USs.

Öhman and Soares (1993) interpreted the data from this experiment as confirmation that a masked 30-ms CS slide cannot be consciously *recognized*. In a subsequent experiment (Experiment 2), they found that masked fear-relevant CS+ and CS− slides elicited differential CRs after a conditioning stage with unmasked slides. We do not dispute the claim that these masked slides were unrecognized, but the problem is that the recognition test is not relevant to the interpretation of whether conditioned responding is mediated by contingency awareness or not. What Öhman and Soares demonstrated was that under these presentation parameters, participants cannot identify a masked snake slide *as a snake* or a masked spider slide *as a spider*, but this is too stringent a test for present purposes. All participants need to do in the masked test, we contend, is to discriminate between the CS+ and CS− slides and to differentially activate an expectancy of the US; they do not need to be able to identify the slides as snakes, spiders, or whatever. A participant may detect certain features (call them X features) in the masked CS+ slide and others (Y features) in the masked CS− slide and may be aware that X predicts the US and that Y predicts no US, without being aware that the X features are components of a snake and the Y features are components of a spider. Another way of putting this point is that the masked CS+ slide simply needs to generalize more associative strength from the unmasked CS+ than does the masked CS− slide and this will be the case if it merely contains a few relevant discriminating features. Hence, differential responding to masked and unrecognized stimuli does not need to be based on automatic or unconscious processes. Nor does the failure of participants to perform above chance in the recognition test mean that "conscious perception of stimuli" was prevented or that participants were "unaware of the eliciting stimulus," as Esteves, Parra, Dimberg, and Öhman (1994, pp. 375–376) claimed.

To be fair, Öhman and his colleagues subsequently acknowledged this possibility. For instance, Öhman, Esteves, and Soares (1995) conceded that an alternative interpretation of the occurrence of differential conditioning combined with chance recognition

> would be that the subjects were able to discriminate but not necessarily recognize the CS+ and the CS− when they were fear-relevant. In this case, they would be able to predict, and thus expect, the shock, even though they would not be able to specify which masked CS-presentation involved the snake and which the spider. Rather they could base their expectancies on vague hunches either from some information leaking through the mask or, perhaps more theoretically interesting, from bodily feedback originated in the conditioned response. (p. 104)

This possibility critically undermines the case for claiming that the masking data provide evidence for automatic or unconscious expression of conditioned responding and effectively invalidates the recognition test.

Another aspect of the results reported by Öhman and Soares (1993), however, does suggest a qualitative difference between masked and nonmasked CS presentations. Öhman and Soares (1993) found differential responding to the nonmasked CS+ and CS− for both fear-relevant and fear-irrelevant stimuli (if anything, discrimination was better for the fear-relevant stimuli), but differential responding to the *masked* stimuli only occurred in the fear-relevant case. As several authors have noted (e.g., Reingold & Merikle, 1990), qualitative differences between responding to putatively subliminal versus supraliminal stimuli offer potentially good evidence for a distinction between unconscious and conscious processes, although it is difficult at present to rule out the possibility that other factors are responsible. For example, Brockdorff and Lamberts (2000) have provided examples of interactions between stimulus type and availability of perceptual features under different exposure durations. One possibility is simply that the snake–spider pairs were more distinct than were the flower–mushroom pairs. This possibility could be tested by evaluating whether the masked flower and mushroom pictures are equally as discriminable as the snake and spider pictures (e.g., by using these stimuli as cues in a nonaversive discrimination learning task).

Whereas the aforementioned studies have examined responding to masked stimuli after acquisition with unmasked stimuli, Esteves Dimberg, et al. (1994) obtained differential acquisition of conditioned responding to masked stimuli and also found in a separate experiment that recognition of these stimuli under the masking conditions they used (30 ms SOA) was at chance. In this case the slides were of happy or angry faces, and differential conditioning required that the CS+ was an angry face. As with the Öhman and Soares (1993) study, however, these results do not establish that participants were unaware of all distinguishing aspects of the stimuli. For differential conditioning to occur in the absence of recognition, it would be sufficient for the CS+ and CS− faces to contain discriminable features and for those features to be differentially associated with the US.

A similar pattern was obtained in a further study (Öhman & Soares, 1994) in which fearful and nonfearful participants saw slides of snakes, spiders, flowers, or mushrooms as the stimuli. SCRs were measured for the fear-relevant and fear-irrelevant stimuli, but no USs were presented at any time. In addition to obtaining differential responding to masked fear-relevant stimuli in the fearful participants, the study included a posttesting self-assessment test of awareness that yielded results at variance with the data from a recognition test (which once again showed chance levels of performance). In the self-assessment test participants were shown the masked stimuli together with novel stimuli and were asked (a) to try to identify each slide and (b) to rate their felt arousal, valence, and control-dominance to each picture using 9-point ratings scales. On this test, participants gave ratings that were consistent with their SCRs: That is, snake-fearful participants who emitted SCRs to masked slides of snakes rated themselves as fearful of the snake but not the spider slides, whereas spider-fearful participants who emitted SCRs to masked slides of spiders rated themselves as fearful of the spider but not the snake slides. Nonfearful control participants, who did not show elevated responses to the masked CS slides, also did not give differential ratings to them. We contend that because this test is not flawed in the way that the recognition test is—that is, because it does not require more detailed information than would be necessary to show differential conditioning—its results are more germane than those of the recognition test to the issue of whether awareness is necessary for conditioning. As Öhman and Soares (1994) acknowledge, the self-assessment results indicate “that some information about the stimuli became available to introspection and the verbal-cognitive system” (p. 238).

Several other recent reports have used variants of the recognition test to index awareness and are therefore also subject to the objection that this test is unfairly biased toward finding “unaware” conditioning. Morris, Öhman, and Dolan (1998) initially presented participants with a pair of unmasked angry faces as CS+ and CS− with a burst of loud noise as the US. Then in a subsequent test the CSs were presented masked and Morris et al. observed differential activation of the right amygdala to the CS+. Even if we assume that amygdala activity plays a causal role in the production of a CR, however, the conclusion that responding was evoked unconsciously is unwarranted. To assess awareness, participants were shown the two angry faces at the outset of the experiment and were instructed to press a button during the test phase whenever they saw one of them; when the faces were masked, they never did so. But the test again requires recognition of the faces *as the target angry faces,* which, we have argued, is too stringent. Moreover, it is a yes–no rather than an *n*-alternative forced-choice test, which introduces the additional problem that participants may simply be biased against reporting positive identifications of the target faces. If participants have very low confidence in their identifications and if they have a conservative response bias, correct identifications will often go unreported.

Studies by Wong and colleagues (Wong, Bernat, Bunce, & Shevrin, 1997; Wong, Shevrin, & Williams, 1994) are also inconclusive because of their use of recognition as the index of awareness and for other reasons as well. For instance, these studies used masked pleasant and unpleasant faces as the CSs, but the allocation of faces to CS+ and CS− was not counterbalanced (CS+ was always an unpleasant face), which means that the effects observed may not have been associative. In the Wong et al. (1997) study, no skin conductance data were reported (the main data concerned event-related brain potentials), whereas in the Wong et al. (1994) study, differential responding to CS+ and CS− was statistically only marginal.

To this point our criticisms of studies using subliminal stimulus presentations have highlighted inappropriate methodologies, but we have not described any direct evidence of our major claim, namely that recognition tests are biased against obtaining evidence of CS awareness. Such evidence is contained, however, in a report by Öhman and Soares (1998) (see also Öhman et al., 1995). In their Experiment 1, Öhman and Soares (1998) obtained differential conditioning to masked fear-relevant CSs (snakes and spiders) but not to fear-irrelevant CSs (flowers and mushrooms). In Experiment 2, the CS–US interval was increased to 4 s to allow self-report measures to be recorded on-line. All groups received differential conditioning to fear-relevant stimuli. One group gave no on-line ratings, one rated shock expectancy, and one gave forced-choice recognition and confidence ratings on each conditioning trial. In the latter two groups, expectancy or recognition responses were made during the CS–US interval. All groups showed differential autonomic conditioning and there was no evidence that the concurrent awareness tasks affected the magnitude of differential conditioning. The recognition group failed to distinguish between snakes and spiders during the masked acquisition trials but did so during unmasked extinction. Crucially, the expectancy rating group showed expectancy discrimination between CS+ and CS− during acquisition, which weakened but remained significant during unmasked extinction.

This set of findings demonstrates explicitly that a concurrent expectancy test (like the self-assessment test of Öhman & Soares, 1994) and a recognition test do not yield concordant findings, with the former but not the latter demonstrating contingency awareness. We argue that this result is precisely what would be expected if the recognition test is too stringent in that it requires object identification, which is not necessary for conditioning. The claim that contingency awareness is necessary for conditioning is not undermined by the finding that participants cannot name the stimuli that constitute the CSs; it is only necessary that they be able to differentiate between them.

If we take the expectancy ratings as a more appropriate index of awareness, then the study shows that at the group level, US expectancy covaried with conditioned responding. But we can go on to ask a more important question: Did participants who were not consciously aware of the *relationship* between the stimuli and shock show autonomic discrimination? To address this question, Öhman and Soares (1998) examined electrodermal responding to the CS+ and CS− in those participants in Experiment 2 who showed expectancy discrimination compared with those who did not. In a between-subjects analysis, participants were classified globally as aware or unaware, and Öhman and Soares (1998) reported reliable differential conditioning in those classified as unaware. However, the classification they used assigned a participant to the "aware" subgroup only if he or she gave a positive shock expectancy rating on at least two thirds of the CS+ trials and a negative expectancy on at least two thirds of the CS− trials. This classification is extremely conservative and means that participants would have been classified as unaware even if they showed a good degree of contingency awareness.

In a second within-subjects analysis, trials were classified as being ones on which shock was expected (positive expectancy ratings) or not (negative ratings). In this analysis, Öhman and Soares (1998) again found reliable differential SCR conditioning both when shock was expected and when it was not. However, the significance of this finding is unclear because shock expectancy is a continuous scale, whereas Öhman and Soares (1998) seem to be construing it as a binary one in this analysis. Consider trials on which the shock was not expected (i.e., ratings between 0 and -100). The fact that SCR magnitude was greater on the CS+ than in the CS− trials does not mean that CRs were dissociated from expectancy ratings because participants may have been more certain that shock would not occur on the CS− trials than on the CS+ trials. A similar argument applies to the trials in which shock was expected. Indeed, consistent with this argument is the fact that SCR magnitude was greater to CS+ when shock was expected than when it was not expected (.49 vs. .44) and was less to CS− when shock was not expected than when it was (.31 vs. .35). These analyses therefore fail to demonstrate either that participants unaware of the contingencies showed conditioning or that differential SCRs occurred in trials that did not receive differential expectancy ratings.

Öhman and Soares (1998), acknowledging that the expectancy data indicated that participants were aware of the CS–US contingencies, speculated that autonomic responses contribute to later cognitive processing. The idea that expectancy ratings derive from autonomic feedback is an interesting one. Evidence against this idea as an account of expectancy ratings with supraliminal stimuli, though, is that US expectancy functions are essentially the same regardless of whether the US is aversive or innocuous (e.g., Siddle, Booth, & Packer, 1987); autonomic responses are typically greater when the US is aversive. It would be interesting to similarly measure expectancy ratings to masked CSs with an innocuous US. If expectancy discrimination was observed, one could argue against the idea that expectancy is derived from autonomic arousal.

Eyeblink Conditioning in Normals

Several studies published during the review period assessed contingency knowledge in a Pavlovian eyeblink conditioning procedure. In this task, participants are typically exposed to CSs, such as lights or tones, followed by a puff of air (US) to one eye with a CS–US interval in the range 400–1,200 ms. Conditioning is assessed by eyelid closure during the CS and is usually scored as present–absent using a criterion of 10–20% of the unconditioned response amplitude. Although on-line expectancy ratings have been obtained by using a simple *yes–no* button press during long (1,400 ms) CS presentations (Perry, Brown, & Perry, 1979), it is more common to assess contingency knowledge by interview or questionnaire at the end of the experiment or at various stages during the experiment.

Clark and Squire (1998) have published an influential article concerning eyeblink conditioning and awareness, which we consider in some detail. Clark and Squire (1998) used both delay and trace eyeblink conditioning procedures and tested a group of 4 temporal lobe amnesics as well as various groups of 10–14 matched controls. We will consider the results for the amnesic patients in greater detail in the following section. The normal controls were exposed to delay conditioning with an interstimulus interval (ISI) of 700 or 1,250 ms or trace conditioning with an ISI of 750 or 1,250 ms (trace interval of 500 or 1,000 ms, respectively). A differential conditioning procedure was used with a tone and white noise (counterbalanced) as the CS+ and CS−. There were 60 trials with each CS presented while participants watched a silent movie. After conditioning, participants' contingency awareness was assessed using a questionnaire comprising 17 true–false items (e.g., "I believe the air puff usually came immediately before the tone"). The participants were divided into aware and unaware subgroups based on their questionnaire scores.

In the trace conditions, participants classified as aware demonstrated differential conditioning, but those classified as unaware did not, leading Clark and Squire (1998) to conclude that "for normal volunteers, awareness . . . was a prerequisite for successful trace conditioning" (p. 77). R. Clark (personal communication, March 17, 2000) has kindly made available to us the original data from the study, including the awareness scores, for further analysis (the published article reports tests of correlations between questionnaire scores and eyeblink discrimination but not tests based on awareness classification and trial blocks). We carried out repeated measures analyses using a multivariate model (O'Brien & Kaiser, 1985), which is appropriate when there is a possibility of differential correlations between the repeats, as is the case with successive trial blocks. Our reanalysis confirms that awareness was significantly associated with differential conditioning averaged over blocks, $F(1, 25) = 11.16$, $p < .05$. Furthermore, analysis of the unaware trace participants alone showed that differential conditioning did not reach significance, $F(1, 13) = 3.39$, $p > .05$.

In delay conditioning, the picture was rather different. Both the participants classified as aware and those classified as unaware showed differential conditioning of roughly equivalent magnitude. Our analyses confirmed that awareness was not a significant determinant of conditioning in the delay groups, $F(1, 21) < 1$, $p > .05$, and that the unaware participants alone showed significant discrimination, $F(1, 11) = 37.75$, $p < .05$. A combined analysis of the trace and delay groups showed that the interaction between condition (trace–delay) and awareness was significant, $F(1, 40) = 5.21$, $p < .05$, indicating that awareness was significantly more strongly associated with trace conditioning than with delay conditioning.

How are these results to be interpreted? Clark and Squire (1998) offered a dual-pathway account based on the proposal that trace conditioning relies on declarative or conscious knowledge mediated by the hippocampus and neocortex, whereas delay conditioning relies on procedural knowledge, which can be learned in an automatic, reflexive manner by subcortical structures. However, this conclusion depends on the particular pattern of results that Clark and Squire (1998) reported. This pattern is strikingly inconsistent with previous studies of awareness in differential eyeblink conditioning in two important ways.

First, previous researchers have found it necessary to use a masking task to obtain sufficient unaware participants for analysis. As Baer and Fuhrer (1982) stated, "Conventional eyelid conditioning procedures, using simple, readily discriminable visual or auditory stimuli, militate against study of the awareness–conditioning relationship because so few subjects remain unaware" (p. 138). Tactics used to reduce awareness include masking tasks and the use of abstract or complex stimuli as CSs, such as the grammaticality of a word pair or category membership. Clark and Squire (1998) showed all their participants a silent movie during conditioning, but this relatively undemanding task has been shown to have little effect on either awareness or conditioning performance (Nelson & Ross, 1974; Ross, 1971). Thus, one unusual feature of Clark and Squire's (1998) results is that they classified so many participants as unaware (26 of the 48 participants) under conditions in which previous researchers have found almost all participants to have been aware.

The second unusual feature of Clark and Squire's (1998) study is that their delay conditioning results are out of keeping with all previous studies of this type. We were able to identify four published studies that reported statistical analyses of contingency awareness in differential eyeblink conditioning. All four of these studies used a delay conditioning procedure and one of the aforementioned tactics to reduce awareness. All four studies reported a significant positive association between awareness and eyeblink discrimination, and none reported significant eyeblink discrimination in the participants classified as unaware (Baer & Fuhrer, 1982; Benish & Grant, 1980; Nelson & Ross, 1974; Perry, Grant, & Schwartz, 1977).[1] Thus, paradoxically, the trace conditioning results of Clark and Squire (1998) show the same pattern as the previous literature, although none of the previous studies used a trace procedure and the delay conditioning results show a different pattern, despite using the same conditioning procedure.

In considering why Clark and Squire's (1998) results are discordant with the prior literature, an obvious candidate is their method of assessing awareness. In fact, Clark and Squire (1998) used an extremely high cutoff for defining awareness, a score of 13 out of 17 (76%) on the questionnaire. They justified this cutoff on the basis that "the binomial probability of correctly answering 13 of 17 true or false questions by chance is $p = 0.05$" (p. 80). In fact, the cutoff is .0245, but it is unreasonable to apply this criterion on a participant-by-participant basis: A group of individuals selected on such a basis may well score significantly above the cutoff *as a group* and it is group performance we are interested in (e.g., in the conditioning data). The use of a high cutoff means that many participants scoring above chance (8.5/17) would have been classified as unaware. Consistent with this argument is the fact that in all four groups the mean score of the unaware participants was greater than 8.5. Of the 26 participants classified as unaware, 18 had a score of 9/17 or greater on the awareness test (binomial $p =$ .038). Hence, there is strong evidence that these supposedly unaware subgroups actually included some participants who did possess a degree of contingency awareness. When we reanalyzed the data with a more conservative cutoff, defining participants as aware if they scored above 9, the magnitude of differential conditioning was reduced but it was still significant in the delay participants classified as unaware. Thus, the high cutoff for awareness contributed to but was not the sole factor responsible for the delay conditioning results.

Clark and Squire's (1998) method of assessing awareness can be questioned on several additional grounds. First, the critical questions concerning CS–US contingencies did not occur until after 28 previous questions concerning other parts of the procedure such, as the silent movie. Clearly this procedure may lead to underclassification of awareness due to forgetting and interference, even in the nonamnesic participants. Second, 17 true–false questions were used to classify awareness of the rather simple differential contingency (see Clark & Squire, 1999; Table 1). These questions were highly redundant and confusable, and participants were not able to change earlier answers. Five of the questions referred to the ordering of the two CSs (e.g., "I believe the static noise and tone were always closely related in time"), which are of questionable relevance to ascertaining participants' knowledge of the CS–US relationships. Finally, the nominally correct answer was "true" for only 4 of the 17 questions, further encouraging false-positive responses. A participant who answered correctly to these four questions would need to resist answering "yes" to an additional nine decoy questions to be classified as aware.

In the light of these limitations and the previous literature, there is good reason to suppose that the majority of participants in the delay conditions would have been classified as aware by a more sensitive and appropriate measure of contingency knowledge. Unfortunately, Clark and Squire (1998) were not in a position to provide us with their questionnaire data scored on a question-by-question basis. We were therefore unable to determine whether participants classified as unaware by the four questions that focused clearly on the differential contingency would have shown differential conditioning. Similarly, we were unable to investigate possible reasons why the questionnaire scores were sensitive enough to reveal an association with trace conditioning but not with delay conditioning. For example, it is possible that the trace procedure made it harder for participants to learn the differential contingency (e.g., Baer & Fuhrer, 1968), thus increasing the number of genuinely unaware participants but easier to discriminate the correct CS–US ordering, thus keeping mean questionnaire scores approximately the same.

In sum, Clark and Squire's (1998) study provides only marginal support for the claimed finding of delay eyeblink conditioning in participants who are unaware of the CS–US contingencies. Clark and Squire used a confusing and delayed measure of contingency knowledge and a conservatively high cutoff for classifying participants as aware. The finding of reliable discrimination in 12 participants classified as unaware in the delay conditions is contrary to the four previously published studies of this type with a total of 395 participants. Clark and Squire's study is therefore in urgent need of replication, using a more immediate and sensitive test of contingency knowledge for comparison with their questionnaire.

In a follow-up study, Clark and Squire (1999) tested additional groups of 8–12 participants with either the delay-1,250 or trace-1,000 procedure and introduced two manipulations designed to modify development of conscious awareness. Some participants

[1] Surprisingly, none of these papers was cited by Clark and Squire (1998).

were provided with a verbal explanation of the differential contingency prior to testing, intended to promote contingency awareness (knowledge manipulation). Other participants were asked to engage in an attention-demanding digit monitoring task during the conditioning session, intended to reduce contingency awareness (distraction manipulation). Four groups were tested in an incomplete factorial design: trace-distraction, delay-distraction, trace-knowledge, and trace-knowledge-distraction. The same 17-item awareness questionnaire as used by Clark and Squire (1998) was administered after conditioning. Mean awareness scores for the four groups were 9.1, 12.8, 16.0, and 13.9, respectively. Although no between-group statistical tests were reported, the pattern of means suggests that the distraction task was only effective in preventing awareness in the trace-distraction group, not the delay-distraction group. Provision of knowledge appeared to increase the questionnaire scores, especially when there was no intervening distraction task.

On the eyeblink measure, the trace-distraction group failed to develop differential responding, but the delay-distraction group showed clear acquisition of differential responding. Clark and Squire (1999) interpreted the delay-distraction results as “suggesting that awareness of the stimulus contingencies was unrelated to conditioning performance” (p. 16). However, comparison with the questionnaire scores shows a very close correspondence between awareness and eyeblink performance: The trace-distraction group showed low questionnaire scores and poor performance, and the delay-distraction group showed higher questionnaire scores and better performance. The mean questionnaire score for the delay-distraction group (12.8) was not only above chance but was hardly lower than the delay-1,250 group from Clark and Squire (1998) that was not exposed to the distraction task (13.4). The results therefore suggest that the distraction task was sufficiently powerful to attenuate trace conditioning (measured both by awareness and by eyeblink responding) but not delay conditioning. One obvious explanation for this pattern is that the delay task is simply easier for participants to learn. Note that this explanation is consistent with our earlier contention that the majority of participants in Clark and Squire’s (1998) delay conditioning groups, who were tested without distraction, were aware of the differential contingency.

The eyeblink performance of the remaining two groups was also markedly different. The trace-knowledge group showed strong differential responding from the first trial block, consistent with the hypothesis that contingency knowledge is critical for eyeblink conditioning. The trace-knowledge-distraction group, by contrast, failed to develop differential responding at all. Clark and Squire (1999) concluded from these results that “having knowledge about the stimulus contingencies is not sufficient to support the acquisition of trace conditioning. Subjects must also be able to have accesss to this knowledge . . . during the conditioning session itself” (p. 17). Support for both this data pattern and interpretation comes from an article published 25 years earlier. In two experiments involving 197 participants, Nelson and Ross (1974) demonstrated that masking tasks depressed differential conditioning performance even in participants who were classified as aware (Experiment 1) or who had been informed of the contingency (Experiment 2). They similarly concluded that a secondary task can interfere with the application of contingency knowledge on a trial-by-trial basis. Importantly, however, Nelson and Ross used a delay conditioning procedure, directly contradicting Clark and Squire’s claim that the role of awareness is restricted to trace conditioning.

In summary, Clark and Squire (1999) have provided additional data that support the relationship between availability of contingency knowledge and differential eyeblink conditioning. Although they again conclude that awareness is not necessary for delay conditioning, this conclusion is based on reanalysis of the Clark and Squire (1998) results, not on any new data. Indeed, the only new data concerning delay conditioning (delay-distraction group) confirm substantial contingency knowledge in this group. No comparable analysis to that reported by Clark and Squire (1998) on aware and unaware subgroups was presented, suggesting that despite the distraction task there were too few participants classified as unaware for such an analysis to be conducted. This failure to present a replication of Clark and Squire’s (1998) most contentious result, conditioning in unaware participants, must cast further doubt on the reliability of that result. Finally, Manns, Clark and Squire (2000b) demonstrated that asking participants to predict the US increased both awareness and differential conditioning, whereas asking participants to predict their eyeblink responses reduced both awareness and differential conditioning. Again, however, these effects were demonstrated using trace conditioning and no delay conditioning data were reported.

Two studies published during the review period examined awareness in a single-cue, rather than differential, eyeblink conditioning design. Papka, Ivry, and Woodruff-Pak (1997) exposed 140 participants to either a 400-ms delay conditioning procedure with a tone CS and an air puff US or an explicitly unpaired procedure with the same stimuli. All participants engaged in one of four tasks that they were instructed to focus their attention on: rhythmic tapping, recognition memory, choice reaction time, or watching a video. These tasks were not selected with the primary aim of distracting attention from the conditioning task but rather to activate brain regions thought to be related or unrelated to eyeblink conditioning. At the end of conditioning, participants were given a semistructured interview. Although relatively little detail is provided, it is stated that participants were generally asked (a) if and how they responded to the tone, (b) if and how they responded to the air puff, (c) whether they noticed a relationship between the tone and air puff, (d) whether they purposefully blinked or withheld blinks, (e) what their level of motivation was, and (f) how difficult they perceived the secondary task to be. Unspecified rater(s) used the responses to the interview questions to judge participants’ awareness of the CS–US contingency and CS responding.

It was found that 84% of the participants exposed to delay conditioning correctly stated that the tone preceded the air puff, and 16% stated that there was no tone–puff relationship. By contrast, 65% of the participants exposed to the unpaired procedure accurately described the lack of relationship between the tone and puff, 27% stated that the tone preceded the puff, and 8% described some other relationship. Questionnaire classification did not vary as a function of the secondary task. Papka et al. (1997) divided the delay conditioning participants into quartiles based on their mean percentage of eyeblinks and found that there was no difference in questionnaire classification across these quartiles. However, this type of analysis only evaluates a simple linear relationship between awareness and eyeblink performance. If there are any factors affecting eyeblink responding other than awareness, and there is good reason to believe there are many such factors, then any relationship with awareness will be masked.

A more sensitive test is to examine the eyeblink performance of participants classified as aware (i.e., that the tone preceded the puff) by comparison with those classified as unaware. The mean percent CRs for the 67 aware participants was 50%, and for unaware participants it was 59%. On the basis of this result, Papka et al. (1997) concluded that eyeblink conditioning performance "is not related to awareness of CS–US contingency" (p. 407). However, this conclusion rests very much on the validity of the postexperimental assessment procedure. As in the Clark and Squire (1998) study, previous research suggests that awareness may have been systematically underestimated. Eyeblink conditioning researchers have found that almost all participants become aware of the CS–US relationship when no masking task is used (this is true even of a differential contingency, which is of course more complex than is a single-cue procedure). Even the most demanding of Papka et al.'s secondary tasks failed to influence either questionnaire answers or conditioning performance, suggesting that these tasks were ineffective in masking conscious awareness. Previous research confirms that single-cue conditioning is even more resistant to masking than differential conditioning (Ross & Nelson, 1973).

Finally, Papka et al. (1997) used an awareness measure that is known to underestimate awareness: a long, recall-based interview (Dawson & Reardon, 1973). Thus, it is entirely possible that virtually all participants in Papka et al.'s study were aware of the tone–puff relationship but that a small fraction (13 of 80) failed to express this knowledge in a free-recall format to the satisfaction of the rater(s). A similar criticism can be made of the one previous study we were able to locate in the literature that attempted to divide participants according to awareness after single-cue conditioning (Frcka, Beyts, Levey, & Martin, 1983).

The other single-cue conditioning study published during the review period (Manns, Clark, & Squire, 2000a) overcame some of these difficulties by using a more difficult trace conditioning task and in a second experiment by assessing awareness very early in the conditioning period. In Experiment 1, 10 normal participants were tested under the same conditions as the trace-distraction group in Clark and Squire (1999), except that instead of differential conditioning there was a single tone CS paired with the air puff US on 120 trials. A further 14 participants were tested in the same way but without the distraction task. At the end of the experiment, all participants completed the same questionnaire as used by Clark and Squire (1998, 1999), except that there were only 7 rather than 17 questions concerning CS–US contingency because of the use of a single CS. Consistent with Clark and Squire (1999), the distraction task reduced both eyeblink performance (percent CRs to the tone) and scores on the knowledge questionnaire. Almost all participants scored at or near ceiling on the questionnaire, precluding analysis of aware and unaware subgroups.

In Experiment 2, 20 normal participants were tested under the same procedure as the no-distraction group of Experiment 1. After 10 trials, after 60 trials, and at the end of the experiment, participants were asked the seven questions concerning contingency knowledge from the questionnaire used in Experiment 1. Again, most participants scored near ceiling on the knowledge questionnaire at the end of the experiment ($M = 6.6$ out of 7). However, after 10 trials their mean score was low enough (5.2 out of 7) that by using a high cutoff (7 out of 7) it was possible to define sufficient unaware participants to test for a relationship with eyeblink performance. Participants classified as aware emitted significantly more eyeblink responses to the CS over the remainder of the session than those classified as unaware. Manns et al. (2000a) concluded "that awareness precedes and contributes to acquisition of trace CRs, or that awareness and trace conditioning develop concurrently" (p. 185).

It is worth noting that this finding of a positive association between awareness and eyeblink conditioning was obtained with a method of assessing awareness that overcomes many of the difficulties we identified with the original Clark and Squire (1998) study. First, the seven questions about the CS–US contingency were not preceded by a large number of unrelated questions. Second, these seven questions did not include any irrelevant questions concerning CS–CS relationships because there was only one CS. Third, the questionnaire was administered early in the conditioning session, thus reducing the problem of a ceiling effect on awareness.

In summary, the two single-cue conditioning articles we reviewed obtained opposite results concerning the role of awareness. Although Manns et al. (2000a) restricted their conclusions to the trace conditioning procedure, they did not test any participants under delay conditioning, so it is as yet unknown whether a similar tactic would yield sufficient differentiation in awareness in delay conditioning to observe a similar association. Similarly, Papka et al. (1997) did not test any participants under trace conditioning, so it is not known whether their null result would also be obtained with the trace procedure. In our view, it is more likely that the difference in outcome of these two studies is due to differences in levels of awareness and means of assessing awareness rather than to a fundamental difference between delay and trace conditioning. It remains possible that the primary difference between these procedures is that trace conditioning is a more difficult task.

Finally, mention should be made of a study that was published some time ago but that represents a striking example of dissociation between awareness and conditioned performance. In a simple but ingenious design, Perruchet (1985) used a single-cue delay eyeblink conditioning procedure with a 50% partial reinforcement schedule. During the intertrial interval, participants were asked to rate their expectancy of the airpuff US occurring on the next trial. The independent variable of interest was runs of trials of the same type, either reinforced or nonreinforced. Perruchet reasoned that participant's expectancy ratings might reveal evidence of the gambler's fallacy—after a run of nonreinforced trials, for example, participants might show a greater expectancy of the next trial being reinforced. By contrast, he observed that traditional "strength" theories of conditioning predict the opposite pattern for eyeblink CRs—after a run of nonreinforced trials, associative strength should be lower than at other times. The results conformed to Perruchet's predictions. Over runs of nonreinforced trials, expectancy ratings increased, whereas percent eyeblink CRs decreased, and over runs of reinforced trials, the opposite pattern was observed. This pattern of results has been replicated in an unpublished study by Bonic (1989).

The strength of Perruchet's (1985) design is that it does not depend on a null result on one measure but rather demonstrates a double dissociation in which an independent variable has an opposite effect on the two measures, CRs and expectancy ratings. Some limitations of the study should also be considered. First, the partial reinforcement schedule used by Perruchet was not random but favored alternation over continuation of runs, so the gambler's fallacy was in fact partially veridical. However, the point remains that CRs did not follow this pattern. Second, in the absence of any other basis for prediction, participants may have exaggerated small changes due to runs so as to make use of the scale provided for expectancy ratings, whereas their "true" expectancy may have remained close to 50%. Third, it is possible that the effect of runs on CRs may reflect a change in performance rather than in associative strength because of the recency of experience of the US as opposed to recency of reinforcement of the CS. For example, recent experience of the US may activate the US representation more strongly or sensitize the response system. Eyeblink CRs seem particularly sensitive to such performance factors, and it is worth noting that electrodermal CRs instead follow the gambler's fallacy pattern (Williams & Prokasy, 1977). Perruchet provided some evidence against the sensitization account in a second experiment in which an unpaired control group failed to show changes in eyeblink probability to the CS over runs of US presentations, but this control is less than optimal in that the participants experienced unpredicted USs and never experienced a CS–US association. Despite these limitations, however, Perruchet's study provides the strongest evidence to date for a dissociation between eyeblink conditioning and expectancy, and his design provides a strong model for future investigations.

Conditioning in Amnesia

A number of studies have examined conditioning and awareness in patient populations, particularly in the classic amnesic syndrome. If conditioning is assumed to be a "procedural" learning task and awareness is assumed to depend on "declarative" memory (Squire, 1994) and if in addition amnesia is a selective deficit in declarative memory, then it follows that amnesics should show normal conditioning together with impaired contingency awareness and should be more likely than controls to show conditioning without awareness. Furthermore, even if they do not show this pattern, there is still the possibility that a comparison between controls and amnesics might yield a decoupling of conditioning and awareness with the two populations being indistinguishable in terms of conditioning but different in the extent of their associated declarative knowledge.

Research has demonstrated fairly clearly that the rate of acquisition in delay eyeblink conditioning is normal or near-normal in temporal lobe amnesics (Clark & Squire, 1998; Gabrieli et al., 1995) but that fine-grained aspects of the CR, such as its timing, are not (McGlinchey-Berroth, Brawn, & Disterhoft, 1999). In contrast, trace conditioning is usually severely impaired, and indeed in some studies in which controls show reliable conditioning, amnesic patients have failed entirely to show any learning (Clark & Squire, 1998; LaBar & Disterhoft, 1998), although the well-known amnesic patient H. M. was found to be able to acquire conditioned responding in a trace preparation (Woodruff-Pak, 1993). Properties of eyeblink conditioning in amnesia are reviewed by McGlinchey-Berroth (2000).

What, then, are the results when amnesics are asked to report their awareness of the CS–US contingency? Daum and Ackermann (1994) described the case of a 63-year-old woman who suffered bilateral thalamic damage as a result of ischaemia with a consequent amnesia. She showed acquisition of eyeblink delay conditioning to a tone that preceded an airpuff on 70% of trials, although the increase in CRs across blocks as well as the terminal level of responding were considerably less than in a group of age-matched controls. Daum and Ackerman noted that although she stated "that tones came together with air, she also reported that airpuffs occurred after every single tone and that she did not have to blink to the tone" (pp. 160–161). The anecdotal nature of the awareness data, however, together with the uncertainty about whether acquisition was normal combine to make the claimed dissociation between conditioning and awareness far from compelling. Other reports by Daum and her colleagues describe normal delay eyeblink conditioning to a tone in patients with temporal lobe lesions (Daum, Channon, & Gray, 1992) combined with impaired conditional discrimination learning (Daum, Channon, & Canavan, 1989; Daum, Channon, Polkey, & Gray, 1991). All control participants in the Daum et al. (1991) study achieved high scores on a postexperimental awareness interview; patients had significantly lower scores than did controls, but there was no evidence of conditioning in the absence of awareness. Schugens and Daum (1999) again found normal levels of delay eyeblink conditioning in amnesic participants but also found that whereas all control participants reported the CS–US contingency, only 2 out of 5 amnesic participants appeared to have any awareness of the contingency.

Woodruff-Pak (1993) reported results from both delay and trace eyeblink conditioning experiments in H. M., another amnesic patient, and two control participants. All participants were able to acquire conditioned responding in both preparations, although there was some evidence that the rate of acquisition in the amnesic patients was abnormal: The amnesics required more trials than did the controls to reach criterion in both delay and trace conditioning. All the participants except H. M. displayed CS–US contingency awareness in a multiple-choice questionnaire following delay conditioning. H. M. not only performed poorly in this postconditioning test but also showed savings in reacquiring responding in a further pair of sessions of trace conditioning administered 2 years after the original training sessions had ended, despite claiming not to remember the previous training sessions, the experimenter, or the apparatus. As we have repeatedly pointed out, however, it is hard to justify attaching much weight to awareness measures taken postconditioning using a questionnaire. The questionnaire results do not rule out the possibility that, given a presentation of the CS, H. M. would have experienced some conscious expectancy of the US sufficient to explain his conditioned responding. It would be interesting in future research to know whether such patients can show long-term savings in reacquisition despite low levels of performance on a concurrent awareness test.

Clark and Squire (1998; see also Clark & Squire, 2000), in the study we have already considered in some detail, used both delay and trace eyeblink conditioning procedures and tested a group of 4 temporal lobe amnesics as well as the various groups of matched controls described previously. In trace conditioning, the amnesics were tested with an ISI of 1,000 ms in a differential conditioning procedure, with contingency awareness being assessed using a postconditioning questionnaire. The amnesics performed statistically no better than chance on the questionnaire ($M = 10.25/17$ correct responses) but also did not show differential conditioning.

In delay conditioning, the amnesics were tested with a 1,250 ms ISI and again they were reported as being unaware of the reinforcement contingency. However, in this case reliable differential conditioning occurred in the amnesic group. Our reanalysis confirmed that amnesia selectively impaired trace conditioning compared with delay conditioning, $F(1, 3) = 10.8$, $p < .05$.

The key result, then, is that reliable delay conditioning occurred in amnesic participants apparently unaware of the reinforcement contingencies. However, as with the data from normal participants, on close inspection the data are not quite so compelling. We have already mentioned the fact that the form of the awareness test was inappropriate in various ways and that an unjustifiably high cutoff (13/17 correct answers) was used to classify participants as aware. An additional problem with the interpretation of the delay conditioning data in the amnesic patients is that 2 of them scored 14/17 correct on the awareness test, actually above the specified cutoff. This was not acknowledged by Clark and Squire (1998) who claimed simply that "the amnesic patients, *none of whom became aware of the CS–US associations* [italics added], . . . acquired delay conditioning at a normal rate" (p. 78). R. E. Clark (personal communication, March 22, 2000) justified the classification of these participants as unaware on the grounds that by the time they took the awareness test, it was actually the third time they were asked these questions (they had previously been in the trace-1,000 condition and in another trace condition) and hence their scores cannot be considered equivalent to those of the controls. This is very likely the case, but it does not justify classifying these participants as unaware. If the assignment of stimuli to CS+ and CS− was counterbalanced in the earlier tasks they received, there is no reason why their scores should be greater than chance on the awareness test for questions specifically about the contingencies between CS+ and CS− and the US. Thus, we contend that the data of these 2 participants cannot be used to support the claim that unaware conditioning is possible in amnesia.

The remaining 2 amnesic participants scored 7 and 10, respectively, on the awareness questionnaire and hence only 1 participant can be said with confidence to have scored at or below chance (8.5/17 correct). This participant showed differential conditioning (19% more CRs to CS+ than to CS− on the final block), but even if we set aside the problems discussed previously concerning the type of awareness test Clark and Squire (1998) used, very little weight can be placed on a result that rests on a single participant.

Lastly, there is a particular problem with using postconditioning tests of awareness with amnesic patients because such patients are well-known to have significantly faster forgetting rates than nonmemory-impaired age-matched individuals, especially when tested using demanding recall tests (e.g., Isaac & Mayes, 1999). Thus, it would be predicted that amnesics would show lower scores than would the controls on the contingency awareness questionnaire, even if they matched the controls in their awareness during the conditioning phase itself. In sum, Clark and Squire's (1998) study provides only very marginal support for the claimed finding of delay eyeblink conditioning in amnesic participants who are unaware of the CS–US contingencies. Clark and Squire (1998) used an inappropriate cutoff for classifying participants as unaware, and their assessment of awareness is likely to have been biased against the amnesic participants.

Bechara et al. (1995) conducted an autonomic differential conditioning experiment with controls, a patient with selective bilateral hippocampal damage, a patient with selective damage to the amygdala bilaterally, and a patient with both amygdala and hippocampal damage bilaterally. The CS+ was a blue slide or a tone in different replications, and the US was a loud noise. The conditioning phase was followed by extinction and then a questionnaire test of contingency awareness. Bechara et al. found normal conditioning combined with contingency awareness in the controls, and neither conditioning nor awareness in the patient with both hippocampal and amygdala damage. The interesting pattern was in the remaining 2 patients. The hippocampal patient showed conditioning but not declarative knowledge, whereas the amygdala patient showed the opposite. Although Bechara et al. did not report any inferential statistics on their data, the results nevertheless suggest a decoupling of awareness from conditioning.

Because the measure of awareness was not on-line, however, it is perfectly possible that the results obtained in the hippocampal patient are simply due to rapid forgetting between the conditioning phase and the test of declarative knowledge. Because the conditioning phase was followed by an extinction phase prior to the awareness test, it is also possible that such a patient, with a profound memory impairment, would have had difficulty recalling the contingency presented in the conditioning phase in the face of interference from the extinction contingency. The results therefore do not challenge the claim that CS–US awareness, *at the time of conditioning*, is necessary (though not sufficient) for conditioning. In the hippocampal patient, knowledge of the CS–US contingency was simply forgotten, we suggest, prior to the awareness test. In the amygdala patient, some other circuit that is also necessary for conditioning (e.g., for attaching emotional valence to the CS) was presumably damaged. Note that there is nothing inconsistent between the claim that the amygdala patient was unable to learn about the emotional properties of the CS and the finding that this person had declarative knowledge of the CS–US relationship: One can know that a CS predicts a US without being fearful of it. Examples of normal contingency awareness in amygdala-damaged patients combined with impaired conditioned responding logically cannot challenge the claim that awareness is necessary but not sufficient for conditioned responding.

Evaluative Conditioning

Evaluative conditioning (EC), first studied by Levey and Martin (1975; Martin & Levey, 1978; see De Houwer, Thomas, & Baeyens, 2001, for an historical review) refers to a form of learning that manifests itself in changes in the affective or hedonic evaluations of stimuli consequent on a conditioning procedure. Specifically, it refers to the transfer of affect from a US to a CS. Although there has been some debate about the robustness of evaluative conditioning (Rozin, Wrzesniewski, & Byrnes, 1998), some authors (e.g., Baeyens, Eelen, & Van den Bergh, 1990; Martin & Levey, 1978) have suggested both that it is real and that it can proceed in the absence of awareness of the CS–US relationship. In this section we review some of the relevant evidence from studies using visual stimuli. In a later section we consider separately data from studies of evaluative conditioning of flavors.

In the most significant study of the role of awareness in evaluative conditioning, Baeyens, Eelen, and Van den Bergh (1990) presented participants with 10 repetitions of a CS–US pair in which the CS slide had previously been evaluated by the participant as affectively neutral and the US slide as either liked (L), neutral (N), or disliked (D). The stimuli were slides of faces. Evaluative conditioning was apparently observed in that on a postconditioning test of affect, the CS slides became affectively positive (liked) if they had been paired with a liked US, negative (disliked) if they had been paired with a disliked US, and remained neutral if they had been paired with another neutral stimulus.

As one test of awareness, at the end of the learning phase Baeyens, Eelen, and Van den Bergh (1990) showed participants each of the CS pictures and asked them to identify which US picture had been associated with it. If participants were unable to select a picture, they were then asked whether the US had been liked, neutral, or disliked. They were classed as aware of the CS–US relationship if they selected the correct US picture, if they selected an incorrect US picture of the correct valence, or if they indicated the correct valence on the second question. Evidence that EC occurred without awareness emerged in the observation that conditioning was statistically significant for CS–US pairs for which participants lacked contingency awareness. In fact, awareness seemed to make no difference to the magnitude of EC.

Of course, this postconditioning test of awareness may have been an insensitive one. Baeyens, Eelen, and Van den Bergh (1990) therefore also used a more sensitive concurrent measure of awareness. One group of participants was required to indicate during the 4-s interval between the onset of the CS and US slides whether they expected a liked, neutral, or disliked US stimulus on that trial. Participants were classified as unaware if they failed to respond correctly on the final three pairings of each stimulus combination, but this classification identified participants as only being unaware of 17% of all contingencies. Not only were participants aware of the majority of CS–US contingencies but moreover for those few they could not report, there was no significant EC. Overall, then, it is somewhat questionable whether EC without contingency awareness occurred in this study. In a subsequent study, Baeyens, Eelen, Crombez, and Van den Bergh (1992) found that as the magnitude of evaluative conditioning increased in groups of participants given increasing numbers of CS–US pairings, awareness indexed by a postconditioning test increased too.

By contrast, other studies from this research group have shown low correlations between awareness and conditioning and in some cases have even been dissociated them (see De Houwer, Baeyens, & Hendrickx, 1997). For example, low correlations have been obtained between the number of CS–US pairings a participant was aware of (assessed after the conditioning phase) and the magnitude of EC (Baeyens et al., 1992), and in another study CS–US similarity was found to have no effect on EC but did enhance contingency awareness (Baeyens, Eelen, Van den Bergh, & Crombez, 1989). As mentioned previously, however, correlational analyses represent perhaps the weakest tests of the claim that conditioning may occur in the absence of contingency awareness. The absence of any correlation between CS–US awareness and EC does not logically entail that there are CSs that show conditioning in the absence of awareness. Indeed, it does not even entail that there are any CSs whose relationship with the US is not consciously known. For example, all CS–US pairings might be consciously known, with some other factor that is necessary for conditioning varying across stimuli. Thus, correlational analyses must be supplemented by analyses showing that some CSs evoke CRs without contingency awareness.

Regardless of whether reliable EC occurred in the absence of awareness in these studies, it has become apparent that the design of the standard EC study is problematic in that because each participant chooses his or her own liked, disliked, and neutral stimuli, the experimenter relinquishes control over the assignment of stimuli to the roles of CS and US. This has led Shanks and Dickinson (1990) and Field and Davey (1999; Davey, 1994; Field, 2000b) to suggest that the EC effects reported by Baeyens, Eelen, and Van den Bergh (1990) are artifactual. A well-controlled within-subjects conditioning experiment requires that the pairing of a particular CS with a particular US is counterbalanced across participants to ensure that any differences in the postconditioning measure can be attributed to the association the CS enters into rather than to properties intrinsic to that particular CS. In the standard EC procedure, however, stimuli usually cannot be counterbalanced in their assignment to USs because different participants select different stimuli as CSs. Moreover, in many studies the experimenter chooses which N stimuli to pair with which L or D stimuli, usually on the basis of perceptual similarity.

The potential drawbacks of this aspect of much evaluative conditioning research cannot be overemphasized. Such allocation procedures open up the possibility that the EC effects are nonassociative. For example, a simple possibility is that the N stimuli the experimenter pairs with the liked USs are actually slightly more liked than are the N stimuli paired with the disliked USs. If affective ratings tend to differentiate with repeated presentation, as in the mere exposure effect (Bornstein, 1989), then increased liking of N stimuli in N–L pairs and decreased liking of N stimuli in N–D pairs would be observed, but not as a result of associative learning. That being the case, participants' inability to verbally report the CS–US contingencies would no longer represent evidence of unconscious associative learning.

A good deal of evidence can now be marshaled in support of this assessment of the standard EC procedure. For instance, Shanks and Dickinson (1990) replicated the basic EC effect but also included a control group in which the N–L and N–D pairings were never implemented: Instead, each stimulus was presented on 10 successive occasions, with the 10 presentations of an N slide being followed by the 10 presentations of its paired L or D slide. Despite this, EC effects were identical in the two groups. N stimuli paired with liked USs received higher ratings than ones "paired" with disliked USs, regardless of whether the participant actually saw the pairings or not.

An even more convincing piece of evidence comes from Field and Davey (1999). These authors used a better control group, noting that the unpaired control group used by Shanks and Dickinson (1990) did not eliminate entirely the possibility of participants learning the relevant CS–US associations across sequential blocks of 10 CSs and 10 USs (see also Davey, 1994). Instead, Field and Davey (1999) developed a block–subblock (BSB) control procedure in which the CSs and USs that are paired in the experimental group are presented in a more adequately random series in the control group. The stimuli were again faces. The key finding was that EC-like effects occurred in this BSB control group as well as in a further control group given the EC test phase without a prior training phase. However, these effects only occurred when the CSs and USs were perceptually similar. Field and Davey (1999) concluded that the EC-like effect occurs because participants tend to form an abstract representation during the initial rating and training phases of a liked face and a representation of a disliked face and tend on the final rating test to change their rating of each face in keeping with the category to which it is most similar. Neutral faces in N–L pairs tend to possess overlapping features with the abstract representation of a liked face and hence tend to be rated more in conformity with this representation, and similarly for disliked faces, without needing to be explicitly paired with the relevant US faces. Regardless of whether this theory is correct (see Field & Davey, 1997, for further evidence), the empirical observation of EC-like effects in unpaired control groups demonstrates that the standard EC effect is not an associative one. Hence, the fact that such effects may occur in the absence of CS–US contingency awareness cannot be taken as evidence for unconscious conditioning.

Some studies have avoided the method of pairing CSs and USs by similarity, either using random or counterbalanced CS–US pairings with visual stimuli. In some of these studies (e.g., Baeyens et al., 1993; De Houwer, Baeyens, Vansteenwegen, & Eelen, 2000; Hammerl & Grabitz, 1996, 2000), fairly clear EC has been observed. Note that our claim is not that all examples of EC are artifactual: Rather, we suggest that some instances are, and these have tended to be the ones that have looked specifically at contingency awareness. In studies that have shown genuine associative EC effects, in contrast, contingency awareness has usually not been the major focus. Studies that did avoid the perceptual–similarity pairing procedure and that also looked at contingency awareness were reported by Shimp, Stuart, and Engle (1991), Baeyens et al. (1993), and Fulcher and Cocks (1997). Shimp et al., for instance, reported 21 experiments in which the CS was a picture of a brand of cola and the US was a composite of four attractive water scenes (e.g., a waterfall). In each study the experimental group received 20 CS–US pairings, together with a number of filler trials, and a control group received the same number of stimulus presentations but in a random order. Clear EC was obtained across the experiments, but more interestingly, the effect depended on contingency awareness. Participants classified as aware using a postconditioning awareness test—which required picking out the brand that had been paired with attractive visual scenes—showed reliable conditioning, whereas those classified as unaware showed no greater conditioning than did participants in the random control group. In Baeyens et al.'s (1993) study the only evidence for conditioning without awareness was the lack of correlation between the magnitude of evaluative conditioning and the number of CS–US pairs recalled. On the awareness test participants overall performed well above chance. Like Shimp et al., Fulcher and Cocks (1997) found that participants classified as aware using a postconditioning recall test showed reliable conditioning, whereas those classified as unaware did not. As mentioned previously, a group that performed the recall test immediately after the study phase scored much higher than did the group that rerated the CSs prior to the recall test, illustrating that postconditioning awareness tests may seriously underestimate the degree of awareness during conditioning. Surprisingly, when the same participants were tested again 2 months later, reliable evaluative conditioning was observed even in participants who could not recall the CS–US pairings. The comparable effect was not observed in the immediate test, however, so until the delayed-test results are replicated it is difficult to assign them much weight.

Hammerl and her colleagues (e.g., Hammerl, Bloch, & Silverthorne, 1997; Hammerl & Grabitz, 1996, 2000) have demonstrated EC in a number of studies in which CSs and USs were randomly matched and in which postconditioning awareness tests yielded low scores. As noted by Field (2000a), however, the awareness tests used in the study by Hammerl and Grabitz (1996) did not challenge participants to recall either specific US slides paired with CS slides or to recall the affective nature of the US slide paired with each CS. Instead, the questions asked in more general terms about the training stage, such as whether there were regular pairings of slides or whether participants believed their ratings had changed during the experiment. Thus, the failure of a participant to give accurate responses on this test in no way proves that he or she was unaware of the stimulus contingencies.

In Hammerl et al.'s (1997) study, by contrast, an awareness test similar to that of Baeyens, Eelen, and Van den Bergh (1990) was used: Participants were shown each of the CS pictures and asked to identify which US picture had been associated with it. They were classed as aware of a CS–US relationship if they chose a US of the correct affective value. Evidence that EC occurred without awareness emerged in the observation that conditioning was statistically significant in participants who lacked awareness of any CS–US contingencies, and this study therefore obtained positive evidence of unconscious conditioning. Two potential issues are worthy of examination in future replications. First, the final awareness test was preceded by a separate awareness interview, and this creates a problem because it is known that such an interview adversely affects performance on a subsequent awareness test: Fulcher and Cocks (1997) found that recall of CS–US pairings was reduced by 25% over an interval as short as 3–4 min filled by an awareness interview. Secondly, as Baeyens, Eelen, and Van den Bergh (1990) showed, postconditioning tests of awareness classify more participants as unaware than a concurrent measure, thus raising the possibility of Type II errors in the classification of participant awareness.

A very interesting and novel form of evaluative conditioning was reported by Hammerl and Grabitz (2000), who examined responses to touched objects. Participants initially rated various objects (e.g., silk, sandpaper), which-could be touched but not seen, as liked, neutral, or disliked. Randomly assigned N–N or N–L pairs (four in total) were then presented in a conditioning phase during which the first stimulus was touched for 2 s, a 3-s interval followed, and then the second stimulus was also touched for 2 s. Hammerl and Grabitz (2000) obtained a reliable conditioning effect on the N–L compared with N–N trials and claimed that this effect was unconscious as participants performed very poorly in a postconditioning awareness test. In this test each of the four CS objects was touched, and the participant was asked to recall the haptic experience of the associated US object. If this proved impossible, the participant reported whether the contingent US was liked, neutral, or disliked and was classified as unaware if neither of these questions could be correctly answered. Hammerl and Grabitz (2000) reported in their first experiment that "all 10 participants were classified as unaware of all four contingencies" (pp. 350–351), but rather than demonstrate lack of awareness, this result seems to imply anomalous behavior by the participants. By chance alone, participants should have been correct on 33% of the awareness test trials merely by guessing between the valence categories liked, neutral, or disliked. The probability of scoring

0/40 correct (10 participants × 4 test trials) is less than one in a million. Thus, the participants must have known what the correct valence of the US was in order not to select it. What is mysterious is why they should have manifested this highly unusual behavior. Overall, pending an explanation of this statistically very unlikely outcome, Hammerl and Grabitz's (2000) results cannot be taken as demonstrating unconscious evaluative conditioning.

Particularly strong evidence against the claim that evaluative learning can be decoupled from awareness has been presented in the study by Purkis and Lipp (2001), described in the Autonomic Conditioning section, which used a masked conditioning procedure with electric shock as the US. Participants made pre- and postconditioning affective ratings of the critical pictures, and awareness was measured both concurrently using an expectancy dial and also using a postexperimental questionnaire, which as described previously were in perfect agreement. Purkis and Lipp's affective rating results were clear-cut: Aware participants rated the CS+ picture as less pleasant (and more exciting) post- relative to preconditioning, but there was no comparable change in unaware participants. From a design point of view, Purkis and Lipp's study probably gets closer than any other to fulfilling the ideal: Critical stimuli were counterbalanced, and a concurrent awareness measure was used. The absence of any hint of evaluative learning in unaware participants therefore represents a powerful challenge to the suggestion that such learning can occur outside awareness.

In summary, the claim that EC can occur for visual stimuli in the absence of contingency awareness is supported by only a small amount of evidence. Many apparent instances of EC are not true associative effects at all (Field & Davey, 1999), and those studies that have avoided this problem have typically yielded little evidence of EC in the absence of awareness. Finally, tests of awareness, have been rudimentary, have often been administered postconditioning rather than concurrently, and little effort has been made to validate them.

Evaluative Conditioning With Subliminal Stimuli

Several studies have asked whether evaluative conditioning can be obtained with subliminal CSs or USs. These studies potentially provide powerful evidence for conditioning without awareness because if the CS or US is not consciously perceived, its association with the other stimulus of the conditioning pair can hardly be conscious. On the other hand, demonstrating that a stimulus presentation is truly subliminal raises a whole set of problematic methodological and theoretical issues of its own, as described previously.

Niedenthal (1990) and Krosnick, Betz, Jussim, and Lynn (1992) reported positive effects with briefly presented USs, but these studies do not demonstrate that the EC-like effects they obtained were associative: For example, they used between-subject designs in which some participants were presented with positive USs and others with negative ones. Without appropriate unpaired control groups, this leaves open the possibility that the positive and negative USs induced different nonassociative changes in affect in the two groups.

De Houwer, Baeyens, and Eelen (1994) conducted a better designed study in which each participant received positive and negative conditioning trials with the CSs being affectively neutral words presented for 2,500 ms, and the USs being either affectively positive or negative words presented for about 29 ms followed by a mask. The allocation of CS and US words was counterbalanced, and there were two presentations of each neutral CS stimulus during training. EC was obtained in that postconditioning evaluative ratings of the CS words revealed US-contingent changes. However, a major drawback of this study was the omission of any objective test of awareness of the US words, such as a discrimination test. This means that although after the experiment participants did not report seeing the US words, they may have been represented fleetingly in consciousness during the conditioning trials.

Moreover, an exact replication of this experiment failed to find an EC effect (De Houwer, Hendrickx, & Baeyens, 1997, Experiment 1), and De Houwer, Hendrickx, et al. (1997) were unable to offer any obvious explanation for the discrepancy. On the other hand, in their second and third experiments, De Houwer, Hendrickx, et al. (1997) did again obtain EC effects in the expected direction. In Experiment 2, awareness was assessed using posttask debriefing as in the De Houwer et al. (1994) study. In Experiment 3, in which the number of presentations of each neutral CS was increased from two to four during training, a much better discrimination test was used in which participants saw CS and US words as in the conditioning phase but were instructed about the presence of the US words and asked to judge whether they were affectively positive or negative. Most participants performed at or close to chance on this test. When aware participants were excluded, some evidence of EC remained in that CS words paired with positive US words were rated as more positive than ones paired with negative US words, at least when participants was the random effect. When items was the random effect, the result was not significant and moreover the reliable effect across participants only occurred with one CS word list and not with the other. Finally, in their fourth experiment, De Houwer, Hendrickx, et al. (1997) again obtained a statistically nonsignificant EC result that was even reversed when each neutral CS was presented twice as opposed to four times. Overall, then, the results of these studies are so inconsistent that little confidence can be placed in the experiments that did yield significant effects.

Flavors and Odors: Evaluative Conditioning and Conditioning of Taste Properties

In addition to studies of evaluative conditioning with visual stimuli, some researchers have explored hedonic conditioning effects using flavors as stimuli. Other research has studied the conditioning of taste properties. Because of their similarity, we consider both cases in this section.

Baeyens, Eelen, Van den Bergh, and Crombez (1990) report a particularly striking dissociation between conditioning of flavors and contingency awareness. Participants in the flavor–positive (FP) group received 12 pairings of a flavor CS+ such as raspberry or apricot, with a positive US, sugar, whereas participants in the flavor–negative (FN) group received pairings of a flavor CS+ with a negative US, polysorbate 20, which has a very unpleasant taste. In fact, the design was a differential conditioning one in that both groups also received 12 further pairings of a different flavor (CS−) with water. At the same time, various color additives (e.g., orange, pink) were added to each solution in an entirely nonpredictive manner: Any given color occurred equally often with the positive (or negative) US and with water. In two further groups (color–positive, CP, and color–negative, CN) Baeyens, Eelen, Van den Bergh, and Crombez (1990) arranged for the colors to be predictive and the flavors to be nonpredictive. After the conditioning phase, participants rated their like or dislike for each flavor and color separately.

Baeyens, Eelen, Van den Bergh, and Crombez (1990) assumed that the predictive CS+ flavors would acquire valence as a result of their pairings with the USs and this turned out to be the case, although the effect was entirely located in the FN group: For this group, the flavor CS+ paired with polysorbate received much more negative ratings than did the CS− paired with water. In the FP group, no such differential conditioning occurred. Of more interest is the fact that no evaluative conditioning occurred to the colored CSs at all. Thus, evaluative conditioning of food likes and dislikes (especially the latter) was obtained for flavors but not colors.

In contrast, participants' explicit knowledge of the color–US contingencies was much better than their knowledge of the flavor–US contingencies. In a test of contingency awareness administered after the EC test, participants had to recall which color(s) and flavor(s) was most often paired with sugar or polysorbate. Participants in groups CP and CN performed this test quite accurately, whereas *none* of the participants in groups FP and FN identified the colors or flavors accurately. This striking double dissociation represents one of the most convincing pieces of evidence for conditioning being decoupled from awareness. Participants who showed EC performed poorly on the awareness test, whereas others who showed no EC performed well on the awareness test. The fact that participants in groups CP and CN did well on the awareness test demonstrates that this test was capable of eliciting some degree of explicit contingency knowledge.

Field and Davey (1997, 1998) have taken issue with Baeyens, Eelen, Van den Bergh, and Crombez's (1990) conclusion that unconscious conditioning mediated the EC effect they obtained. Field and Davey's main concern is with the fact that the only EC effect obtained in the experiment was for the FN group, for whom the flavor CS+ paired with polysorbate received significantly more negative ratings than did the CS− paired with water. In fact, relative to the flavor CS+ in the FP group, there was no change in ratings to the CS+ in the FN group: Instead, it was the CS− that appeared to undergo a differential change in the two groups, becoming much more positive (or at least, less negative) in the FN group. Thus, participants gave approximately equal ratings to a flavor CS+ regardless of whether it was paired with a positive or a negative US, which seems to raise doubts about whether the evaluative conditioning observed was truly associative as opposed to being based on some sort of contrast effect caused by the presence of a very unpleasant taste in the FN but not FP group (but see Baeyens, De Houwer, Vansteenwegen, & Eelen, 1998).

To our knowledge only one attempt has been made to replicate Baeyens, Eelen, Van den Bergh, and Crombez's (1990) study. Dickinson, Wheelwright, and Young (1994) modified the original procedure in one respect, presenting each participant with one CS paired with sugar and another paired with polysorbate, thus partially overcoming the problems inherent in Baeyens, Van den Bergh, and Crombez's between-subjects design. Again, for participants in the flavor group, the predictive CSs were flavors and the colors were nonpredictive, whereas for participants in the color group, the predictive CSs were colors and the flavors were nonpredictive. In contrast to Baeyens, Eelen, Van den Bergh, and Crombez's results, EC occurred for both flavors and colors and did not differ reliably in magnitude. Explicit knowledge of the color–US pairings was greater than for the flavor–US pairings, just as Baeyens, Eelen, Van den Bergh, and Crombez had found. Dickinson et al.'s study therefore does not point to the same conclusion: There was no double dissociation between EC and contingency awareness. The only findings suggestive of a dissociation were that (a) a flavor–color difference was observed in explicit knowledge but not in EC, and (b) participants' explicit knowledge was only marginally significantly greater than chance in the flavor group, despite the fact that they showed reliable flavor EC. These results are quite weak, of course: Apart from anything else, the assessment of explicit knowledge involved a recall test administered after the conditioning and evaluative rating phases and so may have left some aspects of explicit contingency knowledge undetected.

Despite these question marks over their results, Baeyens, Eelen, Van den Bergh, and Crombez's (1990) study provides one of the most intriguing pieces of evidence for conditioning without awareness. Efforts to replicate and explore the findings should be given high priority.

Stevenson, Prescott, and Boakes (1995) studied a similar phenomenon in which taste properties (sweet, sour) become conditioned to odors with which they are associated. Participants tasted one solution comprising an odor (e.g., lychee) paired with sucrose and another solution comprising a different odor paired with citric acid. In a subsequent rating phase, they judged the sweetness and sourness of each of the odors as well as making a hedonic evaluation. Stevenson et al. found that the odor paired with sucrose smelled more sweet and the one paired with citric acid smelled more sour as a result of the conditioning associations. (However, there were no changes in liking or disliking of the odors—thus the procedure does not seem to yield evaluative conditioning). In a final awareness test, participants smelled each odor and judged whether it had been paired in the conditioning phase with a sweet, sour, or some other taste. Overall, participants were rather good at this test, with 25/40 pairings being correctly identified in total (although it is not obvious what the chance level of responding would be). However, Stevenson et al. obtained some evidence of conditioning in participants who failed the explicit knowledge test: The odor paired with citric acid smelled more sour as a result of conditioning in 10 participants who could not identify the US (sweet–sour) previously paired with that odor. The comparable effect for the odor paired with sucrose in 5 unaware participants was in the right direction but not significant. Stevenson et al. also found that the magnitude of conditioning was unaffected by whether participants were classified as aware or unaware.

In a later report, Stevenson, Boakes, and Prescott (1998) replicated these findings using a simpler design in which one odor was paired with sucrose and another with water. Judged sweetness increased for the odor paired with sucrose relative to the control odor paired with water (again there was no change in evaluative ratings). A recall test identical to the one used in the Stevenson et al. (1995) study yielded very similar results, with participants overall recalling the pairings quite well but with measured awareness being uncorrelated with the magnitude of conditioning.

In a second experiment, Stevenson et al. (1998) presented participants with nondifferential conditioning trials in which one odor was paired with sucrose in a solution that was sipped through a straw (thus preventing the odor's detection prior to ingestion) and another odor was paired with sucrose in a solution that was tasted directly by mouth. As before, the perceived sweetness of these odors increased from pretest to posttest. In a new test of awareness, participants tasted several solutions, including the target ones, and judged how frequently each solution had been presented in the experiment. Although a frequency estimate is an unusual measure of awareness, it is not unreasonable to predict a relationship between such an estimate and the participant's awareness of the odor–sucrose contingency. This test yielded interesting results. In the group that tasted solutions using a straw, frequency estimates were significantly more accurate than chance, but in the group that directly tasted the solutions, the estimates were no better than chance. Thus, in the latter group conditioning of the taste properties associated with the odor occurred without any concomitant conscious familiarity of the odor–sucrose compound. In neither group did the magnitude of conditioning correlate with frequency estimates.

In conclusion, the studies by Stevenson and his colleagues (Stevenson et al., 1995, 1998) are beginning to form a persuasive case for the idea that the taste properties of odors can become conditioned without concurrent awareness. The frequency judgment test is an unusual one and it would be interesting to know whether such a test is sensitive to awareness of other stimulus properties (e.g., color) and whether it correlates with other awareness measures such as recognition and recall. Further studies in this area should establish more firmly whether the gustatory system possesses special learning properties that can operate independently of awareness or whether its processing is necessarily linked to awareness.

Conditioning and Anesthesia

A particularly straightforward way to establish unconscious conditioning is to pair a CS and US under general anesthesia. In the dual-process account one might speculate that anesthesia is an ideal manipulation for blocking awareness without affecting conditioned responding. In animals, conditioning under anesthesia has been observed in numerous studies (e.g., Bermudez-Rattoni, Forthman, Sanchez, Perez, & Garcia, 1988; Burešová & Bureš, 1977; Edeline & Neuenschwander-El Massioui, 1988; Ghoneim, Chen, El-Zahaby, & Block, 1994; Pang, Turndorf, & Quartermain, 1996; Rabin & Rabin, 1984). Studies with animals are relevant because whatever one thinks about consciousness in awake animals (Macphail, 1998), it can hardly be disputed that an adequately anesthetized animal is unconscious. Therefore, successful learning of a CS–US relationship under such circumstances provides fairly clear evidence of the possibility of unconscious conditioning. Despite some negative results (e.g., El-Zahaby, Ghoneim, Johnson, & Gormezano, 1994; Kandel, Chortkoff, Sonner, Laster, & Eger, 1996; see Ghoneim & Block, 1997, for a review), the weight of evidence clearly points to the possibility of learning under anesthesia in nonhuman animals.

There has, in contrast, only been a single report with humans, and this did not produce positive evidence of conditioning. Ghoneim, Block, and Fowles (1992) paired a word CS+ with a loud noise US in patients undergoing minor surgery under isoflurane–nitrous oxide anesthesia. No evidence was obtained of autonomic responses to the CS either during the conditioning phase or in a test administered about 3 hr later when patients had recovered from the anesthetic. However, nonanesthetized participants in a control group otherwise treated identically did show reliable conditioning. Hence the dual-process account receives no direct empirical support as yet from studies of conditioning in humans under anesthesia.

Successful conditioning under anesthesia is likely to depend on a range of factors, including the type and dose of anesthetic, the conditioning preparation, testing procedure, and so on (see Andrade, 1995; Ghoneim & Block, 1997). Although there is currently no positive evidence in humans, the animal data suggest that conditioning in unconscious, anesthetized humans may be possible and that further studies to evaluate this prediction should be undertaken. Evidence of conditioning in animals with spinal cord disconnection (see Grau, in press) also suggests that unaware conditioning may be observable in humans. If such conditioning is possible in humans, then it is hard to see how contingency awareness could be causally relevant: Surely the spinal cord cannot encode propositional relationships and any such knowledge encoded in the brain would not be able to influence the CR-generation process. Thus as with anesthesia, future research on conditioning in people with spinal disconnection may provide convincing evidence of its possible dissociation from awareness.

General Discussion

Of the different areas we have surveyed, some yield stronger evidence than others that conditioning may occur independently of contingency awareness. The autonomic conditioning literature as a whole shows a strong concordance between conditioning and contingency knowledge, although there are studies that are suggestive of conditioned responding without awareness under particular circumstances (e.g., extinction; Schell et al., 1991). Studies of the conditioning of taste properties are also suggestive of conditioning without awareness (Baeyens, Eelen, Van den Bergh, & Crombez, 1990; Stevenson et al., 1995, 1998). On the other hand, we found little evidence from studies of subliminal or eyeblink conditioning, conditioning in amnesic patients, or conditioning under anesthesia that is inconsistent with the claim that awareness is necessary for conditioning.

A striking feature of the majority of studies we have reviewed is their failure to use measures of awareness that meet the criteria for validity that we reviewed in the beginning of this article. In particular, researchers have often relied on postconditioning assessments of awareness, have interpolated extinction between training and awareness testing, and have used insensitive measures such as binary scales. There is good evidence that such methods systematically underestimate contingency knowledge (Dawson & Reardon, 1973), and it is far from clear why researchers continue to use them when their potential inadequacies have been so well documented and when alternatives are available. To summarize, the optimal method for assessing awareness is concurrent reporting of either US expectancy (during CS presentation) or CS–US contingency (between trials). If reactivity is a concern, a masking task can be used to restrict development of awareness. If postconditioning tests are to be used, their sensitivity can be maximized by administration immediately after learning, by the use of a recognition rather than a recall format and by the use of a continuous rating scale. Statistically, it is desirable both to compare the degree of conditioning in aware and unaware participants and also to test for the existence of conditioning in participants classified as unaware.

A second feature of the literature is that there is a high degree of variability in the degree of replicability of key results. Many of the most striking results have not been followed up, and in many cases genuine doubts can be raised about their replicability. In other cases the basic finding has been replicated but alternative explanations of the results need to be scrutinized. Thus, it would seem important for researchers to devote more effort to replicating and extending already published findings than is current practice. We already have numerous well thought-out designs and procedures for examining the role of awareness in conditioning, but often these methods are not comprehensively exploited. As examples of studies, we feel it would be valuable to pursue, we cite four: the eyeblink studies of Clark and Squire (1998) and Perruchet (1985), the flavor conditioning study of Baeyens, Eelen, Van den Bergh, and Crombez (1990), and the autonomic conditioning study of Schell et al. (1991).

How well do the three models illustrated in Figure 1 fare against the data? Prima facie, the results are more supportive of the single-process models. Not only were examples of conditioning in the absence of awareness relatively rare but they were often obtained with measures of awareness that may have underestimated conscious knowledge. Furthermore, there was positive evidence for a correspondence between CRs and awareness, especially in autonomic conditioning. Nonetheless, the correlations between CRs and awareness were not perfect in any of the domains we reviewed.

Recall that the strong single-process model (Figure 1a) has limited scope for predicting independence (e.g., low correlations) between CRs and awareness, yet many cases of such relative independence have been described. For example, although performance of eyeblink CRs appears to depend on contingency awareness, it also requires a timing process that takes longer to develop and does not seem to be represented in consciousness (see Prokasy, 1973). On balance, the strong single-process model appears the most straightforward one to reject given the available evidence. On the other hand, the weak single-process model (Figure 1b) is consistent with much of the data we have reviewed. It can accommodate a degree of independence between awareness and CRs because it allows for factors that influence the link between the propositional learning process and (reported) contingency awareness but not the link between the former and CR production or vice versa. This model also predicts that, provided such factors are carefully eliminated, no examples of conditioning in the absence of awareness should be obtained. Although this is plainly the most difficult issue on which to reach a definitive conclusion, we would argue that none of the data strongly challenges the view that conditioning is only observed when awareness is also present. The existence of examples of conditioning unaccompanied by awareness is the most straightforward prediction of the dual-process model (Figure 1c). Whether such cases exist must remain an open question, but there can be little doubt that it is much harder to obtain conditioning without awareness than the dual-process model predicts. Although it is possible that conditioning without awareness occurs reliably within relatively specialized systems (e.g., the gustatory system), the idea that unconscious conditioning is commonplace is clearly contradicted by our review.

Most of the research we have reviewed has addressed the role of awareness in the acquisition of conditioned responding. It is possible, however, that once CRs have been acquired they can be performed automatically, that is, in the absence of conscious awareness. The persistence of SCRs following expectancy extinction reported by Schell et al. (1991) and the observation of SCRs under conditions of backward masking by Öhman and Soares (1993) could be interpreted in this way. Recall, however, that subsequent research by Öhman and Soares (1998) showed a parallel pattern for expectancy ratings and SCRs after masked acquisition, suggesting that CR performance is not dissociated from conscious awareness. We have also argued that the results of Schell et al. (1991) are open to alternative interpretations. Thus, there is little evidence available at present to support the notion of CR production in the absence of US expectancy (see also Dawson & Schell, 1985).

A further possibility foreshadowed in the beginning of this article is that expectancy ratings themselves may reflect unconscious as well as conscious processes. Although it is difficult to see how an unconscious (e.g., excitatory link) conditioning mechanism of the sort proposed in dual-process models could give rise to contingency knowledge, such a mechanism could plausibly lead to activation of a US memory that is experienced subjectively as expectancy of the US. This account would predict congruence between CRs and US expectancy but a lack of congruence between these two measures and contingency knowledge. However, this is not the pattern shown in the data. Studies we reviewed that recorded all three measures (e.g., Marinkovic et al., 1989; Purkis & Lipp, 2001) typically showed congruence between the three measures, and no study showed a dissociation between contingency knowledge and US expectancy. This concordance strongly supports the idea that all three measures derive from a single, common learning mechanism.

Another body of evidence should be mentioned here despite its exclusion from our review. A number of studies have examined the effects of postconditioning verbal instructions on conditioned responding and have demonstrated that such instructions can have a very powerful effect on performance. For example, Grings, Schell, and Carey (1973) and McNally (1981) instructed participants that a previously reinforced CS+ would no longer be paired with the US but that a previously nonreinforced CS− would now be paired with the US and found an immediate and dramatic reversal of conditioned autonomic responding to the CS+ and CS−. The fact that CRs can be affected by verbal instruction provides additional support for single-process models rather than the dual-process model. Propositional knowledge in the form of verbal instructions seems to be able to interact with the chain of processes that lead to the CR, and such an interaction is not easily reconciled with the dual-process model of Figure 1c.

A related area in which the relationship between verbal reports, awareness, and learning has been a focus of study is research on so-called *implicit* learning. Numerous studies have explored the extent to which forms of learning other than Pavlovian conditioning are dissociable from or dependent on awareness and verbal report. Prominent examples include the learning of artificial grammars (Reber, 1967) and of sequences of spatial targets in reaction time experiments (Willingham, Nissen, & Bullemer, 1989). In these and many other situations, evidence has been presented that participants can learn about some contingency or regularity in the environment without being able verbally to report that contingency or regularity (for reviews, see Goschke, 1997; Shanks & St. John, 1994). The parallels between research with these more complex learning tasks and the work on conditioning reviewed in the present article are striking. For example, there has been considerable debate in the implicit learning literature about the adequacy of questionnaire techniques for eliciting retrospective conscious knowledge, about the problem of ensuring that the awareness test measures the same knowledge as the behavioral test (cf. our criticism of work on subliminal conditioning), and about whether patient populations demonstrate selective impairments in declarative knowledge (Kinder & Shanks, 2001; Shanks & St. John, 1994). Given the extensive overlap between the salient methodological issues that have arisen in these two fields, it is plainly worthwhile for researchers interested in the relationship between conditioning and awareness to pay careful attention to the implicit learning literature. This literature is particularly relevant as sophisticated new techniques have been developed that go some way toward addressing the major methodological problems (e.g., Destrebecqz & Cleeremans, 2001; Jacoby, 1991), and some of these techniques may prove to be applicable to conditioning preparations.

Conclusion

In 1989, Boakes wrote the following:

> 'Convincing evidence' can be taken to mean appropriately analysed and statistically significant data which have been obtained from a theoretically coherent experimental procedure employing suitable control conditions and which have not proved difficult to replicate, preferably by researchers holding different presuppositions. By such criteria [Brewer's] conclusion still stands that there is no convincing evidence for conditioning in human subjects without awareness of the contingencies. (p. 389)

In our view, this conclusion is not altered by research from the last decade. The ongoing difficulty in finding convincing evidence of conditioning without awareness in humans poses a significant challenge for the field of animal conditioning. The field has traditionally relied on the assumption that associative processes studied in animals have greater relevance to automatic, unconscious processes in humans than they do to propositional or conscious processes. Despite arguments that some forms of conditioning are represented declaratively (e.g., Dickinson, 1980) and an increasing interest in "animal cognition" (e.g., Pearce, 1997), most contemporary conditioning researchers continue to think of associative learning as a basic process that is quite divorced from higher order cognitive processes. The finding that conditioning in humans is closely tied to conscious awareness raises a fundamentally different perspective: that associative processes in animals represent the precursor to human propositional learning. From this perspective, animal conditioning may provide a crucial tool for understanding the fundamental characteristics of a representational learning system, the system that eventually allowed the emergence of an artificial symbolic code, namely language.

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