



## Evaluative conditioning is a qualitatively distinct form of classical conditioning: a reply to Davey (1994)

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*(Received 16 January 1995)*

**Summary**—Based on a critical review of the literature, Davey (1994) [*Behaviour Research and Therapy*, 32, 291–299] concludes that there is no sufficient evidence to support the theoretical position that evaluative conditioning is a qualitatively different form of classical conditioning. In the present manuscript, we will try to show that Davey's conclusion is biased by: (a) an overemphasis on what he believes to be problematic procedural aspects of previous evaluative conditioning studies; and (b) a selective reading of the available evidence. Finally, an attempt is made to characterize evaluative conditioning phenomena as the output of a Referential Learning System, which can be distinguished from an Expectancy Learning System.

### INTRODUCTION

In evaluative conditioning (EC) research, it has been shown that the contingent presentation of a neutral stimulus (CS) and an affectively relevant (liked or disliked) stimulus (US), results in a change in the evaluation of the originally neutral stimulus into the direction of the valence of the paired US (e.g. Baeyens, Eelen, Crombez & Van den Bergh, 1992a; Martin & Levy, 1978; Stuart, Shimp & Engle, 1987). Much of the interest in EC has arisen from the fact that it does not seem to fit into the current information-processing conceptualization of classical conditioning, which characterizes Pavlovian learning as an instance of signal-learning or expectancy-learning. Two findings are of critical importance: First, in order for evaluative shifts to occur, it does not seem necessary that a *S* is consciously aware of the CS–US contingencies. Second, once evaluative shifts have been established, repeated non-reinforced exposure of the CS does not alter its evaluation (no extinction of EC). These findings suggest that EC does not involve the controlled, signal-learning processes which seem to be critical in traditional Pavlovian preparations (e.g. Dawson & Shell, 1987), a fact from which it is derived that EC is a qualitatively different form of Pavlovian conditioning.

In a recent review, Davey (1994) tries to show that the current evidence does not warrant this conclusion. He does so by pointing out certain procedural differences between EC studies and traditional classical conditioning studies which might be responsible for the different results obtained in these two kinds of studies. If mere procedural aspects are assumed to be crucial, there is no reason to ascribe a special status to EC at a process level. In his article, Davey focuses on the issues of contingency awareness and extinction. In both cases, he views the lack of a random control condition in most EC studies as problematic. In the present paper, we will therefore first consider whether a random control condition is really as crucial as Davey would like us to believe. Next, Davey's arguments concerning contingency awareness will be re-examined, and finally we will discuss the evidence on extinction.

### IS A RANDOM CONTROL GROUP NECESSARY?

Throughout his article, Davey (1994) emphasises that in traditional human autonomic conditioning, responding to a CS paired with a US has to be different from responding to a CS which is randomly paired with a US in order to conclude that conditioning has occurred. However, in (most) EC studies a random control condition was not used. Davey argues that therefore it has not yet been firmly established that the evaluative shifts observed in EC studies are actually Pavlovian associative learning effects. He continues that, because it is not necessarily conditioning that is responsible for the effects observed in EC studies, it should come as no surprise that evaluative shifts can be observed in the absence of contingency awareness (because it is not the contingencies which are crucial), nor that extinction does not occur (because no associations are learned).

Even though it is certainly true that a comparison with a properly designed random control condition provides a methodologically sound way to establish the associative nature of an observed CR in a Pavlovian paradigm, it is definitely not the case that this represents the *only* effective method. We are convinced that the within-*S* control procedures used in most (but not all) EC studies also allow us to conclude in a definite manner that associative processes are responsible for the observed effects. Let us consider the procedure used in several studies conducted at our laboratory. Each *S* first evaluated a set of pictures using a –100 (very disliked) to +100 (very liked) scale. Afterwards, pictures which were rated neutral (near-0) were selected as CSs. CSs could be assigned to either a liked (N–L), disliked (N–D), or neutral (N–N) US. After

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repeated contingent presentation of the pairs, *Ss* were asked again to evaluate the CSs and USs. In describing this procedure, Shanks and Dickinson (1990, pp. 21–22) correctly argue that

“At first sight, this would appear to be a paradigmatic example of a within-subject conditioning experiment, a *design that is generally agreed to represent a well-conducted procedure for demonstrating associative learning*. It equates exposure to all classes of stimuli, thus controlling for non-associative effects, while varying the associations that the CSs enter into” (italics added).

Hence, contrary to Davey (1994), and in line with Shanks and Dickinson, we argue that a random control group is not always necessary in order to conclude that associative learning has taken place. A well-designed differential within-*S* conditioning procedure is also adequate. However, Shanks and Dickinson continued by questioning whether in some EC studies the within-*S* conditioning procedure was well conducted.

“However, such a design assumes that the pairing of a particular CS with a particular US is counterbalanced across *Ss*, so that any difference in the postconditioning measure can be attributed to the association a CS enters into rather than to properties of that particular CS. The problem with the Martin and Levey procedure is that it cannot fulfil this counterbalancing criterion, because each *S* selects CSs and USs. Given this constraint, CS effects may be controlled by using ‘blind’, random assignments of the selected CSs to the selected USs. But once again, this control is not implemented with the standard EC procedure because the experimenter makes an explicit assignment on the basis of content, form and colour.”

Therefore, the truly problematic procedural aspect of some of the early EC studies does not lie in the absence of a random control condition as such, but in the stimulus selection procedure and more particularly in the non-random assignment of CSs to USs. As suggested by Shanks and Dickinson, the requirements of a properly-designed within-*S* control can be fulfilled either by *counterbalancing* the assignment of CSs to USs over *Ss*, or by using a *random* assignment of CSs and USs\* for each *S*. Both procedures have been used in several EC studies. For instance, in all EC studies with gustatory CSs, counterbalancing of CS–US pairs is standardly used (e.g. Baeyens, Eelen, Van den Bergh & Crombez, 1990; Baeyens, Hendrickx, Crombez & Eelen, 1995). In the more recent studies using visual materials (e.g. pictures of faces), random assignment of CS–US pairs has become the norm (e.g. Baeyens *et al.*, 1992a; Baeyens, Eelen, Van den Bergh & Crombez, 1989b; Baeyens, Eelen, Van den Bergh & Crombez, 1992b; Baeyens, Hermans and Eelen, 1993). In all of these studies, substantial evaluative shifts were observed. By randomizing or counterbalancing the CS–US pairs, the procedures used in these studies perfectly fitted the definition of a “well conducted within-subjects conditioning procedure”. Therefore, they provide sound evidence that the observed evaluative shifts are associative in nature.

However, the fact remains that in several of the earlier studies on EC, including the studies on extinction and counter-conditioning (Baeyens, Crombez, Van den Bergh & Eelen, 1988; Baeyens, Eelen, Van den Bergh & Crombez, 1989a), CS–US assignment indeed was not randomized or counterbalanced over *Ss*. Does this mean that all conclusions reached by these studies become invalid? This would be the case if it could be shown that, unlike in the more recent EC studies just mentioned, the stimulus assignment procedure used in these studies was responsible for the observed effects. Shanks and Dickinson (1990) argued that the stimulus assignment procedure could have interacted with the effects of stimulus exposure during the conditioning phase. For instance, the evaluation of stimuli selected to be paired with liked USs (CSs+) might be differently affected by repeated presentation than the evaluation of stimuli selected to be paired with neutral (CSs0) or disliked pictures (CSs–).

The assignment procedure used in the earlier studies was inspired by Levey and Martin (1987), who observed that CS–US perceptual similarity enhanced (but was not necessary condition for) EC. It is important to notice that the stimulus assignment procedure used in the earlier studies only differed from a random assignment procedure in that the experimenter tried to match the CSs and USs on perceptual similarity. Therefore, the only difference that reasonably might be expected to exist between randomly constructed and matched CS–US pairs lies in the level of perceptual similarity between the CS and the US of each pair. If, however, the assignment procedure was not successful, that is, if matched CS–US pairs were as a matter of fact as (dis)similar as randomly constructed CS–US pairs, there would be no compelling reason why one should not regard the matched pairs as equivalents of randomly constructed pairs, in which case a non-associative explanation could be ruled out. It is therefore important to first examine whether or not the stimulus assignment procedure was successful. If matching was successful, it then needs to be shown whether or not the increased similarity between the CS–US pairs created by matching indeed influenced the results.

We first examined whether or not the matching procedure resulted in CS–US pairs becoming more similar, compared to a random assignment procedure. Five independent judges were asked to rate the perceptual similarity of each of the 144 CS–US pairs used in a condition of the extinction-counter-conditioning experiment (Baeyens *et al.*, 1989a). This was one of the early experiments in which the experimenter tried to construct the pairs according to perceptual similarity. The judges were also asked to rate the perceptual similarity of an equal number of randomly created pairs of the same pictures. The judges indicated their impression of overall perceptual similarity between the two members of a pair (similarity in form, colour, composition of the two stimuli) using a –10 (very dissimilar) to +10 (very similar) scale. The actually presented pairs (matched according to the experimenter’s impression of similarity) were indeed judged to be more similar than the randomly composed pairs (means were, +2.08 and –0.68, respectively,  $P < 0.001$ ). We can therefore conclude that the similarity matching procedure indeed was successful. However, only moderate levels of similarity were created by the matching procedure.

Second, it was investigated whether the similarity which was created by matching was actually responsible for the observed effects. First, for each *S* separately, a Pearson correlation was obtained between the average similarity rating of each stimulus pair and the observed evaluative shift for that particular pair (for ND pairs, the inverse evaluative shift rating was used, in order to be able to test for the presence of a general positive correlation between similarity and evaluative shift). If matching was responsible for the observed evaluative shifts, the largest shifts would be expected on CSs for which matching

\*Some might conjecture that randomization does not eliminate the possibility that by mere chance, systematic differences might arise between different conditions. Although this is logically correct, randomization does minimize this possibility. Also, such a critical conception would undermine most experimental psychological research in which randomization of stimuli and *Ss* is commonly used.

was successful. Thus a significant positive correlation was expected. Individual correlations varied between +0.92 and -0.71. For exactly half of the *Ss* a positive correlation was obtained, whereas for the other half a negative correlation was obtained. The average correlation was +0.18, which is not significantly above chance. We also statistically controlled for the effects of CS-US similarity by using the average similarity rating of each pair as a covariate. The effect of regression was not significant [ $F(1,79) = 2.59$ ;  $P < 0.12$ ], whereas the conditioning effect was still highly significant [ $F(1,79) = 11.55$ ;  $P < 0.001$ ]. It can thus be concluded that statistically controlling for the effects of the stimulus assignment procedure does not eliminate the effect at all.\* If the stimulus assignment procedure would have been responsible for the observed evaluative shift, one would have expected a more drastic influence of the effects of this assignment procedure (i.e. the obtained CS-US similarity). In other words, the problem in the earlier EC was the assignment procedure. If, however, we control for the results of this assignment procedure on a *post hoc* basis, large evaluative shifts were still observed. How then can these shifts be explained in terms of an assignment procedure?

As a final argument, we would like to point out that in the extinction-counterconditioning study, extinction did not affect evaluative ratings whereas counterconditioning did. It would really require a lot of creativity in order to explain this pattern of results in terms of "an interaction between the CS-US assignment procedure and the effects of stimulus exposure" (Shanks & Dickinson, 1990, p. 22) or any other kind of non-associative process. Based on the fact that a *post hoc* explanation of the results in terms of the stimulus assignment procedure receives little support from *post hoc* analyses, and based on the implausibility of the alternative explanation, we believe that the results obtained in earlier EC studies also do reflect true conditioning, and the conclusions based on these studies do inform us about evaluative conditioning. Finally, it should be noted that the minor influence of the stimulus assignment procedure on the observed evaluative shifts should not come as a surprise, given the fact that in studies using a random assignment or counterbalancing, large and significant shifts were observed. Also, Baeyens *et al.* (1989b) experimentally demonstrated that the degree of perceptual similarity between the CS and the US of a pair—the only thing that is being manipulated in the stimulus assignment procedure—does not influence the size of the evaluative shifts.

#### IS A RANDOM CONTROL GROUP A GOOD CONTROL FOR EC?

Suppose one uses a standard EC paradigm in which 3 N-L pairs, 3 N-D pairs, and 3 N-N pairs are presented. A quite literal translation of the random control condition in tone-shock conditioning studies to the EC situation would be that in a first block the two stimuli (CS and US) of a first N-L 'pair' are distributed randomly and independently of each other into the time window. Next the two stimuli of an N-D 'pair' are presented randomly, and so on for the remaining stimulus 'pairs'. We believe that this is not the appropriate random control situation for EC. This opinion is directly related to our conception of the EC phenomenon as being the reflection of a merely Referential Learning System, *not* of an Expectancy Learning System (Baeyens, Eelen & Crombez, 1995) (see also below). Namely, the responses measured in a typical tone-shock paradigm reflect acquired *genuine expectancies of real US occurrence*. In this situation, in the random-control group the CS logically cannot lead to any specific, time-locked expectancy of US occurrence-immediately-following CS presentation, and hence should not lead to expectancy-learning. As such, a random-control group is an appropriate control for this type of learning. If, however, the hypothesis is correct that evaluative conditioning does *not* reflect the acquisition of CS-generated US expectancies, but implies the acquisition of a merely *referential* CS-US relation, then the random presentation of CS and US in one specific time window might equally well lead to a kind of referential CS-US relational knowledge, and consequently, to associatively-based valence shifts. From a related point of view, we have demonstrated in one of our studies (Baeyens *et al.*, 1993) that CS-US contiguity and not CS-US contingency seems to be crucial in EC. The above described random control situation does create a kind of CS-US contiguity and thus leaves plenty of room for the acquisition of a referential association being established between the CS and the US. Therefore, a better random control procedure for EC might involve a completely randomized presentation of all stimuli (CSs and USs) involved in the study. In this situation, there would be no systematic contiguity between any particular CS and US whatsoever. It is worth noting that the latter random control procedure was used by Stuart *et al.* (1987) and Shimp, Stuart and Engle (1991). They found significantly larger evaluative shifts in a paired condition than in this type of random control condition. This difference could be demonstrated using different numbers of acquisition trials, using a forward or a backward pairing schedule, and using different CS-US pairing schedules.

#### EVALUATIVE CONDITIONING AND CONTINGENCY AWARENESS

In his article, Davey (1994) first discusses three criteria which have to be met in order to demonstrate that EC does not require contingency awareness. He then admits that these criteria have been met in a study reported by Baeyens, Eelen and Van den Bergh (1990). In that study: (i) significant evaluative shifts were observed in *Ss* who were totally unaware of the relevant contingencies; (ii) the size of this effect did not differ from the effects obtained in *Ss* who were aware of at least some of the contingencies; and (iii) effects were the same for CSs for which *Ss* claimed to be aware of the paired US than for CSs for which *Ss* claimed not to be aware of the contingency. Davey, however, quickly dismisses these findings based on the fact that no random control group was included in the Baeyens *et al.* (1990) study. He reasons that because no random control condition was included, the observed effects might have been due to non-associative processes, and because the learning of CS-US contingencies is not critical for the effects it is not surprising that awareness of the contingencies is also not crucial.†

\*A similar exercise on the data obtained in a study in which we did not attempt to match CSs and USs according to the perceptual similarity criterion revealed the following. The average perceptual similarity rating of the 120 stimulus pairs (20 *Ss* × 6 pairs) was +0.152 (min = -10; max = +10), which did not significantly differ from 0. In this case, an analysis of covariance on the ER difference ratings obtained for the three ND and three NL pairs presented to each *S*, with the average similarity rating of each stimulus pair as the covariate, did not even confirm the borderline significant effect of the regression of Similarity on evaluative shifts,  $F(1,79) = 0.38$ ; *NS*, while again the effect of Conditioning remained clearly significant,  $F(1,79) = 9.81$ ;  $P < 0.002$ .

†Davey tries to demonstrate the validity of his reasoning by referring to a study conducted by Shanks and Dickinson (1990). As Davey willingly admits later on, Shanks and Dickinson's study suffers from serious flaws. Nevertheless, Davey gives much weight to their results.

However, in the previous sections we have shown that a random control group is not required in order to exclude non-associative explanations. Hence, Baeyens *et al.*'s (1990) results are valid and as such strongly support the view that contingency awareness is not necessary in order to obtain evaluative *conditioning* effects. Moreover, Davey also dismisses other relevant evidence. For instance, a number of striking dissociations between contingency awareness and EC have been observed. In one study, we (Baeyens *et al.*, 1989b) found that perceptual CS-US similarity enhanced contingency awareness but not evaluative shifts. In another study (Baeyens *et al.*, 1992a), contingency awareness increased when proceeding for 10 to 20 acquisition trials whereas evaluative shifts actually decreased. An even more striking dissociation was observed in a flavour conditioning study (Baeyens *et al.*, 1990) in which we observed that when a flavour-CS was correlated with a highly aversive US-flavour, none of the Ss demonstrated contingency awareness, but they did show a clear conditioning effect. When a colour-CS was used, however, a substantial number of Ss were contingency-aware, but no conditioning was observed. These findings suggest that evaluative learning and awareness do not always proceed in tandem but are related in an orthogonal manner. Finally, a number of studies (e.g. De Houwer, Baeyens & Eelen, 1994; Krosnick, Betz, Jussim & Lynn, 1992) have found significant evaluative shifts, even if the USs (but not the CSs) were presented subliminally.\* The importance of these studies lies in the fact that because Ss are not even aware of the US itself, it is extremely unlikely that Ss would be able to consciously notice the relevant CS-US contingencies.

After dismissing the evidence for unaware EC, Davey (1994) reverses the angle and proceeds by pointing out that there is no theoretical reason why EC could not be found in the absence of contingency awareness, because "currently accepted models of human classical conditioning do not have a theoretical commitment to conditioning only with awareness" (p. 294). More specifically, "there is no theoretical reason why CS-US expectancies cannot be generated at a sub-verbal level". We believe that Davey confuses two different issues. The first issue is whether or not CS-US contingencies can be *learned* without controlled processing reflected by conscious awareness of the CS-US contingency. The second issue is whether—*once the contingencies have been (consciously) detected*—the acquired CS-US contingencies can influence reaction towards the CS without Ss being aware of it. We do agree that consciously acquired CS-US contingencies can have unconscious effects on behaviour. Öhman, Dimberg, and Esteves (1989), for instance, report several studies in which a CS is presented contingently with a US in a standard classical conditioning situation. During this first phase, Ss can consciously detect the contingency. Afterwards, the CS is presented subliminally but still autonomic responses are affected. This demonstrates an unconscious influence of the previously learned contingency. However, this is not the critical issue. The critical issue is whether or not contingencies can be *learned* in the absence of contingency awareness. As Davey mentions in the beginning of his article, at present the evidence (in expectancy-learning paradigms) quite strongly supports the view that conditioning effects only occur after Ss, at some point in time, have become aware of the relevant contingencies. That is, if Ss were at no time during learning aware of the relevant contingencies, conditioning effects can not be observed under any circumstances. We do not know any "currently accepted model of human classical conditioning" that allows for learning to occur in the absence of contingency awareness. For instance, Davey refers to Öhman *et al.* (1989, p. 188) in this context. However, Öhman *et al.* explicitly state that finding classical conditioning effects in the absence of contingency awareness would "clearly contradict established theoretical notions suggesting that controlled processing is a prerequisite for learning. Given the pivotal nature of this notion in the psychology of learning, its falsification would have far-reaching consequences. Our data so far hardly justify seeking a theoretical alternative."

The important issue is that in EC, contingency awareness does not seem to be necessary in order to obtain evaluative shifts: The orthogonality of evaluative learning and contingency awareness and the existence of EC effects even when the USs are presented subliminally (which virtually excludes the possibility of Ss ever becoming aware of the contingencies) indicates that evaluative shifts can be observed even if Ss never have become aware of the relevant contingencies (Baeyens, De Houwer & Eelen, 1994). As such we believe that EC is a *qualitatively* different form of Pavlovian learning.

Finally, Davey attributes the overall low levels of contingency awareness in EC studies to the fact that "unlike traditional conditioning paradigms which have one US and—at best—two CSs, the EC procedure can contain as many as nine different USs and nine different CSs. . . . If conscious awareness of contingencies in autonomic conditioning procedures is a function of the complexity of the covariation assessment problem, then EC is not simply a qualitatively different kind of conditioning" (Davey, 1994, p. 295). Again Davey confuses two issues, namely the overall level of awareness and the *relationship* between contingency awareness and conditioning effects. We do not want to argue that EC always or necessarily involves low levels of awareness; the overall level of contingency awareness is irrelevant to the issue of whether EC is a qualitatively different form of conditioning or not. We do argue that the acquisition of valence and the acquisition of contingency awareness are unrelated. Moreover, there is no reason to believe that decreasing overall contingency awareness in traditional expectancy-learning studies would result in finding evidence for unconscious autonomous conditioning. Namely, if the hypothesis is true that there is no such thing as an "unconscious genuine US-expectancy", it would logically follow from this that conditioning without awareness would never be evidenced as long as responses are measured which reflect just this active expectancy of real US-occurrence. Finally, it is also worth pointing out that in several flavour conditioning studies conducted at our laboratory (e.g. Baeyens *et al.*, 1990; Baeyens *et al.*, 1995) only 2 CSs and 1 US were used, a situation comparable in complexity to standard autonomic conditioning procedures. Nevertheless, the same dissociation between level of contingency-awareness and level of evaluative learning was the rule in all of these studies.

#### RESISTANCE TO EXTINCTION

Once again, Davey (1994) uses the lack of a random control group as an argument to invalidate the conclusions of relevant EC studies. As we demonstrated earlier on, such an argument has no solid grounds. It is remarkable though that Davey keeps referring to the study of Shanks and Dickinson (1990), despite the obvious flaws of this study.

Davey also addresses other procedural aspects of the EC studies which have demonstrated resistance to extinction (Baeyens *et al.*, 1988; Baeyens *et al.*, 1989a). First he argues that demand characteristic might be responsible for the observed lack of extinction. With regard to the Baeyens *et al.* (1988) study, we agree that the fact that CS ratings were obtained both after acquisition and after extinction represents a weakness in the design in that it makes the data—at least in principle—susceptible to demand characteristics. However, we do not believe that the lack of extinction was related to

\*For some obscure reason, Davey seems to regard these studies as evidence against a qualitative difference between expectancy-based Pavlovian learning and EC.

demand characteristics *de facto*. First, we do not agree that the nature of our cover story promoted stability of the valence ratings as being implicitly desirable. Namely, it was at no time suggested, as Davey argues, that "the stimulus ratings after extinction were needed in order to unambiguously interpret the ER taken at the end of conditioning" (Davey, 1994, p. 296). What the cover story did include was the statement that the so-called second SCR measurement phase (actually the extinction trials) was needed in order to be able to interpret the results from the first and crucial SCR measurement phase unambiguously. Hence, reference was made to the so-called physiological measurement, *not* to the valence ratings. Also, each ER measurement was introduced with the clear and explicit statement that we were only interested in Ss' valence ratings *at the very moment of measurement*. Given these elements, we can see no reason why the cover story itself should have induced a demand for stability of the ERs. We would also like to stress that after a delay of 2 months, the pattern of results was the same as immediately after extinction. It is, however, very unlikely that 2 months after conditioning, Ss would be able to remember their rating profile. Even if Ss could remember some ratings, why would they bother trying to remember them, given the fact that it was stressed that we were interested in how they evaluated the stimuli at that very moment.

Because the procedure used in the Baeyens *et al.* (1988) study was not optimal, a different strategy was used by Baeyens *et al.* (1989a). In this study only one ER measurement was obtained, namely after extinction and not after acquisition, but again no extinction was found. Counterconditioning, however, did have an effect. Nevertheless, Davey still is not convinced by these results. He argues that Ss might have been making *implicit* ERs throughout the experiment and might have remembered these implicit ERs when making ratings after extinction. Ss experiencing counterconditioning trials would not have used these implicit ERs, because they knew that during counterconditioning something "significantly different" was happening. However, this hypothesis seems very implausible. First, why would Ss try to appear consistent with an ER which was never explicitly expressed? Second, Davey's account of evaluative changes in terms of awareness that something significantly different was happening is problematic for several reasons: (i) Why should the counterconditioning treatment and not the extinction treatment be perceived as a "significantly different event"?; (ii) How to explain that in Ss aware of all extinction presentations, and hence aware that something significantly different was happening, no extinction was observed? Davey (p. 296) suggests that it is possible that some extinction had occurred in these Ss, but we want to make clear that these Ss showed no extinction whatsoever; (iii) How would Davey explain that *both* in Ss aware (47%) and in Ss unaware (53%) of the counterconditioning treatment, significant counterconditioning was observed? Finally, it should be stressed that once again Davey seems to have misunderstood our cover story. At no time was it mentioned that the second ER measurement would be used to interpret previously obtained ERs. On the contrary, it was insisted upon that we were only interested in Ss actual evaluations of the stimuli.

Besides the issue of demand characteristics, Davey also discusses two factors peculiar to the EC paradigm which might favour resistance to extinction. The first is the duration of the CSs during extinction trials. Compared to standard autonomic conditioning studies, in Baeyens *et al.*'s (1988, 1989a) studies, the CSs were only presented briefly (1 sec) during extinction. In Davey's opinion, this might be important because in some human Pavlovian conditioning preparations there is some evidence that it is harder to obtain extinction with brief CS exposures. However, the evidence for this is only very limited. Four of the five papers to which Davey refers in this context (Kaloupek, 1983; Miller & Levis, 1971; Stone & Borkovec, 1975; Sue, 1975) deal with exposure to phobic stimuli in clinical patients. Only the study of Sandin and Chorot (1989) concerns extinction with CSs that have been conditioned within a controlled experiment. However, Sandin and Chorot's data are not straightforward in that they only found an effect of CS duration on extinction in Ss conditioned with a strong US. When a weak US was used, CS presentation time did not influence the degree of extinction. Because the USs used in EC studies are typically very weak, Sandin and Chorot's data suggest that the lack of extinction in EC studies can not be attributed to the brief presentation time of the CSs during the extinction phase.

The second procedural aspect that Davey refers to concerns our use of a trace conditioning paradigm. In our studies, the US followed the CS after an interval of 3 sec (not 4 sec as Davey incorrectly reports). Therefore the immediate post-CS period is identical during acquisition and extinction. This might prevent extinction from occurring. We can not dismiss this argument on empirical grounds. There are, however, some data which suggest that this procedural aspect is not important. In a recently conducted flavour conditioning experiment (Baeyens, Vanhouche & Eelen, in preparation), which involved quasi-simultaneous presentations of CS and US, a complete absence of extinction was also observed. One could also consider a study by Elizalde and Sclafani (1990) on animal conditioned flavour preferences, where resistance to extinction was observed in a situation involving both long CS duration and a quasi-simultaneous conditioning paradigm.

#### EVALUATIVE CONDITIONING AND ASSOCIATIVE LEARNING

In a final part of his article, Davey (1994) argues that because US revaluation effects can be observed both in EC and in standard human classical conditioning paradigms, it can be assumed that the underlying associative structure is the same in both cases. We applaud the fact that Davey considers our study (Baeyens *et al.*, 1992b) as valid, despite the apparent lack of a random control condition in this study. As we argued before, such a control condition is indeed not necessary. It is, however, hard for us to understand why Davey questions the associative nature of EC when discussing EC studies on awareness and extinction, while he does accept that Baeyens *et al.*'s (1992b) study "clearly and elegantly demonstrates that EC is mediated by CS-US associations" (Davey, 1994, p. 297). If this is accepted—and we have tried to demonstrate throughout the present article that it should be accepted—much of Davey's critical arguments no longer apply. More importantly, however, we do not agree that the revaluation study suggests that the processes involved in EC and expectancy-learning paradigms are the same. It does indeed suggest that in both cases CS-US associations are involved. But this does not yet explain the two major functional characteristics of EC: resistance to extinction and orthogonality to explicit knowledge acquisition. One possible way to theoretically integrate these properties of EC, and to distinguish EC from expectancy-learning, was proposed by Baeyens *et al.* (1992b), and recently updated in Baeyens, Eelen and Crombez (1995). As we explained in the latter article, this theoretical proposal involves a differentiation between two qualitatively different functional systems, the Expectancy-System (ES) and the Referential System (RS), and, correspondingly, two qualitatively different types of CS-US relations, Expectancy relations and Merely Referential CS-US Relations (Baeyens *et al.*, 1995). At a phenomenological level, some CS-US associations can be qualified as genuine *expectancy* relations. A CS may acquire the capacity to generate the expectation of a US-really-going-to-occur-in-the-immediate-future (see also Dawson & Shell, 1987); hence, the CS becomes a true signal for US occurrence. On the other hand, it is suggested that a CS-US relation may also be conceived of as involving nothing more than the capacity of a CS to activate—consciously

or unconsciously—the US representation, without additionally generating the expectation that the US is really going-to-occur-here-and-now. This latter type of CS-US association may be qualified as a purely *referential* relation.

What we propose next is that the Expectancy/Reference distinction should not be conceived of as a merely phenomenological issue, but as involving two functional systems, requiring different input conditions and different levels of information processing, responding differently to changes in the environmental stimulus contingencies, and finally, involving different modes of behavioral expression. The ES can be described as a system which detects reliable predictors or signals for significant events (USs), and which behaviorally prepares the organism in an economical and fine-tuned way to deal with the impending US. Once acquired that status, a reliable signal for a US will lead to an active and overt behavioral preparation of the organism to deal with the impending US, involving activation of autonomous and motor response systems of either the appetitive type (positive USs), of the defensive response type (negative USs), or of the orienting type (affectively less intense USs). Such response mobilization puts a serious load on the organism's limited energetic and information-processing resources, so the ES only responds to stimuli providing reliable and non-redundant information concerning US-occurrence. One form of competition, determining whether or not the ES will treat a stimulus as a true signal, is the competition between general context or background cues and potential CSs. This is reflected in the ES displaying CS-US contingency sensitivity: an objective degree of statistical correlation between CS and US occurrence is a logical prerequisite for a stimulus to be able to function as a reliable predictor of US occurrence. Responsiveness to non-redundant or the most salient information is accomplished by a further competition between predictors, and is reflected in phenomena such as blocking and overshadowing. Also, and of crucial importance for our argument, as an economical system, the ES should be sensitive to *non-occurrence* of the US. At the trial level, the non-occurrence of an actively expected US generally leads to the positive state of relief (omission of negative US), the negative state of frustration (omission of positive US), or the re-activation of the orienting response (omission of affectively less salient US) (see Gray, 1987; Siddle, 1991). Over a series of extinction trials, the expectancy of a US-going-to-happen-here-and-now is disconfirmed. Hence, to the extent that a CS has acquired real signal value and a response is measured indicative of this signal value, the newly acquired knowledge that the CS no longer signals an impending US will show up in conditioned response decrement.

The RS is a less sophisticated system, which registers co-occurrences between neutral and valenced events. It shapes an organism's likes and dislikes, and broadly tunes the organism towards approaching the 'good' and avoiding the 'bad'. At a response level, a purely referential relation will only be evidenced directly in choice/preference/evaluation situations, involving low response cost, or low differential response cost (e.g. choice between drinking water containing CS+ or CS- flavour, when drinking is necessary anyway; rating a picture/word/flavor CS on a visual-analog scale, when rating is required anyway; or changing the pattern of facial muscle activation and hence the facial display). The RS registers spatio-temporal co-occurrences between CSs and liked or disliked USs, and seems not to require a genuine CS-US correlation (see Baeyens *et al.*, 1993). Even though this has not yet been documented in the literature, we would also predict that the RS should not demonstrate other forms of 'competition' (such as blocking). Next, as the RS is not a system generating real US expectancies which typically involve a relatively high response cost, *there seems to be no good reason why the RS should be affected by non-occurrence of a US*. This insensitivity to US non-occurrence should apply at the trial level (no omission responses) as well as over a series of extinction trials. The latter is indeed what has been observed in the paradigms discussed above. Continuing to (dis)like or (not to) prefer something which has before been paired with something positive (negative) may, as long as no differential response cost is involved, provide not much of an adaptive advantage [unless the probability that the positive (negative) event will re-appear after the CS is higher than that it will appear after another stimulus], but neither represents an adaptive disadvantage (otherwise, the mechanism would not be there). Probably, it can best be conceived of as an *extra-adaptive* feature of an otherwise adaptive learning mechanism (Piattelli-Marmarini, 1989).

#### CONCLUSION

The main motivation for writing this response on Davey's (1994) review was that researchers not familiar with EC might take Davey's arguments on face value. We hope to have shown that many of Davey's critical comments are questionable and that therefore thinking of EC as a qualitatively different form of Pavlovian conditioning is quite defensible. It is this perspective of entwining a new and largely unexplored form of learning that makes EC such a fascinating research topic.

*Acknowledgements*—This research was supported by the National Fund for Scientific Research (Belgium). The authors wish to thank Geert Crombez, Dirk Hermans, and Paul Eelen for their helpful comments on earlier drafts of the manuscript.

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