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Evaluative Conditioning Is Pavlovian Conditioning: Issues of Definition, Measurement, and the Theoretical Importance of Contingency Awareness

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Evaluative Conditioning is Pavlovian Conditioning: Issues of definition, measurement and the theoretical importance of contingency awareness.

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Abstract

In her commentary of Field (1999), Hammerl (1999) has drawn attention to several interesting points concerning the issue of contingency awareness in evaluative conditioning. First, she comments on several contentious issues arising from Field's review of the evaluative conditioning literature, second she critiques the data from his pilot study and finally she argues the case that EC is a distinct form of conditioning that can occur in the absence of contingency awareness. With reference to these criticisms, this reply attempts to address Hammerl's comments by exploring the issues of how awareness is defined, how it is best measured, and whether it is reasonable to believe that EC uniformly occurs in the absence of contingency awareness. The article concludes that the available evidence supports Field's proposition that EC is, in fact, Pavlovian learning.

1 Introduction

Hammerl (1999) has provided food for thought on my original paper (Field, 1999). The intention of the target article was to highlight certain pertinent issues about the role of contingency awareness in evaluative conditioning (EC). The themes of the paper were (a) distinguishing demand awareness from contingency awareness; (b) how might contingency awareness be best measured; and (c) whether the available

evidence suggests that EC can occur without contingency awareness and, if so, is EC is qualitatively distinct from other forms of Pavlovian conditioning. Hammerl (1999) raises questions concerning the literature review, the pilot study data and the conclusions drawn. In this reply I stick to the themes of the target article as a framework from which to address Hammerl's comments.

2 The EC Literature

2.1. Can contingencies be forgotten?

Hammerl raises several objections to my interpretation of the EC literature. First, she suggests that the target article (Field, 1999) implies that the post-experimental awareness measurement occurs hours after the conditioning procedure. Although this impression was not deliberately created, she correctly re-iterates that awareness is measured subsequent to the re-rating of test stimuli. She asserts that this re-rating stage enables subjects to recognize and memorize the crucial stimuli and thus facilitates the subsequent measurement of awareness. Contrary to this proposition, Fulcher and Cocks (1997) showed empirically in a counterbalanced visual EC study that participants were significantly less able to recall UCSs following a standard EC rating stage than when asked to recall UCSs immediately after conditioning. Therefore, even the short re-rating stage to which Hammerl refers has a significant detrimental effect on participants' ability to recall contingencies. Moreover, unless CSs are presented alongside their corresponding UCS during the re-rating stage, it is

unclear why, as Hammerl suggests, this stage should facilitate the measurement of awareness —would it not distract participants' attention away from CS-UCS contingencies?

2.2. Definitional problems: what do awareness measures actually measure?

A second objection raised by Hammerl is that in many EC studies (notably her own) all participants are classified as unaware of all contingencies and so data cannot be subject to the statistical problems discussed in the target article (Field, 1999). If we look at these studies, with the exception of Hammerl and Grabitz (1993) in which the awareness measure is not reported, an awareness interview has been used. First, participants were asked whether anything in the experiment caught their eye or irritated them and if so, what? Second, participants were asked whether their second rating of the stimuli differed from the first and if so in what direction was the change and to what did they attribute this change? Finally, participants were asked whether they noticed regularities or patterns during the presentation of the pictures and if so did they notice these regularities during the presentations or subsequently (See Hammerl and Grabitz, 1996a, p. 283; Hammerl, Bloch and Silverthorne, 1997, p. 497). None of these questions relate to specific contingency awareness (the knowledge that a CS is consistently preceded by a specific UCS, or picture of consistent affective value). Instead, questions relate only to whether participants believe their ratings to have changed or whether regularities in stimulus presentations were detected. It

could be argued that if participants did not notice regularities in the presentation of stimuli then they would be unlikely to possess exact knowledge of which CSs and UCSs were paired. However, it is unclear how participants might interpret the 'regularities' or 'patterns' to which the question refers. The question relating to whether participants believe their ratings to have changed is problematic on two counts: (a) it relates very much to demand awareness (knowledge of what behavioural change is predicted), and (b) it does not refer specifically to either CSs or UCSs. On this latter point, rating changes are expected for CSs but not UCSs, therefore, participants' ratings of stimuli are expected to change on only 50% of occasions. They are, therefore, unlikely to report that their ratings were *generally* changing.

These criticisms by no means reflect on Hammerl and her colleagues — they have merely adapted their interview from other researcher — but they do highlight the point that before general conclusions are made about contingency awareness, researchers must be clear about what has been measured. In terms of Shanks and St. John's (1994) criteria for assessing contingency awareness, the measure described above raises questions regarding both the information and sensitivity criteria (see Field, 1999). In terms of information, the awareness interview lacks precision: it is unclear whether the questions tap demand awareness (awareness that ratings should change) or contingency awareness (knowledge that a particular CS is consistently preceded by a particular UCS, or by a picture of a particular affective value).

Participants are not required to recognise pairings, identify specific CS-UCS pairs, or specify the affective value of the picture paired with a particular CS and so the interview is more likely to address demand awareness than contingency awareness. If the interview lacks precision in terms of the information gathered, it must also lack sensitivity because a measure must surely be insensitive if it taps imprecise information. For these reasons, it is not surprising that Hammerl and her colleagues have found little evidence of contingency awareness in EC. However, it is reassuring that their studies probably do provide support for the argument that EC effects are established in the absence of *demand* awareness.

As a final minor point of clarity, Hammerl notes that I incorrectly describe her studies as nondiscriminative. At the beginning of my target article (Field, 1999) I define a discriminative paradigm as one in which one CS is paired contingently with a UCS (CS+) whereas another is not (CS-). In her studies all CSs are paired contingently with a UCS (a neutral or positive stimulus) so by the definition I used they are not discriminative because no CS-only pairings are used.

3 The Pilot Study

3.1. Response to criticisms

The pilot data presented in the target article (Field, 1999) were an adjunct to the main article: their purpose was to illustrate some of the theoretical points already made.

Hammerl criticises my use of an existing measure of contingency awareness on the grounds that I had previously suggested that the measure was flawed. I'm unclear as to why this objection is made: the point of the study was to compare new with old. To make any judgement about a new measure of contingency awareness it must be compared with one that presides over the literature — even if that measure is flawed. A more valid objection is that the 'old' awareness measure chosen as a comparison was a stringent measure and that a more informative comparison would be with a less-stringent measure. Some degree of misinterpretation appears to have occurred with regard to this issue. Hammerl states that the 'new' recognition measure was compared against the so-called 'strong' measure of awareness in which participants are given each CS as a prompt from which to identify the specific UCS with which it was paired. She suggests that a more instructive comparison would be to compare the recognition measure with what has been called the 'weak' measure of awareness in which subjects have only to identify the affective value of the UCS. I agree. However, the target article clearly states that for the strong measure 'a contingency was classified as aware ... if the subject could identify the exact UCS with which it was paired during conditioning *or chose one of the same affective value*' [italics added](Field, 1999, p. XXX (quote is on p. 27 of target article Ms)). Therefore, the measure used was a compromise between the 'strong' and 'weak' measures of contingency awareness traditionally used. The only difference between the measure of awareness used by Field (1999) and the 'weak' measure to which Hammerl refers

was that participants were not *explicitly* allowed to indicate the affective value of the UCS. They were, however, permitted to pick a different picture of the same affective value as the UCS. As such, her criticism is partly unjustified. Nevertheless, I agree that comparing the recognition measure with the weakest possible measure would be an instructive additional experiment. However, the important point that the data illustrate (regardless of Hammerl's objections) is that different measurement procedures give rise to different contingency classifications. Specifically, even a fairly weak criterion for awareness (although not the weakest) results in considerable overclassification of contingencies of which participants are deemed unaware when compared to a recognition measure.

3.2. Which measure of contingency awareness should be used?

Given that different measurement techniques give rise to different classification profiles for CS-UCS contingencies, the question arises of which method is best. Clearly a good measure of contingency awareness not only has to meet Shanks and St. Johns (1994) criteria, but must also distinguish between contingencies of which participants are aware and those of which participants are unaware. In doing so, contamination can be eliminated (see Field, 1999). To meet Shanks and St. John's (1994) criteria the measure must be precise both in terms of what it measures and how accurately it measures. My opinion, not surprisingly, is that the recognition measure described by Field (1999) meets the information criterion, because participants discriminate between actual and decoy pairings and so it can but only

measure contingency awareness (and not demand awareness). In addition, Field's (1999) data shows that it is more sensitive than some other commonly-used measures (although comparison with an even less stringent measure is necessary before a firm conclusion is made). Finally, because the recognition procedure measures awareness at a *per contingency* level, it allows contamination to be detected and eliminated.

However, one concern might be that the recognition measure is over-sensitive, thus allowing participants to report awareness that they may not really possess. As ever it seems that the sensible recommendation is compromise: use a recognition measure in parallel with other measures (counterbalanced in order across subjects) and categorise contingencies on the basis of consensus between measures. Where no consensus can be found, classify contingencies on the basis of each measure separately, analyse the results, then compare the conclusions drawn. If the conclusions concur then interpretation of the data can be made with some confidence. If the conclusions differ then at least some insight can be gained into the relative sensitivity of the awareness measures.

4 Theoretical Issues

4.1. Hammerl's interpretation of the EC literature

Hammerl cites a large body of work on affective priming, which shows that subliminal priming effects are often much stronger when participants are unaware of

the experimental manipulations. She concludes that these results 'indicate that demand awareness (otherwise as Field assumes) rather inhibits experimental effects than facilitating them'. It is important to be clear about whether we are discussing demand- or contingency-awareness (both defined earlier). In the affective priming literature there are no CS-UCS contingencies to learn (they are not conditioning experiments) and so researchers are not interested in whether subjects consciously associate two stimuli. Therefore, this evidence relates more to demand awareness than contingency awareness. I have already argued that the awareness measures used by Hammerl and her colleagues relate more to demand awareness than contingency awareness and, therefore, I agree that there is evidence that demand awareness sometimes does not influence experimental effects. However, this tells us little about the role of contingency awareness.

In any case, the evidence Hammerl cites suggests that demand awareness inhibits experimental effects in priming experiments, it does not mean that demand awareness cannot inhibit experimental effects in other paradigms (such as conditioning experiments). There are studies suggesting that demand awareness can contribute to experimental effects when conditioning paradigms are used. For example, Allen and Janiszewski (1989) attempted to manipulate both demand- and contingency-awareness and found conditioning effects only in subjects who were demand aware or contingency aware. In addition, many studies have shown that effects believed to represent true associative learning can actually be attributed to the

effects of demand awareness (for example, Page, 1969, 1974; Kellaris & Cox, 1989; Darley & Lim, 1993; but see Shimp, Hyatt, & Snyder, 1993).

Hammerl also summarises some new research in which a distracter task is used to reduce contingency-awareness (Hammerl & Grabitz, 1996b, 1999). I welcome this systematic approach to the contingency-awareness issue, and am especially relieved to see the use of appropriate between-subject controls such as the BSB control (Field, 1996, 1997). Although these studies are, at present, difficult to evaluate (one is available as only an abstract and the other is, as yet, unpublished) they look like interesting work. The general finding is that subjects classified as unaware show conditioning effects whereas those classified as aware show opposite effects (so-called 'conscious counter-control'). There are two issues pertinent to these studies. The first is that although the use of a distracter seems like a neat way of reducing contingency awareness, levels of contingency awareness still have to be measured. Hammerl reports that the 'weak' measure of awareness was used and I can but assume that this measure is the one employed in her previous studies. If so, then I question whether *contingency* awareness has actually been measured (see earlier). Therefore, the results of subjects classified as aware or unaware may reflect demand awareness more than contingency awareness. However, that's not to say that levels of contingency awareness weren't lower in the group that did the distracter task — it's just difficult to tell from the measure used. As a second point, if the measure used was the one that Hammerl traditionally employs then contingency awareness was

not measured on a per-contingency basis. Therefore, conclusions about levels of classification contamination, if any, cannot be made. Despite these observations I applaud this systematic approach and hope that in future work different measures of contingency awareness might be used to unpack the role of contingency awareness in EC further.

4.2. Theoretical basis of EC: is it qualitatively different from other forms of Pavlovian conditioning?

Contrary to Hammerl's interpretation, I had no intention of implying that autonomic conditioning occurs *only* when participants have knowledge of the contingencies of conditioning. The point I tried to highlight was that different methodologies invariably give rise to different conclusions about the role of contingency awareness: typically, recall measures give rise to the conclusion that conditioning occurs without contingency awareness, whereas recognition measures tend to show conditioning only with contingency awareness (Dawson, 1973, Dawson & Schell, 1987). The important implication from this observation is that because EC researchers exclusively use recall measures of awareness it is not surprising that these studies have shown conditioning without awareness. Important is that the experiments showing that EC apparently occurs without contingency awareness have been used to support the theoretical position that EC is qualitatively distinct from other forms of Pavlovian learning (Baeyens, Eelen, & van den Bergh, 1990). If recall measures of awareness give rise to the conclusion of conditioning without awareness in both

autonomic conditioning experiments (see Dawson & Schell, 1987) and EC studies, then EC is not qualitatively distinct using this criterion.

Hammerl concludes by arguing that EC is interesting not just because of the awareness issue, but also because it has been shown to be sensitive to sensory-preconditioning (Hammerl & Grabitz, 1996a) and postconditioning revaluation (Hammerl et al., 1997; Baeyens, Eelen, van den Bergh, & Crombez, 1992). In addition, much is made of the apparent finding that EC is resistant to extinction (Baeyens, Eelen, & Crombez, 1995). There is now substantial evidence that the visual EC paradigm is prone to an artefact that gives rise to conditioning-like effects not due to associative learning (Field & Davey, 1997, 1998, 1999; Shanks and Dickinson, 1990) and so evidence for some of the phenomena cited by Hammerl is equivocal (see Field, 1997; Field & Davey, 1998). Nevertheless, even if we assume that these phenomena genuinely exist, sensory pre-conditioning and postconditioning revaluation are characteristics that we have come to expect in ordinary classical conditioning (see Dickinson, 1980 for a review). This leaves only resistance-to-extinction as a distinguishing characteristic of EC. Although there is evidence for resistance to extinction using visual stimuli (Baeyens, Crombez, van den Bergh, & Eelen, 1988) this evidence has been empirically questioned by Field and Davey (1999). The best evidence for resistance-to-extinction in the EC paradigm comes from studies in which tastes are used as stimuli. Notably, these studies show that neutral flavours acquire negative evaluations through contiguous pairing with unpleasant

flavours (for example see Baeyens, Eelen, van den Bergh, & Crombez, 1990). Nevertheless, there is evidence that resistance-to-extinction can occur in other conditioning paradigms also. First, Sclafani (1991) reviews several studies that show resistance to extinction in Pavlovian conditioning paradigms using food stimuli with rats. Second, Grant (1964) distinguished between Pavlovian A and Pavlovian B conditioning; in the later form the effect of the UCS is independent of instrumental acts by the organism. As such, Pavlovian B conditioning is typified by the conditioning of ecologically important responses such as illness, fear and anxiety. Eysenck and Kelly (1987) argue that extinction is unlikely to occur in Pavlovian B condition and that it is even possible that the strength of the conditioned response will increase over extinction trials (incubation). The EC paradigm that shows the best evidence of resistance to extinction uses flavour stimuli and so could, arguably, be an example of Pavlovian B conditioning because it involves inescapable consequences (illness or disgust). Finally, even in autonomic conditioning paradigms, Davey (1987) has shown that extinction may be highly dependent on cognitive factors: resistance-to-extinction may occur when participants *believe* that their responses are being maintained. Davey (1994) has also pointed out that the experimental subterfuge used in some EC studies may well lead participants to believe that their responses should persist. There is little hard evidence about the belief-state of a participant undergoing an EC extinction procedure; however, it is perfectly plausible that the observed

findings result from the types of cognition that Davey (1987) described in autonomic paradigms.

Hammerl's assertion that, based on the apparent characteristics that have been demonstrated, EC is qualitatively distinct from Pavlovian conditioning seems unjustified. It is equally unclear why, on a theoretical level, EC should be any different from autonomic Pavlovian learning. In my target article (Field, 1999) I suggested that in the autonomic conditioning literature there is, *ceteris paribus*, better evidence that conditioning in the absence of contingency awareness can occur in ecologically-relevant conditioning episodes such as a fear-relevant CS predicting a fear-evoking UCS (see Öhman, Esteves & Soares, 1995). I also suggested that EC may not be distinct from other forms of Pavlovian conditioning but likewise have a subset of ecologically-important domains (e.g. taste) in which the 'normal rules' do not apply (see Field, 1999). These observations might gel with Grant's (1964) idea of Pavlovian B conditioning, in that a certain subset of ecologically important responses may obey different laws of conditioning. Therefore, EC may appear distinct only because its unusual properties have been demonstrated using certain types of conditioning episodes (those that might fall into the category of Pavlovian B conditioning).

The studies that Hammerl cites as evidence for EC in the absence of contingency awareness (Hammerl & Grabitz, 1996b, 1999) used haptic stimuli. Intuitively, these physical sensations could be thought of as having ecological relevance (for example, it might be very important to learn about unpleasant sensations from abrasive

surfaces to prevent injury or pain). Therefore, even if these studies bear up to scrutiny their results support the position that conditioning occurs without contingency awareness only when the learning episode is ecologically relevant (e.g. tasting, smelling, touching). Intuitively, learning these types of responses quickly, and without awareness or extinction may afford an organism adaptive benefit: it will learn — with little cognitive expense — to like/approach healthy foods or pleasant haptic environments and dislike/avoid toxins and environments likely to cause pain. However, it is less clear what adaptive benefit arises from learning to like/dislike stimuli merely from seeing them in parallel with other liked/disliked objects. It is perhaps more plausible that in these circumstances an organism would use the category- and similarity-based behaviour reviewed by Field and Davey (1999).

A convincing account of why EC is a distinct form of Pavlovian conditioning must first unequivocally demonstrate the unusual characteristics with which it is currently associated but outside of ecologically-relevant response systems. Even then, the theoretical basis of EC needs to incorporate some account of the adaptive benefit, if any, of this form of learning. As it stands, the evidence for EC seems only to mimic what's been found in Pavlovian conditioning generally (at least, Pavlovian B conditioning) and in this sense EC is not a distinct form of Pavlovian learning.

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