

## Applications and Extensions

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In the final two chapters of this book, we turn to phenomena whose practical implications are more direct than some of the phenomena previously discussed. In Chapter 10, Robin Murphy and colleagues discuss the “depressive realism” effect. This effect, first reported by Alloy and Abramson (1979), concerns the difference between depressed and nondepressed individuals in their ability to rate zero contingencies. Zero contingencies ( $\Delta P = 0$ ) are where, for example, an individual’s action does not change the probability with which an event occurs. When subsequently asked to rate the level of contingency, depressed individuals are generally *more* accurate than nondepressed individuals. Depressed and nondepressed individuals’ ratings do not appear to differ when the contingency is nonzero. Taken at face value, these results seem to pose certain questions for the position that depression is related to faulty thought processes (e.g., Beck, 1976).

Robin Murphy and colleagues dissect the original Alloy and Abramson (1979) result from the perspective of known phenomena in human associative learning. First, they note that in nondepressed individuals the magnitude of overestimation of zero contingencies is modulated by the base rate of the outcome. The most common result is that as the outcome becomes more likely to occur overall, the zero contingency becomes progressively more overestimated (Vallée-Tourangeau, Holingsworth, & Murphy, 1998). However, this result is not always found (e.g., Wasserman, Chatlosh, & Neunaber, 1983) and Robin and colleagues suggest that, where it is not found, this may be due to the action-stimulus interval employed. In this context, action-stimulus interval (ASI) is the time between an action and the occurrence of the event that it caused. In some experiments ASI is virtually zero, whereas in others it is about 500 ms. Robin and colleagues go on to demonstrate systematically that ASI modulates the extent to which zero contingencies are overestimated. This result is in line with previous demonstrations

that ASI modulates ratings of nonzero contingencies (Shanks, Pearson, & Dickinson, 1989).

A central conclusion from this sort of study is that aspects of experimental design that may appear incidental to a particular experimenter, such as the inclusion of a short ASI, can importantly affect the conclusions one draws. The connection to "depressive realism" is that previous studies of the effect have generally used very long intertrial intervals (ITIs), and studies that have failed to replicate the depressive realism effect (e.g., Dobson & Pusch, 1995) have used relatively short ITIs. From these results, Robin and colleagues suggest that depressed individuals are particularly prone to include the ITI in their assessments of contingency. In other words, depressed individuals are more likely to count the unfilled gap between trials as an example of a situation where they did not act and the outcome did not occur ("cell D" on a standard contingency table), whereas nondepressed people are more likely to ignore the ITI. Robin and colleagues then go on to provide evidence for this position by manipulating ITI systematically with both depressed and nondepressed individuals. One may therefore conclude that although depressed and nondepressed individuals approach the judgment of zero contingencies differently, it is not straightforwardly the case that the depressed individuals are more accurate.

In chapter 11, we move on to a research area with commercial implications as Andy Field introduces the concept of evaluative conditioning (EC). The basic idea of EC is to set up a situation where a neutral stimulus repeatedly predicts the occurrence of a stimulus that is already well-liked. EC has occurred if the neutral stimulus becomes more liked as a result of this treatment. The idea behind EC can be seen in many forms of product promotion. For example, one might follow a product launch with an excellent free lunch, hoping thereby to increase liking of the product. Similarly, the use of pleasant music (or pleasant individuals) in television commercials can be seen as an attempt to increase liking for a product by EC.

Does EC really work? Andy Field reviews the evidence, starting with a broad range of studies that appear to show successful EC for visual, taste, and odor stimuli. However, things do not remain optimistic for long, as Andy reveals a whole host of methodological problems with these initial demonstrations. After one has worked through the claims and counterclaims, it seems likely that EC of tastes reliably occurs and that EC of visual stimuli can occur but is perhaps less reliable.

Andy Field then addresses the question of whether EC is qualitatively different from other types of conditioning. For example, considerable evidence suggests that, in humans, awareness is required for classical conditioning to occur (see chap. 3, this volume). In contrast, some researchers (and presumably some advertisers) seem to assume that EC can occur in the absence of awareness. The evidence for this position seems equivocal at best. Additionally, some researchers assume that EC is highly resistant to extinction, and EC's usefulness as an efficient mar-

keting technique seems dependent on this being true. Some resistance to extinction can be seen in a design where acquisition trials are followed by an equal number of extinction trials. In this situation the extinction trials can fail to reliably change the size of the EC effect. However, such a finding is not unique to EC.

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