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BLACKWELL

Folk Psychology Won't Go Away: Response to Allen and Bekoff

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Abstract: Responding to Allen and Bekoff's (this issue) critique of Heyes and Dickinson's (1990) analysis of the intentionality of animal action, we reiterate that our approach does not assume that a hypothesis can be definitively falsified by the results of a single experiment, and argue that the evolutionary analysis favoured by Allen and Bekoff insulates intentional accounts of animal behaviour from rejection in the usual 'holistic' process of scientific evaluation. Specifically, we present data showing that the maintenance of behaviour on an omission schedule cannot be construed as rational because on these schedules it is reward for withdrawal that restores approach. In addition, we argue that, since behaviour can be affected by the non-intentional properties of representations such as search images and cognitive maps, whether or not these representations have intentional properties can be assessed empirically only through research on instrumental behaviour.

Heyes and Dickinson (1990) argued that animal action does not warrant intentional explanation unless there is experimental evidence, of a specified kind, that the action is based on the rational interaction of an instrumental belief and a desire. Allen and Bekoff (this issue) objected both to the general thrust of Heyes and Dickinson's analysis, and to several of our more specific claims. At the general level, they argued that an intentional account of animal action cannot be accepted or rejected using the methods we proposed because (1) no hypothesis can be tested in isolation and definitively either confirmed or falsified, and (2) the methods we recommend do not take sufficient account of animals' evolutionary history and natural ecology. In reply we should like to reiterate, first, that the approach taken by Heyes and Dickinson (1990) does not assume that crucial tests are possible, and, second, that there is a danger that evolutionary considerations will be used to insulate intentional accounts of animal behaviour from rejection in the usual process of 'holistic' or convergent scientific evaluation.

The section of Heyes and Dickinson's (1990) paper headed 'Manipulating mental states' contained the clearest indications that, having read Quine, we recognize that 'single belief attributions, rather like scientific hypotheses, are

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not testable in isolation. Any apparent falsification of a scientific hypothesis can be compensated for by a revision in the background assumptions' (Allen and Bekoff, this issue, p. 319). In that section, we emphasized the pragmatic value, rather than the logical force, of our criteria of animal intentionality, and acknowledged explicitly that if an animal's behaviour fails to change under the influence of experimental manipulations designed to alter the animal's beliefs and desires, it could mean either that the behaviour is not intentional, or that the experimenter has manipulated the wrong variable(s). We even gave an example of conditions in which a false negative conclusion would be likely: a researcher might naturally expect that making rats thirsty, rather than hungry, would alter their desire for sucrose solution, and, finding no change in a behaviour that caused access to sucrose solution, therefore conclude that the behaviour was not intentional. This would be an error because, as Dickinson and Dawson (1988, 1989) discovered, rats' instrumental behaviour changes with a shift from hunger to thirst, but only when the animals have had an opportunity to consume sucrose solution and food pellets under thirst at some point prior to the instrumental test.

Because errors of this kind are likely, we must be cautious in concluding that a particular instrumental behaviour is not intentional. But we cannot afford to be too cautious. As Allen and Bekoff point out, there can be no hard and fast rules telling scientists when to forsake Newton for Einstein, or mentalism for mechanism. However, unlike Newtonian mechanics, the mentalism embodied in folk psychology seems to have a pervasiveness and sticky staying power, a natural resistance to falsification, which means that, in practice, the challenge is to make it go away. When it is not only the actions of Fodor's Greycat and Allen's Pluto that invite intentional explanation, but also the phototaxis of sunflowers and tactile sensitivity of mimosa, no realist conception of intentionality will be credible until scientists and philosophers can specify conditions in which intentional accounts of behaviour are *not* acceptable.

Instead of specifying such conditions, Allen and Bekoff attempted to save attributions of intentionality from our proposed methods of evaluation (Heyes and Dickinson, 1990). Their objections were motivated by evolutionary and ecological considerations, and they illustrate the staying power of folk psychology, its resistance to falsification. By referring to two examples, however, we shall suggest that Allen and Bekoff's objections are not ultimately successful in showing that our analysis is too harsh or impractical.

Challenging Premise 3 of Heyes and Dickinson's argument, Allen and Bekoff suggested that continued performance on an omission schedule for some target response, R, is compatible with a belief of the form *cause* (R, *access* (O)) because a non-optimum outcome (O) rate (e.g. 30% in the case of Hershberger's chucks approaching a food bowl) might be sufficient to maintain the belief. This hypothesis is not consistent, however, with the cyclic nature of performance on omission schedules, which we illustrated by the anecdote about Tony's amorous adventures (Heyes and Dickinson, 1990, p. 92). Omission schedules tend to induce a pattern of behaviour in which

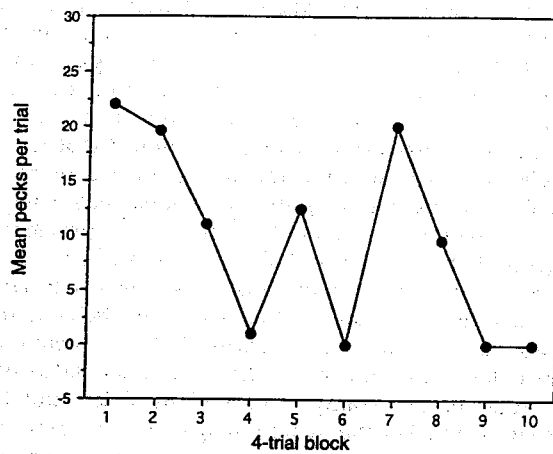


Figure 1 This figure illustrates the pecks of an individual pigeon directed at a small, localized visual stimulus during training on an omission schedule. Prior to this training the stimulus has consistently predicted food and thus at the outset of omission training the stimulus elicited a high rate of pecking on each trial when it was presented. This pecking caused the omission of the food, so that the stimulus ceased to be a signal for food. Consequently, its capacity to elicit pecking extinguished. Once the behaviour had extinguished, however, the stimulus again signalled food, which, in turn, re-initiated pecking. The figure shows two full cycles of this maladaptive behaviour.

fruitless approach to the goal gives way to successful withdrawal, and the very success of the withdrawal promotes further approach. Thus, it is attainment of the goal in episodes where R is not performed that restores responding. An example of this cyclic pattern is illustrated in Figure 1, which shows the rate at which a pigeon approaches and pecks at a small, localized visual stimulus. Prior to the training on the omission schedule, the stimulus had consistently signalled food and thus reliably elicited approach and pecking. Consequently, at the outset of the omission training displayed in Figure 1, consistent pecking at the stimulus caused the food to be omitted on every trial, which, in turn, resulted in the extinction of the signalling properties of the stimulus and hence of its attractiveness. As soon as the bird stopped pecking, however, the stimulus was once again paired with food, thus re-initiating another cycle of the acquisition and extinction of the response. Although Figure 1 only shows the pattern across 40 trials, the cycle will often be sustained indefinitely. It is not, as Allen and Bekoff suggest, that animals on omission schedules merely fail to contact the omission contingency and hence to notice that withdrawal or inhibition of approach (and pecking) is more successful. Instead, and irrationally, it is exposure to the very success of response inhibition that re-establishes the maladaptive behaviour.

Turning to our second example, Allen and Bekoff dispute that practical inference necessarily forms the final common pathway in the causation of behaviour by intentional mental states, and thereby suggest that, even if a response or action fails our tests of intentionality, it could still be a valid behavioural marker of intentionality. We considered such a case (Heyes and Dickinson, 1990, p. 89) when we discussed an action R that is caused by an instrumental belief and a desire, the representational content of which is unrelated to R. While acknowledging that intentional states may well cause such responses, we would draw attention to a major problem: how does one ever distinguish behaviour caused by an intentional state non-rationally related to its content from behaviour caused by states without intentional content? Until Allen and Bekoff can offer us a clear, behavioural basis for drawing this distinction, we are not persuaded that non-intentional reactions can be taken as behavioural indices of the intentional status of the causal antecedent. In terms of the examples offered by Allen and Bekoff, how would we ever know whether the 'search image' of a predator or the 'cognitive map' of a bee has intentional properties (for the predator or bee, rather than the human observer) unless it can control behaviour that is rational with respect to the content of these states? It is not sufficient to appeal to the adaptiveness of the behaviour, because the rationality that matters with respect to intentionality is that of the psychological processes of the individual agent, not of the evolutionary process. To the extent that evolution has ensured that an animal's behaviour is well adapted to its environment, non-intentional behaviour will appear rational in canonical (i.e. natural) situations, and thus the only way of distinguishing between intentional and non-intentional behaviour is by examining it in situations where the contingencies diverge from the canonical.

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