

Imitation and flattery: a reply to Byrne & Tomasello

C. M. HEYES

Department of Psychology, University College London

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Byrne & Tomasello (1995) denied that experiments with rats, *Rattus norvegicus* (Heyes & Dawson 1990; Heyes et al. 1992, 1994) and budgerigars, *Melopsittacus undulatus* (Galef et al. 1986) have provided evidence of imitation, suggesting that the behaviour observed in these experiments could be due to local enhancement (Thorpe 1956) or assigned to categories defined by the authors: 'response facilitation' (Byrne 1994) and 'emulation learning' (Tomasello 1990). In defending the view that, so far, rats and budgerigars have provided better evidence of imitation than any non-human primate (Heyes 1993), I shall question the value of defining imitation in relation to behavioural novelty, and particular dimensions of behavioural resemblance. Like Byrne & Tomasello, I shall focus on the evidence from rats, referring only briefly to studies of budgerigars and primates.

In the experiment by Heyes et al. (1992) which sought evidence of imitation in rats, each 'observer' rat was allowed to watch, through a wire-mesh partition, a 'demonstrator' conspecific pushing a joystick 50 times either to the left or to the right. When the demonstrator had completed 50 pushes, with food reward for each, it was removed from the chamber, and the observer was placed in the compartment with the joystick, and allowed to make 50 pushes itself, with food reward for both left and right responses. For half of the subjects, tested in the 'standard' condition, the joystick remained in the same position between observation and testing. For the other half, in the 'perpendicular' condition, the joystick was moved before testing to the front wall of the chamber (see Fig. 1). In both conditions, the observer rats showed a reliable tendency to push the joystick in the same direction, relative to the actor's body, as had their demonstrators. For example, rats that watched demonstrators pushing to the left, towards L1 in Fig. 1, pushed

predominantly in the direction of L1 if they were tested in the standard condition, and towards L2 if they were tested in the perpendicular condition. Thus, when these observer rats pushed the joystick in the same direction relative to the actor's body as had their demonstrators, the joystick moved in the opposite direction within their visual field (egocentrically defined) and in space (allocentrically defined), to that in which it had moved during observation.

There were two reasons why we thought it appropriate to describe these results as 'imitation'. First, the absence of any effect when rats observed the joystick moving automatically (Heyes et al. 1994) suggested that this was a social learning phenomenon, but it apparently did not conform to any predetermined category of non-imitative social learning (Heyes 1994a, in press). Second, and more important, to describe the rats' behaviour as imitation was consistent with the way in which that term had been used by the earliest and the most influential investigators of social learning, for example, Morgan (1900), Thorndike (1911), Thorpe (1956) and Galef (1988). These authors used 'imitation' to denote a type of social learning in which behavioural matching is achieved through a complex psychological process (Heyes 1994a, in press). It seems that, over the last 100 years, every putative demonstration of imitation in animals has been challenged, and some good reason has been found to doubt its reliability or validity (e.g. Galef 1988, 1992; Visalberghi & Frigaszy 1990). Consequently, it would not be surprising if the behaviour we have observed in rats using the bidirectional control procedure were to suffer the same fate. However, Byrne & Tomasello (1995) have not found a good reason for doubt.

Byrne & Tomasello (1995) raised three objections to our use of the term imitation to describe the rats' behaviour in this experiment. First, they suggested that, in an analogous situation, humans might not behave in the same way as the rats. This intuition may be correct, but a contrast between

Correspondence: C. M. Heyes, Department of Psychology, University College London, Gower Street, London WC1E 6BT, U.K.

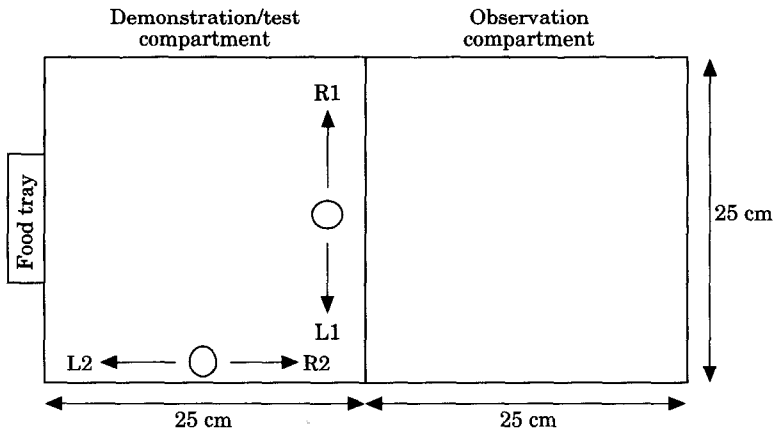


Figure 1. Plan of the apparatus used by Heyes et al. (1992) showing the position and plane of movement of the joystick for rats tested in the standard (L1, R1) and perpendicular (L2, R2) conditions. (Reprinted with the permission of the Experimental Psychology Society.)

the behaviour of rats and people would not show that the rats were not imitating. Unlike rats, people have background knowledge of mechanics, building design and the intentions of people

one plane, parallel to, or coincident with, the nearest wall. However, this would not explain the results in either the standard or the perpendicular conditions because imitating movement

to decide not to imitate in this situation, or to imitate a different feature of the demonstrator's behaviour.

Second, Byrne & Tomasello claimed that the behaviour of the rats could have been due to local enhancement or emulation learning. In the case of local enhancement, they argued that the rats learned during observation that the joystick should be moved towards L1, and that when they were tested in the perpendicular condition the rats thought they were moving it to L1 when they were in fact pushing it towards L2. This is implausible because, as Byrne & Tomasello noted, there is evidence that rats have an 'excellent sense of space'. More generally, arguments of this kind, which suggest that when theory and evidence conflict, the theory is right but the world is wrong, are weak because they tend to make theories untestable.

Byrne & Tomasello's (1995) emulation account of the rats' behaviour suggested that 'the joystick itself, and its position relative to a wall (any wall) is used as a landmark for orientation. Then . . . the observer notes the position of the stick and how it moves relative to the wire grid wall and then transfers that orientation to the joystick in its new position relative to the new wall it is up against.' This implies that joystick movement could have been defined or encoded by the rats relative to just

and right pushing, and yet the behaviour of these two groups was different. Thus, the rats' behaviour cannot be explained in terms of local enhancement or emulation.

Byrne & Tomasello's third objection to our description of the rats' behaviour as imitation suggested that we could not know whether the rats were imitating because their behaviour was 'trivial' and we, the experimenters, did not observe it. I would like to defend the rats against the charge of triviality, but I do not understand it. Joystick-pushing almost certainly lacks adaptive significance for free-living rats, but then most types of behaviour used to test for imitation in primates (e.g. head patting, rake use, spigot turning) also lack adaptive significance, and yet Byrne & Tomasello did not judge these to be trivial. Perhaps Byrne & Tomasello meant that the rats' behaviour was simple, that it did not involve improbable or demanding acrobatics. This is undoubtedly true, but it is not clear why one might expect rats to be less inclined to imitate simple than complex behaviour.

Leaving the rats to face their charge alone, I should say in defence of those running the experiment (Heyes et al. 1992) that we observed just as much of the rats' behaviour as was necessary to establish what we wanted to establish, i.e. that

the observers pushed the joystick in the same direction relative to the actor's body as had their demonstrators. Having found, by watching video-recordings of pilot observers and demonstrators, that they faced the plane of joystick movement, we were able to do this using direction of joystick movement as an indicator of direction of actor movement. As noted above, we regard this directional matching as evidence of imitation because it apparently requires the kind of psychological complexity associated with the term imitation throughout the last 100 years of research on social learning (Galef 1988), and we are pleased to have found a procedure that may be used in further research to identify exactly which psychological processes are involved in imitation. In short, we see the demonstration of imitation in rats (or any other non-human animal) as a means to an end, not an end in itself.

If Byrne & Tomasello believe that the term imitation should be reserved for cases in which another kind of behavioural match is achieved, for example when the same appendage is used, or when the same muscles are flexed in the same order, then it would have been helpful if they had told us why. It is not clear that behavioural matching at the level of either appendages or individual muscles would require more cognitive sophistication, have greater adaptive significance, or be more conducive to cultural transmission, than directional matching. Byrne & Tomasello pointed out that what counts as evidence of imitation depends on our definition of imitation. Surely, our definition of imitation should, in turn, depend on the purpose of our enquiry (Heyes, in press).

Byrne & Tomasello's dissatisfaction with evidence of imitation in budgerigars (Galef et al. 1986) raises another, related issue: the utility of defining imitation in relation to the acquisition of a topographically 'novel' behaviour. (Here, and in a passage quoted by Byrne & Tomasello (Heyes 1993, page 1000), my use of the term 'acquire' does not imply that I am committed to this definition. Among psychologists studying animal learning, a behaviour is said to have been acquired if its strength or frequency has increased in a particular stimulus context or environment.) I think it is not useful to define imitation with reference to topographically novel behaviour for two reasons. First, given our rather poor understanding of the topographic 'grammar' of non-verbal behaviour, it is difficult if not impossible to

distinguish topographically novel behaviour from that which consists of novel recombinations of existing topographically defined elements (Whiten & Custance, in press). Second, whether or not a given example of social learning requires psychologically complex processing (or, for that matter, whether it involves a process that may contribute to adaptation or cultural transmission) does not depend on this distinction. The results of the experiment by Galef et al. (1986) suggested that budgerigars recognize that beak-use by a conspecific is more like beak-use than feet-use by themselves, and vice versa. Whether this is indicative of a complex psychological process depends, not on the novelty of beak-use and feet-use behaviour per se, but on the probability that the birds recognized the similarities by virtue of an innate mechanism. In the absence of any argument or independent evidence that budgerigars have an innate tendency to use their feet when they see conspecifics using their feet, and to use their beaks when they see conspecifics using their beaks, this probability must be assumed to be low. If it were not, then, by the same token, all evidence of imitation in humans could be attributed to 'response facilitation' (Byrne 1994).

Like Byrne & Tomasello, I have space only for an assertion about evidence of imitation in primates: it is not compelling. Of the studies they cited, two have the weaknesses common to anecdotal reports (Byrne & Byrne 1993; Russon & Galdikas 1993), the results of two others could have been due to stimulus enhancement (Hayes & Hayes 1952; Tomasello et al. 1993; see Heyes 1994b for a detailed critique), and a full report on the remaining study has not yet been published (Custance & Bard 1994). Like Byrne & Tomasello, I find it extremely implausible that rats and budgerigars can, while non-human primates cannot, imitate. However, there is a difference between a hunch and evidence, and to interpret the existing research on primates as evidence of imitation is to give undue honour to the human and non-human apes involved. At least for now, it seems that rats imitate and apes are flattered.

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