

[Continuing commentary on Euan M. Macphail, The comparative psychology of intelligence BBS 10, 645-695.]

Specious comparisons vs. comparative epistemology

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Several of the original commentaries on Macphail's target article questioned the logic of excluding perceptual abilities from the arena of comparative cognitive capacity. In his reply, Macphail stands by this separation, and is not deflected from his conclusion that "Intelligent behaviour in nonhuman vertebrates is dominated by association formation". However, the force of the argument in favour of species differences in perceptual complexity is illustrated by the fact that Macphail himself, in his book published in 1982, felt obliged to acknowledge that a) there are "sizeable differences in behavioural repertoire brought about largely by changes in perceptual and motor skills"; b) that these differences are reflected in "biological intelligence" which varies in some degree according to brain size; and c) that this biological intelligence is independent of the "general intelligence" which does not differ between species (Macphail, 1982 pp 337-338: he explicitly follows the suggestion of Jerison, 1973, that biological intelligence is related to "the capacity to construct a perceptual world").

It would appear therefore that there is greater agreement between Macphail and his critics among the commentators than he admits: everyone agrees that species may differ in cognitive capacities related to perception, memory, motor skills and motivation, and everyone agrees that much smaller differences are observable if these are ignored, and attention is focussed on the most rudimentary kinds of reflexive association. No-one is in favour of ranking all species on a single scale of "general intelligence".

There remains, however, a substantial difference of emphasis, with Macphail following a learning theory tradition of wishing to dismiss species specializations in favour of universal laws of association, while others have greater interest in phylogenetic, ecological, anatomical and other 'contextual' variables. An instructive parallel exists in the contrast between the tradition in learning theory of supposing that the laws of association are virtually independent of the details of the neurological mechanisms of support, and the concern of comparative anatomists to assign psychological meaning to the physical products of brain evolution. For habituation, Thompson and Spencer (1966) and Groves and Thompson (1970) proposed that all behavioural phenomena of interest could be studied in the hind limb flexion reflex of the acute spinal cat, whereas others have used the procedures of habituation to map complex attentional and perceptual process in human infants and adults (Adams et al 1975; Atkinson, 1984; Olson, 1976; Sokolov, 1975). For classical conditioning decorticate mammals undoubtedly display acquisition and discrimination with simple stimuli (Oakley and Russell, 1976) and according to Begg et al (1983) the basic associative phenomena may be studied in the mammalian spinal cord. Yet the subtitle and the first page of Pavlov's own summarising work indicate that his goal was the understanding of the "highest nervous activity" of the cerebral hemispheres, and several chapters of it are devoted to systematic work on effects of ablating sensory cortex on reactions to complex stimuli (Pavlov, 1927). Similarly many of the phenomena of Skinnerian conditioning are

obtainable in decorticate (Oakley, 1979, 1983) or even decerebrate (Heaton et al, 1981) higher vertebrates, though Skinner himself believed that they were apparent in human language.

Using Macphail's strategy, one could thus argue convincingly for a "null hypothesis" of brain function - there are neither quantitative or qualitative intellectual differences between a spinal cord and an intact brain, since both are dominated by associative learning, and apparent additional behavioural subtleties beyond spinal reflexes are undoubtedly related to the factor of complexity in the perceptual and motor systems, and are therefore irrelevant! The obvious alternative is to focus on perceptual and motor complexity as a dimension of species specialization, as opposed to the bare capacity for reflexive associations. In his reply, Macphail resists this on the grounds that a specialization such as stereopsis cannot be equated with general intelligence. He is clearly right in so far as it would be pointless to try to construct a single scale of animal cleverness on which moles and bats could be sensibly compared with more visual species. But the absence of a single scale is not the same as the absence of cognitive differences. The importance of ecologically-influenced specializations may indeed render impractical any universal framework for cross species comparisons on "general intelligence", but it is a non-sequitur to draw the secondary conclusions that there are no species-specializations in learning and that intelligent behaviour in nonhuman vertebrates is restricted to association formation. On the contrary one might infer from Macphail's analysis that simple association formation is a given, and intelligent behaviour consists precisely in making use of specialized capacities for perception, memory and action.

The positive side of Macphail's null hypothesis is the idea that invariant environmental imperatives induce compliance with certain universal behavioural laws of association, irrespective of ecological niche. Thus any species needs to reduce initial responsiveness to a repeated irrelevant stimulus and to follow rules of anticipatory response shifts and sensitivity to response consequences which give rise to the success of laboratory associative paradigms. But Macphail (1987, p.655) himself points out that within contemporary learning theory a major question is 'What is the nature of the representation of an event that forms a term in an association?'. Surely the nature of the representations of such events will be a function partly of species-specific perceptual, motor and motivational systems, and thus be a between- species variable? The thrust of modern research is that associations can not be limited to Hull's or Watson's receptor-effector reflexive associations, and must encompass more Tolmanian connections between representations of objects, acts and goals (Dickinson, 1985; Mackintosh, 1983; Walker, 1985, 1987a). But is the frog (or the spinal cord, or *Aplysia californica*) as Tolmanian as the chimpanzee? Despite the problem of negative results, it is not overwhelmingly difficult to collect behavioural evidence for the relative perceptual complexity of stimulus representations, the contingency sensitivity or otherwise of behavioural acts, and the effectiveness of representations of previous and potential events.

There is thus a factor of cognitive complexity in animal behaviour, which may be impossible to use for rank-ordering any given pair of species, but should not therefore be ignored. It is true that evolutionary selection must be for behaviour, rather than any psychological mechanism underlying it, but on the other hand we can be confident that the evolutionary process does not make allowances for "contextual variables", and is appropriately sensitive to negative results. Macphail's position was apparently taken up to refute critics of comparative psychology. But the null hypothesis, by re-introducing Descartes' dichotomy between ourselves and all other species, isolates human cognition

from any biological or evolutionary roots and discourages exploration of relations between human and animal cognition. Developments in neuropsychology, cognitive science and contemporary learning theory suggest that it will be more fruitful to adopt the alternative notion, of a biological intelligence or, rather, biological intelligences, which are explicitly products of functional cognitive requirements. (Clark, 1987; Harnad, 1987; Livingstone and Hubel, 1984; Miyashita & Chang, 1988; Roitblat et al 1984; Walker, 1987b)

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