thought. We believe that we have made some progress. Lombardi, Fachinelli, and Delius (1984; see also Friesel, in preparation; Lombardi et al. 1986) showed that, given propitious circumstances, pigeons can learn to apply an identity/oddity concept to visual shapes and are able to transfer the principle to classify novel visual stimuli. This supports Macphail's (1982) conclusion, based on less satisfactory earlier evidence, that on this basis birds do not differ from primates. Von Fersen (in preparation; see Delius 1987 for a preliminary account) has demonstrated that pigeons can infer transitive relationships between stimuli. Given experience with pairs of stimuli bearing the relations $A \leftarrow B$ and $B \leftarrow C$, in later tests pigeons behave according to the inference $A \leftarrow C$. They are in this respect equivalent to primates (Gillian 1981; McGonigle 1977).

Hollard and Delius (1982; see also Delius & Hollard 1987; Emmerton 1986; Lombardi, in press) found that pigeons are quite capable of solving problems of orientation-invariant pattern recognition. Problems of this kind are frequent nonverbal items in IQ tests for humans. Our results could also be superficially interpreted as generally supporting Macphail's position – assuming that the advantage he concedes to humans is limited to problems in which they can bring to bear linguistic processing. However, the experiments have revealed that pigeons are actually superior to humans in a particular class of orientation-invariance tasks (those that demand mental rotation; Shepard & Cooper 1984). This seems to contravene the spirit of Macphail's proposal. At present we see no alternative than to assume that the pigeons' superior performance on an IQ test item represents an adaptation to a peculiarity in the way of life of birds (Lohmann et al., in press).

Nevertheless, I find nothing wrong with the null hypothesis that there are no differences in intellect among subhuman vertebrates as long as it is only a working hypothesis. I cannot, however, go along with the conclusion that because it has not been so far strongly disproved (and that is already arguable!) "there are neither qualitative nor quantitative intellectual differences among nonhuman vertebrates." That style of argument was many years ago colloquially dubbed the "tobacco company trap" ("cigarettes have not been proven to affect health" therefore "cigarettes do not affect health"), once propagated by some, possibly too well-paid, statisticians. Less well-paid statisticians had much to say at the time on the issue (type II errors and all that). The present-day consensus seems to be that the latter's doubts were quite justified.

I find it instructive to consider briefly a counter-hypothesis. Have comparative psychologists succeeded in proving beyond reasonable doubt that all nonhuman vertebrates are equally intelligent? I would be disappointed if Macphail claimed they had. Thus, despite our own findings I go on assuming, based on good old common sense and much circumstantial evidence, that overall, pigeons are not as intelligent as chimpanzees, but more intelligent than goldfish, at least quantitatively.

There are many more points about which I would like to quarrel. For example, Macphail begins with a broad definition of intelligence, but as the target article proceeds an increasingly meaner definition is surreptitiously introduced but not explicitly stated. One cannot escape the feeling toward the end that as Humpty Dumpty said "when I use a word . . . it just means what I choose it to mean" (Carroll 1871). It is implied, for example, that the fact that humans can learn a language and monkeys cannot reflects a major difference in intelligence. When parrots can learn a fair number of words and pigeons are unable to learn a single one, however, then that apparently signifies no difference at all.

Macphail seems to be naïve about the evidence on genetic variability of individual intelligence. Much has happened in the field of intelligence genetics since the days (and failings) of Burt (1966) and Kamin (1974), Tryon (1940) and Searle (1949), as consulting a recent genetics textbook readily reveals (Vogel and Motulsky 1986). In case there is after all genetic variability,

Clever pigeons and another hypothesis

Juan D. Delius

FG Psychologie, Universität Konstanz, D7750 Konstanz, Federal Republic of Germany

In general terms, Macphail's effort to reestablish the field of comparative psychology of intelligence is most praiseworthy. His book *Brain and Intelligence in Vertebrates* (Macphail 1982) is no doubt a landmark in this previously derelict area. In recent years it has been our own objective, partly inspired by his book, to demonstrate that pigeons are more intelligent than generally

Where is the evidence that pigeons, say, cannot achieve what Savage-Rumbaugh's (1985) chimpanzees achieve?

Finally, I should register my agreement with Barlow's suggestion that language has a function over and above the simple conveying of information. I believe that language does indeed have a dramatic and far-reaching effect on our general intellectual capacity, although precisely how it achieves that effect remains for me obscure.

Perspectives from human intelligence

Attention is drawn by Eysenck, Humphreys, and Hodos to the fact that there are virtually no data on the question of whether performance in one cognitive task correlates with performance in another, and Eysenck and Humphreys argue that, in the absence of such evidence, very little can be said about intelligence in animals. This is an odd conclusion, however, since it would mean that if there were no individual differences in intelligence, intelligence could not be studied scientifically. In fact, as Hodos implies, we can probe the processes that underlie intellectual performance quite well without exploring individual differences. Arguments, for example, about the distinction between short- and long-term memory in humans proceed with very little reference to psychometric data. Similarly, we can reason from phenomena such as overshadowing and blocking to underlying processes, and we can test the validity of those hypothetical processes using other experimental designs. It is the failure to discover phenomena demonstrable in one species which are not demonstrable in another that leads to the inference of similar processes in different species. If different species use the same processes, then those species possess, qualitatively at least, comparable intelligence.

Sternberg takes a distinctly gloomy view of comparative psychology, suggesting that the data available could be taken to support virtually any position. This is surely going too far, since there are many cases in comparative psychology of proposals which have been generally rejected (Macphail 1982). One such example is cited in my target article: Bitterman's (1975) proposal that fish are "pure S-R" animals may now be rejected. The problems are, as Sternberg notes, extremely difficult, but they are not systematically insoluble. The frustration that Sternberg feels arises largely, I believe, from the fact that no convincing demonstrations of differences have emerged; if they *had*, then neither methodology nor theorizing would, I suspect, have attracted such an attack. Comparative psychologists have shown many impressive parallels in vertebrate species across a wide range of phenomena. It is odd that instead of welcoming this positive evidence of important cross-species similarities in learning mechanisms, critics prefer to reject the enterprise because it fails to confirm their predisposition to believe that species differences exist.

Borkowski assumes that I ascribe all variations in human intellectual capacity to variations in the efficiency of a language-acquisition device. Much of his commentary challenges the plausibility of such a view by presenting evidence favoring the notion that language acquisition itself is dependent upon other cognitive skills and structures. I shall not address myself to Borkowski's specific

points, which I am not competent to assess, but I should make clear that I take my interpretation of the nonhuman work as support for the view that measured variations in human intelligence may be due to the operation of environmental factors and to innate variations in noncognitive factors such as motivation (e.g., Macphail 1985b; see also EDITORIAL COMMENTARY).

Nagoshi draws attention to the parallel between the problems facing the comparative psychologist and those facing the cross-cultural psychologist. Although Eysenck, Humphreys, and Sternberg have implied that the comparative psychology of intelligence is in a sorry state compared to the situation in human intelligence, Nagoshi's commentary suggests that comparative psychologists are not alone in their difficulties: It is apparently not yet clear how to decide whether there exist quantitative differences in intelligence between humans of different cultures, and for reasons similar to those which apply in comparative psychology.

Invertebrate intelligence

Regret is expressed by both Griffin and Kupfermann that I restrict my discussion to vertebrates, a regret which I share and which was caused at least in part by limitations of space. I have made clear elsewhere (Macphail 1985c), I am much impressed by the parallels between the phenomena of learning obtained in invertebrates and those known in vertebrates, and see them as evidence for a marked degree of commonality in learning mechanisms between vertebrates and invertebrates. On the other hand, it is surely still the case that we do not yet have enough data to extend the null hypothesis with any confidence to invertebrates. I would argue, however, that the null hypothesis is in a sense the standard against which comparative studies on intelligence in invertebrates should be measured. The null hypothesis for such research, then, must surely be that invertebrate intelligence is no different from vertebrate intelligence. There are at present no data of which I am aware which compel us to reject that hypothesis – but the fact is that we know very little about invertebrates.

Finally, it is reassuring to see Griffin making the point that the assumption that cognitive processes are closely correlated to gross morphology is an outdated one; it provides a nice contrast to Elepfandt's claim that it is old-fashioned to see demonstrations of similar phenomena as evidence for the existence of comparable processes.

Labeling, association, and commensurability

The EDITORIAL COMMENTARY raises two empirical issues. The first of these points to a problem for those who believe that language is not an exclusively human achievement. I am in general sympathy with the point, although I would contest the claim that between-species differences exist in the ability to label objects, and I do not accept that the sign-strings generated (or responded to) in the language-learning programs to date *have* demonstrated sentence formation (or comprehension). I do believe, however, that if nonhumans were capable of sentence formation then they would inevitably become capable of communicating very much more interesting

Macphail seems to have a ready backup argument: since the laws of causality are the same for all nonhuman vertebrates (indeed for all organisms), convergent selection pressure has seen to it that all are equally good at detecting them. Can this be true when vertebrate nervous systems vary in computing power over a factor of at least 10,000 in terms of processing units, and when causality tangles vary in complexity between those involved in, say, a fall from a rotten branch and a fall from power in the course of a lengthy intrigue among peers?

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