of an ORE. In addition to the problems already under consideration in the ORE area (Mackintosh, 1965; Paul, 1965; Sperling, 1965a, 1965b; Theios & Blosser, 1965), it is likely that an additional one, that of phylogenetic level, will also require attention.

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Notes

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Comment on Myers et al

by Stanley Wechkin and Gene P. Sackett

Myers, Horel, & Pennypacker (1965) claim to have established operant control over a vocal response (VR) in *Cebus albifrons*. Unfortunately, it is not clear from their report that the VR functioned as an operant, i.e., as an instrumental response, rather than as a respondent. If the VR is one which would ordinarily be elicited by the presence or anticipation of the US, that is, if the VR were a food call, then the association of the VR with the S^D would be a case of classical conditioning. Unfortunately, we know of no published report in which the food call of this species is adequately described. However, the VR used by Myers et al, namely, one with a frequency of 1000-3000 cps and a duration greater than 0.1 sec. is almost identical to a food call of the rhesus (Rowell & Hinde, 1962) and is probably the same as the food call described by Van Hooff (1962) for a large number of species, including *Cebus albifrons*. It has been frequently observed in zoos and laboratories that this type of VR becomes associated with a particular stimulus, e.g., the appearance of the caretaker who does the feeding. However, to call any response associated with a stimulus an operant is to make the term operant so broad as to deprive it of distinctiveness.

This is not to deny that a primate vocalization can come under operant control. However, even if the authors had unequivocally demonstrated this, their conclusion that "the vocal responses of nonhuman animals and humans seem to be acquired and maintained in basically the same way" would still not follow. The proposition that a response can come under operant control is not identical to one stating that the presence of the response in a larger population is also predicated on operating conditioning. Thus for example, Brener (1965) has recently shown that heart rate can come under operant control, but this is very far from saying that heart rate is a behavior acquired through shaping, selective reinforcement, etc.

In the Wisconsin Primate Laboratory monkeys have been reared in complete isolation, where they have no physical, visual, or auditory contact with other species members, and hence where reinforcement contingencies for vocalizations were absent. These animals do vocalize. Observations during the isolation period by one of us (GPS) indicate that isolates produce essentially the same sounds as non-isolates, although with a lower frequency of occurrence. This suggests that few, if any, of the vocalizations of laboratory-reared rhesus monkeys are *acquired* through conditioning. Furthermore, when isolates were later tested for social behavior some of their vocalizations were "socially appropriate," e.g., cooing under stress, and barking and screeching during aggression. This implies that at least some vocalizations are innate, and depend upon nothing more than appropriate releasing stimuli for evocation and maintenance. Thus, while it may be true that many vocalizations of monkeys can be brought under operant control, this alone does not prove that operant conditioning is either necessary or sufficient for normal development of nonhuman primate vocalization.

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For reply by Pennypacker, Horel and Myers see page 254.