A supplementary note on the feeding behavior of the lizard *Anolis carolinensis*

GORDON M. BURGHARDT

University of Tennessee, Knoxville, Tenn. 37916

Recently this journal published a paper entitled, "Effects of prey movement and background on predatory behavior of chameleons," by Askew et al (1970). Several aspects of this paper are open to critical scrutiny.

The report opens with the assertion that "An examination of the literature indicates that very little has been done on feeding behavior in reptiles." No references are given to support this exceedingly general statement. Moreover, in the entire article only one possibly relevant citation is made—and that to a very broad secondary source. The statement is, in fact, untrue. A considerable literature, particularly in biological journals, exists on feeding in reptiles; a most cursory examination of *Biological Abstracts* or *Zoological Abstracts* would have uncovered some of this. Even a semipopular book such as Oliver (1955) contains a wealth of information. Although some of this information is not experimental, it can be ignored by the experimentalist only to his own detriment, as the ethologists have been telling comparative psychologists for years. There actually are a considerable number of published experimental studies, going back 50 years or more, too numerous to be listed here. At least three have been published in *Psychonomic Science* itself (Burghardt 1966, 1967; Morris & Loop, 1969). Although the literature on reptiles is less voluminous when compared to that of other vertebrate groups, such as birds and mammals, the blanket opening statement by Askew et al is clearly not warranted.

Let us examine the specific problem looked at by Askew et al. The standard herpetological literature contains a paper entitled, "Effects of prey size and movement on the feeding behavior of the lizards *Anolis carolinensis* and *Eumeces fasciatus*" (Burghardt, 1964). This study utilized the same predator lizard species as did Askew et al (which they improperly identified in their title), and the same prey species, mealworms (*Tenebrio molitor* larvae). The same major result is reported in both papers: movement of the prey was of primary importance. In addition, this writer

showed that size was also a factor, whereas Askew et al demonstrated that the background substrate was an additional minor factor. The generality of the importance of movement is attested to by the fact that I used both isolated and grouped Ss in a simultaneous choice situation, while Askew et al used grouped Ss and a successive discrimination procedure.

There are several other methodological differences between the studies, but one more needs to be discussed here. Askew et al produced dead nonmoving mealworms by freezing them and thawing them 24 h later. In planning my study, I first tried this seemingly efficient method. However, I discarded it in favor of a water injection procedure for two reasons. First, 24 h of freezing frequently did not kill the worm; when it thawed, it mischievously began to twitch and then to crawl around. Although this could easily be solved by freezing at lower temperatures or for longer periods, a second more serious problem developed. The frozen mealworms often turned dark rather quickly upon being thawed and exposed to air. This produced a clear visual difference which could very well have an effect

on the lizard's visual response. Interestingly enough, this phenomenon did not seem to have been experienced by Askew et al; they specifically state that their "freezing procedure did not appear to the E's to alter the physical appearance of the worms significantly." A possible reason for this discrepancy is not immediately evident.

This note is not a criticism of the experiment itself; after all, it repeats my own results. Nor do I want to discourage workers from studying reptile behavior. I am convinced, however, that science is best served when investigators are aware of the historical background and literature of the problem area they select and when they possess a healthy regard for the possible relevance of related disciplines. In this era of expanding information and specialization, scholarship becomes increasingly indispensable.

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Reply to Burghardt

HENRY R. ASKEW and MARILYN MUSIMECI
Fordham University, Bronx, N.Y. 10458

We would like to reply to Burghardt's (1971) criticisms of our *Psychonomic Science* article (Askew, Musimeci, Sloane, & Stephan, 1970). First, we would like to admit our error in overlooking Burghardt's (1964) study, which also dealt with feeding behavior in *Anolis carolinensis*. We want to point out, however, that not only did the two experiments differ in major ways procedurally and in terms of a second independent variable investigated, but our paper reported some incidental observations which were made, dealing with a possible interaction between dominance and feeding behavior, which may be the most interesting aspect of the paper in terms of suggesting a direction for further study.

Secondly, Burghardt has taken strong exception to our statement concerning the state of the literature on feeding behavior in reptiles. It is understandable how a worker who has done several studies in this area may disagree with our characterization, but when the area is objectively viewed from the perspective of researchers interested in entering a new problem area, the impression that very little has been done appears accurate and is certainly not "untrue" and "not warranted," as Burghardt argues. Certainly, experimental studies have been done, but the work appears scattered and to have advanced little beyond the preliminary exploration of a few major factors which contribute to feeding behavior. We did not say,