It is clear from these data that the structure of contextual stimulation may influence the perception of focal stimuli. The enhancement of loudness with the simultaneous presentation of the 15-w flash is consistent with an earlier study by Child & Wendt (1938) and a recent report of heteromodal anchor effects by Behar & Bevan (1960).

References

- ALLPORT, F. H. Theories of perception and the concept of structure. New York: Wiley, 1955.
- BEHAR, I., & BEVAN, W. The perceived duration of auditory and visual intervals: Cross-modal comparison and interaction. *Amer. J. Psychol.*, 1961, 74, 17-26.

- BEXTON, W. H., HERON, W., & SCOTT, T. H. Effects of decreased variation in the sensory environment. *Canad. J. Psychol.*, 1954, 8, 70-76.
- CHILD, I., & WENDT, G. R. The temporal course of the influence of visual stimulation upon the auditory threshold. *J. exp. Psychol.*, 1938, 23, 109-127.
- HEBB, D. O. The mammal and his environment. *Amer. J. Psychiat.*, 1955, 111, 826-831.

Note

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Comment

Bitterman's reaction (*Psychon. Sci.*, 1964, 1, 94) to my comment (*Psychon. Sci.*, 1964, 1, 40) on the Longo, Klempay and Bitterman article (*Psychon. Sci.*, 1964, 1, 19-20) indicates that I failed to make clear both my reason for writing the comment and the essential message in the comment, itself. Hence this brief addendum.

In spite of what Bitterman seems to perceive as my primary concern, I was *not* worried about "the discrepancy between the pigeon results and the eyelid results" pertaining to the CS-UCS interval and said absolutely *nothing* that reasonably leads to such a conclusion. Facts are facts and, for sometime, it has been perfectly clear to me that no single function of this type applies generally to all species or to all response systems.

What I was concerned about is the idea that the pro-

cedural distinction between classical and instrumental conditioning is enough to identify a distinction at the level of process. The definition of classical conditioning solely in terms of non-contingent pairings of CS and UCS is like the definition of "fish" solely in terms of aquatic habitat. All I was trying to do in my comment was to prevent the premature exclusion of other distinguishing criteria that may make a whale of a difference.

Presently I believe there is evidence to justify the conclusion that classical conditioning applies only to elicited, involuntary behavior. As Bitterman points out, the independent definition of what is voluntary and what is involuntary is extremely difficult. But difficult is not impossible, and the fact that ordinary people sometimes talk this way does not seem to be a crushing argument against the use of this terminology.

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