different dependent variable. He found that when Ss were given information about two individuals the difference between the similarity attributed to similar and dissimilar strangers was greater for high than for low valence topics (determined by the Allport-Vernon Scale of Values).

Each of the suggested alternative possibilities is now under investigation in work in progress.

References

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Comment

In a recent experiment on "Classical appetitive conditioning in the pigeon," a CS was paired with food, and the anticipatory activity which it came in consequence to evoke was measured (Longo, Klempay, & Bitterman, *Psychon. Sci.*, 1964, 1, 19-20). The results obtained are quite different from those of human conditioning experiments on which Kimble has depended primarily for information about the role of the CS-US interval (Conditioning and learning, Appleton-Century-Crofts, 1961, pp. 155-160), and he sets out to define the discrepancy away (Comment, *Psychon. Sci.*, 1964, 1, 40). Conditioning is not *classical* "just because there is a non-contingent relationship between CR and reinforcement," he asserts; the CR must be a response "of the sort which Skinner called elicited and the layman calls involuntary." If the pigeon's response to the CS was not "elicited" or "involuntary," then the conditioning was "instrumental" rather than "classical," and if "experiments on instrumental conditioning" have no "relevance to the CS-US interval function," then the contradiction between the pigeon data and the human data disappears. Kimble concludes with an appeal for "caution" in the classification of conditioning experiments.

Now *classical conditioning* is a much-abused term which has carried so many meanings that we might do well, as I have suggested elsewhere, to give it up altogether (Techniques for the study of learning in animals: Analysis and classification, *Psychol. Bull.*, 1962, 59, 81-93). There can be little doubt, however, that the usage which Kimble criticizes is the modal one. About the usage which Kimble recommends I shall say only that it does not seem feasible without continuous recourse to the advice of Skinner or of a consulting "layman," on which one might well be reluctant to rely. Just how one decides whether a response has been "elicited" by a stimulus or "emitted" in its presence, I have never been able to understand. As to "the voluntary-involuntary dichotomy," Kimble himself has recognized that it is "difficult to support on objective grounds" (Conditioning and learn-

NEWCOMB, T. M. The acquaintance process. New York: Holt, Rinehart, and Winston, 1961.

SMITH, A. J. Perceived similarity and the projection of similarity: the influence of valence. *J. abn. soc. Psychol.*, 1958, 57, 376-379.

Note

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ing, p. 100). It does not reduce to a skeletal-autonomic dichotomy in Kimble's view, because 'there are some nonautonomic responses which can be conditioned classically' (ibid, p. 100).

Whatever the fate of the term *classical conditioning*, it seems rather obvious to me that our terminology must distinguish clearly between the procedures we employ in learning experiments and the processes we invent to account for their results. Under certain circumstances, S_2 follows S_1 independently of the animal's response to S_1 ; under other circumstances, S_2 follows S_1 only if the animal makes some specified response to S_1 ; under still other circumstances, S_2 follows S_1 only if the animal *fails* to make some specified response to S_1 . Having defined these different procedures, we may then ask about the similarities and differences in the processes responsible for the changes in behavior which they produce. The tendency to blur the distinction between procedure and process which is evident in Kimble's treatment (Conditioning and learning, pp. 78-108) seems to me to lead only to confusion.

The problem posed by the discrepancy between the pigeon results and the human results is an empirical rather than a definitional one; aside from the difference in species, there are a number of differences in method which may be responsible for the difference in outcome and whose significance should be assessed experimentally. Whether there is any relation between CS-US interval functions plotted by instrumental and by classical procedures also is an empirical problem, on which, incidentally, there has been some instructive recent work. The results suggest a rather close relation: From the function plotted for classical aversive conditioning in the goldfish, it is possible to deduce with considerable accuracy the effect of CS-US interval on this animal's performance in the shuttlebox (Bitterman, Classical conditioning in the goldfish as a function of the CS-US interval, *J. comp. physiol. Psychol.*, in press).

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