Meehl, P.E. & MacCorquodale, K. (1951). Some methodological comments concerning expetancy theory. *Psychological Review*, *58*, 230-23

#022

# SOME METHODOLOGICAL COMMENTS CONCERNING EXPECTANCY THEORY [1]

PAUL E. MEEHL AND KENNETH MACCORQUODALE

*University of Minnesota*

In attempting to formulate axioms for Tolman's "expectancy" theory we came to certain methodological conclusions which we shall present briefly in the present paper. Space does not permit us to present the twelve postulates thus far developed, nor can we review here the experimental facts. It is our opinion, however, that the recent published and unpublished research is sufficiently favorable toward an expectancy theory to warrant systematic work on its more rigorous formulation.

We early concluded that certain views which have been linked historically to Tolman's formulation are logically unrelated to the "core" concepts of an expectancy theory. Other views seemed related to Tolman's formulation until preliminary efforts to formalize them indicated their independent status as well. We shall merely list those dogmatically as "properties *not* definitive of an expectancy theory":[2]

1. Gestalt-configural stress.
2. Perceptual field stress.
3. Pure contiguity as a sufficient condition.
4. Specification of reaction-class by reference to position, direction, or locomotion (rather than by effector properties)
5. Discontinuity view of discrimination learning.
6. Insistence upon the learning-performance distinction.

What then *is* the "core" concept definitive of an expectancy theory?

According to both S–R theory and to Tolman, the empirical probability of R in the presence of S can be increased in several ways. That more than one way exists is what generates the learning-performance distinction. Thus, without having interpolated further exposures to S, we can raise the strength of R in its

1 This is a revision, with very minor changes, of a paper read as part of the Report from the Conference on Behavior Theory (Dartmouth Conference) at the 1950 meeting of the American Psychological Association at State College, Pennsylvania. The other members of the Symposium and Conference are in no way to be considered as endorsing the position presented.

2 Detailed justification for rejecting these as "essential" features of an expectancy theory will appear in a joint volume to be published by the Dartmouth Conference. [See references at end of this article.]

presence by starving the rat. But the important point is that operations of this sort are conceived of as acting to change the relation between a theoretically introduced bond sHr and empirical strength. If we examine the manner in which this theoretical bond sHr is *itself* supposed to be strengthened, the three major S–R systems allow for only one kind of operation: *the running off of the sequence S-then-R* (or some sequence having sufficient similarity to it to yield generalization and induction effects). The question regarding the *sufficient* conditions for strengthening this sequence, *e.g.,* drive-reduction, is not relevant here. The mere occurrence of the sequence S-then-R may not be sufficient, but all three major S–R systems (Guthrie, Hull, Skinner) make it necessary.

If a postulate is included which allows the theoretical sHr bond to be strengthened by some operation *not* involving the occurrence of the sequence S-then-R (or topographically similar ones), such a single postulate should have striking and diffuse consequences. If we draw up a table of the various experimental designs possible in the single-unit T-maze, and examine those designs in which "positive" outcomes are *prima facie* embarrassing to non-Tolmanites (*i.e.,* whether they hold in common stimulus-reinforcement, drive-reduction, or simple contiguity views), we find that such designs have a common property. This property, which is not found in the designs easily assimilated by S–R theory, is that *during the acquisition phase no increase in the* (subsequently-to-be-manifested) *preference can be observed.* Whenever such an increase is observed during the so-called "latent" phase, the experiment is not disturbing to an S–R theorist. Now if we ask why Tolman is happy with positive outcomes in such designs, we find that he can assume an increase in the sHr bond by an operation *other* than the running off of the S-then-R sequence. Because this operation is sometimes taken to be essentially the juxtaposition of two S's, there *is* a certain sense in which Tolman has an "S–S" theory (although the two S's are not necessarily the ones Tolman usually mentions).

Let us suppose, to take a concrete example, that a food-satiated rat runs a T-maze with symmetrical social incentives. No differential strength is observed. Then the hungry rat is *placed* (not having run) in one end-box and allowed to eat. "Appropriate" choice on a subsequent run is *prima facie* embarrassing to non-expectancy theories. It appears that the feeding experience was the operation that strengthened one S.R bond over the other; yet this operation does not involve the occurrence of the sequence S-then-R. This seems to us the real reason that it is embarrassing to non-expectancy theories.

That this situation is what is at stake is further indicated by the way a non-Tolmanite is likely to avoid the embarrassment. He invokes a fractional goal response. That is, he speaks of an accumulation of strength in some bond S.R′ which *does* go an during the latent phase; but the response term R′ is either internal or minimal and hence not noticed by ordinary methods.

Tolman here says what the layman would say: "The feeding experience taught the rat that food was to be found in a certain box, and the latent phase had taught him how to get to that box. So now he tends to go there." Here the strength of sHr is said to increase via the occurrence of two S's, without the occurrence of the sequence S-then-R. Let us call the neutral goal-box stimulation $S_2$, the "usual terminator" of the sequence S-then-R. The Tolman postulate would seem to involve the assumption that the strength of the S.R bond can be increased *by bringing its usual terminator* $S_2$ into conjunction with a reinforcer (food, say S*). But this reference to the "usual terminator" can only be understood by thinking of something which the "usual termin*ation*" has set up in the rat. So it seems appropriate to include the usual terminator in one's notation, writing it as "part of" the construct being strengthened during the latent phase. Hence we think in terms of a 3-term designation of the construct, including reference to the eliciting stimulus, the response, and the terminating stimulus. This peculiar "habit" ($S_1R_1S_2$) may be called an "expectancy"; postulates regarding its acquisition, extinction, generalization and "activation" of reaction-potentials are then in order.

Having defined "an expectancy ($S_1R_1S_2$)" implicitly by several postulates which we cannot present here,[3] an expectancy theory seems to involve what might be called a *Postulate of Inference,* as follows:

> The occurrence of a temporal contiguity $S_2S^*$ when the expectancy ($S_1R_1S_2$) has non-zero strength, produces an increment in the strength of a new expectancy ($S_1R_1S^*$). The induced strength increases as a decelerated function of the number of such contiguities. The asymptote is the strength of the inducing expectancy ($S_1R_1S_2$) and the growth rate is a decelerated increasing function of the absolute valence of S*.

Let us suppose that the experimental facts seem to require such a principle. Does its presence define an expectancy theory? No, because a non-expectancy theorist may be able to derive it as a theorem if suitable changes are made in the formulation of the other postulates as well. The use of $r_g$ by Hullians is, of course, the major case of such a claim. Two comments are in order concerning $r_g$ as a mediator of "inference." First, the derivations tend to be made for special setups, rather than as a general "theorem" to be filled in. Secondly, $r_g$ at present is so poorly quantified a construct that it is as readily available a *deus ex machina* for non-expectancy theorists as the concepts of "attention," "emphasis," or "perceptual threshold" are for Tolman. It is not possible at

3 The provisional set of postulates is available from the authors upon request.

present to foresee whether the theorem can be derived and can have the combination of quantitative properties in $r_g$ needed to mediate all of the deductions.

Suppose an inference-postulate is derivable as a theorem from "non-expectancy" principles. What non-verbal difference, if any, remains between Tolman and Hull? We believe this question hinges upon the way the word "response" is used. If this word means an *effector*-event, as it originally did to behaviorists, a Tolmanite creates a non-verbal difference by making $r_g$ "central." For this reason we did not list the centralist-peripheralist issue among the pseudo- or non-defining properties of an expectancy theory. Suppose Tolman asserts that the locus of $r_g$ or, better, of the expectancy which plays the same formal rôle as $r_g$ is "in the brain." There are three main reactions to such a claim:

1. It is not part of theory.
2. It is part of theory but I am not interested in it.
3. It is an interesting part of theory but it is false (peripheralism).

There can be no doubt as to the empirical meaningfulness of the centralist thesis. "Where in physical space-time, in what part or phase of the activities occurring within the rat, do the physiological events occur which play the formal rôle in the causal system specified by the sentences of the theory?" One may not care to discuss this thesis, but it is certainly not a transcendental or "meaningless" question. There are respectable sciences of anatomy and physiology which have these events as their subject-matter; and no one but a dualist doubts that these events are the immediate determiners of the movements of an organism. This being the case, it is hard to understand why such a locus-claim is "not *really* part of the theory." A man's theory about any region of inquiry consists of all the empirically meaningful statements that he makes regarding that region. To say that "$r_g$ develops as a growth-function" is theory, whereas to say that "$r_g$ occurs in the effectors" is not theory, seems to us wholly arbitrary. Nor can such a distinction be defended by the methodological dictum that "If two theories have the same consequences they are indistinguishable." The joker here is the phrase "same consequences." That the consequences must be somehow *empirical* is merely the mark of science; but that they must be *behavioral* is a currently fashionable decision. It may be a good one; but it is only a decision nonetheless!

This brings us to the second attitude, "It's theory but I am not interested in it." Since this is a statement of values, one cannot quarrel with it. But such a value-judgment cannot, by the same token, be imposed on others, *e.g.,* by informing them that the centralist thesis is not part of theory. It has been said that the only reason for constructs is to enable us to "predict behavior." We frankly find it difficult to conceive that people are mainly interested in doing just this. Does anyone but a naturalist have an intrinsic interest in the movements of a rat? We are not erecting theory in order that we may guess whether a rat will turn left or right; on the contrary, we watch which way he turns with bated breath because we wish to confirm our guesses as to the theory. Now this is autobiographical, and others may not share our intrinsic curiosity about the inner events. But those who do not share that curiosity cannot prohibit it in those who do. If a centralist insists that he studies the rat's movements as a *means* to an *end*—to get leads on the character and locus of the inner events—make no mistake about it; he is expressing a very respectable kind of scientific interest. It remains to be seen how far a "purely behavioral" research program can carry him in satisfying such an interest. Let us stress very strongly that *we* have no bets here, and do not intend to desert the T-maze and the Skinner-box for the scalpel. But we do wish to emphasize that any tendency to relegate a "centralism" thesis to the metaphysical limbo upon general *methodological* grounds is doctrinaire. "Purely molar behaviorism" has proved amazingly powerful when expressed as a research preference; but it has no status whatever if treated as if it were a thesis of general epistemology.

Whether the time is ripe for systematic efforts at coördination of molar-behavior constructs with localized inner-events is always problematic. All we can do is to examine critically the evidence for proposed identifications. Whether or not its major contentions are correct, a book such as Hebb's *Organization of Behavior* cannot be dismissed as "irrelevant." It may be tentative, but its neurologizing is not of the bootless and irresponsible sort which we all condemn. Whether it is "premature" remains to be seen. It is our belief that even a purely "molar" research attack is capable of confirming some locus-statements, but not to as high a degree as one could wish. It is, of course, quite possible for a determinedly "molar" behaviorist to ask intelligible questions which he cannot answer (*i.e.*, resolve with any confidence by molar methods). But how far he can go is for future research to decide, and it would be foolish for us to venture any prophecies.

---

Subsequent articles on expectancy theory by Meehl and MacCorquodale:

MacCorquodale, K., & Meehl, P. E. (1953). Preliminary suggestions as to a formalization of expectancy theory. *Psychological Review, 60*, 55-63.

MacCorquodale, K., & Meehl, P. E. (1954). E. C. Tolman. In W. K. Estes, S. Koch, K. MacCorquodale, P. E. Meehl, C. G. Mueller, W. N. Schoenfeld, & W. S. Verplanck, *Modern learning theory* (pp. 177-266). New York: Appleton-Century Crofts.

pdf by LJY, November 2021