# Reexamination of the Biochemical Transfer of Relational Learning

Braud and Braud (*1*) reported successful biochemical transfer of the transposition effect in rats. Donor animals were trained to approach the larger of two circles for liquid reinforcement, and were then killed. Recipients of brain extract from these donor rats were then presented with the original large circle and a new, even larger, circle. These recipient animals approached the larger circle significantly more often than control rats who had received placebo brain extract from untrained donors. The authors interpreted these findings as evidence of a biochemical substrate for relational learning, that is, choosing the larger of two stimuli appeared to be transferred by the brain extract to naive organisms.

This report makes clear that something is apparently being transferred which produces a preference for larger circles. It is not clear, however, that the experiment reported by the Brauds supports their contention that a chemical substrate that specifically predisposes recipient rats to transpositional behaviors has been transferred. There are two major problems with this experiment.

1) The study does not show transfer of true transpositional behavior. In the typical relational learning experiment, subjects would be trained to (for example) approach the larger of two stimuli, and then tested with the original large stimulus and a new larger stimulus. In the study by Braud and Braud, donor rats received first-phase training only, while recipient rats received second-phase testing only. Thus, neither group of rats was required to manifest all the behaviors usually required to define the transposition effect, and neither group may, in fact, have been responding relationally. The study, then, only demonstrates that recipient rats preferred larger circles.

2) In considering the effects of training versus those of no training, they failed to take into account, in both of their experiments, the following variables: (i) the amount of general activity between the two donor groups differed—the experimental group did run in a Y-maze, while control rats sat in home cages; (ii) sensory experience with geometric forms was left uncontrolled—presumably, the control animals did not encounter the patterned visual stimulation that was provided to the experimental animals; (iii) the control rats received no experience in a learning task per se, for example, "control training" in a nontranspositional discrimination task; (iv) the amount of handling by experimenters in conjunction with each group's regimen was not equated; and (v) the study should have contained groups trained to approach the smaller circle, and then recipients tested on the smaller of a pair of stimuli. Discrimination learning studies routinely employ this procedure, irrespective of demonstrations of "no preference" before administration of the treatment variable.

Variables comparable to these have been shown to be major determiners of performance in other biochemical transfer investigations. In a study of memory transfer in planaria, Hartry *et al.* (*2*) found that recipients of worms that had received photic stimuli, or mere handling, were conditioned as fast as or faster than recipients of trained worms, and faster than recipients of worms in other control conditions. Their conclusion, that transfer materials may produce sensitization to particular stimuli, rather than provide the recipient with already formed associative tendencies, seems applicable to the study under discussion here.

Accordingly, one possible alternative explanation for the results reported by Braud and Braud (*1*) is that one or more of the uncontrolled variables identified above may have produced a biochemical substrate that sensitized rats to a variety of visual stimuli, and that the data reflect exploratory behaviors motivated by the greater intrusiveness of larger circles. Given these problems in interpretation, control procedures in experiments such as this must routinely match groups for degree and kind of activity, handling, and general stimulation, in order to maintain that the transference of some learned behavior is uniquely associated with the reference learning experience in donor organisms.

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Yaremko and Hillix have criticized our study of biochemical transfer of relational responding (*1*) on two counts. First, they argued that our study did not show "true transpositional behavior." They correctly note that in our study, donor rats received first-phase training only, and recipient rats received second-phase testing only. They state further that neither group of rats was required to manifest all the behaviors usually required to define the transposition effect and that neither group may, in fact, have been responding relationally. In answer to this argument, we reply that our donor rats received first-phase training only, and our recipient rats received second-phase testing only, because this was the only meaningful way to conduct our experiment. We could not have tested the donors transpositionally (that is, exposed them to the original large stimulus and a new larger stimulus) before killing them. If we had done that, it could have been argued that any effect obtained in the recipients could be a "direct transfer" of such second-phase learning or activity, rather than an effect attributable to first-phase donor training. It was this latter effect that we intended to, and did in fact, show. We demonstrated that recipient rats would perform appropriately, not only under conditions identical to those under which donors had been trained, but also under the slightly altered new conditions demanded by the transposition experiment. Also, testing the recipients under first-phase conditions (with the original donor stimuli) first (i) would have had no bearing on a transposition effect, and (ii) might actually have interfered with the recipients' subsequent relational responding to the new stimuli. A preference of the recipients for the first-phase larger stimulus would simply suggest transfer of the very behavior for which the donors had been directly trained. Furthermore, it has been shown many times (*2*) that the biochemical transfer effect as shown in recipient animals is dependent on time and experience; thus, by the time the recipients could have been retested in the second-phase paradigm, the effect already might have dissipated due to either the passage of time or to an extinction effect occurring as a result of testing the recipients repeatedly without reinforcement. The addition of reinforcement would prevent extinction, but would open the study to the criticism that our extracts merely facilitated learning. We avoided this criticism by our use of nonreinforced test trials rather than of a learning paradigm. It is surprising that such a "general facilitation of learning" criticism is still voiced, in view of the existence of evidence to the contrary (*3*). Even if we had tested our recipients twice, first with the original stimuli and then with the new transposition stimuli, Yaremko and Hillix could still have argued that our recipients simply preferred large circles, and this time twice instead of only once. One could in fact argue that the subjects of any "larger-than" transposition experiment simply prefer larger stimuli, but isn't that what "larger-than" transposition experiments are all about?

In their second criticism, Yaremko and Hillix argue that our transfer effect may have been due to factors, other than learning itself, to which the donors were exposed. This is the "sensitization" argument, which they support by quoting a single study of planaria. Yaremko and Hillix do not mention that many reliable transfer effects have been demonstrated since that study in 1964, some involving considerable specificity (*4*), in spite of excellent controls of confounding variables such as differential handling, activity, sensory experience, and so forth. In response to data from recent well-controlled experiments, proponents of "mere sensitization" are being forced to posit "differential sensitization" or "specific sensitization"—other words for learning or acquired information, which appears to be transferred after all.

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## Food Habits of Early Man: Balance between Hunting and Gathering

Leopold and Ardrey (*1*) have argued that (i) there is a wide range of toxic or poisonous materials present in plants, (ii) the major means by which man eliminates these materials is by cooking, (iii) the regular, controlled use of fire is relatively late in human evolution (about 40,000 to 50,000 years old), and (iv) anthropologists have overestimated the importance of vegetable foods in the dietaries of early human societies.

There are many objections to both the substance and the logic of this argument. For example, although the authors present a long list of the variety of toxic effects animals experience when eating certain plants, they give no sense of (i) how *statistically* widespread these actions are either in any complete range of plants which people are known to eat, or in the array of plants present in any particular environment; (ii) how much the plant kingdom of 40,000 years ago may have resembled that of today; (iii) the relative importance of toxicity according to the parts of plants which people ordinarily consume (berries, nuts, seeds, and fleshy roots are all usually more important as food than are leaves); (iv) the extent to which "emergency foods" could be regularly eaten (we know that people are generally particular about what they eat, and that a much larger inventory of edible, but uneaten foods is available to any human group); (v) the variety of methods other than cooking that exist for removing toxicity, such as drying, soaking, pressing, and leaching; and, most important, (vi) the relative importance of cooked compared to uncooked vegetable foods in the diets of present-day primitive people and whether, in any case, cooking is necessary or only desirable for some other reason (such as "palatability," or the ease with which skin can be peeled from a tuber).

I do not know to what extent the food habits of modern "hunter-gatherers" can be used to help reconstruct the subsistence pattern of preagricultural societies. Leopold and Ardrey make only one reference to this subject. R. Lee informs me that the !Kung Bushmen of the Kalahari desert, the one group to which the authors refer, eat more than 50 percent of their vegetable foods in the uncooked state (*2*). The Gadio people of New Guinea, with whom I have worked, depend to a significant extent on the wild plant foods of the sort Leopold and Ardrey consider. About 8 percent (by weight) of the vegetable food portion of their diet is from wild plants (excluding processed sago flour, which accounts for 22 percent); garden food constitutes 65 percent and hunted animal food 4 percent. While the majority of these wild plant foods are cooked, this has nothing to do with removing "toxic properties," for, with one exception, they possess none (most of the leafy greens, barks, fern fronds, and fruits are also occasionally eaten in the raw state).

Although the question of the relative dependence of primitive people on wild plant foods has been only marginally investigated by anthropologists, there are several studies of this subject which the authors have not considered (*3*). In general, however, there are excellent nutritional, ecological, and energetic reasons for contending that plant foods have almost invariably been of much greater quantitative importance in primitive dietaries than have the useful, but in many ways supplementary "fruits of the kill."

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While Dornstreich says that he finds "many objections to both the substance and the logic" of our assertions regarding the limitations of vegetable foods by toxic substances, his list of objec-

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