Response to "Wolof `Magical' Thinking Culture and Conservation Revisited" by Judith T. Irvine

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RESPONSE TO "WOLOF 'MAGICAL' THINKING
Culture and Conservation Revisited"
By Judith T. Irvine

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I should like to reply to Judith Irvine's article in the September 1978 issue (pp. 300-310) in which she revisited my research on the development of conservation among the Wolofs of Senegal. Indeed, Irvine collected new data from unschooled adults in Taiba N'Diaye, the very village which was the source of my original data on conservation development in an unschooled Wolof sample. My motivation to respond is severalfold. First, I would like to dispute Irvine's conclusion "that cultural conventions governing the organization of talk are more likely to explain the responses of unschooled Wolof than is 'magical thinking'" (p. 300). Second, in so doing, I would like to clarify the nature of the data and procedures on which my original chapter (Greenfield, 1966) was based. And, finally, I should like to make the case that Irvine has relied too exclusively on anthropological methodology in her study. Had she tempered her approach with considerations of psychological methodology, notably the concept of replication, her revisitation would have rested on more solid ground and yielded more conclusive evidence.

Irvine's basic notion is that, by modifying the setting of the conservation interview and the way informants are asked to elaborate on their answers, she has obtained different results from mine, revealing unschooled Wolofs to be more competent with conservation judgments than they appeared under the circumstances of my original study.

Let me begin by asserting that Irvine's apparently different results are mainly an artifact of a failure to replicate my pro-

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cedure. Although she intended to make "only small changes in method" (p. 303), one actual change was major, with far-reaching consequences for a comparison of the two studies. In my original study, the evaluation of conservation was based on quantity judgments in two situations: in one, water was transferred from one of two identical "standard" beakers (each containing the same amount of water) into a taller, thinner beaker; in the other, water was divided among six smaller beakers. In the first case, the participant was asked to compare amount of water in the tall, thin beaker with the amount in the remaining "standard"; in the second case, the comparison was with the water in the six small beakers. My criterion for classifying a participant as having demonstrated conservation was a judgment of "same amount" in both parts of the conservation procedure. As I mentioned in my original chapter (p. 247, 253), division of the water into six small beakers created a more difficult conservation test for the unschooled sample than transfer into the tall, thin beaker, Irvine's *only* test. When I went back and tallied the results of the two parts of the original conservation procedure separately, I found that, whereas only 50% of the oldest unschooled group gave conservation judgments on the harder part, 83% did so on the easier one, and results for the middle age group were virtually the same (55% and 85% conservation on the two parts, respectively). Irvine implies that all of the unschooled adults she interviewed give conservation answers by the end of the interview. But these results are not very different from mine, especially considering that Irvine tested adults and also had a very small sample (5). A much larger sample would be required to reliably distinguish my 85% rate of conservation on part 1 of the procedure from a 100% rate.

If Irvine's basic results are not demonstrably different from my own, then what about her criticisms of my interview method and its social context, elements which she says caused my supposed underestimation of Wolof conservation concepts? My response is two-fold. On the one hand, certain differences, like the fact that Irvine had spent more time in the village, do not appear to have made a difference in the results. On the other hand, the wording of my interview was much more accurate than what is implied in Irvine's article. Irvine presents a protocol of a participant in her study who initially says "this one is more," justifying the judgment with a magical action reason ("because you poured it"); however, this person goes on to say, "The glasses are not the same, but the waters are the same" (p. 306). Irvine's main point is that this person would have been judged a nonconserver had the interview stopped sooner or the person chosen not to elaborate his answer. Her implication is that I failed to elicit conservation judgments because respondents did not elaborate. While this point is correct with reference to Irvine's interview, it is not the case for mine. This is because her initial question was linguistically ambiguous, whereas mine was not. After Irvine's respondent states that the "standard" beakers are the same, using two different terms for equal, Irvine transfers the water and asks "And now?"—a question so ambiguous or vague that the respondent is free to focus on glass, water level, quantity, or any attribute he or she pleases in his/her answer. This particular person starts his answer by talking about the glasses, saying they are different; he then switches to talking about water quantity, saying it is the same. It is true my interview was brief; but my basic question was much more specific than Irvine's. In Wolof my questions went as follows:

> Ndah sa verre bi ak suma verre bi nyo yemle ndoh; wala suma verre bi mo upa ndoh; wala sa verre bi mo upa ndoh?

A literal translation into English yields the following:

> Does this glass of yours and this glass of mine have equal water; or does this glass of mine have more water; or does this glass of yours have more water? (p. 232).

This question is specific because all quantity terms—equal, more, less—which could refer ambiguously to either glass, water level, or amount are modified with the word "water" *(ndoh),* thus eliminating all ambiguity according to adult Wolof informants. In fact, my more formal interview was based on linguistic information I elicited in informal situations very similar to those on which Irvine based her results.

Irvine also implies that reticence interfered with my results and cites page 230 of my chapter. But what I actually say there is that Wolof children talked less than American children, *restricting themselves to answering questions.* Indeed, they did answer the conservation questions, including responding to requests for reasons. My discussion of reasoning among unschooled groups would not have been possible had the children not provided reasons for their quantity judgments. Hence, I do not feel that reticence had a general impact on my results. I did, in fact, successfully modify the wording of part of the conservation interview for unschooled subjects (as described on p. 232) in order to elicit reasons from them.

There is yet another piece of evidence indicating that my interview was valid and that my results were not artifactually contaminated either by "cultural conventions concerning the organization of talk" or by the social context of the experiment. In a second condition where the participants, rather than the experimenter, transferred the water themselves in the conservation procedure, I obtained much higher rates of conservation (82%) for the eight- to thirteen-year-olds than with the original procedure (again using the criterion of success at both halves). Moreover, "magical action" reasons were nonexistent under these circumstances. *Yet the interview had not changed.* From this, it follows that the form of the interview could not have been artifactually responsible either for the high rate of nonconservation responding when the experimenter poured or for the "magical action" reasons. Finally, when the experimenter again transferred the water in both parts of the conservation procedure, as a posttest to the "pour yourself" treatment, 100% of the eight- through thirteen-year old group gave conservation responses to both parts of the test. *Under these circumstances, the rate of conservation equalled that found by Irvine with much older subjects; and this rate occurred without any modification of my original interview procedure.*

"Magical action" reasons involved nothing more than reasoning, as though the experimenter was able to change the amount of water through the pouring transformation. That this reasoning was nonexistent when participants poured themselves is further evidence for the reality of such reasoning in my unschooled Wolof sample. Furthermore, the fact that the second part of the procedure (transfer into six small glasses) was more difficult than the first part (transfer into a tall, thin glass) also corroborates an analysis which says that unschooled Wolof participants are led astray by the action components of the transformation. That is, because transfer of water into six small beakers involves six pouring actions, an action-oriented child would have greater opportunity to view the experimenter as "changing" the amount of water in this part of the procedure.

Irvine rightly points out that Wolofs are very concerned with water and its transfer in their everyday life, especially because theirs is an arid climate. She hypothesizes that their everyday experience with water should foster the development of conservation. This reasoning makes sense and may even explain why the removal of the experimenter as an authority figure in my "do-it-yourself" pouring procedure was so effective in eliciting conservation responses. Indeed, Bovet's Algerian data indicate that procedures which do not make contact with everyday experience are not effective in inducing conservation (Bovet, 1974). This implies that the success of the "do-it-yourself" pouring condition as a brief training procedure was based on the close rapport between conservation of liquid quantity and unschooled Wolofs' everyday experiences with water.

My conclusion from all of this is that cross-cultural research requires an integration of anthropological fieldwork methods with methods of psychological experimentation. Neither suffices alone. Irvine used an informal linguistic interview about the meaning of Wolof quantity terms as the context in which to assess conservation concepts. I conducted such interviews, but used them as the basis for developing a formal interview procedure. In this way, my interview had the benefits of the linguistic investigation, but it also had the uniformity required for a psychological experiment. Irvine attempted to use anthropological fieldwork methodology by itself. In so doing, she neglected to conduct a true replication of my study, thus ignoring a canon of psychological methodology and making her interpretation of results quite dubious. This discussion will have served a useful purpose if it demonstrates that it is no longer sufficient for anthropologists to rely exclusively on informal fieldwork methods any more than cross-cultural psychologists can ignore the social, cultural, and linguistic contexts in which their experiments are situated.

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