

RESPONSE TO EGGHART

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Received 25 March 1987, 3 November 1987

ABSTRACT: The mention of my work in a publication by Egghart may give rise to unwarranted implications regarding my views on animal behavior, in general, and, in particular, on behavioral responses of honeybees that are relevant to the "dance language" controversy. My MS clarifies the issue and presents an unorthodox view regarding the role of behavioral responses such as honeybee dances in evolution.

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Egghart (1985) points out a very interesting correlation between body-size and honeybee "dialect." She proposes that this correlation may, in turn, be due to the effects of energetics on evolution, via an effect of body-size and geographically controlled climate on energetics. She suggests that the presence of distance information in honeybee dances may be due to physiological responses rather than instincts or a human psychic level. Thus far I find her ideas enlightening and most valuable.

In connection with her latter suggestion, she cites a publication of mine (Rosin 1980a), obviously referring to my views on instincts, i.e., inherited behavior (or genetically determined behavior, in more modern terms) and on the possibility of a human psychic level in lower animals, thereby implying that our views support one another. Here, however, I find it necessary to disassociate myself from any unwarranted implications to my views that might arise from her reference to my work.

My views, opposing the existence of instincts and of a human psychic level in lower animals, do not stand isolated. My views form an integral part of the paradigm of Schneirla's school in animal behavior which I find necessary to accept. As a result of my belief in Schneirla's school, I naturally believe that the presence of distance information in honeybee dances is due neither to instincts nor to a human psychic level. On this rather trivial issue, Egghart and I fully support one another. There are, however, certain aspects of Schneirla's school which Egghart obviously does not accept. On major issues our views are diametrically opposed. Thus, Schneirla's school leads me to accept none of the details of Egghart's proposal as to how distance information could come to exist in honeybee dances nor the possible existence of a honeybee "dance language," whereas Egghart takes the existence of the "dance language" for granted.

The whole field of animal behavior has been in what Kuhn (1970) terms a pre-paradigmatic stage, with European Ethology (founded by Lorenz and Tinbergen) and Schneirla's school (Maier and Schneirla 1964; Schneirla 1972) as major contenders for the status of general ruling paradigm. A pre-paradigmatic stage differs from a revolutionary stage primarily in the absence of a single ruling paradigm accepted by all, in the former stage. The two stages thus share in common the inevitable misunderstandings between supporters of different paradigms and the inevitable involvement of subjective criteria (in addition to objective ones) in the decision whether or not to accept or reject a

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specific paradigm. Consequently, my acceptance of Schneirla's school inevitably and appropriately involves also subjective criteria, i.e. elements of personal belief. It is my evaluation that critics of Schneirla's school for the most part misunderstand and misinterpret various elements of the school. Even Hinde (1982), who has come closest to Schneirla's school among European Ethologists, still misunderstands and misinterprets certain aspects of Schneirla's school and thus still lags behind. Among others, Hinde misses Schneirla's stress on qualitatively different psychic levels, in accordance with different phyletic levels. (Hinde does not even mention L. Morgan, the British psychologist from whom Schneirla openly borrowed the concept of qualitatively different psychic levels, nor Morgan's canon). It is that aspect of Schneirla's school which Egghart also does not accept, as her writing clearly shows.

I cannot accept the details of Egghart's proposal regarding the presence of distance information in dances, because I see the proposal as ultimately requiring instincts or a far higher psychic level than one should uncritically attribute to insects. Moreover, I do not accept Egghart's basic premise that distance information is determined solely by the flight from the hive to the food. That premise is not accepted even by Frisch, the originator of the "dance language" paradigm, who concluded that both the flight to the food and back have an effect. More recent findings lead me to suspect that the flight to the food may have no effect at all. I will grant Egghart that the expenditure of energy during flight has an effect. But, contrary to her, I believe that the effect is indirect only and is mediated via processes such as the production of toxic waste-products in muscle and hemolymph as a result of the break-down of energy-producing substances. (Here one should take into account the continually ongoing homeostatic processes which remove such waste-products.)

As far as the "dance language" paradigm is concerned, the controversy between Frisch (1967) and Wenner (1971, 1974) is well known. I have openly joined the controversy on Wenner's side, primarily thanks to Schneirla's school which leads me to believe that honeybees cannot (as opposed to do not) have a "dance language" (see below). A honeybee "dance language" must be based either on instincts which Frisch takes for granted or on a human psychic level which Griffin (1976) seems to propose. Both possibilities are unacceptable from the point of view of Schneirla's school. A third possibility is often implied by supporters of the "dance language" paradigm who assume that honeybees already have a mechanism (irrespective of what the mechanism is based on) which induces the forager's dance as a result of the forager's flight. All that is presumably necessary is for the mechanism to simply operate in the reverse in dance-attendants to induce in them an imitation of the forager's flight as a result of an imitation of the forager's dance. This possibility is untenable for at least three reasons. The first is that mechanisms which achieve a certain end-result do not of themselves also operate in the reverse. To also operate in the reverse, they must be specifically and deliberately designed to do so, as many man-made mechanisms are designed to do. Evolution is, however, not goal-directed and, although it can lead to mechanisms which achieve a certain end-result, it cannot design them to operate in the reverse. [See Kuhn (1970) as to why Darwin's theory of evolution was so revolutionary in the first place.] The second reason is that honeybees do not in the first place have a mechanism which induces the forager's dance as a result of the forager's flight. One could assume, of course, that such a mechanism exists, but not without postulating instincts or a human psychic level to

explain why foragers do not dance in the absence of dance attendants. A third is that even if the mechanism existed and could operate in the reverse, one would still have to postulate instincts or a human psychic level to explain why "laid-off" foragers fly to their own familiar site after attending a dance with the familiar odor even when the dancer indicates a totally different site.

Schneirla's school provides no proof that the "dance language" does not exist. My belief that honeybees do not (as opposed to cannot) have a "dance language" is based on much more substantial grounds. For one, there is not and there has never been any valid proof for the existence of such a "dance language." Many presumed proofs were invalidated by Wenner's group (Wenner 1971, 1974); others were invalidated by myself (Rosin 1975, 1978, 1980a,b, 1984); the invalidation of those still remaining is forthcoming (in preparation). For another, the "dance language" paradigm began as a highly revolutionary hypothesis which succeeded atypically quickly due to a belief in valid causes for crises in the pre-"dance language" stands. These causes now turn out not to have been valid, which leads to the conclusion that the "dance language" revolution is unjustified in the first place (in preparation). Here I should only note that one cause for a crisis was due to Frisch's early erroneous conclusion that honeybees had a poor sensitivity to odors (Frisch 1920). The other cause for a crisis was due to the possibility that without a "dance language," we would end up with totally non-adaptive honeybee dances. By now, not only staunch opponents of the "dance language" paradigm like Wells & Wenner (1973) but even a staunch supporter like Gould (1976) have found it necessary to consider the possibility that honeybee dances are not adaptive at all.

Schneirla's school thus only provided the strong motivation to seek the means to invalidate claims for the existence of the "dance language." (Often, it also provided a general idea of what to look for.) In short, Schneirla's school prompted an extremely critical attitude towards any claim based on either theoretical or experimental grounds that the honeybee "dance language" must exist.

I should add that contrary to Egghart, I do not accept the suggestion by Wilson (1975) that honeybee waggle-dances or just the waggle-run constitute a re-enactment or miniaturization of the forager's flight. Depending on the distance of the food-site, waggle-dances can have waggle-runs in either one or two major directions. The forager's flight is, however, of one type only, irrespective of the distance.

I should like to add a brief note regarding the possibility of honeybee dances having no adaptive value at all. Hinde's views cannot accommodate such a possibility. At the most, he states that behavioral responses are not always adaptive (Hinde 1982). Schneirla's school, on the other hand, might accommodate such a possibility because the school requires viewing dancing as a simple behavior not above the low psychic level of insects in general. If such viewing is to be achieved at all, it might only be achieved by breaking down dancing behavior into very simple components. I believe this can be done. Once so, problems of adaptivity would have to be addressed to each simple component, instead of to the dance as a whole.

To show that the idea of a simple explanation for honeybee dances is not too far fetched, here is a very brief outline. I believe that dance-speed is determined by indirect after-effects of the expenditure of energy during flight, in the manner noted above. A "preferred" direction is determined by after-effects of the flight, in terms of a different adaptation to light in the two eyes (and compounded by a "transformation from light to gravity," when relevant). The direction of the waggle-run

(which is not invariably identical with the "preferred" direction) is determined by the "preferred" direction and dance-speed. The exit from the waggle-run is a "turn alternation" resulting from the preceding entry into the waggle-run. The curved portion of the dance is due to a combination of a tendency to re-enter the "preferred" direction and a tendency to turn towards "dance-attendants" who by the end of the previous waggle-run have changed sides in relation to the "dancer."

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