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REPLY TO BAWA

CARLOS M. HERRERA
Estación Biológica de Doñana, Sevilla-12, Spain

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Bawa's objections to my analysis of dispersal-related maternal effort in bird-dispersed plants relate to: (1) its validity as a test of the model independently proposed by Givnish (1980) and himself (Bawa, 1980) to explain the evolution of dioecy among vertebrate-dispersed plants; and (2) my criticisms to two of its assumptions. With regard to the former, Bawa provides a detailed expansion of some cautionary notes I was careful to present in my paper at the time of evaluating my results as a proper test of the model, and adds some further reasonable criticisms. I therefore have to agree with Bawa in that "the possibility that observed results are due to inadequacies of the test cannot be ruled out entirely," to use my own earlier words.

With regard to (2) above, Bawa objects to my criticisms relating to the principle of male-female resource allocation, and to the relationship between dispersal success and crop size. With respect to the former, I did not negate its existence, but rather expressed my belief, based on published evidence, that paternal and *dispersal-related* maternal reproductive investment do not always extensively share the same resources. Bawa contends that to the extent that dispersal-related maternal reproductive investment depends upon stored resources, it is influenced by investment in paternal function. I fully agree with this view, and it was precisely for this reason that I adduced examples illustrating that dispersal-related maternal expenditure does not depend upon stored resources; these examples led me to conclude that the principle of male-female allocation is not "a generally valid assumption when one is dealing with dispersal-related female expenditure." The conflict between mine and Bawa's views at this regard apparently stems from personal biases in the choice of relevant studies: while he highlights those investigations whose results give support to the male-female allocation principle, I instead emphasized studies illustrating the opposite. The very fact that available literature provides room for two such contrasting choices of examples further suggests that the principle of male-female allocation may not be a generally valid assumption.

With respect to the second model assumption, the way Bawa attempts to refute my evaluation of Howe and co-workers' work seems to me quite surprising. In his description of the model, the only evidence adduced by Givnish in support of his central assumption of steadily increasing dispersal success with increasing crop size were the papers by Howe and Estabrook (1977; a theoretical approach) and Howe and Vande Kerckhove (1979; a field study). Bawa, on his part, stated that ". . . with an increase in female reproductive effort there is a disproportionate increase in female fitness, as also argued by Givnish (1980). Note that owing to inter- and intraspecific competition for seed dispersers, there is usually selection for spatial and temporal peaks in fruit production (Howe and Estabrook, 1977) . . . In sum, dioecy may be more easily established in animal-dispersed species because of disporportionate gains in female fitness with an increase in the reproductive effort." I reviewed these and more recent Howe's publications on seed dispersal and found no general agreement between their results and the assumption of increasing returns in fitness with increasing maternal expenditure (crop size). In refuting my criticisms, Bawa states that "there are some problems in applying the results of Howe's studies to the seed-dispersal hypothesis." Keeping in mind that it was precisely Howe's earlier work which provided the meager evidence initially supporting the assumption of the model, it remains unclear to me to what extent Bawa's opinions on the applicability of Howe's work to the model are actually disproving his and Givnish's earlier work.

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