



# Sexual conflict and female choice

William G. Eberhard<sup>1</sup> and Carlos Cordero<sup>2</sup>

<sup>1</sup>Smithsonian Tropical Research Institute, and Escuela de Biología, Universidad de Costa Rica, Ciudad Universitaria, Costa Rica

<sup>2</sup>Instituto de Ecología, Universidad Nacional Autónoma de México, Apdo. Post. 70-275, Circuito Exterior, Ciudad Universitaria, Coyoacán 04510, Distrito Federal, Mexico

Chapman *et al.* [1] nicely summarize some recent thinking about male–female conflict, but repeat a widespread inconsistency between old and new versions of sexual conflict, and give an overly optimistic impression of support for new models of antagonistic coevolution.

They define conflict broadly as ‘differences in the evolutionary interests between males and females’. Such conflict is hardly a new idea. Traditional darwinian sexual selection by female choice [2] inevitably involves male–female conflicts of interest. Chapman *et al.*’s characterization of the traditional view as ‘one of cooperation and harmony between the sexes’ misreads history. This broad definition contrasts with how the authors propose to distinguish sexual conflict from other models of sexual selection. Here, they emphasize the truly new, but much narrower conflict hypothesis: ‘the force driving the evolution of the [female] preference is better described as a general female avoidance of male-imposed costs, rather than, as in traditional direct models, acquisition of benefits from preferred males’. Inconsistent use of old, broad definitions, and new, narrow definitions has plagued recent literature on sexual conflict.

How strong is the support for the new, narrower idea of sexual conflict? Some impressions of conflict, based on observing female ‘resistance’ behavior, or from documenting reproductive costs to females, are misleading [3,4]. A female can gain by being ‘manipulated’ by a male if her indirect gains via increased manipulative abilities of her male offspring are greater than the male-imposed reduction in her own reproduction; under some conditions, female susceptibility to males can be advantageous [4,5]. Chapman *et al.* argue that such ‘indirect benefits [to the female] are expected to be a weak force in the face of direct selection on preference’, but cite only a theoretical model as evidence. The track record of quantitative conclusions based on mathematical models of sexual selection is rather dismal [2]. Recall, for instance, the now discarded dogma that Fisherian female choice was unlikely because quantitative models had ‘proven’ that there is little or no heritable variability for sexually selected traits in males. The conflicting demonstrations regarding the feasibility of handicap models constitute another example. In addition, empirical evidence indicates that indirect benefits to the female are not necessarily small [6]. Thus, the studies that

Chapman *et al.* cite as documenting the overall cost of manipulation for females, none of which took this possible indirect benefit into account, fail to demonstrate a net cost rather than a net benefit.

A second weakness is that costs and benefits cited by Chapman *et al.* were measured under captive rather than field conditions. It is trite, but nevertheless true [4], that fitness measures made in captivity do not reliably document selection in nature. For instance, reductions in female lifespan in the lab [7] might be irrelevant in nature if females die at earlier ages under natural conditions. Ecological realism is especially important for traits possibly involved in male–female conflict [1], because the demonstration of conflict depends on precise quantitative balancing of costs and benefits. Finally, Chapman *et al.* do not discuss morphological evidence from many other species that speaks strongly against the importance of new male–female conflict models [8–11].

## References

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