behavior in ducks are extremely interesting, and we urge that the behavior be studied and interpreted as carefully as possible. Generalizing from the behavior of urban mallards to natural wild populations must be done cautiously, if at all.

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References and Notes
7. We thank M. G. Anderson, J. Burns, K. Cheng, J. L. Hoogland, A. Pace, and H. B. Tordoff for discussions of these points.
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I agree entirely with McKinney, Barrett, and Derrickson that caution is needed in generalizing mallard behavior from urban to natural wild populations; accordingly, I am sure we would all welcome studies in relatively undisturbed habitats. I also agree with them and with Hailman that postcopulatory display by a successful rapist is paradoxical. One possibility, however, is that signaling of this sort reduces the likelihood that other rapists will also attempt to copulate with the female in question. The mated male would be less deterred since he need not contend with a risk factor that could operate on rapists—possible aggressive intervention from the hen’s mate.

Hailman maintains that rape and the mated males’ responses to rape are unlikely to be fitness-enhancing in the way I described (1), since early-season copulations rarely lead to conception. Instead he interprets these behaviors as helping to establish and maintain the pair-bond. However, this seems unlikely. Female mallards show vigorous escape and distress responses to rape attempts, and rape is apparently a major cause of death among free-living ducks (2). This suggests that it would be unlikely to promote bonding by the females concerned. Conceivably, however, rape could still be adaptive for the individual rapists (according to either Hailman’s hypothesis or my own), if each individual male is following his best strategy—whether to establish a bond or to inseminate a female—despite the consequence of increasing the female victim’s chance of being injured or killed. This situation would resemble Hardin’s “tragedy of the commons” (3) in that, as individuals each seek a selfish, personal benefit, a public “resource”—the females—may be endangered.

In the Seattle area, mallard egg-laying typically occurs from late April until late May. The earliest I have found eggs was 4 April; the latest, 10 July (this presumably was a renesting). Unfortunately, I have no behavioral data for June or July. The 89 rapes I observed and reported (1) were distributed as follows: January, 8; February, 13; March, 12; April, 25; and May, 31. My original report presented these same data as “rapes per female per observation hour” (I, p. 788); the same seasonal trend was apparent. In addition, of the 31 cases I observed of males intervening during rape attempts, 29 occurred during either April or May, the time of effective copulation. And finally, all 39 observed forced pair copulations (FPC’s) took place in April and May. Although not necessarily contradicting Hailman’s hypothesis, these data also accord with the one originally put forward, especially since both rape attempts and FPC’s actually coincide with the fertilizable period of females, despite Hailman’s implication to the contrary.

Accordingly, it remains at least a tenable hypothesis that both rape and the mated males’ responses to it are ultimately motivated by fitness considerations deriving from the possibility of impregnating the female. A similar suggestion has been made for wild pintails (4), for which successful nests were found to be initiated late in May and during June, after copulation between mated pairs had ceased. Successful rapes which continued into June may well have been responsible for their fertility, especially considering the brief survival of sperm in the female’s reproductive tract (as noted by McKinney et al.). I unfortunately have no data for mallards concerning copulation frequency between mated pairs. Such data might help assess the possible function of rape in fertilizing renesting females.

On the other hand, I am admittedly at a loss to explain very early rape attempts (January through March) on the basis of potential insemination. Theory should ideally explain even these few exceptions. But in puzzling over early rapes, perhaps we are simply expecting too much precision from behavioral adaptation of this sort; that is, depending especially on the cost-benefit considerations, rapists and respondents to rapists need not necessarily be under a rigidly narrow seasonal schedule. Also, my interpretation and Hailman’s are not mutually exclusive.

Finally, contrary to Hailman’s implication, I certainly do not wish to substitute “reasonableness” for facts. I believe that our differences arise primarily from our different orientations: he speaks from an ethologic tradition and I from a sociobiologic one. Both may be “correct”; one or the other may eventually prove more scientifically fruitful. At this point, more data are needed to resolve the issue. As a start, I suggest the following questions. (i) Do rapists perform fewer postcopulatory displays when other potential rapists are around? Do other potential rapists perform more rapes when no postcopulatory display has been given by a previous rapist? What are the relative reproductive success of such individuals, as a function of these behaviors? (In this, as in all questions of reproductive success, genetic markers would probably be necessary.) (ii) Are rapists more or less successful than otherwise comparable non-rapists in eventually achieving pair-bonding? (iii) How successful are rapists in actually inseminating females, and how does this success vary with the responses of the female, her mate, other rapists, the season, and other characteristics of the rapists themselves? (iv) To what extent, if at all, does the reproductive success of mated males correlate with their performance of FPC’s? (v) What is the effect of FPC’s on subsequent maintenance of the pair bond? This is but a partial list. If the direct application of evolutionary biology to mallard rape appears to suggest more questions than it answers, then this is all to its credit.

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References and Notes
5. In my report (1) two changes should be made on page 789: (i) Column 1, paragraph 2, lines 16 and 17: “[x = 38.45, a = .01, P < .001 (one-tailed test)]” should be replaced by “([Fisher’s exact probability test, P = .052].”). (ii) Column 2, paragraph 1, lines 4 and 5: “([Fisher’s exact probability test, P = .052])” should be replaced by “([x = 38.45, a = .01, P < .001 (one-tailed test)].)” * Present address: Departments of Psychology and Zoology, University of Washington, Seattle 98195.

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