basin. They incorrectly cite three published sources (*1–3*) as an intended appeal to add validity to Herreid's observations (*4*). Eakin (*1*) makes no reference to drift in or around the Sithylemenkat Lake basin. Also, the basin is not indicated on either of his maps. The Sithylemenkat Lake basin is shown on the map by Karlstrom (*2*) to be located in an undifferentiated unit (*5*) containing mostly alluvium with possible deposits of eolian, colluvial, fluvial, marine, and glacial origin. As to Patton and Miller's appeal to the map by Coulter *et al.* (*3*), Sithylemenkat Lake is shown on that map (*3, 6*) as being more than 20 km from any glaciated area. Wahrhaftig has published a map (*7*) showing the areas in Alaska covered by Pleistocene glaciers, and it does not support Herreid's work (*4*) in any manner. Moreover, Pewe (*8*) in a 1975 publication shows a map (*8*, p. 16) indicating the extent of Quaternary glaciations in Alaska, and it does not provide any support for glaciation in the area. Perhaps Patton and Miller are not aware of Pewe's work (*8*).

I do not think that my inclusion of the Sithylemenkat Lake basin in the northern Ray Mountains is unreasonable. The center of Sithylemenkat Lake is 44 km north of the drainage divide in the center of the Ray Mountains, and of course mountains do have width. The reference to the Ray Mountains as a geographic area was to help my readers, and interestingly this is supported by Eakin (*1*, p. 15). Eakin (*1*, p. 14) indicated that the Ray Mountains were located on the headwaters of the Ray, Tozitna, and Kanuti rivers. Sithylemenkat Lake is in the upland part of the Kanuti drainage basin, and the center of the lake is 27 km northwest of the drainage divide in the headwaters of the Ray River. Locate Sithylemenkat Lake and the Ray Mountains on the color composite of Landsat scene 1341-21130 and one will see easily the continuity of terrain which could warrant the inclusion of the lake area in the northern Ray Mountains.

Herreid's description of drift (*4*) in the basin is moot when taken in consideration of his preceding discussion of altiplanation terraces (*9*), nivation effects, and solifluction of materials situated physically above the deposit of so-called drift.

Since Patton and Miller do not indicate in their comments a direction for the flow of glacial ice which they favor forming the lake basin but indicate some relation to a piedmont glacier to the northwest, I assume that their glacial ice moved toward the southeast when it scooped out the basin (*10*). I find incredible an attempt to account for the creation of a basin which exhibits the morphology of the Sithylemenkat Lake basin with ice flowing from the northwest toward the southeast. Regardless of the direction of the flow of the assumed glacial ice, the creation of the lake basin by glacial ice would make it one of the most unusual glacial landforms on Earth or Mars. Most lakes created by piedmont glaciers are similar to those created by large ice sheets (*11*) and are elongate and unlike the Sithylemenkat Lake basin.

If one were to assume that the basin was formed by glacial ice, it would seem more reasonable to consider that a cirque glacier created the basinform. However, this consideration is also difficult to support. One factor is the wall height to diameter ratio, which is unlike that for cirques (*11*, p. 133) but similar to that of meteorite impact craters (*12*). Another factor is that the snowline must be at or slightly above the cirque floor (*11*, p. 136) for the creation of a cirque glacier. The floor of the Sithylemenkat Lake basin is less than 200 m above sea level, and work by Pewe (*8*, pp. 21–23) indicates that the snowline in this area was probably no lower than 900 m in Illinoian or Wisconsinan time. All of the Sithylemenkat Lake basin is below 900 m.

In my report I qualified the statements about the relationships of the nickel concentrations and the magnetic low to the lake basin. The statements by Patton and Miller about these items are repetitious.

Patton and Miller make no comment about what they can or cannot see on the Landsat imagery, but this is immaterial as they indicate difficulties with the aerial photographs of the area. I have closely studied stereo aerial photographs of the area in an effort to find landforms which would provide a clue as to the creation of the basinform. There are no elongated ice-scoured features to be seen. In order to enhance the microtopography for better interpretation purposes SLAR (*13*) imagery of the area was acquired. There are no indications of glacially related features in or around the basinform on the SLAR imagery. The SLAR imagery of the area also enhances some of the major fracturing to a certain degree. Owing to this enhancement the SLAR imagery indicated subtle radial fractures which cannot be easily recognized on the aerial photographs.

A possible shatter cone has been found in the area. Although it strongly resembles a shatter cone resulting from meteorite impact, it has not been verified as such, and the collection locality has yet to be precisely determined. At present I do not consider it as having any relationship to the Sithylemenkat Lake basin. It is most probable that any shatter cones associated with Sithylemenkat Lake basin would be in the bedrock beneath the lake.

I have done recent fieldwork in the basin which has produced evidence strongly supporting a meteorite impact origin. The fracture systems noted on the remote sensing data have been confirmed on the ground. A search was also made on the ground for glacial evidence and none was found.

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5. This unit is denoted with the symbol Qu, and is labeled as "Undifferentiated upland valley and lowland alluvium."
6. On the map by Coulter *et al.* (*3*) Sithylemenkat Lake is shown as a rough circle under the first "m" in the word "Old Dummy."
7. C. Wahrhaftig, *U.S. Geol. Surv. Prof. Pap. 482* (1965), p. 12.
8. T. L. Pewe, *U.S. Geol. Surv. Prof. Pap. 835* (1975), p. 1.
9. Altiplanation is incorrectly spelled antiplanation in Herreid's report (*4*).
10. Herreid apparently assumed an eastward or southeastward direction of flow for the glacial ice (*4*, p. 10).
11. R. F. Flint, *Glacial and Quaternary Geology* (Wiley, New York, 1971), p. 143.
12. N. M. Short, *Planetary Geology* (Prentice-Hall, Englewood Cliffs, N.J., 1975), p. 83.
13. Acronym for side-looking airborne radar.

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Rape Among Mallards

Barash (*1*) has assumed that the pairs of mallards (*Anas platyrhynchos*) he saw in Seattle, Washington, from January through May were permanent mates and that the rapist was attempting to fertilize the female. He has reported two behavioral strategies of the female's mate: (i) to intervene aggressively, in which case "this behavior was apparently successful in preventing sperm transfer by the rapist (that is, neither 'bridling' nor 'nod-swimming' occurred)"; or (ii) to force a copulation with the "just-raped female [which] conveys the benefit of introducing his sperm as quickly as possible to compete with those of the rapist."

For this interpretation to apply, at least two assumptions must be true: (i) the pair imposed upon must be the ultimate breeding pair, and (ii) the birds involved must be fertile at the time of the rape. Existing literature shows that rape occurs when neither condition is met, and it is unlikely that the conditions were met throughout Barash's study period.

Although there are probably geographic differences, a New York study showed that percentages of mallards traveling in pairs rose rapidly in early winter, and then more slowly until all birds were in pairs by April (*2*). However, these pairs are not necessarily permanent (*3*), mallards forming trial liaisons as part of courtship. This fact was reported by Brock (*4*) in 1914 and the trial liaisons are mentioned in a paper (*5*) cited by Barash.

Perhaps even more importantly, mallards copulate for months prior to egg-laying, a fact documented in a paper (*6*) cited by Barash. Yet one study (*7*) showed that after removal of the drake, fertility of eggs laid in the first week was 64 percent, in the second 37 percent, and in the third week only 3 percent. Furthermore, a study of spermatogenesis (*8*) shows that males cannot produce sperm in winter months. In fact, Höhn (*9*) showed years ago that the testes and penis do not even begin to redevelop until March [see also (*10*)], with development reaching a fairly sharp peak in May and June.

In sum, copulation commonly occurs among nonfertile mallards, which may not even be the ultimate breeding pair, and pairs are intruded upon by nonfertile rapists. Therefore, even if Barash's explanation applies to certain of his birds in the latter part of his study period, it cannot be a general explanation of rape in the mallard and seems unlikely to apply to his birds from January through March.

Studies by my students and me (*11*) suggest that copulations within the "consort" pair test the liaison for compatibility and fidelity of potential mates, and might also stimulate physiological readiness for reproduction (*12*). This view is hardly original, as it appears in Höhn (*10*), Johnsgard (*2*), the McKinney paper (*5*) cited by Barash, and probably elsewhere. In fact, our interpretation of nonfertile consort copulation and rape further suggests that Barash's explanation may not even be correct for birds late in his study period, when they might be permanently paired and fertile.

My data show that postcopulatory display of the male occurs after mountings in which thrusting by the male can be seen, usually with contact between cloacal regions of the two birds, but does not occur when no thrusting takes place (*13*). Therefore, bridling and nod-swimming cannot indicate sperm transfer because these displays occur during winter copulations when no sperm can be transferred. I suggest that the male nod-swims in front of the female to impress his individual characteristics upon her as the male with which she is "copulatorily" compatible (*11*); McKinney (*5*) reached the same conclusion.

Because the winter rapist cannot inseminate the female, it seems reasonable to suppose that his behavior functions similarly to that of the consort: it helps create, test, and strengthen a bond, in the case of rape by attempting to replace the consort in the liaison. Upon successful contact and thrusting, the rapist displays his individual characteristics to the female in the same way the consort does. It is still of advantage for the consort to disrupt the rape attempt, or to show the female immediately afterward that he, too, is still compatible, as by the actions Barash describes.

My tentative interpretation of postcopulatory display following nonfertile copulation and rape suggests a further difficulty with Barash's explanation, one that applies even to rape of a fertile female of a breeding pair. If the rapist were merely attempting a one-time insemination, why should he perform postcopulatory display?

Since the publication of E. O. Wilson's (*14*) influential book, there have been increasing numbers of papers with "sociobiological" interpretations. Apparent reasonableness of interpretation, however, is no substitute for facts concerning the basic biology of the species in question.

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12. J. P. Hailman, *Optical Signals* (Indiana Univ. Press, Bloomington, 1977).
13. I have one exception in my notes to date, but I may have missed seeing cloacal contact and thrusting before the postcopulatory displays.
14. E. O. Wilson, *Sociobiology* (Harvard Univ. Press, Cambridge, Mass., 1975).
15. I am grateful to J. R. Baylis and S. Witkin for criticizing the manuscript and to an anonymous reviewer for distinct improvement in wording.

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Barash's report (*1*) on rape in mallards (*Anas platyrhynchos*) is an interesting attempt to investigate the selective basis of a male's strategies in response to attempted rape of his mate. A fair appraisal of the significance of Barash's conclusions requires consideration of the following points.

1) Barash's birds were free-flying and unmarked, which introduces uncertainties into the interpretation of complex events for which knowing the identity of individuals is essential.

2) Urban mallard populations contain birds derived in varying degrees from domesticated and game-farm stocks [for example (*2*)] in which behavior is probably altered to some unknown degree by selection for domestication. Also, artificial crowding (as in city parks) can cause gross disruption of breeding activities in mallards (*3*), but Barash gave no information on the degree of crowding of his birds.

3) Barash reported rape behavior during January to May of three different years, yet did not give local dates of egg-laying. Such knowledge is crucial for evaluating the significance of raping in the fertilization of eggs, because mallard sperm remain viable for only 7 to 14 days after insemination (*4*). This is our most serious criticism of the study.

4) Barash assumed that the success of rapists can be ascertained by the presence or absence of male postcopulatory displays, but our observations on mallards in flight pens (*5*) contradict this. We have found that males rarely perform displays after apparently successful rapes, and the same was true in our studies of two closely related species, green-winged teal (*Anas crecca*) and northern pintail (*Anas acuta*) (*6*). Indeed, it seems that selection would favor rapists who omit these displays. Why should a rapist advertise his success if this promotes sperm competition (through forced pair copulation) by the female's mate?

5) Barash's comparison of mallards and bluebirds seems of limited value because of the great differences between these species in all aspects of pair formation, territoriality, and general breeding biology.

We agree with Barash that the possible selective consequences of raping 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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7. We thank M. G. Anderson, J. Burns, K. Cheng, J. L. Hoagland, 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" (1, 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 adaptations 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 successes 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 nonrapists in eventually achieving pairbonding? (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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5. In my report (1) two changes should be made on page 789. (i) Column 1, paragraph 2, lines 16 and 17: "[$\chi^2 = 38.45$, $\alpha = .01$, $P < .001$ (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 "[$\chi^2 = 38.45$, $\alpha = .01$, $P < .001$ (one-tailed test)]."

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