These errors have no substantial qualitative impact on the results. Correcting coefficients for the original trial 8 actually increases the predicted number of extinctions from 27 to 29 and leaves most other parameters largely unchanged: the maximal human population growth rate (1.90%), the relative amount of primary production consumed by herbivores (0.554), and the median time to extinction (895 years). So, contrary to Slaughter and Skulan's speculation, the model still leads to multiple extinctions of deer-sized species, with victims having r_m values as high as 0.28 (e.g., *Stockoceros conklingi*).

The reason for the almost unchanged results is feedback: Larger and more rapidly growing prey populations merely make life easier for hunters, thereby fueling the strong, indirect interspecific competition that generates most extinctions. High game abundance increases both the equilibrial proportion of calories obtained by hunting (0.294) and final human population density (28.31 people/100 km^2). However, both figures are still easily within the known ranges for hunter-gatherers in a variety of habitats (*1, 2*).

Despite the increase in the predicted human population density, variations of the model still show that even marginal populations could generate a mass extinction. Lowering the hunting ability coefficient to 0.30 drops the population density by almost half to 15.23 people/100 km^2, and yet 24 species still go extinct. Decreasing the caloric subsidy from plants and small game by one-quarter cuts the density to 15.91 people/100 km^2, but still leads to 25 extinctions. A decrease of one-half drops the density to just 5.75 people/100 km^2, but still leaves 14 species extinct and six others doomed.

In sum, the model's results are, if anything, improved by these minor corrections. Indeed, a simulated extinction of herbivores ranging in size from mammoths to four-horned antelopes is inevitable in the simulation regardless of how one fixes all sorts of parameter values: the relative degree of human hunting ability; the initial geographic point of invasion; prey dispersal rates; and direct competition for food among prey species (*3*). Likewise, exactly the same 29 extinctions result—albeit at slightly different times—if one changes either the 3% maximum population growth rate or the 40% upper limit to killing rates when prey are superabundant. Ultimately, the only important factors in this model are the undeniable ones: substantial variation among prey species in reproductive rates, strong dependence of human population growth on prey availability, and the broad, unspecialized predatory habits of humans.

Standard ecological theory (*4*) shows that these factors lead inexorably to strong apparent competition, and therefore to mass extinction.

JOHN ALROY
National Center for Ecological Analysis and Synthesis, 735 State Street, Suite 300, Santa Barbara, CA 93101–3351, USA. E-mail: alroy@nceas.ucsb.edu

References and Notes
1. D. S. Wilkie, B. Curran, R. Tshombe, G. A. Morelli, *Conserv. Biol.* **12**, 137 (1998).
2. J. Steele, J. Adams, T. Sluckin, *World Archaeol.* **30**, 286 (1998).
3. J. Alroy, *Science* **292**, 1893 (2000).
4. R. D. Holt, J. H. Lawton, *Ann. Rev. Ecol. Syst.* **25**, 495 (1994).
5. I thank R. Slaughter and J. Skulan for their helpful feedback, and R. Holt and R. Whatley for comments.

Unpublished Record of a Career in Meteoritics

IN HIS NEWS FOCUS ARTICLE ABOUT METEORITICIST John Wood and his views on the state of meteoritic science, Richard A. Kerr says, "The editor of the field's lead-

ing publication, *Meteoritics and Planetary Science*, had asked Wood to submit a manuscript, but the two of them could not agree on its format" ("A meteoriticist speaks out, his rocks remain mute," 31 Aug., p. 1581). The manuscript referred to was based on the Harold Masursky Lecture that Wood delivered at the 2000 Lunar and Planetary Science Conference.

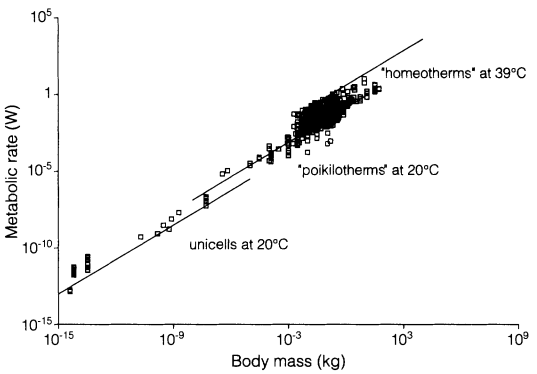
Wood's paper was sent to three reviewers, and the consensus was that the paper was not acceptable for publication in *Meteoritics and Planetary Science*. However, I was hopeful that with revision it might be publishable in the journal's supplement, which contains papers of value to the community but not necessarily appropriate for the journal. I requested changes accordingly, which were of substance, not format. The paper did not seem to me to be an assertion that chondrule research over the last four decades had been futile. Rather, it suggested that the avenues of interpretation pursued by this particular researcher had been fruitless. It was a valuable record of a frustrating career over a unique time in planetary science history, and I regret that it has not yet been made publishable.

DEREK SEARS

Editor, *Meteoritics and Planetary Science*, Cosmochemistry Group, Department of Chemistry and Biochemistry, University of Arkansas, Fayetteville, AR 72701, USA. E-mail: dsears@uark.edu

CORRECTIONS AND CLARIFICATIONS

REPORTS: "Effects of size and temperature on metabolic rate" by J. F. Gillooly, J. H. Brown, G. B. West, V. M. Savage, E. L. Charnov (21 Sept., p. 2248). In Figs. 1, 3, and 4, a systematic error was made in the units of metabolic rate: Instead of watts (joules per second), as was shown, the units should have been joules per minute. Thus,



the value of metabolic rate shown on the published figures is a factor of 60 too large; to obtain the correct value in watts, the number shown in each Fig. must be divided by 60. Consequently, in figure 4, where the authors compared their temperature-normalized plot with that of A. M. Hemmingsen, his data are correctly expressed in watts, whereas the authors' are in joules per minute and should therefore be reduced by a factor of 60. The corrected version of Fig. 4, in which both sets of data are expressed in the same units (watts), is shown here. In addition, there was a mislabeling in Fig. 2: The ordinate should have read ln [LS/($M^{1/4}$)] rather than ln [LS ($M^{1/4}$)], so that the unit of the quantity in square brackets is days per $gram^{1/4}$ and not simply days. These corrections do not affect the conclusions or the nature of the results of the paper.

NEWS OF THE WEEK: "Peer review and quality: a dubious connection?" by M. Enserink (21 Sept., p. 2187). A quotation from Tom Jefferson's presentation at the Fourth International Congress in Biomedical Publication held in Barcelona, Spain, 14 to 16 September 2001, was incorrectly stated. The quotation, cited in the first paragraph on p. 2188, should have been, "If I manufactured a drug called peer review and applied to the Food and Drug Administration for its registration on the basis of currently available evidence, they would collapse laughing."