that we have recently completed at Texas A&M University. In short, a well-supported phylogeny for the order Cetartiodactyla (Cetacea and Artiodactyla) was found by using eight independant nuclear DNA fragments. I wish to clarify an incomplete quote. We are not the first group to sequence or analyze multiple nuclear DNA genes in an attempt to determine the position of the Cetacea. For a recent thorough review of this topic, interested readers are referred to Gatesy *et al.* (*1*) and references therein.

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Cold Fusion Prediction

Eugene F. Mallove (Letters, 18 June, p. 1929) maintains his continued enthusiasm for cold fusion and writes about me, "Garwin and others are simply ignoring data in favor of their theories that these low energy nuclear reactions are impossible. Garwin's quoted assertions indicate a paradigm paralysis that is familiar to historians of science. Its remedy is a hard look at data, not uninformed opinion."

In the spirit of informing your readers, I suggest a look at the Mallove prediction to be found at www.math.ucla.edu/~barry/CF/mmbet.html. This URL documents a bet between Eugene Mallove and Barry Merriman and others as to whether by 19 July 1996 "cold fusion (CF) will be widely accepted as existing; as energy producing; or as economically viable." Mallove's written testimony to the subcommittee on energy of the U.S. House of Representatives Committee on Science, Space, and Technology (5 May 1993) includes, "Prototype cold fusion home heating units are widely expected to emerge this year or next...."

I would love to see cold fusion a reality. However, my own calendar reads 1999, and I have yet to see any home heating units or electrical power generation by cold fusion.

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Growing Metallic Whiskers: Alternative Interpretation

Additional experiments have shed new light on the phenomenon reported by one of us and L. Farber (Reports, 7 May, p. 937). The fabrication details of the porous samples examined in this work, Ti_2GaN, $TiGa_3$, and $FeGa_3$, are described elsewhere (*1*). X-ray diffraction of the samples indicated that they were predominantly single phase, with small (≈5 vol. %) amounts of unreacted Ga. Two sets of samples were prepared; one set was quenched in water from the processing temperature of 800°C, the other was furnace cooled. The lattice parameters of all samples tested remained unchanged before and after the growth of the Ga filaments, implying that it is unlikely that the crystalline lattice is the Ga source (*2*).

The surfaces of the furnace-cooled samples were sporadically covered by nonwetting Ga droplets (*1*). The droplets, which appear to be connected to the substrate by what can best be described as liquid Ga stringers or ligaments (*1*), increased in size with time. Samples that were slowly cooled did not grow whiskers. Conversely, the quenched samples grew whiskers identical to those previously observed (*2*). The mutual exclusivity of the whiskers and droplets implies that their source is identical. Because the lattice is not the Ga source, by a process of elimination, we believe that it must be unreacted Ga trapped in the internal surfaces or pores. Given that the formation of the droplets results from Ga dewetting of the internal surfaces, we conclude that the driving force for the growth of the whiskers is the overall reduction in surface energy and not a reaction with the atmosphere or a phase transition (*2*). Furthermore, the whiskers are not monolithic, but are comprised of bundles of Ga fibrils (*1*). The growth habit of these fibrils is unknown, but must reflect a strong anisotropy in growth along a given crystallographic direction.

Last, the necessary requirements needed to grow whiskers are the right combination of surface diffusivity, anisotropic growth, and nonwetting. If these conditions can be achieved for higher melting-point metals, such as Bi and Sn, they could be grown as whiskers as well.

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References and Notes

1. Relevant text and figures can be found at www.materials.drexel.edu/faculty/Barsoum/Abstracts/A21.htm
2. M. W. Barsoum and L. Farber, *Science* **284**, 937 (1999).
3. We thank L. Ho-Duc for help in carrying out some of the experiments and M. Gamamik and T. Twardowski of Drexel University for many helpful discussions. Partially supported by the Division of Materials Research of the National Science Foundation (DMR 9705237).

Minority Data

I was surprised to read in the article by Jeffrey Mervis (News Focus, 28 Aug. 1998, p. 1268) that my own mathematics department was ranked second in the nation for producing 12 minority Ph.D.s in

mathematics during the time period 1 July 1992 to 30 June 1996. I did not recall that my department had produced more than a handful of minority Ph.D.s. I was positive that no African-American or Native American had received a doctorate from this department. Mervis's data were obtained from the National Opinion Research Center (NORC), a contractor to the National Science Foundation. According to those data, 74 Ph.D.s were awarded in mathematics in the United States during this time period. The breakdown was as follows: one Black, three Mexican-Americans, eight other Hispanics, 43 Whites, and 19 Asians. Apparently, citizenship is not considered when this data is collected.

I obtained a listing of the students who received doctorates in mathematics, applied mathematics, and statistics from our university. I counted 65 doctorates for that time period. There were no African-Americans, Native Americans, or Chicanos in that list. There was one person whose mother was born in Latin America who could be considered a U.S.-born minority. Seven Mexican nationals were on the list. The discrepancy in data might come from several sources. Apparently, computer theory and practice was considered mathematics for reporting purposes. Twenty-six doctorates in computer science were produced in that time period, and there was one Chicano on the list. Also, several students received doctorates in May 1992 and August 1996 who could have been counted.

Mervis's article paints a depressing picture of the education of the minority population, but it is apparently even worse than it appears.

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Rankings of Research-Active Departments

One might expect that the well-publicized, extremely competitive nature of the job market for tenure-track faculty positions recently would have an interesting side effect: with an oversupply of qualified applicants for all open positions, the relative quality of faculty across different institutions should be evening out. To study this possibility, I have analyzed the National Research Council rankings of the home departments of U.S. authors in *Science* and *Nature* for 1999 and 1989 (volumes 244 and 284 in *Science* and 339 and 399 in *Nature*) (*1*). It would be interesting to extend the study to international authors, but unfortunately a unified international ranking of departments is not available.

The mean ranking of an author's department for the 1989 volumes is 19.2 ± 1.5 (n = 163; the uncertainty is taken as the standard deviation divided by the square root of n). The mean for 1999 is significantly lower, 24.3 ± 1.3 (n = 244). There is no significant difference between the two journals in a given year. Since *Science* and *Nature* have remained the premier research journals over this 10-year span, the natural explanation for this trend is a decrease in the variation of faculty quality between different institutions. Such information should be kept in mind when interpreting rankings of research-active departments.

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