Letters

"My Mom, the Professor"

Why isn't this phrase heard more often in the technologically most advanced country in the world, especially in the laboratory sciences like chemistry or physics? Is it a peculiarity of the discipline? Is it because there are so few women chemistry and physics professors in the upper ranks of academia? Or is it also due to the time demands of responsible motherhood and the 60- to 80-hour macho work weeks required of males and females alike during their pretenure life? Is it the ticking of the biological clock as the young woman receives her Ph.D. or M.D. degree around the age of 27, completes her postdoctorate stint near her 30th birthday, and enters, as assistant professor, the 6-year race toward academic tenure in competition with her male colleagues? When should these superwomen decide to become supermoms?

Yet there are countries in which there are many more women (and mothers) among the higher academic ranks of scientists than in the United States. Argentina and the Philippines are two examples. The reasons for these differences are complicated, but one of them is the availability and affordability of domestic help, which permits raising an infant at home rather than having to depend on institutionalized childcare.

Now that the American stigma of the working mother is rapidly disappearing, now that it is recognized that women are the largest untapped human resource in science, and now that more women graduate students are entering scientific disciplines from which they were earlier barred by cultural or operational factors, is it not time to take steps that would facilitate their decisions about childbearing and rearing? Let me offer one modest proposal along those lines:

The bright young woman Ph.D. or M.D. has no difficulties these days securing fellowship support. In the majority of American universities, she can now compete openly for entering assistant professorship positions. What most cannot afford during that period is raising a child. Why not make available—on a competitive basis related to professional promise or performance—5-year grants (at a level of about $20,000 to $25,000 per year) for domestic childcare support? A woman scientist would be eligible to apply as soon as she has secured a postdoctoral or junior academic position, but actual payment and start of the 5-year grant would only commence a couple of months before the expected birth of the baby. Would such a program stimulate some promising young women scientists to become mothers at a time when they would otherwise feel they could not afford it? Would such financial support attract some women into demanding scientific careers when they are otherwise not prepared to do so because of their desire for childbearing and childcare in the home?

I propose a pilot program on the order of $1 million whereby a foundation or government agency would initially commit itself to fund perhaps ten such 5-year grants. It would signal to American professional women that childbearing is not considered a biological burden but rather a societal benefit deserving societal support. The number of actual applicants will indicate whether such a scheme fulfills an unsatisfied need. At the end of the trial period, or perhaps when the majority of initial grantees have passed beyond 3 years of support, the recipients will be asked to report to what extent such a program has actually facilitated their decision to become mothers at an earlier age or to even have children at all. If successful, such a program could then be enlarged and be made a permanent component of our science grant programs. It might even encompass other disciplines where the time demands of the profession and the obligatory absence from the home have also proved to be impediments to motherhood. "My mom, the professor" might then be heard more frequently.

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Arctic Dinosaurs and Terminal Cretaceous Extinctions

E. Brouwers *et al.* (Reports, 25 Sept., p. 1608) present fascinating evidence suggesting that hadrosaurid, tyrannosaurid, and troodontid dinosaurs inhabited, on a year-round basis, an area located north of the Late Cretaceous Arctic Circle. They logically infer that these dinosaurs were able to survive weeks or months of total darkness, reduced temperature, and (for the hadrosaurids) reduced food supply. This information is of great value to our understanding of dinosaur ecology.

Unlike Brouwers *et al.*, I do not believe that this evidence tests the hypothesis that dinosaur extinction resulted primarily from darkening of the earth caused by atmospheric dust (the latter resulting from volcanism or meteor impact). Most dinosaurs inhabited areas normally unaffected by lengthy periods of darkness, and thus would not have been preadapted for survival during the catastrophe. Even Arctic dinosaurs would have been affected detrimentally by any event that either increased the length of the dark season or created a second period of darkness (and thus a second lean period for herbivores) during a single year. Only a catastrophe beginning during the earlier part of the normal season of darkness would be expected to have had minimal effect on populations of Arctic dinosaurs.

It appears, then, that the new data slightly constrain the time of year at which a terminal Cretaceous catastrophe may have occurred, but do not invalidate or cause serious problems for a model of extinction that incorporates long-term darkness as a factor.

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Although my work is cited by Brouwers *et al.* as they suggest temperature parameters for the occurrence of Cretaceous dinosaurs on the Alaskan North Slope, I disagree with some of their statements on the probability of freezing temperatures on the North Slope during the Late Cretaceous and question the relevance of Alaskan dinosaurs to Cretaceous-Tertiary (K-T) boundary extinctions. Moreover, I emphasize that the latest Cretaceous temperature estimates for the North Slope on the basis of paleobotanical data have a degree of uncertainty that is compounded by long-term temperature fluctuations and the uncertainty of the precise age of the dinosaurs.

One estimate for the Campanian (*1*), which is consistent with latitudinal temperature gradients at lower latitudes, is a mean annual temperature (MAT) of 8°C and a cold month mean (CMM) of 4°C; other North Slope data suggest an MAT sometime during the late Campanian or the Maestrichtian, or both, of 2° to 6°C (*2*), which allows a CMM near or somewhat below 0°C. The CMM of −11°C from (*2*) is an absolute minimum and not a suggested CMM because the climate was maritime (*3*); and, as emphasized by Brouwers *et al.*, depends on inferences concerning invertebrates. Further, lower latitude early Maestrichtian plants show evidence of a cooler climate than the Campanian, with the late Maestrichtian being warmer than the Campanian (*1*). If the dinosaurs described by Brouwers *et al.* are late Maestrichtian, they may have lived in a CMM of more than 4°C. The discussion by Brouwers *et al.* does not consider all these variables or the fact that the long Arctic winter night lacks diurnal