Letters

Groundwater Contamination

We were pleased to see additional attention given to the groundwater contamination problem in Philip H. Abelson's editorial of 18 May (p. 673). Although we agree with his conclusion, we are concerned that one statement may be read or used out of context: "An obvious method of avoiding future additional groundwater problems would be to stop pouring wastes into the ground." As pointed out in the National Research Council's *Groundwater Contamination* report, legislation has severely limited the amount of wastes that can be disposed of in surface waters and the atmosphere. Burial in the ground therefore has become the most often used option for the disposal of the hundreds of millions of tons of wastes produced each year. The report states that to reduce the amount of wastes for disposal a strategy needs to be developed that provides for the segregation, treatment, and disposal of wastes according to their chemical affinities. The report also stresses that the subsurface *can* be safely used for waste disposal *if* sites are selected, designed, and engineered in terms of hydrology, geology, hydrogeochemistry, microbiology, and the nature of the wastes.

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NAS and the Soviet Academy

I would like to comment on some of the statements in the briefing "NAS to explore expansion of programs with Soviets" by John Walsh (News and Comment, 18 May, p. 696).

In February 1980, the council of the National Academy of Sciences (NAS) voted to suspend scientific symposia held under an exchange agreement between NAS and the Soviet Academy of Sciences. Individual exchanges were not affected by this action, which was taken because of the treatment by the Soviet Union of Andrei Sakharov, a Foreign Associate of NAS, a man of unique scientific distinction, and a great contributor to the scientific community.

NAS took no action in February 1980 with regard to Poland or Afghanistan. For some time NAS has had a Committee on Human Rights, now chaired by Lipman Bers. The committee acts on behalf of individuals from the scientific community anywhere in the world who are victims of repression or whose human rights have been violated. The action taken by NAS with respect to Sakharov was in the tradition symbolized by the Committee on Human Rights.

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Sex Differences Among the Mathematically Precocious

Two letters to the editor (23 Mar., p. 1247) referring to the 2 December 1983 report by Camilla P. Benbow and Julian C. Stanley (p. 1029) discuss the correct interpretation of a study by Fox, Brody, and Tobin (*1*) of social processes that inhibit or enhance the development of competence and interest in mathematics among highly able young women in 1982 (reported at the January 1982 AAAS annual meeting in Washington, D.C.). As senior investigator for that study, I would like to react to those letters.

In our study, we did not seek social explanations for sex differences in performance on the Scholastic Aptitude Test in Mathematics (SAT-M). We were concerned with identifying factors that might explain differences in interest in accelerating the study of mathematics among those students who had very high scores on the SAT-M in the 1979 Johns Hopkins Talent Search. Our two primary samples were girls who scored 500 or higher and later participated in an accelerated mathematics program and girls who scored 500 or higher and did not elect to accelerate. For comparison we selected two groups of boys, those who did accelerate and those who did not, matched with the samples of girls on SAT-M scores, geographic location, and school characteristics. Thus, when we compared boys with girls, we were comparing students with approximately the same SAT-M scores.

Although these findings do not relate directly to the issue of sex differences in test performance as it is being debated, I personally believe that sex differences in test performance on the SAT-M result in part from differences in confidence and early learning experiences. But on this point I can only speculate. Perhaps our samples of high-scoring girls are more atypical of girls in general than are our samples of boys atypical of boys in general with regard to the types of nurturing they received from parents and schools.

The fact that Benbow and Stanley find far more boys than girls scoring above 600 on the SAT-M should not be ignored, but what does it mean? The SAT-M is not a pure measure of innate ability, but rather a measure of ability as it has developed in interaction with educational experiences within and outside of schools. The SAT-M has not yet been shown to accurately predict adult creative achievement in mathematics or engineering or success in a career. All boys do not score higher than all girls on this test, so surely gender is not the sole factor related to performance on the test. Do more boys than girls score very high on this test because of an innate male advantage in learning mathematics independent of experience? This has not yet been proved. In time we may know more about the development and functioning of the brain as it relates to hormones and genes and to the manifestation of specific abilities. At present, we should be cautious about touting the "superiority" of one sex over the other. Perhaps girls will be found to be superior to boys in some types of learning tasks. On standardized tests of achievement girls tend to do better than boys on decimal problems, while boys have the advantage on fraction problems. If there is a female advantage in thinking about or learning decimals, surely we will want to modify instructional strategies to accommodate these differences, rather than saying, "boys can't learn decimals and should avoid careers in accounting."

Personally I believe that arguing for the superiority of one group over another in terms of innate potential on the basis of crude measures is not good science or socially productive. I am concerned that prolonged debate of this issue (especially in the popular press) on the basis of research that does not address all the relevant dimensions could be harmful in that many able females may become discouraged or be discouraged by teachers or parents who misunderstand the difference between speculation and fact.

Those who argue for the biological basis of differences seem to be saying that it is important to make people aware of sex differences in mathematical test performance so that "unrealistic expectations" will not be set for girls. While I understand the concern about quotas being set for colleges or industry, surely there is harm in the misconception that sex differences between groups mean all men are better than all women. In the not so distant past women have been discouraged from attempting careers in fields dominated by men. If more men than women possess the necessary combination of abilities to succeed in some endeavors, let this be demonstrated fairly in an open arena of competition in the classroom and on the job. We should not erect psychological barriers to thwart the achievement of those women who do have the talent. Instead we should work to create a society in which individuals are valued and evaluated for their achievements independent of their race, creed, or sex. More research is needed to learn how biological factors relate to intellectual performance, but more research is also needed to study the social factors that influence the development of the intellectual abilities. Surely today's world is so complex that modelers of human behavior must look at both biological and social factors.

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Biological Diversity

David Jablonski *et al.* (9 Dec., p. 1123) report fossil evidence suggesting that unstable, nearshore habitats serve as the source of species with major evolutionary innovations rather than more stable, offshore habitats. This finding was considered sufficiently newsworthy to rate commentary by Roger Lewin (Research News, 9 Dec., p. 1112). However, it is interesting to note that a similar phenomenon has been previously described by neontologists and is referred to in the ecological literature as the "taxon cycle" (*1*).

For example, it has been shown that geographically widespread species of ants in Melanesia (*1*) and birds in the West Indies (*2*) are generally confined to ecologically marginal habitats, while older, more specialized species in these taxa inhabit stable, mature ones. The species occupying marginal habitats are good colonizers, however, and are able to expand their ranges to offshore islands with smaller endemic faunas. There the colonizing species are able to increase their ecological amplitude and invade the more stable habitats. The penetration of these habitats by colonizing species is aided both by the large population reservoirs that are maintained in marginal habitats and by new adaptations that allow the colonizers to usurp niche space already occupied by endemic species. Once the more stable habitats have been penetrated, differentiation in morphology and ecology is rapid; populations become more specialized; and speciation often occurs. By this process, species evolving in ecologically marginal environments not only contribute directly to species diversity in mature habitats, but they also play a major role in the fragmentation and further speciation of older taxa.

Now that this phenomenon appears to be more general and has been documented as occurring in ancient as well as recent communities, I hope further work on this interesting aspect of the genesis of biological diversity will be undertaken by both paleontologists and neontologists.

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We were aware that one possible mechanism for the large-scale patterns we observed in the evolution of marine benthic communities might be analogous to the "taxon cycle" proposed to explain present-day distributions of Melanesian ants and West Indian birds. However, not all ecologists accept the validity of the taxon cycle (*1*), and there are a number of important differences between the hypothesized taxon cycle and the paleontologic results discussed in our paper.

1) Different hierarchical levels and time scales. Individual species are the units that pass through taxon cycles seemingly on time scales of thousands of years, whereas our paleontological patterns were detected at the ordinal level over tens of millions of years. A variety of origination, extinction, and interaction processes could underlie this larger-scale pattern.

2) Different phylogenetic structure. The taxon cycle is proposed to occur phyletically, within individual species. The major faunas of the Phanerozoic that spread successively across the continental shelf are ecologic and higher taxonomic groupings that exhibit net statistical trends; they certainly are not monophyletic groups.

3) Different proposed driving mechanisms. Ricklefs and Cox (*2*, p. 196) assert that "the progress of a species through the taxon cycle reflects effects of progressively reduced competitive ability caused by 'counterevolution' of an island biota to that species, coupled with strong competitive pressure from subsequent immigrants." There is little reason to infer that the Paleozoic fauna dominated by suspension-feeding brachiopods competitively drove the trilobite-rich Cambrian fauna from nearshore habitats; net changes in tolerance to physical environmental extremes over the history of a clade is one of several more plausible explanations that do not require continuous phyletic evolution in response to rampant competitive exclusion (*3*).

Although the analogies can be intriguing and a stimulus to interdisciplinary research, the simple extrapolation of short-term intraspecific processes to more sweeping time scales and taxonomic levels can also be highly misleading.

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