Letters

Toujours Gaia

Charles Mann's article "Lynn Margulis: Science's unruly Earth mother" (Research News, 19 Apr., p. 378) was vastly entertaining and spicily written, an example of the new Kitty Kelley school of science journalism. What made it unusual was that the victim of the contumely and scorn was not a public figure, or an entertainer, but a working scientist.

Lynn Margulis is among the most eminent of living biologists, a scientist of stature superior to those selected to denounce her. She is the staunchest of colleagues I have known in 50 years of scientific research, and the sharpest of my critics. I feel a sense of outrage when attacks on her reputation are made because she almost alone among biologists chose to collaborate with me and to share my view of Gaia theory as a straight piece of hard science up for trial. It would have been much easier for her to have remained secure and rested on the laurels worthily gained from the acceptance of her own radical contribution, the endosymbiont hypothesis.

I admit that we have often been provocative. We had to be or our work would have been ignored. I now realize that our provocation was a mistake. Nothing we could say or offer as evidence would convince the closed minds of our opponents. In the case of discoveries such as the global distribution of the chloroflurocarbons and the finding that dimethyl sulfide is the natural sulfur carrier, I have found that the initial scorn and rejection was soon followed by the development of the topic as a major scientific interest. To the real scientists I say, be patient and don't knock Gaia too hard; before long she may be paying your grants.

JAMES E. LOVELOCK
Coombe Mill,
St. Giles on the Heath,
Launceston PL15 9RY, United Kingdom

Once again neo-Darwinism has received the death sentence, this time from Lynn Margulis. Asserting that the theory is "in a complete funk," Margulis says that speciation and evolutionary novelties are based on the accumulation of symbionts and not mutations.

Before we throw mutations on the scrap heap, however, we should remember the large body of fact showing that both evolution and speciation are based on changes in genes. While there are occasional reports of adaptive change in prokaryotes due to cytoplasmic factors (*1*), the vast majority of evolutionary changes in both prokaryotes and eukaryotes occurs at chromosomal genes (*2*). If Margulis' hypothesis were true, adaptive changes in the phenotype would frequently segregate as cytoplasmic factors or cellular organelles.

The same is true for speciation. While hybrid sterility can occasionally be caused by parasitic bacteria (*3*), nearly all genetic analyses show that reproductive isolation between species is caused by changes in genes and not by symbionts. This holds for both prezygotic isolation (*4*) (mating discrimination and self-fertility in plants) and postzygotic isolation (*5*) (hybrid sterility and inviability). In some cases the genes have been sequenced or mapped to quite restricted sections of the chromosomes (*6*).

It could be argued that although the small adaptive changes amenable to genetic analysis are based on mutations, large evolutionary novelties are still due to symbionts. This question will remain unanswered until we know the developmental-genetic basis for such novelties. But until Margulis can explain how such complex adaptations as wings, jaws, flowers, and operons can be caused by symbionts, a genetic explanation seems more likely.

Finally, Margulis' theory does not explain the evolution of the symbionts themselves, unless one believes in an infinite evolution-

ary regress of microorganisms swallowing ever smaller microorganisms.

JERRY COYNE
Department of Ecology and Evolution, University of Chicago, 1101 East 57th Street, Chicago, IL 60637

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Learning from the Acid Rain Program

The News & Comment account by Leslie Roberts of the National Acid Precipitation Assessment Program (NAPAP) (15 Mar., p. 1302) identifies the difficulties that this program has encountered in the course of its 10-year odyssey. But these difficulties hardly add up to explaining the chief charge—that NAPAP contributed little to the national debate over acid rain policy. The fundamental reasons for this outcome, I believe, were (i) a legislative charter that simply passed to the Executive Branch responsibility for direction of the program and coordination of agencies having different missions and different objectives, and (ii) lack of initiative and leadership at the White House level. The result was that the science and policy of acid rain were placed in separate compartments and kept there for 10 years.

The NAPAP experience provides a lesson for the global change program. Up to now, government discussions about global change policy have floated silently between the State Department and the President's staff, with little visible exposure or regular contact with the Global Change Research Progam. It is not too late to do something about this. It should be feasible to link policy and science within a single recognized component of the Executive Office of the President. This would help scientists become aware of and sensitive to critical policy questions and would help policymakers understand the scientific and factual bases for the issues they must face. By keeping science and policy in separate compartments the Administration sharpens the suspicion that it is stalling to avoid effective actions (*1*).

ROBERT G. FLEAGLE
Department of Atmospheric Sciences, University of Washington, Seattle, WA 98195

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With respect to the lessons learned from the acid rain program for research on global change, action is already under way to ensure a proper balance of science, assessment, and policy. A statement from the Second World Climate Conference (*1*, p. vi) specifically calls for

> a special initiative [that] would create a network of regional interdisciplinary research centres, located primarily in developing countries, and focusing on all the natural science, social science, and engineering disciplines required to support fully integrated studies of global change and its impacts and policy responses . . . and [to] study the interaction of regional and global policies.

A proposal is already before the world scientific community, international organizations, and national governments to estab-