will mean more gadgets, more and better bombs, more drastic, unnecessary changes in the conduct of our life. Really now, do we honestly need communications satellites? I have talked to many nonscientists, and to them, more scientific research means deadlier wars. Right or wrong, this is a conception, and it is up to us scientists to do something about this, or else we will be damned, and, I think, rightly so.

PHILIP SIEKEVITZ
Rockefeller Institute,
New York, New York

Western Snake River Fault Zone

Malde [*Science* **130**, 272 (1959)] has described a zone of northwest-trending, high-angle faults which have displaced the western Snake River Plain downward relative to highlands on the north by at least 9000 feet; 5000 feet of the movement occurred between the early and middle Pliocene, and the balance occurred in Cenozoic time. His studies are based in part on numerous gravity measurements, and from an "analysis of a 50-milligal residual anomaly associated with the steep gravity gradient near Mountain Home, it is calculated that from 13,000 to 38,000 ft of rocks about as dense as Columbia River basalt have been dropped down against the Idaho batholith."

Kirkham [*J. Geol.* **39**, 210 (1931)], from a plane table traverse of Squaw Butte near Emmett, calculated a thickness of 17,000 feet for the Columbia River basalt exposed in the butte. He said that this thickness would not hold if faults were found. While the faults are not particularly obvious on the surface, they are easily observed from an airplane. The rocks are tilted at various angles, from 8 to 40 degrees, and form narrow north-south wedges. Horizon markers are not easily identified in the Columbia River basalt, but it is obvious from the large amount of displacement visible from the air that the actual thickness of the basalt is of the order of 3000 rather than 17,000 feet. I have studied these basalts over large areas of Oregon, Washington, and Idaho and believe that a thickness in southern Idaho of appreciably more than 4000 feet is not likely. It might be argued that Snake River basin was a downfaulted basin in which the basalts pooled and became unusually thick. However, nothing in the appearance of the basalts next to the major fault zone indicates that the basalts are ponded. It seems more likely that steep gravity gradient near Mountain Home is due to thin wedges of Columbia River basalt downdropped along the border of the plain against the Idaho batholith, leading to an error in calculated thick-

ness similar to that which caused Kirkham to miscalculate the thickness in Squaw Butte.

HAROLD T. STEARNS
Post Office Box 241, Wahiawa, Hawaii

Competitive Exclusion Principle

It may seem unwarranted to extend the series of comments on the competitive exclusion principle, which has now proceeded from Hardin [*Science* **131**, 1292 (1960)] to Cole [**132**, 348 (1960)] to Savile [**132**, 1761 (1960)], with asides by Van Valen versus Cole [**132**, 1674 (1960)]. However, two points in the discussion by Savile deserve comment.

He asserts that "the absence of clearly defined associations" emphasizes the reduced importance of competition in the arctic flora. Considerable recent vegetational research, some of which I summarized in an article in *Science* [**128**, 115 (1958)], suggests that clearly defined associations may not be characteristic and that the vegetation in an area may be regarded as changing in a manner most effectively treated as a continuous variable. It is, therefore, questionable whether the absence of clearly defined associations emphasizes anything. Savile's suggestion that the flora, or better the vegetation, be described in terms of major habitats, if these are discernible independently of the vegetation itself, certainly has merit.

Secondly, Savile refers to "closely related species with identical ecological requirements." If we must beware of assuming that species have different ecological environments because they do coexist, we must also beware of speaking of two species' having identical ecological requirements. The ecological demands of well-known species are not usually known in detail, and there is always the possibility that some essential requirement of a species may escape our most careful observation or experimentation. It may be argued that the plant as it integrates the multiple and undetermined components of the environment is the most, and perhaps the only, adequate measure of its environmental requirements. In any event, the assertion of ecological identity is fraught with at least as many difficulties as the assumption of lack of identity.

ROBERT P. MCINTOSH
Department of Biology, University of Notre Dame, Notre Dame, Indiana

McIntosh's first point concerns the permanence of associations. With our currently ameliorating climate I feel certain that all plant associations are changing throughout temperate Canada, but that does not rob the association concept of its usefulness, if we use it with discretion. The lack of such associations in most arctic habitats, whether we use this term or indulge in circumlocution, still reflects meager biological competition—as others with arctic field experience have agreed since my note appeared.

In his final paragraph McIntosh questions my phrase "identical ecological requirements" used in connection with disease resistance. I used the words, following ecological practice, with some misgiving, realizing that a semantic wrangle might ensue. Consider the extreme case of a single plant species with two populations differing by a single gene that governs disease resistance. The parasite being part of the environment, these populations have different ecological requirements. Thus, if we insist on complete precision, no two species will ever have identical requirements, and the exclusion principle becomes completely meaningless, whereas, if we realize that usable definitions of biological phenomena must generally be flexible, the principle can be moderately useful under many, but not all, circumstances.

D. B. O. SAVILE
Canada Department of Agriculture, Ottawa