LETTERS

Community Ecology

In a recent account of the current argument raging in community ecology (Research News, 12 Aug., p. 636), Roger Lewin portrays Evelyn Hutchinson as the wandering pilgrim who, upon tasting the waters of Santa Rosalia, is reborn spiritually. His disciple MacArthur establishes the church of community ecology. Subsequently, generations of believers punish the unbelievers for violating the first commandment of community ecology: thou shalt keep no non-competitive god before me. At last, an atheist, Simberloff, arrives to win the hearts and minds of the ecological masses over the current church establishment (Roughgarden and Diamond) who now clutch onto the miter of power. The end, I suppose, will be a sort of 20th-century history of nullist totalitarianism.

This characterization does a disservice to Hutchinson and MacArthur and diminishes the current controversy to one of religion, rather than substance. Hutchinson brought formalism to modern ecology and built upon the previous era of theory so typified by Lotka and Volterra. His influence goes far beyond those corixid bugs. MacArthur—a brilliant mathematical ecologist—formulated a series of theories which either still hold great influence (optimal foraging theory, theory of limiting similarity, stability of food webs, theory of island biogeography) or have been since toppled (broken stick model of species abundances). Simberloff's complaints are substantive, but tend to center around the equilibrium theory of biogeography. It is true that he sees the poor testing of this theory (and others of MacArthur) as symptomatic of a sick science of ecology. With this I agree. But does MacArthur's work somehow stand out as the least testable, or is it merely on center stage because of its brilliance? If it was so obviously the wrong theory, then one can only blame the wide-eyed followers for missing this for so long.

One wonders what to make of the claim that MacArthur's brand of theory led a "generation of ecologists" in an unpromising direction until someone demonstrated that the emperor had no clothes. This sort of curious thinking blames the brilliant leader for misleading the dull followers. We can see an important principle for the study of scientific achievement. A field's health is inversely proportional to the blame given to innovators of that field for leading the field "astray." The degree to which we feel disillusioned by MacArthur is the very degree to which we have either shirked our duty or allied ourselves with a rather sick science. I am sure that ecologists are more to blame than MacArthur for the current state of theoretical community ecology.

JEFFREY S. LEVINTON
Ecology and Evolution Department, Division of Biological Sciences, State University of New York, Stony Brook 11794

Myeloma and Atomic Veterans

R. Jeffrey Smith, in his article "Study of atomic veterans fuels controversy" (News and Comment, 19 Aug., p. 733), says that in our telephone conversation I described as "a sop to the veterans" the recommendation of our 1981 panel (*1*) for a closer scrutiny of a list of alleged myeloma victims. It is most unlikely that I said any such thing. First, I did not then and do not now think that the recommendation stemmed from any other motive than the wish to see whether or not there was evidence of increased myeloma risk among early entrants to the bombed areas. Second, while I was frank with Smith, I was also aware that I was talking to a reporter, and even if I had thought that the recommendation stemmed from ulterior motives I think I would have been wise enough not to acknowledge it. Third, the word "sop" was not then in my vocabulary. If, as he also states, I described the motivation behind the study as primarily political rather than scientific, I had in mind the broad issue of study of these veterans, not the specific question of whether the list (or lists) of myeloma victims could be validated.

Let me clarify the line of thinking that Smith, or his editor, chose to highlight on page 734. I did not argue, as Smith says in a paraphrase of my remarks, that "an excess is so unlikely that a scrupulous search is unnecessary." The reasoning which I tried to get across was that any large-scale and expensive scientific study must be justified either by evidence that there is something there to be found or by the fact that a negative finding would be of value. Since the overwhelming consensus is that one would not expect an observable increase in myeloma risk among these veterans, the finding of no increased risk would have no scientific value. If the lists informally collected by veterans' organizations led one to believe that there was an excess—despite what one expected—a