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 noncompetitive 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

full-scale epidemiologic study would be justified. Since a closer perusal of these lists does not support this belief (*2*), the full-scale study is not justified. Our 1981 report specifically states that "This [the investigation of multiple myeloma in these veterans] should not *at first* involve a full-scale epidemiologic study" (emphasis added). The implication is clear that the full-scale study might follow if evidence of increased risk were found.

In my opinion, the 1983 multiple myeloma panel carried out fully the recommendation of the 1981 panel.

BRIAN MACMAHON
School of Public Health,
Harvard University,
Boston, Massachusetts 02115

References

1. "Panel on Feasibility and Desirability of Performing Epidemiological Studies on U.S. Veterans of Hiroshima and Nagasaki, Final Report to Director, Defense Nuclear Agency" (National Research Council, Washington, D.C., 1981).
2. "Panel on Multiple Myeloma among Hiroshima/Nagasaki Veterans. Final Report" (National Research Council, Washington, D.C., 1983).

R. Jeffrey Smith writes about a recent National Research Council (NRC) report assessing the evidence for allegations concerning an unusually high incidence of multiple myeloma in U.S. veterans who participated in the occupation of Hiroshima and Nagasaki.

The article does not distinguish between the motivation for directing a question to the NRC and the manner in which the NRC addresses the questions it accepts. Federal agencies bring questions to the NRC for a variety of reasons, sometimes because they need an authoritative answer to a scientific question that lies at the heart of a social or political issue. It is the NRC's function to find in the question those elements where scientific analysis and judgment of the facts by a balanced committee will assist the agency and the nation in determining their policies. Contrary to the implication in the article that the NRC mixed politics with science, the authoring committee in its report dealt only with the scientific issues.

The comments by Seymour Jablon and Brian MacMahon should be understood in the context of the distinction I have drawn above.

A second important point is that the study was *not* intended to be primarily an epidemiologic investigation. Its purpose, to paraphrase the contractual charge, was to examine allegations that there is an unusually high incidence of multiple myeloma among veterans of the Hiroshima and Nagasaki occupation forces and to present what is currently known about the incidence rate of multiple myeloma in similar populations. That is exactly what the NRC committee did, and its report does not portray the study as a full-scale epidemiologic effort. When one considers the charge to the committee, the methods used to identify and evaluate all possible claimants were appropriate. Except for the official from the National Association of Atomic Veterans, the persons quoted in Smith's article appear to agree with our committee's basic findings, and we believe that the NRC has successfully answered the scientific question that it was asked to address. Unfortunately, the social and political concerns survive.

ALVIN G. LAZEN
Commission on Life Sciences,
National Research Council,
Washington, D.C. 20418

Control of Ph.D. Programs

Dennis Doverspike (Letters, 5 Aug., p. 506) comments on my remarks (Letters, 24 June, p. 1336) and raises to the most general level the issues posed by the differences between Steven W. Mosher and the Stanford anthropology department (News and Comment, 13 May, p. 692). I would like to clarify a few issues raised in my *Society* article (July/August 1983, pp. 4–15), from whence my letter derived, and to place in sharper relief the issue of who should control Ph.D. programs.

First, no one is seriously asserting "that only scholarship and not behavior should determine who is awarded a Ph.D." Rather, the notion of misbehavior is so broad that for it to be employed as grounds for dismissal requires the public documentation of such presumed personal forms of misconduct. To do otherwise is to return graduate studies to a darker age of not so long ago, when behavior was linked to conformity and even to denigration rituals as "proof" of scholarly worth.

Second, professional responsibility is precisely what caused candidate Mosher such grave anguish. Responsibility to women who were victimized by unwanted eighth- and ninth-month abortions and responsibility to parents faced with official criticisms of those who had more than one or two children and were encouraged to engage in unofficially sanctioned acts of infanticide. It is precisely this ambiguity in the notion of professional responsibility—to anthropology as a science of discovery or to a discipline with a narrow code of conduct—that led