Letters

The Value of Systematics

In his recent editorial, "Time to revive systematics" (13 Dec., p. 1227), E. O. Wilson argues for increased attention and funding for systematics. As a naturalist, I share Wilson's fascination for natural diversity and value the contributions of systematics to biology. It is also evident that many programs of research, development, and conservation, particularly in tropical areas, are hampered by inadequate knowledge of the most basic attributes of their subjects: taxonomic position, general biology, geographical distribution. Many ecologists and evolutionists believe that describing new creatures is a key to solving these problems. But I suggest that faith in a pervasive, intrinsic value of systematic knowledge may be misplaced. Biological exploration is always accompanied by the aesthetic and emotional sensations of discovery and undoubtedly will yield some of the hoped-for genotypes, pharmaceuticals, and other natural products. But in truth, bringing the next million species into the realm of human knowledge will probably have little impact on the study of evolution, ecology, and physiology, on the development of biological resources, or on our rationale for conservation. General patterns of diversity are sufficiently clear in outline to argue for saving large tracts of the tropics and other areas for further study and for future exploitation of their diversity, as well as for the ecological and economic good sense of maintaining intact natural systems. I am not so pained by the disappearance of species before their discovery by science as I am by the decrease in the quality of human life that goes hand in hand with their demise.

Wilson is right, of course, to defend systematics as a priority in science. I believe that this task will be made easier by more clearly distinguishing two separate roles of systematics and defining their relations to the rest of science. The first is the museum's traditional endeavor of discovery and description. Whether there remain one or ten or 30 million undescribed forms, this task is uncompletable (hence priorities must be defined); it will require the enlargement of museum collections and the publication of thousands of systematic monographs each with a small audience; and it will be done primarily by individuals trained specifically for the task (requiring some coordination of educating and employing institutions), and who, with a few exceptions, will dedicate their entire professional lives to this great task. These professional systematists will rely on their sister sciences for grounding in evolution, ecology, genetics, morphometrics, statistics, biogeography, and biochemistry—so necessary for their work. They will serve, as they do now, a wide variety of interests in academics, applied sciences, and the marketplace.

The second great contribution of systematics, in my opinion sadly languishing, is the perspective it offers evolutionists, ecologists, and other natural scientists. The great questions posed by biologists concerning evolution, adaptation, and diversification arose from phenomena revealed by systematists. Many would insist that this contribution is historical. Indeed, the rush of biology to experimental science has left a sense of embarrassment over systematics. But this rush has also overly narrowed our perspectives to local, contemporary processes. Diversity strikes a balance among a hierarchy of processes in time and space ranging from local population interactions, through regional processes of speciation and habitat specialization, to global patterns of origin and dispersal. Only systematics, and the related disciplines of paleontology and biogeography, can provide this perspective.

Fascination with nature and faith in potential dividends of systematics research are not enough to bring about Wilson's wished-for resurgence. Systematics will not assume a major role in science curricula until its contributions foster scholarship of compelling interest to the biologists who control curricula and research funding. Systematics is not taught in museums; it is taught in biology departments, where it must compete with genetics, behavior, cell biology, and physiology for the interest of students. Only when historical and comparative perspectives are reintegrated into our curricula will the value of systematics become apparent to the next generation of professionals and policy-makers.

ROBERT E. RICKLEFS
Department of Biology,
University of Pennsylvania,
Philadelphia 19104

Response: The only important point of disagreement between Ricklefs and myself is the degree to which biological diversity should be mapped. For reasons given in an earlier and more extensive article (*1*), I believe that we should aim for total knowledge at the species level. Each species is unique, intrinsically valuable, and the potential source of new knowledge still difficult or impossible to imagine. It would be a failure of vision to assume that we can answer the interesting questions of evolutionary biology with only a subset of the extant species. These questions are still crudely formulated and incomplete, to put the matter gently, and few can be answered definitively with the range of data now being acquired.

But of course Ricklefs is right in saying that the problem is the value placed on such knowledge. How can systematics resume a major role in science curricula and biological diversity acquire a more general interest? By the same means that the epidermal antigens of *Xenopus* and the moons of Uranus have acquired a relatively large audience: the perception of the intellectual importance of the subject, however remote from direct human experience. The magnitude and cause of biological diversity is not just the central problem of systematics; it is one of the key problems of science as a whole. It does matter a great deal whether there are 1 million or 30 million forms. It also matters why a certain subset exists in each region of the earth, and what is happening to each one year by year. Unless we go for the whole package, we will fall far short of understanding life, and due to the accelerating extinction of species, much of our opportunity will slip away forever.

Lest this be viewed as an expensive Manhattan Project unattainable in today's political climate, let me cite the estimates I have made of the maximum investment required, one that omits computer-aided information processing: 25,000 professional lifetimes (4,000 systematists are at work full or part time in North America today) whose final catalog would fill 60 meters of library shelving for each million species. Computer-aided techniques could be expected to cut the effort and cost substantially. In fact, systematics has one of the lowest cost-to-benefit ratios of all scientific disciplines.

EDWARD O. WILSON
Museum of Comparative Zoology,
Harvard University,
Cambridge, MA 02138

REFERENCES

1. E. O. Wilson, "The biological diversity crisis: A challenge to science," *Issues Sci. Technol.* **2**, 20 (1985).

Anasazi Astronomy

Michael Zeilik's assessment (Reports, 14 June, p. 1311) of the Fajada three-slab site contains significant errors and omissions. His principal argument rests on equating *historic* and *prehistoric* practices and relies primarily on his reference to a paper by Ellis (*1*). Her paper, in turn, not only does not provide the evidence he attributes to it, but contradicts his argument by stressing the changes introduced by European contact.