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.

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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.

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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.

Calculations Zeilik presents to support his argument refuting the lunar markings are in error by factors of 2 or more, as well as being internally inconsistent. The shift of the average limiting position of the moonrise shadow edge after the major extreme is 2.6 centimeters in 2 years (not "1.5 cm") and 5.6 centimeters in 3 years (not "a little over 2 cm"). The correct values make the marking of the lunar cycle significantly more evident. The argument that the shadows are not "reliably marked" on the "weathered petroglyph" disregards that when the spirals were first made and used they were not weathered. Zeilik supports his proposal that a lunar marking was, instead, a mid-May solar marking by citing "important corn and bean planting" at the historic Hopi Pueblo. His reference (*2*), however, makes no mention of any particular planting time in mid-May, but stresses that planting was determined by season and weather. In describing "the gist" of our reports of the lunar markings (*3*), Zeilik describes incorrectly, or omits mention of, several key features of the site that underscore these markings and their symmetry.

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3. A. Sofaer, R. M. Sinclair, L. E. Doggett, in *Archaeo-astronomy in the New World*, A. F. Aveni, Ed. (Cambridge Univ. Press, New York, 1982), pp. 169–181; A. Sofaer and R. M. Sinclair, in *Astronomy and Ceremony in the Prehistoric Southwest*, J. Carlson and W. J. Judge, Eds. (Maxwell Museum Technical Series, Univ. of New Mexico, Albuquerque, in press).

Response: Sofaer and Sinclair raise two main points, first, the usefulness of ethnographic analogy and, second, the visibility of the motion of the shadow edges cast by the moon.

A methodological framework for the use of analogy is where ethnographic data can "serve as resources for testing hypotheses which seek to relate material and behaviorial cultural phenomena" (*1*, p. 63). Pueblo sunwatching practices (*2*) show the importance of anticipatory observations. A conservative hypothesis is that anticipation was an important aspect of a Chacoan Anazasi calendar. This does not "equate" past and present, but forms a baseline for evaluating calendrical sites. Ellis (*3*) implicitly uses a similar approach in her analysis, while suggesting that practices may have been more elaborate in pre-Hispanic times.

As Sofaer and Sinclair correctly note, my calculation contained an error: their values are correct. This change makes the motions more evident, but they will amount to roughly a centimeter per year and only about a millimeter per month in the 2 years before the standstill. The visibility of such motions in moonlight and on a rough rock surface still limits the usefulness of the site for anticipating the standstills.

For Hopi planting dates, a more specific reference is a paper by Forde (*4*, p. 385 and figure 6), who indicates that the main corn planting occurred in the third week of May. This and other dates were announced ahead of time by the official Sunwatcher.

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International Congress Attendance

Roger Lewin's article "Archeology congress threatened" (News & Comment, 22 Nov., p. 921) expresses a misconception that I ask to be allowed to correct: that is the statement that the decision was "to deny attendance to anyone working in South African institutions." The ban is wider than what is represented in the article.

On receiving the circular letter sent to all scientists living in South Africa denying them participation in the so-called World Archaeological Congress, I wrote pointing out that I was born and educated in England, could travel on a British passport, and am not (nor ever have been) employed by any South African university or other institution, being a self-employed professional man and private scholar. The reply from the World Archaeological Congress states that I cannot participate while I am domiciled in South Africa.

May I add that I am appalled that scientists in England should deny fellow members of distinguished British scientific bodies such as the Royal Society, the Royal Anthropological Institute, and the Society of Antiquaries the right to attend an international congress in England, and this on solely political grounds.

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