

### **Herbs and Clearcutting: Reply to Elliot and Loftis and Steinbeck**

Duffy and Meier (1992) asked if the herbaceous understory communities in the southern Appalachians differed between primary and secondary forest sites and if such differences decrease in time as forests recover from clearcutting. Steinbeck claims that our article is "little more than opinion masquerading as science." He is of course entitled to his opinion and I appreciate his willingness to express his views openly. Elliott and Loftis express "serious concern" that "major methodological flaws" exist in our work. I address their concerns below.

Elliott and Loftis claim that choice of plot size and number of plots are critical to any study of vegetation diversity. They invoke a hypothetical 50% reduction in plant density between two plant populations to show how our sampling might be biased, but such a 50% reduction in population between primary and secondary herbaceous communities would prove the point of our paper.

Elliott and Loftis further suggest that species richness may vary greatly between plots. For this to affect our results, however, there would have to be much more difference between plots within second-growth than within primary forest. For example, to use their model and our data, a second-growth forest with only 6.6 species per plot and

ten plots could have 66 total species at 100% turnover between plots. A primary forest with 10.9 species would have the same total number of species as the secondary site, with 40% less turnover than the secondary site. Their model would also suggest that there are fewer microsites with associated species in primary forest, a scenario that would seem at odds with the spatial variability generated by gaps, the overstory, pit-and-mound topography, and other microtopographic gradients (e.g., Bratton 1976; Peterson et al. 1990). We can dull Occam's razor with further scenarios on how nature might work, but I would welcome a demonstration that the plot size is inappropriate or the sample size insufficient, using actual understory herb data from the Appalachians.

We chose 1.0-m<sup>2</sup> plots to optimize plot and sample sizes. This size is widely used in herbaceous studies (e.g., Oosting & Billings 1939; Whitaker 1966; Brewer 1980; Hicks 1980; Phillips & Shure 1990; Boring et al. 1981; Davison & Forman 1982; Reader & Bricker 1992; DeMars & Runkle 1992). If we have sinned, I believe we are in excellent company.

Elliott and Loftis are joined by Steinbeck in expressing concern that by avoiding sampling in *Rhododendron maximum* areas, the samples are "not random as claimed . . ."; that analysis ". . . depends on the difference in *R. maximum* composition . . ."; and that "not sampling the entire community will bias the sample." First, the study looked at understory herbs, not at *Rhododendron*. *Rhododendron* and herbs are almost mutually exclusive in coves and have long been considered two distinct communities (Oosting & Billings 1939; Oosting & Bourdeau 1955). Second, our sampling was random within the herbaceous areas we sampled, but we never claimed it was random with respect to any wider areas, nor does there appear to be any reason that it should be.

Contrary to Steinbeck's claim, we

did not use our similarity ratios to compensate for lack of sampling throughout the growing season. We used the ratios to reduce bias caused by differences in altitude and exposure between sites. The ratios might then have shown a pattern of recovery of secondary sites when none was evident in the untransformed data. This was not the case.

Both sets of authors claim we should have sampled repeatedly throughout the growing season. Had we the resources, we might have done so. With limited time and funds, we chose to concentrate on the spring herbaceous community for which the coves are known (Braun 1950). Elliott and Loftis present the hypothesis that summer or fall herbaceous diversity may actually be greater in secondary than in primary forests. I would encourage someone to test this.

Both sets of authors feel that we cannot generalize from data extending almost 90 years to what may happen several centuries into the future. On the other hand, Elliott and Loftis generalize that forests become primary at 200 years.

Our data show no sign of recovery for sites up to almost 90 years old. Ninety years is 60% of the longest harvesting cycles used by the Forest Service in coves (USDA/FS 1986) and more than twice the shortest. When or even if recovery occurs is speculation. I believe that our hypothesis that recovery takes at least several centuries is far more realistic than invoking some ecological *deus ex machina* to transform the 45–87-year-old secondary forests we studied into the ecological equivalents of primary forests within a 150-year harvest cycle. In the meantime, the facts suggest that clearcutting of southern Appalachian forests is not in compliance with the 1976 National Forest Management Act.

There are endless possible objections that might be lodged against any research project, including ours, but, in the absence of data, these remain abstract arguments. I welcome

these arguments if they are precursors to investigations that will appear in the refereed literature, and I invite our fellow researchers, especially graduate students, to consider eastern primary forests as rich subjects for the study of biodiversity.

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