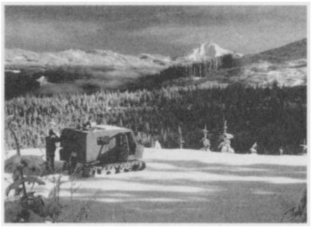

CONTRIBUTIONS

Commentary

Postscript on an Ecological Giant: Frank E. Egler

It has been over a year since the death of Dr. Frank E. Egler, a true giant of ecology. The Resolution of Respect written by Robert Burgess (1997) was a thoughtful commentary on an individual with an unusually broad background, not only in the physical and biological sciences, but in the humanities as well. I had the privilege of working with Frank after I moved to Connecticut in the early 1950s. It was a sort of post-doc experience: we battled the indiscriminate right-of-way vegetation "brush" sprayers and chemical companies of the 1960s and collaborated on a series of studies on the vegetation of Connecticut's natural areas. It was a maturing, eye-opening time. While we surveyed the Yale Natural Preserve in New Haven (Egler and Niering 1965), I learned that as a graduate student, under G. E. Nichols at Yale, Egler had reluctantly helped *plant* the missing "climax" trees (eastern hemlock). Although we observed the successful growth of those trees with fire protection in the early 1960s, Egler felt that various oaks and other associated hardwoods of the Central Hardwoods region represented the regional forest type in New Haven. Egler indicated that, under Nichols, he almost failed to receive his Ph.D, since his views about vegetation dynamics differed from the traditional ecological thinking at the time. His dissertation concerned the regional forest vegetation of the lower Berkshires (Egler 1954), the *real* Hemlock–White Pine–Northern Hardwoods forest region.

Following his early critical review of current ecology textbooks (Egler 1951), he set out to write his own (Egler 1977), which unfortunately was never commercially published. Since he could not find a publisher who would accept his format, the 527-page opus was printed by photo-offset from his typewritten copy and distributed to the major libraries of the world and to a few of his friends. Only 450 copies were printed, of which I have a cherished example, Number 4. Few ecologists have been exposed to this volume, but those who have are impressed with the quality of his prose and his holistic understanding of vegetation science. I hope that this opus will become more widely available; it will be especially useful to the younger members of the ecological community, who may have missed having a brush with this man of such insight and brilliance. In one of his early papers, "Vegetation of Southeast Oahu, Hawaii" (Egler 1947), he states:

> Plant succession *is deservedly one of the very creditable developments of students of American vegetation. In this study of Oahu, however, the writer prefers to use the term* vegetation change, *so as to embrace any and all kinds of temporal alterations within and between communities. The term* Succession, *in the minds of some, appears to denote a sucession of step-like metamorphoses from one association to another. Furthermore, the retrogressive–progressive agreement makes it necessary for one to know whether he is "coming" or "going," a stand which the writer cannot always take for Oahu, and which others usually settle more by faith than by empirical knowledge. The* climax, *and God, have certain things in common for certain botanical atheists. To paraphrase Julian Huxley, the writer does not believe in the climax, because he thinks the idea has ceased to be a useful hypothesis.*

I am repeatedly amazed at the number of my contemporaries who have never been exposed to such insight, which was set forth so early in the ecological literature. Ecologists today are accepting a more open-minded view of the concepts treated by Egler, including that of the "flux of nature."

Frank Egler was an ecological maverick ahead of his time, who antagonized some of his colleagues because he could not stand mediocrity. He was a genius who often found it difficult to deal with most people, and he was constantly posing questions for which answers were not readily available. But isn't that what makes ecology interesting?

His book, the *Wild Gardener in the Wild Landscape*, was written under the name Warren G. Kenfield

(Kenfield 1966) and is a classic in the basic vegetation science dynamics of old-field landscapes and the introduction and management of stable vegetation types. Those of us who were in on the secret of Egler's pseudonym (an anagram) found it amusing that there was even a biography of Kenfield on page 231. He simply did not want to be interrupted by the general public, and was always "out of the country" when contacted by the publisher (Hafner). Fortunately, this fine book is now available from the Connecticut College Arboretum in the second revised edition.*

Throughout his life, he lived on and expanded his family home lands (Aton Forest) in northwestern Connecticut into a 450-ha (1,100-acre) natural area preserve and field research area.

With the establishment of Aton Forest Inc. and the Aton Forest Fellowship Trust, it is anticipated that a sizable endowment will continue to promote the holistic type of ecology (including humans) which he fostered over his long and productive career. He has left a remarkable legacy in the old fields and woodlands at Aton Forest, where long-term ecological processes can continue to be documented and studied. Frank Egler left the world better than he found it, by acquiring and protecting a legacy of "natural" and managed ecosystems where future scientists can attempt to understand the systems he felt were "not more complex than we think, but more complex than we can think."

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*Available for $25.95, plus $3.00 shipping and handling. Connecticut College Arboretum, Connecticut College, New London, Connecticut 06320.

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Mostly A Misunderstanding, I Believe

I read Dale and Van Winkle's (1998) reaction to my "editorial" (Aber 1997) on a lack of rigor in ecological modeling with much satisfaction. The points of agreement greatly outnumber the points on which we disagree. It seems that the crux of the disagreement derives from a misuse of language on my part that can be easily corrected.

Dale and Van Winkle open by stating that "belief" in models is an inappropriate goal, in that belief implies acceptance on faith or trust, rather than on compelling information. That was a surprising definition of the term to me, but, as it turns out, one supported by *Webster's.* I agree here that accepting models (or choosing not to) without critical evaluation is at the heart of the problem presented by modeling in ecological research.

The list of statements to which Dale, Van Winkle, and I would all ascribe seems to include: (1) the value of increasing rigor in the process of publishing models, (2) the advantages of taking a minimalist approach by using the simplest model that proves "adequate" (as well as agreement on the difficulty of defining "adequate" in a general way), (3) the fact that a model represents a set of working hypotheses and assumptions about the important interactions within a system, (4) the value of models that "fail," and (5) the value of documenting the modeling process.

I would also agree with two additional points made by Dale and Van Winkle, which they expressed as possible areas of disagreement. These include: (1) that models are never complete and never represent perfect knowledge of the system, and (2) that sources of uncertainty need to be understood and presented in papers. Indeed, it is the frequency with which models are presented that match observed data exactly (which can only occur with negative degrees of freedom and a lack of rigorous validation, as discussed in my original letter) that causes the largest rift with field scientists, who know that the unknowns are substantial and important.

I can detect only one area in which there might be an important difference in the approach to modeling expressed in my letter and that of Dale and Van Winkle (1998). That is in the value of the modeling process in the absence of substantial quantitative information. Dale and Van Winkle suggest that "The empirical information for rigorous calibration or validation commonly is not available," but then go on to describe the value of the modeling process in assisting scientists in "sharing their expertise to develop a simulation model." Two things trouble me about

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