stay there to breed; females change breeding sites more often. Chapter 38 presents a life-table, an estimate of 35% annual adult mortality (expected lifespan upon achieving adulthood is 2½ years), and an estimated mortality of young birds in their first winter of about 60% in 8 months. Chapter 39 delineates the insectivorous diet of the warblers (beetles are a mainstay of the breeding adults while lepidopteran caterpillars are major items for the young). Chapter 40 describes miscellaneous behaviors and Chapter 41 presents plumages and molts.

Despite the wealth of information presented, I am disappointed in this monograph for two main reasons. 1) The order of the chapters did not flow smoothly. Nesting biology is described chronologically through Chapter 28. The next chapters (on territories and mating systems) seem to belong earlier, and I found myself waiting impatiently for the discussion of matters in these chapters that were hinted at in the chronological presentation. Similarly, site fidelity (Chapter 37) would seem to fit better in temporal sequence, perhaps near Chapter 5. Chapters 33 and 38 should be adjacent to each other. Reasons for this organization may be historical (the book was written over a period of about 10 years), but the result is nevertheless disconcerting and, to me, disruptive to understanding a coherent story. Cross-referencing is generally good, but in a few cases is missing. For example, on pp. 11–12, habitat partitioning of species coexisting with prairie warblers is discussed with no reference to the later (pp. 56–59) discussion of interspecific aggression. 2) The material presented is seldom adequately related to its ecological framework and much relevant literature is ignored. Thus, polygyny in prairie warblers is not compared either to available theory or to that of other birds. Variations in feeding rates or growth rates are not discussed, nor are the consequences of different responses to cowbird parasitism, to name just a few of the possibilities. What is the biological significance of patterns of "atteniveness," of which ornithologists seem so fond? With 518 pages of densely packed information in the text itself, extensive discussions were undoubtedly out of the question but the decision to omit these considerations altogether was unfortunate.

In short, this is an ornithological treasure, full of gems of observation (such as the apparent preference of predators for nestlings rather than eggs, which operates to the disadvantage of the early-hatching cowbirds) and thoughtful, careful analyses of data. Ecologists will find it useful as a data source for their own comparisons. Many should read it as an object lesson in patience and perseverance in the field and clarity of data reduction and presentation.

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Succession^1 An Unfinished Revolution

Succession, one of the oldest shared research commitments in ecology, can be thought of as having gone through four periods, during each of which the concept has often been profoundly modified. The first period between 1859 and 1900 was formative, during which most of the architecture of the theory was laid out. This was followed by a developmental and elaborative period from 1900 to about 1925 or 1930, dominated by Clements and Cowles. During this "classical" period the details of the concept were worked out and many anomalies resolved or at least discussed. Also a radically different view arose at this time in the ideas of Gleason but it was not understood. This is still largely true even today. The period 1930 to 1947 might be called the scholastic interval. After the somewhat arbitrary date of 1947 there was a major loss of faith in the traditional ideas of succession. This seems to have resulted (at least in part) from a change to a generation which had not been raised in the early 1900's viewpoint of biology and ecology. What resulted is the confused period in which we still find ourselves. This is a time in which it is hoped that by taking a contemporary idea, as for example an ecosystem, and retaining some traditional ideas as for example "community development" that a suitable compromise theory of succession will emerge. Needless to say it has also given rise to a reasonable amount of fatalism and cynicism. I suspect we may be in the beginning of a fifth period and hence the title of this review "an unfinished revolution." This period will see the development of a new view of community dynamics profoundly different from succession.

The Benchmark series sets as its goal to assemble papers of distinguished calibre which have contributed to the development of a specific topic. These collections are held together by the viewpoint of the editor(s) as presented in the introductory comments to the papers. This is necessary since the history of all subjects is too rich in detail to be simply passive records of developments. Reviews of books of this type of necessity become appraisals of the editor's organization and viewpoint as they contribute to an understanding of the historical and philosophical development of the concept under consideration. Golley's approach and organization gives no more than a pedestrian insight into the concept. Succession is in a very precjarous state today. An historically perceptive collection of papers and a penetrating analysis of them would have served contemporary ecologists better in understanding and modifying the concept.

Golley divides the collection of papers and excerpts from books in ecological succession into: the precursors, the pattern, mechanism, interpretation and community stability. "The precursors" was for me the most disappointing part of the book because it contained only one short excited paper of W. M. Davis on geomorphological cycle. Golley correctly identifies in the preface the need to understand the social, cultural and scientific milieu in which the early ideas of succession developed. Furthermore he identifies the three major influences on the concept: the uniformitarianism and dynamics in geology, evolution and the community idea. However in total this section falls far short of giving us the historical and philosophical roots of succession.

The kernel of the idea of succession is obviously evolution yet Golley does not discuss this in any detail. Specifically succession as formulated in the works of Warming, Cowles and Clements was modeled after neolamarkian evolution. This is not generally recognized today since the almost universal abandonment of these kinds of arguments in the 1930's.

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Certainly the value of the "Precursor" section would have been greatly enhanced by even short quotes from the evolutionary ideas of Herbert Spencer, Ernest Haeckel, E. D. Cope, J. Le Conte or any of many others from the end of the last century. For example, Joseph Le Conte, a California geologist, in a book entitled Evolution (1897) writes:

"The courses of change or adaptive modifications or the factors of evolution, are at least four well known, and probably many more still unknown: 1. The physical environment—heat and cold, dryness and moisture—affects function of organs and function affects structure and both changed function and changed structure are inherited by offspring..."

and Henry Cowles in this same tradition says in his classical 1899 paper on sand dune succession:

"The ecologist, then, must study the order of succession of the plant societies in the development of a region, and he must endeavor to discover the laws which govern the panoramic changes. Ecology, therefore, is a study in dynamics. For its most ready application, plants should be found whose tissues and organs are actually changing at the present time in responses to varying conditions. Plant formations should be found which are rapidly passing into other types by reason of a changing environment."

Similar statements can be found in Warming's 1895 German edition of his text on ecology. Of particular note is that most neo-lamarckian statements have been removed or softened in the 1909 English edition, which almost everyone reads today.

The ideas of E. D. Cope also bear a striking resemblance in outline to the idea of succession as expounded by Clements. Evolution to Cope was a progressive change in organisms, stimulated by environmental stresses that generally tend to complicate their form and function. New species evolve by deflection, acceleration or retardation of the process of developmental changes. This results in terminal addition or subtraction of stages.

Knowing the widespread belief in the analogy between organism development and evolution to social levels of organization as e.g. by Herbert Spencer and of course F. E. Clements, the generalization of neo-lamarckian ideas from organisms to plant and animal communities was obvious and not particularly novel. In fact it reminds one of the numerous implicit thermodynamic arguments in ecology today.

Part II, "Pattern," consists of five papers or parts of papers: H. C. Cowles' summary of sand dune succession in the Physiographic Ecology of Chicago, W. S. Cooper's primary succession study at Glacier Bay, one of the early papers on old field succession by W. M. Crafton and B. W. Wells, V. E. Shelford on succession in pond fish and finally E. P. Odum on organic production and turnover in old field succession. The intention of Golley in this second part is to introduce examples of succession in different habitats.

It has always interested me that ecologists seem to take pride in considering themselves to be strict empiricists. Consequently a statement such as Golley's in this section, that the stages of succession are especially obvious in certain habitats, appeals to this bias. However the time required for succession to "occur" is long enough that it precluded direct observation of it. Thus observation and measurements of succession cannot be made without recourse to the theory to provide instruction in data collection and interpretation.

The difficulty, therefore, with "Pattern" is that it gives the impression that data which shows succession can be collected by neutral, theory-free observation. The papers in this part do not represent agnostic descriptions of vegetation change but they incorporate the basic assumptions of that successional theory used so that data can be gathered to reveal the nature of things from the standpoint of that theory.

This is well demonstrated in the selected papers by Cowles and Cooper. One of the characteristic means of studying succession has been to relate time sequence to more easily studied geographic sequences. Today we know this time-space "conversion" as a kind of ergodic hypothesis from stochastic theory. Cowles and Cooper adopted it primarily from the physiographic cycle ideas of Davis. Davis' erosion cycles fit different parts of the landscape into a landform evolutionary sequence. These ecologists then simplified the vegetation on these different stages of landscape evolution into a successional sequence using the temporal ordering of the geologists.


The title of this section is certainly appropriate because the concept of succession has always tried to give a mechanistic interpretation. Cowles and Clements had hoped, as the quote from Cowles had already indicated, to establish a connection for the cause of succession by the environmentalism arguments of neo-lamarckian evolution. Consequently early ecologists had great faith in morphological and physiological being plastic in the face of the environment. This is illustrated in Clement's concepts of ecads, indicators and phytometers. However these attempts fell short of the desired goal. This was not because they did not have the right idea; certainly the causes or processes of succession must relate the dynamics of the environment to the evolutionary adaptations of the species. They did not succeed because the evolutionary explanation was not appropriate. In fact only in the last two decades has ecology been able to incorporate in a meaningful manner the advances of genetics of the last 50 years.

The papers by Oosting, Humphrey and Keever which start this section, bring us into the investigations of the mechanisms of succession after the loss of faith caused by the abandonment of the neo-lamarckian paradigm. The vision that the early 1900's ecologist had of the organization of communities by "evolutionary morphology and physiology" had faded. Instead we are treated in these two papers to the frustrated hope that by detailed study and observation the process behind successions will be revealed. Why were not these papers preceded by ones by Livingston, Ganong, Transeau or others which would have come from the classical period in which there was believed to be a central viable mechanism?

The remaining two papers are largely outside the classical theory of succession. They show the eclectic nature of the contemporary idea of succession and how this freedom from most of the classical views allows different cognitive commitments. This allows Rice, Pancholy, Marks and Bormann to collect data with different assumptions from the classical model and thus to "create" new and often disturbing explanations (at least from the view of the classical model). I would have liked to have seen also a paper such as L. Forcier, Science 189:808-809 (1975), which incorporated life history strategy explanations as the cause of succession. This is surely the modern synthesis.

Part IV: "Interpreting," gives six papers: two by Clements on succession and the nature of climax, Gleason on the individualistic concept, Whittaker's criticism of the climax theory, Odum's strategy of ecosystem development and Druey and Nisbet's critical evaluation of succession. This part presents at least three variants of the theory of succession. However, Cooper's 1927 paper on "Fundamentals of vegetation change" and A. G. Tansley's 1935 paper on "The uses and abuse of vegetation terms and concepts" (the ecosystem paper) are surprisingly not present. Also the more
contemporary relational theory of pattern and process by C. G. van Leeuwen (\textit{Wentia} 15:25–46) is not included.

All of the contributors to the idea of succession none have had such a continued influence as Clements. However he is even more misunderstood than his dialectic partner Gleason. Contemporary ecologists cannot read Clements and hope to understand him. He is a product of another age, he speaks a different language and expresses a different cognitive commitment, appropriate for a different world view. It is a view of typology vs. populations, neo-lamarckian vs. mendelian evolution, organismic vs. systems approaches, is aristotelian vs. hypothetical-deductive methods and linguistic vs. mathematical descriptions. It is therefore again a disappointment that Golley has not more penetratingly introduced the reader to what Clements is going to say and why Gleason’s ideas are such an anathema to them.

Contemporary views of succession in terms of species diversity, productivity, niche width, etc. (e.g., Odum) are foreign to Clements’ theory. He believed consistently in his assumption of how communities are put together, that the community is a natural organismic unit.

The individual species in a community are held together by the social bonds of the plant dominant(s). The dominant is sufficient to describe and understand the community because it causes the occurrence of the other species. Succession is the slowly revealed potential of the community by the changing dominants until the community reaches a point, the climax, in which its full potential under a particular environment (particularly climate) is developed.

The Clementsian concept of succession by dominants is a form of Aristotelian essentialism. Succession has over the last 30 years attempted to divest itself of this kind of essentialistic definition and as a result has become interested in the relationships and organization of all species populations. This is certainly a more Gleasonian viewpoint.

Although I found the Drury and Nisbet paper interesting, I think it should have been better introduced. It cannot but raise serious questions as it proposes to show that contemporary data do not support succession. However, given the confused state of succession theory and the eclectic theoretical origins of their data, I simply do not know what to make of it. I would include this paper only if I warned the reader that its sweeping suspicions of the reliability of succession and the alternatives it suggests are the rumblings of a change in paradigm.

Part V, “Stability,” is very confusing to fit into the rest of the collection. It contains Lewontin’s paper on the meaning of stability, MacArthur’s on food web complexity and stability, Hurd et al.’s on trophic level stability and diversity, Louck’s on diversity, efficiency and stability and May’s on the stability of large complex systems.

Disturbance has certainly been the nemesis of classical succession because it has introduced heterogeneity and lack of compositional stability. For classical succession disturbance of some kind resulted in the development which was the phenomena of investigations. However for succession to return to the climax or stable stage, relatively long times between disturbances were assumed. This was usually considered to be thousands of years. If disturbances became too frequent then the whole idea of a stable final stage where certain assemblages of species stay relatively constant was difficult. At first subclimax and other terms were introduced to try to handle these problems. A. Watt in 1947 faced up to the recurrent nature of these patterns. Since that time slow but steady progress has been made to deal with the heterogeneity of the environment. Golley gives us none of this work. Instead we get only one paper on disturbance and several which explore the meanings of stability from a theoretical and ecosystem approach.

In conclusion I must say that I found Ecological Succession a tired record of events with none of the tension, excitement or controversy that has and does exist. Most of all there was none of the anticipation of the dramatic changes that are beginning to happen. Consequently the book is an unfortunately lost opportunity.

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**Mathematical Bioeconomics**

Colin W. Clark has written a book principally aimed at professionals in the area of the economics of conservation of productive resources. I am pleased to report, in light of the tautness of this review, that it is not likely to go out-of-date for some time.

Prospective purchasers and/or readers should be aware that Mathematical bioeconomics was not made part of a series of mathematics books, nor assigned the first word of its title, lightly. Familiarity with differential equations, and their graphs, is assumed from the beginning.

Topics considered include population growth models; the economics of open-access fisheries; the effects of inflation and interest on the capital investment necessary to harvest; applications of optimal control theory; the theory of taxation as a means of controlling harvesting; the effect of supply and demand on exhaustible resources; the theory of dynamical systems, with applications to competition and predation; consideration of the effects of delays, by use of difference equations; growth and aging; harvesting of interacting species, either independently or together.

The book would serve as a text. It is provided with summaries, examples, and problems. There is a bibliography, and also suggestions for supplementary reading. The index seems adequate.

Clark’s heart seems to be in theory, and in fisheries theory at that. Yet, he appears to be a realist. There are many examples of real-world situations, such as the anchovy crisis of 1973 and the attempts of the International Whaling Commission to control whaling. He is continually stressing optimum, and seems quite conversant with real and theoretical economics. Biological considerations, such as species interactions, life history differences, and the possibility of extinction, are not overlooked. Although he is not opposed to the use of computer simulations, and has discussed them to some extent, his attitude, which seems sane, is that “from the scientific point of view the results of such [computer simulation] exercises at best serve as illustrations of a general theoretical framework” (p. 210). This is probably why he has neglected