bles of interest such as the probability of identity between two genes or measures of linkage disequilibrium. These variables can change each generation by a series of steps including mutation, random drift, and intra- and inter-chromosomal crossing-over in that order. After each step the expectations of the variables are written in terms of their values after the previous step. By algebraically combining these equations, we find an approximation (it can no longer be called an expectation) of the change from one generation to the next, from which equilibrium values can be obtained. There is no attempt, except by computer simulations in some cases, to determine the stability or uniqueness of these equilibria or the rate at which they are approached. The number of repeats per chromosome is assumed to be constant, so that each unequal crossover that produces a duplication of one gene must be immediately followed by a deletion of one gene. Unequal crossovers where the two chromosomes are m genes out of register is assumed to be equivalent to m unit cycles as above. The complexity of the theory necessitates these simplifying assumptions.

I had some difficulty with the terminology and definitions and had to rely on the diagrams and equations to determine the meaning of some terms. Thus "family" refers to those related genes on one particular chromosome, with "chromosome" and "family" used interchangeably. The word "lineage" refers to a particular allele. Thus, paradoxically, a gene becomes a different lineage from its ancestors when a mutation occurs. A "family type" identifies a particular group of lineages. Potentially more serious is the ambiguous definition of the fundamental variable, $x_{i,k}$, used throughout the book. In the original paper, this variable was defined as the frequency of the kth lineage in the ith chromosome. However, chromosome labels are ambiguous when crossovers between homologues are considered. Therefore, $x_{i,k}$ has now been redefined (p. 23) as the frequency of the kth lineage in the ith family type. As such, $x_{i,k}$ must be constant through time; only the frequencies of the family types (p_i) are variable. Yet throughout the book $x_{i,k}$ is treated as a variable with quantities such as "$\Delta x_{i,k}$" and the variance of $x_{i,k}$ taking nonzero values. The meaning of this enigmatic variable must be clarified before the theory can be understood.

Finally, it is necessary to ask whether it is advisable to use population genetics as a means of drawing inferences concerning genetic mechanisms, as Ohta does here in comparing the somatic and germ-line hypotheses for immunoglobulins. In most cases, the uncertainties involved in population genetic modeling are so great that it is more efficient to learn the genetic mechanism by other means and then use this information in drawing inferences concerning population genetics. There is also a much broader question along these lines. At what point is our understanding of a given biological situation sufficiently deep to justify the development of such an intricate mathematical theory? This question constantly plagues molecular population geneticists, and it is especially troublesome here with new discoveries concerning multigene families being developed at a breathtaking pace. Theoretical work in this area might be more useful after the experimental facts are better understood.

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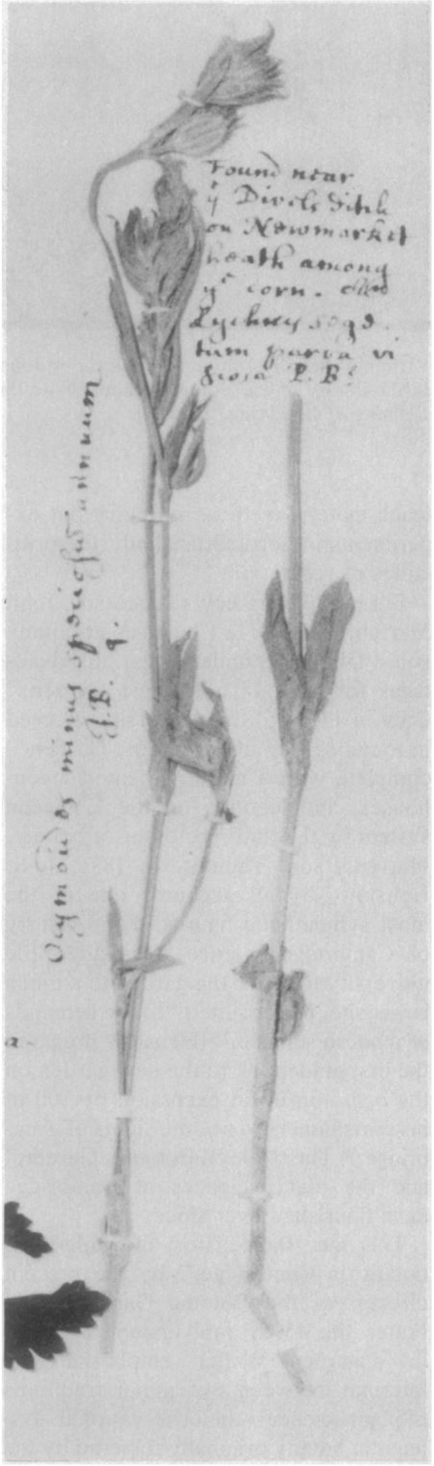
A Botanical Garden

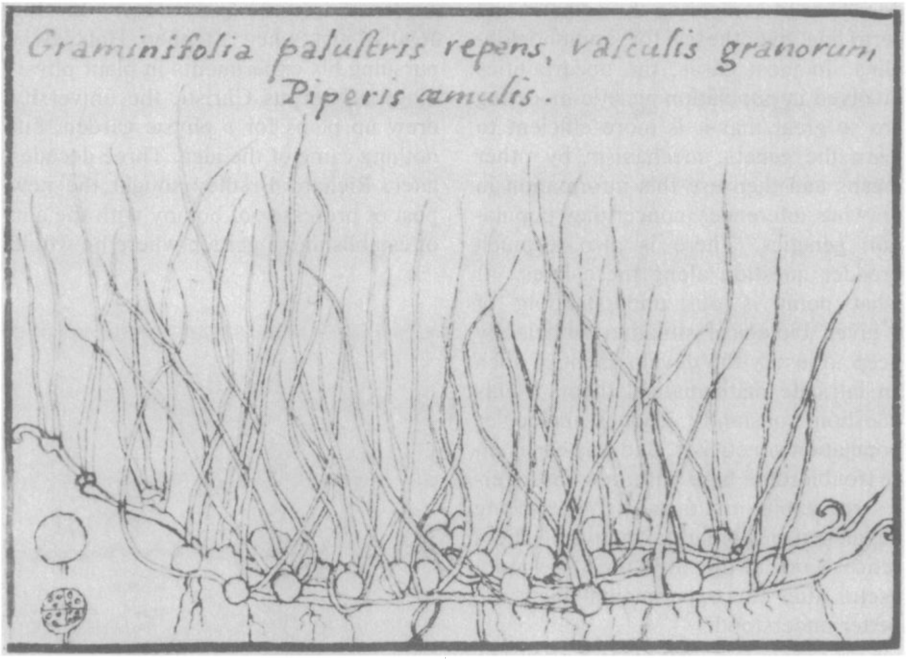
The Shaping of Cambridge Botany. A Short History of Whole-Plant Botany in Cambridge from the Time of Ray into the Present Century. S. M. WALTERS. Cambridge University Press, New York, 1981. xvi, 122 pp., illus. $42.50.

Advice to weary travelers: seek out botanical gardens. They are quiet, beautiful, instructive—and they usually have benches. It is a pity that they so often lack guidebooks. The plants are labeled, but visitors are hard put to find out much about a garden's history or the contributions to science made there. Cambridge University's Botanic Garden is now a happy exception.

Cambridge was late in establishing a botanical garden, though not for want of people willing to try. Most European universities of any note had set up gardens (as part of humanist-inspired reforms of medical teaching) between the mid-16th and mid-17th century; Oxford started its garden in 1621. The first attempt at Cambridge came in 1588 when the herbalist John Gerard recommended himself to the university as a suitable person "for planting of gardens." A century later, the great botanist John Ray lamented the lack of a botanical garden at Cambridge; had he not been forced to leave the university (on religious and political grounds), it is likely that he would have tried to set one up to complement his researches in the fields and gardens around Cambridge. In the 1690's, just when Stephen Hales was pursuing his experiments in plant physiology at Corpus Christi, the university drew up plans for a physic garden, but nothing came of the idea. Three decades later, Richard Bradley sought the new post of professor of botany with the aim of establishing a garden where he would



"Specimen of the cornfield weed *Silene gallica*, found near the Devil's Dyke, the only Cambridgeshire specimen known in Ray's own Herbarium." [From *The Shaping of Cambridge Botany*]



"Unpublished illustration of *Pilularia* by John Martyn, preserved in a bound manuscript Flora in the library of the Botany School. Note the cumbersome pre-Linnaean name." [From *The Shaping of Cambridge Botany*]

teach materia medica and carry out experiments in horticulture and "the principles of vegetation."

But in 1735 Bradley's successor, John Martyn, gave his last lectures in botany still "labouring under great disadvantages for want of a Botanic Garden." Only in 1762 did the university succeed in founding "a public Botanic Garden," complete with a curator, heated greenhouses, and lectures on the Linnaean system by the third professor of botany, Martyn's son, Thomas. In 1831, J. S. Henslow—by all accounts one of the most sympathetic figures in the history of Cambridge science—persuaded the university to move the garden to a much larger site, better suited "to the demands of modern science." (Henslow prepared the first guidebook to the new garden on the occasion of an excursion by 200 of his parishioners to see the sights of Cambridge.) The "New Botanic Garden" and the plant sciences at Cambridge have flourished ever since.

This fine "short history of whole-plant botany in Cambridge," by the present director of the Botanic Garden, celebrates the 150th anniversary of Henslow's garden. Walters emphasizes the interplay between two major traditions of plant science—the observational, systematic botany originally fostered by the demands of medicine, and the experimental and applied physiology favored by Bradley and Henslow—and shows how the proposed and actual gardens at Cambridge accommodated both. He tells the story very well, using unpublished archives to good effect, and illustrates it handsomely. This would be the ideal book to take along to the garden but for one astonishing omission: there is no plan of today's garden.

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A Framework for Systematics

Systematics and Biogeography. Cladistics and Vicariance. GARETH NELSON and NORMAN PLATNICK. Columbia University Press, New York, 1981. xiv, 568 pp., illus. $35.

Ask systematic biologists to summarize their studies, and most will likely draw branching diagrams. Such diagrams work as summaries because not only characters of organisms but also relationships between them are best understood in hierarchical terms. Though methodological schools may be characterized by what their diagrams summarize—similarity in phenetic dendrograms, genealogy in Hennigian cladograms, and the full glut of historical miscellany in a Darwinian traditionalist's tree—these differences perhaps hide a basic unifying theme. This observation is hardly novel, but in this book Nelson and Platnick provide probably the first study of its implications: If all branching diagrams are summaries, how do the various kinds summarize? Is there a general framework for thinking about branching diagrams? If so, might it permit evaluation of the effectiveness of the kinds of diagrams as summaries? In addressing these questions, the authors are led to examine the fundamental nature of the systematic process.

Despite the book's subtitle, Nelson and Platnick do not find their general framework for systematics in classical Hennigian cladistics. Their system is similar and uses the same terms, but it is fundamentally different in being nontemporal and without historical predicates. Cladograms become diagrams that "depict structural elements of knowledge," rather than show the sequential order of speciation events; synapomorphies are not shared derived characters but instead are pieces of information that define a group of taxa. For the authors, systematics is the resolution of conflict among definable groups on the hypothesis that the world is ordered and only one self-consistent set of groups (nameable in classification) exists for a given collection of taxa. If this hypothesis is true, then conflict results from groupings of taxa that are mistaken, either because different characters in some taxa have been called the same or because the presence of a character has been overlooked in other taxa. This simple view of systematics as a careful sifting of evidence should appeal to practicing taxonomists, since it corresponds to what they spend their days doing.

In their review of the history of systematics, Nelson and Platnick find pre-Darwinian taxonomic practice to fit their view of the discipline despite the disparate theoretics. They examine more thoroughly how suitable their model might be for understanding post-Darwinian practice. Using straightforward examples as much as formal argument, they treat the relationships between cladograms and phyletic trees for two, three, and four taxa (they suggest that problems involving more taxa can be reduced to series of three-taxon problems). The choice of a tree that best summarizes a given set of data turns out to depend on the same factors used in choosing a best-summarizing cladogram; "cladograms and . . . trees, therefore, seem merely to imply alternative strategies for arriving at the same result" (pp. 214–215). Most realistic situations, with conflicting information on group membership, seem to require for resolution assumptions about the future sampling of characters. To resolve conflicts between trees, for example, is to assume that the relative frequency of characters present in two or