**Chapter 10. Summary**

In support of the thesis that abundances in natural communities follow the logistic-J distribution, I have brought forward evidence of five kinds:

1. An extensive study of 125 randomly selected biosurveys from the field literature that found the logistic-J distribution (and no other) present,

2. A stochastic species hypothesis embedded in a family of dynamical systems all of which produce the logistic-J distribution (and no other),

3. A demonstration that natural communities obey the stochastic species hypothesis in the long term,

4. Successful prediction, in the statistical sense, of maximal abundance in natural communities

5. Evidence of the logistic-J distribution in evolutionary patterns of taxonomic abundance.

The analysis revealed the presence of the logistic-J distribution in field data, simultaneously excluding previous proposals. The dynamical systems, based on birth/death probabilities, produce the logistic-J distribution and no other. Moreover, I have presented arguments in favour of the long-term equality of these probabilities in natural populations. The predictive powers of the theory are illustrated by the successful predictions of maximum abundance in samples by the parameter $\Delta$ in a nontrivial way. Taxonomic abundances, as explored in Chapter 8, also appear to follow the logistic-J distribution as a manifestation of that distribution in ancient communities.

On the negative side not only did the metastudy exclude other proposals for species abundance distributions, but it was clearly demonstrated that the logistic-J distribution, when subjected to a logarithmic transformation of the abundance axis, will produce truncated, bell-shaped unimodal distributions. In view of this fact and in the absence of more sensitive tests of samples having this shape, researchers may no longer claim that this shape indicates the presence of the lognormal distribution in the data. Indeed, the author has proved that the “veil curve” is not a vertical line but a sigmoidal curve with a formula of its own. When subtracted from the sample distribution, another lognormal curve with a distinct tendency to have very low values at abundance 1, must perforce emerge. (Dewdney 1998b)

Logistic-J theory has an exact form that makes new ecological tools available for theory and practice. These include the use of basic computer tools that generate specific distributions, test goodness of fit, and simulate the sampling process exactly. These tools form the basis for application programs that predict richness of communities based on sample data, produce canonical sequences, and calculate (true) sample overlap. Mathematical tools include the general theory of sampling, analytic tools for stochastic systems, formulas for solving logistic-J equations, and formulas for rarefaction curves under replacement and non-replacement sampling.

The mathematics involved in these developments is all at the college level: a one year course in the differential and integral calculus, as well as elementary probability theory and elementary algebra. Slightly more advanced topics include distribution theory and basic real analysis, and elementary algebra. With the exception of the perturbation theory appealed to in Chapter 7, as well as a few applications involving convergent sequences of real numbers, the topics should be within the grasp of any student or researcher who has (what used to be, at least) the standard mathematical background for undergraduate science degrees.

**10.1 Research Methods**

The key ingredients in the research described in this monograph are the methods and formulas developed up to this point. Principally, these include the theoretical tools developed here, along with an experimental milieu in which definitive experiments can be carried out. These are not just "models" of what goes on when one takes a sample, they are precise descriptions. The program SampleSim, for example, produces reliably typical samples of any distribution whatever, as long as the user doesn't mind typing in all the abundances.

A third factor of critical importance in the exact approach employed in this monograph is the huge mass of biosurvey literature that presently languishes on library shelves and in relict databases. This is a resource akin to gold and it awaits mining of the invaluable data it contains. That literature was the source of the confirmatory approach used in the metastudy reported in some detail in Chapter 7. Other uses include a host of studies wherein for many of these datasets, one can apply the methods of this monograph to arrive at reasonable estimates for richness of source communities, whether terrestrial or aquatic, macroscopic or microscopic.

**10.2 If one can't be wrong, one can't be right**

Throughout this book I have declared that worked examples serve merely to illustrate the theory, rather than to confirm or support it, as if the tests and all the programs that have been written to implement them had no other purpose. This is far from the case early in any research program that brings logistic-J theory to bear on a specific problem.

At every stage in my own inquiries, the programs were used as a check on the theory's development. One often takes wrong turns when developing formulae, for example. Logical errors, incorrect assumptions, and algebraic slips all bedevil the process. On the way to developing a formula, one passes through various stages where interim tests can be performed. The testing process is relatively simple, in principle. If the formula or method fails in a mere handful of examples, the researcher backtracks through the development process to discover the error.

The process just outlined is assuredly not a matter of just altering the theory until it works, so to speak, as if some kind of fudging process were involved. Sometimes a basic assumption at the entry point of the idea turns out to be wrong and the entire approach must be abandoned. The error is fatal. This has happened several times in the course of the developments presented in this book. But if the critical intermediate stages in the development of a particular approach are all confirmed in this manner, the last stage is usually purely mathematical and in this stage one simply searches for mathematical errors before accepting the theory as correct. At that point, one already knows.

I have claimed that previously used methodology in the analysis of various proposals for abundance distributions has been based on a misconception of the statistical behaviour of field samples. Owing to normal variation within abundance categories, whether within the community itself or originating in the sampling process, more than a few such samples are required to establish the presence of a particular theoretical distribution proposal; samples from a community that seem closer to Proposal A today may well favour Proposal B tomorrow. In fact one may predict, sight unseen, that such an outcome would be a regular occurrence. Thus any conclusions based on an inadequate review of biosurveys rely largely on coincidence and tell us nothing about underlying shapes.

Another methodological problem has to do with the apparent abandonment of appropriate measures of how well field data fit proposed abundance distributions. In a field populated by researchers keen to quantify their data, the nearly complete lack of goodness-of-fit tests (See the end of Section 1.2.) presents something of a puzzle. Quite apart from their role in rejecting fits, such tests at least provide an objective measure of how closely a given field histogram matches a particular theoretical proposal.

Indeed, the problems faced by ecological theorists go well beyond the ones mentioned in this book. The warnings of R. H. Peters appear to have gone largely unheeded since the publication. of his book, *Critique for Ecology* (1991). In it Peters gives many examples of problems in several key areas and summarizes the situation as follows:

> "The weakness of the central constructs of contemporary ecology results because ecology compounds its single failings. Operational impossibilities spawn tautological discussions that replace predictive theories with historical explanations, testable hypotheses with the infinite research of mechanistic analysis, and clear goals for prediction with vague models of reality. The resulting melange obscures appropriate research and attainable goals with sloppy, ineffective activity. As a result, the central constructs in ecology yield predictions with difficulty and these are often so qualitative, imprecise and specific that they are of little interest and less utility. The complexity of contemporary ecology makes criticism difficult, because the critic scarcely knows where to begin. This predicament protects ecological constructs from all but sustained critical scrutiny."

Sooner or later the good ship *Ecology* must throw overboard much of the ballast that has grounded it in the shallows. Only then can it make for the open waters and limitless horizons of exacting (and exciting) science.

## 10.3 Disclaimer

The reader has no doubt noticed that, with the exception of the two major experiments reported in this book, most of the “experiments” are rather short, being just long enough to carry the point. As I have explained earlier, long experiments are not needed when their main role is not confirmatory (although they serve in that capacity as a byproduct), but illustrative.

Examples of distributions, whether from the field or generated by computer, are deliberately ragged in the sense that no “doctoring” or “cleanup” of the data was undertaken. The collective impression to be left by these examples is that real data is rarely nice, whether it’s a sample from the field or from the computer. They cannot be told apart in any case.

I have been rather slow to insert many of the topics discussed here into the theoretical literature, several articles being “due” in this sense. I confess to having been selfish, moreover, in keeping the pleasure of these developments to myself. In a nutshell, they came too fast to publish separately.

I have deliberately avoided the use of the word “model”, except in one or two isolated instances where it seemed appropriate from the point of view of my own background in mathematics, physics, and other sciences. The word is so badly overused, often referring to rather weak echoes of a natural system under study, that I am inclined to avoid it altogether for fear that some would regard the logistic-J distribution as a “model”; few astronomers ever refer to the elliptical orbit of planets as a “model” in this sense.

References to past work of others occur somewhat sparsely in this book for two reasons. First, much of past research is simply wrong and there is no point in prolonging its life. In other cases, the discovery of systematic errors in past methodology do not require the singling out of many individuals, but only as few as necessary to carry the point as examples. Second, many of the items of information adduced to an argument or discussion are either a) commonplaces or b) derived *ab initio* by the author without being aware that others may have made the same observation. In the latter cases the author happily surrenders any claims of priority.

Finally, I must ask a favour of those who would attempt to harmonize, incorporate, or unify the logistic-J distribution with previous proposals, not to. That game, I should think, is over.