They say that historians "strangely" do not accept Judge Earl Larson's judgment, despite the evidence gathered, despite the high legal standards that Judge Larson applied to his conduct of the trial, and despite the fact that Sperry Rand (holders of the invalidated patent) did not appeal the verdict. The judgment of the court *is* relevant to any discussion of who invented the computer; a glance at nearly all the scholarly histories written in the past few years shows at the very least that scholars have given it some weight. But what constitutes a "fact" as historians accept it is very different from what a court of law accepts. The Burks acknowledge this, but they do not accept it.

What is most regrettable about this book is that it weakens Atanasoff's case by trying to make his contribution seem greater than it was. Atanasoff should be judged on the machine that he built. It was one of the first machines to use the speed of electronic components to solve complex problems not solvable by manual methods. This was quite an achievement, one that historians have gradually come to recognize and credit him for. Its influence on the ENIAC does not change what it could or could not do. Above all, it is unfair to judge Atanasoff, as Burks and Burks do, on the basis of what he "might" have done, had circumstances (that is, his transfer to other war-related work in the Washington, D.C., area) been otherwise. Perhaps he and his assistant Clifford Berry might have worked out the few remaining bugs in their computer; perhaps Atanasoff could have inaugurated the modern "computer revolution" directly, instead of via the ENIAC and the Moore School. But he did not, and that is a fact that Alice and Arthur Burks seem unwilling to accept. As I said, the book does not suffer from the fact that neither Arthur nor Alice Burks knew Atanasoff at that time, owing to their diligent and careful research. But the fact that neither author is a historian by training is a more serious problem, as their treatment of this theme makes evident.

The authors clearly would like this book to settle the issue of who invented the digital computer "once and for all." Unfortunately that is not to be. Their analysis of Atanasoff's work and and his link to the ENIAC project is an important piece of the mosiac of early computer history. But their argument about Mauchly's role and their accusations of unethical behavior on his part are way off the mark and will have the effect of obscuring the truth, not clarifying it. That is a shame, because it mars what otherwise is a fine piece of work.

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Immune Regulation

Recombinant Lymphokines and Their Receptors. STEVEN GILLIS, Ed. Dekker, New York, 1987. xxviii, 325 pp., illus. $89.75. Immunology Series, vol. 36.

The study of lymphokines (perhaps "cytokines" is a better generic term) is an area in which data accumulate so quickly that it is virtually impossible for a book to be current even at the time of publication. Thus, as Gillis notes in his thoughtful preface, such a book is only "a still life of the field of immune regulation at a given point in time—unfortunate only in that the field has continued to move at its rapid pace." Nevertheless, I believe that this book provides a comprehensive review of a large number of topics and serves as an excellent resource for either a student or an established investigator. The chapters are generally well written for a diverse audience and well referenced. They treat interferon, interleukin-2 and its receptor, interleukin-1 and its receptor, interleukin-3, erythroid-potentiating activity, granulocyte macrophage colony-stimulating factor, immunoglobulin E–binding factor genes, interferons, tumor necrosis factor, and lymphotoxin.

Although it encompasses a large number of the known factors, the book is not, nor does it claim to be, comprehensive; it omits detailed discussions of other cytokines such as interleukin-4 and interleukin-6. The depth of detail varies from chapter to chapter, and there is a tendency for some of the authors to overemphasize their own work rather than summarize the overall field. For example, in the chapter on the human IL-2 receptor, the authors state that "purification of the IL-2 receptor was made possible by the generation of a monoclonal antibody, termed 2A3," an antibody that they identified, but they do not mention monoclonal anti-Tac antibody, described by Uchiyama *et al.* in 1981 and generally considered the prototypic anti–IL-2 receptor antibody. The chapter on the murine IL-2 receptor focuses exclusively on complementary DNA cloning and omits the extensive biochemical characterization published in the literature. Nevertheless, both of these chapters are compendiums of important information.

I found the chapter on the proIL-1β gene by A. Webb, L. Rosenwasser, and P. Auron especially interesting. Among other topics, it very nicely reviews the structural similarities of the amino acid sequences and of the genomic organizations of IL-1α and IL-1β, as well as the data suggesting that the IL-1β gene represents an active retroposon derived by duplication of the IL-1α gene. The authors also discuss the regulation of IL-1β gene expression. S. Dower provides an excellent chapter on the IL-1 receptor wherein he summarizes elegant kinetic binding data of IL-1α and IL-1β to the IL-1 receptor and provides a detailed summary of the cellular distribution of IL-1 receptors. *Recombinant Lymphokines and Their Receptors* is a valuable resource that summarizes a large quantity of data in a generally clear and concise form, and as such it represents an extremely useful "still life" of the field.

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Techniques for Ecologists

Developments in Numerical Ecology. PIERRE LEGENDRE and LOUIS LEGENDRE, Eds. Springer-Verlag, New York, 1987. xii, 585 pp., illus. $149.70. NATO Advanced Science Institutes Series G, vol. 14. From a workshop, Roscoff, France, June 1986.

If you bring together a dozen experts on a range of numerical techniques with three times as many ecologists for a week in a cool and windy marine station, what would you expect? Well, what has resulted here is a series of interesting, individual, accounts of a diverse set of techniques, supplemented by a short series of inadequate responses from the ecologists, guessing about the usefulness of the techniques. The reason for such a response is simple. Just discussing new, computer-based, conceptually difficult techniques is no basis for deciding if they are useful. They must be shown to be useful in a variety of data sets, and then tried on your own data, on your own computer system, before you can say whether or not a particular technique is helpful in understanding a particular type of problem.

So, in my view, this book is best regarded as a source of various advanced and unusual techniques of data analysis, of use in a great variety of sciences, not just ecology. On the whole, the accounts are not intended to be comprehensive or exhaustive; this is not a textbook. On the other hand, the techniques that are described are, usually, given in some detail so that they could be used efficiently once computer programs had been acquired, or modified, or written.

The distribution of space devoted to topics starts geometrically, ends up J-shaped. The relative lengths are difficult to judge, as each paper is produced on the author's own printer, giving an unusual variety of type sizes and legibility and inducing a jaundiced glance at the price. There are about 200 pages on ordination and scaling, 100 on clustering, 50 on fractals, 25 on path analysis, and then 60 on spatial analysis, by 5, 3, 1, 1, and 2 authors respectively.

In treating ordination and scaling, Gower and Carroll give masterly accounts of the techniques used at Rothamsted and Bell Laboratories, including principal components and principal coordinates, canonical and correspondence analyses, Procrustes analyses, the use of transformations, and several varieties of multidimensional scaling, including what are called internal and external analysis. Classical methods are two-way, by rows and columns; three-way analyses are also dealt with here, but not higher dimensions. A couple of unexciting ecological examples are used, but scarcely interpreted: "It . . . remains for ecologies to 'interpret' the . . . effects on the seaworm species"—and no doubt to say what a seaworm is and to distinguish "*interpret*" from *interpret*.

The other techniques are dealt with in the same vein. Bezdek deals with nonstandard clustering, especially using fuzzy sets, and Legendre and Lefkovitch treat constrained and conditional clustering. These are all techniques that should be used more widely, and the scarcity of examples is disappointing. Fractals, I am sure, are important, but Frontier fails to mention most of the published accounts of their use in ecology and clearly failed to convince the ecologists of their relevance. Path analysis, spatial point patterns, and spatial autocorrelation are dealt with most competently by de Leeuw, by Ripley, and by Sokal and Thomson, respectively, though again their usefulness is left in doubt. Thus Ripley, "*None* of the transect methods detect visually obvious spatial patterns."

Some of the authors give excellent advice. Bezdek says try two or three different algorithms and think hard if the results disagree. Heiser, on joint ordination of rows and columns by unfolding, says, "Attacking very simple problems by very powerful tools is usually unwise" and notes that his techniques are most useful for large, relatively unstructured data sets in which there is not much information about mechanism.

The final 90 pages of the book give working group reports from microbial, benthic, and pelagic ecologists, from limnologists, and from plant and animal terrestrial ecologists. These tend to be even more individual than the techniques chapters, with many didactic but disputable statements: "Studies of vegetation . . . strictly confined to the species comprising the vegetation are outside the domain of ecology"; "This is not to say that there are no uses for PCA . . . in . . . ecology"; "The best technique is often the one best known to the user. Ecologists should not dissipate their time and effort in mastering irrelevant techniques." This reluctance to try a variety of techniques, many of which may indeed be relevant, is discouraging. The weather must have been bad in Roscoff.

One good justification for these sour quotes, though, is that numerical ecology is defined as the analysis of data sets. Analytical and simulation models are excluded, and the interaction between data and hypothesis is scarcely considered. So the techniques discussed here cover only a limited part of quantitative ecology, but nevertheless they should be known to all practitioners. Only if they are used as part of exploratory and explanatory programs will the relative usefulness of different techniques became clear.

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