maturing attention span sets this certain level of complexity and thereby sets a limit on learning. [Pp. 111–12. Emphasis mine.]

This seems to me all wrong. Surely more thorough learning of a skill itself frees more of one's ability to notice and to learn other things; when we first ride a bicycle, or ski, it takes all of our attention; our later ability to do it with less demand on our attention is surely not to be explained solely in terms of a general maturation of attention span. The problem here is that Cunningham is taking his own specifications too seriously, ignoring their lack of any provision to treat bundles of elements as a single element. He himself in fact continually finds it essential, in order to talk about his model's operation, to allow the notion of a memory element to refer to things at very different levels, from something like a single neuron's firing clear up to a whole complicated concept or skill. It seems to me that this is correct, but that it is essential to give the mechanism itself this same kind of ability. If it had this, he would not find himself obliged to keep relying on a general maturation in the size of attention span to explain people's increasing ability to learn about and attend to one thing as they do another.

However, despite problems of this kind, Cunningham has succeeded better than I thought anyone could succeed just yet in stating a set of rules capable of developing a structure which in turn seems basically capable of producing a really human-like range of intelligent behavior. Herein lies the first reason I suspect his book may well point to a major new paradigm for cognitive theorists: it would move computer-simulation efforts up a level of abstraction. Although attempts at building computer programs to simulate cognitive behaviors have taught us a great deal, these efforts, after first illustrating a few exciting concepts, then seem always to disappear in a morass of details. The exploration of general laws —science—loses out to a peculiar sort of programming pedantry; each project turns into a tour de force in the management of detailed instructions. I suspect this is inevitable as long as researchers attempt directly to program machines *to perform* cognitive tasks, rather than *to learn to perform* them. The only way for a simulation project to remain an exploration of general principles, then, is for its members themselves to program only learning systems, leaving all performance capabilities but the simplest reflexes to be developed by the learning systems. Earlier efforts to program general learning systems, such as the EPAM programs, simply did not develop structures of sufficient flexibility to produce capabilities across an interesting range of cognitive performance. Cunningham's looks to me as though it may, given sufficient tinkering.

There is a second focus of present computer-simulation efforts which Cunningham's work would change. This is the tendency among computer simulators to let the sequentiality that is natural to a computer lead them to view cognition as also guided by essentially sequential processes, and thus to tacitly assume that cognitive processing is the running of programmed sequences of actions—routines that have been previously designed to attain goals or solve problems in the way of goal attainment. This view is likely to seem natural to philosophers who are used to treating distinctions such as objective-relativistic as strict dichotomies (see for example Shapere's attack on Kuhn's "paradigm concept" in *Science* **172**, 706 [1971]), and to others who are accustomed to thinking of thought or natural language as some sort of symbolic logic. Whereas most computer simulation has adopted this kind of view, Cunningham's theory (and the brief proposal for a simulation of it that he gives) would lead to simulations that treat behavioral and cognitive acts as emerging from a "richly parallel" process, a sort of seething, or "pandemonium," to use Selfridge's term, that goes on more or less continually in a very complex memory. To anyone who thinks that human thought is better characterized by something like Molly Bloom's soliloquy than by, say, a proof in symbolic logic, this will seem a turn in the right direction.

In summary, I think Cunningham's book is of major significance, even though I must also add that I think it pushes discursive treatment of his theory about as far as it is fruitful to go. If he or anyone else really wants to discover the potentials and flaws of this conception, the way to proceed now is to start trying to make it run as a program.

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Use of Diffraction Patterns

Optical Transforms. H. Lipson. Academic Press, New York, 1972. xii, 436 pp., illus. $22.50.

This book combines two areas that are not usually considered together, yet have basic similarities. One is the study of crystals, organic molecules, polymers, and other such structures by means of the diffraction patterns they produce. The other is holography and the processing of information by means of coherent optical systems. In each case, the formation of diffraction patterns is of essential importance. Since the two areas are widely separated in practice, there is a question of whether any single individual could have written a book such as this. In fact, this book has about a dozen authors, each of whom has contributed a chapter or two.

The greater part of the book deals with the first area, the use of diffraction patterns to unravel the structure of molecules and crystals. The various authors treat related topics, one choosing organic molecules, another polymers, another crystals, and yet another the basic theory of Fourier synthesis. The similarity of the topics and the diversity of authorship result in some overlap of material, but this effect seems to be minimal and is not undesirable, since the viewpoints and tutorial approaches are different.

The diffraction patterns may be formed by illumination of the materials with x-rays or with electrons. Simulation studies are made by examining the diffraction patterns formed from specially prepared masks illuminated with coherent light. Inferences drawn from these various patterns or from their comparison, along with computer analyses and much ingenuity, lead to determination of the molecular or crystal structure. The procedure, described by the various authors in different ways for various classes of objects, appears to be difficult, laborious, and quite challenging.

The text is supplemented by hundreds of pictures of diffraction patterns, which considerably assist the reader in his comprehension of the material.

The other basic area is treated in the two chapters on holography and optical processing. Each is capably written and gives a proper account of its subject, both from the historical viewpoint and in terms of current activities. Optical processing, in the sense in which the term is used here, is concerned with the alteration of images by interposition of various types of masks in the diffraction field between object and image. The variety of results available is essentially unlimited, and the entire theory of linear filtering, a major area by itself, becomes the theoretical basis for the process.

The principal topics of the book are tied together by introductory discussions of diffraction and coherence. The chapters are self-contained and therefore may be taken up in any order.

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Gas-Phase Chemistry

Unimolecular Reactions. P. J. ROBINSON and K. A. HOLBROOK. Wiley-Interscience, New York, 1972. xviii, 372 pp., illus. $19.95.

Unimolecular reactions are those in which a single chemical species decomposes or rearranges. In gases, these are the ones whose rates are most susceptible to theoretical explanation and prediction. They have been studied since the beginning of modern physical chemistry, and the field has been a model of how science ought to work. There have been a 1928 theory so far ahead of its time as to remain the standard treatment for 30 years, a rivalry between two mutually exclusive successors, a resulting outburst of inspired experimentation, and a culminating synthesis of ideas well described in this book.

The coverage of literature is extremely complete. Every piece of work of any surviving importance is there. The thoroughness with which the theoretical development is surveyed does not, however, detract from the experimentalist's outlook that characterizes the book. It is primarily about how to apply the fully developed theory.

The earlier treatments are introduced in a way which might at first lead the uninitiated reader to suppose they are still under test. This notion is quickly dispelled by the main part of the book, in which the authors undertake to induce people to make more widespread use of the theory. They do this by presenting a carefully structured general framework and a detailed analysis of the mathematical complexities that arise in practice. Best of all, they lead the reader by the hand through a complete "RRKM" calculation for a realistically chosen example. These parts alone are enough to sell the book. The reader who already has experience in unimolecular chemistry will turn first to the absolutely mandatory appendix A—comparison of the notational eccentricities of the principal researchers. In its later chapters, the book is an exhaustive survey of experimental results, including descriptions of chemical activation and of isotope effects.

It is hard to find anything to criticize. The authors have in a footnote anticipated my objection to calling the molecule's reactive configuration an "activated complex." An illustration of the confusion this engenders is the appearance and disappearance of maxima in their potential energy diagrams, which may perplex some readers. Imaginative solution chemists can also use the theory, and the authors might have given them a little more encouragement.

The book is billed as a text. Besides this, I think it will remain an important reference work for many years to come. Its publishers have produced it with a promptness that I hope will be diligently imitated by others. I was astonished to find on page 103 the very recently discovered molecule that may be the herald of the next round of surprises in this rich and unpredictable area of research.

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Plant Problems

The Dynamics of Meristem Cell Populations. Proceedings of a conference, Rochester, N.Y., Aug. 1971. MORTON W. MILLER and CHARLES C. KUEHNERT, Eds. Plenum, New York, 1972. xviii, 310 pp., illus. $19.50.

Years ago I was told by a famous animal cell biologist that root tips made such good experimental material because all the cells were the same. This volume clearly and emphatically makes the case that plant meristems, both roots and shoots, are far more complex than even the most knowledgeable assumed a decade or two ago.

This volume ranges over a variety of topics concerned with meristems: their physiology, their dynamics, and the effect of radiation on them. The contributors include John Torrey, Jack Van't Hof, Elizabeth Cutter, Ernest Ball, F. A. L. Clowes, D. Davidson, Francesco D'Amato, Alan Haber, and others. The papers themselves contain large numbers of facts and are thoroughly documented. One of the most interesting, however, is one by J. R. K. Savage and M. W. Miller that is essentially a theoretical treatment of the problem of the heterogeneity of the cell population with regard to the collection of data. They deal specifically with radiation and chromosomal aberration, but the problems they discuss are applicable to many other situations.

Despite the obvious qualifications of the authors and the importance of the topics, the volume as a whole is disappointing. Many of the data presented have already been published, yet the papers are written as research reports rather than reviews. Moreover, little effort has been made to interest the reader in the topics. At least half of the papers are unnecessarily difficult to read. The transcribed discussions at the end of the papers could just as well have been left off for all they add of either information or interest.

More important than the writing is the feeling of frustration generated in the reader. The problems are there, even more problems than a few years ago, yet the answers seem no closer. The quiescent center in roots is a good example. As the papers by Torrey, Clowes, and Davidson make abundantly clear, the quiescent center exists and many of its characteristics are known in great detail. Yet, we still do not know its function. Torrey's proposal that it is the center of cytokinin production is interesting, but one made at least ten years ago and still no more than an interesting hypothesis.

The field of plant development is an exciting one, but if this volume is any indication it has gotten bogged down in details, and new, different approaches will be needed to answer the many questions that remain.

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Research on Muscle

Muscle Biology. A Series of Advances. Vol. 1. R. G. CASSENS, Ed. Dekker, New York, 1972. x, 300 pp., illus. $17.50.

This volume is based on a lecture series sponsored by the Institute of Muscle Biology in Wisconsin. There are