both a cross-cultural overview and a more detailed case study. This part of the book offers a useful introduction to the ethnology of sex roles. The authors do not, however, present convincing proof of their thesis that women's roles in economic production determine their general status in society. First of all, they fail to show how the general status of women can be discussed in cross-culturally meaningful terms. (It should be pointed out, in all fairness, that as yet no one else has done so either.) Second, their investigation of women's economic roles seems to be hampered rather than helped by their adherence to the familiar categories of American cultural evolutionism. Martin and Voorhies themselves point out significant variations in the sexual allocation of productive roles within such subsistence types as foraging, horticulture, and pastoralism. This is a welcome contribution, but it leads the reader to wonder why the authors did not go on to order their data in some other manner. A more systematic approach to the analysis of modes of production as social systems would seem to be required.

The category in Martin and Voorhies's typology that comes closest to revealing some overall pattern of male and female roles is agriculture, that is, cultivation involving such techniques as plowing, the use of draft animals, and irrigation. According to Martin and Voorhies, it is adaptive in such systems for men to take over the major share of heavy work that is carried out away from the home. They claim that, in agricultural societies, "women dropped out of the mainstream of production for the first time in the history of cultural evolution" (p. 290). This had certain consequences for women's status in general: "The exclusion of women from major economic-event systems outside the household signals their increasing isolation from central roles in other societal institutions as well" (p. 240).

The pattern of sexual division of labor in which men are the major economic providers and women's activities are largely confined to the domestic sphere has, in Martin and Voorhies's view, persisted into the industrial period as a sort of hangover from earlier agricultural systems. Interestingly enough, this analysis absolves Martin and Voorhies from having to account for sexual inequality in industrial societies in the same positive functional terms that they apply to all other societies. On the contrary, they claim that sexual inequality is dysfunctional within the industrial mode of organization, since it is wasteful of individual talents and aptitudes.

There are several problems with this line of reasoning. For one thing, it is not clear that agriculture suffices to account for a pattern of sexual differentiation in which women's activities revolve around a relatively narrow domestic sphere while men move in a wider social world, since such a pattern is found in nonagricultural (and nonindustrial) societies as well. For another, it is not so easy to dismiss the functions that sexual inequality has taken on in industrial societies; both of Martin and Voorhies's case studies—of the United States and the Soviet Union—provide ample evidence of this. It seems to me that Martin and Voorhies's argument is basically an ideological one, incorporating elements of laissez-faire (the inherent logic of industrial society is that each individual be free to develop his or her natural propensities and interests), utilitarianism (sexual inequality will disappear when people realize that it doesn't pay), liberalism (what is good for one currently disadvantaged segment of society is better for all), and positivism (answers to political problems will emerge from objective scientific research).

In making these criticisms, I do not wish to detract from the value of *Female of the Species* as a general introduction to the study of sex roles. It should be read by anyone interested in the subject. However, I feel it important to point out that an uncritical mingling of moral and scientific perspectives is limiting in both directions: just as there is no direct path from political commitment to scientific knowledge, so there is no direct path from scientific knowledge to a system of ethics.

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Approaches to Biological Information Processing

Physics and Mathematics of the Nervous System. Proceedings of a summer school, Trieste, Italy, Aug. 1973. M. CONRAD, W. GÜTTINGER, and M. DAL CIN, Eds. Springer-Verlag, New York, 1974. xiv, 584 pp., illus. Paper, $18.50. Lecture Notes in Mathematics, vol. 4.

Many experimental biologists dismiss with contempt the approach of even very able theoreticians to developmental or neurophysiological problems. The outsider need look no further than this volume to understand why. One or two papers apart, only the section on cellular and sensory biophysics demonstrates that recourse to mathematics is sometimes worth the effort, and it is no accident that this occurs in the most traditional part of the book. The remaining papers describe attempts to elucidate problems of biological information processing, but in one way or another they all make the same error of strategy—engaging in the search for a general theory before and actually instead of tackling any of the particular problems at hand. This has been a fruitful strategy in other branches of science, but there scientific intuition has been honed by decades or centuries of empirical study. With problems of biological information processing there has been almost no experience, and one's intuition is at best untrustworthy. It may even be that biological information processing admits of no general theories except ones so unspecific as to have only descriptive and not predictive powers.

There are a number of candidates for the general theory. I take the liberty of setting out the most common ones here, in the hope of preventing yet another generation of theoreticians from being seduced by them.

1) *Catastrophe theory.* The most forgivable candidate, since it is the only one that rests on a mathematical foundation of genuine power and beauty, is catastrophe theory as enunciated by René Thom. Güttinger's paper shows clearly the reasons why the advocates of catastrophe theory believe it is important for biology. The import of Thom's deep theorem is that when a dynamical system becomes unstable and undergoes a discontinuous change, that change ultimately can occur in one of only a very small number of ways (seven for conventional space-time). Hence if a functioning organism is regarded as a dynamical system, each sudden change can be classified as one of these canonical discontinuities, and the behavior of the system near the discontinuity can be captured geometrically. This approach has been applied (by E. C. Zeeman and others) to phenonema as diverse as the heartbeat, the conduction of the nervous impulse, the division of a cell, the breaking of a wave, and the switch from fight to flight. Its spectacular generality has led to claims that catastrophe theory will become the "applied mathematics" of development and of the nervous system, being comparable in importance to the differential calculus.

The objection is simply this: that these "catastrophic" events are distinctive and important only when they are uncommon, in systems that are predominantly continuous; and that is precisely not the nature of the central nervous system. At the level where one isolates an information-processing problem and devises a method of solving it, the catastrophes that underlie the method's implementation have been left far behind. For example, a digital computer is full of catastrophes—one occurs every time a flip flops—but they are irrelevant to the description of what a program does and how. Catastrophe theory characterizes the passage from the continuous to the discrete, but it has nothing to say about complex descriptions or procedural specifications that are written and executed in a language whose entities are already discrete.

2) *Automata theory.* This and the following approaches rest on less sophisticated mathematics. The argument is rarely formulated, but proceeds roughly as follows. The brain is made of locally active pieces, glued together in an essentially simple way. The way to understand it is therefore to study the class of computations that can be performed by an abstract entity that captures certain properties of local autonomy and simplicity of interconnection.

Such devices are called cellular automata, and Merzenich's paper provides a readable introduction to them. They are of interest in their own right, but studying them will not provide insights into the workings of the brain. The reason is that very weak machinery produces a computational engine that is universal; and given a particular method and moderate ingenuity, one can usually devise an automaton that implements it efficiently. The important question is, What processes need implementing? Abstract studies cannot help to answer this, because they introduce no notion of what constitutes a *useful* process. To do this, you have to study information-processing problems, not particular pieces of computing machinery.

3) *Learning automata theory.* This theory says that since learning occurs in the brain we shall come to understand it by studying automata that change themselves (sometimes called "self-organizing systems"). The papers by Dal Cin and by Vollmar are examples from this volume describing work that appears to be founded on this view. The theory deals only with changes that occur at a very low level, yet except in simple negative feedback situations it is not at such a low level that the interesting phenomena of learning are captured. Anyone who has ever written a machine-code program knows that random low-level changes in the definition of a procedure cause havoc. And again, digital computers have always had "memories," yet psychologists do not find the kind of "learning" they do very interesting. The point is that although at some stage a low-level change must occur, why the change does what it does requires an explanation at a much higher level of description. (There is an analogous argument about genetic programs, which says that most of the time evolution cannot proceed by changing random instructions in the machine code; the lowest level at which viable changes can be made is that at which subroutine calls can be altered.)

4) *Neural net theory.* This combines the limitations of the two previous theories, and arises from a belief that there is something computationally very special about neurons. This belief lies behind much of the section on network physiology in this book. Experimental biologists as well as theoreticians are prone to this error, but its effects for them are less disastrous. If one studies the details of synaptic transmission out of a belief that it will throw light on the computations performed by the brain, it is not fatal that the belief is mistaken, because something interesting about synaptic transmission will probably emerge.

The neural net theory states that the brain is made up of neurons, connected either specifically (for small structures) or randomly (for large ones). Hence, in order to understand the brain we need to understand the behavior of these assemblies of neurons. Here there are two problems. First, the brain is large, but it is certainly not wired up randomly. The more we learn about it, the more specific the details of its construction appear to be. Hoping that random neural net studies will elucidate the operation of the brain is therefore like waiting for the monkey to type Hamlet. Second, given a specific function of inevitable importance (like a hash-coded associative memory), it is not too difficult to design a neural network that implements it with tolerable efficiency. Again, the primary unresolved issue is what functions you want implemented, and why. In the absence of this knowledge, a neural net theory, unless it is closely tied to the known anatomy and physiology of some part of the brain and makes some unexpected and testable predictions, is of no value.

5) *Characterizing the computational power of a system.* Another way of approaching biological information processing is to attempt to prove that a system—a set of enzymatic pathways, for example—is in principle as computationally powerful as some class of finite automaton (as is done in Rossler's paper and in others in the section on molecular and modifiable automata). This is interesting, but probably a waste of time. On the other hand it is not a waste of time to take a specific structure, such as an oak leaf or a chick wing, and ask what process could generate one, subject to the constraint that if changed slightly the same process could be used for making, say, an elm leaf or a chick foot.

The mysteries of development and of the central nervous system will ultimately be explained in terms of processes, data structures, virtual machines, methods, algorithms and the particularities of their implementation, control structures, and types and styles of representation of knowledge together with detailed specifications of the knowledge required for different tasks. A novel feature of the contemporary scientific scene is that the computer allows one to try out information-processing theories on real-world data. One can argue that a clever enough scientist might not need direct computational experience to formulate the appropriate methods and prove that they will work; but the intuitions needed for understanding biological information processing are not easily available. Only by wresting them from actual experience does one gain a feel for what questions need to be asked, and develop a language in which to ask them. Even with this help, progress is slow, and only small advances have so far been made. But without it, larger ones never will be.

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Periodicity

Circannual Clocks. Annual Biological Rhythms. Proceedings of an AAAS symposium, San Francisco, Feb. 1974. ERIC T. PENGELLEY, Ed. Academic Press, New York, 1974. xiv, 524 pp., illus. $22.50.

It comes as no surprise to any of us that biological events are timed precisely and periodically on a yearly basis corresponding to the earth's travels about the sun. Truly remarkable, however, is the fact that when organisms are isolated experimentally from the obvious cues of the yearly cycle the events may still be precisely and periodically timed. In this situation, the period of the cycle is not exactly one year, but deviates from it. E. T. Pengelley initiated the use of the word "circannual" to refer to these persistent rhythms, and the word carries the implication that the rhythms can be generated from within the organism.

Circannual Clocks is a collection of papers from a symposium. The volume will not be rapidly outdated because, as Menaker notes in his concluding remarks, "the major difficulty in the study of circannual rhythms is a consequence of the ratio of the period length of a single circannual