we employ. Symbolically, we may express the yield as a function of several variables together representing the management policy. This construct is similar to Wright's "adaptive surface," which specifies the fitness of a population as a function of its genetic composition. Wright was interested in the "search strategy" by which the population could "find" the genetic composition of maximum fitness as quickly as possible; similarly, Watt requires a means for speedily finding the optimum management policy. If there were anything substantial to say on this subject, it would be of great interest, but the subject is hardly born yet, and Watt can only present abstractions unsupported by concrete applications. Our ignorance permits the contrast between Wright's emphasis on search strategies with a random component and Watt's emphasis on purely deterministic strategies.

The two most interesting aspects of this book are its reliance on computers and its reductionist approach. The reductionist attitude is very fashionable in biology nowadays, and quite possibly we can never claim to understand ecology until we can pursue a child's question why a lion eats zebras to an explanation of the chemistry of the lion's hunger and the more mysterious logic behind the events in the lion's brain. Before the time of computers, one could claim that such an attitude put the cart before the horse, for we would be busy explaining before we quite knew what to explain. A necessary prelude to the kinetic theory of gases was an empirical thermodynamics singling out pressure, temperature, and volume and, more generally, energy and entropy, as variables whose relations were important. Without such a theory, essays into molecular mechanics would have lacked purpose and direction; moreover, molecular mechanics has little to add to many of the predictions of "classical thermodynamics." Does Watt err by studying ecosystems through detailed analysis of their components? His specific objective, and his use of computers, vindicate this approach. He knows what he wishes to optimize, and to achieve this aim he has machinery to employ models as complex as our ignorance may demand. To learn what questions to ask first, he may make preliminary models of his system to test the relative importance of different variables. In no way does his investigation lack purpose.

Watt's reliance upon computers raises more interesting problems. There are three possible attitudes toward computers:

1) One may abstain from their use as a discipline to encourage simple and understandable thought. According to this attitude, a question so complex as to require computation is the wrong question to ask; one has to think further about what is really important, what constitutes the essence of the problem.

2) One may use them to simulate special cases, to learn what questions to ask of paper-and-pencil mathematics. According to this attitude, one seeks a theory simple enough to work out with paper and pencil, but a computer helps in the search.

3) One may view the computer as an extension of the mind, taking the attitude that we understand a system if we can construct a computer program that properly simulates its behavior. Watt thinks the last attitude is appropriate to the study of complex systems. Computation permits maximum interaction between model, experiment, and observation, for there is no need to gloss over biological complexities, and no obstruction to generating meaningful predictions.

Watt's reliance on computers is part of a phenomenon of very deep significance. In the past, a model or theory of a system usually served two purposes: it accurately predicted the system's behavior, and it was a description of the system substantially simpler than the system itself. The model helped us to understand "why the system behaves as it does" as well as to predict its behavior. The two requirements were really one: to predict a system's behavior one had to construct a model simple enough to understand, simple enough to let us work out the consequences of our assumptions. The bond between theory and application was that prediction required simple yet meaningful description. Computers disrupt this bond: one no longer needs *simple* theory for prediction. Indeed, for accurate prediction we quickly resort to models too complex to understand. This proliferating complexity may have strange consequences. After all, one can *predict* planetary motion quite accurately by introducing enough Ptolemaic epicycles. Yet, even though it makes no difference to the relativity theorist whether we center our world on earth or sun, we cannot "understand" the dynamics of planetary motion unless we do away with the epicycles by sending the earth round the sun. Many scientific revolutions, many advances of understanding, were brought about by the demands of simplicity. Since applications do contribute to such advances, the computer's dissociation of prediction and understanding may greatly affect the development of science. It is distressingly easy to see how such a dissociation could lead to a proliferation of abstract theory, no longer needed for, and therefore undisciplined by, application; and an applied technique more dependent than ever on elaborate machinery, and thus more a slave of circumstance.

Watt's book betrays some haste. The book is technically nearly self-contained (requiring only a little elementary calculus and statistics), yet many readers will find the mathematics quite difficult; a little more time and thought would surely have made the book far more accessible. In later portions of the book, many ideas are expressed in Fortran language; I hope others do not find the algebra as difficult as I did the Fortran. On the other hand, the book is well put together, the equations nicely displayed, and so on. I think the book is one of substance, perhaps the greatest to appear in ecology since the fundamental works of Elton, Gause, and Volterra.

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Responses to Light

Insect Photoperiodism. STANLEY D. BECK. Academic Press, New York, 1968. viii + 288 pp., illus. $12.50.

In both plants and animals many seasonal adaptations, involving growth pattern and timing of reproduction and dormancy, are mediated by responses to seasonal differences in the duration of daylight, that is, in day-length. The physiological mechanisms underlying such biological responses to day-length constitute the usual subject matter of photoperiodism. The vast literature in this area is generally dismaying to the student; the terminology is inconsistent and the concepts are often vague; the experimental designs are commonly confusing; and the distinction between hypothesis and experimental observation is seldom kept clear.

There is, then, a real need for an incisive book which would provide a lucid introduction to the subject. *Insect Photoperiodism*, however, does not meet this need; in my opinion, it will be only confusing to the student and annoying to the specialist.

A primary difficulty lies in Beck's delineation of his subject matter: "photoperiod" is defined as any light-dark cycle, and photoperiodism is considered to include all aspects of biological rhythmicity affected by light cycles (rather than only adaptation to season, mediated by day-length). This leads, inevitably, to semantic confusion: "In nearly all cases, photoperiodic responses of insects and other animals have been shown to be based on the effects of the environmental photoperiodic rhythm on internal biological rhythmic processes" (p. 1). If "photoperiodic responses" are taken to include the entrainment of circadian rhythms of eclosion and activity, this statement is partially valid; but to the reader who thinks of photoperiodism in terms of day-length responses (the traditional usage), the statement is patently misleading, since the available evidence for an internal rhythmic substrate underlying the seasonal responses of insects is either inconclusive or, in some cases, apparently negative (as Beck also recognizes, p. 177 ff). Throughout the book, wherever I came upon "photoperiodism" and "photoperiodic," I found it necessary to ask myself precisely which meaning was intended. My initial annoyance rapidly degenerated into despair.

The book provides an extensive treatment of insect circadian rhythms (eclosion, locomotor activity, and so on) and their phase-shifting and entrainment by light cycles, but never any clear indication of the relevance of these studies to seasonal adaptation. And relevance aside, the portions of the text on circadian rhythms are still probably the weakest component of the book. Bünning, who has so strongly emphasized the contributions to the study of leaf movement rhythms made by early workers such as Pfeffer (1875) and Darwin (1881), is nevertheless credited with the "pioneer studies of daily rhythms in plants" (p. 62). It is asserted that no systematic studies of "Aschoff's rule" have been made with insects (p. 67), although references to Lohmann's work with *Tenebrio* (for instance, by Hoffmann, in *Circadian Clocks*, 1965) must have passed through Beck's hands. There is an extensive discussion of *Drosophila* phase-shifting experiments from Pittendrigh's laboratory, but the proposed interpretation (p. 77), separating phase-shifts due to light-on and those due to light-off, clearly exceeds anything warranted by the data or proposed by Pittendrigh.

Beck is hesitant to use the term "biological clock," and I concur; but the logic of his reservations escapes me:

> The existence of a biological clock could be deduced only if a degree of temporal freedom could be demonstrated. That is, if a given response to a stimulus were capable of expression at any time following stimulation, but could be held in abeyance until the passage of some arbitrary length of time, the response could then be said to be governed by a biological clock. This is not known to occur in any biological system; all responses are determined (p. 58).

The last sentence seems to imply that a biological clock would, by definition, violate strict determinism (and hence invoke vitalism?); but the preceding statements suggest that a biological clock is simply synonymous with a temporal "gating" mechanism, such as has already been demonstrated for development and eclosion of *Drosophila* —or have I misunderstood?

Other dogmatic statements in the text must be considered wrong on several counts: "There is general agreement that the insect responds to the light-on and light-off signals and to the time elapsing between these signals, rather than to the presence, absence or duration of light energy, per se" (p. 103). Not only is there no general agreement on this issue, but the entrainability of circadian rhythms, in the presence of natural twilights and even sinusoidal light cycles, clearly demonstrates that discrete on-off "signals" are not essential to synchronize a circadian rhythm. And Beck himself discusses in detail the many cases in which continuous light or continuous darkness—without on and off "signals"—can evoke the same growth or reproductive response as an appropriate light cycle.

I would like to find some aspect of this book about which I could wax enthusiastic; my reaction, however, was consistently negative. Even the 16-page bibliography, which I had thought might prove useful to entomologists, is apparently unreliable. A spot-check of the German-language references (without a check of original sources for pagination and so on) revealed four typographic errors (spelling and the like) on page 244, and others on pages 248, 252, 254, 256, 257, 264, and 265. The much-needed incisive introductory text on photoperiodism has yet to be written.

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Origins of an Institution

The Royal Society. Concept and Creation. Margery Purver. M.I.T. Press, Cambridge, Mass., 1967. xviii + 246 pp., illus. $7.

Because of the central role which the Royal Society, founded in 1660, played in the scientific revolution, there have been a number of attempts to isolate just those elements which led to its formation.

Scholars have looked among the welter of utopian ideas and programs, ephemeral movements and organizations which flourished in the confused period of the Civil War and Commonwealth for likely candidates to which they can attribute parentage. Joining these scholars in their search, Margery Purver has challenged their usual methods and assumptions. Instead of concentrating on the mechanics and locations of the various organizations and movements that have been suggested as "precursors" of the society, she insists that only a study of the aims and philosophical inspirations of these precursors will enable us to know which one led to its formation and wherein its "unique character" lies. She has been able to enlighten us on some important though isolated points, but in general her book is naive, based on spotty research, and marred by an artless construction which all too frequently merely strings together overly long and often unnecessary quotations.

There are two major parts to Purver's thesis. First, she claims that the first historian of the Royal Society, Thomas Sprat, is correct in his assertion that the society's origins were at Oxford, rather than in London, as most subsequent accounts have maintained. Her case, however, rests in large measure on a misreading of the evidence. In trying to prove Sprat's reliability on the issue of the society's origins and its philosophical leanings, for example, she rests her case on the close supervision given Sprat by the members of the society; her evidence, however, indicates merely close supervision of the examples of the society's