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Some Mathematical Models in Science

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The term *model* is used (and misused) in many ways. Few of us today, for example, would think of atomistic theory as a model. And yet not much more than 100 years ago it was precisely that—a plausible model which accounted neatly for Dalton's laws governing chemical reactions. Similarly the "gene hypothesis" in the early days of the century provided a model to account for Mendelian laws.

Of course, as we all know, subsequent developments endowed both atoms and genes with a "reality" of their own, thus promoting the models of yesterday to the exalted positions of universally accepted theories of today (*1*).

Few models are that good or, for that matter, that lucky, and I deal in what follows with much less spectacular ones. Limitations of space allow only a cursory discussion of a very few models. The few were chosen to illustrate a variety of ways in which models came into scientific thinking, but I make no claim to exhaustive coverage. I have naturally chosen the ones which I understand and with which I am familiar. They are *my* pet models.

I limit myself also to mathematical models—that is, to models which can be described symbolically and discussed deductively. "Nature," remarked Fourier, "is indifferent toward the difficulties it causes a mathematician," and, because of this, mathematical models must of necessity be greatly simplified. There are, roughly speaking, two different types of models: (i) descriptive—that is, models designed to account for observed phenomena, and (ii) conceptual—models constructed to elucidate delicate and difficult points of a theory. Not always is it easy (or even desirable) to draw a sharp dividing line between the two types, but the extremes on both sides are clearly recognizable.

Descriptive models are of particular interest to a working scientist, and they, too, fall into two distinguishable categories, which, for want of better terminology, I will call fundamental and ad hoc.

If, for example, we disregard the mutual attraction of planets and look upon their motions as caused solely by the gravitational pull of the sun, we have a fundamental model of our planetary system. It is fundamental because it uses the inverse square law of attraction, which, barring small relativistic corrections, is the correct law, and it is only a model because it does neglect the interactions between planets. It is, by the way, an excellent model for, as is well known, it accounts for all three of Kepler's laws.

Models of Hemoglobin

Ad hoc models are the most common; as a rule, they represent a formalization of a "working hypothesis" in a situation where a phenomenon studied is, at best, only partly understood. As an example, let me discuss briefly the dependence of the fraction (f) of oxygenated hemoglobin on the concentration of oxygen.

A (hemoglobin) molecule is assumed to consist of n ($= 4$) subunits arranged cyclically, and one further assumes that each subunit can be occupied or not occupied by a ligand, which is oxygen in the case of hemoglobin.

Suppose that $p(+1)$ is the probability that a given subunit is occupied and that $p(-1)$—that is, $1 - p(+1)$—is the complementary probability.

The ratio

$$\alpha = \frac{p(+1)}{p(-1)}$$

can be taken to be the measure of concentration of oxygen.

The simplest hypothesis is that subunits are occupied independently, and in this case an elementary calculation shows that the average number of occupied subunits per molecule is

$$f = \frac{\alpha}{1+\alpha}$$

(the Michaelis equation). The graph of f plotted relative to α (Fig. 1) does not fit experimental data, which have a pronounced "sigmoid" character (Fig. 2).

So far so good. To explain the sigmoid character of the f-versus-α curve, three models have been proposed—one (the "MWC model") by Monod, Wyman, and Changeux; the others by Koshlund, Nemethy, and Filmer and by Colin Thompson (*2*). The last two yield identical results, though the formulations appear to be different. Here I deal only with Thompson's treatment, using his presentation of the MWC model as well.

If we write

$$p(+1) = \frac{e^J}{e^J + e^{-J}}, \quad p(-1) = \frac{e^{-J}}{e^J + e^{-J}}$$

(so that $\alpha = e^{2J}$), the assumption of independent occupancy can be stated as follows:

$$P(\mu_1, \mu_2, \ldots, \mu_n) = \frac{e^{J\mu_1} e^{J\mu_2} \ldots e^{J\mu_n}}{(e^J + e^{-J})^n}, \quad \mu_j = \pm 1$$

Here $P(\mu_1, \ldots, \mu_n)$ is the probability of occurrence of the configuration $(\mu_1, \ldots, \mu_n)$; $+1$ stands for an occupied subunit and -1, for an unoccupied one.

The MWC model assumes in effect that the molecule can exist in two states (R and T) described by the

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parameter ε; R corresponds to $\varepsilon = 1$ and T, to $\varepsilon = 0$.

The "occupancy configuration" is now described by $(\mu_1, \mu_2, \ldots, \mu_n; \varepsilon)$ —a symbol that describes which subunits are occupied (the μ's) as well as the state of the molecule (ε).

The basic assumption of the MWC model is, then,

$$P(\mu_1, \ldots, \mu_n; \epsilon) = \frac{\epsilon\, e^{J\mu_1} e^{J\mu_2} \ldots e^{J\mu_n} + \hat{L}(1-\epsilon)\, e^{J'\mu_1} \ldots e^{J'\mu_n}}{Z}$$

where $\hat{L}$ is interpreted as the equilibrium ratio of the T and R states and Z is the normalizing factor needed to make the probabilities of occurrence of all configurations add up to 1.

If one now sets

$$e^{2J'} = c\alpha (\alpha = e^{2J})$$

and

$$L = \hat{L} c^{-n/2}$$

it is a simple matter to obtain the MWC result

$$f = \frac{\alpha(1+\alpha)^{n-1} + Lc(L+c\alpha)^{n-1}}{(1+\alpha)^n + Lc(L+c\alpha)^n}$$

(remember that for hemoglobin $n = 4$).

It is perhaps worth noting that it is somewhat misleading to interpret the MWC assumption for P as implying that a molecule can exist in two states and that *in each state the subunits can be occupied independently*. If this were so we would obtain

$$f = P\frac{\alpha}{1+\alpha} + Q\frac{c\alpha}{1+c\alpha}$$

where P and Q are the probabilities of finding the molecule in the states R and T, respectively (*3*).

The Thompson model (taking a cue from models of cooperative phenomena in the theory of magnetism) assumes

$$P(\mu_1, \mu_2, \ldots, \mu_n) = \frac{1}{Z}\, e^{U\mu_1\mu_2} e^{U\mu_2\mu_3} \ldots e^{U\mu_{n-1}\mu_n} e^{U\mu_1\mu_n} e^{J\mu_1} \ldots e^{J\mu_n}$$

where U (> 0) is a parameter which measures the degree of "cooperativity" between neighboring subunits and Z is an appropriate normalizing factor.

The calculation is now a little more difficult, though well known to all familiar with the so-called one-dimensional Ising model.

The result is

$$f = \alpha\left\{\left[(1+\alpha+\delta)^{n-1}\left(1+\frac{2K+\alpha-1}{\delta}\right) + (1+\alpha-\delta)^{n-1} \times \left(1-\frac{2K+\alpha-1}{\delta}\right)\right] / \left[(1+\alpha+\delta)^n + (1+\alpha-\delta)^n\right]\right\}$$

where

$$K = e^{-4U}$$

and

$$\delta = [(\alpha-1)^2 + 4\alpha e^{-4U}]^{1/2}$$

Taking $U = 0.55$, and, of course, $n = 4$), we obtain an extremely good fit of experimental data.

Which of the models is "right"? This question lies outside mathematics, and it is not certain whether it can be answered at all in any sensible way.

If any moral is to be drawn from our discussion it is that the "sigmoidicity" of the f-versus-α curve can be accounted for by a number of models (at least two!) none of which clearly violates any of the sacred principles of chemistry.

If one takes dogmatically Mach's view that "economy of thought" is the determining criterion for choosing among competing theories or models, then the Thompson model (or, equivalently, the model of Koshlund *et al.*) wins over the MWC model, since it uses only *one* parameter (U), whereas the MWC model requires *two* ($\hat{L}$ and c). Nature, however, has been known to be "uneconomical," and perhaps, until more chemical clues become available, we will have to be content with quoting from a poem by Thomas Gray: "Where ignorance is bliss, 'tis folly to be wise."

An Adapting Automaton

Let me now turn to an entirely different type of ad hoc model.

The experimental background is as follows. A human observer placed before a radarscope is told to watch for a possible signal at a certain point on the screen. The experimenter decides at random to send or not to send a signal, the probability of his sending one being p. At specified times the observer is asked whether the signal is present or not; he is told whether the answer was right or wrong, and his "betting average" is calculated. The reason for the observer's uncertainty is the presence of random noise which, when the signal is sufficiently weak, interferes with perfect detectability.

The observer can commit two kinds of errors: (i) he will mistake a noise peak for the signal ("false alarm"), and (ii) he will fail to detect the signal when it has actually been sent (with possibly catastrophic implications when the game is "for real"); the "betting average" is the weighted mean (with weights $q = 1 - p$ and p, respectively) of the two possibilities.

Let us now postulate an "ideal observer" (*4*) who, first of all, knows the probability density $P_1(x)$ of noise alone and the probability density $P_2(x)$ of noise plus signal (Fig. 3). He also is assumed to know the probability p of the signal's being sent, and, being "ideal," he naturally wants to minimize the overall probability of committing an error.

A simple calculation shows that the optimum criterion is the following. If the deflection x on the radarscope satisfies the inequality

$$qP_1(x) - pP_2(x) < 0$$

the ideal observer should say that the signal is present, and if

$$qP_1(x) - pP_2(x) > 0$$

he should say that the signal is not present.

If the density functions P_1 and P_2 are such that there is a unique solution $\bar{\theta}$ of the equation

$$qP_1(x) = pP_2(x)$$

then the optimum criterion given above is of the "threshold type"; that is, the observer should say "Yes" if $x > \bar{\theta}$; he should say "No" if $x < \bar{\theta}$.

Under these circumstances the minimum probability of error is

$$p - \int_{\bar{\theta}}^{\infty} [pP_2(x) - qP_2(x)]\, dx$$

Now, the interesting and somewhat surprising thing is that a human observer after a period of training performs nearly as well as the ideal observer—that is, he too achieves the near-optimum (the minimum) probability of misguessing.

The surprise is, however, diminished if we realize that an extremely simple automaton can also perform optimally.

The automaton simply sets an arbitrary threshold θ_1 and says "Yes" if $x_1 > \theta_1$ and "No" if $x_1 < \theta_1$. If the answer is right, the threshold is kept for the next reading. If, however, the answer is wrong, the threshold is changed by $+\Delta$ or $-\Delta$, depending on

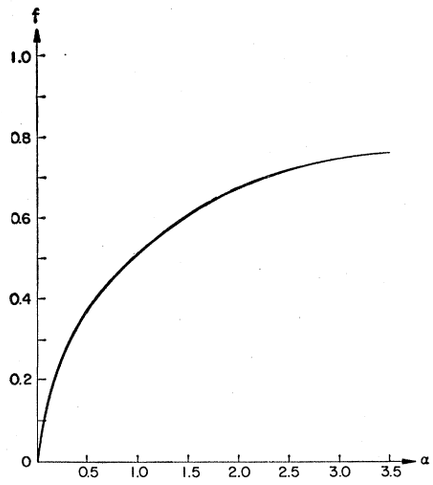

Fig. 1. The fraction (f) of oxygenated hemoglobin plotted relative to the concentration (α) of oxygen under the Michaelis assumption.

the kind of error. In this way a second threshold θ_2 is obtained, and the process is repeated over and over again.

The general rule is as follows: θ_{n+1} equals θ_n if the answer at the nth stage was correct; it equals $\theta_n + \Delta$ if the error at the nth stage was of type i; and it equals $\theta_n - \Delta$ if the error at the nth stage was of type ii.

The automaton does not know it, but it performs what is known as a "random walk" with transition probabilities

$$P(\theta_{n+1} = \theta_n + \Delta) = q\int_{\theta_n}^{\infty} P_1(x)\,dx$$

$$P(\theta_{n+1} = \theta_n - \Delta) = p\int_{0}^{\theta_n} P_2(x)\,dx$$

$$P(\theta_{n+1} = \theta_n) = 1 - p\int_{0}^{\theta_n} P_2(x)\,dx - q\int_{\theta_n}^{\infty} P_1(x)\,dx$$

It may be easily seen that no matter what θ_n is, the probability that it will move toward $\bar{\theta}$ is greater than the probability that it will move away from $\bar{\theta}$.

Thus $\bar{\theta}$ exerts a kind of attraction, and it is highly probable (this can be shown by a mathematical analysis) that the successive thresholds will all tend to be close to $\bar{\theta}$; thus a near optimum performance (5) is achievable (fluctuations around $\bar{\theta}$ are unavoidable owing to the fact that Δ is of nonzero size). As with all probabilistic models, there is always a possibility of the automaton's going haywire and running away. Unpleasant as it may be to have to acknowledge that we cannot exclude such erratic behavior, we may console ourselves with the thought that neither can it be excluded in human observers.

This model is not meant to be in any way indicative of the process by which human observers detect signals. In the light of more recent experiments (6), the very existence of sensory thresholds is apparently in some doubt. What the model does provide is a *standard* against which the performance of human observers can be judged.

The model also proves that simple automatons can be programmed for adaptation. Note that our automaton will approach optimum performance no matter what p, P_1, and P_2 are, as long as the equation

$$pP_2(x) = qP_1(x)$$

has a unique solution.

From the purely mathematical point of view the automaton merely solves the equation

$$pP_2(x) = qP_1(x)$$

by what is called the stochastic search, but it does it without knowing what equation it is supposed to solve!

Does Entropy Never Decrease?

Conceptual models, as I said at the beginning, are designed to elucidate difficult and delicate points of a theory.

Though I have done it repeatedly on past occasions, let me once again discuss the Ehrenfest urn ("dog flea") model.

The model was designed to deal with the apparent paradoxes which plagued the kinetic theory of matter almost from its inception.

Kinetic theory envisages matter as *conservative dynamical* systems of a vast number of degrees of freedom (a monoatomic gas, for example, is a system of $6N$ degrees of freedom where N is the number of molecules—that is, $N = 6.06 \times 10^{23}$ per mole). There are certain general theorems which such systems, when isolated, obey, one of them being the Poincaré recurrence theorem to the effect that, unless one starts at $t = 0$ with initial positions and momenta belonging to a small exceptional set (in technical terminology the exceptional set is of measure zero on the energy surface), the system will return over and over again arbitrarily close to the initial state. This "quasi-periodic" behavior of isolated conservative dynamical systems contradicts the approach to equilibrium—or, equivalently, the increase of entropy—which lies at the very basis of thermodynamics. It would thus seem that no logically consistent derivation of the laws of thermodynamics from a mechanistic viewpoint is possible.

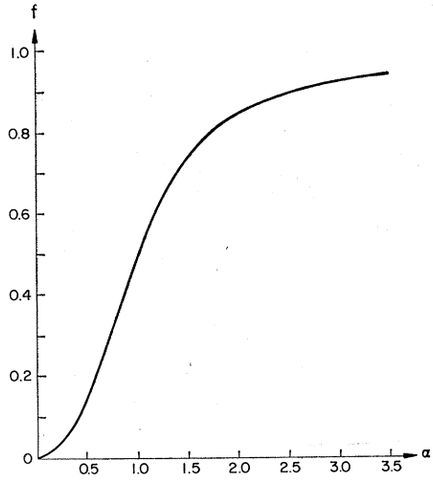

Fig. 2. The fraction (f) of oxygenated hemoglobin plotted relative to the concentration (α) of oxygen as actually measured.

To resolve this intolerable state of affairs a deeper formulation of the laws of thermodynamics becomes necessary. This is achieved through the intervention of a *statistical point of view*, and the Ehrenfest model illustrates the situation with remarkable simplicity and clarity.

Imagine two urns, I and II, in which there are distributed $2R$ balls numbered consecutively from 1 to $2R$. An integer from 1 to $2R$ (inclusive) is chosen at random, and the ball bearing that number is moved from the urn in which it is to the other urn. The process is then repeated over and over again, the successive choices of integers being independent of each other.

Let $P(n \mid m; s)$ denote the probability

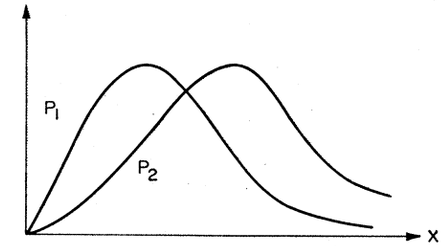

Fig. 3. The probability density $P_1(x)$ of noise alone and the probability density $P_2(x)$ of signal plus noise.

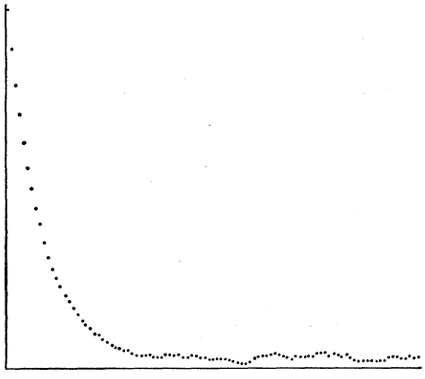

Fig. 4. Approach to equilibrium of the Ehrenfest model in a digital-computer experiment.

that m balls will be in urn I after s drawings if n balls were there originally. One can calculate $P(n \mid m; s)$ explicitly, but the formula is unfortunately quite complicated. It is easy, however, to calculate the average number of balls in urn I after s drawings, $< m(s) >_n$, given that $m(0) = n$—that is, that one has started with n balls.

The result is

$$< m(s) >_n = \sum_{m=0}^{2R} mP(n \mid m; s) =$$

$$R + n(1 - \frac{1}{R})^s$$

which indicates that, *on the average*, one approaches equalization; in fact, the approach is exponential.

On the other hand it is quite easy to prove that, with probability 1 (which does not mean absolute certainty), the initial state n is bound to recur over and over again, and this is a clear analog of the quasiperiodic behavior of dynamical systems.

However, the average time (actually, the average number of drawings) before state n recurs for the first time can be shown to be

$$\frac{2^{2R}}{\binom{2R}{n}}$$

which for n differing significantly from R (that is, far from equalization) is enormous. Over times very small compared to this enormous "Poincaré cycle," but still very large compared with durations of ordinary experiments or even with the duration of life on our planet, the standard deviation of $m(s)$ about its mean $< m(s) >_n$ is negligibly small relative to the mean, so that, over such periods of time, we shall observe exponential equalization even though over much, much longer periods the process is fundamentally quasiperiodic (*7*).

With the help of high-speed computers one can actually test the model.

Figure 4 is an "experimental graph" obtained with $2R = 16{,}384 = 2^{14}$ balls with $n = 2R$ (that is, as far away from equalization as possible) and with s running from 1 to 200,000 (such a run takes less than 2 minutes!).

The early part of the graph is a nearly perfect exponential.

This "experiment," incidentally, shows the computer in a new role—that of providing "artificial reality" for testing conceptual models.

Condensation in a One-Dimensional World

Finally, let me discuss briefly a model which is partly realistic and partly conceptual.

It is quite realistic to consider a gas as a dynamical system with molecules interacting through two body central forces. The intermolecular potential $\phi(r)$ can be assumed (not unrealistically) to be infinite for $0 < r < \delta$ (hard-core assumption makes it impossible for the molecules to come closer than δ from each other) and to be negative for $r > \delta$. It is also necessary to assume that $\phi(r) \to 0$ sufficiently rapidly, as $r \to \infty$, and at the very least that

$$\int_{\delta}^{\infty} r^2 \phi(r) dr$$

be finite.

If the molecules are at $\vec{r}_1, \vec{r}_2, \ldots, \vec{r}_n$, the potential energy is

$$E = -\sum_{1 \leq i < j \leq N} \phi\left(\left|\vec{r}_i - \vec{r}_j\right|\right)$$

where $\left|\vec{r}_i - \vec{r}_j\right|$ denotes the length of the vector $\vec{r}_i - \vec{r}_j$.

If our gas is in equilibrium at absolute temperature T, statistical mechanics provides us with a recipe for calculating its equation of state.

The recipe is as follows. Set

$$Z_N(\mathcal{V}) = \frac{1}{N!} \int_{\mathcal{V}} \cdots \int_{\mathcal{V}} e^{-E/kT} \, d\vec{r}_1 \ldots d\vec{r}_N$$

$\mathcal{V}$ being the container of volume V, and each integration (with respect to $d\vec{r}_1, d\vec{r}_2, \ldots$) being triple ($k$ is the Boltzmann constant).

Under appropriate conditions on ϕ, one then shows that, as N and V approach infinity in such a way that the specific volume $v = V/N$ remains constant, the limit

$$\lim \frac{1}{N} \log Z_N(\mathcal{V}) = -\frac{\psi(v, T)}{kT}$$

exists. The function $\psi(v,T)$, defined by the above limit, is the familiar free energy (per molecule), and pressure is then given by another familiar formula—namely,

$$p = -\frac{\partial \psi}{\partial v}$$

This is the equation of state.

One of the outstanding (and largely unsolved) problems is that of proving that, below a certain critical temperature, the isotherm has a flat part corresponding to coexistence of gaseous and liquid phases (the problem of condensation).

The problem simplifies greatly in one dimension. In fact, if exponential attraction—that is,

$$\phi(r) = \alpha\gamma e^{-\gamma r}, \; r > \delta (\delta > 0)$$

—is assumed while the hard-core assumption [$\phi(r) = \infty$. $0 < r < \delta$] is maintained, the exact equation of state can be calculated in terms of the largest eigenvalue of a certain integral equation (γ is a parameter whose reciprocal is a measure of the range of the attractive potential). For $\gamma > 0$, all isotherms are analytic and the model does not exhibit condensation. But in the limit $\gamma \to 0$ (weak long-range forces) one obtains rigorously the van der Waals equation with the so-called Maxwell construction (below the critical temperature) which gives the flat (horizontal) part of the isotherm (*8*).

Moreover, as shown recently by Dyson (*9*), if the attractive part of the potential is a linear combination of exponentials—that is, if

$$\phi(r) = -\sum_{1}^{\infty} \alpha_k e^{-\sigma_k r}$$

where the α_k and σ_k are positive constants and

$$\sum_{1}^{\infty} \frac{\alpha_k}{\sigma_k}$$

converges while

$$\sum_{1}^{\infty} \frac{\alpha_k}{\sigma_k^2}$$

diverges with sufficient rapidity, then the one-dimensional model will exhibit condensation without the artifact of an extra limiting process ($\gamma \to 0$).

These models are realistic insofar as we use more or less realistic interactions and fundamental inasmuch as we follow the strict dicta of statistical mechanics. They are conceptual and ad hoc, because, after all, there are no one-dimensional gases. Still, the study of such models has been most profitable, especially since it throws considerable light on the *mathematical mechanisms* which may be responsible for the phenomenon of condensation.

Conclusion

Models are, for the most part, caricatures of reality, but if they are good, then, like good caricatures, they portray, though perhaps in distorted manner, some of the features of the real world.

The main role of models is not so much to explain and to predict—though ultimately these are the main functions of science—as to polarize thinking and to pose sharp questions. Above all, they are fun to invent and to play with, and they have a peculiar life of their own. The "survival of the fittest" applies to models even more than it does to living creatures. They should not, however, be allowed to multiply indiscriminately without real necessity or real purpose.

Unless, of course, we all follow the dictum, attributed to Oswald Avery, that "you can blow all the bubbles you want to provided *you* are the one who pricks them."

References and Notes

1. It is interesting to note that, both in the case of atoms and in the case of genes, the underlying "discrete structure" was suggested by the appearance of definite ratios. This contrasts sharply with the origin of definite ratios of frequencies of tones of a string or a drum, or, for that matter, of energy levels of an atom. Here the underlying structure is continuous, as described by differential equations, and the discrete spectra arise because these equations have nontrivial solutions only if a certain parameter belongs to a discrete set of values.
2. See C. J. Thompson, *Biopolymers* **6**, 1101 (1968). Here one finds, also, references to Monod, Wyman, and Changeux and to Koshlund *et al.*
3. Since $P + Q = 1$ and $Q/P = \hat{L}$, the formula becomes

$$f = \frac{1}{1+\hat{L}} \quad \frac{\alpha}{1+2} + \frac{\hat{L}}{1+\hat{L}} \quad \frac{c\alpha}{1+c\alpha}$$

4. The theory of the "ideal observer," as well as a discussion of some of the experiments performed at the Radiation Laboratory during World War II, can be found in *Threshold Signals*, J. L. Lawson and G. E. Uhlenbeck, Eds. (McGraw-Hill, New York, 1950) (M.I.T. Radiation Laboratory Series, vol. 24). The theory is an adaptation of the Neyman-Pearson ideas relating to the testing of hypotheses.
5. See M. Kac, *IRE (Inst. Radio Engrs.) Trans. Profess. Group Inform. Theory* **IT-8**, 126 (Feb. 1962).
6. See, for example, J. A. Swets, *Signal Detection and Recognition by Human Observers* (Wiley, New York, 1964). I am grateful to R. W. Taylor of the Advanced Research Projects Agency for calling my attention to this reference.
7. For a more complete discussion see, for example, M. Kac, *Probability and Related Topics in Physical Sciences* (Interscience, New York, 1959).
8. M. Kac, G. E. Uhlenbeck, P. C. Hemmer, *J. Math. Phys.* **4**, 216 (1963).
9. F. J. Dyson, in preparation.

Aquatic Weeds

The rampant quality of aquatic weeds has become one of the symptoms of our failure to manage our resources.

L. G. Holm, L. W. Weldon, and R. D. Blackburn

In the evolution of the city as a habitat, in the conversion of virgin lands to intensified farming, and in the alteration of watercourses with locks, dams, and reservoirs, man is the interloper. At his behest the natural order of things is set aside. As a result of his activities and their byproducts, new species and numbers of weeds, rodents, insects, and diseases appear where they could not, or did not, exist before. One of our priceless treasures, fresh water, is changed as civilization draws near. Its quality usually becomes poorer; it is seldom improved by man. Communities, planned and unplanned, locate on the water's edge to use navigation routes, irrigate land, and develop power. As a result the watercourses are heated, polluted, and fertilized; the levels fluctuate, and new biological pests are introduced because of man's commerce and mobility.

Several "explosions" of aquatic weeds in the great rivers and lakes of the warm regions of the world have forced us to recognize the power of such infestations. They destroy fisheries, interfere with hydroelectric and irrigation schemes, stop navigation, and bring starvation and disease problems to riverine communities. The rapid growth of weed infestations has been both spectacular and frightening, and the publicity devoted to several of these problems in the past decade has made us aware that something is wrong.

Aquatic weeds obstruct water flow, increase evaporation, cause large losses of water through transpiration, and prevent proper drainage of land. Weeds may interfere with navigation, prevent fishing and recreation, depress real estate values, and present health hazards. In the western United States, Timmons (*1*) showed that 17 states lost 1,966,000 acre-feet of irrigation water annually because of aquatic and ditchbank weeds. This water, valued conservatively at $20 per acre-foot, is worth $39,230,000 (*2*). This is enough water to irrigate 132,000 to 315,000 hectares of cropland. In the United States there never has been an evaluation of the total nonagricultural losses due to aquatic weeds. It is certain that this loss, too, would be very high.

The aquatic environment is complex and is of interest to scientists in several disciplines. The management of aquatic vegetation is not a new science, but rather an old field of botany that has been recently revitalized because of increased demand on our fresh waters and the exponential growth in problems caused by aquatic vegetation.

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