Psychoanalysis has had to face a similar problem and has done so with only partial success. At its best, psychoanalysis is a cooperative project in which two people try to learn why one of them is suffering, so that he can become whole. The process of discovery is not a mechanistic one of finding a diseased part, but rather involves an unfolding, penetrating to new layers, uncovering a hidden system in which changes must take place before the deepest elements reveal themselves. Furthermore, the analyst must be able to understand and analyze the patient's transference, and to help the patient overcome his often deep dependency on the analyst. Thus, this very special type of investigation and therapy requires exceptional self-knowledge as well as skill on the part of the analyst, and considerable attention must be paid to the analysis of psychoanalysts so that they confront their own motivation and guard against the tendency to exchange the difficult role of scientist for the seductively easier one of either ideologist or medical high priest. In fact, from the start, the science of psychoanalysis has been weakened by ideological rifts and struggles. I suspect that this danger is present also in Freire's "social psychoanalysis" and that his pedagogues or "coordinators" must themselves go through some process of conscientization or critical self-awareness as well as methodological and theoretical training, if they are to guard against becoming manipulators, oppressing others with their own demands for greater militancy, cooperation, and so forth.

To a certain degree, all pedagogues can learn from Freire, just as all therapists can learn from psychoanalysis. But there is a particular problem about the revolutionary pedagogue as proposed in *Pedagogy of the Oppressed*. Freire is not explicit about how and under what auspices such an individual would enter the teaching situation. The analytic patient experiences a need and seeks help. In contrast, Freire was originally sent by the governments of Brazil and then Chile to educate the poor. In effect, he represented the liberal-reformist governments of Goulart and Frei, which sought to liberate and increase the consciousness of forgotten citizens in order to strengthen themselves against entrenched interests. Under what auspices does the nongovernmental pedagogue enter the scene? Is he invited by the people? And how does he describe himself?

A final word should be said about the difficulties of applying Freire's methodology, developed in rural Latin America, to a highly industrialized society such as the United States. On the one hand, the method may be useful for teaching more than simple literacy. There seems to be a need for culture groups, dialogue, and conscientization, and it is possible that groups of people will wish to undergo such a process, here as well as there. On the other hand, the symptoms of suffering and the quality of the consciousness are very different in the two types of societies.

There, in the "culture of silence," command of the word is the beginning of awareness. People say and write what they experience. They do not play games or confuse themselves with their roles. To become conscious is to wake up, as it were. Here, in the bureaucratic-organizational and multimedia society, the word tends to be made abstract and alienated from feeling and action. As a number of psychologists and analysts have pointed out, many people do not know what they feel, or have suppressed feeling in order to fit their roles. The sense of wonder has been lost. In rural Latin America, hopelessness has been caused by scarcity and oppression. Here it often comes from consumerism, anxiety about the future, and the lack of responsiveness or joy in human relations. To apply Freire's approach to our own society requires considerable study.

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A New Outline in Biology

Genetics of the Evolutionary Process. THEODOSIUS DOBZHANSKY. Columbia University Press, New York, 1970. xiv, 506 pp., illus. $10.95.

Over 30 years ago, in *Genetics and the Origin of Species*, Dobzhansky surveyed accumulated information from a century of field studies and some 30 years of experimental laboratory work, producing a highly original synthesis of evolutionary genetics. Through three editions (1937, 1941, 1951), this book served as a guiding reference and motivating force in evolutionary biology, a field to which the author has been one of the half-dozen most important contributors of original research. Now, in the year of his 70th birthday, Dobzhansky has given us a new outline of the biological theory of evolution, with special emphasis on its genetic aspects. With regard to the quality of this work, it is perhaps sufficient to note that it fully maintains the standards set in Dobzhansky's other books and that the author has again accomplished his objectives in masterly fashion.

The merit of the new book is not that it achieves a seminal synthesis establishing a whole new discipline, as *Genetics and the Origin of Species* did, but rather that it provides a comprehensive, authoritative, balanced, and insightful account of what we know and what we should like to know about the genetics of populations. The content is strictly up to date, and the author's concern for the historical development of ideas about evolution adds an extra dimension to the work, which is overall a model of organization and clear and concise exposition. Reading Dobzhansky's book, one becomes acutely aware of the shortcomings of books that attempt to cover a discipline by chapters written by different authors, for the thematic development and integration of ideas are outstanding.

The book opens with a critique of the reductionist approach to biology and a survey of the basic findings of molecular genetics as they relate to evolutionary theory, then turns to a consideration of mutation and normalizing selection. (Some typographical errors in the genetic code, table 1.2, and in the amino acid sequences in figure 2.2 of my review copy apparently have been corrected in later printings.) Chapter 5, in which Dobzhansky reviews recent evidence of the adaptive and coadaptive nature of inversions in *Drosophila* and of chromosomal modification in speciation, is especially outstanding. In the treatment of balancing selection and genetic load, considerable attention is properly given to new findings on the effects of mutants in heterozygous state and the influence of genetic background and to recent revolutionary changes in the concept of genetic load. The subject of genetically effective population size is particularly well presented, but a revised edition may include major changes in this section, for genetic drift and effective population size are currently being viewed in new ways by Kimura and others in relation to problems of the maintenance of protein polymorphisms in populations. Some "non-Darwinian" evolutionists may feel that their contributions have been slighted, yet I think the major arguments of the proponents of evolution by "random walk" are satisfactorily outlined. The Cavalli-Sforza–Edwards "phylogenetic tree" of human racial evolution is presented without critical comment, and several pages are devoted to phylogenies based on amino acid substitutions in cytochrome c and hemoglobin and to the concept of evolutionary clocks. Dobzhansky's treatment makes it clear that not all the arguments advanced by the neutralists have been effectively countered by those who would attribute most or all protein polymorphism and evolution to selection. Chapters 9 through 11, dealing with types and patterns of variation in populations, methods of reproductive isolation between species, and patterns of species formation, contain relatively little new material of major consequence, and these subjects have been covered in greater detail in Mayr's recent books. In the final chapter, Dobzhansky, touching upon such widely divergent topics as the mysticism of Teilhard de Chardin and the "eccentric" genetic system of *Oenothera*, sketches out major areas of knowledge and ignorance of evolutionary patterns and processes.

One of the dominant themes of the work is the "balance model" of genetic population structure, which "envisages a greater or lesser proportion of the gene loci being in the heterozygous state, frequently though not invariably for pairs of interacting alleles that give heterosis." The recent demonstration of extensive allelic variation at structural gene loci clearly supports this model (finally sinking the "classical" model, which assumes that there exists a "normal," largely homozygous type of each species or population). But the new discoveries do not, of course, necessarily support the "balance model" insofar as it envisages the variation's being maintained largely by heterosis or other forms of balancing selection.

I think that Dobzhansky has to a remarkable degree accomplished his aims of picturing evolutionary genetics as a field in an exciting period of development and of showing how current discoveries are upsetting "some classical theories that have acquired a status almost of dogmata." The book will serve as a text, a critical review for workers in evolutionary genetics, and an authoritative source of information for those biochemists, molecular biologists, ecologists, and others whose work leads them to considerations of the evolutionary process. In performing these functions it may well prove to be as important a contribution as *Genetics and the Origin of Species*. The next major development that lies ahead in evolutionary biology will be the integration of the growing body of data from biochemistry and molecular biology with that from population genetics, systematics, and ecology. Meanwhile, we have in Dobzhansky's book a superb evaluation of the present state of affairs from a master of the field.

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Mathematics as Metaphor

Dynamical System Theory in Biology. Vol. 1, Stability Theory and Its Applications. ROBERT ROSEN. Wiley-Interscience, New York, 1970. xiv, 302 pp., illus. $17.95. Series on Biomedical Engineering.

Everybody likes to discover general and unifying principles in biology. Unfortunately, the grammatical form of principles that claim generality does not tell how useful those principles are in particular experiences. To show that the modern theory of evolution and the fundamental models of molecular biology are widely useful in detail has required the imagination and hard work of naturalists and biochemists. The division of scientific labor may make it economical for some people to concentrate on empirical foundations, others on conceptual interpretation, unification, and development. But either without the other yields the sound of one hand clapping.

Because the elaboration of language is, for some people, much easier than the labor of establishing concordance or tension between general principles and experience, there is an enormous temptation to spin out language that has the sound and syntax of general principles and to declare it scientifically satisfying without scrupulous regard to its relations to reality. Rosen's newest book is not the only recent work on theoretical biology that succumbs to this temptation.

A "dynamical metaphor," according to Rosen, is a system of ordinary differential equations of specified form the qualitative behavior of which in some way resembles the qualitative behavior of a class of biological phenomena. Rosen proposes that dynamical metaphors be accepted as explanations of phenomena they resemble.

For example, a Rashevsky-Turing construction, as presented here, is a finite set of first-order, linear, autonomous, homogeneous ordinary differential equations. The coefficients of these equations are chosen so that (i) the variables can be identified with concentrations in an open chemical system, (ii) the unique critical point of the system is asymptotically stable, (iii) the location of the critical point depends on the coefficients (interpreted as rate constants), and (iv) "overshoots" in adjusting to perturbations can occur.

Rosen says,

> Now these four properties of open systems, as opposed to closed systems, are in a qualitative sense highly reminiscent of morphogenesis, and other characteristic features of metabolizing organisms. . . . many of the dynamical properties of organisms can be *explained* simply by knowing that the organism is in fact an open chemical system, and without knowing anything further about the specifics of its dynamics [pp. 194–95].
>
> What we really mean, then, when we say that the Rashevsky-Turing constructions can explain the phenomena of morphogenesis is the following: *Any particular morphogenetic system, with its own definite physicochemical structure, can be considered as a realization of some Rashevsky-Turing system,* by the proper identification of observables of the morphogenetic system with the state variables of the corresponding Rashevsky-Turing system. . . . [Though] clearly radically different from the more conventional explanations and descriptions built out of specific physicochemical models, . . . explanations of this kind are at least equally valid as those based on specific model-building, *and must be explicitly accepted scientifically on an equal footing* [pp. 189–90].

No single instance of any particular morphogenetic system is shown in this book to behave in detail like the state variables of any Rashevsky-Turing system. But all real morphogenetic systems must behave so for the same trivial rea-