

Concepts of Biology

Toward a New Philosophy of Biology. Observations of an Evolutionist. ERNST MAYR. Belknap (Harvard University Press), Cambridge, MA, 1988. xii, 564 pp. \$35.

Whether biology is an autonomous discipline or should ultimately be reduced to a branch of physics and chemistry has been a subject of debate for several decades. Ernst Mayr tells us in the first essay in *Toward a New Philosophy of Biology* that the issue is not one of "constitutive" reduction, since "none of the events and processes encountered in the world of living organisms is in any conflict with a physico-chemical explanation at the level of atoms and molecules" (p. 11, my italics). Mayr would also seem to believe that organisms are exhaustively composed of atoms and molecules, so that no residue remains once these are all taken into account: Mayr is not a vitalist. What is at stake in his view is "theory reduction, which postulates that the theories and laws formulated in biology are only special cases of theories and laws formulated in the physical sciences, and that such biological theories can thus be reduced to physical theories" (p. 11).

According to Mayr, "The attempt to unify science by reducing biology to physics has been a failure" (p. 11), and although "establishing and substantiating the autonomy of biology has been a slow and painful process . . . the evidence in support of the autonomy of biology has grown exponentially in recent years" (pp. 13–14). Mayr argues his case for the "emancipation" of biology not with logical or epistemological arguments but by enumerating, "one by one, some of the fundamental differences between organisms and inert matter" (p. 14), discussing in turn the complexity of living systems, organization in populations, possession of a genetic program, comparative versus experimental method, concepts in biology, laws versus theory, prediction, and teleology. As should be apparent, these items include theoretical and methodological issues that go much beyond differences "between organisms and inert matter." Nevertheless, the reader may wonder why they would amount to a case against the goal of reducing the theories of biology to those of physics and chemistry.

Mayr is among the greatest evolutionary theorists of our times, credited with the incorporation of systematics into the Synthetic Theory, the prevailing paradigm of

current evolutionary thought. His prolific output includes *Systematics and the Origin of Species* (1942), *Animal Species and Evolution* (1963), and the massive historical exposition *The Growth of Biological Thought* (1982). *Evolution and the Diversity of Life* (1976) is a collection of essays. The present book is also a collection of essays, most of which were originally published during the last decade (only two are also included in the previous collection). Five of the essays are primarily philosophical, whereas the majority deal with conceptual (and historical) issues of major evolutionary import. It is Mayr's hope that the book "will help to strengthen the bridge between biology and philosophy" (preface).

Organisms are adapted to certain ways of life and their parts adapted to perform certain functions. Fish are adapted to live in water, with a distinctive body shape and fins adapted for swimming and gills adapted for breathing in water. Before Darwin, the marvelous adaptations of organisms were generally attributed to the design of an omniscient Creator. A watch betrays the existence of a watchmaker; the functional design of organisms and their features would seem to argue for the existence of a designer. It was Darwin's genius to discover that the directive organization, or teleology, of living things could be explained as the result of a natural process—natural selection. The traditional connection between teleological phenomena and supernatural causes led scientists to reject teleological accounts as unscientific. Yet, as Mayr puts it, "The occurrence of goal-directed processes is perhaps the most characteristic feature of the world of living organisms" (p. 45); and "If teleological means anything, it means goal-directed" (p. 43, his italics). Mayr prefers, however, to use the synonym "teleonomic" (a term that was called a "genteelism" by Peter Medawar) to refer to goal-directed processes. The term "teleonomy" was introduced by C. S. Pittendrigh in 1959, because, according to Pittendrigh, "Teleology in its Aristotelian form has, of course, the end as immediate, 'efficient,' cause" (quoted on p. 63). This reflects a monumental misunderstanding: Aristotle explicitly contraposed teleological explanations to efficient ("causal," in the modern sense) explanations.

Mayr proposes the following definition: "A teleonomic process or behavior is one which

owes its goal-directedness to the operation of a program" (p. 45, his italics). The weight of this definition rests on the term "program," which is defined a few pages later as "coded or prearranged information that controls a process (or behavior) leading toward a given end" (p. 49, his italics). The two definitions are largely circular and leave us in the dark as to what is meant by "coded or prearranged information." The matter becomes troublesome when we are told that "the simplest program is perhaps the weight inserted into loaded dice . . . so that they are likely to come to rest at a given number" (p. 49). But assume that I find a small rock the configuration or weight distribution of which is such that the rock is likely to come to rest in a particular position. Has this rock a program? Is its behavior teleonomic? If so, natural teleonomy could hardly be distinctive of organisms. If what makes the weight in the loaded dice a program is that it was intentionally introduced rather than the result of a natural process, then teleonomic processes would necessarily imply a creator or designer, something Mayr surely rejects. Mayr's definition of teleonomy as a result of the operation of a program is unsatisfactory.

In 1959, Michael Scriven published an essay entitled "Explanation and prediction in evolutionary theory" (*Science* 130, 477–482) that was favorably received by many biologists. These seemed bothered by the claim upheld by most philosophers of science that science must be "predictive." Biology, said Scriven, is not predictive but "retrodictive." Mayr endorses this view: "As Scriven . . . has pointed out, the ability to predict is not a requirement for the validity of a biological theory" (p. 20). Mario Bunge's statement that "a theory can predict to the extent to which it can describe and explain" is cited with disapproval. "It is evident," comments Mayr, "that Bunge is a physicist; no biologist would have made such a statement" (p. 31).

I must confess that I am a biologist who is more in agreement with Bunge than with Mayr on this particular issue. The crux here is a distinction between temporal and logical prediction. Biological theories cannot predict, for example, the future course of events in an evolutionary lineage. But biological hypotheses and theories are tested, like any other scientific ones, by ascertaining whether predictions logically derived from a theory are indeed borne out in the world of experience. The predictions may be about events that occurred in the past but are unknown. A prediction states, in such a circumstance, that when the state of affairs is ascertained it will be as anticipated by the theory. It is thus possible to test hypotheses even in such biological subdisciplines as

phylogenetics, the reconstruction of evolutionary history. Assume that I propose that chimpanzees and humans are more closely related by descent to each other than they are to the orangutan. Numerous predictions can be derived: for example, that the primary structure of proteins (that is, their amino acid sequence) will be more similar on average between humans and chimps than between either one and the orangutan. We can then proceed to investigate the matter and, according to the outcome, accept or reject the hypothesis.

Mayr is more at home with evolutionary concepts and problems than with philosophical issues. *Toward a New Philosophy of Biology* has much to offer by way of insightful discussion of adaptation, natural selection, the origin of species, and macroevolution. There are also six chapters on Darwin. For Mayr, "The Darwinian revolution was the most fundamental of all intellectual revolutions in the history of mankind. While such revolutions as those brought about by Copernicus, Newton, Lavoisier, or Einstein affected only one particular branch of science, or the methodology of science as such, the Darwinian revolution affected every thinking man. A world view developed by anyone after 1859 was by necessity quite different from any world view formed prior to 1859" (p. 182). One can only agree.

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A Global Brain Theory

Neural Darwinism. The Theory of Neuronal Group Selection. GERALD M. EDELMAN. Basic Books, New York, 1987. xxii, 371 pp., illus. \$29.95.

While not the first to use Darwinian analogies and biology's powerful population thinking to approach the problem of how to wire up a brain, Gerald Edelman came to it from having tackled the analogous self-organization problems in the immune system; in the last dozen years, he has studied the counterpart selectivity theories for the nervous system in greater depth than anyone else, and so this book has been eagerly awaited.

Neural Darwinism is a fine example of Edelman's broad, bottom-up approach to how nervous systems get themselves organized, store information, and create new behavioral patterns. It is in the tradition of Donald Hebb's 1949 *Organization of Behavior*, asking "What is the nature of categoriza-

tion, generalization, and memory, and how does their interaction mediate the continually changing relationships between experience and novelty?" (p. 241). It should be read by neurobiologists, developmental biologists, the cognitive cognoscenti—including the artificial intelligentsia—and by all those hopeful technologists who are flocking to the banner of neural-like networks as an alternative way of shaping up smart machines.

But because *Neural Darwinism* is so ambitious (a "global brain theory") in its attempt to model neural ontogeny, it is an easy book to misjudge—and an even easier book to lay aside and never finish. It is marred by mind-numbing sentences such as "As a result, combinations of those particular groups whose activities are correlated with various signals arising from adaptive behavior are selected" (p. 5) and by nonbiological terminology such as "re-entrant" and "degeneracy." Edelman seldom unbends enough to use the tutorial approximations "feedback" and "different ways of doing the same thing." Incredibly, there is no glossary.

And the introduction omits the very items that could motivate readers to endure the theoretical presentation. Several decades ago, biologists began to realize that there was a lot of cell death going on in developing nervous systems, and theorists began to suggest that carving away cells might create functional patterns corresponding to long-term memory storage. Richard Dawkins in a 1971 *Nature* paper (which Edelman omits) made this explicit, though J. Z. Young's 1965 model for octopus memory is closer to the modern mainstream in selectively eliminating some synapses rather than eliminating entire cells.

Whatever the synapse turnover rate is (and no one even has estimates), there is an imbalance in the rates of making and breaking synapses during childhood. It causes us to reach adolescence with little more than half the number of cortical synapses that we had eight months after birth. What principles control the editing? Here, surely, is "neural Darwinism" in action. Since Edelman's models seem particularly relevant to the postnatal tuning-up process, it is even more surprising that this conceptually important background, from research on both humans and monkeys (for example, *Science* 232, 232 [1986]), is omitted. Edelman treats "wiring up" as preceding "tuning up," but such data suggest overlap throughout childhood.

Because biologists are often impatient with even lucid theoretical discussions, some will unfortunately skim until reaching the unusually attractive specific examples—which, in order to achieve their clarity, lack

the richness of Edelman's more general theory. Most attractive of all is the single fold-out color plate: this computer display reminds me of the back side of a colorful tapestry, little threads running here and there, as if they were axons in a tangential section of brain; their colors denote synaptic strengths between "cortical neurons." In the first frame, thanks to the randomized initial conditions, the picture is so haphazard as to suggest that Jackson Pollock himself had finally designed a true *tabula rasa*.

As the neural-like network gains experience (the sensory surface is stimulated, one point at a time and each point connects to many "cortical neurons"), one starts to see (in the second time frame of the color plate) red patches of strongly connected cells emerging from an increasingly blue boundary area where cells are weakly interconnected. Groups emerge, the physiological boundaries becoming far sharper than the underlying smear of anatomical connections—and all without instruction. Once you comprehend it, you may feel that this one color plate is worth the price of the book.

Eventually, in this map of a model hand, each patch will correspond to a top or bottom surface of a finger, looking not unlike the detailed maps of somatosensory cortex in monkeys, the plasticity of which has been studied by Michael Merzenich and colleagues. More impressively, Edelman and co-workers Leif Finkel and John Pearson can mimic the cortical rearrangements that occur when a finger is amputated (or overstimulated), though I note a revealing exception. Real cortical maps globally rearrange themselves, including boundaries between more distant digits—but the model's map shows only a local effect on the boundary between the affected digit and its immediate neighbor. It is as if, were an enlarging California to expand north into mid-Oregon, the Oregon-Washington and Canadian borders remained fixed (rather than also distorting, as real cortical boundaries tend to do).

Not everything that involves random initial conditions and selective survival deserves to be called Darwinism. The dance evolutionary biologists call the "Darwinian two-step," randomness-then-selection continuing back and forth for many rounds to increasingly shape up nonrandom-looking results, usually cannot be seen in Edelman's examples of neural Darwinism; these repeated injections of randomness lie at the heart of what some would consider as delimiting Darwinism from simpler forms of self-organization such as clumping and zero-sum "Matthewism."

And while the group selection of the subtitle may involve both groups and selec-