HUXLEY'S PLACE: ON THE DAIS


Even though nearly all evolutionary biologists know that T. H. Huxley debated Soapy Sam Wilberforce on the issue of Darwin's theory, only a minority among them could name a single one of Huxley's scientific papers. Yet Huxley was a professional academic, with a good academic reputation based on publication, before he became the celebrity-scientist who argued for evolution. M. A. di Gregorio looks for the real T. H. Huxley in the right place, the scientific papers that reflect Huxley's most concentrated and serious thought.

Most studies of the history of science have considered Darwin and Huxley together as a single personality, with its private (Darwin) and public (Huxley) aspects. Di Gregorio clearly separates the two men and shows that there were fundamental differences between them in their scientific philosophies and interests. These two philosophies distinguish among productive biologists today, and so di Gregorio's book offers more to interest biologists than merely correcting a detail of Victorian history.

Most biologists honor Darwin and Wallace for proposing natural selection as a mechanism for evolution. We now describe that mechanism in terms of gene frequencies of populations, which would have been impossible during Darwin's lifetime, mostly because no knowledge existed of the mechanism of heredity. The controversy in Darwin's time, however, did not concern this mechanism, but focused on whether evolutionary change occurred and on the historical event posited by Darwinians: that humans had evolved from an ape-like ancestor. Huxley and Haeckel largely contributed to this emphasis on what we now call phylogeny and taxonomy, rather than on mechanisms of change. Di Gregorio argues that Haeckel strongly influenced Huxley, perhaps more strongly than Darwin did. Di Gregorio examines Huxley's scientific papers and notes a lack of any evolutionary consideration before 1868, a decade after Darwin and Wallace's mechanism had been proposed. But it was probably Haeckel's Deszendenztheorie of 1866 that showed Huxley how he could apply evolutionary theory to his own research data.

Di Gregorio sees Huxley as "a fore-runner of pragmatism" who evaluated "laws" in science by their generality, simplicity, and accessibility through observation; such laws are not accepted or rejected on any absolute standard of experimental proof, but only by comparison to alternative formulations of "natural law." To di Gregorio, Huxley "can be viewed as following the traditional British empiricism of the epistemological analysis of sensory experience"; for Huxley, a law would be accepted or rejected by an absolute criterion: whether the expected consequences of the "law" occur. I believe di Gregorio is essentially correct in his contrast but inaccurate in his taxonomy of philosophy. On p. 49, he quotes Huxley's unfavorable review of Vestiges of Creation: "To use a phrase of M. Compte's—the mind of the Vestigarian [i.e., the anonymous author, Robert Chambers] is in the metaphysic stage, and confounds its own abstractions with objective fact." Di Gregorio does not follow up this evidence that Huxley was familiar with the French Auguste Compte's "positive philosophy," but it would explain the philosophical profile of Huxley that di Gregorio convincingly outlines if we see Huxley as a conscious follower of Compte's positivism.

Comte postulated that human thought passed through three major stages: a theological stage that viewed the objects of nature as being like the mind itself, with the feelings that are, in fact, the human response to those objects (this theological stage would range through substages from a "fetishistic," with each object considered alive and with good wishes and hostilities of its own, to a "polytheistic," with classes of objects each having its own anthropomorphic spirit animating them, to the "monotheistic," with a single god creating and controlling nature); a metaphysical stage, when nature is no longer thought of as the creation of anthropomorphic gods(s); instead, the ideas of nature that must be assumed to account for order are considered real in themselves, and nature is described and explained through philosophical arguments and manipulations of these abstract ideas; and the scientific or positive stage, when metaphysical speculations are discarded as unproductive and insoluble and scientific observation of real objects is the only test of truth about nature.

It is interesting that Compte's stages present a sort of evolutionary hypothesis, although confined to one species (humans). Compte conceived this as an actual scenario of change in human cultures through time and also as an ontogenetic sequence for a single human mind. If Huxley enthusiastically supported Compte's positivism, it would not have been hard for him to accept extending the idea of historical change to biological structure, so long as he could see himself making this extension in the third stage, on the basis of Darwin's scientific formulation, rather than through relapsing into the second stage (like the unfortunate Chambers). Even worse would have been relapsing into the first stage; Lamarck's perfectionist theory of evolution did not attract Huxley, perhaps because its assumption that every claw had a will to better itself smacked of the "fetishistic" substage of the theological stage. A single historical scenario that would serve for both the individual and entire species is obviously analogous to Haeckel's biogenetic law, and if Huxley accepted Compte he would be well conditioned for accepting Haeckel without much questioning of what the mechanism might be for making ontogeny recapitulate phylogeny.

Di Gregorio produces compelling evidence from Huxley's published and unpublished early scientific work that in his earlier years, Huxley was an idealistic typologist (i.e., in a metaphysical stage). The English translation of Compte's Cours de Philosophie Positive appeared in 1853, about a year before Huxley's review of the Vestiges. I suspect that Chambers had the misfortune to fall victim to a new convert to positivism, Huxley, who was behaving like those who, just after giving up tobacco, like to wander into the smoking car of railroad trains and insist everyone put out any cigarettes, cigars, or pipes. Although Huxley did not incorporate evolution into his scientific publications until 1866, in papers before that time, he appears to consider scientific laws as broad general statements rather than real entities.

Darwin and Wallace proposed a materialistic, as opposed to metaphysical, mechanism for modifying organisms, but it differed from previously suggested mechanisms in natural science because it was a statistical mechanism, like supply and demand in Adam Smith's economics. It is impossible to test such a mechanism by the "single-exception-that-falsifies-the-hypothesis" method of classical physics, and so it would not yield to any experimental method conceivable to Compte. Huxley's 1860 review of The Origin of Species (quoted by di Gregorio) notes the essential identity of nat-
Podolsky’s method to aid crop planting and pest management strategies assumes that temperature is the factor that primarily controls the environment. This method, which he terms phenological forecasting, combines phenological curves for the particular organism with site-specific phenotemperature nomograms (PTN). Phenological curves are the number of days a particular life stage of an organism lasts (e.g., from sprouting to mass flowering or time of egg development) as a function of the average temperature during that time span. The nomograms depict how average temperature changes over the year for a specific site. By combining the temperature nomograms with the biota-specific phenological curves, an organism’s development time at various times of the year are obtained.

Underlying the general method, a number of ecologically important factors must be considered to make the method useful. For example, with Anopheles mosquitoes, where the period of human susceptibility to malarial infection is worrisome, researchers must consider multiple cohorts of mosquitoes. Egg production as a function of temperature is also important.

Factors other than temperature influence many biota. Podolsky knows this and includes these factors in his method by generating different temperature phenological curves for different levels of these other controlling factors. Thus, he gives phenological curves for cotton grown under both normal agricultural conditions and on soil with crust and insufficient soil moisture. In cases where more than one additional factor are important a researcher must draw separate curves for all combinations of levels of each controlling factor.

The author conscientiously includes critiques by other Russian scientists of his method. The most serious criticisms are the difficulty of obtaining data to draw the phenological curves, the fact that many biota may respond to temperature thresholds and not temperature averages, and that diurnal changes in temperature can result in average temperatures giving an inaccurate portrayal of growth.

I am impressed both by the large number of organisms Podolsky has treated, and by the wide geographical area for which he has generated PTNs. These range all over the USSR and into Israel and cover such diverse biota as lily-of-the-valley, anopheles mosquitoes, and bacteria used for milk culture.

I find the book interesting mostly from a historical perspective. Most of this work was done during the 1950s and 1960s in the USSR. As such, it presages and to some degree parallels the development of ecological simulation. In some ways it could be looked at as a graphical analog to simulation. Many identical data are used to develop simulation models for similar processes. Podolsky’s methods differ in that they are mostly spatially extensive; simulation models tend to be site specific. In cases where temperature is not the only major factor, simulation is preferable to the method outlined here. In fact, this book can serve as a guide to intelligently interpreting the output of simulation models; I found this book most interesting in this respect.

The author makes some use of a computer, especially to do the calculations in constructing the PTN curves. Considering the current availability of computerized data processing and statistics, the method is ripe for further computer implementation, including using regression to fit the phenological curves to data and having the computer overlay the PTN and phenological curves. I find the large number of PTNs presented somewhat unnecessary.

Podolsky writes clearly, but uses language unfamiliar to English-speaking people, which makes reading somewhat slow going. The book shines in its case studies of various biota. The data and discussion here are very valuable. But the book’s length and its somewhat Byzantine organization lower its value as a home reference text considerably.

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A NEW APPROACH TO RESEARCH ON EVOLUTION?


The editors clearly state in their preface why they have collected these articles. They believe that “evolution theory is in crisis,” and that a change is on