

Life Sciences in the Twentieth Century

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FOREWORD

This is the third guide in the series *Teaching the History of Science: Resources and Strategies*, published under the auspices of the Committee on Education by the History of Science Society. These guides, written by specialists, are intended for the use of historians of science as well as general historians and any other teachers who wish to begin to revise a history of science course or to incorporate new topics into an existing course. The guides will be published in the Newsletter first, then as a pamphlet.

The first guide, *The Scientific Revolution*, appeared in the July 1986 issue of the Newsletter; the second, *Science, Technology, and Public Policy*, in the April 1987 issue. The editorial board for each guide is drawn from the Society's Committee on Education. The committee welcomes comments on the value of these guides, as well as on suggested topics for future guides.

INTRODUCTION

In recent years, scholarly work in the history of twentieth-century life science has increased dramatically. This development represents a break in two traditions within the field of history of science: first, from the dominating influence of the history of the physical sciences and mathematics; and second, from the focus on the period before the twentieth century.

Over many years of teaching I have found that the history of twentieth-century biology has a special interest for both graduate and undergraduate students from a wide variety of backgrounds. Students whose primary interest lies in the sciences (especially the biomedical sciences) find that they come to view current biological problems in a new perspective when they are familiar with their historical and philosophical background. Students whose primary focus is history and the social sciences have often found themselves more interested in science, especially the more generally accessible life sciences, when viewed in a historical, philosophical, or sociopolitical context. This guide provides an introductory bibliography for teachers at the college and university level who wish to include topics in the history of twentieth-century life science in courses ranging from biology proper to social and intellectual history, as well as in courses in the history of science itself.

The guide is restricted to the life, or biological, sciences and excludes medicine and health policy. While the main focus will be on the history, as opposed to the philosophy, of the life sciences, some philosophical issues will necessarily be included. The major trends I have chosen—heredity, development, evolution, physiology, ecology, and animal behavior—are those that have commanded the most attention from biologists themselves in this century. The final topic—the relation of biology to social and political issues—is an area that has increasingly become of interest to historians and sociologists of science, as well as to a growing number of biologists themselves.

GENERAL SOURCES

To understand the major developments within twentieth-century life science it is important to know something about its nineteenth-century background.

A particularly significant field is that of morphology, especially from the 1870s on. The mainstays of morphology were such disciplines as comparative anatomy, systematics, paleontology, and embryology, all aimed in one way or another at elucidating phylogenetic (evolutionary) history. It was a method of investigation—largely descriptive and often speculative—as much as a set of conclusions. One instructive study is Jane Maienschein's "Cell Lineage, Ancestral Reminiscence, and the Biogenetic Law," *Journal of the History of Biology*, 1978, 11:129-158, which discusses the influence of Ernst Haeckel and other morphologists on several key twentieth-century biologists. A history of the general relationship (one would say subservience) of embryology to evolution can be found in Stephen Jay Gould's *Ontogeny and Phylogeny* (Cambridge, Mass.: Harvard Univ. Press, 1977), a stimulating book treating both history and contemporary theory (as so rarely happens) within the same volume. A nineteenth-century encyclopedia entry, Patrick Geddes's "Morphology," *Britannica* (9th ed., London, 1883), Vol. XVI, pp. 837-846, provides a thumb-nail portrait of morphology as it was understood in its heyday.

Two short papers provide additional general perspectives on developments in biological theory during the nineteenth century. J. W. Wilson's "Biology Attains Maturity in the Nineteenth Century," in *Critical Problems in the History of Science*, edited by Marshall Clagett (Madison: Univ. Wisconsin Press, 1959), pp. 401-408, emphasizes particularly the roles of cell and protoplasm theory as organizing principles of mid- and late nineteenth-century biology. Paul Farber's discussion paper "The Transformation of Natural History in the Nineteenth Century," *Journal of the History of Biology*, 1982, 15:145-152, argues that natural history became more analytical and causal in the nineteenth century, seeking explanations rather than remaining content with cataloguing and description.

The only readily available work to deal exclusively with the twentieth century is Garland E. Allen's *Life Science in the Twentieth Century* (New York: Wiley, 1975; Cambridge Univ. Press, 1978). While not attempting to cover all topics, this book has chapters on the nineteenth-century background and on heredity, evolution, embryology, molecular biology, and general physiology. An extensive bibliographical essay surveys the pre-1978 literature in the field. The book is based on the theme that the life sciences in the twentieth century broke from the descriptive and speculative tradition that had dominated much of late nineteenth-century biology. Allen's thesis has been challenged in a provocative series of essays by Jane Maienschein, Ron Rainger, and Keith Benson, published with the author's own response and an evaluation of the discussion by Frederick B. Churchill, in *Journal of the History of Biology*, 1981, 14:83- 191.

Jane Maienschein surveys a number of major works in the history of twentieth-century life science in America in "History of Biology," *Osiris* 1985, N.S., 1:147-162. Although devoted to American biology, Maienschein's essay is particularly valuable since it focuses on a number of current historiographical issues.

In such a fast-moving field as the history of twentieth-century biology periodical literature serves as a valuable teaching resource. Numerous pertinent articles may be found in *Journal of the History of Biology*, *Mendel Newsletter*, *Journal of the History of Medicine and Allied Sciences*, and *Bulletin of the History of Medicine*. The now-defunct *Studies in History of Biology* (Johns Hopkins Univ. Press, 1977-1984) was devoted to longer essays in the history of biology and focused on the twentieth

century. Two journals that intermix history and philosophy of biology particularly well are *History and Philosophy of the Life Sciences* (Stazione Zoologica, Naples) and the newly launched (Spring 1986) *Biology and Philosophy*.

EXPERIMENTAL EMBRYOLOGY

By the late nineteenth century various biologists began to seek ways of incorporating more experimental methods and causal explanation into what had been basically a descriptive science. This trend was initially most prominent in embryology (developmental biology, in today's terms), but it soon spread to other fields such as heredity, evolution, and, ultimately, ecology, and animal behavior.

A useful collection of contemporary reviews papers discussing the state of embryological and morphological work in the 1890s comes from the Marine Biological Laboratory's famous Friday evening lecture series (1889-1899). Selected papers from this series have been reprinted with commentary by Jane Maienschein as *Defining Biology: Lectures from the 1890s* (Cambridge, Mass.: Harvard Univ. Press, 1986). This collection provides easy access to some authoritative but not overly technical primary source material that could easily be assigned to undergraduate students.

Secondary material on Wilhelm Roux, Hans Driesch, and early experimentation is not as plentiful as the subject demands. A brief and highly informative history of the idea of experimentation on embryos is Frederick Churchill's "Chabry, Roux, and the Experimental Method in Nineteenth-Century Embryology," in *Foundations of Scientific Method*, edited by Ronald Giere and Richard S. Westfall (Bloomington: Indiana Univ. Press, 1973), pp. 161-205. A significant issue in the Roux-Driesch controversy was the old seventeenth- and eighteenth-century debate over epigenesis and preformation—whether the embryo develops by organizing less-formed material into the structure of embryonic parts (epigenesis) or merely grows in size from an already-formed, miniature adult (preformation). An informative and insightful study of that controversy, from Roux and Driesch to twentieth-century figures such as C. O. Whitman, E. B. Wilson, and E. G. Conklin, is Maienschein's "Preformation or New Formation—or Neither or Both?" in *A History of Embryology*, edited by T.J. Herder, J. A. Witkowski, and C.C. Wylie (Cambridge: Cambridge Univ. Press, 1986), pp. 73-108. The Roux-Driesch debate is clear and straightforward, and easily accessible to almost all students.

Jane Oppenheimer's *Essays in the History of Embryology and Biology* (Cambridge: MIT Press, 1967) contains number of valuable contributions, most of which deal with early or mid-twentieth-century embryology. Viktor Hamburger's essay "Embryology," in *The Evolutionary Synthesis*, edited by Ernst Mayr and William Provine (Cambridge, Mass.: Harvard Univ. Press, 1980), pp. 96-112, provides an excellent overview of the growth of the whole field of embryology in the twentieth century.

Among the most significant developments in experimental embryology are those of Hans Spemann (1869-1941) and his school at Freiburg between 1900 and 1933. Despite his eminence and his receipt of the Nobel Prize in 1935, Spemann has enjoyed little attention from historians. Viktor Hamburger has written several essays on this school, of which he was a preeminent graduate, with special reference to the most important concept to emerge from their work, the "organizer theory." A general perspective is given in "Hans Spemann and the 'Organizer Concept,'" *Experientia* 1969, 25:1121-1125. A more recent essay by Hamburger details the work of one of Spemann's most notable women students and her role in discovering the organizer concept: "Hilde Mangold, Co-discoverer of

the Organizer, *Journal of the History of Biology*, 1984, 7:1-11. Hamburger has also prepared a book-length monograph on the Spemann school: *The Heritage of Experimental Embryology: Hans Spemann and the Organizer* (Oxford Univ. Press, 1988).

Kenneth R. Manning's *Black Apollo of Science: The Life of Ernest Everett Just* (New York: Oxford Univ. Press, 1983) is probably closer to institutional and social history than to history of embryology per se, yet Manning's biography of one of America's few prominent black scientists includes discussion of Just's various studies on fertilization, early embryonic development, and the properties of the cell surface. It also includes a great deal of information on racism in science, the problems of funding scientific research, hiring practices in American universities, black education, work in research institutions such as the Marine Biological Laboratory, and the relationship between the American and European scientific (largely, of course, biological) communities in the period 1920-1940. *Black Apollo* can thus serve as a useful introduction to many aspects of the social history of American biology.

Heredity (Genetics)

Because of the central position that genetics has come to occupy in twentieth-century biology, its history has been more fully documented than that of fields such as embryology. A recent and general source for views on the history of heredity is Ernst Mayr's massive *The Growth of Biological Thought* (Cambridge, Mass.: Harvard Univ. Press, 1982), especially Part II (Chs. 14-19). A useful introduction to the secondary literature is Mayr's essay review: "The Recent Historiography of Genetics," *Journal of the History of Biology*, 1973, 6:125-154. The most general and in some ways most complete source of ideas on heredity from the Greeks through the nineteenth century is F. J. Cole's *Early Theories of Sexual Generation* (Oxford: Clarendon Press, 1930). It does not treat the period after the rediscovery of Mendel in 1900, and it represents an older form of scholarship that is more descriptive than analytical. Yet it contains a wealth of information arranged chronologically. A more recent (and still available) general history is Hans Stubbe's *History of Genetics* (Cambridge, Mass.: MIT Press, 1972). Less comprehensive but more pertinent to those with an interest in the twentieth century are L. C. Dunn, *A Short History of Genetics* (New York: McGraw-Hill, 1965), and A. H. Sturtevant, *A History of Genetics* (New York: Harper & Row, 1965). Both focus on the period since the rediscovery of Mendel and are what would today be called highly "internalist" histories (that is, focusing on the history of the scientific ideas apart from their broader social and historical context). In the same vein, but more detailed about the actual genetics, is E. A. Carlson's *The Gene: A Critical History* (Philadelphia: Saunders, 1966). All three books have useful bibliographies of primary (mostly published) materials, with Carlson's being the most extensive.

A good deal of scholarly work exists on the life and work of Gregor Mendel (1822-1884), most of it pertinent to the history of twentieth-century genetics. Despite its age, Hugo Iltis's *Life of Mendel* (New York: Norton, 1932) is still a standard source. A more recent, broader work is by the historian of science Robert C. Olby. *In The Origins of Mendelism* (New York: Schocken Books, 1966), Olby summarizes much of the nineteenth-century work leading up to Mendel and analyzes Mendel's own work in this context. Vitezslav Orel's brief biography, *Mendel* translated by Stephen Finn (New York: Oxford Univ. Press, 1984) focuses on Mendel's work, but it is especially useful in describing the scientific, cultural, and agricultural milieu in Moravia at the time Mendel lived.

Mendel's actual work, including the "problem" of his rediscovery, is the subject of numerous writings. One of the most accessible is Curt Stern and Eva Sherwood's *The Origin of Genetics: A Mendel*

Source Book (San Francisco: Freeman, 1966). This book is particularly useful for students, since it contains reprints of a number of important papers by Hugo de Vries and RA. Fisher.

The historical problem of Mendel's long neglect (thirty-five years from the publication of his paper on hybridization in 1866 until its rediscovery by Carl Correns, Erich Tschermak, and de Vries) has been the subject of many scholarly inquiries. Most of the arguments are summarized in Olby's *Origins* and Mayr's *Growth*. Robert Olby also published a challenging essay: "Mendel No Mendelian?" *History of Science*, 1979, 17:53–72. Olby's point is that much of what we call "Mendelian" today was not a part of Mendel's original conception and can be claimed to be so only if we read the ideas back into his work. Augustine Brannigan in "The Reification of Mendel," *Social Studies of Science*, 1979, 9:423-454, argues that Mendel's main interest in hybridization lay in the light it could throw on the species problem, an issue that was less important to his contemporaries than the practical questions of animal and plant breeding. By 1900, Brannigan suggests, the species question had become of greater interest to the biological community, and hence there was a more favorable atmosphere for understanding and accepting Mendel's main argument.

Prior to the rediscovery of Mendel's work, Francis Galton (1822-1911) had already initiated a totally different approach to the problem of heredity: what became known as biometrics, or the measurement of traits within populations. The biometrical movement developed an extensive research program with Galton and, later, his protege Karl Pearson (1857-1936) as its leaders. A good introduction to the issues is William Provine's *Origins of Theoretical Population Genetics* (Chicago: Univ. Chicago Press, 1971; esp. Chs. 2 and 3).

Historians with a more explicitly sociological point of view have since traced the origin and content of early biometrical work to the social class interests and perspectives of its founders. Of particular interest is Donald MacKenzie's *Statistics in Britain, 1865-1930* (Edinburgh: Edinburgh Univ. Press, 1981). MacKenzie emphasizes how the development of biometrical thought was intimately connected to social purposes such as eugenics in the work of Galton, Pearson, and R. A. Fisher.

The course of the debate between biometricians and Mendelians makes an important case study of the role of methodology in science. The controversy is the subject of the chapters in Provine's *Origins* and of two interesting papers: Lyndsay Farrell, "Controversy and Conflict in Science: The English Biometric School and Mendel's Laws," *Social Studies of Science*, 1975, 5:269-301; and Donald MacKenzie and Barry Barnes, "Scientific Judgment: The Biometry-Mendelian Controversy," in *Natural Order: Historical Studies of Scientific Culture*, edited by Barry Barnes and Steven Shapin (Beverly Hills, Calif.: Sage, 1979), pp. 191-210.

An important aspect of the history of Mendelian theory in the twentieth century has been the field of cytology—the study of cell structure and function. Most important for heredity, of course, is study of the cell nucleus, in particular the chromosomes. There was considerable debate in the late nineteenth and early twentieth centuries on the function of the chromosomes and the significance of the events surrounding mitosis and meiosis. A detailed and extensive analysis is Hermann J. Muller's introduction to the reprint edition of E. B. Wilson's *The Cell in Development and Inheritance* (New York: Johnson Reprint, 1966). Another comprehensive study is William Coleman's "Cell, Nucleus and Inheritance: An Historical Study," *Proceedings of the American Philosophical Society*, 1965, 109:124-158, which moves chronologically from Darwin's pangenesis (his provisional speculations about hereditary units) to the more empirical work of August Weismann (1834-1937), to the discovery of nuclein, or DNA. This is a marvelous survey presented with a keen historian's eye.

The so-called classical school of genetics arose after 1910 and is best characterized as the synthesis of Mendelian breeding experiments with cytological studies of chromosome structure and behavior. This work developed chiefly in the United States, though major contributions came from England, Germany, and Scandinavia. Several books deal wholly or in part with this period and can be used as a basic entry into the wider literature of twentieth-century genetics. Dunn (*A Short History*) and Sturtevant (*A History*) were both prominent practitioners of classical genetics, and it is not surprising that their histories concentrate heavily on the classical period. Carlson also focuses on that period in *The Gene*. Several biographies provide a good introduction to the basic developments. Thomas Hunt Morgan (1866-1945) is considered to be one of the major architects of the chromosome theory of heredity. His life and work is the subject of two biographies.

With mutation, one of the most influential concepts put forward in twentieth-century genetics was the distinction between genotype and phenotype, first enunciated by the Danish botanist Wilhelm Johannsen (1857-1927) in 1909. His work has been admirably discussed by Frederick Churchill in "Wilhelm Johannsen and the Genotype Concept," *Journal of the History of Biology*, 1974, 7:5-30.

In addition to some skepticism about the importance of chromosomes, another stumbling block to the acceptance of Mendelian theory emerged in the 1920s and the 1930s: the concept of cytoplasmic inheritance. Cytoplasmic inheritance refers to the possibility that some traits are transmitted from parent (usually mother) to offspring through the egg cytoplasm rather than through the nucleus. Cytoplasmic inheritance and the opposition it met from classical geneticists are beautifully laid out in Jan Sapp's "The Struggle for Authority in the Field of Heredity, 1900-1932: New Perspectives on the Rise of Genetics," *Journal of the History of Biology*, 1983, 16:311-342. This article is a particularly fine integration of technical, sociological, and intellectual history. Complementing Sapp's study is a recent paper by Jonathan Harwood, "The Reception of Morgan's Chromosome Theory in Germany: Inter-war Debate over Cytoplasmic Inheritance," *Medizinhistorisches Journal*, 1984, 19:3-32, which describes the mixed reception Morgan's chromosome work received in Germany owing to the prevailing views there (and elsewhere in Europe) on the role the cytoplasm might play in heredity. Sapp's more recent book, *Beyond the Gene* (Oxford Univ. Press, 1987), spells the story in more depth than his earlier article.

The history of human genetics has been the focus of only a few writings, despite its obvious-importance and interest. Three of these deserve special mention: Charles E. Rosenberg, "Charles B. Davenport and the Beginnings of Human Genetics," *Bulletin of the History of Medicine*, 1961, 35 (3): 266-276; Curt Stern, "Mendel and Human Genetics," *Proceedings of the American Philosophical Society*, 1965, 109 (4): 216-226; and, for post-1930s developments, the later chapters of Daniel J. Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (New York: Knopf, 1985).

The relationship between genetics and embryology in the twentieth century presents a curious mixture of integration and fragmentation. The overall history has been told by Viktor Hamburger in Mayr and Provine's *The Evolutionary Synthesis*, pp. 97-111. More recent is Scott Gilbert's insightful essay "The Embryological Origins of the Gene Theory," *Journal of the History of Biology*, 1978, 11:307-351, which traces the development of the gene theory to embryological concerns with the problems of differentiation and control. The philosophical roots of the split between genetics and embryology after about 1915 are explored in Garland E. Allen, "T. H. Morgan and the Split Between Embryology and Genetics, 1910-1935," in *History of Embryology*, ed. Herder et al, pp.113-144 (see Section 2 above).

Historical explication of the socioeconomic (agricultural) context of the origin of genetics has had a slow but increasingly promising start. One important proponent of this interpretation, Vitezslav Orel, Director of the Mendelianum of the Moravian Museum in Brno, has traced such interest in the work of Mendel himself. Orel's short paper "Interest in Hybridization in Moravia before Mendel Came to Brno," *Journal of Heredity*, 1973, 64(1), suggests a far closer relationship between Mendel's interest in genetic transmission (as we call it today) and the problems of agricultural productivity than has been recognized before. See also V. Orel and R.J. Wood, "The Sheep Breeders' Legacy to Gregor Mendel," in *Gregor Mendel and the Foundation of Genetics*, edited by Orel and Anna Matalova (Brno: Mendelianum/Czechoslovak Society for the History of Science, 1983), pp. 57-75.

Several essays by Charles Rosenberg have suggested the importance in America of agriculture to the growth of genetics in the twentieth century. One of the few historians to follow Rosenberg's lead has been Barbara Kimmelman, whose article "The American Breeders' Association: Genetics and Eugenics in an Agricultural Context, 1903-1913," *Social Studies of Science*, 1983, 13:163-204, shows clearly the importance many agriculturalists saw in Mendel's work. A far more radical, overtly economic analysis can be found in R. C. Lewontin and Jean-Pierre Bertan, "Technology, Research, and the Penetration of Capital: The Case of U.S. Agriculture," *Monthly Review*, 1986, 38:21-34; and Bertan and Lewontin, "The Political Economy of Hybrid Corn," *Monthly Review*, 1986, 38:35-47. Both articles are clearly presented and provocative.

Evolutionary Theory

The checkered career of Darwinian theory in the twentieth century provides insights into both the history of ideas and the actual problems inherent in the content of evolutionary theory itself. I have found teaching the history of evolutionary thought, from before Darwin through the present day (or at least up through the period of synthesis in the 1930s), an exciting and rewarding classroom experience.

The most general and up-to-date source covering the breadth of evolutionary theory is Peter Bowler's *Evolution: The History of an Idea* (Berkeley: Univ. California Press, 1984). Bowler includes interesting chapters on the social implications of evolutionary theory (these cover Social Darwinism, eugenics, and race movements of the late nineteenth and early twentieth centuries), as well as on the modern debates (neo-Lamarckism, punctuated equilibrium, and creationism). This is a useful book. Equally valuable for its sweep, but more comprehensive than Bowler, is Ernst Mayr's *Growth, Part II*. Chapters 7-13 deal explicitly with evolutionary theory in the period from the Enlightenment to the present. As a participant in the synthesis of the 1940s and 1950s himself, Mayr writes from a special perspective, but his knowledge of the field, especially systematics and its integration into evolutionary theory, is prodigious. Both Bowler and Mayr have extensive bibliographies of the primary and secondary literature. Mayr's book is less useful to students than Bowler's paperback, which serves well as a textbook.

A number of varied sources are available on more specialized topics within twentieth-century evolutionary theory. Stephen Jay Gould's essays from *Natural History* often deal with historical aspects of evolution, paleontology, and systematics. Many of the best of these have been collected into three paperback volumes: *Ever Since Darwin* (New York: Norton, 1977), *The Panda's Thumb* (New York: 1980), and *Hen's Teeth and Horse's Toes* (New York: Norton, 1983). Selected wisely for class use, these articles can be captivating; they are also good history. They are however, episodic, making it difficult to trace development or continuity.

The ill repute into which Darwinian theory had fallen by 1900 is chronicled in recent through study by Peter Bowler, *The Eclipse of Darwinism* (Baltimore: Johns Hopkins Univ. Press, 1983). The teacher interested in reading contemporaries' own accounts of their views on Darwin should see Vernon L. Kellogg's *Darwinism Today* (New York: Holt, 1907), a masterful summary with extensive excerpts from original sources.

The evolutionary synthesis itself has been the subject of a number of recent articles and monographs. The best overall introduction is Provine's *Origins*, which focuses particularly on the work of R.A. Fisher (1890-1962), J.B.S. Haldane (1892-1964), and Sewall Wright (1889-1988). Although it does not deal much with the social or historical context, Provine's book is concise, straightforward, and eminently clear. Mayr's *Growth* devotes one chapter (12) to the "synthesis" proper, and another (13) to "post-synthesis developments." These chapters serve as a useful introduction, though, as always, events are seen from Mayr's particular perspective as one of the "synthesizers." A more detailed and varied set of interpretations can be found in Mayr and Provine's *Evolutionary Synthesis*. An interesting collection of papers can be found in a volume edited by Marjorie Grene, *Dimensions of Darwinism: Themes and Counter Themes in Twentieth-Century Evolutionary Theory* (Cambridge: Cambridge Univ. Press, 1983). An important though not-much-discussed contribution to the development of evolutionary theory came from the Russian school of population genetics in the 1920s and 1930s, spearheaded by Sergei S. Chetverikov (1880-1959). Two articles by Mark B. Adams detail clearly and elegantly the developments in Russian natural history and population genetics that were eventually to be carried to the West by Theodosius Dobzhansky (1900-1979): "The Founding of Population Genetics: Contributions of the Chetverikov School, 1924- 1934," *Journal of the History of Biology*, 1968, 1:23-39; and "Towards a Synthesis: Population Concepts in Russian Evolutionary Thought: 1925-1935," *Journal of the History of Biology*, 1970, 3:107-129.

With regard to individual figures important in the synthesis, two are subjects of recent significant biographies. Joan Fisher Box's *R. A. Fisher: The Life of a Scientist* (New York: Wiley, 1978) provides a lively and candid portrait of her father as a mathematician driven to biology by his social, eugenical concerns. William Provine's *Sewall Wright and Evolutionary Biology* (Chicago: Univ. Chicago Press, 1986) is definitive in delineating Wright's important, but largely unsung, contributions to modern population genetics. Provine has also written a lengthy introduction to a series of collected papers by Theodosius Dobzhansky, who more than anyone else carried out the fieldwork to complement Wright's theoretical formulations: "Origins of the Genetics of Natural Population Series," in *Dobzhansky's Genetics of Natural Populations, I-XLII*, edited by R. C. Lewontin, John A. Moore, William Provine, and Bruce Wallace (New York: Columbia Univ. Press, 1981), pp. 5-83. This essay is among the best of the few discussions available on the importance of Dobzhansky's many contributions to the development of twentieth-century evolutionary theory.

Though superseded by both Darwin's concept of natural selection and Weismann's concept of the continuity of the germ plasm, the idea of inheritance of acquired characteristics continued to attract a certain following in the United States and Europe (including Russia) well into the twentieth century. A comprehensive survey of the topic is L.I. Blacher's *The Problem of the Inheritance of Acquired Characteristics*, English translation edited by Frederick Churchill (Washington, D.C.: Smithsonian Institution Library, 1982). Blacher begins with Lamarck, proceeding through Darwin to the present. Perhaps in response to the Lysenko affair in the USSR in the 1940s and 1950s, the author is particularly vehement in denying the possibility of inheritance of acquired traits. Not so, however, is the journalist turned historian of science Arthur Koestler, whose biography of the

neo-lamarckian Paul Kammerer, *The Case of the Midwife Toad* (New York: Random House, 1972), is a blatant attempt to rehabilitate the neo-lamarckian idea. Koestler is an accomplished writer, and his book makes compelling reading. But his conclusions about the inheritance of acquired traits must be taken with a grain of salt.

Compared to other areas of evolutionary thought, the history of human evolution has received much less attention. Darwin's thoughts on the matter are the subject of Howard Gruber's *Darwin on Man* (Chicago: Univ. Chicago Press, 1981). John Reader's older *Missing Links: The Hunt for Earliest Man* (London: Collins, 1961) was written before the bulk of newer discoveries and hence seems outdated even in its historical perspective. Much more current and historically illuminating is Peter Bowler's *Theories of Human Evolution: A Century of Debate, 1844-1944* (Baltimore: Johns Hopkins Univ. Press, 1986). Several papers can be used to illustrate specific aspects of the topic. For the social center of some of the early work on fossil human: see Michael Hammond's "The Expulsion of the Neanderthals from Human Ancestry: Marcellin Boule and the Social Context of Scientific Research," *Social Studies of Science*, 1982, 12:1-36. For a particularly fascinating study of accounts of human evolution as examples of heroic narratives, see Misia Landau's "Human Evolution a Narrative," *American Scientist*, 1984, 72:161-168. As an interesting sidelight the story of the Piltdown forgery has been told on numerous occasions, but by none so well as in Stephen Jay Gould's essay "The Piltdown Conspiracy," *Natural History*, August 1980, 89:8-28, and the follow-up, "Piltdown in Letters," *Natural History*, June 1980, 90:12-30.

The topic of scientific creationism and its history is treated in Section 8.

The area of speculation and experimentation on the origin of life provides a number of intriguing and informative case histories in the development of biology. The broadest as well as the most readable account is John Farley's *The Spontaneous Generation Controversy from Descartes to Oparin* (Baltimore: Johns Hopkins Univ Press, 1974). This is a fascinating book highly recommended for students.

Biochemistry & Molecular Biology

Biochemistry interfaces with biology and chemistry and is concerned with the chemical processes that take place within living cells. Modern biochemistry developed out of and largely came to replace what in the nineteenth and early twentieth centuries was called physiological chemistry, which dealt more with extracellular chemistry, such as the chemistry of digestion and of body fluids.

Biochemistry as such is largely, though not exclusively, a twentieth-century discipline. Molecular biology, on the other hand, has come to mean the study of the function and the three-dimensional structure of such biologically important macromolecules as proteins and nucleic acids. Molecular biology is as much an interface of biology with physics as of biology with chemistry. In many respects biochemistry and molecular biology represent the realization of the dream of early twentieth-century mechanistic biologists, who were convinced that the most fundamental biological processes could ultimately be understood in terms of the laws of physics and chemistry. For a good general introduction to the historical traditions embodied in biochemistry and molecular biology, see Scott Gilbert's "Intellectual Traditions in the Life Sciences: Molecular Biology and Biochemistry," *Perspectives in Biology and Medicine*, 1982, 26: 151-162.

The history of biochemistry is a young but growing field of investigation. Among the existing general texts are two that attempt to be systematic and cover the subject from antiquity: Henry M. Leicester's *Development of Biochemical Concepts from Ancient to Modern Times* (Cambridge, Mass.: Harvard

Univ. Press, 1974); and Joseph S. Fruton's *Molecules and Life* (New York: Wiley, 1972). Both are relatively straightforward accounts that present a historical framework on which more interpretative views can be hung. Perhaps more interesting reading is a collection of essays edited by Joseph Needham, *The Chemistry of Life: Lectures on the History of Biochemistry* (Cambridge: Cambridge Univ. Press, 1970), which contains papers by a number of eminent biochemists and historians on various aspects of the history of biochemistry.

Much of the history of twentieth-century biochemistry owes its origin or stimulus to the pioneering work of a chemist turned historian, John T. Edsall, emeritus professor of biological chemistry at Harvard. The results are summarized in Edsall and David Bearman's "Historical Records of Scientific Activity: The Survey of Sources for the History of Biochemistry and Molecular Biology," *Proceedings of the American Philosophical Society*, 1979, 123(5):279-292. A recent work that discusses the social context of scientific research in American biochemistry is Robert E. Kohler's *From Medical Chemistry to Biochemistry* (Cambridge: Cambridge Univ. Press, 1982). Kohler traces the shift from physiological chemistry to modern biochemistry in the light of reform in medical education and the institutionalization of academic disciplines in the period 1870-1930.

Some of the developments that have come to fruition in molecular biology since the 1950s originated in the area known as biochemical genetics. While the history of biochemical genetics has not yet received the attention it deserves, some inroads have been made. Bentley Glass's older "A Century of Biochemical Genetics," *Proceedings of the American Philosophical Society*, 1968, 109(1): 227-236, is still a good introduction for the general reader by another eminent scientist turned historian.

The history of the discovery of DNA has been told a number of times by writers of very different genres, from actual participants to journalists and historians of science. Of the several books on the subject, clearly the most authoritative and comprehensive are Robert Olby's *The Path to the Double Helix* (Seattle: Univ. Washington Press, 1974) and Horace Freeland Judson's *The Eighth Day of Creation* (New York: Simon & Schuster, 1979). The former is by a historian of genetics and asks the more broadly historical questions. Judson's work is of no less high quality, however, and in some areas, such as the relation of biochemistry to molecular genetics, is more complete than Olby's. Judson has done heroic service by interviewing many of the principals, and his work is liberally illustrated with candid photos that add to its human interest.

James D. Watson's *The Double Helix* (New York: Atheneum, 1968) was the first book on the history of the DNA discovery. It is a relatively brief work and reads like a detective story, but as history it has to be taken with a grain of salt. Of particular interest in the DNA story is the work of Rosalind Franklin (1920-1958), the brilliant X-ray crystallographer at Kings College, London, whose studies were of critical importance to Watson and Francis Crick in providing precise data on interatomic distances within the DNA molecule. Anne Sayre's *Rosalind Franklin and DNA* (New York: Norton, 1975) is a masterful essay in scientific biography. It portrays particularly well the problems that women scientists such as Franklin experienced in trying to get and hold research positions in a field dominated (like most sciences) by men. In a similar vein, and also dealing with the history of genetics, Evelyn Fox Keller's *A Feeling for the Organism: The Life and Work Of Barbara McClintock* (San Francisco: Freeman, 1983) raises forcefully the issue of women in science and their struggle to be accepted as equals. For those who only want to dip briefly into the subject, Alfred E. Mirsky's shorter "The Discovery of DNA," *Scientific American*, June 1968, 218:78-88, presents the basic outline of events and conceptual problems. A more sociological and contextual account of the "path to the double helix" can be found in Donald Fleming's "Emigré Physicists and the Biological

Revolution,” in *The Intellectual Migration: Europe and America, 1930-1960*, edited by Donald Fleming and Bernard Bailyn (Cambridge, Mass.: Harvard Univ. Press, 1969). This masterful essay traces many aspects of the origins of molecular biology to the emigration of European (especially Austrian) physicists such as Max Delbrück and Erwin Schrödinger to England and the United States in the 1930s, with their interest in seeking new laws of physics in the study of living matter.

The role of the Rockefeller Foundation, under the aegis of Warren Weaver, in actually influencing the growth of molecular biology has become the subject of a lively controversy. An essay by Lily E. Kay, “Conceptual Models and Analytical Tools: The Biology of Physicist Max Delbrück,” *Journal of the History of Biology*, 1985, 18:207-246, challenges the notion that the field of “molecular biology” was actually created by Weaver and the Rockefeller Foundation in the 1930s. A similar interpretation is offered by Pnina Abir-Am in “The Discourse of Physical Power and Biological Knowledge in the 1930’s: A Reappraisal of the Rockefeller Foundation’s ‘Policy’ in Molecular Biology,” *Social Studies of Science*, 1982, 12:341-382. One of a younger group of sociologically oriented historians of science, Abir-Am questions the role of private philanthropy in actually creating and shaping the development of the “field” of molecular biology. A similar point is made by Robert Kohler in “The Management of Science: The Experience of Warren Weaver and the Rockefeller Foundation Programme in Molecular Biology,” *Minerva*, 1976, 14:279-306. See also Edward Yoxen, “Giving Life a New Meaning: The Rise of the Molecular Biology Establishment,” in *Scientific Establishments and Hierarchies* edited by Norbert Elias, Herminio Martins, and Richard Whitley (Dordrecht: Reidel, 1982), pp. 123-143.

An interesting, though older and by now “classic” source is *Phage and the Origins of Molecular Biology*, edited by John Cairns, Gunther Stent, and James D. Watson (Cold Spring Harbor, N.Y.: Cold Spring Harbor Laboratory of Quantitative Biology, 1966). A number of the essays included are historical or reminiscent in character, and indeed the volume might be viewed largely as primary source material. For a neglected side of the story, see Seymour S. Cohen’s “The Biochemical Origins of Molecular Biology (Introduction),” *Trends in Biochemical Sciences*, 1984, 9:334-336, which argues that many of the histories of molecular biology have ignored the contributions of biochemistry to molecular genetics in general and to the discovery of DNA in particular.

The development of molecular genetics on the heels of Mendelian genetics has raised the inevitable philosophical question of whether the discovery of DNA represents the ultimate reduction of biological to physicochemical processes. Numerous philosophers of science have approached the issue, especially the basic question of what exactly is meant by “reductionism.” A useful exploration of this issue, specifically in relation to genetics, is by Kenneth Schaffner in “Approaches to Reduction,” *Philosophy of Science*, 1967, 34:137-147. In another paper Schaffner argues that molecular biology was not built on a conscious attempt to reduce Mendelian to molecular genetics: “The Peripherality of Reductionism in the Development of Molecular Biology,” *Journal of the History of Biology*, 1974, 7:111-139. On a slightly different track, David Hull maintains that molecular genetics is not logically deducible from Mendelian genetics: see “Reduction in Genetics—Biology or Philosophy?” *Philosophy of Science*, 1972, 39:491-499. Contrary to Schaffner and Hull, William K. Goosens maintains that Mendelian genetics *was* reduced to the chemical level by molecular genetics: “Reduction by Molecular Genetics,” *Philosophy of Science*, 1978, 45:73-95. Although this topic moves from history into philosophy, students find it challenging. Because of the centrality of molecular genetics to modern biology, it is particularly relevant to raise these philosophical questions in the context of the history of genetics.

Physiology

Despite its great importance in the growth of the biological sciences in this century, physiology has received far less attention from historians of science than is its due. This may be in part because physiology's close historical relationship to medicine has made it seem more the province of historians of medicine. There is also the fact that physiology underwent its revolution in methodology over a century ago, with the widespread introduction of physicochemical and experimental methods to supplement descriptive, anatomical work. Whatever the exact reason, the result is that the reader looking for an entrance into the general history of twentieth-century physiology will have to consult a variety of separate, more specialized sources.

Rather than covering the vast array of subjects that rightfully fall under the history of physiology (such as plant physiology and pathology, etc.), I focus on three areas that have been major concerns in the twentieth century: general physiology, neurobiology and endocrinology. For a brief introduction and overview of twentieth-century physiology, it is worthwhile to consult Karl E. Rothschuh's *History of Physiology* (Huntington, N.Y.: Krieger, 1973). Chapter 7 (pp. 264-361) deals with the twentieth century; while it does not provide in-depth coverage, the broad outline establishes the framework within which more specialized topics can be placed.

The Prussian-born American physiologist Jacques Loeb (1859-1924), a long-time investigator at the Rockefeller Institute and a close professional friend of such figures as T. H. Morgan, Boss Harrison, J. McKeen Cattell, and W.J. V. Osterhout, set the style of experimental and quantitative biology that influenced a whole generation of biologists, especially in the United States. Loeb championed what he called "the mechanistic conception of life"—the title of a major address he gave in 1911 and of a book of essays collected in 1912 (Cambridge, Mass.: Harvard Univ. Press, 1964). The reprint edition benefits from a superb introduction by Donald Fleming. *The Mechanistic Conception of Life* was a celebration of the mechanistic materialist viewpoint in twentieth-century biology. The introduction and a selection of several of the essays make for stimulating reading and are well within the reach of most undergraduates, especially those with a year or more of college biology. A new biography of Loeb is Philip J. Pauly's *Controlling Life: Jacques Loeb and the Engineering Ideal in Biology* (New York: Oxford Univ. Press, 1987). As the title suggests, Pauly emphasizes that Loeb's guiding ideal was the scientific control of life.

Opposition to the "mechanistic conception of life" came from a number of sources—principally embryology and areas of general physiology—from the 1920s onward. Prominent among those who advanced a more holistic approach were the physiologist Walter Bradford Cannon (1871-1942) and the physiological chemist Lawrence J. Henderson (1878-1942). Cannon's work, is summarized in his popular book *The Wisdom of the Body* (1932; New York: Norton, 1960), which is eminently accessible to undergraduate readers. Henderson's work is summarized, along with a number of other chemical topics, in his semipopular *The Fitness of the Environment* (1913; Boston: Beacon Press, 1958). This paperback edition contains an introduction by George Wald. The development of the idea of homeostasis is the subject of a superb essay by Donald Fleming, "Walter B. Cannon and Homeostasis," *Social Research*, 1984, 51:609-640.

Henderson's work has been the subject of several studies. John Parascandola's "Organismic and Holistic Concepts in the Thought of L. J. Henderson," *Journal of the History of Biology*, 1971, 4:63-113, relates Henderson's scientific to his philosophical work. Henderson and Cannon were strongly interested in social regulation and equilibrium, as was fitting for products of the "Progressive Era," and sought in physiological processes analogies for the notion of social and economic balance. A specific discussion of Henderson's view of the interrelationship between social

and physiological equilibrium theory can be found in Cynthia Eagle Russett's *The Concept of Equilibrium in American Social Thought* (New Haven, Conn.: Yale Univ. Press, 1968). See also Stephen J. Cross and William R. Albury, "Walter B. Cannon, L.J. Henderson, and the Organic Analogy," *Osiris*, 1987, N.S. 3:165-192.

Endocrinology (the study of the nature and effect of hormones, or "chemical messengers," produced by the endocrine glands) is an area of general physiology that has shown enormous growth in the twentieth century. It has also been the subject of numerous historical studies. Arthur F. Hughes has prepared a brief but useful introduction titled "A History of Endocrinology," *Journal of the History of Medicine and Allied Sciences*, 1977, 32(3): 292-313. While it is largely descriptive and chronological, Hughes's study demonstrates the close link between clinical pathology and the gradual discovery of the role of hormones in maintaining physiological balance. The history of endocrinology is the subject of a special issue of the *Journal of the History of Biology*, 1976, 9. A general introduction to the historiography of endocrinology is provided for the volume by Diana Long Hall and Thomas F. Click (pp. 229-233). Hall has explored some social and technical aspects of the history of sex-hormone research in "Biology, Sex Hormones, and Sexism in the 1920s," *Philosophical Forum* 1974, 5:81-96. In this fascinating article she suggests that sexist biases about the importance of male over female hormones proved to be a barrier to the technical solution of problems associated with extracting, isolating, and characterizing the chemical nature of sex hormones (principally testosterone and estrogen) in the 1920s. On a somewhat more specific aspect of endocrinology, Michael Bliss's *The Discovery of Insulin* (Chicago: Univ. Chicago Press, 1982) provides a close picture of the technical problems that investigators in any field of endocrinology had to surmount in order to identify, isolate, and purify a given hormone. The insulin story also provides a fascinating picture of the role of drug companies in encouraging and financing hormone research in the period (1920s) before government subsidy of basic scientific research.

Although neurobiology (as it is now called) has always been subsumed under physiology, its rapid growth in the twentieth century, along with its institutionalization in separate university departments and separate funding programs, has made it an almost completely autonomous discipline. Neurobiology can be divided into two major areas: neurophysiology, or the study of the process by which nerve cells transmit a message; and neurology, the study of the structure and organization of the nervous system. One of the few comprehensive historical reviews of both areas is Mary A. B. Brazier's "The Historical Development of Neurophysiology," in *Handbook of Physiology*, edited by J. Field, H. W. Magoon, and V.E. Hall (Baltimore:Waverly, 1960), Section I, Volume I, pp. 1-57. Although this article lacks historical analysis, it does trace the history of a variety of neurophysiological problems from ancient times to the early decades of this century. Another general work, though less systematically organized than Brazier's, is *The Neurosciences: Paths of Discovery*, edited by Frederic G. Worden, Judith P. Swazey, and George Adelman (Cambridge, Mass.: MIT Press, 1975). Two articles in this collection stand out as particularly interesting: Richard Jung's "Some European Neuroscientists: A Personal Tribute" (pp. 477-511), and Judith P. Swazey and Frederic G. Worden's "On the Nature of Research in Neuroscience" (pp. 569-587). Swazey and Worden look at the development of twentieth-century neurobiology in terms of Thomas Kuhn's concept of scientific revolution.

Two major questions confronted neurologists at the end of the nineteenth and beginning of the twentieth centuries: What was the basic anatomical element of the nervous system (individual cells, or a continuous nerve network)? How were parts of the nervous system (e.g., peripheral nerves and

spinal cord) integrated to produce an overall functioning system? The first question involved considerable debate in the period of the 1870s through the 1890s, though it was resolved ultimately in favor of the neuron theory (individual nerve cells as the basic structural and functional unit of the nervous system) by the early 1900s. Central to that debate was the work of the Spanish cytologist Santiago Ramón y Cajal (1852-1934), whose autobiography *Recollections of My Life*, translated by E. Horne Craigie with the assistance of Juan Cano (Philadelphia: American Philosophical Society, 1937), contains considerable information about the debate, the clash of paradigms, and Ramón y Cajal's exquisite techniques for bringing about the resolution. A more recent and historically oriented account is Susan Billings's "Concepts of Nerve Fiber Development 1839- 1930," *Journal of the History of Biology*, 1971, 4:275-306, which shows how study of the embryological development of the nervous system (which Ramón y Cajal wisely exploited) helped to demonstrate that the nervous system arises from many discrete individual cells.

The structural and functional organization of the nervous system has been an area of great advancement during the twentieth century. Much work on the mode of action of the reflex response (as well as on how reflexes are learned) and on the relation between inhibition and excitation of nerve tracks was done by Russian neurologists in the latter part of the nineteenth and especially the early part of the twentieth century. The chief figures there were Ivan Michailovich Sechenov (1829-1905) and Ivan P. Pavlov (1849-1936). Pavlov's interest in digestion led him, under Sechenov's influence, to study the now-classic conditioned reflex involved in salivation. Pavlov's life and work is the subject of one English-language volume: B.P. Babkin's *Pavlov, A Biography* (Chicago: Univ. Chicago Press, 1949). This source provides valuable insight into a whole school of neurological work that has had as much influence on psychology as on neurobiology in this century.

While the general features and functions of the reflex were understood by the turn of the century, its manner of organization (especially in terms of connections with the brain) was not. A towering figure in elucidating the relationship between central and peripheral nervous systems, and especially the integrative function of the spinal cord, was the British physiologist Charles Scott Sherrington (1857-1952). Ragnar Granit's biography, *Charles Scott Sherrington, An Appraisal* (London: Nelson, 1967), is outstanding, though it suffers somewhat from historical presentism since the author is himself a distinguished neurobiologist. That deficiency is not a problem in Judith Swayze's *Reflexes and Motor Integration: Sherrington's Concept of Integrative Action* (Cambridge, Mass.: Harvard Univ. Press, 1969). Swayze concentrates on a detailed but clear and insightful analysis of Sherrington's scientific background, his experimental methods, and the development of his hypotheses about integrative action.

The history of neurophysiology is less extensively explored than that of neurology, partly because major progress has been made only in the past several decades. What few historical sources exist treat specific problems and are thus episodic. Concerning the development of the neurotransmitter hypothesis (that conduction across the synapse between adjacent neurons occurs by a chemical rather than electrical process), its antagonists and protagonists, see Michael V. L. Bennett's "Nicked by Occam's Razor: Unitarianism in the Investigation of Synaptic Transmission," *Biological Bulletin*, Suppl., June 1985, 168:159-167. This article is the only source I know of that deals with this intriguing controversy in mid-twentieth-century neurophysiology.

Ecology & Animal Behavior

The science of ecology—the study of the interrelationships among the biological and physical components in the natural world—has emerged as a distinct discipline only in the twentieth century.

While naturalists from ancient times to the late nineteenth century noted the interdependence among organisms and the adaptations of organisms to their environment, the attempt to study those interactions as part of a larger natural system has occurred-only recently.

An excellent general introduction to the problems and recent sources in the history of ecology is Frank Egerton's two-part bibliographical essay "The History of Ecology: Achievements and Opportunities," *Journal of the History of Biology*, 1983, 16:259-310, and 1985, 18:103-143. Another interesting general work is Donald Worster's *Nature's Economy, the Roots of Ecology* (Garden City, N.Y.: Anchor Books, 1979). Worster's book is essential background reading and will provoke thought about, if not agreement with, the dialectic he sees between the views of earlier, romantic naturalists and those of hard-nosed quantitative ecologists in the period since the early 1900s.

As a field, ecology seems to have followed much the same historical development as other fields of biology, passing from a descriptive phase in the nineteenth century to a quantitative, experimental, and analytical phase in the twentieth. The contradiction within ecology during this period was that between holistic and reductionistic, analytical and integrative, thinking. An interesting survey that comes to much the same conclusion is Eugene P. Odum's "The Emergence of Ecology as a New Integrative Discipline," *Science*, 1977, 195:1289-1292.

As some scholars have pointed out, the development of ecology has been closely tied to evolving concepts of nature. Two very interesting papers, both dealing with the changing concept of nature, highlight this general view. One is Roderick Nash's "The Exporting and Importing of Nature: Nature-Appreciation as a Commodity, 1850-1980," *Perspectives in American History*, 1979, 12:519-560, in which the author argues that by the beginning of the twentieth century North America had little frontier or wilderness left. This, Nash claims, led to changing patterns of travel to "experience wild nature" in other areas such as Africa. Nash sees "nature appreciation" as a commodity for tourism, explaining the rapid growth of conservation movements in this country between 1890 and the present as an attempt to save the wilderness for the tourist industry. On a somewhat different theme, Donald Fleming, in "Roots of the New Conservation Movement," *Perspectives in American History*, 1972, 67:7-91, compares the old conservation movement of the Progressive Era (the period discussed by Nash) to the "new" environmental and ecological movement emerging in the late 1960s. Fleming argues that the new environmentalists represented a resurgence of a basic American transcendentalism and an opposition to the Judeo-Christian tradition of conquering nature. Both articles treat the general concern for environment, and by association its "scientific" side (ecology), in terms of American social and intellectual history. More directly concerned with the history of the scientific side is Sharon Kingsland's *Modeling Nature: Episodes in the History of Population Ecology* (Chicago: Univ. Chicago Press, 1985), which deals with a series of important topics in twentieth-century ecology and evolution: mimicry, cryptic coloration, the concept of the niche and the community, island biogeography, and population structure.

Ethology as a field has received scant attention from historians, though several scholars have works in progress that should change this situation in the not-too-distant future. There is one general historical introduction, W. H. Thorpe's *The Origins and Rise of Ethnology* (New York: Praeger 1979), but it is a summary by an eminent elder statesman in the field, not a critical history. Of special value is John R. Durant's "Innate Character in Animals and Man: A Perspective on the Origins of Ethology," in *Biology, Medicine and Society, 1840-1940*, edited by Charles Webster (Cambridge: Cambridge Univ. Press, 1981), pp. 157-192. A similarly brief but useful overview is Richard W.

Burkhardt's "The Development of an Evolutionary Ethology," in *Evolution from Molecules to Man*, edited by D. W. Bendall (Cambridge: Cambridge Univ. Press, 1983), pp. 429-444.

Of the many biologists who have worked to establish the field of ethology in the twentieth century, only two have received biographical or historical treatment: William Morton Wheeler (1865-1937) and Konrad Lorenz (1903-). Wheeler's pioneering work with ant social organization and behavior was instrumental in establishing ethology as a significant and serious branch of modern biology. His biography by Mary Alice Evans and Howard Ensign Evans, *William Morton Wheeler, Biologist* (Cambridge, Mass.: Harvard Univ. Press, 1970), is a rich source of information about, the history not only of ethology but of many other aspects of twentieth-century biology. The more recent *Konrad Lorenz*, by Alec Nisbet (New York: Harcourt Brace Jovanovich, 1976), portrays not only Lorenz himself but many of his contemporaries and explores the controversies that molded ethology between 1935 and 1965. Nisbet is perhaps too enamored of his central character and does much to apologize for Lorenz's overt Nazi connections, but the biography covers a period of ethology not treated in detail by other writers. More scholarly and philosophical analyses of Lorenz's work have come from several historians of science: see, for example, Theodora J. Kalikow's "Konrad Lorenz's Ethological Theory: Explanation and Ideology," *Journal of the History of Biology*, 1983, 16:39-73. Kalikow's work is thorough, fair, and insightful, facing squarely the ideological basis of Lorenz's work that may have brought him into step with Nazi racial theory. In addition, there is Robert J. Richards's superb study of Lorenz's instinct theory: "The Innate and the Learned: The Evolution of Konrad Lorenz's Theory of Instinct," *Philosophy of the Social Sciences*, 1974, 4:111-133.

BIOLOGY AND SOCIETY

In teaching the history of twentieth-century biology (or any science, for that matter) there is a tendency to stick to the better-established areas and to treat science in a kind of social vacuum. I have often found it valuable to introduce one or more topics loosely called "biology and society" into courses in the history of biology. By "biology and society" I mean not so much the social history of science (which is important, too) as the direct use of an area of biology to attain or support a specific social or political end (e.g., the use of microbiology for germ warfare, of embryology for legislation on abortion, or of genetics for deciding immigration or reproductive policy). Such topics intrigue students and can introduce them to ways of thinking more carefully about similar issues today (e.g., sociobiology or the claim that criminality is genetic). I include just a few topics in this category, with references to the most basic and accessible literature on each.

Eugenics. Eugenics was a movement prominent in England, the United States, Germany, several Scandinavian countries, and, to a lesser extent, France and Russia from the early 1900s until the mid to late 1930s. It represented an attempt to use the then-new science of Mendelian genetics to explain and ultimately resolve many current social problems: chronic unemployment and poverty (pauperism), feeble-mindedness, alcoholism, prostitution, rebelliousness, and criminality.

Several general histories of eugenics have been written over the past two decades. The oldest, but in some ways the most comprehensive historically, is Mark Haller's *Hereditarian Attitudes in American Thought* (New Brunswick, N.J.: Rutgers Univ. Press, 1963). This book traces the intellectual history of eugenics from its nineteenth-century background. More limited in scope but excellent within its own bounds is Kenneth Ludmerer's *Genetics and American Society* (Baltimore: Johns Hopkins Univ. Press, 1972). Ludmerer examines specifically the efforts of eugenicists to

influence legislation on immigration restriction in the United States in the 1920s. For a more radical and overtly political analysis of the same period, see Garland E. Allen's "Genetics, Eugenics and Class Struggle," *Genetics*, 1975, 79:29-45; and "The Founding of the Eugenics Record Office at Cold Spring Harbor, 1910-1940: An Essay in Institutional History," *Osiris*, 1986, N.S. 2:225-264. Daniel J. Kevles's *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (New York: Knopf, 1985) compares British and American eugenics on such issues as the importance of race and class, immigration and sterilization legislation, and education (propaganda). The latter chapters deal with the post-World War II development of eugenics as it was ultimately supplanted by what Kevles sees as the more scientifically valid study of human heredity. For British eugenics, Geoffrey R. Searle's *Eugenics and Politics in Britain 1900-1914* (Leyden: Noordhoff 1976) examines in detail the British eugenicists' emphasis on national degeneration and class structure. On the more contemporary issue of sterilization and genetic counseling, Philip Reilly has presented a remarkable historical and social analysis: *Genetics, Law and Social Policy* (Cambridge, Mass.: Harvard Univ. Press, 1977). Reilly's book can be used as a follow-up to Ludmerer's *Genetics and American Society*.

Population and Birth Control There has been much discussion and controversy in recent years about the world's so-called overpopulation problem. Many population control groups—privately and governmentally funded—have arisen in Western countries that seek, through a variety of means, to limit population growth, especially in the non-Western, Third World countries. Like eugenics before it, the ideology of population control holds that a whole host of social problems have a biological cause: in this case, too high a reproductive rate. Advocates of population control are known as Malthusians, or neo-Malthusians, after Thomas Robert Malthus, whose *Essay on Population* of 1798 first posed the thesis that poverty and hunger were caused by overpopulation.

Population control is inevitably related to birth control (the planning of family size by individual couples), and the two have interacted continuously throughout the present century. One of the best historical treatments of both movements is Allan Chase's *The Legacy of Malthus* (New York: Knopf, 1977). Although somewhat disorganized, Chase's book covers the birth control and eugenics movements in the early decades of the century and their eventual evolution into the population control movement after World War II. Two books dealing more with the birth control movement alone are Linda Gordon's *Woman's Body, Woman's Right: A Social History of Birth Control in America* (New York: Basic Books, 1978) and James Reed's *From Private Vice to Public Virtue: The Birth Control Movement and American Society since 1830* (New York: Basic, 1978).

Several stimulating and provocative works exist on the population control movement. Paul R. Ehrlich's *The Population Bomb* (San Francisco: Freeman, 1968) is useful because it states the neo-Malthusian position clearly and forcefully and has had an enormous influence on population thinking in the past seventeen years. The opposite point of view is presented in several highly effective sources. One of the most concise and readable, though less comprehensive, is Barry Commoner's "How Poverty Breeds Overpopulation (and Not the Other Way Around)," *Ramparts*, 1975, 13:21-25. More complete and detailed, though less easy to read, is Bonnie Mass's *Political Economy of Population Control in Latin America* (Montreal: Editions Latin America, 1972). This work contains a wealth of information about the socioeconomic effects of U.S. corporations, banking interests, and large-scale loans on the development of poverty and hunger in Third World countries.

Recombinant DNA and Genetic Engineering. In recent decades the dangers as well as the benefits of advances in recombinant DNA technology have been the subject of much debate, both inside and outside of the biological community. Volumes have been written on the subject from all

sorts of perspectives—social and political, economic, ethical and moral, and technological. The topic of genetic engineering—specifically that branch of it associated with DNA manipulation—is one virtually all students are acquainted with. A collection of original papers from the mid 1970s can be found in *The Recombinant DNA Debate*, edited by David A. Jackson and Stephen P. Stich (Englewood Cliffs, NJ.: Prentice-Hall, 1979). These papers cover such issues as safety, decision making, and the role of universities in relation to business, secrecy of research, patent ownership, and the like. A systematic historical treatment of the whole recombinant DNA debate is offered in Sheldon Krimsky's *Genetic Alchemy: The Social History of the Recombinant DNA Controversy* (Cambridge, Mass.: MIT Press, 1982). This book presents a thorough, well-written, and balanced picture of the original controversy as it arose out of genetically remaking the adeno-S-40 virus in the mid 1970s. This is the most scholarly and authoritative study of this episode currently available. On the specific issues recombinant DNA research has raised for universities with respect to patents, secrecy, and publication rights, see Charles Weiner's "Universities, Professors, and Patents: A Continuing Controversy," *Technology Review*, 1986, 83:33-43. This article traces the history of university relations to patents from the early 1920s through the recombinant DNA controversy of the 1970s.

Scientific Creationism. The topic of scientific creationism—cumbersome, sometimes frustrating, but always intriguing—often comes up for discussion in both biology and history or sociology of science classes. Several collections present the various arguments for and against creationism, especially its recent claims to be a "science." The most complete is the volume edited by J. Peter Zetterberg, *Evolution versus Creationism: The Public Education Controversy* (Phoenix: Oryx Press, 1983). This volume contains articles on evolution and science education, statements of the creationist position today, biologists' responses to various creationist arguments, reprints of various state creationist laws, and the well-publicized Arkansas decision against creationism in the schools (1982). Another collection of essays, all against the creationist position, is *Science and Creationism*, edited by Ashley Montague (New York: Oxford Univ. Press, 1984), now available in paperback. Lengthy defenses of modern Darwinism against the creationists' attacks are Douglas J. Futuyma's *Science on Trial: The Case for Evolution* (New York: Pantheon Books, 1983) and Michael Ruse's *Darwinism Defended: A Guide to the Evolution Controversies* (Reading, Pa.: Addison-Wesley, 1982). Ruse's book has more historical and philosophical detail, but in many ways Futuyma's is a clearer and more up-to-date presentation of the evolutionary position. An examination of the controversy from a religious perspective is *Evolution and Creation*, edited by Ernan McMullin (South Bend, Ind.: Univ. Notre Dame Press, 1985). The brunt of McMullin's own essay, as well as of others in the volume, is that there is no necessary contradiction between a creationist and an evolutionary point of view. To my mind the best account of the historical and sociological roots of the current controversy is Dorothy Nelkin's *The Creation Controversy: Science or Scripture in the Schools* (New York: Norton, 1982), subtitled "A History of the Struggle between Creationists and Scientists from the Nineteenth Century to the Arkansas Trial." On a more limited but equally interesting level is the autobiography of John T. Scopes, who went on trial in Dayton, Tennessee, in 1926 for teaching evolution in his high school biology class: John T. Scopes and James Presley, *Center of the Storm: The Memoirs of John T. Scopes* (New York: Holt, Rinehart & Winston, 1967).