Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology

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We didn't sit down and forge a synthesis. We all knew each other's writings; all spoke with each other. We all had the same goal, which was simply to understand fully the evolutionary process. . . .

. . . By combining our knowledge, we managed to straighten out all the conflicts and disagreements so that finally a united picture of evolution emerged.

The theory of evolution is quite rightly called the greatest unifying theory in biology.

Ernst Mayr

The struggle to unify the biological sciences is one of the central features of the history of biology. Emerging only in the nineteenth century,[1] biology was characterized by disunity to such

1. Just when biology emerged as legitimate and autonomous science has been a contentious issue for historians of biology. Though the term was coined in the early years of the nineteenth century, an *autonomous science* of life, I will argue, was not as strongly defensible until evolution was articulated. Only with evolution, which defied reduction to physics and chemistry because of its metaphysical components, at the same time that it *introduced* a causo-mechanical agent for evolutionary change, could biology claim autonomy. This took place in Thomas Henry Huxley's England, and most likely in the thought of Huxley himself, who adopted the term "evolution." Huxley had the following to say on the emergence of biology: "the conscious attempt to construct a complete science of Biology hardly dates further back than Treviranus and Lamarck, at the beginning of this century, while it has received its strongest impulse, in our own day, from Darwin" (*The Crayfish: An Introduction to the Study of Zoology*, 4th ed. [London: Kegan, Paul, Trench, 1884], p. 4). Huxley may be viewed as a chief discipline builder for biology. My argument is supported by the recent work of Joseph Caron, "'Biology' in the Life Sciences: A Historiographical Contribution," *Hist. Sci., 26* (1988), 223–268; see also Gerald Geison, *Michael Foster and the Cambridge School of Physiology* (Princeton: Princeton University Press,

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an extent and for so long that repeated attempts to unify this science through professional societies proved to be a nearly impossible task. Charting the rocky road toward organized biology in America during the years 1889—1923 — a key period for the institutionalization of biology — historian Toby Appel concluded: "Numerous biological sciences were established in America, but no unified science of biology."[2] So formidable was this task, that the hope of ever formulating a unified biological society representing a unified science of biology appeared to have been largely abandoned by 1923.

By the early 1950s, however, the organization of biological knowledge had been greatly transformed. With the formation of the American Institute of Biological Sciences,[3] the first umbrella-like organization representing the heterogeneous practices of the biological sciences was intact. Simultaneously, there had also grown an awareness of a feeling of unity within the biological sciences.[4] So powerful was the conviction that biology had become a unified science, that George Gaylord Simpson could introduce his biology textbook of 1957 with the following assertion: "This book is based on strong convictions. We believe that there is a unified science of life, a general biology that is distinct from a shotgun marriage of botany and zoology, or any others of the special life sciences. We believe that this science has a body of established and working principles. We believe that literally nothing on earth is more important to a rational living being than basic acquaintance with those principles."[5]

1978). One could make a strong argument that Haeckel was as instrumental to biological discipline building in the German context.

2. Toby Appel, "Organizing Biology: The American Society of Naturalists and Its 'Affiliated Societies,' 1883—1923," in *The American Development of Biology*, ed. Ronald Rainger, Keith R. Benson, and Jane Maienschein (Philadelphia: University of Pennsylvania Press, 1988), pp. 87—120.

3. Founded in 1947.

4. Hamilton Cravens has pointed out that the years 1920—50 also witnessed movements to support interdisciplinary scholarship in America. During this period institutional and intellectual networks were assembled to lend an increasing feeling of unity. See Hamilton Cravens, "Behaviourism Revisited: Developmental Science, the Maturation Theory, and the Biological Basis of the Human Mind, 1920s—1950s," in *The American Expansion of Biology*, ed. Keith R. Benson, Jane Maienschein, and Ronald Rainger (New Brunswick, N.J.: Rutgers University Press, 1991), pp. 133—163.

5. This excerpt is taken from the preface to the first edition (1957) of George Gaylord Simpson, Colin S. Pittendrigh, and Lewis Tiffany, *Life: An Introduction to Biology*; reprinted in George Gaylord Simpson and William S. Beck, *Life: An Introduction to Biology* (New York: Harcourt, Brace, and World, 1965), p. v.

The years between 1923 and 1950, which spanned the interwar period and World War II, were tumultuous years rocked by global events and global disturbances. The culmination of the modern period, these years witnessed the rise of not only international artistic and political movements, but also related philosophical movements such as logical positivism.[6] Members of the Vienna Circle, the logical positivists were at this time actively embarked on an Enlightenment ideal to unify all of the sciences. Within the biological sciences themselves, this interval of time witnessed a major disciplinary realignment as evolutionary biology emerged as a discipline of the biological sciences. Most important of all for the unification of biology, these were also the years long-recognized by historians of biology as constituting the period of the evolutionary synthesis.[7]

The present article examines the interplay of these factors, which I argue were closely related, tightly woven, nurturing the growing belief in biology as unified science. Close examination of the unification of biology also sheds light on a central problem of the history of biology — namely, the evolutionary synthesis. In discussing the synthesis, I will focus on three features: the unification of biology through the articulation of Theodosius Dobzhansky's evolutionary genetics framework, the consequent binding of the heterogeneous practices of biology, and the publication of Julian Huxley's *Evolution: The Modern Synthesis*.[8] The central argument I will make is that the evolutionary synthesis signaled the unification of the biological sciences. Evolution, purged of unacceptable metaphysical elements, became the "central science" of biology which bound together, and grounded, the heterogeneous practices of biology into a unified and progressive science. The science of evolutionary biology — reworked into an experimental and quantitative practice — also emerged at this time. Biology itself in turn became a unified science to rival Newtonian physics and chemistry, in a manner that still preserved its autonomous status. The unified biology — through the central science of evolution as it emerged from the synthesis — would in turn be centrally situated within the positivist ordering of knowledge.

6. Peter Galison has recently brought into relief the interplay of these artistic, political, and philosophical movements: see "Aufbau/Bauhaus: Logical Positivism and Architectural Modernism," *Crit. Inq. 16* (1990), 709–752.

7. By far the most comprehensive treatment of the synthesis is Ernst Mayr and William B. Provine, eds., *The Evolutionary Synthesis* (Cambridge, Mass.: Harvard University Press, 1980).

8. Julian Huxley, *Evolution: The Modern Synthesis* (London: George Allen and Unwin, 1942).

Only within a positivist theory of knowledge, which then held sway, was the unification of biology, and the unification of science, desirable.

WOODGER'S "*BIOLOGICAL PRINCIPLES*," THE UNITY OF SCIENCE, AND THE AUTONOMY OF BIOLOGY

The abandonment of the hope that a unified science of biology could ever be possible took place at a time when the foundations and status of biology were being questioned. Within an increasingly positivist philosophical framework, biology, with its remnants of vitalistic thinking and nonrigorous methodology, was seen as filled with speculation. The outstanding critic — but hopeful reformer — of biology was J. H. Woodger, who in 1929 published his *Biological Principles*. In his book, Woodger hurled criticism after criticism at what he viewed as a science in its infancy and rife with metaphysics. Still in the "metaphysical stage" of its development, biology would come of age only after critical thought had purged it of its more speculative features and after fundamental axiomatic principles had been established. Only after biology had paid "critical attention to the purification of its concepts," and only by "making sure of its foundations," would it become a mature science.[9] For Woodger, biologists, who thought they had found their Newton in Darwin, were mistaken, since biology had not yet reached a stage in its development comparable to eighteenth-century physics. Meant to imitate Robert Boyle's "Sceptical Chemist," Woodger's book would function as the "Sceptical Biologist."

The biological sciences were especially problematic to Woodger because they appeared to be increasingly fragmented and disunified. With fields like cytology and genetics, which had emerged around the turn of the century, the discipline of biology appeared to be hopelessly splintered. Woodger opened his book with the following thoughts on the state of biology in the 1920s:

> If we make a general survey of biological science we find that it suffers from cleavages of a kind and to a degree which is unknown in such a well unified science as, for example, chemistry. Long ago it has undergone that inevitable process of subdivision into special branches which we find in other sciences, but in biology this has been accompanied by a characteristic

9. J. H. Woodger, *Biological Principles: A Critical Study* (New York: Harcourt, Brace, 1929), p. 84.

> divergence of method and outlook between the exponents of the several branches which has tended to exaggerate their differences and has even led to certain traditional feuds between them. This process of fragmentation continues, and with it increases the time and labour requisite for obtaining a proper acquaintance with any particular branch.[10]

To Woodger, this fragmentation was increasingly alarming, for it seemed that the biological sciences could never rival the unity of more mature sciences like physics and chemistry. This did not bode well for the establishment of a unified science. One way to achieve such unity would be through the articulation of a central biological principle of great "unifying power": "But whereas in some sciences this process has been accompanied by the attainment of generalizations which have tended to knit the several branches into a single whole, in biology the disruptive process has not been compensated by the help of any principle of such unifying power, and the possibility of a unified biology seems to recede more and more from our grasp."[11] Repeated attempts to unify and synthesize the diverging branches of biology had only brought out underlying differences, however, and had made biology's unstable grounding all the more evident. Woodger wrote: "The general theoretical results which have been reached by investigation along the lines of physiology, experimental morphology, genetics, cytology, and the older descriptive morphology are extremely difficult to harmonize with one another, even although, for various reasons, these difficulties are not apparent on a *prima facie* view. As soon as we do attempt such a synthesis we are confronted with contradictions which appear to rest on the fundamental biological antitheses. Instead of a unitary science we find something more approaching a 'medley of *ad hoc* hypotheses.' "[12]

The belief that all the sciences were unifiable had been one of the cherished ideals of Enlightenment thought.[13] Heir to this thought, Auguste Comte in the mid-nineteenth century had articulated a positive philosophy that stated that the sciences went through three stages in their historical development: the theological, the metaphysical, and the positive. Examining the history of the sciences within Western thought, Comte postulated that

10. Ibid., p. 11.
11. Ibid.
12. Ibid., p. 12.
13. Belief in the unity of knowledge deeply structures Western thought. Plato discusses the unity of knowledge in his *Timaeus*; see *Timaeus and Critias*, trans. Desmond Lee (London: Penguin Books, 1965).

each science was dependent for its grounding on previously existing sciences, so that biology was dependent on chemistry, which was dependent on physics, which in turn was dependent on astronomy.[14] Though each science matured as it followed its own logic — revealed through close historical study of the science — there would still be an underlying unity to all the sciences. The progressive "growth" of knowledge within this framework often drew on the botanical metaphor of the branching "tree of knowledge."[15]

In the 1920s a belief in the unity of science was one of the fundamental tenets of the logical positivists of the Vienna Circle, the new heirs to Enlightenment thought. In the late 1920s and the 1930s Otto Neurath, Rudolf Carnap, and the Chicago-based Charles Morris, members of this influential circle, spearheaded a movement to unify all of the sciences that had emerged in the nineteenth and early twentieth centuries. Their "Unity of Science Movement" was based on their foundational belief that all the sciences — physical, biological, and social — were not only dependent *on*, but reducible *to*, physicalist terms that held one common method and protocol language. The movement not only upheld unification but, drawing from their intellectual progenitor Ernst Mach, stated that the unification of science had to take place by the elimination of metaphysics.[16] All true, legitimate

14. Comte was the thinker most responsible for promulgating the notion of a positivistic ordering of knowledge. For Comte, sociology — the science of society — emerging from physiology (biology), was to be the final science. See his *Cours de philosophie positive*, published in six volumes (Paris: Bachelier, 1830–42). Joseph Caron in his "'Biology' in the Life Sciences" (above, n. 1) introduces a discussion of Comte's use of the term biology and its close relation to physiology.

15. Representations of the tree of knowledge were to proliferate in the Enlightenment. One of the most famous is included in Diderot and d'Alembert's *Encyclopédie*.

16. In an essay entitled "Ernst Mach and the Unity of Science," Philipp Frank summarized Mach's position on the unity of science in the following phrase: "He [Mach] proclaimed . . . *the unification of science by means of the elimination of metaphysics*." Frank continued: "It is just this sentence that is the clue to the understanding of Mach's doctrine, of his papers, which seem to deal with so many subjects and such different fields of science. . . . And it is just this program of Mach that we may adopt as the program of our 'Unity of Science Movement,' of our Congresses and of our Encyclopedia." Frank's essay was published in the official journal for the unity of science, *Erkenntnis*, and was later translated and republished in a collection of his essays. This quotation is included in Robert S. Cohen and Raymond J. Seeger, eds., *Ernst Mach: Physicist and Philosopher*, Boston Studies in the Philosophy of Science, 6 (Dordrecht: D. Reidel, 1970), pp. 235–244.

sciences had to be purged of their metaphysical elements as they became grounded in fundamental axiomatic principles. The Unity of Science Movement swept intellectual circles in the late 1920s and the 1930s as international congresses were held, journals like *Erkenntnis* were established, and collaborative efforts to integrate knowledge within an *Encyclopedia of Unified Science* were launched.

Woodger himself had been steeped in contemporary philosophical movements; he was an active member and outspoken advocate of the Unity of Science Movement, he attended congresses, and he corresponded with other members.[17] Consideration of the philosophical positions of progenitors of the logical positivists like Ernst Mach, of logical empiricist Bertrand Russell, and of mathematician-philosopher A. N. Whitehead and C. D. Broad, was clear in Woodger's *Biological Principles*. But while he was to support strongly the Unity of Science Movement, and to urge the axiomatization and unification of biology, he was also to explicitly articulate a position for biologists that was antireductionist and antiphysicalist.[18] The positivist drive to unify the sciences would, in fact, raise a series of complex and difficult issues for Woodger and other biologists.

While biology clearly had to grow out of its "metaphysical" stage of development and become a unified mature science,[19] full disciplinary unification — in a Machian and 'Unity of Science Movement' sense — was actually dangerous to biology. If with unification there came a *disciplinary reduction*, then biology as a discipline was threatened with engulfment by the physical sciences.[20] Nor could life phenomena, which biology as the

17. Woodger's role in the development of logical empiricism was recognized by Joergensen: see Joergen Joergensen, "The Development of Logical Empiricism," in *Foundations of the Unity of Science*, ed. Otto Neurath, Rudolf Carnap, and Charles Morris (1970), II. After writing his *Biological Principles*, Woodger completed another book in 1937 on a related theme, *Axiomatic Method in Biology*; he also contributed a monograph for the *Encyclopedia of Unified Sciences* with the title, *The Technique of Theory Construction*. Woodger was to serve on the advisory committee for the *Foundations of the Unity of Science: Toward an International Encyclopedia of Unified Science*. I am indebted to Gerald Holton for directing me to this literature on Woodger and the Unity of Science Movement.

18. Woodger's positivistic ordering was the following: physics, chemistry, biology, and psychology. Psychology and sociology were later combined into the larger category of the social sciences.

19. Within the framework articulated by Comte the process of maturation and progression through these stages was inevitable.

20. For this reason, many biologists in the 1920s and 1930s were to sympa-

science of life sought to explain, be easily seen to be reducible to, or to obey, the laws of physics and chemistry, at least not without a great deal of discussion. For Woodger, who discussed and evaluated at length both vitalistic and mechanistic thinking in the history of biology, a middle ground — based solely on biological and not physical principles — would have to be found in order to explain life phenomena. Both vitalism, which was too metaphysical, and mechanism, which drew too heavily on Newtonian physics, were inadequate for a mature science of life. For Woodger, the understanding of complex life phenomena would have to come solely from "observation and experiment" as articulated through exemplars in the physical sciences, rather than from any metaphysical and speculative considerations; but the biological principles he sought had to come from biology's *own* guiding logic rather than the logic of any physical science. If biology were to preserve its autonomy, and its independent existence as a science, it could in some measure be dependent *on*, but could not be reduced fully *to*, physics and chemistry. If anything (and this was an increasing sentiment among biologists), physics and chemistry would have to accommodate biology.[21]

According to contemporary philosophers of biology, Woodger's influence was insignificant in the development of the philosophy of biology. Yet given that no less a philosophical authority than the *Encyclopedia of Philosophy* is replete with Woodger's problematic under the heading "Biology," this is a difficult position to support.[22] However one wishes to situate Woodger within the history of the philosophy of biology, his position in the biological sciences accurately mirrored biologists' self-perceptions in the 1920s. Pointing out the immature state of biology, the lack of

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thize with the philosophical position articulated by A. N. Whitehead. In developing an antiphysicalist philosophical position, Whitehead had been drawing on his knowledge of biology to construct an organismic philosophy.

21. A similar threat of disciplinary subsumption for the social sciences was of great concern to Otto Neurath as well. For a discussion of Neurath's views on this problem see Danilo Zolo, *Reflexive Epistemology: The Philosophical Legacy of Otto Neurath* (Dordrecht: Kluwer, 1989), esp. chap. 5, "The Unity of Science as a Historicosociological Goal: From the Primacy of Physics to the Epistemological Priority of Sociology."

22. Paul Edwards, ed., *The Encyclopedia of Philosophy* (New York: Macmillan, 1967), pp. 310–318. The entry for biology was written by Morton O. Beckner; the assessment of Woodger's *Biological Principles* at the conclusion of the entry reads as follows: "An influential and classical source of subsequent work in the philosophy of biology, partially Whiteheadian." Beckner also cited J. S. Haldane's *Philosophical Basis of Biology*, along with sources from authors like E. S. Russell, Ludwig von Bertalanffy, and Ernest Nagel.

secure foundations with clearly articulated biological principles, and the disunified state of the biological sciences, Woodger's book became an urgent plea to axiomatize and unify the biological sciences and to bring them in line within the positivist ordering of knowledge — between physics and chemistry, on the one hand, and psychology and the other social sciences, on the other.

The call to axiomatize biology was echoed by J. S. Haldane, who faced some of the same issues head-on in his 1931 book, *The Philosophical Basis of Biology*.[23] Haldane's discussion and endorsement of the need to ground biology in fundamental principles closely resembled Woodger's, an allegiance that Haldane explicitly favored. Both urged a critical examination of the fundametal logic of the biological sciences, which would be based on biology's own guiding principles, with biological explanation drawn on "facts" gleaned solely from observation and experiment. Only in this manner could biology become a legitimate and mature science, yet preserve its autonomous status. Haldane explicitly stated that "biology must be regarded as an independent science with its own guiding logical ideas, which are not those of physics."[24] Both Haldane and Woodger denied the ability of mechanistic principles derived from Newtonian physics to explain life phenomena exclusively, and both highlighted the inadequacy of vitalistic thinking. Walking this fine line between mechanistic and vitalistic thinking in a manner that preserved the autonomy of biology, at the same time that it made possible scientific inquiry within a positivist philosophical framework, would prove to be *the* central problem of the twentieth-century biological sciences.[25]

23. J. S. Haldane, *The Philosophical Basis of Biology* (London: Hodder and Stoughton, 1931). In a supplemental section, written after the lectures that gave rise to his book, Haldane reviewed three books that had appeared at the same time, all of which discussed the fundamental principles of biology: Woodger's *Biological Principles*, E. S. Russell's *Interpretation of Development and Heredity*, and L. Hogben's *Nature of Living Matter*. While he disagreed mildly with Russell for upholding what he viewed as a standard "organismal" view of life, Haldane launched a full-blown attack against Hogben, who represented the strictly mechanistic conception of life that Haldane wished to avoid: "the foundations of this interpretation were entirely rotten. Moreover, physics was apparently almost entirely mechanistic, whereas fundamental mechanistic interpretation is now acknowledged to be impossible in physics. Professor Hogben stands bravely on a burning deck whence others have fled or are preparing to flee. We cannot but admire his courage" (pp. 164—165). For Haldane, the tension between vitalism and mechanism was remedied through a "holistic" view of life that emerged from the interaction of organism with environment.

24. Haldane, *Philosophical Basis*, p. 150.

25. To contemporary philosophers of biology, the issue of the autonomy of biology is still considered central to any discussion of the philosophy of biology.

One thing was sure, by the 1930s biology was reaching a secure enough stage in its maturity for there to grow the conviction (in enough biologists' minds, at least) that the physical sciences would have to accommodate biology, and not the other way around. Haldane wrote: "That a meeting-point between biology and physical science may at some time be found, there is no reason for doubting. But we may confidently predict that when a meeting-point is found, and one of the two sciences is swallowed up, that one will not be biology."[26]

Nor were Woodger and Haldane the only biologists to grapple with the complex issues brought on by the positivistic drive toward the unity of science.[27] Julian Huxley echoed both Woodger and Haldane in considering the maturation of biology and its

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Ernst Mayr, responding to Ernest Nagel and Carl Hempel, has contributed greatly to a viewpoint that gives autonomy to biology at the same time that biology becomes a legitimate science. See Ernst Mayr, *The Growth of Biological Thought* (Cambridge, Mass.: Belknap Press of Harvard University Press, 1982); see also his more recent collection of essays (especially "Is Biology an Autonomous Science?") in *Toward a New Philosophy of Biology* (Cambridge, Mass.: Belknap Press of Harvard University Press, 1988), and "How Biology Differs from the Physical Sciences," in *Evolution at a Crossroads*, ed. David Depew and Bruce Weber (Boston: MIT Press, 1985). And see my discussion below of the "Post-Sputnik Biological Sciences."

26. This quotation, found on p. 33 of Haldane's *Philosophical Basis of Biology*, is taken from an address of 1908 reprinted in 1919 in *The New Physiology*. Haldane was also concerned to preserve distinctness between biology and psychology: "In discussing the fundamental axiom of biology I have endeavoured to distinguish biology from the physical sciences and to illustrate the distinction. But the existence of conscious behaviour makes it necessary also to distinguish biology from psychology, the science, or rather great group of sciences or departments of knowledge, dealing with our experience when it is regarded as actually perceived and an expression of voluntary action" (Haldane, *Philosophical Basis*, p. 95). The relationship between biology and psychology was not as great a concern to biologists, since the threat of disciplinary engulfment through reduction was not as great a problem. Psychologists were, however, concerned with a disciplinary reduction to biology.

27. Hans Driesch was another such biologist. In 1934 Driesch faced members of the Vienna Circle at the International Congress in Prague. Adhering to the strongest possible vitalistic philosophy, Driesch came under heavy fire from the Vienna Circle because he supported nationalist philosophy and its quest for the "World Spirit" with the worst possible (in the Vienna Circle's view) brand of science. Peter Galison gives an account of how the Vienna Circle reacted to Driesch's plenary address in his Aufbau/Bauhaus article; I thank Galison for pointing this out to me. The antithesis of the Drieschian vitalistic position is best exemplified by Jacques Loeb. For the "architects" of the evolutionary synthesis, Loeb — the most extreme of mechanists — would be *too* mechanistic and reductionistic. A middle road between the two positions would prove to be the most effective to give legitimacy and autonomy to the biological sciences.

relationship to physics and chemistry;[28] W. B. Turrill — a close colleague of Huxley's — echoed Woodger's plea for an established logic or a "biologic" of the biological sciences;[29] and in America, William Morton Wheeler, reflecting on the difficulty of unifying a fragmented science, remarked that it might take "a few super-Einsteins" to unify biology.[30] Another biologist who would face the same issues was the Johns Hopkins—based Herbert Spencer Jennings. Sympathetic to the Unity of Science Movement as well, Jennings was acutely aware of the dangers of unification. As the threat of disciplinary reduction to physics and chemistry loomed large, he constructed his own rationale for defending the independent existence of biology. Drawing heavily on evolution, he adopted a version of emergent evolution: having emergent properties, biology could not be reduced to the physical sciences. In a highly polemical 1927 article, Jennings laid bare his political drive to raise biology to an independent science. The connection between evolution and biology was made transparent as he triumphantly proclaimed: "emergent evolution is the Declaration of Independence for biological science!"[31]

These authors were widely read by biologists. A concern with the place of biology among the sciences; its maturity, status, and legitimacy as autonomous science; the methodology of biology, the key role of observation and experiment; and a belief in the unity of science — all the critical considerations articulated by Woodger and reinforced by Haldane and others were trasmitted, in turn, to incipient biologists through the textbooks of biology. Nor had these issues gained centrality *solely* from a one-way traffic of influence emanating from Woodger and Haldane: Woodger and Haldane (Woodger especially) had been responding to persistent concerns that accompanied the emergence of the

28. Huxley wrote: "Every science arrives at a stage during which it makes its main broad contributions to practical human affairs. Biology is clearly on the verge of such a phase, while it is already over for physics and chemistry, and psychology and sociology cannot hope to reach it for perhaps another century" (Julian Huxley, "Biology and Physical Environment," in *What Dare I Think? The Challenge of Modern Science to Human Action and Belief* [New York: Harper and Row, 1931], p. 4).

29. W. B. Turrill, "The Expansion of Taxonomy with Special Reference to Spermatophyta," *Biol. Rev., 13* (1938), 342—373.

30. W. M. Wheeler, *Essays in Philosophic Biology* (Cambridge, Mass.: Harvard University Press, 1939). Wheeler pointed out that the disunity in the biological sciences was indicative of the rich activity in those sciences.

31. H. S. Jennings, "Some Implications of Emergent Evolution," *Science, 65* (1927), 19—25. William Morton Wheeler was to also adopt a version of emergent evolution.

biological sciences.[32] In writing their books, they were articulating and extending, but also codifying further, what had emerged as the disciplinary problematic[33] for the biological sciences. These concerns, in turn, were increasingly becoming part of the established and received wisdom of the profession. By the 1930s the belief in, and the drive toward, the unity of science and the unification of biology, as codified in the textbooks of biology, had been rendered tacit, unarticulated knowledge.[34]

As the driving force of positivism became more intense, and as belief in the unity of science became part of the received wisdom of biology, the threat of disciplinary subsumption became more imminent. Thus, with the peaking of logical positivism in the 1930s, there also arose biological movements that, in reacting to, or adjusting to, a positivist framework, made attempts to preserve the autonomy of biology. These movements, variously characterized as "holistic," "organicist," "emergentist," or "organismal," upheld in some manner the view that there were independent properties to life.[35] The independence of life made possible the independent existence of the science of life, biology. The rise of an avowed "neo-vitalistic" way of thought was explicitly articu-

32. Woodger cites many of his biological contemporaries. Interestingly enough, Woodger had been reading and citing Arthur Dendy, *Outlines of Evolutionary Biology* (New York: Appleton, 1912); this book, which has been viewed as a minor text in the history of biology, may well prove to have been more influential to a wider audience.

33. The inherited set of problems, discursively expressed, as codified in the textbooks (which discipline as they reproduce knowledge), accompanied by the tools and technologies that lead to negotiated solutions to common problems. This bears some resemblance to Kuhn's disciplinary matrix.

34. See Gairdner Moment, *General Biology for Colleges* (New York: Appleton-Century, 1942). The 1942 edition, compared to the 1950 (postsynthesis) edition, reveals the more secure location of evolution as a legitimate area of scientific inquiry.

35. The movements were simultaneous with a resurgence of right-wing ideology. Henri Bergson — the most prominent philosopher in the early years of the twentieth century — was heavily supported by right-wing groups as well as the Catholic church in France. Bergson and Bergsonianism were part of a larger "revolt from mechanism" that had accompanied the rise of positivistic philosophy and the revival of the occult in France. See R. C. Grogin, *The Bergsonian Controversy in France, 1900–1914* (Calgary: University of Calgary Press, 1988), for a discussion of the cultural milieu surrounding Bergson and Bergsonianism. The logical positivists and the "architects" of the evolutionary synthesis may well be viewed as silencing Bergson. Fisher, Haldane, and especially Huxley, whose early book, *The Stream of Life*, espoused a moderate Bergsonianism, had all been responding to Bergson in the 1920s; see Julian Huxley, *The Stream of Life* (New York: Harper and Row, 1927).

lated by the same J. Arthur Thomson[36] and Patrick Geddes who most strongly supported the unity of science in their textbook, *Life: Outlines of General Biology*.[37]

While justifying the independence of biology and the independence of life phenomena, these movements could still be accused of being too speculative, mystical, and metaphysical, and therefore of constituting bad science within a positivist framework. The most effective manner to achieve autonomy, unify biology, and accommodate positivistic science (the same fine line walked by both Woodger and Haldane) would lie in the elaboration of a position more reminiscent of Jennings's. Evolution, purged of unacceptable metaphysical elements, would function as the phenomenon that could make biology an autonomous science, at the same time that it served as the "unifying principle" that Woodger had sought. By the late 1940s, evolution would be made to "lift" biology above the physical sciences at the same time that it "bound" the fractured biological sciences. But first, evolution — threatened with extinction — had itself to be made into a more positive science. One outcome of the "evolutionary synthesis" would be the making of such a science.

THE DECLINE OF NATURAL HISTORY AND EVOLUTIONARY STUDIES

Within a positivist theory of knowledge,[38] the branches of biology that suffered most from critical methodological scrutiny were the natural history—oriented sciences. Especially vulnerable

36. Thomson had been a proponent of Bergson. He was also to support Hans Driesch for the Gifford Lectureship.

37. See J. Arthur Thomson and Patrick Geddes, *Life: Outlines of General Biology*, 2 vols. (London: Williams and Norgate, 1931). A 1925 edition of an earlier book by Geddes and Thomson entitled *Biology* had been cited by Woodger in his *Biological Principles*. Thomson and Geddes recommended Whitehead for further readings under the heading "Biology and Philosophy"; in the addendum to the second volume (p. 1499), they recommended Woodger's *Biological Principles* for further reading. Thomson and Geddes had explicitly discussed the orderly arrangement of the sciences. While each of the sciences rested on a base of other sciences, the most universal of which was logic, each successive science "retained its own distinctiveness" through a "fresh Emergence." They singled out sixteen fields on their "Graphic," each of which deserved full investigation for its "own sake, and for its services to others"; in their view, "The Unity of Science" was thus to be "realized" (see the "Explanation of 'Sciences in General' End-Paper").

38. I am using the term "positivist" here to denote positivistic philosophy preceding the logical positivism of the Vienna Circle.

were evolutionary studies. Not only did evolution reek of metaphysical and vitalistic elements — witness the popularity of such views as directed evolution, Lamarckism, emergent evolution, and the numerous other agents of evolution outlined by Darwin and his heirs — but it also defied the great method for grounding positivistic claims to knowledge: experimentation. Nor was there much *direct* empirical evidence for evolution, which was a historical science.

The rise of genetics[39] — the first unquestionably mechanistic, materialistic, law-like, and experimental biological science — at the turn of the century, combined with the increasing experimentalization of older established biological sciences like embryology and physiology, led to a period of turbulence for natural history.[40] As academic institutions, especially in America, were engaged in reforming and restructuring their curricula and instituting new departments, experimental sciences were increasingly favored over natural history or descriptive sciences. By the 1930s evolutionary studies and natural history were faced with extinction. In America, courses of instruction were unavailable at places like Harvard, while course catalogues indicate a proliferation of courses on experimental biological sciences.[41] General biological textbooks devoted less and less space to evolution, which often appeared as an afterthought in the final chapter.[42] Even histories of biology — a new historical genre — such as the great Norden-

39. Unlike the "-ologies," which were logocentric or descriptive sciences, genetics was an "-ics" word, meant to emulate physics and other exact sciences. For a discussion of the privileged location of genetics in the biological sciences, see V. B. Smocovitis, "Talking about Sociobiology," in *Social Epistemology* (forthcoming).

40. See Garland Allen, *Life Science in the Twentieth Century* (New York: Wiley, 1975). See also Elizabeth Gasking, *The Rise of Experimental Biology* (New York: Random House, 1970).

41. Course catalogues at key American institutions like Harvard University during the years 1897—1967 are a powerful indicator of the history of evolution in America. The first course at Harvard with a *sole* emphasis on evolution was given in 1908—9. Entitled Zoology 20d, *Investigation of the Factors Involved in Evolution*, this was not a regular course of instruction, but a "Course of Research." It was given by W. E. Castle at the Bussey Institution, and was offered for only one year. Nearly a twenty-year interval divided this course from the next course with a *sole* emphasis on evolution: Biology 112a, *Problems of Evolution*, which was offered in the academic year 1937—38. This course was intended primarily for graduate students and was taught by Edward Murray East, a geneticist at the Bussey Institution. East died in 1938, and the course was not offered further. Ernst Mayr taught Biology 147 (later changed to 247), *Systematics and Evolution*, in 1958.

42. This includes general zoology textbooks and botany textbooks.

sköld, indicated that evolution was thought to be a dead or dying subject.[43] Fewer and fewer periodicals were willing to publish scientific articles on evolutionary subjects. A quick survey of articles in the leading forum for naturalists in America, the *American Naturalist*, indicates the extent to which nonevolutionary geneticists and embryologists had infiltrated what had been the naturalists' journal. In the mid-1930s there was even an attempt to take over the *American Naturalist* and turn it into a journal for genetics.[44]

Funding for evolutionary research was nearly impossible to obtain, moreover. The enlightened despot of biological finance, Warren Weaver at the Rockefeller Foundation, was unsympathetic to nonexperimental and nonmedical fields.[45] Those most unsympathetic to evolution were developmental biologists like E. G. Conklin, and some geneticists like T. H. Morgan, who were impatient with what they saw as an unrigorous science. Even though Morgan was thought to have softened his harsh early stance on evolution by the 1930s, his book, entitled *The Scientific Basis of Evolution*, implied that there had been no scientific basis for evolution before Morgan himself.[46]

The decline in evolutionary studies was felt widely at the time. Where it was most devastating, was in Darwin's own homeland.

43. Erik Nordenskiöld, *The History of Biology* (New York: Knopf, 1928).

44. The *American Naturalist* had been privately owned by Jacques Cattell Press. In the mid-1930s the journal began to accommodate the market for not only experimental sciences like genetics but also sociology and psychology, and thus devoted less and less space to traditional naturalist articles. The move to found a society for the study of evolution was part of an increasing need that naturalists-systematists felt for creating an information service, with a journal to publish work that would otherwise have gone to the *American Naturalist*. I am at present preparing an article on the founding of the Society for the Study of Evolution based on documents recently deposited at the American Philosophical Society, which will discuss the status of the *American Naturalist*. The documents indicate an increasing dissatisfaction with existing journals in the 1930s. My perspective has been reinforced by Ernst Mayr (letters from Ernst Mayr to author, February 27, 1989, and August 15, 1989).

45. Both Ernst Mayr and G. Ledyard Stebbins were unable to obtain funds from the Rockefeller (letter from Ernst Mayr to author, August 15, 1989; letter from G. Ledyard Stebbins to author, May 4, 1989). This was to become even more problematic after the "discovery" of DNA. See Mayr's impassioned plea to continue the support of "classical" biology in the face of the "new" biology. Ernst Mayr, "The New versus the Classical in Science," *Science, 141* (1963), 765.

46. T. H. Morgan, *The Scientific Basis of Evolution* (New York: W. W. Norton), 1932. For a discussion of the development of Morgan's views on evolution, see G. E. Allen, "Thomas Hunt Morgan and the Problem of Natural Selection," *J. Hist. Biol., 1* (1968), 113—139; idem, *Thomas Hunt Morgan: The Man and His Science* (Princeton: Princeton University Press, 1978).

Julian Huxley coined the phrase "the eclipse of Darwin" to highlight the downturn in his *Evolution: The Modern Synthesis.* Historians and biologists have long recognized this period in the history of biology, but few have interpreted the phenomenon in a disciplinary matter.[47] The eclipse of Darwin referred not just to the demise of Darwin's theory of natural selection — natural selection was unsatisfying and was never widely or exclusively accepted, even by the most fervent of Darwin's followers — but also to the demise of natural history and evolutionary studies, the field that Darwin had represented to his heirs. So long as the agents of evolution were unquantifiable, and evolution remained a nonexperimental practice, evolution was bound to be a speculative, an unrigorous, and ultimately an endangered science.[48] Embodying this antievolution attitude, the physiologist (and devotee of Jacques Loeb) W. J. Crozier asserted to his students in introductory biology at Harvard: "Evolution is a good topic for the Sunday supplements of newspapers, but isn't science: You can't experiment with two million years!"[49]

As positivistic currents increasingly structured modern thought,[50] nonexperimental sciences fell to accusations of being inexact, imprecise, and unrigorous. This tension between experi-

47. See Peter Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolutionary Theories in the Decades Around 1900* (Baltimore: Johns Hopkins Press, 1983); Mayr, *Growth of Biological Thought* (above, n. 25).

48. Writing about the state of Darwinism at the turn of the century, Vernon L. Kellogg explicitly stated that Darwinism was undergoing methodological scrutiny and was coming under fire from "German biologists" and experimental biology itself. In his introductory discussion of the "Death-Bed of Darwinism" he wrote: "there is going on a most careful re-examination or scrutiny of the theories connected with organic evolution, resulting in much destructive criticism of certain long-cherished and widely held beliefs, and at the same time there are being developed and almost feverishly driven forward certain fascinating and fundamentally important new lines, employing new methods, of biological investigation. Conspicuous among these new kinds of work are the statistical or quantitative study of variations and that most alluring work variously called developmental mechanics, experimental morphology, experimental physiology of development, or, most suitably of all because most comprehensively, experimental biology." Kellogg continued: "Now this combination of destructive critical activity and active constructive experimental investigation has plainly resulted, or is resulting, in the distinct weakening or modifying of certain familiar and long-entrenched theories concerning the causative factors and the mechanism of organic evolution. Most conspicuous among these theories now in the white light of scientific scrutiny are those established by Darwin, and known, collectively, to biologists, as Darwinism" (Vernon L. Kellogg, *Darwinism Today* [New York: Henry Holt, 1907], pp. 1–2).

49. G. Ledyard Stebbins, unpublished autobiographical manuscript, "Getting There Is Half the Fun," p. 10.

50. This included the progenitors of the logical positivists.

mentalists and nonexperimentalists just around and after the turn of the century has long been recognized by historians of biology Garland Allen and Ernst Mayr.[51] Genetics, physiology, embryology, and other experimental sciences leaned to one side, while natural history and evolutionary studies (nonexperimental, unquantifiable sciences with little direct empirical evidence) leaned to the other side.[52] If observation and experiment — essential positivistic criteria for legitimate science — were not part of the methodological apparatus of the discipline, the discipline came under fire, and the tensions only increased with time.[53] By the late 1920s — as Woodger was writing his book — the lack of rigor and grounding in evolutionary studies had become even more acute. The 1930s soon witnessed the emergence of a group of biologists who were to serve as bridge-builders and "architects" by adopting methodologies from the physical sciences to make evolution a more positive science. In so doing they constructed a unified and autonomous science of biology. "Modernizing" evolution, they also preserved the naturalistic, Darwinian tradition that had gone into decline.

THEORY AND EXPERIMENT IN EVOLUTION

The critical moment for evolutionary studies — and the unifica-

51. See Garland Allen, "Naturalists and Experimentalists: The Genotype and Phenotype," *Stud. Hist. Biol., 3* (1979), 179—209. The conflict between naturalists and experimentalists is the basis for the "Allen" thesis. The applicability of this thesis has been a contentious issue for historians of biology. My own position strongly upholds the Allen thesis. Within a positivist framework, nonexperimental sciences would be greatly suspect. Within this framework, the conflict between geneticists and naturalists, which Ernst Mayr has recognized, is also understandable. Genetics, being experimental, and an established mechanistic and materialistic science, would be favored over nonexperimental and descriptive sciences.

52. Vernon Kellogg's 1907 *Darwinism Today* supports this argument. Kellogg concluded with the following thoughts: "We are ignorant; terribly, immensely ignorant. And our work is, to learn. To observe, to experiment, to tabulate, to induce, to deduce. Biology was never a clearer or more inviting field for fascinating, joyful, hopeful work. To question life by new methods, from new angles, on closer terms, under more precise conditions of control; this is the requirement and the opportunity of the biologist of to-day. May his generation hear some whisper from the Sphinx!" (p. 387).

53. These criticisms were leveled not only at the biological sciences, but at other disciplines as well. The discipline of history, for instance, emerged as a legitimate area of inquiry only when scientific — and positivistic — standards began to be adopted in the late years of the nineteenth century. "Observation," and eventually — through quantitative modeling in the 1960s — "experiment," were adopted by historians. History in this manner became a "social science."

tion of biology — came with the successful adoption of experimentation in evolutionary practice through mathematical modeling. This took place during the celebrated interactions between mathematical theorists and field biologists: in England, between R. A. Fisher and E. B. Ford, and in America, between Sewall Wright and Theodosius Dobzhansky. To be sure, there had been other attempts at experimentation in evolution,[54] including J. W. H. Harrison's work on melanism in Lepidoptera, H. C. Bumpus's natural experiments with sparrows, W. F. R. Weldon's work with crabs, and even the much-celebrated experiments of Weismann and Darwin himself[55] — but none of these attempts had led successfully to the most "objective" type of knowledge: the attachment of numbers to nature within a mechanistic and materialistic framework.[56] Only after the variables of natural selection, genetic drift, and mutation were articulated by mathematicians in the early years of the twentieth century, to formulate the "Hardy-Weinberg Equilibrium Principle,"[57] and only after there was

54. In the early years of the twentieth century the move to experimentalize evolution was institutionalized officially by the founding of an experiment station expressly devoted to this end: in 1904 the Carnegie Institute of Washington supported the founding of the Station for Experimental Evolution at Cold Spring Harbor, Long Island, N.Y. The first director was C. B. Davenport.

55. J. W. H. Harrison, "The Induction of Melanism in the Lepidoptera and Its Evolutionary Significance," *Nature, 119* (1927), 127—129; H. C. Bumpus, "The Elimination of the Unfit as Illustrated by the Introduced Sparrow, *Passer domesticus*," in *Biological Lectures from the Marine Laboratory, 1898* (Boston, 1899), pp. 209—226; W. F. R. Weldon, "An Attempt to Measure the Death Rate Due to the Selective Destruction of *Carcinus moenas* with Respect to a Particular Dimension," *Proc. Roy. Soc. London, 58* (1895), 557—561.

56. Additional evidence for this comes from examination of a book meant to introduce readers to "great experiments in biology," in which the ordering of the topics roughly reflects chronological experimentalization (and also legitimation): The Cell Theory (Cytology), General Physiology, Microbiology, Plant Physiology, Embryology, Genetics, and lastly Evolution. Four examples of classic experiments in evolution are given: selections from C. Darwin and A. R. Wallace (not experimental), G. H. Hardy (mathematical, but not experimental), N. H. Horowitz (experimental, but biochemical), and Th. Dobzhansky (experiments with natural populations of organisms). The ordering as well as the selections justify my sense that it is Dobzhansky's work with natural populations that forms the critical moment of the experimentalization of evolution. See Mordecai L. Gabriel and Seymour Fogel, eds., *Great Experiments in Biology* (Englewood Cliffs, N.J.: Prentice Hall, 1955).

57. The "Hardy-Weinberg Principle" describes the conditions under which evolutionary equilibrium is maintained — that is, the conditions under which changes in gene frequencies or genotypes *do not* take place. For a history of mathematical population genetics see William B. Provine, *The Origins of Theoretical Population Genetics* (Chicago: University of Chicago Press, 1971); idem, "The Role of Mathematical Population Geneticists in the Evolutionary Synthesis of the 1930s and 1940s," *Stud. Hist. Biol, 2* (1978), 167—192.

agreement that these were the legitimate variables in evolution,[58] could they be measured and made to work in natural populations. By demonstrating how these variables could work in natural populations, the newly constructed mathematical models — systems of interacting variables — of R. A. Fisher, J. B. S. Haldane, and Sewall Wright had utility for field-oriented naturalists needing a workable methodology.[59]

These mathematical models had not arisen de novo, however. Fisher, Haldane, and Wright had been issued detailed prescriptives by biologists in the field as well as biologists on the farm, and all three had had views of population structures in mind as they began to articulate their models and transmit their representations to field biologists Ford and Dobzhansky. Historians know little about the nature of the dialogue between Fisher and Ford, who engaged each other in person and left little correspondence behind, but historian William Provine has mapped out the manner and proximity of the interaction between their American counterparts, Dobzhansky and Wright.[60] The interaction between Wright and Dobzhansky did not lead to a miraculous correspondence of mathematics and nature, but was the product of an ongoing dialogue between workers at the desk and workers in the field and on the farm. Both "theorist" and "experimentalist" were engaged in practical attempts to solve agreed-upon problems within evolutionary studies, the most central of which was the determining of the agents responsible for evolution. The mathematical models of Wright and the organismal models of Dobzhansky became developed into coadapted heuristics, which successfully led to the attachment of numbers to nature.[61]

58. See William B. Provine, *Sewall Wright and Evolutionary Biology* (Chicago: University of Chicago Press, 1986), for a discussion of the negotiations leading to agreement over the relative importance of these variables.

59. See Provine, *Origins*; idem, "Role of Mathematical Population Geneticists"; S. Wright, "Evolution in Mendelian Populations," *Genetics, 16* (1931), 97—159; idem, "The Roles of Mutation, Inbreeding, Crossbreeding, and Selection in Evolution," *Proc. Int. Congr. Genet., 6*: 1 (1932), 356—366; J. B. S. Haldane, "A Mathematical Theory of Natural and Artificial Selection," *Trans. Proc. Cambridge Phil. Soc.* (1924—1932); idem, *The Causes of Evolution* (1932; repr. Ithaca, N.Y.: Cornell University Press, 1966); R. A. Fisher, *The Genetical Theory of Natural Selection* (Oxford: Oxford University Press, 1930).

60. Provine, *Sewall Wright*, pp. 327—365.

61. Dobzhansky actively sought a model organism, *Drosophila pseudoobscura*, to fit Wright's schemes. *Drosophila melanogaster*, the familiar tool of the Morgan school's genetics, did not display phenomena that could be made to work with Wright's evolutionary models. Provine recounts the story of how Dobzhansky went into a state of "scientific schizophrenia" until Robert D. Boche gave him *Drosophila pseudoobscura* (Provine, *Sewall Wright*, p. 333).

The quantification of evolution[62] — the attachment of numbers to "nature" — and the growing measurability and testability of natural selection were part of a process that would eventually lead to general support for natural selection as the primary *mechanism* of evolution.

THE "PURIFICATION" OF EVOLUTION AND THE "UNIFICATION" OF BIOLOGY

While organic evolution had been perceived as a causo-mechanical explanation[63] for organic change, no real, indisputable causo-mechanical explanation had been satisfactorily demonstrated. For readers of the *Origin of Species*, natural selection was viewed as one of several "agents" (that is, a causo-mechanical "agent") of evolution. Belief in the agency of natural selection was the outcome of the manner in which Darwin had structured his argument for natural selection, arguing from analogy with artificial selection. While natural selection did not take place through the "hand" of a selector, the view of natural selection as agent still had built into it a degree of purposiveness, of teleology, and therefore it had a "metaphysical" taint. This was one reason for the increasing decline in evolutionary studies.

With the work of the modelers, and the adoption of experimentation, the view of natural selection as agent began to diminish, so that by the 1930s natural selection took on a causo-mechanical existence. Terms borrowed from the physical sciences, like "cause" (Haldane's preferred word), "factor" (Wright's preferred word), and finally "mechanism" (Dobzhansky's and Huxley's preferred word) slowly supplanted the term and the view of selection as agent, although Huxley viewed selection as both agent and mechanism simultaneously and Fisher still viewed it as an agent (I will discuss this below).[64]

All three of the mathematical modelers were attempting to bring biology to par with the physical sciences, as they drew on, and modeled after, the repertoire of the physical sciences.[65] This

62. Mutation, migration, population structure, and systems of mating, as well as random genetic drift, became quantifiable and measurable.

63. This is the precise phrase used by Vernon Kellogg in his 1907 book.

64. These terminological variations are still present in evolutionary biologists' vocabularies, though "mechanism" is the preferred word.

65. I am not here claiming that the modelers were *strict* physicalists who wished to reduce biology to physics, but that they were drawing from exemplars in the more exact and rigorous physical sciences. The exemplar of *the* scientific method was Newtonian physics, as articulated through Newton's Rules of

was the case for Haldane, whose major work was entitled *The Causes of Evolution* and who himself was a supporter of the Unity of Science Movement,[66] and to a lesser extent for Sewall Wright, for whom natural selection was a mathematical "factor."[67] The wish to bring biology to heel within the physical sciences was most virulent in Fisher, who clearly stated his intent to model evolution after physics and chemistry. The title of Fisher's 1930 book, *The Genetical Theory of Natural Selection*, raised the status of natural selection from fact to genetical theory, as Fisher articulated his "fundamental theorem of natural selection."[68]

As natural selection became measurable and testable, and came to be seen as a causo-mechanical explanation for organic change, much of the metaphysical and speculative status of the phenomenon came to be removed.[69] As natural selection and the other "factors" in evolution came increasingly to be seen in physicalist terms, the factors took on a causo-mechanical reality. Simultaneously, these "mechanisms" — now made to be mechanisms — came to be aligned with the material basis of evolution. By the mid-1930s all the components were assembled to make evolution as mechanistic and materialistic a science as possible, grounded

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Reasoning and then transmitted to a wider audience — including Darwin himself — through philosophers like William Whewell and John Herschel. Fisher, who espoused a form of indeterminism, and especially Wright, who upheld a form of pansychism, could hardly be deemed *strict* physicalists. For a discussion of the metaphysics of Fisher and Wright see M. J. S. Hodge, "The Metaphysics of R. A. Fisher and Sewall Wright," forthcoming.

66. In 1936 Haldane attended the Second International Congress for the Unity of Science held in Copenhagen and gave a paper on "Analysis of Causality in Genetics" (Box 3, folder 2, Joseph Regenstein Library, The University of Chicago).

67. The mathematical underpinning of Wright's conception of evolution is indicated by his notion of natural selection as factor. Interestingly, Sewall Wright never published a major book in the 1930s to rival Fisher or Haldane. For both Fisher and Haldane, who had greater political ambitions, a book-length treatment of evolution made more sense. Wright was eventually to publish his book between the years 1968 and 1978: *Evolution and the Genetics of Populations*, 4 vols. (Chicago: University of Chicago Press, 1968—78).

68. Fisher's fundamental theorem was meant explicitly to resemble the law that held the "supreme position among all laws of nature," the second law of thermodynamics. Among other resemblances, he noted that both had the properties of populations or groups of aggregates, and both were statistical laws. If the chemists could have such a supreme law, Fisher argued, the biological sciences could have one as well. Natural selection, as Fisher described it, became the biological analogue of the second law of thermodynamics.

69. Though many of the metaphysical features of selection were removed, I will argue shortly that *enough* of the teleology was left behind for there to be an autonomous science of life.

firmly in the Hardy-Weinberg Equilibrium Principle, and following "observation and experiment," but in a manner that also lent autonomy to the biological sciences. The bringing together of the mechanical cause of evolutionary change with the material basis of evolution would prove to be the most difficult, but key, feature in the making of a science — now Newtonian — of evolution. This was to come through the work of Sewall Wright, and especially Theodosius Dobzhansky.

For Dobzhansky, who apprenticed in the laboratory of classical geneticist Thomas Hunt Morgan,[70] the material basis for evolution — that which Darwin and his heirs had sought — became the gene, arrayed in linear fashion on the material carriers of heredity, the chromosomes.[71] Close examination of the behavior of chromosomes in natural populations of *Drosophila pseudoobscura* led to the determination of inversion frequencies and formed the basis of what is known as Dobzhansky's studies of the genetics of natural populations.[72] Genetic mechanisms that accounted for microevolutionary change (evolution below the species level) also accounted for larger-scale or macroevolutionary change (evolution including, and above, the species level); this continuum therefore accounted for mechanisms of speciation that had a genetic grounding.[73]

The alignment of the material basis with the mechanical cause of evolution bore close resemblance to Newtonian mechanics. The gene, which after the work of Morgan and his group became an *entity* that functioned as the *particle* of heredity,[74] became the

70. Dobzhansky had also worked with Iurii Filipchenko in Russia. For an intellectual biography of Dobzhansky, see W. B. Provine, "Origins of the Genetics of Natural Populations Series," in *Dobzhansky's Genetics of Natural Populations I—XLIII*, ed. R. C. Lewontin et al. (New York: Columbia University Press, 1981), pp. 5—83. Dobzhansky actually learned much of his classical genetics through his dialogue with A. H. Sturtevant.

71. The chromosome theory of heredity, sometimes referred to as the Sutton-Boveri theory, had been articulated in 1902—3. It pointed to the chromosomes as the material carriers of heredity. The "gene" — as constructed by workers at the turn of the century to become the *unit* or particle of heredity — was seen to be carried on the material of the chromosomes, made observable by the advent of dyes, stains, sectioning techniques, and the imaging technology of the microscope. The gene can be viewed as the analogue of the particle in Newtonian physics; hence, the debates focusing on the gene bear some resemblance to the debates in Newtonian physics.

72. For a historical description of the "GNP" series see Provine, "Origins of the Genetics of Natural Populations Series."

73. Dobzhansky introduced notions of reproductive isolating mechanisms to account for the origin of species.

74. Particularistic theories of heredity had been favored over blending theories for this reason.

unit of evolutionary change, and selection would become the primary driving or motive *force* to propel evolutionary change.[75] The gene — by the Morgan school's standards — had been constructed to particularize and individuate, and at the same time to limit the rate of, change, and mutations became the determinants or *fluxions* of evolutionary change. Phenotypic saltations, observable as the result of the chromosomal alterations prevalent in model plant organisms like *Oenothera lamarckiana*, were therefore to be tempered through the adoption of model animal systems like *Drosophila melanogaster*, which would restrict evolutionary change to the level of small, individual differences. Such *point* mutations instead of *macro*mutations would thus *limit* the rate of change. The final result, Dobzhansky's "synthesis," offered an account of evolutionary change that would limit — and make deterministic — the *rate* of evolutionary change. From then on, measures would be taken to calculate and determine *evolutionary rates of change*. Evolution, in turn, would be redefined as the "change in gene frequencies." Viewed as a problem in accounting for change, the Hardy-Weinberg Equilibrium, which effectively set the conditions under which there would be *no* evolutionary change, converted the variables of natural selection, mutation, population structure, random genetic drift, migration, and systems of mating into causal *explanations* for evolutionary change. As in Newton's laws of motion, there could be no evolutionary change without one of these *causes* of evolutionary change. Evolutionary change would thus be constructed on models of physical change so that evolution would demonstrate law-like regularities analogous to the law-like regularities in Newtonian physics.

Dobzhansky had drawn heavily, consciously, on the "classical" genetics of the Morgan school, which, in its mechanistic and materialistic nature, most closely resembled "classical" physics. Genetics (and the physical world of the gene) was used as the grounding for Dobzhansky's new "evolutionary genetics" — a new term — and formed the basis for his belief in the continuum between microevolution and macroevolution (which stretched from the gene to the human, and to human culture). The title of his book published in 1937 reveals this grounding: *Genetics and the Origin of Species* offered a framework that brought together the material basis for evolution, determined first through the work of geneticists, with

75. This substituted selection pressure for the mutation pressure present in the mutation theory of Hugo de Vries. Dobzhansky's evolutionary genetics therefore stressed the *creative* element of natural selection, whereas de Vries had stressed the *eliminative* element. For Dobzhansky, the unit of evolutionary change was the gene; for de Vries, it was the species.

causo-mechanical explanations — made mechanical through the models — for evolution.[76] Hence, the "evolutionary synthesis," held by commentators to involve the synthesis between "genetics and selection theory," can be reinterpreted as the bringing together of the material basis of evolution (the gene) with the mechanical cause of evolutionary change (selection), to make a mechanistic and materialistic science of evolution that rivaled Newtonian physics while still preserving the autonomy of the biological sciences. Dobzhansky's alignment of the material basis with the mechanical cause in turn gave rise to a science obeying the methodology of "observation and experiment," evolutionary genetics, which had the added bonus of working in *natural* populations of *Drosophila*. Dobzhansky was aware of this grounding himself and reflected on this in his oral memoirs of 1962:

> Genetics is the first biological science which got in the position in which physics has been in for many years. One can justifiably speak about such a thing as theoretical mathematical genetics, and experimental genetics, just as in physics. There are some mathematical geniuses who work out what to an ordinary person seems a fantastic kind of theory. This fantastic kind of theory nevertheless leads to experimentally verifiable prediction, which an experimental physicist has to test the validity of. Since the time of Wright, Haldane, and Fisher, evolutionary genetics has been in a similar position.[77]

This mechanistic and materialistic framework grounded in genetics, and ultimately in the mathematical principle of the Hardy-Weinberg Equilibrium, would also account for higher-level phenomena, including not only the origin of species, but also the origin of humans, the mind, and culture, now unified and reducible to lower-level phenomena. The continuum, as it was constructed, could therefore legitimate as it connected inquiry into the *mechanics of change in human culture* with the material on which the mechanics acted, as well as introducing inquiry into a mechanistic and materialistic theory of mind — all ultimately reducible to the gene. The continuum that Dobzhansky's framework provided would also ground — through a fundamental

76. Theodosius Dobzhansky, *Genetics and the Origin of Species* (New York: Columbia University Press, 1937).

77. As cited on p. 277 of Provine, *Sewall Wright*, from the Oral Memoir of 1962, pp. 500—501, Columbia University Archives, New York.

axiomatic mathematical logic — the social sciences.[78] But for Dobzhansky, and other evolutionists, this continuum involved only partial reduction.[79] While the possibility of reduction from higher levels to lower levels existed, measures could and would be taken to ensure that certain phenomena were not subject to reduction to the physical world. Emergent properties, which in some measure were metaphysical,[80] would therefore be evoked by biologists to construct a meaningful life, devoid of complete determinism; at the same time, these properties would justify the autonomy of the biological sciences.

This continuum was *the* most important undergirding feature of Dobzhansky's framework. So long as the continuum between the gene and the human (grounded in Newtonian physics and chemistry and the Hardy-Weinberg Equilibrium Principle) existed, there would be unity between the levels of evolution. Though the ultimate grounding was to be the physical world of the gene, for Dobzhansky (and others) there would be enough of the metaphysical world left behind at higher levels (like mind and culture) to make possible a meaningful life, avoiding a completely genetically deterministic view of life.[81] Reducible to the physical world and ultimately to a mathematical logic, Dobzhansky's continuum could function as a unifying argument and a *unification event* by

78. With the maturation and institutionalization of the social sciences in the 1960s, the nature vs. nurture debates would sweep across universities in the U.S. and Britain.

79. Ernst Mayr was to support a partially emergentist model; see his discussion in *Growth of Biological Thought* (above, n. 25), pp. 63—64. Social scientists were to also adopt emergentist models to support nurture over nature.

80. Whether emergent properties are metaphysical is a contentious issue. I would argue that in a *disciplinary* sense emergentism functions in the same manner as vitalism, teleology, and other unarticulated metaphysical elements — all "lift" biology from complete reduction to the physical sciences.

81. As a group, the architects of the evolutionary "synthesis" would negotiate and strike just the right balance between mechanistic materialism and some form of emergentism. Those who rejected a completely mechanistic and materialistic framework include E. Mayr, R. A. Fisher, S. Wright, C. H. Waddington, D. Lack, B. Rensch, and J. Huxley, as well as Dobzhansky himself. Of the group of "synthesis" architects the only person who upheld mechanistic and materialistic evolution was Simpson, for whom evolution was a historical process, one that dealt with unique historical events. In Simpson's historicist perspective, chance events, contingencies, introduced indeterminism into the evolutionary system. Geneticists like T. H. Morgan, A. H. Sturtevant and C. Bridges upheld mechanistic and materialistic frameworks. See the discussion below on R. Goldschmidt. Will Provine provided this list of those who upheld and rejected completely mechanistic and materialistic evolution. See also the discussion below of the Post-Sputnik Biological Sciences.

the standards of logical positivism. Obeying the common method of observation and experiment, reducible through logic, and obeying axiomatic mathematical principles, evolution and biology would in turn bind and unify the sciences.

The quantification and measurability of these factors, the consequent quantification of evolution, the alignment of the mechanical cause with the material basis for evolution, and the beginnings of the adoption of natural selection as the *primary* mechanism of evolution led simultaneously to the ejection of "alternative" mechanisms of evolution.[82] Directed evolution (aristogenesis, nomogenesis, orthogenesis, etc.), Lamarckian inheritance, and emergent evolution, among others, were ejected from mainstream evolutionary studies, as what appeared to be a narrowing or streamlining of evolutionary theory took place. This is the phenomenon that Will Provine has characterized as the "evolutionary constriction."[83] With these metaphysical elements ejected, a unification event — based on a Machian unification argument — was beginning to take place.

But there was one complication in aligning the material basis of evolution with the mechanical cause of evolutionary change. For Dobzhansky as for Wright, strongly selectionist models of evolution did not resonate with their view of evolution in natural populations.[84] Strongly selectionist models had been favored by R. A. Fisher, the same individual who persisted in viewing natural selection as an agent. For Fisher, the belief in the agency of selection and the power of selection was inextricably linked to his deeply held eugenical commitments. His 1930 book devoted a great deal of space to a discussion of the eugenicist agenda. If selection had enough agency (and at the same time were a mechanical principle), then all the more possible and rapid the "improvement" of humans. Just as important as the commitment to human "improvement" was the closely related "progressive" view of evolution, made all the more obvious with the publication of Huxley's *Evolution: The Modern Synthesis*. Wright and Dobzhansky's initially strong support of random genetic drift diminished as the result of a combination of factors: the drive for the improvement of humans, and the increasing necessity for progres-

82. They became alternative, once biologists could make *one* of them primary.

83. W. B. Provine, "Progress in Evolution and Meaning in Life," in Matthew Nitecki, *Evolutionary Progress* (Chicago: University of Chicago Press, 1988), pp. 49—74.

84. This feature of the Wright and Dobzhansky framework has been discussed at length by William B. Provine in his biography of Wright.

sive evolution within a positivistic theory of knowledge.[85] In such a philosophical framework, more strongly selectionist models *would* be favoured by biologists who patterned themselves after physicists at the same time that they pointed the way to the improvement of humanity and painted a progressive and optimistic picture of the world. This was the packaging that Julian Huxley was to put together; and Huxley's packaging was to prove the most immediately efficacious — to a wide audience — to lend both unity and autonomy to the biological sciences. The ultimate push to adopt more selectionist models would come from *outside* the local network around Dobzhansky and Wright that wished to preserve some measure of progress. Evolutionary models favoring random genetic drift, which enforced a stochastic view of evolution — and culture — would not be favored in a postwar frame of mind seeking to improve the world.[86] So powerful would be the need for a progressive and selectionist framework in the 1940s that even Dobzhansky and Wright would come to adopt more strongly selectionist models.

For the time being, however, the alignment of the mechanical cause (selection) and the material basis (the gene) for evolution, and the ejection of *enough* of the metaphysical components, along with the establishment and extension of experimentation in natural populations, began to legitimate the long-beleaguered evolutionary studies and unify the fractured biological sciences within the continuum from the gene to the human. Purged of its unacceptable metaphysical elements, and grounded ultimately in the mathematics of the Hardy-Weinberg Equilibrium Principle, evolution was becoming a purified science more secure in its foundations, a science that could *begin* to meet even Woodger's criticisms.[87] Evolution would be used to lend both unity and autonomy to the biological sciences. Evolution in turn would unify the sciences. The "ultimate" questions of the meaning of life and human origins would be cojoined, and reduc*ible* through logic, to the physicalist, mechanistic, and materialistic frameworks of the physical sciences.[88]

85. I will discuss progressive evolution shortly.

86. Too much stochasticity in the form of random genetic drift made the system *too* unpredictable. A middle ground — deterministic enough to make predictions, but having enough indeterminism — through metaphysical or emergentist phenomena would be favored. At the same time this balance made possible a meaningful life with humans as agents of their own free will.

87. The sense that evolution is one of the "softer" of the biological sciences is still prevalent.

88. Taking *The Meaning of Evolution* to the wider audience in 1949,

BINDING THE HETEROGENEOUS PRACTICES OF BIOLOGY: DOBZHANSKY'S "*GENETICS AND THE ORIGIN OF SPECIES*" AND *THE* "*COLUMBIA CLASSICS IN EVOLUTION*"

The genetic grounding Dobzhansky had adopted rapidly became the foundation for what Woodger had pointed out were the unstable biological sciences. What for Woodger had appeared to be a ground of "fundamental biological antithesis," difficult to harmonize and synthesize because of underlying contradictions, became the *common* ground of genetics and selection theory — the "synthesis" that Dobzhansky's evolutionary genetics had achieved. Evolutionary genetics thus became the ground on which the heterogeneous practices of the biological sciences were stabilized and bound.

A group of biologists, in close dialogue with the extraordinarily charismatic Dobzhansky, began to link up and legitimate their *own* practical concerns with the framework provided by the "synthetic" evolutionary genetics.[89] By 1936 Dobzhansky was preparing to present his evolutionary genetics — linked with these practical concerns — to an even wider audience of evolutionists by writing what became the first textbook of evolution. The experimental and mathematical practices of evolution and Dobzhansky's

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Simpson would write: "It is assumed that a material universe exists and that it corresponds with our perceptions of it. The existence of absolute, objective truth is taken for granted as well as the approximation to this truth of the results of repeated observations and experiments. That such assumptions are debatable is evident from the violence with which they have been debated at various times. In practice, however, we all have to take it either that they are true or that we necessarily proceed *as if* they were true. Otherwise there is no meaning in science or in any knowledge, or in life itself, and no reason to enquire for such meaning" (George Gaylord Simpson, *The Meaning of Evolution: A Study of the History of Life and of Its Significance for Man* [New Haven: Yale University Press, 1949], p. 7).

89. See Clifford Geertz's reflections on charismatic figures in *Local Knowledge* (New York: Basic Books, 1983), and see also idem, *The Interpretation of Cultures* (New York: Basic Books, 1973). The authors of the Columbia Classics had started engaging in dialogue (both published and personal) with one another in the mid-1930s. Before these books appeared all of the authors had published series of papers on related themes, which the others had been reading. Hence Dobzhansky's framework developed as a result of a multidirectional traffic on influence, negotiated and renegotiated by members inside and outside Dobzhansky's local group. Group dynamics were complex. Dobzhansky had personally drawn in Stebbins and others to his evolutionary genetics. Dobzhansky and Huxley had been in close touch with each other and had made moves to start an official society in 1939, only to be thwarted by wartime preparations. Mayr was also in contact with Huxley and read and commented on early chapters of Huxley's 1942 book.

evolutionary genetics became codified and disseminated with the publication in 1937 of *Genetics and the Origin of Species*, which served to initiate practitioners into the craft. But Dobzhansky's book, which drew heavily on calculations of gene frequencies and defined evolution in terms of changes in those frequencies, was hardly about evolution as a whole.[90] Nor could genetics by itself account for the entirety of the evolutionary process. If genetics indeed had a place in accounting for the origin of species, as Dobzhansky argued, then so too did other neighboring biological disciplines.

The audience for Dobzhansky's book consisted of diverse groups within evolutionary studies who read, responded to, and further legitimated and extended the evolutionary framework provided by Dobzhansky's evolutionary genetics. In so doing they began to bind the heterogeneous[91] practices of evolution into an evolutionary *network* grounded in genetics and selection theory. The publication of *Genetics and the Origin of Species* signaled and served as catalyst for the publication of a series of books now known as the Columbia Classics in Evolution: Ernst Mayr's *Systematics and the Origin of Species*, G. G. Simpson's *Tempo and Mode in Evolution*, and G. L. Stebbins's *Variation and Evolution in Plants*.[92] These books were written by individuals who, in engaging in dialogue with Dobzhansky, in turn legitimated as they grounded *their* disciplines with Dobzhansky's experimental evolutionary genetics. Each of the authors had inherited from his discipline a different set of problems, practical in nature, which Dobzhansky's book and its evolutionary program offered in some measure to resolve. Each read *his own* meaning into Dobzhansky and responded in turn. And each offered an amendment to Dobzhansky's framework.

90. Ernst Mayr was to play an increasingly vital role in pointing out that this definition of evolution, which did not account for the origin of discontinuities (especially speciation), was incomplete. In the late 1950s he was to describe the narrow population geneticists' view of evolution as "bean-bag genetics," and to promote the transformational features of evolution. See his inaugural lecture of 1959 at the Cold Spring Harbor Symposium, "Where Are We?" *Cold Spr. Harbor Symp. Quant. Biol., 24* (1959), 409—440; reprinted in Ernst Mayr, *Evolution and the Diversity of Life: Selected Essays* (Cambridge, Mass.: Belknap Press of Harvard University Press, 1976), pp. 307—328.

91. The term "heterogeneous" had been used in 1934 by Max Black: see chap. 1, "The Heterogeneity of Science," in Rudolf Carnap, *The Unity of Science*, trans. Max Black (London: Kegan Paul, Trench, Trubner, 1934).

92. Ernst Mayr, *Systematics and the Origin of Species* (New York: Columbia University Press, 1942); G. G. Simpson, *Tempo and Mode in Evolution* (New York: Columbia University Press, 1944); G. L. Stebbins, Jr., *Variation and Evolution in Plants* (New York: Columbia University Press, 1950).

To Mayr, the appeal of Dobzhansky's framework lay in its populational features and its support of a biological species concept.[93] For practicing ornithologists like Mayr in the 1930s and 1940s, successive populational samples instead of the solitary "type specimen" had become the working unit of the taxonomist. Dobzhansky's framework with its emphasis on natural populations and subspecies thus made tractable the problems of the working taxonomist, and gave plausible causo-mechanical explanations, or mechanisms for speciation, which took into account geographic variation within slow, gradual rates of change. Dobzhansky's framework also had the most pleasing aspect of returning systematics to the field. But while Dobzhansky emphasized the populational features of evolution and opened inquiry into the mechanisms of speciation, his book failed to discuss in sufficient detail the topic heralded by the title: the origin of species. In stressing a definition of evolution based solely on gene frequencies, Dobzhansky's framework did not sufficiently take into account the primary concern for systematists and a central component of evolution — accounting for the origin of organic discontinuities. Redressing a perceived imbalance in Dobzhansky's emphasis on genetics to the exclusion of systematics, Mayr's *Sytematics and the Origin of Species* — as the title indicates — was therefore meant to be a direct response to *Genetics and the Origin of Species*.[94]

To Simpson, the measurability of natural selection as outlined in Dobzhansky's book meant that paleontology could be rid of the metaphysical horrors of directed evolution, which, following H. F. Osborn's directives, had pervaded paleontology. In Simpson's view, natural selection, modified in such a manner that "quantum evolution" could take place to account for accelerations in evolutionary change, made possible the resolution of problems faced by practicing paleontologists. The paleontological framework of Simpson's *Tempo and Mode in Evolution*, in turn, also had a

93. Dobzhansky's interest in populations was an outcome of his background in systematics, which he had inherited from his Russian mentors. For a historical discussion of the Russian context and Dobzhansky's origins see Mark Adams, "The Founding of Population Genetics: Contributions of the Chetverikov School, 1924—1934," *J. Hist. Biol.*, *1* (1968), 23—39; and idem, "Towards a Synthesis: Populations Concepts in Russian Evolutionary Thought, 1925—1935," *J. Hist. Biol.*, *3* (1970), 107—129.

94. Ernst Mayr's own historical sense, correctly so, is that the synthesis was in part a switch from a typological to a populational way of thinking. It was — for him. See Mayr's "Prologue" to Mayr and Provine, *Evolutionary Synthesis* (above, n. 7).

powerful effect in validating Dobzhansky's framework. By providing *observable* evidence of the evolutionary process, Simpson was to legitimate evolution as a historical *science*. The "woefully inadequate" fossil record was used once again to buttress an evolutionary perspective favoring slow, gradual change with enough room for evolutionary quantum "jumps."[95]

For Stebbins, the appeal of Dobzhansky's framework was in making workable (within an evolutionary framework) the chaotic profusion of plant data that had accumulated over the years. By the 1940s, the work of practical breeders and agriculturalists had made observable the curious behaviors of chromosomes, and the noncharacterizable mating habits of plants. These problematic phenomena became tractable once Stebbins envisioned them as *genetic systems* — apomixis, hybridization, and polyploidy — which themselves were subject to the mechanism of selection.[96] To plant taxonomists, moreover, the biological species concept as offered by Dobzhansky was one way of making tractable the long-held problem of species in plants. With open or indeterminate genetic systems, plants also had complex variation patterns, since genotypic and phenotypic responses were difficult to distinguish; as a result, a belief in Lamarckian or "soft" inheritance had been widespread in botanical circles. Natural selection, as a mechanism, helped dispel such speculative points of view and rid botany of the belief in Lamarckian inheritance. For Stebbins, therefore, the dialogue with Dobzhansky led to his account of *Variation and Evolution in Plants*.[97]

With the exception of embryology and physiology, for which the gene could not be reconciled with mechanical developmental

95. S. J. Gould's historical sense that for paleontologists the synthesis led to the rejection of directed evolution is also correct — for practicing paleontologists. See S. J. Gould, "G. G. Simpson, Paleontology, and the Modern Synthesis," in ibid., pp. 153—172.

96. Stebbins drew heavily on C. D. Darlington's *Recent Advances in Cytology* (Philadelphia: Blakiston's, 1932). Darlington subsequently rewrote his final chapter on genetic systems, which was highly controversial, and published it as *The Evolution of Genetic Systems* (Cambridge: Cambridge University Press, 1939).

97. There is much to Mayr and Provine's historical sense that botany was in some manner delayed in "entering the synthesis." Botany by the late 1940s, unlike systematics and paleontology, consisted of a much more heterogeneous assemblage of practices: taxonomy, morphology, genetics, ecology, paleobotany, etc. Stebbins, as practitioner in all these areas, had to bring into line a great deal more data: at 643 pages, his book was the longest and last of the synthesis classics. See V. B. Smocovitis, "Botany and the Evolutionary Synthesis: The Life and Work of G. Ledyard Stebbins," Ph.D. diss., Cornell University, 1988.

or physiological principles,[98] the Columbia Classics bound the heterogeneous assemblage of practices of closely neighboring disciplines of the biological sciences. These books in systematics, paleontology, and botany represented only a microcosm,[99] however, and an American microcosm at that,[100] of the work in evolutionary studies drawing on the mechanistic and materialistic frameworks that had been established by the mathematical modelers in conjunction with field biologists in the 1930s. By the time of the Classics' publication and dissemination, the critical moment for unification — in a Machian sense — had already taken place. The Columbia Classics, in responding to and citing each other, served to add to the sense of unity that had already started to emerge in the mid-1930s. Taken from the local audience of evolutionists — now linked in a complex reticulum — to the wider audience of scientists, Dobzhansky's evolutionary genetics would be used to bind further, situate, and sustain biology within the positivistic ordering of knowledge. The network that thus coalesced around evolutionary genetics, which had been grounded in genetics and ultimately in physics and chemistry and a mathematical logic, was to become even further extended, linking an even greater heterogeneous assemblage of practices. The publication of Julian Huxley's *Evolution: The Modern Synthesis* — which would take evolution to an even wider audience — signaled the unification of biology in a manner that also justified the unification of science.

EVOLUTION: THE MODERN SYNTHESIS

While Dobzhansky and his *Genetics and the Origin of Species* offered a mechanistic and materialistic framework[101] stretching from the gene to the human, which lent some measure of autonomy to biology, and which bound together representatives of biological disciplines through representative texts, the individual who did most to promote the newly emerging sense of unity in the

98. These two disciplines had stressed the transformational instead of the populational features of evolution. Both had been buffeted by the extremes of vitalism and mechanism.

99. See Mayr and Provine, *Evolutionary Synthesis*, for a full list of the historical actors and central texts.

100. It was no accident that these disciplines were represented. All had been heavily institutionalized in American museums, American herbaria, and American agriculture research stations.

101. This was not, however, a completely mechanistic and materialistic framework. This became apparent during the antireductionist debates in the early 1960s.

biological sciences and to extend this unity to the wider community, at the same time that he offered a framework to preserve the autonomy of biology, was Julian Huxley.[102] Huxley's role in the "evolutionary synthesis" has been misunderstood by historians of science. A voracious reader, an international traveler, and an indefatigable promoter of science, especially biological science, he was acutely aware of the criticisms made of evolutionary and biological practice. He was especially sensitive to these criticisms, since much of his life was devoted to leading a crusade, very much in the tradition of his grandfather Thomas Henry Huxley, to ground a humanistic philosophy in evolution. If one were to commit to a materialistic and mechanistic philosophical framework — as both Huxleys had — then constructing an ethical system, and constructing a meaningful existence for "Man," would have to come from some variant of *progressive* evolution. Julian Huxley's vision of such an "evolutionary humanism" was the central feature of his worldview and of his scientific endeavors.[103]

Belief in progress — an Enlightenment ideal — had been hard to sustain in the modern world, however, given the bloody aftermath of the First World War, the widespread sense of cultural degeneration, and the growing belief in the decline of the West. With the rise of collective movements like communism, fascism, and Nazism, and with the onset of the Great Depression, the drive to ground an ethical system within a progressive, optimistic, and coherent worldview that gave a measure of autonomy to the individual, intensified in the 1930s. For Huxley, a grounding in evolution and the construction of an evolutionary humanism became an imperative for the future of "modern man." From its inception, Huxley's major contribution to the growing literature on evolution, *Evolution: The Modern Synthesis*, was also to act as remedy for the ills of the modern world; avowedly progressive, liberal, and internationalist, it was Huxley's own ideological "testament of youth."

The decline in evolutionary studies had alarmed Huxley in part because it undermined his evolutionary humanism and his pro-

102. Huxley was also to promote unity in another sense: in 1942 he wrote a manuscript entitled "Unity in the U.S.A."; Papers of Julian Sorell Huxley, Box 65, The Fondren Library, Rice University, Houston, Tex. (hereafter cited as Huxley Papers). Political unity and global unity were central concerns for Huxley.

103. See the insightful recent article by John C. Greene: "The Interaction of Science and World View in Sir Julian Huxley's Evolutionary Biology," *J. Hist. Biol., 23* (1990), 39–55. I am in agreement with the specific features of Greene's interpretation, though I would not make the distinction between Huxley's science and his worldview.

gressive worldview. The purification of evolution, its experimentalization, and the establishment of biology's own fundamental principles had early been a central feature of Huxley's lifework. Not only had he been an active contributor to the literature of evolution, but he was also instrumental in helping to found the Society for Experimental Biology in 1925,[104] as well as collaborating with H. G. Wells on an ambitious project to lay the groundwork for *The Science of Life*.[105] By the mid-1930s Huxley had been even more actively reading and contributing to the literature in evolutionary studies (although his early training was in embryology) and had been in dialogue with his American colleagues Dobzhansky and Mayr.[106] In Britain, he was in close contact with other evolutionists like Haldane, C. D. Darlington, and C. H. Waddington, but was in even closer contact with Fisher and especially Ford. The other individual who inspired Huxley was Thomas Hunt Morgan, to whom he dedicated his book.[107] If any one person in the 1930s could summarize the modern evolutionary "state of the art," in palatable form for a *wide* audience of readers, that person was Julian Huxley.[108]

The opportunity to publish a synthetic work that would draw together the burgeoning literature in evolutionary studies within his evolutionary humanism came with the request to give the presidential address to the Zoology Section of the British Association in 1936. In the essay written for this occasion Huxley expressed his wish for a unified biology and his observation that a move for unification was taking place: "Biology at the present time is embarked upon a phase of synthesis after a period in which new disciplines were taken up in turn and worked out in comparative isolation. Nowhere is this movement towards unification more likely to be fruitful than in the many-sided topic of evolution; and already we are seeing its first fruits in the reanimation of Darwinism which is such a striking feature of post-war biology."[109]

104. For a history of the Society for Experimental Biology see M. A. Sleigh and J. F. Sutcliffe, "The Origins and History of the Society for Experimental Biology (Comprising The Origins of Society by Lancelot Hogben, F.R.S. Aspects of the History of the Society [1923—1966])," catalogued with the Huxley Papers.

105. See H. G. Wells, J. S. Huxley, and G. P. Huxley, *The Science of Life*, 2 vols. (Garden City, N.Y.: Doubleday, 1931).

106. Huxley made repeated visits to Mayr and discussed chapters of *Evolution: The Modern Synthesis* prior to publication.

107. The dedication reads: "Dedicated to T. H. Morgan: many-sided leader in biology's advance."

108. J. B. S. Haldane was another popular writer.

109. J. S. Huxley, "Natural Selection and Evolutionary Progress," Presidential Address at Annual Meeting, *Rep. Brit. Ass. Adv. Sci.* (1936).

This essay, entitled "Natural Selection and Evolutionary Progress," formed the basis for Huxley's 1942 book. From chapter 1, "The Theory of Natural Selection," which raised the status of natural selection to a theory (for Fisher, it will be recalled, natural selection was only a *genetical* theory), to the final chapter, "Evolutionary Progress," natural selection was used to ground a progressive vision of the world within Huxley's evolutionary humanism.

The evolutionary framework that Huxley adopted was strongly selectionist and bore the imprint of Fisherian evolution, even though he acknowledged the importance of random genetic drift and what he called the "Sewall Wright Phenomenon", and also acknowledged nonadaptive evolution. Huxley's treatment of the current state of evolutionary studies represented the diverse sets of data from international workers, paid attention to evolutionary phenomena in nonanimal systems, and took into account evolution at different levels (genic, chromosomal, individual, etc.). Hence the book appeared to consider all evolutionary points of view — yet the very structure of the book revealed Huxley's intent to ground a progressive evolution in natural selection.

Writing what would become the standard for disciplinary histories of modern evolution (and formulating the central problem for subsequent historians of biology), Huxley introduced his book by describing the woeful state of evolutionary studies that had accompanied the rise of Mendelism and the eclipse of Darwin. Chapter 1, which ostensibly was to discuss "the theory of natural selection," became a historical account of the philosophical and methodological struggle to lend scientific legitimacy to evolution. Singling out evolution as the "most central and most important of the problems of biology," Huxley urged his readers to "attack" this problem with "facts and methods" from every branch of the sciences. Biology, he recognized, was embarking on a "phase of synthesis" to bring biological and other scientific disciplines together. Aware of what he referred to (in a rather breezy manner) as the recent "movement towards unification,"[110] he

110. Huxley appears not to have been a formal member of the Unity of Science Movement. He had been in very close touch with Bertrand Russell, since at least 1919, and he drafted his lecture notes on "The Principles of Biology" at just the same time that he began to correspond with Russell. Parallel developments in Huxley's and Russell's view of scientific principles deserve close reconsideration. I have found no correspondence between Huxley and the Vienna Circle at this time. Huxley had in his possession an autographed copy of Charles Morris's 1956 book *Varieties of Human Values*. The copy was signed "With warm regards, Charles Morris," and we may infer that Huxley was on

further urged his readers to turn all of the scientific disciplines on this most central and most problematic "many-sided topic of evolution." Discussing the criticisms made of Darwinian methodology, and summarizing Darwin's argument for natural selection as based on "three observable facts of nature" and "two deductions from them," Huxley made it clear to the reader that natural selection — based on a deductive logical step, for Darwin — was now a "fact of nature capable of verification by observation and experiment." For Huxley, natural selection itself — through the work of evolutionists in the early 1930s — had become one of the fundamental *principles* of biology. With this fundamental principle, now capable of verification through observation and experiment, evolution and Darwinism were "reborn" like a "mutated phoenix risen from the ashes of the pyre." Natural selection would become an even firmer foundation for grounding Huxley's progressive evolution. The central problem of the biological sciences — evolution — was subject to the methodology of observation and experiment, and had become a more logical and positive science.[111]

Most important was the unified picture of biology that Huxley presented to his readers. For Huxley, the unification of biology (that great struggle for Woodger's generation) had begun to take place in the twenty-year interval preceding his own account; this "period of synthesis" had led to a science that could, in his mind, rival the unity and legitimacy of physics.[112] This "more unified science" had, in turn, made possible the rebirth of Darwinism. In a revealing passage, Huxley summarized the tale of the unification of biology and this rebirth of Darwinism:

> Biology in the last twenty years, after a period in which new disciplines were taken up in turn and worked out in comparative isolation, has become a more unified science. It has embarked upon a period of synthesis, until to-day it no longer presents the spectacle of a number of semi-independent and largely contradictory sub-sciences, but is coming to rival the unity of older sciences like physics, in which advance in any

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somewhat cordial terms with Charles Morris. One intermediary between Huxley and the Unity of Science Movement may have been Haldane: Huxley's copy of Charles Morris's *Varieties of Human Values* is catalogued with the Huxley Papers.

111. Huxley, *Evolution: The Modern Synthesis* (above, n. 8), pp. 13—28.

112. To biologists, physics and chemistry appeared to be unified sciences. How physicists and chemists perceived their disciplines is a separate issue.

> one branch leads almost at once to advance in all other fields, and theory and experiment march hand-in-hand. As one chief result, there has been a rebirth of Darwinism.[113]

While Huxley's words indicated that the unification of the biological sciences had taken place, giving biology a unity and methodological legitimacy, through theory and experiment, that could rival physics, Huxley was also to construct a framework that would make defensible the autonomy of this now unified biological science. It was through his version of progressive evolution that the delicate balance between unity and autonomy, mechanism and vitalism — the same fine line that Woodger, Haldane, and other biologists were to walk in the twentieth century — would be achieved. This was to come from the reborn Darwinism, progressive evolution by means of natural selection.

Given that Huxley endorsed a view of natural selection as a mechanistic principle, however, belief in evolutionary progress was exceedingly hard to sustain. If selection were strictly mechanical and nonteleological, then one could not ascribe purposiveness or directionality to evolution. There could be no evolutionary progress if there were no goal or endpoint for the process of evolution. Articulating as nonteleological a version of natural selection that could still somehow give direction and make possible progressive evolution, and at the same time adhering to selection as a mechanistic — and therefore legitimate — scientific principle, was the challenge that Huxley faced in the final chapter of *Evolution: The Modern Synthesis*.[114]

The manner in which Huxley articulated a progressive evolution grounded in natural selection was complex. By closely linking evolutionary progress with technological progress — humans were, after all, unique in their capability to modify their environment for their own purposes — he claimed that the human would in the same manner be able to control its *own* development, through conscious, willful use of its "Mind." Human improvement,

113. Huxley, *Evolution: The Modern Synthesis*, p. 26. A portion of this quotation echoes a similar passage in Neurath's work: "The new *Encyclopedia* so aims to integrate the scientific disciplines, so as to unify them, so as to dovetail them together, that advances in one will bring about advances in the others" (Otto Neurath, "Unified Science as Encyclopedic Integration," in *International Encyclopedia of Unified Science*, ed. Otto Neurath, Rudolph Carnap, and Charles Morris, vol. 1, no. 1 [Chicago: University of Chicago Press, 1938], p. 24).

114. See Provine, "Progress in Evolution" (above, n. 83), for a discussion of evolution, progress, and Julian Huxley.

and autonomy for the individual, were all combined in progressive evolution; and humans had been selected as the unique and "highest" of all organisms on earth.[115] Progress, in this form at least, could therefore make possible a meaningful life, but also preserved *enough* teleology to make biology an autonomous science.

The framework Huxley provided was to do one more thing: it would help stabilize an ideology. The last two pages of *Evolution: The Modern Synthesis* reveal the inextricably culturally embedded features of that framework. Echoing George Orwell and Eugène Ionesco, Huxley revealed his fear of the great collectives of the 1930s that threatened to lead to the "subordination of the individual," and his second fear of leading a life whose purpose would be fulfilled in "a supernatural world."[116] The struggle between these two opposing extremes was the struggle Huxley saw facing his modern world. To provide solutions to these global problems, at the same time that he resolved the "central" problem of evolution, was his hope in upholding a progressive view of evolution. With selection acting on the individual level, the individual could be "unique" at the same time that it existed in a social group. Neither totally mechanistic/materialistic (hence avoiding the politically extreme left wing of atheists and communists), nor too vitalistic/spiritual/mystical (hence avoiding the extreme right wing of fascists, Nazis, and religious fundamentalists), Huxley's evolutionary framework balanced just enough mechanism and materialism with purpose and progress that would sustain and justify the moderate and liberal ideology. The threat to evolution that Huxley attributed (at the end of his first chapter) to the extremes of Henri Bergson, the ultravitalistic metaphysician, and William Bateson, the ultramechanistic materialist, was thus to be neutralized by a mechanistic yet purposive view of evolution.[117] With the end of

115. This position is summarized in a manuscript of 1949 entitled "Evolutionary Humanism," Huxley Papers, Box 67.8.

116. Concern with the metaphysical features of life had been apparent in Huxley's early work; see Huxley, *Stream of Life* (above, n. 35).

117. See Grogin, *Bergsonian Controversy* (above, n. 35), for an account of the "revolt from mechanism." Bateson's well-known book of 1894 with the title *Materials for the Study of Variation* is an indicator of the frustration that Bateson had encountered when he could not reconcile the causo-mechanical agent of selection with any material basis for variation. His book was meant to instruct workers to search for the material basis for variation. His excitement with the "rediscovery of Mendel" was due to his seeing the material basis for variation aligned with a mechanism for evolutionary change that behaved in "law-like" fashion. Bateson, like his contemporary Hugo de Vries, had been working with plant material and focusing on the species — not clearly defined — as the unit of

World War II imminent, Huxley and his purposive, selectionist framework would in turn sustain an increasingly moderate, popular liberal ideology with a view of an independent biological science.

Loaded with a political and ethical perspective couched in terms of the recent developments of science, Huxley's book was not favorably received by the most local audience of evolutionists. Unlike Dobzhansky's *Genetics and the Origin of Species*, Huxley's book lacked technical detail and originality, and it appeared somewhat disorganized. Unlike the Columbia Classics, it did not serve to bind together the heterogeneous practices of the biological sciences; rather, with its grounding in selection and its argument for a unified, autonomous science of biology, it helped to take this "modern" evolution to the wider audience of scientists — physical, biological, and social — in the 1940s and 1950s. For this wider audience, Huxley was to use the unified biological sciences, now "modernized" and rivaling both physics and chemistry, to help extend and legitimate biology. Most important, with its emphasis on evolutionary progress, Huxley's book offered an inquiry — similar to his grandfather's[118] — into an ethical system, an ethos, grounded in evolution, now a legitimate science, with its fundamental principle of natural selection, verifiable and testable through observation and experiment. Evolution was portrayed as a science as mechanistic and materialistic as possible, yet preserving enough emergentism to lend autonomy to biology and the human, and at the same time to generate a measure of faith and goodness to a world grown weary of global disturbances.

As the horrors of the Holocaust, the Cold War, and the nuclear nightmare were made more apparent, the belief in selection as Huxley and others were articulating it — offering a sense of progress, a liberal ideology, and an optimistic and coherent worldview with humans as the agents of their own evolution — intensified yet further. For evolutionists living within such a world, only through some form of evolutionary humanism would human "improve-

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evolution. Hence both men were to uphold strongly saltationist points of view, given the model "planty" organisms they had adopted. Such saltationist points of view at the turn of the century were enormously popular with geneticists, many of whom had been converted from practical plant breeders housed in horticultural and agricultural institutions. Bateson's initial address on genetics, as well as his announcement of the rediscovery of Mendel, it will be recalled, was to an audience of the Royal Horticultural Society. Bateson — especially as discipline builder of genetics — deserves close reconsideration by historians of science.

118. While the two Huxleys shared certain fundamental assumptions about evolution and ethics, their formulated ethical systems were very different.

ment"[119] be thought possible. Such a view would also help account for, justify, and accelerate the unsurpassed success and inexorable progress of atomic-age, and then space-age, technology.[120] By the early 1960s — the same historical moment characterized as the most prosperous and optimistic in recent American history — the belief in selection culminated in the most extreme *pan*-selectionist doctrines. What has come to be called the "hardening" of evolution around a strongly selectionist framework took place at this time.[121]

Nor was Huxley the only evolutionist to uphold a view of evolutionary progress in the 1940s. Dobzhansky, Mayr, Simpson, and Stebbins all came to subscribe to versions of evolutionary progress at the same time that they made natural selection a mechanism.[122] Each of these evolutionists in some measure addressed the "future of Mankind," either in concluding chapters of their early books, or in more popular books they were to write in later stages of their careers.[123] The belief in the continuum between the gene and the human brought these humanistic concerns within the materialistic and mechanistic frameworks of genetics.[124]

119. The word "eugenics" was purged from biologists' vocabularies after the horrors of the Holocaust and Nazi medicine were made apparent.

120. Especially in the U.S. The sense of easy progress and optimism that characterized postwar American culture was not mirrored by the war-torn continent. This accounts for the view that the evolutionary synthesis was primarily an American (to some extent, an Anglo-American) phenomenon. The shift in evolutionary studies from Europe to the U.S. after the war was also reinforced by the founding of the Society for the Study of Evolution in the U.S. Huxley had organized a British society that stressed systematics, but this organization was not as broad in its scope as the SSE.

121. See S. J. Gould, "The Hardening of the Modern Synthesis," in *Dimensions of Darwinism*, ed. Marjorie Grene (Cambridge: Cambridge University Press, 1983), pp. 71–93.

122. See Nitecki, *Evolutionary Progress* (above, n. 83), for an indication of how contentious the subject of evolution and progress has been.

123. See, for instance, Th. Dobzhansky, *Mankind Evolving: The Evolution of the Human Species* (New Haven: Yale University Press, 1962), and *The Biology of Ultimate Concern* (New York: World Publishing, 1967); see also G. G. Simpson, *The Meaning of Evolution* (above, n. 88) and *This View of Life: The World of an Evolutionist* (New York: Harcourt Brace Jovanovich, 1964). Simpson's closing thoughts in *The Meaning of Evolution* echo Huxley's evolutionary humanism: "It is another unique quality of man that he, for the first time in the history of life, has increasing power to choose his course and to influence his own future evolution. It would be rash, indeed, to attempt to predict his choice. The possibility of choice can be shown to exist. This makes rational the hope that choice may sometime lead to what is good and right for man. Responsibility for defining and for seeking that end belongs to all of us" (p. 348).

124. Punctuated equilibrium and the critique of the adaptationist program,

Evolutionary progress through the mechanism of evolution, though it appeared to be a contradiction, struck just the right balance between purpose, progress, and mechanistic materialism — it was deterministic enough to be predictable, yet not enough to remove free will — for a wide audience of evolutionists, who took their views through semipopular works and essays to the wider audience of scientists.[125]

For practitioners of biology who sought legitimacy and wished to avoid engulfment from the physical sciences, moreover, evolution made an autonomous science of life defensible. Evolution as a mechanistic and materialistic science preserved enough teleological components through evolutionary progress, and hence contained enough emergent features, to give the science of life independence from the physical sciences. Natural selection — the primary mechanism — had just the right measure of mechanical *and* teleological: biology was preserved as an autonomous science, but a science that could still rival physics and chemistry through observation and experiment.

The middle ground that Woodger and others had sought was thus found, and the principle of great "unifying power," the "generalization" that would "knit" the "several branches" into a whole of biology, now existed — problematic because it contained some metaphysical elements, but just enough metaphysical properties to lend autonomy to biology. Experimental and quantitative in its own right, what was emerging as the science of evolutionary biology would serve to unify and bind the heterogeneous practices of the biological sciences at the same time that it would "lift" biology from reduction to the physical sciences. Evolution would become the "central science" of biology.

But for the wider audience, evolution by means of natural selection, as Huxley had been promoting it, was to *become a fact.*[126] The publication of Huxley's book — along with his numerous essays, speeches, reviews, and even encyclopedia entries —

launched by S. J. Gould and R. C. Lewontin, was to construct an argument that would lead to a sundering of this continuum; see S. J. Gould and R. C. Lewontin, "The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme," *Proc. Roy. Soc. London*, ser. B, *205* (1979), 581—598. For a response to the Gould and Lewontin argument see Ernst Mayr, "How to Carry Out the Adaptationist Program?" *Amer. Nat., 121* (1983), 324—334.

125. Another example of this balance between evolutionary progress, purpose, and mechanistic materialism in semipopular form is the published book of Dobzhansky's 1961 Silliman Lectures, *Mankind Evolving* (above, n. 123).

126. For Darwin's readers only the facticity of evolution had been established. The causo-mechanical agent had not been demonstrated.

was to connect and cross-link the network that had coalesced around Dobzhansky, with Fisher and Ford, and in turn with the wider audience of scientists. As the war ended in 1945 and communication networks, extensively developed through wartime, were redirected to peacetime operations, and as the postwar optimism and accumulation of economic and material resources (in the United States) were redirected toward reconstructing and stabilizing the global order, the cross-linking of the networks of scientific practice accelerated further. A compound-complex reticulum connecting the disciplines of the sciences had emerged. The heterogeneity of not only biology but science[127] would now be bound by the central science of biology, reducible to the level of the gene, and obeying the axiomatic Hardy-Weinberg Equilibrium Principle.[128] Biology, occupying the midpoint in this continuum from the physical world of the gene to the social and cultural, was the domain that would provide the most information and hold the key to the future of "modern man" — the *object* of study of both the biological and social sciences.[129]

The Enlightenment ideals of the proper systematic study of "Man," culminating in evolutionary humanism, liberalism, progress, and the unity of science and of all knowledge, would hold sway by the early 1950s. Knowledge would be unified by reduction to the physical sciences, while the diversity and variety of knowledge would emerge from the social sciences above. Evolution, partly reducible *to* the physical world, but also emergent *from* the physical world, would lead ultimately to the *progressive divergence of knowledge*; and the ever-branching, ever-ramifying "tree of life" was to map one-to-one with the ever-branching, ever-ramifying "tree of knowledge."

The "Unity of Knowledge" was the name of a conference with the central theme of "Man's Right to Knowledge and the Free Use Thereof," held in honor of the bicentennial of Columbia University in 1954.[130] The list of contributors to and participants in this

127. The heterogeneity of science had been Carnap's lament.

128. For this reason the sense arose that mathematical population genetics forms the "core" of evolutionary theory.

129. Once again accounting for the nature-nurture debates that flared after this time.

130. The conference took place at Arden House, Harriman, N.Y., on October 27—30, 1954. Members of the planning committee included Albert Hofstadter, Paul Lang, Ernest Nagel, Marjorie Hope Nicolson, I. I. Rabi, and Lionel Trilling. The panels for the conference were organized and managed by Horace Friess, Ernest Nagel, and Jacques Barzun. Also helping with the general sessions were John A. Krout, Henry P. Van Dusen, Edgar Grim Miller, and Philip C. Jessup. For details of the conference and the edited proceedings, see Lewis Leary, ed, *The Unity of Knowledge* (New York: Doubleday, 1955).

conference included luminaries from all the existing disciplines of knowledge: Pierre Teilhard de Chardin, Theodosius Dobzhansky, Julian Huxley, Talcott Parsons, B. F. Skinner, Harold Urey, Niels Bohr, John Von Neumann, Willard Van Orman Quine, Ernest Nagel, and Philipp Frank.[131] The compound-complex reticulum, a polysemous web, linked the metaphysician with the mathematician — the physicist, the chemist, the biologist, the psychologist, and the sociologist with the philosophers and logicians of the Vienna Circle.[132] All were enveloped by the same Enlightenment ideals writ large: progress, unity, and diversity, and a liberal, evolutionary humanism.[133] The title of Huxley's contributed paper, which articulated an evolutionary humanism through evolution by means of natural selection, reveals Huxley's own role in the modern synthesis: the *determination* of "Man's Place and Role In Nature."[134]

While this larger, global network was striving to reach consensus for the unification of knowledge, the more local network that had coalesced around evolutionary genetics had been secured by becoming officialized and institutionalized as the central component of the biological sciences. Becoming redefined as evolutionary biologists, the life-long task of these scientists would be to balance the positivistic ordering of knowledge from deep within the biological sciences. Disciplining evolutionary biology — the *fulcrum* of the biological sciences — they were to act as unifiers, negotiators of the location of biology, preserving the whole of the positivistic ordering of knowledge.

131. See the Leary volume for a complete list of attendees.

132. Philipp Frank, it will be recalled, had been a promulgator of the Machian position that the unification of the sciences had to take place through the ejection of metaphysics.

133. The driving force behind these Enlightenment values, it will be recalled, was the Newtonian mechanistic picture, which preserved enough metaphysics for a meaningful life devoid of complete determinism. The unification of knowledge — which brought "Man" into the deterministic fold of mechanistic and materialistic genetics — was therefore the culmination of Enlightenment thought. One feature that emerged from the "Unity of Knowledge" conference was the need to exert caution with respect to political unity. This was one way to avoid the twin specters of communism and fascism. Leary's introduction also suggests that conference attendees were to modify their originally "bold concept of a unity of knowledge" to a more "manageable" and "semantically more sound" concept of the unification of knowledge; See Leary, "Preface," in *Unity of Knowledge*, pp. xi.

134. Dobzhansky was to make "Man" the "Centre of the Universe" in the final section to *Mankind Evolving* (above, n. 123). In this final section, he was to make explicit his warm regard for Teilhard de Chardin; see P. Teilhard de Chardin, *Le phénomène humain* (Paris: Du Seuil, 1955); English trans., *The Phenomenon of Man* (New York: Harper and Row, 1959).

DISCIPLINING EVOLUTIONARY BIOLOGY

The local network around Dobzhansky's evolutionary genetics had coalesced rapidly — in fact, a consensus that a common ground existed arose within a few years of Dobzhansky's initial work on the genetics of natural populations. At the same moment that evolutionary studies reached their most endangered point, the new experimental and mechanistic and materialistic evolutionary practices promised to help preserve evolutionary studies. Along with the consensus that there was a common ground there had simultaneously come a consensus that the evolutionary practices that resolved problems of speciation should be secured and sustained by being institutionalized. In 1939 at the American Association for the Advancement of Science (AAAS) meetings in Columbus, Ohio, Julian Huxley met with Dobzhansky, Mayr, and Carl Epling to suggest the formation of an official Society for the Study of Speciation, which would function as an information service.[135] At the same time, Huxley was also to bring together a wide group of practitioners interested in problems of speciation in Britain under the rubric of the "new systematics."[136]

While a Society for the Study of Speciation was officially founded in the United States in 1940 under the secretaryship of A. E. Emerson, the outbreak of the war interrupted the initial impetus and thwarted further plans. Support for an informal communication service between evolutionists continued, however. On the West Coast the "Biosystematists," a San Francisco Bay—area informal cooperative organization, was to continue to support evolutionary studies; but it was the collaboration between New York—based paleontologists, systematists, and geneticists that would exert pressure to institutionalize evolutionary studies. In the early 1940s this latter group, with the initiative of Walter Bucher at Columbia University, made additional moves to launch a "synthetic attack" on the "common problems of evolution" by forming a cooperative and coordinated organization.[137] These plans were put into effect in 1943 when the Committee on Common Problems of Genetics, Paleontology, and Systematics

135. "The Society for the Study of Speciation," Reviews and Comments, *Amer. Nat.*, *75*: 756 (1941), 87—89.

136. See Julian Huxley, ed., *The New Systematics* (Oxford: Clarendon Press, 1940).

137. See the historical "Foreword" to the edited volume of the proceedings of the Princeton conference written by Glenn L. Jepsen, in *Genetics, Paleontology, and Evolution*, ed. Glenn L. Jepsen, Ernst Mayr, and George Gaylord Simpson (1949; repr. New York: Atheneum, 1963).

was established under the auspices of the National Research Council.[138] During the difficult war years, communication between evolutionists took place through a series of mimeographed bulletins edited by Ernst Mayr in which these "common problems" were discussed by the local network of evolutionary practitioners.[139] It was through these communication bulletins — written in a series of letters or rapid exchanges between members of the local group — that a consensus emerged that there was in fact a common ground and a common *field* that should be officialized.[140] Returning from military service abroad, Simpson identified the emergence of this common field in the final mimeographed bulletin of 1944:

> This series of bulletins, compiled and edited by Dr. Mayr who continues this task, has accomplished a great deal more than the expression of a few facts and opinions, useful as these have also been. From the whole series of letters in the bulletin there has emerged concrete evidence that a field common to the disciplines of genetics, paleontology, and systematics does really exist and this field is beginning to be clearly defined. Some, at least, of the more hopeful approaches to these common problems are indicated and exemplified. The existence of geneticists, paleontologists, and systematists interested in these problems and competent to attack them has been demonstrated. Their interest has been stimulated and made more concrete and their competence in the joint field has been increased by the exchange of views with students of other specialties. Thus great progress toward the goal of the committee has been made.[141]

By this time, too, the Columbia Classics' representing of the heterogeneous practices of biology was reaching the wider audi-

138. This was a joint or interdivisional committee organized by the Division of Geology and Geography and the Division of Biology and Agriculture of he National Research Council.

139. I have consulted the set of mimeographed volumes in the holdings of the Provine evolution collection in Marathon, N.Y., as well as an unpublished manuscript by Ernst Mayr, "History of the Society for the Study of Evolution" (dated most likely around 1947), included with the bundle of mimeographed bulletins.

140. The dialogical format of these bulletins facilitated the construction of a disciplinary discourse.

141. Introductory remarks by G. G. Simpson, Bulletin no. 4, November 13, 1944.

ence to garner further support and belief in the emergence of a now common field of practice. But it was only after the war that major moves could be made to redirect available resources to peacetime operations, such as the planning of major conferences and the creation of new societies. With the end of the war, what had been the defunct Society for the Study of Speciation, with the help of the Committee on Common Problems in Genetics, Paleontology, and Systematics, was re-created under the initiative of Ernst Mayr: on March 30, 1946, at St. Louis, Missouri, fifty-eight attendees — the "founding fathers"[142] — signed a document, entering a confederacy under the title of the Society for the Study of Evolution (SSE).[143] Within the year, the first annual meeting was held in Boston (December 28—31), at which time the members of the society felt themselves to be sufficiently established to begin a journal, *Evolution*, with Ernst Mayr as editor, which would promote the new experimental practices rather than just descriptive or taxonomic practices of evolution;[144] funds had come from a "Post-War Expenditure Fund" granted through the American Philosophical Society. At this meeting a constitution was drafted that would fix and sustain the common goals of the society.

Within a year of the drafting of the constitution, the society held its first major meeting at Princeton, during which it was agreed that "a convergence of evolutionary disciplines"[145] had taken place.[146] Writing the summation for the edited volume of the proceedings, H. J. Muller drew the parallel between an evolutionary convergence of types and the convergence between disciplinary types like geneticists and paleontologists. The end result of this fusion was a new and higher type, through a process of synthesis: the synthetic type of evolutionist. Muller's summation, which outlined the consensus in evolutionary practice and reprob-

142. Ruth Patrick was the only female signatory.

143. Alfred Emerson presided at this meeting. The first president of the SSE was G. G. Simpson, with Ernst Mayr as secretary.

144. Ernst Mayr was instrumental in raising support for the journal, as well as being a key, if not *the* key, player in the founding of the SSE. Simpson played an important role in assisting Mayr to obtain start-up funds.

145. This was H. J. Muller's subtitle in the "Summation" to the Princeton volume; see H. J. Muller, "Redintegration of the Symposium on Genetics, Paleontology, and Evolution," in Jepsen et al., *Genetics* (above, n. 137), pp. 421—445.

146. Both Ernst Mayr and G. Ledyard Stebbins have the sense that evolutionary biology emerged at about the same time as the Princeton meetings (letter from Ernst Mayr to author, August 15, 1989; letter from G. L. Stebbins to author, May 4, 1989).

lematized the now common field, indicated a shift in what was emerging as the evolutionary disciplinary problematic. The points of agreement included the primacy of natural selection as a mechanism of evolutionary change, the gradual rate of change operating at the level of small, individual differences, and the continuum between microevolution and macroevolution — reconstituting what Muller viewed as the original Darwinism torn apart by "over-zealous "mutationists'" in the geneticists' camp and paleontologists who would embrace Lamarckian inheritance and an "inner evolutionary urge." Both these disciplines now had a "common ground of theory."[147] Though his closing thoughts indicated an awareness that a mechanistic and materialistic view of evolution was nonpurposive and nonprogressive and did not bode well for the future of all species, Muller echoed Huxley in his belief that humans could somehow rise above their own evolutionary destiny: "If, then, we wish evolution to proceed in ways that we consider progressive, we ourselves must become the agents that make it do so. And all our studies of evolution must finally converge in that direction."[148]

While there appeared to be agreement on the common problems and common practices leading to a common ground, there was also dissent as well. One individual, in particular, threatened to upset the emerging consensus from within the evolutionary ranks. His name was Richard Goldschmidt. Though he was an insider in the emerging field, he was an outsider as well.[149] For Goldschmidt, who was concerned with development, the proper unit of evolution was not the gene, which served a structural role, but the protein, which served a regulative role. In his opinion, genetics without biochemistry — what he called physiological genetics[150] — could not account for the developmental features of evolution. In upholding the protein as the unit of evolution instead of the gene, Goldschmidt was challenging one of the central features of Dobzhansky's framework. But he was challenging more than just this: in espousing genetic "position effects," which could lead to rapid saltatory events (producing his famous "hope-

147. But it would be the interlocking of the practices that bound the biological sciences.

148. Muller, "Redintegration," p. 445.

149. Scott Gilbert has offered an account of Goldschmidt as "outsider": see "Cellular Politics: Ernest Everett Just, Richard B. Goldschmidt, and the Attempt to Reconcile Embryology and Genetics," in Rainger et al., *American Development of Biology* (above, n. 2), pp. 311–364.

150. The title of his 1938 book: R. Goldschmidt, *Physiological Genetics* (New York: McGraw Hill, 1938).

ful monsters"), he was counteracting the efforts made to limit the rate of change to the level of small, individual differences, worked out at the level of point mutations. Furthermore, by espousing such unpredictable mechanisms of evolutionary change, leading to large-scale effects that could give rise to higher-order phenomena, Goldschmidt was also challenging the continuum between microevolution and macroevolution. In postulating different mechanisms operating at different levels of evolution he impugned not only the emerging evolutionary consensus, but the very ground on which it was being built — the classical genetics of the Morgan school. The title of Goldschmidt's 1940 book, *The Material Basis of Evolution*,[151] was a direct response to Morgan and implied that Morgan's 1932 book, *The Scientific Basis of Evolution*, was not materialistic enough!

For Dobzhansky, and for Mayr and others of the emerging new field, Goldschmidt posed a series of ultimate challenges that threatened to upset the delicate balance that Dobzhansky's framework had struck. The protein as the evolutionary unit instead of the gene made Goldschmidt's evolution too materialistic; position effects and saltatory changes made evolution too indeterministic; and postulating different mechanisms at work within microevolution and macroevolution threatened to unwind and disunify the biological sciences from within. For these reasons, controversy to the point of acrimony erupted between Goldschmidt and Dobzhansky, whose framework was challenged, and Mayr, who most promoted the view of biology as autonomous science. Goldschmidt's closing thoughts in his 1940 book hit an especially vulnerable spot in the delicate balance that the architects had worked so hard to strike:

> The following period of experimental biology was skeptical of, if not actually hostile to, evolution, as it could not be attacked in laboratory experimentation. Mechanism became unpopular and vitalistic and teleological trends invaded evolutionary thought in the form of creative evolution, emergent evolution, psycho-Lamarckism. The rise of genetics brought back a mechanistic attitude; evolution started to become an exact science. Just as there is no room for transcendental principles in experimental physics and chemistry, in the same way a factual attack upon the problems of evolution can work only

151. Richard Goldschmidt, *The Material Basis of Evolution* (1940; repr. New Haven: Yale University Press, 1982).

> with simple mechanistic principles. Genetics showed the evolutionists that evolution can be attacked scientifically only on the basis of known analyzable processes, which are by their very nature relatively simple. But, just as has been the case in chemistry and physics, mechanistic analysis of evolution will sooner or later reach a point where an interpretation in terms of known processes will meet with difficulties. In such a situation chemistry and physics have never invoked transcendental principles on the assumption that nature is so frightfully complicated that it cannot be understood otherwise. The actual developments have shown that this is not the case. The modern development of the electronic theory has shown that rather simple principles govern the most complicated phenomena of matter. Of course, there is always an unexplained residue on which the investigator may train his personal metaphysical predilection, but certainly no chemist would look to metaphysics for an explanation of a difficult phenomenon, say catalysis. In the same way the evolutionist, who meets with difficulties in mechanical interpretation at a lower level, may enjoy letting loose his metaphysical yearnings. But as an investigator he can only work under the assumption that a solution in terms of known laws of nature is possible.[152]

For Goldschmidt, evolution as the architects were constructing it was therefore *too* metaphysical and not materialistic *enough*. In pointing this out, he threatened to destabilize the fine line that the architects were trying to walk between the unity of the science and the autonomy of biology: the end result for biology, as he envisioned it, would be engulfment by the physical sciences. But the dialogue with Goldschmidt was also to have a securing effect in that it helped articulate and refine what had emerged as the disciplinary problematic of evolutionary biology. In disciplining the study of evolution — through the determination of who and what counted as "outside" — the "inside" members of the society would also negotiate the disciplinary standards and reconstruct the disciplinary problematic of evolutionary biology. Goldschmidt was eventually to be "marginalized," though he would be resurrected as a "heretic" and an antihero by the next generation of evolutionists[153] — but well before then, evolution and biology

152. Ibid., p. 397.

153. It was Stephen J. Gould and others who were to portray Goldschmidt as a "heretic" and an antihero. Gould introduced the reissue of Goldschmidt's book for Yale University Press in 1982. For Gould, who inherited problems of

would be fixed and sustained and secured by becoming legitimate disciplines of knowledge.

By the late 1940s, then, the legitimation of evolutionary studies, the rebirth of Darwinism, and the emergence of an experimental biological science of evolution — evolutionary biology — had been instituted officially by the formation of a recognized scientific society, the Society for the Study of Evolution, and a literary forum for expressing concerns, the journal *Evolution*. Simultaneously, a realignment of biological disciplines began to take place as the group of biologists finding a "common ground" of genetics and selection theory redefined their disciplinary identities as evolutionary biologists. Also at this time, the first umbrella-like organization for the biological sciences, the American Institute of Biological Sciences (AIBS), was formed. While both the SSE and the AIBS had benefited from postwar optimism and the boom in available resources, the two societies were to be closely linked in deeper ways, since evolution would form the central science of the unified biological sciences.[154] Never reaching departmental status, nor having any one tie to any conventional research institution, nor even meeting any economic or service-related activity, evolutionary biology as legitimate science would emerge and be sustained because of its unifying properties, which made biology an independent yet unified science within the positivist ordering of knowledge.[155] The unifying

development as well as problems in accounting for rates of evolutionary change as made apparent in the fossil record, the position that Goldschmidt represented closely resembled his own. The sundering of the continuum between microevolution and macroevolution in the late 1970s and early 1980s also led to the sundering of the continuum that gave rise to sociobiology. Hence, the major amendments to evolutionary theory in the early 1980s and the emergence of paleobiology were also to sustain what (in the 1980s) was a politically moderate position. For Gould et al. the autonomy of biology and the role of evolution as the "central organizing principle" would come from an argument they inherited from Simpson — chance and contingency in the form of unique historical events. For a recent articulation see S. J. Gould, *Wonderful Life: The Burgess Shale and the Nature of History* (New York: W. W. Norton, 1989).

154. Initially, members of the SSE balked at officially enrolling their society within the larger category of the AIBS. This resistance was in part due to financial concerns, but also to the fact that the AIBS included experimental biologists who had been denigrating the descriptive and nonexperimental sciences. The members of the SSE had much closer ties to the AAAS, to which a large number belonged. The initial move to found what eventually would become the SSE, it should be recalled, took place at the 1939 AAAS meetings. The closer tie to the AAAS justifies the argument that evolutionary biology was supported by the wider audience of scientists.

155. At present there are departments with joint appellations such as

principle that Woodger had sought now existed as a legitimate science.

No less an authority than Julian Huxley himself was self-consciously aware that a new science — and a central one at that — had emerged in the second quarter of the twentieth century. Borrowing a term from the title of a book by Arthur Dendy, *Outlines of Evolutionary Biology*,[156] Huxley employed "evolutionary biology" as a substitute for "evolutionary studies" in his *Evolution: The Modern Synthesis*.[157] After the end of the war, and with the formation of the Society for the Study of Evolution, the phrase "evolutionary biology" became an accepted disciplinary appellation.[158] In an address of 1949 Huxley told the following tale of evolutionary biology:

"Ecology and Evolutionary Biology" at American universities, and there are numerous centers and programs, but there are no (and have been no) exclusive departments of evolution or evolutionary biology in the U.S.; practitioners of evolution reside and have resided in no one locale and include settings as diverse as universities, museums, and agriculture research institutes. Evolutionary biology is unlike any of the other disciplines examined by students of science studies. For some of the literature examining the emergence of disciplines see Robert Marc Friedman, *Appropriating the Weather: Wilhelm Bjerknes and the Construction of a Modern Meteorology* (Ithaca, N.Y.: Cornell University Press, 1989); David Edge and M. Mulkay, *Astronomy Transformed: The Emergence of Radio Astronomy in Britain* (New York: Wiley, 1976); Robert E. Kohler, *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline* (Cambridge: Cambridge University Press, 1982); Thomas Söderquist, *The Ecologists: From Merry Naturalists to Saviours of the Nation* (Stockholm: Almqvist and Wiksell International, 1986); Gerard Lemaine et al., eds., *Perspectives of the Emergence of Scientific Disciplines* (The Hague: Mouton, 1976).

156. Dendy, *Outlines of Evolutionary Biology* (above, n. 32). The phrase "evolutionary biology" first appears in a passage from naturalist Grant Allen's *Vignettes from Nature*: "and it is these self-same odd, overgrown outer flowers which make the guelder rose so interesting a plant in the eyes of the evolutionary biologist" (Grant Allen, *Vignettes from Nature* [London: Chatto, Winding, and Picadilly, 1881], p. 93, S. J. Gould provided Allen's citation.

157. In telling the history of what would be the new discipline, Huxley used the phrase "evolutionary studies" — though not strictly in a disciplinary sense — in chap. 1 of *Evolution: The Modern Synthesis* (see p. 23); on p. 31 he explicitly used the phrase "evolutionary biology" in a disciplinary sense. He had been citing Dendy's book in personal notes in the 1930s, and had been actively using the phrase "evolutionary biology" in a disciplinary sense in his publications in the 1920s. English biologists may have adopted his disciplinary sense of the phrase much earlier than their American counterparts. In 1938 Gavin de Beer edited a volume with the title *Essays on Aspects of Evolutionary Biology Presented to E. S. Goodrich.*

158. Both Mayr and Stebbins agree that the phrase gained widespread acceptance shortly after the formation of the SSE. Mayr explicitly used the phrase in a disciplinary sense in a letter to John Aldrich dated August 6, 1947

> One of the outstanding events in scientific history has been the emergence, during the second quarter of the present century, of evolutionary biology as a science in its own right. In the phase that followed on Darwin's *Origin of Species* our scientific forebears spoke of the evolution theory, much as in the phase that followed Pasteur, they spoke of the germ-theory of disease. But by the early 20th century, the germ-theory of disease had become swallowed by the science of germs — bacteriology and microbiology — to which it has given rise. In a rather similar way, the evolution theory has today been swallowed in the science of evolution — evolutionary biology. The difference is that, while microbiology is a departmental branch of science, involving a certain definable field, evolutionary biology is a central science, with ideas demarcating all other branches of the life-sciences.
>
> This, you may say is by now a commonplace. However, I do not consider that all the implications of evolutionary biology have been grasped. They have not been fully grasped in the branches of biology: and they have hardly been grasped at all in relation to science as a whole, from physics on the one hand to psychology and the human and social sciences on the other.[159]

Though he was self-aware — indeed, played an important role in the emergence of evolutionary biology — Huxley underestimated (in the above passage at least) the sensitivity of his wide audience of readers and the eagerness with which they had sought to unify all of the sciences within a coherent worldview. Physicists (who themselves had set the standards for the quest for grand unified cosmic theories), chemists, astronomers, and social scientists did in fact rapidly adopt versions of the progressive evolutionary framework that drew on evolution by means of natural selection.[160] The end result would be the development of an evolutionary *cosmology* — with cosmological change demonstrat-

(Ernst Mayr Papers, Library of the American Philosophical Society, Philadelphia). Evolutionary biology was also included in the subtitle of the journal *Evolution*, which was initially to be called *Evolution: An International Journal of Evolutionary Biology*. In the minutes of second annual meeting of 1947, this title was amended to *Evolution: An International Journal of Organic Evolution.* I could find no reason for this change in title in any of the documents I examined in the archives of the American Philosophical Society.

159. "Evolution and Scientific Reality," manuscript dated 1949, Huxley Papers (above, n. 102), Box 67.7.

160. I thank Max Dresden and Pierre Noyes at the Stanford Linear Accelerator for sharing their historical perspectives of the development of physics in

ing the same law-like regularities with clearly defined mechanical causes of change and material entities on which they acted.[161] Cosmic, stellar, planetary, chemical, and organic evolution emerged as a continuum in a "unified" evolutionary cosmology by the 1950s.[162] But the ultimate proof of the unified biological science and its secure location would come from philosophers who reexamined the foundations of biology in the 1950s.

Writing within the vehicle founded by the Unity of Science Movement to encompass all of knowledge, *Foundations of the Unity of Science*, Felix Mainx, who can be viewed as occupying the philosophical niche formerly occupied by Woodger, had a perspective on the status of the twentieth-century biological sciences vastly different from Woodger's. Rather than hurling criticism after criticism at a disunified and fragmented infant science, Mainx could instead tell the tale of the science of biology, empirical, grounded, and unified. Though the central problematic of biology, evolution, was by no means completely understood,

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the interwar and postwar periods. Both had closely followed developments in evolutionary biology. Both had closely read *Evolution: The Modern Synthesis* and other essays by Huxley, as well as the semipopular works of the Haldanes. Another physical scientist (trained in mathematics) who also drew heavily on Huxley's framework was Jacob Bronowski. Huxley and Bronowski collaborated on writing volumes of the Macdonald Illustrated Library in the years 1963—65. Bronowski's televised *Ascent of Man* series echoed Huxley's evolutionary humanism. Adopting both Huxley's and Bronowski's evolutionary humanism, Carl Sagan televised this evolutionary philosophy through his series *Cosmos*. Episode 2 of *Cosmos*, entitled "One Voice in the Cosmic Fugue," which laid the groundwork for the series (episode 1 was an introductory synopsis of the 13-part series) introduced evolution by means of natural selection. Sagan demonstrated the efficacy of natural selection through the example of the Heike crab — an example used also by Huxley in his essay "Life's Improbable Likenesses," in *New Bottles for New Wine* (New York: Harper and Row, 1957), pp. 137—154. The example of the Heike crab had been a favorite with H. J. Muller, and both Huxley and Sagan had close ties to Muller. In the early 1980s, Huxley's evolutionary humanism — arguably not significantly altered — was therefore to be transmitted through the latest technology to one of the largest popular audiences of all time: 16 million viewers.

161. One of the strongest supporters of the study of evolution was Harlow Shapley. It was with Shapley's backing that Mayr and Simpson were able to obtain start-up funds from the American Philosophical Society to found their society — while developmental biologists and physiologists like E. G. Conklin vetoed proposals to establish an official society for the study of evolution.

162. With the "discovery" of Precambrian microfossils in 1954 through the imaging technology of the electron microscope, Elso S. Barghoorn and Stanley A. Tyler introduced cellular evolution into the continuum (this was later extended by Lynn Margulis). Biochemical and molecular evolution were introduced in the 1960s by biochemists like Richard Dickerson. Each of these evolution communities represents a diverse and heterogeneous set of practices.

nor even necessarily testable (by Mainx's philosophical standards),[163] questions raised by considerations of evolution had performed an "invaluable heuristic service in all branches of biology."[164] What had formerly been mistaken as disparate points of view had undergone a synthesis, which had reconciled differences of opinion and conflicts, leading to the construction of a coherent and consistent evolutionary worldview:

> In recent years a clear and far-reaching approximation of the various viewpoints has taken place, and various books, as well as discussions at congresses and symposia, allow us to recognize clearly the development of a new synthesis of all possible points of view. Moreover, in this most difficult branch of biological investigation a phenomenon has become clear which in many other branches of biology, and in all pure sciences, can be regarded as a touchstone for the fundamental confirmation of the methodical path of these sciences: the spontaneous convergence of lines of development in the science toward a closed, consistent picture of the world.[165]

Mainx concluded with the following thoughts on biological science:

163. Other philosophers would continue to view evolution as a highly problematic science: see Marjorie Grene, "Two Evolutionary Theories," *Brit. J. Phil. Science, 9* (1959), 110–127; and Karl Popper, *Objective Knowledge* (Oxford: Clarendon Press, 1972). Both were to modify their initially critical positions. One philosopher who viewed evolution on favorable scientific terms was Ernst Cassirer: see *The Problem of Knowledge: Philosophy, Science, and History since Hegel*, trans. William H. Woglom and Charles Hendel (New Haven: Yale University Press, 1950). Mayr responded to each of these philosophers.

164. Felix Mainx, *Foundations of Biology*, in *Foundations of the Unity of Science: Toward an International Encyclopedia of Unified Science*, 1, no. 9 (Chicago: University of Chicago Press, 1955), p. 52. The exact quotation reads: "Although the whole complex of problems thrown up by the theory of evolution must in many of its parts always remain in the stage of a hypothesis which is not testable in practice, yet it has done invaluable heuristic service in all branches of biology and has therefore become an indispensable part of the method of biology. In view of the multiplicity of points of view regarding evolutionary questions indicated above, it is not surprising that representatives of the various subdivisions of biology, such as systematists, morphologists, paleontologists, biogeographers, and geneticists, often put the problems differently and give the hypothesis a different meaning from their several points of view or estimate their empirical confirmation differently. Differences of opinion which come to light in this way have often originated clarifying discussions and so led to fruitful new efforts. Unfortunately such conflicts are often unpleasant and fruitless, owing to a lack of understanding of the logic of science."

165. Ibid., p. 52.

> As an unavoidable consequence of its rich development, biology has experienced an especially marked subdivision into special branches, and this carries with it a certain danger of onesidedness. The synthesis of the results of biology nevertheless goes on throughout consistently and fruitfully and leads to a constant development of the science. There is in biology no "crisis," as has sometimes unjustly been stated. The synthesis of the results of biology with those of the remaining natural sciences has been fruitfully established in many borderline regions and leads to an empirical world picture which is on the whole consistent and unified, if incomplete.
>
> A critical study of foundations and methods which could only be hinted at in this monograph would certainly be very useful in biology. But here biology occupies no special position, because such problems are common to all of the empirical sciences.[166]

By 1955 biology had become not only a unified science, and an empirical science, but a mature science secure of its foundations and well positioned within the positivist ordering of knowledge — intermediate between the physical sciences and the social sciences. Evolution, stretching from the gene to the human and to human culture, would bind and link the mechanistic and materialistic frameworks with the human sciences.[167] Reduc*ible* to the physical world of the gene, and grounded ultimately in the fundamental mathematical principles of population genetics, the disciplines within the positivist ordering of knowledge stood autonomous yet united. Biology would not be any more or less outstanding or problematic than any of the physical sciences. Biology, now an axiomatic science with its *own* logical principles, was no longer in its metaphysical stage of development but had become a positive science to rival physics and chemistry. Biology — indisputable now as *the science of life* — would also make possible a more meaningful existence for "modern man." Evolution lent unity to the mature science of biology, but also brought forth the unification of science; and evolutionary biology emerged as the central balancing point of the unified sciences. In the preface to the first volume, entitled *The Theory of Evolution*, of the Pelican Biology series in 1958, John Maynard Smith could therefore write: "The main unifying idea in biology is Darwin's theory of evolution through natural selection."[168] In this very same year E. O. Wilson

166. Ibid., pp. 84—85.

167. Sociobiology was to emerge from the continuum between genetics and the social sciences.

168. John Maynard Smith, *The Theory of Evolution* (Harmondsworth,

— inheriting the synthesis — taught the first course at Harvard University initiating practitioners into the new emerging central science, Biology 144: *Evolutionary Biology.*

The unification of biology and the emergence of evolutionary biology took place just as the centenary of the publication of Darwin's *Origin* was approaching. Gathering to reexamine and reassess the work of this "great man of science," evolutionary biologists and historians would begin to contribute to the burgeoning literature of Darwin studies.[169] Rereading the present into the past,[170] they reinvented Darwin and Darwinism as *neo*-Darwinism, and reinterpreted *his* "theory of descent with modification" as evolution by means of natural selection.[171] Darwin was to be reconstructed once again as the "founding father" of the discipline of evolutionary biology.[172] Yet though Darwin was to be repeatedly hailed as the Newton of biology,[173] it was the "modern

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Middlesex: Penguin, 1958), p. 11. See also Michael Abercrombie's "Editorial Foreword" to this volume.

169. The fourteenth annual meeting of the SSE was held in conjunction with the University of Chicago Centennial Celebration, November 24—28, 1959; see the three volumes edited by Sol Tax (especially vol. 3) for an account of the festivities, which included commemorative ceremonies with participants in full academic regalia (Sol Tax and Charles Callender, eds, *Evolution after Darwin*, vol. 3 [Chicago: University of Chicago Press, 1960]). The new literature included Loren Eiseley, *Darwin's Century: Evolution and the Men Who Discovered It* (New York: Doubleday, 1958); C. D. Darlington, *Darwin's Place in History* (Oxford: Blackwell, 1959); Gertrude Himmelfarb, *Darwin and the Darwinian Revolution* (London: Chatto and Windus, 1959); Gavin de Beer, *Charles Darwin: Evolution by Natural Selection* (Edinburgh: Nelson Press, 1963). Scientists were to favor de Beer's reading of Darwin; Himmelfarb was not to fare as well.

170. This phrase is borrowed from Galison's historiographic article on Maxwell; see Peter Galison, "Re-reading the Past from the End of Physics: Maxwell's Equations in Retrospect," in *Functions and Uses of Disciplinary Histories*, ed. Loren Graham, Wolf Lepenies, and Peter Weingart (Dordrecht: D. Reidel, 1983), pp. 45—51.

171. See, for instance, the volume of selections from Darwin and Wallace compiled by Gavin de Beer, *Evolution by Natural Selection* (Cambridge: Cambridge University Press, 1958); and see de Beer's biography of Darwin.

172. "Founding father" stories emerge from, and sustain, disciplinary identities. The identity of the founding father is altered and permuted as it is reconstituted with each telling of the story. This disciplinary interpretation resolves the problems introduced by Jan Sapp in "The Nine Lives of Mendel," in *Experimental Inquiries*, ed. H. E. Legrand (Dordrecht: Kluwer Academic, 1991); and in *Where the Truth Lies* (Cambridge: Cambridge University Press, 1990). Darwin has had — and will have — many, many lives.

173. Julian Huxley reaffirmed Darwin as the "Newton of Biology" in his essay prepared for the University of Chicago Centennial Celebration: see "Emergence of Darwinism," in *Evolution after Darwin*, ed. Sol Tax, vol. 1, *The*

synthesis" that would function as the biological analogue of the "Newtonian synthesis" in the grand narrative of the history of science.[174]

THE UNITY OF LIFE AND THE DIVERSITY OF LIFE: BIOLOGICAL AUTONOMY IN THE POST-SPUTNIK BIOLOGICAL SCIENCES — A POSTSCRIPT

While the struggle to unify the biological sciences was over by 1955, the struggle to preserve and maintain the unity, the autonomy, and the location of the biological sciences would continue. The "architects" — a self-designated term — of the modern synthesis would function as the "unifiers" of the biological sciences. Redefining as they renegotiated disciplinary boundaries, the architects were to preserve the delicate balance between unity and autonomy in the biological sciences.

With the launching of Sputnik, American science increasingly came to represent a greater heterogeneity of biological practices. Debates of the relationship between these biological sciences flared up repeatedly through the 1960s, with discussions focusing on the location of the various fields of the biological sciences on the "totem pole" of science.[175]

One of the newer biological sciences to emerge at this time had sprung out of the burgeoning U.S. space program. Drawing heavily on biochemistry, investigators of the new "exobiology"[176] began to examine closely the biochemical basis for the origins of life on earth and on other worlds: arguing from a probability argument based on evolution by means of natural selection, they

Evolution of Life, pp. 1—21. According to Huxley it was Alfred Russel Wallace who first called Darwin "the Newton of Natural History." Darwin is still frequently referred to as the "Newton" of biology. Philip Kitcher used this phrase on p. 54 of *Abusing Science: The Case against Creationism* (Boston: MIT Press, 1982). More recently, Mayr has reaffirmed this argument: see Ernst Mayr, "The Ideological Resistance to Darwin's Theory of Natural Selection," *Proc. Amer. Phil. Soc., 135* (1991), 123—139.

174. It may be argued that the architects of the evolutionary synthesis function as the analogues of Newton. The argument that the Newtonian synthesis and the evolutionary synthesis — especially as it emerged with sociobiology — bear some resemblance to each other has been made by Gerald Holton: see "Analysis and Synthesis as Methodological Themata," in *Case Studies in the Scientific Imagination* (Cambridge: Cambridge University Press, 1978), pp. 111—151.

175. See Charles C. Davis, "Biology Is Not a Totem Pole," *Science, 141* (1963), 308—310.

176. The alternative was "esobiology." These terms were coined by Joshua Lederberg. I thank Carl Sagan for this information.

upheld the existence of extraterrestrial forms of life inhabiting these other worlds.[177] With their search for extraterrestrial intelligence (SETI),[178] exobiologists were able to capture the scientific limelight. For "esobiologists" the biochemical basis for the origins life threatened a complete reduction, and hence engulfment by the physical sciences. Disciplining to preserve the delicate balance once again, Simpson entered the exobiology circle to construct his celebrated argument for "the nonprevalence of humanoids."[179] Evoking historical contingency and the role of chance in evolutionary events, he successfully negotiated the boundaries of evolutionary biology, giving enough autonomy to the biological sciences. In so doing he also reintroduced the issue of historical contingency back into evolutionary biology.[180]

But by far the greatest danger to the preservation of this balance was to come from the molecular basis of life. As research in molecular biology and biochemistry intensified, the links between physicists and chemists and biologists solidified further. With the articulation and refinement of the molecular basis for genetic change, biology faced its greatest threat of complete engulfment by the physical sciences.[181] The beginnings of the split between organismic and molecular biology were felt in the 1950s at Harvard, where Ernst Mayr defended the category of organismic biology against molecular biology in a celebrated exchange

177. The "Drake Equation," based on the work of Frank D. Drake at Cornell University, was used to calculate the probability of the existence of extraterrestrial intelligence.

178. Originally the group met under the rubric CETI (*c*ommunication with *e*xtra*t*errestrial *i*ntelligence). American CETI conferences began officially in 1961 under the auspices of the National Academy of Sciences; see C. E. Sagan, ed., *Communication with Extraterrestrial Intelligence* (Boston: MIT Press, 1973), for the conference proceedings of the international meetings held in Byurakan in 1971.

179. George Gaylord Simpson, "The Nonprevalence of Humanoids," *Science, 143* (1964), 769—775. See also Simpson's exceedingly vitriolic "Added Comments on the 'Nonprevalence of Humanoids' " in *Communication with Extraterrestrial Intelligence*, pp. 362—364.

180. Evolutionary biologists (with some exceptions) have never been strong advocates of the SETI program. See Edward Regis Jr., ed., *Extraterrestrials: Science and Alien Intelligence* (Cambridge: Cambridge University Press, 1985), for confirmation of this; Ernst Mayr's contributed essay, "The Probability of Extraterrestrial Intelligent Life," constructs an argument similar to Simpson's.

181. For a historical discussion of what many identified as the "DNA bandwagon effect" and how it fueled antireductionist philosophical arguments, see John Beatty, "Evolutionary Anti-Reductionism: Historical Reflections," *Biol. Phil., 5* (1990), 199—210.

with biochemist George Wald.[182] In the early 1960s the architects and unifiers were to address the "crisis" — once again — in the biological sciences.[183]

Warding off reduction to the physical sciences became a primary concern for Ernst Mayr, the architect most sensitive to philosophy at this time. It was because he himself had sympathized (initially at least) with the logical positivists and had advocated the unity of science that Mayr was the first to be alerted to the dangers of the physicalist reduction that accompanied the unification of the sciences along with the rise of molecular biology.[184] With the combination of the increasing emphasis placed on population (what he termed "bean-bag") genetics[185] in the 1950s, and the popularity of the physicalist philosophy of Ernest Nagel, the unification of biology threatened to lead to a complete reduction to the physical sciences. Writing for the wider audience of scientists in 1961, Mayr constructed a powerful argument supporting the autonomous yet unified status of the biological sciences.[186] Comparing causality in biology and physics, he argued that "causality in biology" was a "far cry from causality in classical mechanics." The structure of biological science was far more complex and had its *own* emergent properties unlike *any* of the physical sciences. Biology — from then on — would consist of *two* biologies: functional biology and evolutionary biology. While materialistic and mechanistic principles accounted for functional biology, properties in some manner emergent yet well within legitimate science were to account for evolutionary biology. Evolutionary biology — the biology of ultimate, not proximate, causes — would therefore "lift" biology, through an argument for "emergence," from complete reduction to the physical sciences, and at the same time would become the unifying element of a unified biology. In so arguing, Mayr was

182. See Ernst Mayr's Cold Spring Harbor address, "Where Are We?" (above, n. 90), with its justification of the equal ranking between evolutionary and molecular biology.

183. See G. G. Simpson, "The Crisis in Biology," *Amer. Schol., 36* (1966—67), 363—377; Th. Dobzhansky, "On Cartesian and Darwinian Aspects of Biology," *Grad. J., 8* (1968), 99—117.

184. Ernst Mayr had been aware of the Unity of Science Movement but rejected it when he realized that the unity of science would take place by the reduction of biology to physics (letter to author, October 8, 1991).

185. Mayr, "Where are We?" p. 309.

186. Ernst Mayr, "Cause and Effect in Biology," *Science, 134* (1961), 1501—1506.

beginning to ground a new *philosophy of science*, based not solely on physics, but on biology! In 1963 Simpson picked up and extended Mayr's argument for two biologies to not only argue against reduction to the physical sciences, but also argue for the *centrality of biology* in the drive to unify the sciences. He wrote the following for a wide audience: "Biology, then, is the science that stands at the center of all science. It is the science most directly aimed at science's major goal and most definitive of that goal. And it is here, in the field where all the principles of all the sciences are embodied, that science can truly become unified."[187]

But the danger of complete reduction to molecular biology — and ultimately physics — was avoided, indeed made to work *for* biology, through the unifying argument articulated by Dobzhansky. Stretching the continuum from the gene to the human, Dobzhansky incorporated the molecule as the new level of evolution. This new level would serve integrative functions accounting for both the unity of life, with its connection to the physical sciences, and the diversity of life, with its connection to organismic biology. Physics, chemistry, and molecular biology accounted for the unity of life, whereas organismic biology, ecology, and the social sciences would account for the diversity of life. Evolutionary mechanisms were responsible for the unity *and* diversity of life, so that the unity of the sciences was preserved. But it was the organismic biological sciences — situated between the molecular and physical sciences and the social sciences — that would answer the question: What is "Man"?[188] "Molecules to Man" and "Unity and Diversity of Life" became the slogans of biological sciences in the 1960s.[189]

187. G. G. Simpson, "Biology and the Nature of Science," *Science, 139* (1963), 81—88; reprinted in *This View of Life* (above, n. 123), quotation on p. 107.

188. See Th. Dobzhansky, "Biology, Molecular and Organismic," *Amer. Zool., 4* (1964), 443—452; idem, "Are Naturalists Old-Fashioned?" *Amer. Nat., 100* (1966), 541—550. Though Dobzhansky was the architect who contributed most to the making of a mechanistic and materialistic science of evolution, he was also to reject mechanistic materialism most emphatically. Of the unifiers, Dobzhansky was the *least* positivistic; in his 1964 article, he held Comte responsible for the hierarchical ordering of knowledge. In limiting mechanistic materialism, Dobzhansky was to closely echo Teilhard de Chardin in his view of a mystical form of humanism. Dobzhansky was a member of the Eastern Orthodox Church.

189. One of the most widely used biological sciences textbooks in the 1960s, produced by the Biological Sciences Curriculum Study (BSCS), was the "Blue" version, which had as its overarching theme, "Molecules to Man." The textbook also incorporated the theme of the "Unity of Life and Diversity of Life." For the

Evolutionary biology itself came through this period intact and began to be included in routinized high-school curricula as it emerged — unquestionably — as the "central organizing principle" of the biological sciences. The most ambitious enterprise to reform the curriculum of biology in America, the Biological Sciences Curriculum Study (BSCS),[190] drew on the expertise of some of the unifiers: G. Ledyard Stebbins, Jr., and G. G. Simpson, as well as H. J. Muller.[191] By 1973 — two years before his death — Dobzhansky extended the centrality of evolution even further as he defended evolution against American fundamentalist assaults, and preserved at the same time the unity of the biological sciences. Writing in the *American Biology Teacher*, he asserted: "Nothing in biology makes sense except in the light of evolution."[192] Adopting this famous phrase, textbooks of biology in the 1970s and 1980s made the centrality of evolution part of the received and established wisdom of the profession. The most popular textbook devoted expressly to evolutionary biology, Douglas J. Futuyma's *Evolutionary Biology*, promoted as it defined evolutionary theory in unquestionably Newtonian terms: "The theory of evolution is a body of interconnected statements about natural selection and the other processes that are thought to cause evolution, just as the atomic theory of chemistry and the Newtonian theory of mechanics are bodies of statements that describe causes of chemical and physical phenomena."[193]

But it was through the selective writing and rewriting of the history of biology that the belief in biology as unified yet autonomous biological science was to be continuously reinforced.[194] Just as the neutral theory of evolution was exploding on the scene, punctuated equilibrium was being introduced and the critique of

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historical origins, organization, and impact of BSCS see William V. Mayer, "Biology Education in the United States during the Twentieth Century," *Quart. Rev. Biol., 61*: 4 (1986), 481–507.

190. BSCS was an outgrowth of the AIBS. It was established in 1958. See Mayer, "Biology Education."

191. Other biological luminaries who participated include Marston Bates, Daniel Arnon, Garrett Hardin, Joseph Wood Krutch, Alfred Romer, Paul Sears, Philip Handler, and Bruce Wallace; see ibid.

192. Th. Dobzhansky, "Nothing in Biology Makes Sense Except in the Light of Evolution," *Amer. Biol. Teacher, 35* (1973), 125—129. Leigh Van Valen provided this citation.

193. Douglas J. Futuyma, *Evolutionary Biology*, 2nd ed. (Sunderland, Mass.: Sinauer, 1986), p. 15.

194. See Kitcher, *Abusing Science* (above, n. 173), for an example of how the history of biology and the philosophy of biology sustain the view of biology as unified science.

the adaptationist program was being formulated — all of which led to major amendments in evolutionary biology that began to disunify biology;[195] just as a totally mechanistic, materialistic, reductionist biology — devoid of progress and purpose, and with no meaning to life — was gaining acceptance, Ernst Mayr once again stepped in. Drawing together a group of original participants, scientists, philosophers, and young historians of science, Mayr reintroduced and revivified the belief in a unified science of biology. Borrowing and adapting the title of Huxley's 1942 book, the group concentrated their efforts on figuring out "what actually happened" during the "evolutionary synthesis" of the years between 1920 and 1950. Looking for theories and trying to fix *the* synthetic theory at the core, they were frustrated by discovering only that the synthesis was a "moving target."[196] Spilling gallons of ink on the subject, and engaging in heated disputes for nearly a decade, the growing numbers of commentators on what became *the* "synthesis" would only agree in making this count as a historical "event." The Columbia Classics in Evolution — reissued at the same time, with historical prefaces from the next generation of architects — also sustained the belief in biology as unified science. The historical note to the reissue of Dobzhansky's *Genetics and the Origin of Species* begins with the assertion: "Evolution, the proposition that all organisms are related by descent, is the central organizing principle of biology."[197] What the architects had worked to construct, had by 1982 become a matter of fact.

The long-awaited volume of Mayr's 1974 meeting appeared in 1980 and began to serve as the textbook initiating practitioners into the bourgeoning new field of the history of biology.[198] Adopting contextualist historiographic frameworks, and examining the interplay of the material, literary, and social practices deeply embedded within the Western mentalité, this generation of archi-

195. The sundering of the continuum between microevolution and macroevolution, brought on by the punctuationalist view of evolutionary change, would construct an argument that would begin to disunify the biological sciences and would lead to the emergence of the discipline of paleobiology. The clearest discussion of macroevolution is Steven Stanley, *Macroevolution: Pattern and Process* (San Francisco: Freeman, 1979).

196. See Richard Burian, "Challenges to the Evolutionary Synthesis," *Evol. Biol., 27* (1988), 247—269.

197. Niles Eldredge and S. J. Gould, "A Note on the Series," in 1982 reissue of Theodosius Dobzhansky, *Genetics and the Origin of Species* (New York: Columbia University Press, 1982), p. ix.

198. Ernst Mayr also produced his monumental *Growth of Biological Thought* (above, n. 25) at roughly this time.

tects-turned-historians were to historicize to reconstruct[199] the "meaning of the synthesis." The meaning of the synthesis — to a close reader of texts — has always resided in the textbook title: *The Evolutionary Synthesis: Perspectives on the Unification of Biology.*

Historiographic Endnote

Readers who are familiar with contextualist historiography of science will be vexed by the form of contextualist history I have here adopted. There are no "internal" or "external" factors "influencing" or "impacting" on a theory, and I have consciously melded and collapsed the personal, institutional, and social "features" of science *and* history. While I have entertained, indeed been inspired by, the recent contextualist accounts of science in the work of Norton Wise and Crosbie Smith (*Energy and Empire: William Thomson, Lord Kelvin, 1828–1907* [Cambridge: Cambridge University Press, 1989]), and especially that of Steven Shapin and Simon Schaffer (*Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* [Princeton: Princeton University Press, 1985]), I have here adopted a form of contextualism more closely resembling the recent historiography of the cultural history of France, which draws on the work of Lynn Hunt, Keith Michael Baker, Roger Chartier, and others of the fourth generation of the Annales school (for a useful synopsis see Lynn Hunt, *The New Cultural History* [Berkeley: University of California Press, 1989], as well as Keith Michael Baker, *Inventing the French Revolution* [Cambridge: Cambridge University Press, 1990], and Roger Chartier, *Cultural History* [Ithaca, N.Y.: Cornell University Press, 1988]). The differences between the two forms of contextualism hinge on the view of practice. The latter school situates scientific practices within wider cultural practices; thus, material, literary, and social practices — as articulated especially clearly in *Leviathan and the Air-Pump* — are subsumed by the cultural practices embedded within a larger cultural mentalité (in this case the West), which are *discursively* expressed. The difference between these two contextualist accounts is identical to the tension of ideology vs. mentalité in historiography (see Michel Vovelle, *Ideologies and Mentalities* [Chicago: University of Chicago Press, 1990], for a discussion). I lean more heavily on the latter since I would argue that belief in the meaning of life, as well as the unity of knowledge, emerges from commitments *deep*

199. A construction of a construction, as Geertz would say.

within the Western "way of thought." While I uphold a constructivist theory of knowledge, I have subsumed the social construction of knowledge within the larger cultural construction of knowledge. My position echoes much of the work in post-colonialist ethnography and draws on historiographic models adopted from cultural anthropologists. In anthropological terms, this history constitutes an "insider's" account, with critical distance gained through the adoption of critical tools — in this case, an examination of discursive practice — that effectively "defamiliarize the familiar." I have drawn on the work of James Clifford, Donna Haraway, and especially Renato Resaldo (see his recent *Culture and Truth* [Boston: Beacon Press, 1990]). I have also found historical guidance in the eloquence of Michel de Certeau, *The Practice of Everyday Life* (Berkeley: University of California Press, 1988), and especially *The Writing of History* (New York: Columbia University Press, 1988). Veteran readers may be pleased to note the return of intellectual history, admittedly a "new" form, with culturally embedded features of scientific practice.

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