Shaping Science with the Past

Textbooks, History, and the Disciplining of Genetics

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Declaration

This dissertation is the result of my own work and includes nothing which is the outcome of work done in collaboration. It does not exceed the established word limit.

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Summary

Science is generally not thought of as being deeply historiographical. Although it is clear that scientists frequently write about history in their work—that, for example, they identify the significance of an advance by situating it historically, or refer to a historic source of authority in order to add legitimacy to a position—it is often supposed that the historical claims of scientists are incidental to the scientific. This thesis contests basic assumptions of this view. In a study of the textbooks of twentieth century Anglo-American genetics—of a place where the canon of a science is consolidated, as the heterogeneous approaches and controversies of its practice are rendered unified for its reproduction—I develop a novel taxonomy of the forms in which history can be written, and of the scientific functions that they can serve. Progressing from an analysis of narrative historical accounts, to latent and embedded formulations of the past, I demonstrate the ways in which geneticists used history-writing in the disciplining of the foundations, future practitioners, conceptual order, and boundaries of their science.

After an introductory chapter identifying some of the ways in which the textbooks and historical accounts of a science may be contributory, rather than intellectually external and temporally subsequent, to its formation and development, I advance the central argument of this thesis in four chapters. Each examines a different form of history-writing. In the first, I explore the disciplining of the foundations of genetics, with a study of the explicit, narrative histories of hereditary science that were written in three important first-generation genetics textbooks. Identifying radical differences in their accounts of the same nineteenth-century figures, experiments and theories, I argue that these different ways of consolidating history were connected to fundamentally different ideas of the conceptual foundations of the science, and that they were used to advance divergent visions of the science's future. I then look at the historical case-based and problem-solving method of teaching that was developed in the 1920s-1940s to convey the science of genetics. I argue that this method created "virtual historical environments" that allowed students to learn and practice not only the principles that were studied by geneticists and were explicitly taught as rules in the text, but also the tacit skills needed to follow, find, and understand these rules. Here, history was used in the disciplining of the mind of the student. In the third chapter, I look at the "standard historical approach" to teaching in the 1930s-1950s, exploring the establishment of this approach, the functions and consequences of literary devices on which it relied, and the ways in which the meaning of facts and theories were shaped within it. My central contention is that a notion of history was constitutive of the organizational logic, narrative structure, and inner rationality of textbook genetics, thereby performing a powerful function in the disciplining of the conceptual order of the science. The fourth chapter explores the sense of history embodied in the use of the concept of "classical genetics" in textbooks of the 1960s-1970s. Tracing the semantic development of "classical" from its first uses in the 1920s, I argue that this term was a politically powerful concept in the language of geneticists: at first used to define and establish sources of scientific authority, it was subsequently developed in arguments about the philosophical and ideological character of genetics, and eventually served to establish the disciplinary identity and boundaries of the science. By differentiating these various uses of "classical," I show that the disciplinary power of this term—which is derived from the authority of history—relied on the effacement of its historicity and the situations in which it was created and deployed.

With this thesis, I push the boundaries on common conceptions of what is involved in, and what should be counted as, the "history" and "writing" of history-writing. Advancing a novel taxonomy of the forms in which the historical can appear, I provide a starting point for further historiographical research on the subtle yet powerful ways in which the historicity of our past can make claims upon us.

Contents

INTRODUCTION

Textbooks and Histories in Science

Science is not often thought of as being deeply historiographical. Although scientists would acknowledge that they write about history in their work—that, for example, they identify the significance of an advance by situating it historically, or refer to a historic source of authority in order to add legitimacy to a position—most would also contend that their history-writing is incidental to their scientific claims. And non-scientists would often agree. It would appear to many that the historical claims in scientific writing are constitutive not of the science itself, but of the narratives used to explain the science; on this view, history is a frame in which scientific claims are presented, and from which they can be removed. Some may also suggest that the historical claims of scientists extend only insofar as current scientific problems require and that they are not, therefore, actually statements about the past and should not be counted as history-writing. In this thesis, I challenge this strong differentiation of the historical from the scientific and the assumptions about history-writing on which it is based. In a study of the textbooks of twentieth century Anglo-American genetics—of a place where the canon of the science was consolidated, as the heterogeneous approaches and controversies of its practice were rendered unified for its reproduction—I develop a novel taxonomy of the forms in which history can be written, and of the scientific functions that they can serve. Progressing from an analysis of narrative historical accounts, to latent and embedded formulations of the past, I demonstrate the ways in which geneticists used history-writing in the disciplining of the foundations, future practitioners, conceptual order, and boundaries of their science.

The Functions and Characteristics of University Science Textbooks

Placing textbooks at the center of my analysis, I work against a common hierarchical ordering of historical sources, suggesting that written work should not be valued less than experimental practices, nor published writing less than laboratory notes, nor textbooks less than journal articles.[1] Although textbooks may appear to be removed from, and unrepresentative of, the practice of the science that is presented within their pages, this appearance is the result of an inadequate picture of the work done by their authors. Textbooks are not passive representations of a science that exists elsewhere, but rather interpretations that shape the science as they present it; it is in their pages that disparate scientific practices and theories are brought together and presented as being a coherent science for the first time. Thus textbooks are not simple artifacts of a science, but rather heterogeneous tools, and they should be analyzed as processes, not objects. A textbook must be understood in terms of the numerous places in which it is used, for in each of them its salient features and functions are created or reformed. What a textbook is and does depends on whether it is in the hands of students, reviewers, practitioners, non-scientists, or somewhere in-between.[2]

In the hands of students textbooks are, according to Thomas Kuhn, "pedagogical vehicles for the perpetuation of normal science."[3] They discipline the student's conceptual orientation, method of investigation, and sense of history. Suggesting that textbooks cannot separate facts from methods of investigation, Kuhn writes: "I suspect that students will learn both together as samples of accepted achievement, which is only

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[1] As discussed in G. Myers, *Writing Biology: Texts in the Social Construction of Scientific Knowledge* (London: The University of Wisconsin Press, 1990), 5, it has been suggested that textual analysis fails to get at the heart of scientific practice: see, e.g., B. Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Milton Keynes: Open University Press, 1987), 61; M. Lynch, *Art and Artifact in Laboratory Science: A Study in Shop Work and Shop Talk in a Research Laboratory* (London: Routledge, 1985), 143-154; and H. Collins, *Changing Order: Replication and Induction in Scientific Practice* (London: Sage, 1985), 73. For a discussion of the value of textual analysis in the study of scientific practice, see the first chapter of *Writing Biology*, "Controversies about Scientific Texts." A general case for the study of textbooks is made in J. Issitt, "Reflections on the Study of Textbooks," *History of Education* 33 (2004).

[2] NB: Unless otherwise stated, my claims about "textbooks" throughout this dissertation should be read as referring to university science textbooks that would have been identified by their authors in these terms—a definition that excludes many of the written works that have been used in teaching science, especially prior to the twentieth century. For a general discussion of the category "textbooks," see Issitt, "Reflections on the Study of Textbooks," 684-687.

[3] T. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970), 137.

to say that I suspect they will learn paradigms."[4] In Kuhn's discussion of the dogma-enforcing functions of textbooks, he notes an interesting asymmetry between the pedagogical literature of the sciences and that of the humanities: whereas introductory university science is generally taught with textbooks that are written for students, introductory courses in many of the humanities are taught using articles and monographs that are produced by and for practitioners of the discipline. A student in an introductory course on European history, for example, will generally not read a textbook, but rather the seminal works that have been written by historians of Europe.[5] Because of this asymmetry and the homogeneity of science textbooks, the books that compete for use in a science course differ primarily in level and detail, whereas those that compete for use in a humanities course differ in terms of approach, substance, and conceptual structure.[6] It is for this reason that textbook science can perform powerful dogma-enforcing functions, according to Kuhn. It has been suggested, however, that Kuhn's account of the functions of textbooks places too much emphasis on the relationship between the textbook and the psychology of the student. Greg Myers, for example, has argued that because most scientists will eventually reappraise the textbook knowledge they have learned, textbooks cannot perform these dogma-enforcing and psychology-forming functions and thus, "If textbooks are still important, it cannot be on the kinds of psychological grounds offered by Kuhn."[7] In drawing this conclusion, it seems to me that Myers goes too far; for although a scientist can certainly reevaluate the information contained within the textbook that introduced him to his subject, it does not follow that he can completely overcome the early conditioning of his scientific mind. However, Myers' criticism does rightly point to a significant limitation in any interpretation of textbooks that focuses solely on their role in the beginning of the life of a scientist.

In addition to performing key pedagogical functions, textbooks play an important role in the literature of a science, shaping the development of its conceptual order. They

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[4] T. Kuhn, "Discussion," in Scientific Change, ed. A. C. Crombie (London: Heinemann, 1963), 391.

[5] T. Kuhn, "The Function of Dogma in Scientific Research," in *Scientific Change*, ed. A. C. Crombie (London: Heinemann, 1963), 350: "Perhaps the most striking feature of scientific education is that, to an extent quite unknown in other creative fields, it is conducted through textbooks, works written especially for students."

[6] Ibid., 350-351.

[7] G. Myers, "Textbooks and the Sociology of Scientific Knowledge," *English for Specific Purposes* 11 (1992): 6.

are not just repositories into which established and distilled journal science is downloaded, but rather sites of production. This often unrecognized aspect of the textbook's place and function has been identified in the model of science proposed by Ludwik Fleck, who suggests that scientific ideas and facts are conceived and developed in four types of literature, each of which is used by a different part of the "thought collective." For those students being initiated into the esoteric circle of the science there is "textbook science"; inside the esoteric circle of experts there is "journal science" and "vademecum (or handbook) science"; and outside the esoteric circle there is "popular science." The ideas and facts of expert science do not arise from within the esoteric circle of laboratories and journal publications alone. Rather, they are created and developed during their circulation through the four types of science.[8] Although Fleck only briefly mentions the "socio-intellectual form" of textbook science and does not elaborate on its specific functions, the nature and function of textbooks are indirectly illuminated by his comments on handbook science.[9] For example, in a discussion of the requirement of internal consistency, Fleck suggests: "The vademecum is therefore not simply the result of either a compilation or a collection of various journal contributions. The former is impossible because such papers often contradict each other. The latter does not yield a closed system, which is the goal of vademecum science."[10] For this reason, the writing of a handbook must be understood as the production—rather than the mere presentation—of science: "This [the handbook] is the means by which exoteric knowledge, knowledge originating in other collectives, and strictly specialist knowledge are all selected, blended, adapted, and molded into a system." Through the "selection and orderly arrangement" of the science into this system, the author shapes the future of the science: "The plan according to which selection and arrangement are made will then provide guidelines for future research. It governs the decision on what counts as a basic concept, what methods should be accepted, which research directions appear most

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[8] For a systemic textual analysis of how facts are actually constituted in various forms of scientific writing, see Myers, *Writing Biology*.

[9] L. Fleck, *Genesis and Development of a Scientific Fact*, trans. F. Bradley and T. J. Trenn (London: University of Chicago Press, 1981), 112.

[10] Ibid., 119.

promising."[11] Much the same can be said of a textbook, for the textbook is the only other place where the entire science is brought together and presented as a cohesive system. Like a handbook, a textbook is not just a collection of a science that exists elsewhere, but rather a presentation that is the result of significant interpretation and conceptual ordering. When seen in this context, as a part of the production of disciplinary order and coherence, textbooks are opened to new types of analysis. If textbooks do in fact provide a space in which significant aspects of scientific consensus are *first* produced and established—and are therefore responsible for resolving or eliminating conflicts in the scientific community—it is important to question traditional understandings of the actors involved in the process by which science "progresses." It will be important to explore the narrative devices used in creating internal coherence, or the illusion thereof, within the text of the textbook. And it will be essential to use textbooks as a source when tracking and understanding the ways in which new ideas become established.

Although Fleck is certainly correct in suggesting that science develops as ideas travel back and forth across the various types of scientific literature, it is important to note that the textbook is often used as if it were a mere reference point—a collection of established and static scientific facts. When used in this way, textbooks must be understood in terms of their place in the hierarchy of scientific authority. They must be seen as the conclusion of a process of accreditation, in which claims of fact travel from journal articles, to articles citing journal articles, to review articles, to textbooks; at each stage in this development, fewer claims are made, and those that remain are more established as facts.[12] Thus, a salient feature of textbooks is that their authors give only a few references in support of the theories and facts that they select and present; and when they do cite an article, it is often a review, and not the paper in which the claim was first made.[13] Discussing the self-justifying structure of textbooks, Myers notes that they often just refer to other parts of the textbook, thereby setting up a self-contained reference system that is very different from that found in articles: "the scientific article depends on

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[11] Ibid., 119-120.

[12] This aspect of textbooks has been briefly discussed in J. Ziman, *An Introduction to Science Studies: The Philosophical and Social Aspects of Science and Technology* (Cambridge: Cambridge University Press, 1984), 66.

[13] Myers, "Textbooks and the Sociology of Scientific Knowledge," 11-12.

a vast network of references to other texts that themselves refer to further texts… In the process of accreditation, an author furthers a claim by using earlier texts and, more important, by getting later texts to use it."[14] When looking at how accreditation culminates in textbooks, it is also important to explore if and how textbooks are subsequently brought back into the literature of the science as authoritative sources. In my brief study of one of the most widely used genetics textbooks of the 1930s-1960s, *Principles of Genetics* (Sinnott et al.), I found that the textbook functioned as a "closed" source of authority for authors of articles in genetics and general science journals.[15] For example: in the journals *Science* and *Genetics*, this textbook was cited in claims about the results of particular genetic crosses,[16] the lethality of almost all homozygous deficiencies in *Drosophila*,[17] the definition of "population,"[18] the preparation of cornmeal for *Drosophila* experiments,[19] and the interdependence of gene functions.[20] Occasionally, *Principles* was used to support explicit claims about matters of consensus—to prove that it was, for example, "generally agreed" that a claim was true.[21] Although these uses of *Principles* are in some respects similar to the uses of journal articles, they differ in one central respect: whereas a journal article that is decades-old will rarely be cited, except as support for a claim about "history" rather than "science," the 1950 edition of *Principles* was cited as an authoritative source in *Science* as recently

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[14] Ibid.: 11.

[15] In addition, to gain a sense of whether the citation of textbooks was unique to a limited period in the development of genetics or to this particular textbook, I looked at all of the citations in the first issue of *Genetics* in 1950, and in 1975. In both, approximately thirty percent of all articles cited textbooks: e.g., E. J. Gardner and C. M. Woolf, "The Influence of High and Low Temperatures on the Expression of Tumorous Head in Drosophila Melanogaster," *Genetics* 35 (1950); I. Opatowski, "On the Interpretation of the Dose-Frequency Curve in Radiogenetics," *Genetics* 35 (1950); D. Marinkovic and F. J. Ayala, "Fitness of Allozyme Variants in Drosophila Pseudoobscura. I. Selection at the Pgm-1 and Me-2 Loci," *Genetics* 79 (1975); M. Wasserman and H. R. Koepfer, "Fitness of Karyotypes in Drosophila Pseudoobscura," *Genetics* 79 (1975); and C. Wills, J. Phelps, and R. Ferguson, "Further Evidence for Selective Differences between Isoalleles in Drosophila," *Genetics* 79 (1975).

[16] J. C. DeFries, J. P. Hegmann, and M. W. Weir, "Open-Field Behavior in Mice: Evidence for a Major Gene Effect Mediated by the Visual System," *Science* 154 (1966): 1578.

[17] R. D. Milkman, "The Genetic Basis of Natural Variation. II. Analysis of a Polygenic System in Drosophila Melanogaster," *Genetics* 45 (1960): 386.

[18] J. B. Hughes, G. C. Daily, and P. R. Ehrlich, "Population Diversity: Its Extent and Extinction," *Science* 278 (1997): 689.

[19] E. S. McDonough, "Inhibition of Mold Contamination in Drosophila Food Using Sodium Orthophenylphenate," *Science* 118 (1953): 388.

[20] R. Blanc, "Dominigenes of the Vestigial Series in Drosophila Melanogaster," *Genetics* 31 (1946): 395.

[21] Ibid.

as 1997.[22] When confronted with this picture of the non-pedagogical uses of textbooks, the importance of textbooks in a science's system of authority becomes apparent, as do some important features of the textbook. Insofar as the authority of the textbook does not rely on citations, the way in which it establishes and presents its authority is unique. Given that all claims in textbooks cannot have reached the same degree of accreditation, the text will include narrative devices that differentiate claims of differing degrees of certainty.

The writing of textbooks also plays an important role in the development of the pedagogical aims of the science. The content of the claims made in textbooks is in part shaped by the framework in which they are written—by the pedagogical theories and ideas that guide the author's formulation of the text. While textbooks are generally written as pedagogical tools, they can be intended to produce a wide variety of results. They can, for example, be designed to convey a body of theories, facts, ideas, or knowledge; or alternatively, to foster the development of a method of investigation. When the goals of the author are informed by general pedagogical theory, the textbook is not just shaped by the scientific practice, but also by the broader culture. The use of pedagogical theory must not only be seen as playing a crucial role in a relationship between authors and students, but also as participating in and arising out of the development of the philosophy of science education. The goal of science education was a subject of great concern during the 1920-1960s, and the teaching of genetics was itself seen as an emerging science. In 1925, for example, Edmund W. Sinnott and L. C. Dunn discussed the need to invent a new model of teaching and explained their attempts to do so in the preface to *Principles of Genetics*: "Genetics is still so young a science and is changing so rapidly from year to year that no uniform practice has been established for the text-book treatment of its subject matter."[23] William E. Castle agreed when reviewing the textbook, stating that he welcomed and applauded "any new contribution

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[22] Hughes, Daily, and Ehrlich, "Population Diversity: Its Extent and Extinction," 689.

[23] E. W. Sinnott and L. C. Dunn, *Principles of Genetics: An Elementary Text, with Problems*, 1st ed. (New York: McGraw-Hill, 1925), ix.

to the only-partly-solved problem of how successfully to teach genetics."[24] With time, methods became established and popular textbooks were copied. For example: James Watson's *Molecular Biology of the Gene* used modified illustrations from *Principles*,[25] and the method of teaching "three-factor mapping" developed by Sinnott and Dunn in 1925 became standard.[26] Given the influence of pedagogical conventions, a textbook—even when it is a first edition—must be viewed as text with a significant history.

It is not just in the context of pedagogical development, however, that the historicity of textbooks is connected to their functions. Although this feature of textbooks may not be an important one in the minds of the authors producing them, textbooks are nevertheless received and used as markers of time. For example: in the 1910s-1940s, the rapid rate of progress in the science of genetics was generally mentioned in the first paragraph of reviews of genetics textbooks.[27] And new textbooks were taken to be indicative of this progress. This gave them value, independent of their pedagogical utility. According to A. F. Blakeslee, for example: "The two publications cited above are of interest from the light they throw upon the rapid evolution of genetics within recent years, entirely apart from the information which they may lay before the student."[28] Ernst Caspari likewise suggested: "A comparison of the present text with the previous ones serves best to demonstrate the enormous changes genetics has undergone in the last decade."[29] This use of textbooks was invited by their unique significance in the literature of science. Unlike journal articles, which only claim to deal with an aspect of the science, textbooks appear to be total and coherent representations of the basic

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[24] W. E. Castle, "Some New Books on Genetics," Review of *Genetics in Plant and Animal Improvement,* by D. F. Jones, *Principles of Genetics*, by E. W. Sinnott and L. C. Dunn, and *Animal Genetics* by F. A. E. Crew, *Science* 62 (1925): 567.

[25] W. G. Whaley, "Edmund Ware Sinnott," *Biographical Memoirs of the National Academy of Sciences* 54 (1983): 355.

[26] M. Chalfie, "Is the Traditional Way of Teaching Three-Factor Mapping Sufficient?" *Trends in Genetics* 13 (1997): 94.

[27] E.g., Castle, "Some New Books on Genetics," 567: "our knowledge of genetics has been increasing so rapidly that no text remains up-to-date unless it is frequently revised or rewritten." See also N. M. Grier, Review of *Genetics*, by H. E. Walter, *The American Midland Naturalist* 12 (1930): 166; E. M. East, "Genetics," Review of *Elemente der Exakten Erblichkeitslehre*, by W. Johannsen, *Botanical Gazette* 57 (1914): 239; J. A. Detlefsen, Review of *Genetics: An Introduction to the Study of Heredity*, by H. E. Walter, *Science* 56 (1922): 145; and T. Just, Review of *The Principles of Heredity*, by L. H. Snyder, *The American Midland Naturalist* 26 (1941): 440.

[28] A. F. Blakeslee, Review of *Principles of Genetics*, by E. W. Sinnott and L. C. Dunn and *Recent Advances in Plant Genetics*, by F. W. Sansome and J. Philip, *Science* 77 (1933): 284.

[29] E. Caspari, Review of *Principles of Genetics*, by E. W. Sinnott, L. C. Dunn, and Th. Dobzhansky, *Science* 112 (1950): 725.

features of the science; and thus, comparisons between textbooks of different times allow for claims about progress. In this capacity, textbooks serve as markers of time. And as markers, they function in two ways. At the moment of production, a textbook identifies what it means to be a practitioner in the present, declaring as much as possible what the discipline is about. When time passes and the textbook loses currency, it becomes a token of old science, and a point of reference against which new textbooks can define progress. Thus, if use determines function, the nature of a textbook is not only that of a resource in the present; it is, in addition, that which becomes a sign of the past.

Thus far, I have discussed the nature of textbooks by identifying features that arise from their use in six aspects of a scientific practice—the reproduction of the science, the formation of the science's conceptual order and consistency, the accreditation of its facts, the establishment of its pedagogical tools and aims, and the evaluation of scientific progress. In fulfilling each of these functions, a textbook takes on different salient characteristics, which I have begun to identify. My list and discussion are necessarily incomplete, however, given the scope of this introduction. To fully understand the functions of textbooks, one would need a far more complete picture of their use. As articulated by L. C. Cronbach at a major conference on "the textbook" more than fifty years ago: "One cannot really judge the functional contribution of the text alone, for the text-in-use is a complex social process wherein a book, an institution, and a number of human beings are interlaced beyond the possibility of separation."[30] To understand the text-in-use would require an analysis of reading practices that are, in the words of Adrian Johns, "no less skillful, and no less local, than the conducting of an experiment."[31] Even this type of analysis, however, would only begin to uncover the functions and uses of textbooks. In addition, it would be essential to trace the history of the textbook through the entire "communication circuit" in which it is involved.[32] This

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[30] L. J. Cronbach, "The Text in Use," in *Text Materials in Modern Education: A Comprehensive Theory and Platform for Research*, ed. L. J. Cronbach (Urbana: University of Illinois Press, 1955), 188.

[31] A. Johns, *The Nature of the Book: Print and Knowledge in the Making* (London: University of Chicago Press, 1998), 48.

[32] For a discussion of the value of studying a text in terms of its circulation through a "communication circuit," see R. Darnton, *The Kiss of Lamourette: Reflections in Cultural History* (London: Faber and Faber, 1990), esp. 111-113, 136-137. In the fields of education and policy—in which studies of textbooks are not uncommon—some research has addressed aspects of the communication circuit of textbooks. See, e.g., the collections of articles in L. J. Cronbach, ed., *Text Materials in Modern Education: A*

would involve looking at how resources passed from the field, to the author, to the manuscript, to the publisher, to the printed materials, to the distributor, to the teacher, to the teacher's lessons, to the student, to the lessons noted and stored; and conversely, at the paths by which information about the reception of the textbook by students, teachers, and reviewers was communicated to market research companies, publishers, and authors, as well as at how this communication shaped the writing of future texts. This type of analysis would certainly help create a more robust picture of the uses of textbooks, yet it too would be but another beginning, for as Adrian Johns has also argued: "Any printed book is, as a matter of fact, both the product of one complex set of social and technological processes and also the starting point for another."[33] And just as a textbook is the product of various modern technological production systems, it is itself a form of technology—a craft that shapes the perspective of the producer and user. Thus, an analysis of the place of the textbook in the full communication circuit would require a project on the scale of Jim Secord's *Victorian Sensation*.[34] But given the questions about textbooks that motivate this thesis, it has been infeasible to write such a history. While I will look at book reviews, I have set aside many important questions about use and reception. This is in part because a substantive analysis along these lines would have required primary sources that are unavailable for the textbook that most interested me.[35]

Comprehensive Theory and Platform for Research (Urbana: University of Illinois Press, 1955); J. Y. Cole, ed., *The Textbook in American Society* (Washington, DC: Library of Congress, 1981); P. G. Altbach et al., eds., *Textbooks in American Society: Politics, Policy, and Pedagogy* (Albany: State University of New York Press, 1991); and J. G. Herlihy, ed., *The Textbook Controversy: Issues, Aspects, and Perspectives* (Norwood, NJ: Ablex, 1992).

[33] Johns, *The Nature of the Book: Print and Knowledge in the Making*, 3.

[34] J. A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation* (Chicago: University of Chicago Press, 2003).

[35] I considered following the example of Secord, and the suggestions of Cronbach, Johns, and Darnton, and I did extensively research the history of the most widely used textbook of the period in the history of genetics that interested me—Sinnott, Dunn, and Dobzhansky's *Principles of Genetics*. But I was unable to find a wealth of archival material. The L. C. Dunn Papers and Theodosius Dobzhansky Papers in the American Philosophical Society contain very little that sheds light on the production of the textbook; for example, the only manuscript material is for a chapter of an edition that was never published due to Dunn's death, and the little correspondence they had about the textbook most often pertained to this unfinished edition. In the transcripts of the extensive McGraw-Hill Oral History Project at Columbia University, there does not seem to be any discussion of the publication of *Principles of Genetics* or any other college science textbooks (although changes in the writing and publishing of college textbooks for courses in vocational education are discussed). From the Tracy Sonneborn manuscripts collection at the University of Indiana, I obtained a few references to *Principles of Genetics* in teaching notes, but I did not find any other similar documents elsewhere. The archives of Harvard University—which contain a substantial collection of course syllabi and reading lists dating to the nineteenth century—have few such documents for genetics courses, as these materials were often not saved by science departments. It is important to note, however,

But more centrally, it is because writing such a history of a textbook would have been incompatible with a central aim of this thesis, which is to develop a systematic and substantive analysis of the scientific uses and functions of history-writing.

Uses of History and the Nature of History-Writing

In *The Structure of Scientific Revolutions*, T. S. Kuhn famously described scientists' accounts of the history of science: "Partly by selection and partly by distortion, the scientists of earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific."[36] While Kuhn did not provide detailed support for this claim—or for any of his other novel and provocative suggestions about scientists' uses of history—historians and sociologists of science have since shown interest in these questions. The disciplinary functions of history-writing, for example, have been explored in several excellent studies.[37] A special issue of *Osiris* has been devoted to commemorative practices in science.[38] And, with respect to genetics in particular, the uses of Mendel have been the subject of much work.[39] In this section, I will attempt to categorize and briefly summarize key functions and forms of history-writing that have been identified and discussed in this developing body of literature.[40]

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that there might be suitable material related to the production and use of other textbooks. If one were interested in this type of project, it would be worth exploring the William G. Whaley Papers at the University of Texas at Austin, which contain third edition corrections and fourth edition plans for *Principles of Biology*, as well as correspondence with Harper and Row; the A. H. Sturtevant Papers at California Institute of Technology, which contain a manuscript of *An Introduction to Genetics* and correspondence with co-author George Beadle; and the Adrian M. Srb Papers at Cornell, which contain some correspondence concerning one of the most widely-used genetics textbooks of the 1960s, *General Genetics.*

[36] Kuhn, *The Structure of Scientific Revolutions*, 138.

[37] See, e.g., N. Jardine, "The Mantle of Müller and the Ghost of Goethe," in *History and the Disciplines: The Reclassification of Knowledge in Early Modern Europe*, ed. D. R. Kelley (Rochester: The University of Rochester Press, 1997); J. Martin, "Explaining John Freind's History of Physick," *Studies in the History and Philosophy of Science* 19 (1988); and the collection of articles in L. Graham, W. Lepenies, and P. Weingart, eds., *Functions and Uses of Disciplinary Histories* (Dordrecht: D. Reidel, 1983).

[38] P. G. Abir-Am and C. A. Elliott, eds., *Commemorative Practices in Science: Historical Perspectives on the Politics of Collective Memory*, vol. 14 (1999).

[39] See, e.g., A. Brannigan, "The Reification of Mendel," *Social Studies of Science* 9 (1979); R. Olby, "Mendel No Mendelian?" *History of Science* 17 (1979); L. A. Callender, "Gregor Mendel: An Opponent of Descent with Modification," *History of Science* 26 (1988); and J. Sapp, "The Nine Lives of Gregor Mendel," in *Experimental Inquiries*, ed. H. E. L. Grand (Kluwer, 1990).

[40] Note that this discussion will focus on the *functional consequences* of history-writing in *university* science, and therefore will not address all of the ways in which scientists and educators have intended to

A function of history that has been the subject of a few studies is one that emerges in times of conflict and uncertainty, when history-writing is used to direct science, by either justifying and catalyzing a revolution, or counteracting and preventing change. The factual and neutral appearance of a historical account is used to make the research, discoveries, and advances of one faction appear to be inevitable. Through this "normalization" or "fatalization" of the direction in which the science will progress, history-writing has a powerful effect on practitioners, the public, and those funding science.[41] One such use of history-writing has been identified in a study by Wolf Lepenies, who notes that accounts of the discovery of the double helix were written while the development of molecular biology was ongoing; thus what started on the margins of biology revolutionized the discipline, but was presented as the seemingly natural direction of its development.[42] Rachel Laudan has identified a similar use of "stipulative history" in Charles Lyell's *Principles of Geology*: the historical introduction was meant to precipitate a revolution by showing that progress in geology was being hindered by the dominant methodology, and by identifying the merits of Lyell's alternative method.[43] These types of historical accounts, which are written to catalyze change, glorify the work of certain individuals and set them up to become founding

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use history of science. For example: I will not discuss whether or not lessons in the history of science have actually had a humanizing effect on scientists or whether they have fostered a stronger democracy, although these are "functions" that they have been intended to serve. For discussions of these goals, see A. K. Mayer, "Moralizing Science: The Uses of Science's Past in National Education in the 1920s," *British Journal for the History of Science* 30 (1997); A. K. Mayer, "Fatal Mutilations," *History of Science* 40 (2002); and M. Shortland and A. Warwick, eds., *Teaching the History of Science* (Oxford: Blackwell, 1989). There are also several doctoral dissertations on this topic, including A. K. Mayer, "Roots of the History of Science in Britain 1916-1950" (Unpublished Ph.D. Thesis, The University of Cambridge, 2003); E. A. Melia, "Science, Values and Education: The Search for Cultural Unity at Harvard under Charles W. Eliot, A. Lawrence Lowell and James B. Conant" (Unpublished Ph.D. Thesis, The Johns Hopkins University, 1995); and W. J. Sherratt, "History of Science in Education" (Unpublished Ph.D. Thesis, University of Leicester, 1980).

[41] The idea of "normalization" comes from Kuhn, *The Structure of Scientific Revolutions*, Chapter 11. On D. von Engelhardt on the "fatalization" of the history of science in the nineteenth century, see N. Jardine, *The Scenes of Inquiry: On the Reality of Questions in the Sciences* (Oxford: Clarendon Press, 1991), 236.

[42] W. Lepenies, "Introduction," in *Functions and Uses of Disciplinary Histories*, ed. L. Graham, W. Lepenies, and P. Weingart (Dordrecht: D. Reidel, 1983), xvii.

[43] R. Laudan, "Redefinitions of a Discipline: History of Geology and Geological History," in *Functions and Uses of Disciplinary Histories*, ed. L. Graham, W. Lepenies, and P. Weingart (Dordrecht: D. Reidel, 1983), 81-84, 87-91, 94-95. See also R. Porter, "Charles Lyell and the Principles of the History of Geology," *British Journal for the History of Science* 9 (1976): 91-31.

founders.[44] It is not just during periods of unrest, however, that history-writing plays an important role in the creation of "revolutions."

After a science has undergone a significant change, practitioners generally provide historical explanations of why it happened in order to stabilize the new science.[45] And when, as is often the case, an account of the actual cause of the change does not appear logical to the new scientific community, these *post hoc* explanations rely on historical revisionism. This happens, for example, when the change was the product of scientific reasoning that no longer appears valid, or when it was the product of social, political, or economic causes. In these cases, a historical account can be used to project the post-revolution standards back onto the transitional period, and help turn the revolution into a new foundation. It is in these contexts that founding-father myths are also often created; the supposed innovations of founding fathers are often just articulations, generalizations, or refinements of the current scientific practice. In these cases, history-writing provides rational explanations for the acceptance of methods, experiments, and theories that were in fact adopted on other grounds.[46]

Retrospective reinterpretations of history also serve important pedagogical functions. When, for example, teachers want students to learn a theory or law by "discovering" it, either experimentally or on paper, they often provide an account of how it was first discovered. This popular form of teaching can give the student a sense of the logic of the science, and of how ideas, theories, and discoveries relate to and build on each other. But in using this technique, teachers must often significantly reinterpret the past. If there has been a change in the significance of a principle or equation—or in the context and community in which it was developed—it will not make sense to have the student mimic the actual steps taken in its discovery or derivation. Thus, it is necessary to develop a new way for the student to arrive at the same conclusion. Retrospectively

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[44] For an excellent discussion of the production and functioning of founding founders, see B. Bensaude-Vincent, "A Founder Myth in the History of Sciences? The Lavoisier Case," in *Functions and Uses of Disciplinary Histories*, ed. L. Graham, W. Lepenies, and P. Weingart (Dordrecht: D. Reidel, 1983).

[45] E.g., Laudan shows that in the early histories of plate tectonics, what was seen as a Kuhnian revolution by many geologists was stabilized by histories that characterized it as being similar to revolutions in other sciences: Laudan, "Redefinitions of a Discipline: History of Geology and Geological History," 84-87, 91-93, 95-98.

[46] This type of history-writing is famously described in Kuhn, *The Structure of Scientific Revolutions*, 138. See also Jardine, *The Scenes of Inquiry*, 149-150.

reconstructing it, the author is able to present it in such a way that the student sees it in terms of its place in the modern science. Like histories that retrospectively rationalize change, this pedagogical revisionism presents the past in terms of the present.[47]

Another crucial function of histories comes from their role in the formation of disciplines. Whether or not disciplinary categories are the product of intentional or unintentional anachronism, they are generally retrospective historiographical artifacts: the names of disciplines do not simply label existing practices, but rather assist in uniting disparate fields of practice into an apparently natural and cohesive entity. They are, in Ian Hacking's vocabulary, "interactive kinds," altering and informing the behavior of the group of individuals to which they are applied.[48] The name of a discipline, for example, plays a key role in institutionalization, providing the basis for the creation of university departments and research centers, the development of professional societies, and the general allocation of resources. In addition to performing these crucial economic and political functions, the disciplinary category shapes the vocational identity of scientists by directing the lines of questioning they pursue, defining the tools and methods they employ, and structuring their relationships with institutions. Moreover, in the competition between fields of research, powerful but vaguely defined senses of disciplinary identity can help a research program assert primary ownership or control over experimental practices that it shares with other fields. Disciplinary categories do not just function within the domain of scientific practice, but also perform an important role in the organization, packaging, and transmission of concepts and techniques from the scientific field to outside groups, including the general public, grant-giving foundations, and governmental regulatory bodies. In these ways, amongst others, the conception of a science that is in part established through history-writing performs crucial disciplinary functions.[49]

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[47] For a brief discussion of physicists' pedagogical reinventions of Maxwell's equations, see P. Galison, "Re-Reading the Past from the End of Physics," in *Functions and Uses of Disciplinary Histories*, ed. L. Graham, W. Lepenies, and P. Weingart (Dordrecht: D. Reidel, 1983), 48.

[48] On "interactive kinds," see the fourth and fifth chapters of I. Hacking, *The Social Construction of What?* (Cambridge, MA: Harvard University Press, 1999), esp. 103-109, 115-119, 123, 130.

[49] For more on the nature and functions of disciplines, see the discussions of "scientific fields" and "credit" in P. Bourdieu, "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason," *Social Science Information* 14 (1975); "research programs" and "disciplinary programs" in T. Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines* (Stanford: Stanford

When historical claims are used to present an image of an experiment, they perform a very different type of function: the creation of virtual-witnesses.[50] In part, this type of history-writing takes place in detailed descriptions of the experiment that was performed and explanations of what should be done to replicate the experiment. But not all history is presented with words. Equally powerful are the accompanying figures, including data tables, schematic representations of the study, and images of and from the experiment. All of these aspects of the text have a shared purpose—to enable the reader to imagine an experimental scene that they did not in fact witness. And thus they must be understood as a type of history-writing that not only reports on what was seen, but also and more importantly provides the basis for a new type of seeing. Through these literary devices, the reader becomes a virtual witness, who can testify to the validity of the author's claims of fact. In this capacity, history-writing increases the number of witnesses involved in testimony so dramatically that it transforms the means by which facts are constituted.

Another important function performed by history-writing is the legitimization of claims, questions, and research methods—a function that is often achieved by redefining or referring to the canon, tradition, community, or rituals of a science. A variety of literary technologies are employed towards these ends.[51] The legitimacy of a research method, for example, can be established with a historical account of how it performed when tested against precedent and standards, thereby presenting the reader with its calibration, or by giving a causal account of the operation of the method, thereby

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University Press, 1997), 46-62; "discursive formation" in M. Foucault, *The Archaeology of Knowledge* (London: Tavistock Publications, 1972), 21-76; and "trading zones" and "intercalcation" in P. Galison, *Image and Logic: A Material Culture of Microphysics* (London: University of Chicago Press, 1997), 781-844. A detailed case study of discipline formation can be found in R. E. Kohler, *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline* (Cambridge: Cambridge University Press, 1982). For an excellent historical account of the idea of disciplines, see D. R. Kelley, "The Problem of Knowledge and the Concept of Discipline," in *History and the Disciplines: The Reclassification of Knowledge in Early Modern Europe*, ed. D. R. Kelley (Rochester: The University of Rochester Press, 1997).

[50] This idea of "virtual-witnessing" and its functions comes from S. Shapin, "Pump and Circumstance: Robert Boyle's Literary Technology," *Social Studies of Science* 14 (1984): esp. 490-497. See also S. Shapin and S. Schaffer, "Seeing and Believing: The Experimental Production of Pneumatic Facts," in *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985), esp. 60-65. This idea has also used by Robert Kohler in his analysis of work in genetics: "A strategy of literary disclosure may also have inspired the peculiar narrative form of the three monographs that Bridges and Morgan published…in which all the results were presented in chronological order, replicating the history of how mutants and maps had accreted." See R. E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (London: University of Chicago Press, 1994), 75-77.

[51] In discussing these uses of history, I draw on Jardine, *The Scenes of Inquiry*, 123-124.

illustrating its reliability; in both of these ways, history-writing is used as a means of vindication. This is very different from the type of legitimacy that is bestowed on a line of questioning when, for example, an author situates it within a historical account of the nature and purpose of the scientific community, thereby establishing its relevance. A third type of legitimization is achieved through rationalization, when a contested proposition is supported by reference to historically established explicit norms. And lastly, historical claims can be used as a means of authorization, as is the case when a proposition gains legitimacy through references to a canonical object or figure, such as a classic paper, or a founding father.

Accounts of history are also used by practitioners of a new branch of a science to justify and establish the ascendancy of their faction over the field from which it emerged.[52] A powerful form of this type of history-writing deploys the narrative plot structure of "supersessionism," which involves three general moves: first, the members of the new faction claim that their work is qualitatively different from that of their parental field; they then redefine the subject matter of the original field in terms of their new agenda; and once these redefinitions become established, they claim that the original field has failed where they succeed, and that they are thus in fact the true heirs of the original legitimate field.[53] While this supersessionist plot structure has been primarily associated with religious historiography, Scott Gilbert shows that these are precisely the order and type of claims found in the historiography of genetics by geneticists. This line of rhetoric commenced in the 1920s, when geneticists began to emphasize the qualitative differences between genetics and embryology. Within a decade, the two fields were widely seen as having their own rules of evidence, exemplars, organisms, journals, and vocabulary.[54] And with the rise of genetics in the interwar period, key hereditary phenomena came to be redefined in genetic terms: heredity was defined as the transmission of genes, no longer including development; studies of development were

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[52] Martin, "Explaining John Freind's History of Physick," and S. Gilbert, "Bearing Crosses: A Historiography of Genetics and Embryology," *American Journal of Medical Genetics* 76 (1998).

[53] Gilbert, "Bearing Crosses," 169.

[54] Ibid.: 174; e.g., although the dominant research program in embryology in the late 1920s involved the transplantation and physical manipulation of embryos, Morgan's *The Theory of the Gene* presented embryology as the study of gene expression—as a complement to genetics, which he characterized as the study of gene transmission.

said to focus on gene expression, rather than the rules by which complex structures form from simpler ones; and genetically based definitions of embryology replaced the previous morphological ones.[55] As these reconceptualizations of key ideas became established, geneticists gained the conceptual resources needed to reject much of embryology; for when important biological questions were reconceived in these new genetic terms, embryologists were unable to provide satisfactory answers. It is in this way that geneticists presented themselves as extending, completing, and supplanting the work of embryologists, thereby redefining embryology in terms of its replacement by genetics.[56]

A Structural Outline and *Précis* of the Thesis

Although historians and sociologists of science have identified rich and varied ways in which history and textbooks have been put to use, they have left many sources and questions untouched. First, there has not yet been a systematic analysis of the uses of history-writing in university science textbooks. Second, those studying the functions of textbooks have rarely analyzed actual texts or used them as primary sources in the study of twentieth-century scientific practice.[57] Finally, and most importantly for this project, the scholarship on the scientific use of history has—as identified above—focused on just one type of history-writing: explicit statements about the past, in the form of narrative histories. In building on these three areas in this thesis, I will question common conceptions of the nature of "history-writing" and what is conveyed in the textbook-teaching of "the principles of genetics." With respect to the principles of genetics, I will

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[55] Ibid.: 176.

[56] Note that this only a cursory summary of some of the main points in Gilbert's detailed analysis of the supersessionism in genetics—an argument that does not lend itself to brief summary.

[57] The notable exceptions include a brief analysis of textbook language in Myers, "Textbooks and the Sociology of Scientific Knowledge," and two studies of how molecular genetics and Watson and Crick's 1953 hypothesis concerning DNA were assimilated into the scientific literature of genetics: B. Gaster, "Assimilation of Scientific Change: The Introduction of Molecular Genetics into Biology Textbooks," *Social Studies of Science* 20 (1990); and M. Winstanley, "Assimilation into the Literature of a Critical Advance in Molecular Biology," *Social Studies of Science* 6 (1976). In addition, there is an excellent collection of articles dealing with mostly eighteenth- and nineteenth-century chemistry textbooks in A. Lundgren and B. Bensaude-Vincent, eds., *Communicating Chemistry: Textbooks and Their Audiences, 1789-1939* (Canton, MA: Watson, 2000). Note that twentieth-century *school* textbooks have been extensively studied; for an interesting study of the politics of education, see C. Cody, "The Politics of Textbook Publishing, Adoption, and Use," in *Textbooks and Schooling in the United States*, ed. D. L. Elliott and A. Woodward (Chicago: University of Chicago Press, 1990).

show how and why we might think of them not only as “the laws of genetics proposed by geneticists,” in the sense meant by the common textbook title *Principles of Genetics*, but also as “that which is most basic to being a geneticist,” including a grasp of the conceptual order of the science and the categories of family resemblance on which it is based. Regarding “history-writing,” I will argue that many representations of the past are not presented as such, and thus that the existing scholarship on the disciplinary uses of history, with its focus on narrative history-writing, only begins to illuminate the myriad ways in which scientists reconstruct and marshal history in the advancement of particular disciplinary agendas. Identifying and analyzing latent deployments of the past, I will make a significant departure from the existing literature and develop a novel taxonomy of the rich forms and functions that history-writing takes and serves. In this reorientation of previous approaches to the subject, I will push the boundaries on common conceptions of the “history” and the “writing” of history-writing, pointing the direction towards new lines of historiographical research that will allow us to better understand the ways in which the historicity of our past makes claims on us.

The central argument in my analysis will be developed across four chapters, each of which will focus on a different type of history-writing, aspect of genetics, and disciplinary function. In short, the chapters will progress from an analysis of manifest histories of genetics, to studies of various latent and embedded forms of the historical; and from the disciplining of the history of the science, to the disciplining of its students, conceptual order, and eras. In other words, this thesis will be organized as an analytic narrative about uses of history. However, I will attempt to develop this analysis in such a way that I concurrently present a historical narrative about the development of some aspects of the science of genetics: after first looking at the textbook histories of genetics that were written around the 1910s, I will explore the historical case-based style of teaching that was developed in the 1930s, followed by the use of history as a logic of organization that became ubiquitous by the 1950s, concluding with an analysis of the concept of “classical genetics” that was presented in the textbooks of the 1960s.

Having roughly outlined the structure of my analysis, I think it useful to briefly discuss the substance of each chapter. In Chapter 1, I will focus on the explicitly historical claims about past scientists, experiments, theories, facts, and discoveries that were presented in the first generation of Anglo-American textbooks to be written after the rediscovery of Mendel's laws. I will show that during this early period in which "genetics" was not yet established as a discipline, authors presented radically different accounts of the nature of the study of heredity: J. Arthur Thomson advanced a science based on knowledge of the fundamental units comprising the architecture of the hereditary material; William Bateson presented a science founded on the mathematical regularity of Mendel's laws; and William E. Castle advocated a science focused on the coming-into-being of organisms. Their different pictures of the science informed how the authors consolidated the disparate nineteenth-century studies of heredity; the same figures, theories, and discoveries were united into three distinct historical foundations, each of which provided support for a different future science. Although this extensive heterogeneity was significantly reduced in the following generations of textbooks, as key features of "the history of genetics" became standardized, aspects of Thomson's, Bateson's, and Castle's three different visions of past and future can be seen in the genetics of the following decades.

In the second chapter, I will shift from an analysis of the histories that were used to teach about the past, to an analysis of the historical, case-based method that was developed in the 1920s-1940s to convey "the science" of genetics. Here, I will explore a type of history-writing in which the historicity of the history was not always apparent. Drawing on Kuhn's idea of the exemplar, I will argue that history-writing did not only take place in explicitly historical claims about a past case, but rather extended into the illustrations, data tables, and problem-solving exercises that gave it substance. My central contention will be that these features of the text together constituted "virtual historical environments," which allowed students to learn and practice not only the principles that were studied by geneticists and were explicitly taught in the text, but also those that were followed by geneticists, and were taught only implicitly as tacit skills needed to follow, find, and understand the explicit principles. Defining disciplines as including the sets of practices and institutions that make rules executable, I will suggest that the virtual historical environments of textbooks were essential in the disciplinary reproduction of genetics.

Chapter 3 will focus on the use and development of the "standard historical approach" to teaching in the 1930s-1950s, and how it was related to the conceptual order of genetics. With this method of teaching, the textbook became a microcosm of the history of the discipline, bringing the student into the mindset of the geneticist by introducing topics in order of their historical development. The first section of this chapter will trace the history of this approach, discussing geneticists' ideas about the organization of textbooks and the significance of the wide-spread decision to begin teaching the science with Mendel's laws. In the second section, I will develop a case study of the early use of this historical approach in *Principles of Genetics* by L. C. Dunn and E. W. Sinnott. Analyzing the literary devices used in implementing this approach, I will identify a dialectical narrative structure with which key twentieth century theories and discoveries were portrayed as extensions of Mendelism, and with which the inner logic and disciplinary coherence of genetics were presented. The conceptual order of the science was not, however, stable, as I will show with a further case study of *Principles of Genetics* in the third section of this chapter. Here, I will trace the ways in which the authors, over five editions, revised their presentation of the set of experiments, facts, and principles related to the chromosome theory of heredity, showing that the hierarchical organization of evidence and theory was highly malleable and could be rearranged to create very different presentations of the purpose of the science. Taken together, the three sections of this chapter will show that history was constitutive of the organizational logic, narrative structure, and inner rationality of the science conveyed by genetics textbooks.

In chapter four, I will explore the sense of history embodied in use of the historiographical concept of "classical genetics" that was developed in textbooks of the 1960s-1970s. With this differentiation of the science into classical and modern eras, authors presented molecular genetics as a revolution in geneticists' understanding of heredity. But, as I will show, this idea of "classical genetics" was not merely an anachronistic description projected onto history. The use of the term in the 1970s was, rather, just one redefinition in a series of reappropriations of an idea that had developed over five decades. Tracing the history of the conception and development of "classical" in the language of geneticists, I will discuss how and why it was used to refer to an exemplary case in the 1920s, an ideology in the 1920s, a philosophy of science in the 1930s, a type of genetics in the 1950s, and a historical era in the 1960s. I will argue that in all of these cases, "classical" was a politically powerful concept: at first used to define and establish sources of scientific authority, it was subsequently developed in arguments about the proper relationship between science and politics, and eventually served to establish the disciplinary identity and boundaries of the science. Identifying these genealogical and functional connections is not, however, the primary purpose of this chapter. The diversity in uses of "classical" is perhaps even more important. In differentiating these various uses, I will suggest that the disciplinary power of this term—which is derived from the authority of history—relied on the effacement of its historicity and the situations in which it was created and deployed.

In the concluding chapter, I will revisit the common notion that the use of history in science textbooks facilitates learning by giving life to the historical context in which facts and theories were developed. Summarizing general aspects of my argument, I will suggest that this view is mistaken—that history-writing in genetics textbooks was not primarily about the past, but rather about the scientific community and its future. I will outline some ways in which my analysis could be extended, and conclude with a brief reflection on the forms and functions of the writing of history.

The History and Historiography of Genetics

Although my primary aim in this thesis is to advance a novel taxonomy of the forms and functions of history-writing that can be applied generally, I hope that this analysis will be of particular value to those interested in the development of Anglo-American genetics in the twentieth century—a field that I chose to study for a variety of reasons. At heart, this choice was motivated by my ethical and political interests in the relationship between genetics and contemporary medicine, and the manner in which geneticists' modes of seeing life have shaped lay people's conceptions of themselves and their bodies. By studying the early development of the ways in which geneticists presented their science—from the origin of "genetics" textbooks in the early 1900s, to the establishment of the textbook category of "classical genetics" in the mid-twentieth century—I hoped to develop a critical perspective on modern public understandings of the meaning of genetic knowledge. In addition, I was intrigued by the visibility of the non-professional historiography of genetics: it seemed, for example, that its "founding father" story has a public status unlike those of other sciences, and that *Genetics* is unique amongst comparable scientific journals in its inclusion of a historically-oriented article in every issue. Finally, the fact that geneticists have often characterized the subject matter of their science in historical terms made the field an aesthetically interesting focal point for research on the forms and functions of claims about history.[58]

While an exploration of the above themes did not fall within the scope of this project, I attempt to extend the literature on the history of genetics in ways that will assist future work in these and other areas. Thus, in concluding, it is perhaps worth noting just a few of the well-established lines of research on which I build. In Chapter One, the significant body of scholarship on the debate between the biometricians and the Mendelians at the turn of the century provides background for my analysis of the history written by Bateson in *Genetics and Eugenics*.[59] Identifying the ways in which Bateson's textbook advanced a disciplinary framework that differed radically from that of J. Arthur

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[58] See, e.g., T. Dobzhansky, "Biological Adaptation," *The Scientific Monthly* 55 (1942): 402, stating that the "gene embodies…the most fundamental, and yet frequently overlooked, attribute of the living matter: it carries its history within itself"; T. Dobzhansky, "Position Effects on Genes," *Biological Review* 11 (1936): 382, describing the chromosome as "a harmonious system which reflects the history of the organism and is itself a determining factor of this history"; R. W. Gerard, "Units and Concepts of Biology," *Science* 125 (1957): 431, describing genes and other biological units as "carriers of the past"; and W. Johannsen, "The Genotype Conception of Heredity," *American Naturalist* 45 (1911): 139, characterizing the "genotype view" of an organism as "an 'ahistoric' view of the reactions of living beings," as opposed to the "transmission view." See also, e.g., F. R. Lillie, "The Gene and the Ontogenetic Process," *Science* 66 (1927): 367, and L. C. Dunn, *A Short History of Genetics: The Development of Some of the Main Lines of Thought, 1864-1939* (New York: McGraw-Hill, 1965), xii-xiii.

[59] See, e.g., P. J. Bowler, *The Mendelian Revolution: The Emergence of Hereditarian Concepts in Modern Science and Society* (Baltimore: Johns Hopkins University Press, 1989), 68-73; W. B. Provine, *The Origins of Theoretical Population Genetics* (Chicago: University of Chicago Press, 1971), Chapters 2, 3; K. Kim, *Explaining Scientific Consensus: The Case of Mendelian Genetics* (New York: Guilford Press, 1994); D. Mackenzie, *Statistics in Britain* (Edinburgh: Edinburgh University Press, 1981); B. Norton, "Biology and Philosophy: The Methodological Foundations of Biometry," *Journal of the History of Biology* (1975); and L. Farrell, "Controversy and Conflict in Science: The English Biometric School and Mendel's Laws," *Social Studies of Science* 5 (1975). For contributions to this body of scholarship that provide excellent summaries of it, see D. J. Kevles, "Genetics in the United States and Great Britain 1890-1930: A Review with Speculations," *Isis* 71 (1980): esp. 441-445; and R. Olby, "The Dimensions of Scientific Controversy: The Biometric-Mendelian Debate," *British Journal for the History of Science* 22 (1988): esp. 301.

Thomson and William Castle, I also attempt to build on the excellent work that has been done on the struggle for authority between competing conceptualizations and approaches to the study of heredity in the early twentieth century.[60] In addition, through a careful reconstruction of changing accounts of Mendel in the preface histories of these first-generation genetics textbooks, I develop and clarify some oversights in the extensive literature addressing the representations and uses of his life and work.[61] In the following two chapters, I extend the Mendel scholarship in a new direction, exploring the pedagogical functions of the Mendelian exemplar and the narrative devices used to present Mendel's laws as the foundation of genetics. In Chapter Three's analysis of the "historical" ordering of textbooks, I also develop an analysis of textbook representations of the chromosome theory of heredity that builds on the valuable historical and philosophical literature on the rise of the chromosomal approach to genetics.[62] Finally, in Chapter Four's analysis of the conception and development of "classical genetics," I attempt to provide a new insight into the significance of the work of two controversial geneticists who have been the subject of substantial research: T. D. Lysenko and Richard Goldschmidt.[63]

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[60] See, e.g., R. Falk, "The Struggle of Genetics for Independence," *Journal of the History of Biology* 28 (1995): 219-246; J. Harwood, *Styles of Scientific Thought: The German Genetics Community 1900-1933* (Chicago: University of Chicago Press, 1993), esp. Part I; Kohler, *Lords of the Fly*, esp. Chapter 3; R. Olby, *Origins of Mendelism* (Chicago: University of Chicago Press, 1966); J. Sapp, "The Struggle for Authority in the Field of Heredity, 1900-1932: New Perspectives on the Rise of Genetics," *Journal of the History of Biology* 16 (1983): esp. 313-318; and J. Sapp, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics* (New York: Oxford University Press, 1987).

[61] See, e.g., Brannigan, "The Reification of Mendel"; Callender, "Gregor Mendel: An Opponent of Descent with Modification"; F. Di Trocchio, "Mendel's Experiments: A Reinterpretation," *Journal of the History of Biology* 24 (1991); Olby, "Mendel No Mendelian?"; and C. Zirkle, "Gregor Mendel and His Precursors," *Isis* 42 (1951).

[62] See, e.g., G. E. Allen, "T. H. Morgan and the Split between Embryology and Genetics, 1910-1935," in *A History of Embryology*, ed. T. J. Horder, J. A. Witkowski, and C. C. Wylie (New York: Cambridge University Press, 1983); S. G. Brush, "How Theories Became Knowledge: Morgan's Chromosome Theory of Heredity in America and Britain," *Journal of the History of Biology* 35 (2002): 503-506; A. G. Cock, "William Bateson's Rejection and Eventual Acceptance of the Chromosome Theory," *Annals of Science* 40 (1983); J. Gayon and R. M. Burian, "France in the Era of Mendelism (1900-1930)," *Comptes Rendus de l'Académie des Sciences, Paris, Sciences de la Vie* 323 (2000); S. Gilbert, "Bearing Crosses: A Historiography of Genetics and Embryology," *American Journal of Medical Genetics* 76 (1998); S. Gilbert, "The Embryological Origins of the Gene Theory," *Journal of the History of Biology* 11 (1978); and Harwood, *Styles of Scientific Thought*, 33-45.

[63] See, e.g., Michael Dietrich, "On the Mutability of Genes and Geneticists," *Perspectives on Science* 4 (1996): 321-345; Michael Dietrich, "From Gene to Genetic Hierarchy: Richard Goldschmidt and the Problem of the Gene," in *The Concept of the Gene in Development and Evolution: Historical and Epistemological Perspectives*, edited by P. J. Beurton, R. Falk and H. Rheinberger (Cambridge: Cambridge University Press, 2000); Michael Dietrich, "From Hopeful Monsters to Homeotic Effects: Richard Goldschmidt's Integration of Development, Evolution, and Genetics," *American Zoologist* 40 (2000): 738-

In addition to building on existing areas of research, I attempt to contribute to the literature on the history of genetics by identifying reasons for rethinking some of its basic assumptions. With my analysis of functions of the term “classical,” for example, I council for a more cautious use of a term that is generally used uncritically by historians, philosophers, and sociologists in their work on genetics. Revealing vast heterogeneity in the subject matter characterized as “classical” by geneticists, I suggest that the historiographical use of “classical genetics” in contemporary critical scholarship reifies a category that should in fact be problematized. In addition, I make a more general case for a reconceptualization of what is involved in doing “the science” of genetics. In my analysis of the development of the concept of “classical genetics,” the “historical” presentation of the subject matter of genetics, and the framing and definition of the science with preface history, I identify various ways in which authors of textbooks were involved in creating new modes of ordering key scientific concepts and theories. These chapters provide reason for rethinking the boundaries of scientific practice—for seeing the writing of textbooks and the writing of history as part of the “scientific” practices of geneticists. Because an extensive exploration of the relationships between textbook, journal, and laboratory science was beyond the scope of my project, however, these claims are often made speculatively, indicating important areas for future research.

747; S. F. Gilbert, "Cellular Politics: Ernest Everett Just, Richard B. Goldschmidt, and the Attempt to Reconcile Embryology and Genetics," in *The American Development of Biology*, ed. R. Rainger, K. R. Benson and J. Maienschein (Philadelphia: University of Pennsylvania Press, 1988); D. Joravsky, *The Lysenko Affair* (Cambridge: Harvard University Press, 1970); Z. Medvedev, *The Rise and Fall of T. D. Lysenko* (New York: Columbia University Press, 1969); and N. Roll-Hansen, *The Lysenko Effect: The Politics of Science* (Amherst: Humanity Books, 2005).

CHAPTER 1

Textbook Histories:
The Disciplining and Consolidation of the Study of Heredity

Mendel's studies were rediscovered in 1900 but their place in the life sciences was contested for decades, as the significance, importance, and meaning of Mendel's methods and conclusions were established through their subsequent use and application. The significance of "Mendel's principles" was dependent on the ability of Mendelians to show the principles' validity for a broad class of traits and organisms. The importance of Mendel's research methods was determined by the outcome of attempts to use them in a wide range of experiments. And the general meaning of his work was established by the ways in which it was positioned and affiliated with respect to other research practices, theories, and teachings in the life sciences. It was not just Mendelism, however, that was being shaped in the early twentieth century. The nature of hereditary science was itself highly malleable in the years between the rediscovery of Mendel's laws and the establishment of the chromosome theory of heredity.[1]

In this chapter I will explore the consolidation of "the study of heredity" into "genetics" in the textbooks of the early 1900s, arguing that this disciplining of the science was in part achieved through history-writing.[2] Textbook authors used explicit accounts of the history of hereditary science to identify important nineteenth century scientists as ancestors of the modern science and define the nature of their contributions, thereby establishing exemplars and key sources of authority. These histories were also used to delegitimate past work, providing a "history of errors," or "anti-canon," that was

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[1] For an overview of the competing lines of research during this period, see J. Sapp, "The Struggle for Authority in the Field of Heredity, 1900-1932: New Perspectives on the Rise of Genetics," *Journal of the History of Biology* 16 (1983): 313-318.

[2] NB: I will use the terms "the study of heredity" or "hereditary science" to refer to the field of inquiry and research as textbook authors did prior to the disciplinary deployments of "genetics."

just as important as the presentation of positive exemplars.[3] With both types of history-writing, textbook authors presented a conception of what the science was about. Once defined, the aim of the science directed the types of questions that its students asked, discouraging certain lines of inquiry. The future of the science of heredity was thus guided by conceptions of its past and, as I will show, it was with competing accounts of history that the future was contested.

This chapter will explore the explicit histories written in J. Arthur Thomson's *Heredity,* William Bateson's *Mendel's Principles of Heredity,* and William Castle's *Genetics and Eugenics*—authors and textbooks with significant roles in the formation of genetics.[4] I will show that these authors had radically different conceptions of the nature of the study of heredity, and that their different conceptions of the science informed the ways in which they interpreted primary sources and reconstructed the contemporary historiography of their field.[5] Thomson, Bateson, and Castle consolidated the same nineteenth century figures, theories, and discoveries into three distinct historical foundations, each of which provided support for a different type of hereditary science. Although this extensive historiographical heterogeneity was significantly reduced in subsequent generations of textbooks, as key features of "the history of genetics" became standardized, aspects of Thomson's, Bateson's, and Castle's three different visions of the

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[3] The idea of the anti-cannon comes from J. Rée, *Philosophical Tales: An Essay on Philosophy and Literature* (London: Methuen, 1987), 37-38. Rée suggests that philosophy is unique in that it has an anti-canon, "exemplary erroneousness is the strongest grounds for canonization," and that works accused of error are not deleted from the canon, but rather elevated as "anti-classics." It is my contention that such an "anti-canon," or history of errors, is equally important in the practice of science. Gaston Bachelard identified this history of scientific errors as "lapsed history," but did not see the ways in which it was used by modern science; for an overview of Bachelard's position, see C. Chimisso, *Gaston Bachelard: Critic of Science and the Imagination* (London: Routledge, 2001), 98-99, 148-149.

[4] The other widely-distributed first generation Anglo-American textbooks were G. A. Reid, *The Laws of Heredity* (London: Methuen, 1910), and H. E. Walter, *Genetics: An Introduction to the Study of Heredity*, 1st ed. (New York: Macmillan, 1913). Walter's textbook was in use for over thirty years, through four editions (1913, 1922, 1930, 1938) and numerous reprintings (e.g., 1915, 1916, 1925, 1928); its wide international circulation, including a translation into Japanese, was discussed by R. Clement, "Obituaries: Herbert Walter," *The AUK* 64 (1947). For a brief description of "pioneer and standard" textbooks in this period, see W. E. Castle, "Some New Books on Genetics," Review of *Genetics in Plant and Animal Improvement,* by D. F. Jones, *Principles of Genetics*, by E. W. Sinnott and L. C. Dunn, and *Animal Genetics* by F. A. E. Crew, *Science* 62 (1925): 567.

[5] Unfortunately the scope and purpose of this chapter make it infeasible to compare the histories written in the textbooks, the historical scholarship on which they were based, and the historical scholarship that was not consulted; this type of comparative analysis would certainly be interesting, and especially so for the second half of the century, when primary sources were rarely used by textbook authors, and both professional geneticists and professional historians were writing histories of genetics.

nature of the study of heredity—and its past and future—can be seen in the genetics of the following decades.

J. Arthur Thomson and Weismannism: Heredity as the Architecture of Germplasm

At the turn of the twentieth century, Sir J. Arthur Thomson—Regius Professor of Natural History at the University of Aberdeen, and Lecturer on Zoology and Biology in the Royal Colleges of Physicians and Surgeons in Edinburgh—was regularly recruited by publishers to write textbooks for college courses in the natural sciences. In this capacity he was very successful, authoring widely used texts for courses on heredity, sex determination, and general biology.[6] Thomson's *Heredity*, first published in 1908, was the first full-length textbook in English to deal with the topic of heredity after the rediscovery of Mendel's laws. It was generally well received. According to the review in *Nature*, "most of the facts on which we found our notions of heredity are set out lucidly, as are almost all the theories ever based on them."[7] In *Science*, it was said that *Heredity* "fulfils its purpose as an 'introduction to the study of heredity' excellently well."[8] And the reviewer for *Botanical Gazette*, describing it as "a broad and comprehensive treatment of the subject of heredity, a veritable mine of valuable data concisely presented and clearly discussed," suggested that its value was not only pedagogical: "the worker in these fields will find it almost indispensable for reference."[9]

When Thomson wrote *Heredity*, he was "most widely recognized as the translator of Weismann's works and the exponent of Weismannism," and his textbook was seen in

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[6] P. J. Bowler, "From Science to the Popularization of Science: The Career of J. Arthur Thomson," in *Science and Beliefs: From Natural Philosophy to Natural Science, 1700-1900*, ed. M. D. Eddy and D. Knight (Aldershot: Ashgate, 2005). Thomson's widely used textbooks include: P. Geddes and J. A. Thomson, *The Evolution of Sex* (New York: Humboldt, 1890); J. A. Thomson, *Outlines of Zoology* (Edinburgh: Young J. Pentland, 1892); P. Geddes and J. A. Thomson, *Evolution* (London: Williams and Norgate, 1911); P. Geddes and J. A. Thomson, *Sex* (New York: H. Holt, 1914); and J. A. Thomson and P. Geddes, *Life: Outlines of General Biology* (London: Harper & Brothers, 1931). Note that many of these textbooks went through multiple editions. For a discussion of the academic and publishing culture in which Thomson wrote these textbooks, see P. J. Bowler, "Experts and Publishers: Writing Popular Science in Early Twentieth-Century Britain, Writing Popular History of Science Now," *British Journal for the History of Science* 39 (2006).

[7] G. A. Reid, "Heredity," Review of *Heredity*, by J. A. Thomson, *Nature* 78 (1908): 361. See also the positive reviews of the later editions: e.g., G. H. C., "Our Bookshelf," Review of *Heredity*, by J. A. Thomson, *Nature* 104 (1919).

[8] J. P. McMurrich, Review of *Heredity*, by J. A. Thomson, *Science* 28 (1908): 212.

[9] R. R. Gates, "Heredity," Review of *Heredity*, by J. A. Thomson, *Botanical Gazette* 47 (1909): 156.

this light.[10] For example: Ernst Haeckel, under whom Thomson had trained, rejected Weismann's theory of the continuity of the germplasm, but noted that Thomson "made a thoroughgoing defense of it in his important work *Heredity*."[11] The review of the textbook in *Science* criticized Thomson for believing "that heredity can be discussed and understood at present only on the assumption of the existence of such material bases of inheritance."[12] Even Thomson acknowledged his Weismannian orientation in the preface to the textbook: "I have tried to avoid partisan handling of any theme, though I have been at no pains to conceal my general adherence to what is called Weismannism."[13] Thus, it was generally recognized at the time of publication that some of Weismann's theories were advocated in *Heredity*. As I will show, however, the Weismannian character of the textbook extended beyond Thomson's self-conscious advocacy, pervading his presentations of the nature of hereditary science and its history.

In the introductory chapter of *Heredity*, Thomson traced the origins of hereditary science back to the ancients' attempts to understand the Fates. He wrote: "In the olden days thoughtful men seemed to see the threads of life within the hands of the three sister Fates." Arguing that scientists "in the days of scientific enlightenment" still thought in terms of these three Fates, he suggested that the difference in the modern era was that the Fates had merely been reconceptualized as "heredity," "function," and "environment." These were the three "factors that determine life." They were, according to Thomson, "the three sides of the biological prism by which, scientifically, we seek to analyze the light of life, never forgetting that there may be other components which we cannot deal with scientifically, just as there are rays of light our eyes can never see."[14] The scientific study of heredity was, according to Thomson, the modern manifestation of man's never-ending attempt to understand the nature of life. This was the picture of the science's past that introduced the study of heredity and was further developed in later accounts of the history of theories of heredity.

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[10] Ibid.: 154. For more on Weismann's work and theories, see F. B. Churchill, "August Weismann and a Break from Tradition," *Journal of the History of Biology* 1 (1968).

[11] E. Haeckel, "Charles Darwin as an Anthropologist," in *Darwin and Modern Science: Essays in Commemoration of the Centenary of the Birth of Charles Darwin and of the Fiftieth Anniversary of the Publication of The Origin of Species*, ed. A. C. Seward (Cambridge, England: University Press, 1909), 140.

[12] McMurrich, Review of *Heredity*, by J. A. Thomson: 211.

[13] J. A. Thomson, *Heredity*, 1st ed. (London: J. Murray, 1908), vii.

[14] Ibid., 3.

In a chapter devoted to "History of Theories of Heredity and Inheritance," Thomson argued that the history of hereditary theory was the history of theories of the nature of the hereditary material. Emphasizing that these theories were merely "supplemented by a theory of variation," he developed a position that differed from that of many of his contemporaries who thought of heredity in terms of the study of evolution.[15] On Thomson's account, this conceptual hierarchy was misguided: "There would have been heredity even if there had been a monotonous world of Protists without any evolution at all, but there could not have been any evolution in the animate world without heredity." The physical basis of heredity was, according to Thomson, "a *condition* of all organic evolution."[16] And for this reason, the study of the germplasm was the most fundamental line of inquiry in the life sciences. In a later discussion of the aims and requirements of a theory of heredity, he wrote: "The main object of a theory of heredity is to express in as simple terms as possible the nature of the genetic relation which binds generations together, and to interpret the facts of inheritance in terms of this relation."[17] Unlike some of his contemporaries who thought about "the nature of the genetic relation" in terms of phenotypic similarity, Thomson conceived of it in terms of germinal similarity. And thus, he argued that biological research should be focused on "the architecture of inheritance…the manner in which the inheritance is organized within the germ-cells."[18] In advocating this view, Thomson appealed to the standards set in the physical sciences: "Chemists frame hypothetical conceptions regarding the structure of chemical molecules…physicists make similar mental pictures—imaginary models—of the constitution of atoms and so on. Can biologists do the same in regard to the material basis of inheritance? This is the fundamental problem of inheritance."[19] In explaining what was required of such a model, he noted: "It must be harmonious with the large generalizations of inheritance, such as Mendel's law or Galton's law."[20] Thomson neither rejected Galton's law nor saw Mendel's law as the foundation of hereditary science, as his did his contemporary William Bateson, but rather saw them as

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[15] Ibid., 397.
[16] Ibid., 4.
[17] Ibid., 395.
[18] Ibid., 396.
[19] Ibid.
[20] Ibid.

generalizations that needed to be understood in terms of the physical basis of heredity. Throughout the textbook, Thomson emphasized differences between the study of inheritance and the study of heredity.[21] The study of inheritance, according to Thomson, aimed to "compare the characters of an organism with those of its parents and its offspring" and arrive at laws that captured the facts, such as Mendel's and Galton's laws: "It is, in the main, an observational and statistical study." The study of heredity, on the other hand, tried to develop some conception of "the precise organic relation which binds generation to generation," such as Weismann's theory of the continuity of the germplasm.[22] The life sciences, Thomson suggested, should be founded in the latter.

Thomson's ideas about how to explore the nature of life and how hereditary science should develop in the twentieth century were not only informed by his conception of the physical sciences, but also by a conception of their historical development. In an introductory chapter, for example, he suggested: "one of the distinctive features of the nineteenth century has been a reduction in the number of supposed separate powers or entities."[23] Recounting a history in which the terms "Caloric", "Light", "Force", and "Matter" had been resolved into "simpler" and "more precise" concepts, he concluded that "Heredity" needed to be similarly reconceptualized:

> In view of this progress…it cannot be a matter for surprise that a biologist should affirm that to speak of the 'Principle of Heredity' in organisms is like speaking of the 'Principle of Horology' in clocks. The sooner we get rid of such verbiage the better for clear thinking, since heredity is certainly no power, or force, or principle.[24]

In this way, Thomson was attempting to distance hereditary theory from the vitalistic theories of the nineteenth century, which he explicitly criticized elsewhere: "In the popular, if not also in the biological mind, there often lurks the idea of a hypothetical agent possessing the organism and uniting the congeries of its characters."[25] He proposed, and reiterated throughout the text, that the word "heredity" should be used as "a convenient term for the genetic relation between successive generations."[26] This

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[21] Ibid., 4-6, 8-9, 13-16, 396-398.

[22] Ibid., 9

[23] Ibid., 7.

[24] Ibid., 7-8.

[25] Ibid., 4.

[26] Ibid., 6, 8.

would, according to Thomson, make the scientific terminology "quite objective" and "in line with what has occurred in other departments of science."[27] Thomson was not, however, merely removing the vitalistic tendencies in the language. His claim that heredity was "no power, or force, or principle," which he also repeated throughout the textbook, supported a picture of the science aimed at the discovery of the material architecture of heredity.[28]

In the following chapters of the textbook, Thomson projected this conceptualization of the goals of hereditary science back onto the hereditary experiments and theories of the nineteenth century, constructing a past for his hereditary science. The works of key nineteenth century figures were presented in ways that supported Thomson's understanding of the importance and purpose of the science. In a discussion of the elementary units of heredity, for example:

> It is interesting to notice that whether we consider Weismann's theory of the determinants composing the germ-plasm, or the Mendelian theory of the segregation of characters in the germ-cells, or De Vries's Mutation Theory, we are led to the theoretical conception of elementary units. And again, we find the late Professor Weldon referring to Galton's Law in the following terms: 'The Galtonian theory postulated the presence of "elements" in the germ-cells of one generation, which are of two kinds.'[29]

Here, and in individual chapters devoted to the work of Mendel, Francis Galton, and Hugo de Vries, Thomson identified Weismannian ideas in the theories of seminal historical figures.

In the "Historical Note" that introduced a chapter on the statistical study of heredity, for example, Thomson described Francis Galton—well-known, amongst other things, for his work in biometrics—as the first person to approach the Darwinian problem from a statistical point of view.[30] And in the chapter, which included extensive quotations from Galton's *Natural Inheritance* (1889), he explained the law of ancestral inheritance and the ways in which it had been developed and used in the study of heredity. It was not the specific predictions of the law, according to Thomson, that were

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[27] Ibid., 8.

[28] Ibid., 6, 8.

[29] Ibid., 91.

[30] Ibid., 313. Thomson's presentation of this history was based on J. T. Metz, *History of European Thought in the Nineteenth Century* (London: Blackwood, 1903), esp. 618, which Thomson quoted. For more on Galton's contributions to genetics, see R. S. Cowan, "Francis Galton's Contributions to Genetics," *Journal of the History of Biology* 5 (1971).

most important: "it is quite legitimate to accept *the general idea* of this Law without accepting the fixity of the fractions of partial inheritance which it expresses."[31] He identified this "general idea" using the words of Karl Pearson, who had described the law as saying: "The degree to which a parental character affects offspring depends not only upon its development in the individual parent, but on its degree of development in the ancestors of that parent."[32]

Although Thomson frequently quoted Galton's disciples in describing Galton's law, his presentation was distinctly Weismannian. At various points throughout the chapter, he discussed how the law's statistical predictions about the distribution of traits in a group compared to the distribution of germplasm in reproduction. For example, after discussing how parental hereditary contributions join in their progeny, he concluded:

> if the concept of the continuity of germ-plasm be correct, the contribution from the father is made up of the contributions of his two parents, and the contribution of the mother is made up of contributions from her two parents. And so on backwards. Thus we reach the idea, so often referred to in this volume, that an individual inheritance is a mosaic of ancestral combinations.[33]

Here, Thomson did not mention Weismann explicitly, but presented Galton's law as the conceptual extension of Weismannism, suggesting that from the Weismannian postulate, "the continuity of the germ-plasm," the central concept in the law of ancestral inheritance followed. When describing the significance of Galton's law for the study of heredity, Thomson referred to individual organisms, stating that the law "formulates the share which the various ancestors have on an average in the inheritance of any given individual organism."[34] He did acknowledge, and emphasize, that Galton's law was about statistical averages, not the materiality of individual cases: "This is a statistical conclusion, not a physiological interpretation. It deals with average heritages and applies to masses rather than to the component individuals considered separately."[35] Immediately after drawing this distinction, however, he attempted to diminish its importance: "But while Galton did not mean his Law to apply to individual cases, it must

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[31] Thomson, *Heredity*, 1st ed., 328.
[32] Ibid., 329.
[33] Ibid., 323.
[34] Ibid.
[35] Ibid., 327; see also 326.

be approximately true of a large number of individual cases in any generation."[36] Here, and elsewhere in the chapter, Thomson tried to draw links between Galton's statistical averages and the material heredity of individuals; it was in material bodies, not statistical averages, that his Weismannian theory of heredity was based. Thus, he presented the "ancestral contributions" calculated by Galton's law as though they were physical—a feature of the textbook that was noted in the review in *Nature*: "Readers of 'Heredity' will be sure to conceive a contribution as an actual something contributed to the germ-plasm by the progenitor."[37]

In the conclusion to the chapter on the statistical method, Thomson discussed the possibility of translating Galton's statistical conclusions into physiological conclusions. In response to A. D. Darbishire, an anti-Mendelian biometrician who had rejected the possibility of finding a physiological correlation to Galton's statistical laws and argued that Galton's law did not "pretend to account for anything," Thomson wrote: "Galton's statistical conclusion may 'not pretend to account for anything,' but there must be something in individual heredity to account for it." Thomson then defended those who had tried to apply Galton's law to individual cases, suggesting that the germplasm of an individual might in fact contain contributions from all of its progenitors.[38] And in the final section of the chapter, he made the link to Weismannism explicit, discussing whether Weismann's or Mendel's physiological theory fitted best with the law of ancestral inheritance. Highlighting the lack of harmony between Galton's law and Mendelism in several known cases, Thomson suggested that "a conceivable physiological interpretation" might be provided "along the lines of Weismann's germinal selection of determinants."[39] In this way, he presented a potential alliance between Weismann and Galton—and between Weismannism and the statistical approach to heredity.[40] In the following chapter on the experimental approach to heredity, he likewise linked Weismann to Mendel.

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[36] Ibid.

[37] Reid, "Heredity," 361.

[38] Thomson, *Heredity*, 1st ed., 332-333.

[39] Ibid., 335.

[40] Although most reviews did not comment on this feature of the chapter, the review in *Nature* suggested that Thomson was inconsistent insofar as he supported incompatible aspects of Weismann's and Galton's theories: Reid, "Heredity," 361.

Although Thomson's presentation of Mendel's work was informed by the writings of the central British Mendelians, Bateson and R. C. Punnett, whom he quoted regularly, he did not describe the importance of Mendel's work in the same way that they did. Because Thomson was not primarily concerned with evolution, but rather with the material basis of life, he approached Mendelism from this perspective. As in his discussion of Galton's law, his account of Mendel's work did not emphasize the discovery of constancy, order, or ratios—the central discoveries according to Bateson and Punnett. He did not state that Mendelian phenomena were laws of nature, as some advocates of Mendelism did. Rather, he characterized Mendel's theory as "a very important conclusion…which is often *briefly referred to as* 'Mendel's Law'."[41] According to Thomson, Mendel's conclusion was not a law, but rather an interpretation: "Mendel discovered an important set of facts, and he also suggested a theoretical interpretation—the theory of gametic segregation."[42] On Thomson's view, an "interpretation" was not a law, nor was it a sufficient conclusion. In the first chapter of the textbook he had argued that hereditary science needed "to pass from a model of interpretation to a causal one," and that biologists needed to replace "Principles of Heredity" with knowledge of the architecture of heredity.[43] Arguing that Mendel's "interpretation" was incomplete insofar as it failed to account for the material, causal mechanism by which segregation occurred, Thomson suggested that it be further elaborated. He asked rhetorically: "Is this not a possible expression of a struggle among hereditary items or homologous determinants, and in line with Weismann's theory of germinal selection?"[44] Advocating this theory, Thomson provided a quote from *Experimental Zoology*, in which Morgan said that it seemed "highly probable" that "the chromosomes are the vehicles of the hereditary qualities."[45]

In suggesting a physiological account of segregation, Thomson broke with Bateson, who was committed to a dynamic and holistic view of the cell and was outspoken in his skepticism towards cytological approaches to heredity. According to

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[41] Thomson, *Heredity*, 1st ed., 337 (emphasis added).
[42] Ibid., 347.
[43] Ibid., 7-8, 12.
[44] Ibid., 348.
[45] Ibid. Thomson was quoting T. H. Morgan, *Experimental Zoology* (New York: Macmillan, 1907), 72.

Bateson, Mendelism was not caused by the segregation and independent assortment of material particles located in the nucleus, which he saw as a morphological and reductionistic view; he described Mendelism as though it were the unit characters that were segregating, and only occasionally noted that it was really the "ultimate factors which cause those characters to be developed."[46] Thomson, however, nevertheless presented Bateson's "elaborations" on Mendel's work as evidence to support his cytological conception of segregation. For example, after explaining Bateson's research and theory of multiple segregating allelomorphs, Thomson suggested: "the added conception of allelomorphs or contrasted unit characters…seems to us to bring the Mendelian theory into close approximation to the Weismannian conception of the struggle and interaction and co-operation of determinants."[47] Here, Thomson tried to bring contemporary Mendelian research in line with Weismannian theory. He reinforced this connection in his summary of Mendel's work, stating that the idea that allelomorphs behaved "as if they were discrete units" was one of Mendel's four original conclusions.[48] By emphasizing this aspect of Mendel's thought, Thomson was able to later use Mendel's results to support, by analogy, Weismann's theory of determinants:

> Mendel's discoveries lead us to regard the inheritance as built up of 'items,' which may be inherited independently—*e.g.*, unit characters corresponding to the 'unit characters' of the organism…These correspond to Weismann's primary constituents or determinants—the germinal representative of the independently heritable and independently variable characters of the organism.[49]

Drawing this parallel between their theories, Thomson connected Mendel with Weismann. On this account, the most important consequence of Mendel's discovery was not its contribution to knowledge of variation, the mechanism of evolution, or the mathematical order underlying heredity—as many of his contemporaries suggested—but rather its significance for the study of the architecture of heredity.[50]

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[46] W. Bateson, *Mendel's Principles of Heredity*, 1st ed. (Cambridge, England: University Press, 1909), 11.

[47] Thomson, *Heredity*, 1st ed., 356.

[48] Ibid.

[49] Ibid., 373. See also Thomson, *Heredity*, 1st ed., 369, where he wrote that Mendel's work "corroborates Weismann's picture of an inheritance as composed of numerous sets of determinants or primary constituents, each corresponding to an independently variable and heritable structure."

[50] See Thomson, *Heredity*, 1st ed., 370-373, where the impact of Mendelian research on evolutionary theory was only discussed briefly.

Thomson's textbook did not go unnoticed by the British Mendelians, who understood the science of heredity and the significance of Mendel's work in very different terms. And the year after *Heredity* was published, R. C. Punnett added a scathing review of the textbook to a new edition of his widely-read *Mendelism*.[51] Describing Thomson's treatment of the subject as "unsatisfactory and unsatisfying," and providing extensive criticisms of what he considered to be its many errors, Punnett stated that it was written "without the knowledge which actual contact with the facts can only supply."[52] One of his main criticisms was that Thomson had failed to organize the material in a clear plan, which would have been acceptable in the nineteenth century, according to Punnett, because the "facts of heredity then formed a confused medley, without central thread or clue by which they could be related to one another." But with the rediscovery of Mendel's laws, Punnett argued, the study of heredity had been given an order that should be represented in its textbooks.[53] Presumably, the order Punnett had in mind was something like that found in the textbook he recommended in his preface—*Mendel's Principles of Heredity,* which was authored by his colleague William Bateson and published in 1909.

William Bateson and Mendelism: Genetics as the Laws of Inheritance

William Bateson was described as "the real founder of the science of genetics" by William Castle, and as the "*vox clamantis in deserto*" by L. C. Dunn.[54] And he was, in fact, involved in all aspects of the earliest phase of the consolidation of genetics.[55] It was Bateson who proposed the name "genetics" to mark the disciplinary independence of what had once been seen as a branch of physiology. He introduced the terms "allelomorph," "heterozygous," and "homozygous," giving names to concepts that

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[51] R. C. Punnett, *Mendelism*, 1st American ed. (New York: Wilshire, 1909), 90-99. This review was originally published as a magazine article: R. C. Punnett, "Old Bottles," *The New Quarterly*, October 1908.

[52] Punnett, *Mendelism*, 1st American ed., 91, 98.

[53] Ibid., 91.

[54] L. C. Dunn, *A Short History of Genetics: The Development of Some of the Main Lines of Thought, 1864-1939* (New York: McGraw-Hill, 1965), 64; and W. E. Castle, "The Beginnings of Mendelism in America," in *Genetics in the Twentieth Century*, ed. L. C. Dunn (New York: Macmillan, 1951), 60.

[55] For an account of Bateson's contributions written by a first-generation geneticist, see T. H. Morgan, "William Bateson," *Science* 63 (1926).

became essential for geneticists, and were often later associated with Mendel.[56] As director of the John Innes Horticultural Institute, he led one of the major British research programs on heredity and genetics. His textbook, *Mendel's Principles of Heredity*, was a standard throughout the first decades of the twentieth century, and was described by William Castle as "the authoritative interpretation of Mendelism."[57] A review in *Botanical Gazette* agreed, identifying it as "indispensable for reference by all students of heredity as a compendium of Mendelian phenomena."[58]

Mendel's Principles did not just document Mendelism, however, but also polemically discredited other lines of research, presenting and advocating a new discipline of "genetics." When Bateson wrote the textbook, a decade-long conflict between British Mendelians and biometricians was coming to its end. Of the presumably numerous causes of the conflict, the one that provided the terms in which it was conducted was a disagreement about the hereditary mechanism by which evolution occurred.[59] Karl Pearson and W. F. R. Weldon, central figures in the British school of biometrics, argued that selection could shift the mean phenotype towards which a population regressed, thereby causing gradual evolution. The statistical basis for this model came from Pearson's modifications to Galton's law of ancestral heredity, and the data was provided by Weldon's experiments. Bateson and his Mendelian colleagues vehemently rejected this model, arguing that evolution could only occur through saltative transmutation. The results of Bateson's morphological experiments in the 1890s suggested that evolution could not be a process of gradual adaptation, and the

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[56] See, e.g., R. C. King, *Genetics*, 1st ed. (New York: Oxford University Press, 1962), 68.

[57] W. E. Castle, Review of *Mendel's Principles of Heredity*, by W. Bateson, *Science* 40 (1914): 246. See also W. E. Castle, Review of *Mendel's Principles of Heredity*, by W. Bateson, *Science* 30 (1909): 481. *Mendel's Principles* was published in 1909, and reprinted with additions in 1913.

[58] R. R. Gates, "Mendelism," Review of *Mendel's Principles of Heredity*, by W. Bateson, and *Mendelism*, by R. C. Punnett, *Botanical Gazette* 48 (1909): 62.

[59] Many factors other than this "scientific disagreement" have been identified as the deeper cause of the conflict, including personality clashes, limited academic resources, research programs informed by different socio-economic ideologies, incompatible philosophies of science, and divergent methodological commitments. See, respectively, W. B. Provine, *The Origins of Theoretical Population Genetics* (Chicago: University of Chicago Press, 1971), Chapters 2, 3; D. J. Kevles, "Genetics in the United States and Great Britain 1890-1930: A Review with Speculations," *Isis* 71 (1980); D. Mackenzie, *Statistics in Britain* (Edinburgh: Edinburgh University Press, 1981); B. Norton, "Biology and Philosophy: The Methodological Foundations of Biometry," *Journal of the History of Biology* (1975); and L. Farrell, "Controversy and Conflict in Science: The English Biometric School and Mendel's Laws," *Social Studies of Science* 5 (1975). For a summary of this historiography, see Kevles, "Genetics in the United States and Great Britain 1890-1930," 441-445; and R. Olby, "The Dimensions of Scientific Controversy: The Biometric-Mendelian Debate," *British Journal for the History of Science* 22 (1988): esp. 301.

rediscovery of Mendel's work in 1900 provided evidence of the hereditary mechanism that could drive discontinuous evolution. The disagreement between the Mendelians and biometricians turned into a significant conflict in the early 1900s, unfolding in the academic, public, and private domains.[60] It took place in struggles over the control of scientific institutions, such as the Royal Society's Evolution Committee: founded by Galton, and controlled by Weldon and Pearson in the late nineteenth century, the committee was taken over by Bateson in 1900.[61] The controversy also surfaced in struggles over journals and publishing, such as when Pearson took extreme measures to prevent the publication of pro-Mendelian research in *Biometrika*.[62] Throughout the conflict, Bateson used experimentation as a polemical tool with which to exclude biometricians from the field, denying the legitimacy of purely statistical approaches to heredity and evolution.[63] And he continued to advance this position when writing about the history of hereditary science in *Mendel's Principles*.

The history of the study of evolution presented in *Mendel's Principles* suggested that studies of the origin of species belonged within the domain of experimental studies of heredity, and outside the scope of statistical analysis and descriptive natural history. This division of fields was justified by two sets of historical claims. In the first, Bateson identified evolutionary science as having its origins in the domain of naturalists using experimental science to study the possibility of common descent. He argued that the phrases "evolution" and "origin of the species" should not be associated with Darwin, but rather with these naturalists working in the decades before the publication of *Origin of the Species*: "If we could ask those men to define the object of their experiments, their answer would be that they were seeking to determine the laws of hereditary transmission with the purpose of discovering the interrelationships of species." These naturalists, according to Bateson, observed the visible structures of organisms but were interested in the "hidden properties of living things" and had, by the mid-nineteenth century,

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[60] Kevles, "Genetics in the United States and Great Britain 1890-1930," 448-449.

[61] A. Brannigan, "The Reification of Mendel," *Social Studies of Science* 9 (1979): 433.

[62] Kevles, "Genetics in the United States and Great Britain 1890-1930," 448-449.

[63] P. J. Bowler, *The Mendelian Revolution: The Emergence of Hereditarian Concepts in Modern Science and Society* (Baltimore: Johns Hopkins University Press, 1989), 68-73. See also Sapp, "The Struggle for Authority in the Field of Heredity, 1900-1932," esp. 330-332.

developed breeding techniques that brought them close to discovering the mathematical order hidden within hereditary phenomena.[64]

Bateson's presentation of the first half of the nineteenth century as an era of experimental breeders interested in the origin of species set up a second claim—that Darwinism took hold of the study of the species problem, leading naturalism into a period of darkness. According to Bateson, "the experimental study of the species problem" was "in full activity" when Darwin published the *Origin of the Species*; but because Darwin showed that the theory of evolution was "plainly deducible from ordinary experience," he made "the reality of the process…no longer doubtful." It was because of this "triumph of the evolutionary idea," Bateson argued, that naturalists gradually lost their interest in experimental breeding. After the 1860s, their work on the species problem was "practically abandoned" in years "marked by apathy characteristic of an age of faith."[65] Darwinism not only failed to provide a satisfactory account of evolution, but also stifled the progress of experimental breeding, leading to "years of wasted effort," on Bateson's account: "a separation was effected…between those who lead with theoretical opinion and those who…have retained an acquaintance with the facts," or in other words, between statisticians and experimental researchers.[66] Amongst the "grievous" consequences of this separation, he highlighted one: "the conclusion that evolution must proceed by insensible transformation of masses of individuals has become an established dogma."[67]

Bateson not only criticized Darwin, but also those who—following in the Darwinian tradition—advocated gradual evolution and pursued non-experimentally based studies of heredity.[68] It was in this context that the biometricians were condemned: "Of the so-called investigations of heredity pursued by…Pearson and the

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[64] Bateson, *Mendel's Principles of Heredity,* 1st ed., 2.

[65] Ibid., 2-3.

[66] Ibid., 3.

[67] Ibid., 3-4. A similar account was given in R. C. Punnett, *Mendelism*, 3rd ed. (London: Macmillan, 1911), 11: "Indeed, the effect of Darwin's *Origin of the Species* was to divert attention from the way in which species originate…biologists accepted the notions of variation and heredity there set forth and ceased to take any further interest in the work of hybridizers."

[68] For a general discussion of anti-Darwinism at the turn of the century, see P. J. Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900* (London: Johns Hopkins University Press, 1983).

English Biometrical school it is now scarcely necessary to speak."[69] Their statistical method, according Bateson, "resulted only in the concealment of that order which it was ostensibly undertaken to reveal."[70] Those who later studied the history of science would find it "inexplicable that work so unsound in construction should have been respectfully received by the scientific world." The discovery of Mendelian segregation had made it "obvious" that their methods were "useless." Faced with these facts, biometricians had been forced to either "abandon their delusion" or "deny the truth of Mendelian facts." In "choosing the latter course" they had succeeded in delaying recognition of the value of Mendelism, but their denials had eventually "lost their dangerous character" and had come to be "regarded as merely formal."[71]

Bateson's version of the history of biometrics was not undisputed, but neither was it seen as factional. William Castle, a zoologist at Harvard who was not involved in the dispute, wrote in his book review: "The present work omits the controversial features of its predecessor, which happily are no longer required."[72] Bateson's earlier text, *Mendel's Principles of Genetics: A Defence,* had been seen as both a defense of the Mendelian approach and an attack on Weldon. But in 1909, Castle noted without disagreement that Bateson merely censured biometricians "for persistently closing their eyes to the fact that Mendel has opened a path."[73] On Castle's view, Bateson's criticisms of biometrics were not polemical and factional, but reasonable.

Thus far, I have shown that Bateson projected the early twentieth century conflict between biometricians and Mendelians onto the nineteenth century study of heredity. He periodized the nineteenth century, dividing the work of naturalists into experimental and theoretical schools of thought. He used history-writing, rather than overt argument, to suggest that the theory and statistical approach of Darwin and the Neo-Darwinians obstructed progress. By delegitimating this line of research, he began to present a history of Mendelian science that was distinct from the general history of the hereditary sciences.

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[69] Bateson, *Mendel's Principles of Heredity,* 1st ed., 6.
[70] Ibid., 6-7.
[71] Ibid., 7.
[72] Castle, Review of *Mendel's Principles of Heredity*, by W. Bateson: 482.
[73] Ibid.

By partitioning the past, he began to identify disciplinary boundaries for the present and future—boundaries that he further defined in his historical discussion of Mendel.

Bateson's account of nineteenth century naturalists and Darwinian theory provided the stage on which he elevated Mendel and Mendelian genetics. According to Bateson, the early hybridizers' work on evolution and the species problem culminated in the experiments of Mendel. Bateson presented Mendel as someone interested in solving the question of the origin of species—not as someone trying to discover the laws that govern heredity, as he would be presented in the next decade. Bateson supported this account with a quotation of Mendel saying, "This much I *do* see, that nature cannot get on further with species-making in *this* way. There must be something more behind."[74] Bateson, who read this statement as implying that Mendel was interested in developing an alternative to Darwinism, then concluded: "With the views of Darwin which at that time were coming into prominence Mendel did not find himself in full agreement, and he embarked on his experiments with peas."[75] The claim that Mendel set out to challenge Darwin's theory set up an opposition between the ideas of Mendel and Darwin, thereby creating a historical precedent for the Mendelians' conflict with the Darwinian biometricians. In addition, it allowed Bateson to define the aspects of Mendel's work that he hoped modern studies would emulate.

According to Bateson, Mendel had realized that the early hybridizers' failure to reach "definite and consistent conclusions was due to a want of precise and continued analysis." Explaining that Mendel had therefore started with pure-breeding and homogenous materials, studied each character separately, divided the generations into clear groups, and recorded the results from each individual separately, Bateson identified the basic steps of the breeding experiments he was advocating.[76] He emphasized that it was this systematic experimental approach that had allowed Mendel to discover the laws of segregation and independent assortment, stating that the connection between the experimental data and the 3:1 and 9:3:3:1 ratios was obvious: "The conclusion which Mendel drew from these observations is one which will suggest itself to anyone who

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[74] Bateson, *Mendel's Principles of Heredity,* 1st ed., 311 (emphasis original).

[75] Ibid.

[76] Ibid., 7.

reflects on the facts."[77] Bateson further suggested that these ratios clearly indicated the operation of a universal orderly system, and that it was also obvious that this system involved independently segregating units. The ratios were, according to Bateson, "exactly what would be expected" if the germ cells were carrying either the dominant or recessive character but not both.[78] This account of history "naturalized" the meaning of Mendel's data and the significance of his laws, suggesting that they would have been apparent to any neutral observer at anytime—that it was only the dogma of the Darwinian age that prevented this recognition. The naturalization of this knowledge was supported by the story of the rediscovery of Mendel. The simultaneous and independent discovery of the same phenomena in three countries suggested that these truths were natural features of the world, independent of culture and waiting to be rediscovered.[79]

Mendel's studies were paradigmatic of the experimental biology Bateson advocated, and provided strong support for his attempts to reorient the study of the biological sciences.[80] Bateson did note, however, that the reasons why nineteenth century naturalists pursued their studies were not the same as the reasons for which he was advocating Mendelian research. He explained that while he shared their interests in the mechanism of evolution, he had developed a different set of primary concerns: "The time has now come when appeals for the vigorous prosecution of this method should rather be based on other grounds. It is as directly contributing to pure physiological science that genetics can present the strongest claim."[81] As he explained in the preface to

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[77] Ibid., 10. See also Punnett, *Mendelism*, 3rd ed., 11: "Had Mendel's paper appeared a dozen years earlier it is difficult to believe that it could have failed to attract the attention it deserved."

[78] Bateson, *Mendel's Principles of Heredity,* 1st ed., 10.

[79] Ibid., 7. For further discussion of this theme, see P. J. Bowler, *The Mendelian Revolution: The Emergence of Hereditarian Concepts in Modern Science and Society* (Baltimore: Johns Hopkins University Press, 1989), 103, in which the Mendel story is described as "a myth created by the early geneticists to reinforce the belief that the laws of inheritance are obvious to anyone who looks closely enough at the problems."

[80] In writing this history of the study of heredity and Mendel's experiments, Bateson not only wrote a polemic that advanced his vision of the science of genetics, but also created an exemplar with which to teach it. For example: he not only suggested that the principles were obvious to anyone who saw the data, but also, and more importantly, taught the student to *see* the principles as a natural feature of the data. I will return to this topic in the next chapter.

[81] Bateson, *Mendel's Principles of Heredity,* 1st ed., 4. The distinction between what Bateson claimed for twentieth century genetics and for Mendel has been lost in some historical revisionist work on the rediscovery of Mendel. See, e.g., Brannigan, "The Reification of Mendel," in which he argues that Mendel's work was neither ignored in the 1860s nor simply re-discovered in 1900. According to Brannigan, Bateson reconstructed Mendel in terms of his own work on variation and natural selection and thereby decontexualized Mendel from the hybridizing tradition in which his work would have been

the textbook, Mendelian research would continue to have relevance for "the great problems of Biology…especially that of Evolution, and the nature of Variation."[82] But, Bateson suggested, the future importance of genetics had to do with its ability to reveal the order of heredity: "we have reached a point from which classes of phenomena hitherto proverbial for the seeming irregularity can be recognized as parts of a consistent whole."[83] According to Bateson, the "fact of segregation was the essential discovery which Mendel made," and it was significant for two reasons: because it explained "discontinuity" in the variation of animals and plants, and more importantly, because it identified "regularity" in hereditary transmission.[84] Through the application of the principle of segregation, the "vast medley of seemingly capricious facts" of heredity was being "shaped into an orderly and consistent whole."[85] Genetics had become "an organized branch of physiological science" that was disclosing "a new world of intricate order previously undreamt of."[86]

In addition to defining the significance of Mendel's laws in the context of studies of heredity, Bateson spoke about their meaning for the life sciences in general. He argued that Mendel's experiments and the science of genetics would place biology on a new scientific foundation, comparable to those of physical sciences. About Mendel's laws he wrote, "The experiments which led to this advance are worthy to rank with those which laid the foundation of the atomic laws of chemistry."[87] Through Mendel's discovery, the science of heredity was becoming a science based in physical laws, thus providing a foundation for biology: "workers in various departments of biology will realize that here at last is common ground."[88]

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understood, making his work appear revolutionary for its time. But, as I have shown here, Bateson did not in fact "reconstruct" or "overlook the original intent of" Mendel's work.

[82] Bateson, *Mendel's Principles of Heredity,* 1st ed., vi.

[83] Ibid., v.

[84] Ibid., 13.

[85] Ibid., 17. He reiterated this point throughout the text. See, e.g., Bateson, *Mendel's Principles of Heredity,* 1st ed., 5: "every fragment of solid evidence will quickly take its place in the development of a coordinated structure."

[86] Bateson, *Mendel's Principles of Heredity,* 1st ed., 17.

[87] Bateson, as quoted in Thomson, *Heredity*, 1st ed., 336; see also: "The breeding-pen is to us what the test-tube is to the chemist—an instrument whereby we examine the nature of our organisms and determine empirically their genetic properties."

[88] Bateson, *Mendel's Principles of Heredity,* 1st ed., 4.

Bateson did not only use history-writing to promote a vision of Mendel's work and exclude certain traditions and ideas from the study of heredity, but also to identify who was part of the scientific tradition and why. It was in this way that he further defined the goals and aims of the future science. On his account, only a few notable advances were made between 1870-1900, the most important being attributable to Galton, Weismann, and De Vries. By identifying what he saw as their important contributions, Bateson shaped them into the historical foundation of future Mendelian science. He praised Weismann, for example, on the grounds that he had challenged advocates of the inheritance of acquired characteristics to provide experimental proof for their theory. It was Weismann's challenge to the commonly accepted dogma of Darwin and his contemporaries that revealed the "utter inadequacy" of the evidence on which these beliefs were based. This was, according to Bateson, Weismann's only contribution to the science of heredity: "Weismann's contribution, though negative, has greatly simplified the practical investigation of genetic problems."[89] He did not identify any positive proposals made by Weismann, such as his theories concerning the continuity of the germplasm and hereditary determinants—the proposals for which he was most well known, and the ideas around which Thomson had based his textbook. Bateson was committed to a holistic view of the gametic cell, and rejected the chromosomal theory of inheritance as well as Weismann's suggestion that the germplasm was particulate. Bateson, and many of his contemporaries, saw these ideas as related to the preformationist theories of the nineteenth century. Thus he distanced his new science, and its historical foundations, from these apparently old-fashioned ideas; he removed Weismann's work from the nineteenth century studies of cytoplasm, and placed it instead within the experimental study of the cause of variation. Bateson brought Weismann, but not most of his theories, into the historical foundations of Mendelian science.

Mendel's Principles also identified Galton as a key figure in the history of the modern science of genetics. While Bateson criticized the biometricians' extensions of Galton's work, he described Galton's work in a manner amenable to his conception of genetics. According to Bateson, Galton made key contributions insofar as he sought to

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[89] Ibid., 5. Punnett also said that Weismann's work "will be remembered for one notable contribution to the subject." See Punnett, *Mendelism*, 3rd ed., 12.

develop "exact knowledge of heredity" and formulated a mathematical model that quantified the hereditary relationship between generations: "He pointed out that the phenomena manifested regularity, and he made the first comprehensive attempt to determine the rules they obey."[90] Bateson emphasized the mathematical predictions made by Galton's law: "It was through his work and influence that the existence of some order pervading the facts became generally recognized."[91] Here, Bateson described Galton's work in the same terms that he used to discuss Mendel's. Presenting Galton as someone who sought to find law-like order in heredity and proposed such a law, Bateson brought Galton into the history of the modern science of Mendelism. With these claims, he presented a picture of Galton's work that differed significantly from Thomson's. Whereas Thomson highlighted Galton's law of ancestral heredity because of its bearing on questions about the material basis of heredity, Bateson commended Galton for attempting to identify mathematical ratios beneath the surface of heredity. Whereas Thomson presented the mathematical generalizations of Galton's law as its greatest weakness, Bateson treated them as being one of its most significant strengths. Thomson had hoped that the law, which had been designed to identify phenotypic patterns between averaged generations, might be expanded to identify germplasm patterns between individuals. Bateson, on the other hand, thought that there was little such correspondence: acknowledging that there was "admittedly a statistical accord between Galton's theory and some facts of heredity," he argued, "no one familiar with breeding or even with the literature of breeding could possibly accept that theory as a literal or adequate presentation of the facts."[92] Their presentations of the law were shaped by their visions of the purpose of hereditary science. On Bateson's account, Galton's law was not a conceptual extension of Weismannism, but rather an early step towards a hereditary science based on physical laws.

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[90] Bateson, *Mendel's Principles of Heredity,* 1st ed., 5.

[91] Ibid., 5-6.

[92] Ibid., 6.

William E. Castle and Evolution: Genetics as the Coming into Being of Organisms

At the time of Weldon's death in 1906, the Biometry-Mendelism controversy was dying down in Britain, and Mendelian research was growing rapidly in the United States, where it received support from research foundations that had been established in the 1890s, the growing interest and involvement of American farms and breeders, and government funding for agricultural research made possible by the Adams Act of 1906. This institutional wealth not only provided a direct stimulus for research, but also created an environment in which advocates of different methodological and theoretical orientations often cooperated. The US quickly became the center of progress in Mendelian research; by 1907, British Mendelians were avidly reading American journals, such as *The American Naturalist* and *Science*.[93]

The disciplinary development of genetics in the US was significantly shaped by the work of William E. Castle, head of the Bussey Institution at Harvard.[94] Like Bateson in Britain, Castle was involved in every aspect of the institutionalization of genetics. In 1903, he published what would soon after be considered the first scientific journal article on genetics in the United States, and in 1909, he published a widely-celebrated article proving the validity of Weismann's distinction between germ and somatic tissues. He was the first to use *Drosophila* for any extensive laboratory experiments, and introduced the organism to Morgan, who subsequently led the *Drosophila* revolution in genetics. Castle also pioneered the use of mice in genetics, supervising the use of mice as a research subject by Little, who was later largely responsible for turning the mouse into a standard experimental animal in biology and genetics.[95] As a professor at Harvard, Castle taught one of the first university courses in the US devoted to genetics, "Genetics

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[93] On the institutionalization of genetics in the United States, see Kevles, "Genetics in the United States and Great Britain 1890-1930," 450-455; Sapp, "The Struggle for Authority in the Field of Heredity, 1900-1932," 332-341; J. Harwood, *Styles of Scientific Thought: The German Genetics Community 1900-1933* (Chicago: University of Chicago Press, 1993), 143-145; and C. Rosenberg, "The Social Environment of Scientific Innovation: Factors in the Development of Genetics in the United States," in *No Other Gods* (London: Johns Hopkins University Press, 1997).

[94] Castle's contributions and importance are discussed in L. C. Dunn, "William Ernest Castle, 25 October 1867–3 June 1962," *Biographical Memoirs of the National Academy of Sciences* 38 (1965).

[95] R. E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (London: University of Chicago Press, 1994), 23; and K. Rader, "The Mouse People: Murine Genetics Work at the Bussey Institution, 1909-1936," *Journal of the History of Biology* 31 (1998): esp. 327-329, 553-554. See also K. Rader, *Making Mice* (Princeton: Princeton University Press, 2004).

and Eugenics," which drew 134 undergraduate and graduate students in 1912-1913.[96] And three years later, he published the first edition of a textbook with the same name, *Genetics and Eugenics*, which was reprinted twice within the year and went through four editions by 1931.[97] This textbook was, according to a review for *Science* by eminent Cold Spring Harbor geneticist C. B. Davenport, "probably the standard college text-book covering the whole field in a broad fashion," and it was used widely through the 1930s.[98] But even after his textbook ceased to be used, Castle's influence on the teaching of genetics continued through the work of his students; L. C. Dunn, for example, co-authored *Principles of Genetics,* the standard textbook of the 1930s-1950s.[99]

The introduction to *Genetics and Eugenics* began with a definition: "Genetics may be defined as the science which deals with the *coming into being* of organisms."[100] While Castle did not fully explain whether he meant individual organisms or types of organisms, he clarified his position when he stated that genetics "does not refer…to the first creation of organic beings, but rather to the present and every-day creation of *new individuals or new races*."[101] With this definition of genetics, Castle presented a substantive connection between the coming into being of new individuals and the coming into being of new races—between ontogeny and phylogeny. He did not propose the connection as strongly as Haeckel in his theory of recapitulation, but did suggest that the two were both the subject of the same science. His conception of hereditary science differed radically from that of Thomson and Bateson: genetics was not about the germplasm or the constancy of nature, according to Castle, but rather about development—whether on the scale of individuals, or species.

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[96] "Reports of the President and the Treasurer of Harvard College, 1912-1913: The Zoological Laboratory," (Harvard University Press, 1913), 215. In 1916, Castle introduced a new graduate-only course titled "Genetics."

[97] New editions were published in 1920, 1924, and 1930. Numerous reprintings include those in 1916, 1917, 1917, 1921, 1927, and 1929.

[98] C. B. Davenport, Review of *Genetics and Eugenics*, by W. E. Castle, *Science* 61 (1925): 542.

[99] See also, e.g., W. R. Singleton, *Elementary Genetics* (New York: D. Van Nostrand, 1962), vii, where he notes his indebtedness to Castle; and H. E. Walter, *Genetics: An Introduction to the Study of Heredity*, 4th ed. (New York: Macmillan, 1938), vii, where he explains that the textbook is "the direct outcome of some years of inspiring association with Professor William E. Castle of Harvard University."

[100] W. E. Castle, *Genetics and Eugenics: A Text-book for Students of Biology and a Reference Book for Animal and Plant Breeders*, 1st ed. (Cambridge, MA: Harvard University Press, 1916), 1 (emphasis original).

[101] Ibid. (emphasis added).

In conceptualizing, structuring, and promoting his account of genetics as a science of "coming into being," Castle used and reshaped the idea of evolution. In contrast to Bateson and Thomson who, for different reasons, saw studies of heredity as providing the foundation of the life sciences, Castle suggested: "From the philosophical standpoint genetics is only a subdivision of evolution."[102] To understand how and why Castle presented a different picture, we must note that this statement was not only about the disciplinary place of genetics in the life sciences, but also about the way in which he envisioned what it was that geneticists studied. In both respects, his claim was unlike those of Thomson or Bateson, for Castle was not only referring to Darwin's theory of organic evolution when he spoke of evolution. His account of the history of the theory of evolution redefined its basic content. Like Bateson, Castle emphasized that Darwin was not the first to propose a theory of evolution: "the idea of organic evolution had often been suggested before his time."[103] In Castle's textbook, however, this statement was not part of a criticism of Darwinian theory. Castle was, rather, broadening the notion of evolution, and thus setting up very different relationships between studies of evolution and studies of genetics. According to Castle, the evolutionary idea was hundreds of years old, originating in theories about the inorganic world:

> The principle of evolution had long been recognized in relation to inorganic things. In chemistry, physics, and astronomy, the constancy and indestructibility of matter were fully established. It was recognized that more complex states of matter…may arise out of the simple 'elements'…and that out of such compounds the elements may by suitable means be recovered again unchanged and in the original proportions.[104]

Here, Castle suggested that the study of "evolution" was the study of one of the most fundamental processes in the organic and inorganic world. This was a point that he reiterated throughout the textbook. For example: "the evolution theory teaches…that all things, organic and inorganic, are constantly undergoing change, yet nothing wholly new comes into being, for everything new arises out of something which existed before."[105] Given this broad sense of the nature of evolution, Castle defined the theory of evolution, with emphasis, as: "*an attempt to explain the present condition of the world in terms of*

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[102] Ibid., 4.
[103] Ibid., 7.
[104] Ibid.
[105] Ibid., 4.

simpler pre-existing conditions."[106] With evolution conceived in these terms, the study of genetics became just one way of thinking about how complexity emerged out of elements that remained in some sense the same. In this presentation of genetics, Castle highlighted an aspect of genetics that Thomson also thought was central—the partial constancy of the germplasm beneath the somatic change. But whereas Thomson saw the partial continuity of the germplasm as germinal stability over time, Castle saw this as gradual but constant change.[107]

Castle's organization of subject matter in *Genetics and Eugenics* was based on his interpretation of the nature of evolutionary theory and its relation to his conception of genetics as a science of "coming into being." Thus the first six chapters discussed, in order, Darwin's theory of evolution, other contributions to the theory of evolution, research on acquired characters, Weismann's theory of heredity, the study of variation, and mutation theory. These ideas were presented as a continuous, rational, conceptual development: Darwin proposed the theory of natural selection, this theory needed an account of the origins of variation, Lamarck's theory of acquired characteristics provided one, Weismann disproved Lamarck's theory and developed an alternative account based on modification of germ cells, and De Vries built on Weismann's account with his theory of mutation. Placing this disparate set of theories into a linear narrative, Castle framed each one in terms of its relation to evolutionary theory. For example, the chapter on Weismann began by focusing on the emergence of new organisms: "Weismann believed that a new type of organism arises only in consequence of the origin of a new type of cell."[108] The chapter on mutation likewise opened with new species: "The theory that new races and species originate discontinuously, and not gradually, has received its strongest support from the work of the Dutch botanist, Hugo de Vries."[109] And the chapter on acquired characters began by defining Lamarck as "the greatest evolutionist before Darwin."[110] In these chapters, Castle brought key historical figures from the

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[106] Ibid., 7 (emphasis original).

[107] A similar view was presented in H. E. Walter, *Genetics: An Introduction to the Study of Heredity*, Rev. ed. (New York: Macmillan, 1922), 18: "*The most invariable thing in nature is variation*" (emphasis original).

[108] Castle, *Genetics and Eugenics*, 1st ed., 47.

[109] Ibid., 71.

[110] Ibid., 18.

nineteenth century into a narrative about knowledge of evolution and the coming into being of organisms. He did not only present the progression of ideas about evolution as a logical narrative, but also as a historical one, even when the ideas discussed in the chapters were not in chronological order. For example: some of the ideas discussed in the second and third chapters, such as Lamarck's theory of inherited characters, were developed prior to Darwin's theory of evolution, which was the focus of the first chapter. However, Lamarck's theory was presented in terms of its relevance in a post-Darwinian era. Consequently, these chapters read like a historical progression of ideas about the ways in which change and complexity originate.

Castle provided extensive accounts of the history of the subject matter, including extended and sometimes multiple-page quotations from both primary scientific sources, such as Darwin's and Lamarck's works, and histories of science, such as H. F. Osborn's 1893 *From the Greeks to Darwin* and R. H. Lock's 1906 *Recent Progress in the Study of Variation, Heredity and Evolution.* For example: he included a three page quotation from Darwin's *Variation of Animals and Plants under Domestication*, in order to provide the student with a sense of "how some of these evidences first presented themselves to Darwin's mind and how he came later to value them."[111] In addition, Castle provided a step-by-step explanation of how Darwin's ideas developed, beginning with his original formulation of the theory of natural selection in *Origin of the Species*, and ending with Darwin's later speculations about the cause and inheritance of variation. The extensive use of primary and secondary sources in Castle's historical claims did not, however, mean that he did not shape and present key nineteenth century figures in line with his vision of the discipline of genetics. On the contrary, he "disciplined" many of the same figures as Thomson and Bateson. And his focus on "coming into being", rather than statistical or material constancy, involved reinterpreting the work of Mendel, Weismann, and Darwin in ways that Bateson and Thomson had not.

Castle's account of Mendel's experiments was nearly antithetical to Bateson's on several key points.[112] While both authors located Mendel's work within the tradition of experimental hybridization, they disagreed about whether Mendel's work and results

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[111] Ibid., 14.
[112] Ibid., 82.

were particularly unique. Castle suggested that the idea of segregation had been first proposed in France by Charles Naudin, "who made a comprehensive survey of the facts of hybridization and came very near to expressing the generalization which Mendel reached four years later." According to Castle:

> This idea of the segregation of potentialities in the germ-cells of the hybrid was adopted by Mendel. He added to it the conception that the segregation applies to *single* potentialities or characteristics rather than to all the potentialities of a species at once, and the result is what we call Mendel's Law.[113]

On this account, Mendel's work was not unique and he did not deserve credit for the laws that bore his name. Castle was thus able to conclude: "Mendel was so little known when his discovery was published that it attracted little attention and was soon forgotten."[114] Unlike Bateson's account of history, in which Darwinian dogma blinded naturalists to the significance of Mendel's paper, Castle's interpretation of Mendel's work did not require an extensive explanation of why it was unnoticed. He not only broke with Bateson on the status of Mendel as a founding father, but also on the nature of scientific discovery. He used the example of Mendel to emphasize the communal aspect of scientific discovery: "Like all great discoveries it was not made out of hand, nor as the result of one man's work alone."[115] Mendel was not presented as a lone monk working in isolation, as in Bateson's textbook, but rather as someone working within a historical tradition: "Mendel added one final touch to the work of his predecessors as summarized by Naudin."[116] On Castle's account, Mendel merely made the next small step in a long-standing historical development of ideas. The degree to which Castle and Bateson differed on Mendel is epitomized in their understanding of Mendel's approach. Contradicting one of Bateson's central claims, Castle wrote: "Had Mendel lived forty years later than he did, he would doubtless have been a devotee of biometry, for he had a mathematical type of mind."[117]

In addition to downplaying the importance of Mendel, Castle praised the late-nineteenth century period that Bateson had characterized as being an era of dogmatism

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[113] Ibid., 87 (emphasis original).
[114] Ibid., 82.
[115] Ibid., 87.
[116] Ibid.
[117] Ibid., 82.

and darkness. Because Castle thought that Mendel's law of heredity seemed "to require for its explanation some such system of determinants as Weismann had hypothecated and located in the chromosomes," Weismannism occupied a central place in his textbook. Castle presented the period from 1880 to 1900 as being marked by "extreme speculation concerning evolution" which "found its culmination in Weismann's brilliant essays."[118] It is useful to note, by way of contrast, that Bateson had said that Weismann merely "deserves mention" for his "useful work" that revealed the "utter inadequacy" of evidence for the Darwinians' theory of acquired characters. Whereas Bateson focused on the negative proof provided by Weismann's work, the proof against acquired characters, Castle focused on Weismann claims about the nature of the germplasm—the distinction between the germplasm and the somatic cells.[119] Here, Castle agreed with Thomson about the consequence of Weismann's work, but the reasons why Castle valued it and the ways in which he historically situated it were very different. Whereas Thomson focused on Weismann's contributions to the architecture of inheritance, Castle presented Weismann's theory of the determinants as an answer to the problem of the origin of change—a central problem for genetics, especially when it was conceptualized as the study of the continual transformation of the organic world.

Although the protagonists of Bateson's and Thomson's accounts of history appear very differently in *Genetics and Eugenics*, Castle used many of the same narrative devices that they used. For example: the similarity between his Darwin and the Mendelians' Mendel is striking. In Bateson's account, Mendel's work was ignored because Darwinian "dogma" caused "apathy characteristic of an age of faith."[120] In Castle's account, it was Darwin who suffered this fate: "Darwin lived in a time particularly inhospitable to the idea of organic evolution, partly because of theological, and partly because of scientific dogma."[121] Whereas Bateson suggested that the importance of Mendel's paper would have been immediately recognized in the pre-Darwinian era, Castle wrote of Darwin's theory: "Had the idea been brought forward

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[118] Ibid., 55.

[119] Ibid., 47. This point is also made in Reid, *The Laws of Heredity*, 124, and Walter, *Genetics*, Rev. ed., 84-85.

[120] Bateson, *Mendel's Principles of Heredity,* 1st ed., 2-3.

[121] Castle, *Genetics and Eugenics*, 1st ed., 8.

centuries before accompanied by proofs such as Darwin advanced in its support, it undoubtedly would have met more ready acceptance than it found in the last century."[122] Bateson characterized Mendel as following in the tradition of experimental hybridizers and succeeding where they failed because he "surmised that their failure…was due to a want of precise and continued analysis."[123] Similarly, Castle stated that Darwin's predecessors had "formulated more or less clearly the same line of explanation which he adopted," but "had failed to put it to the test of long-continued and detailed observation and experiment, which alone sufficed firmly to establish it."[124] The similarity between the Mendel and Darwin stories continues beyond the details about Mendel himself. Whereas the simultaneous discovery of segregation by Correns, De Vries, and Tschermak was a key aspect of Bateson's account of Mendelian history, Castle focused on the fact that Darwin and Wallace independently developed theories "containing precisely the same explanation of organic adaptations" and decided to simultaneously publish their papers.[125] In these ways, Castle's history conveyed many of the same naturalizing claims about geneticists' knowledge that Bateson promoted. They were, however, advanced in support of very different visions of the nature of the discipline of genetics.

Historiography and "Textbook History"

While the primary aim in this chapter is to show how Thomson, Bateson, and Castle used history-writing to advance their very different conceptions of the nature of the study of heredity, it is worth noting that all three employed historiographical forms that have generalizable features. "Periodizing" history, for example, draws chronological boundaries that define the scientific tradition. Through periodization, the theories, practices, and institutions of a science are defined, presented, and established through the selective recruitment of the past. This is often achieved with accounts of revolution and momentous discovery—narratives in which founding fathers are separated from their

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[122] Ibid.
[123] Bateson, *Mendel's Principles of Heredity,* 1st ed., 7.
[124] Castle, *Genetics and Eugenics*, 1st ed., 8.
[125] Ibid., 13.

scientific culture and cast in terms of the practices and ideas of the present. Kuhn described this process:

> Partly by selection and partly by distortion, the scientists of earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific.[126]

As a retrospective rationalization of doctrines, practices, or ideas whose formation was in fact mediated by other means, periodization brings the past into the interpretive framework of the local scientific culture. Periodizing accounts can often be complemented by "naturalizing" histories, in which scientific theory and discovery are presented as corresponding to human-independent facts about the world. Here, history-writing is used to suggest that the world speaks for itself—that the facts determine their interpretation. Human-dependent factors, such as culture, are presented as obstacles that get in the way of science. Naturalization can be achieved through a form of history-writing that Bachelard named "recurrent history."[127] In recurrent history, the sanctioned past is arranged in a more or less continual sequence, as that which led to the present and anticipated it. The present is both the culmination of the past and the standpoint from which its rationality is understood. Through this type of naturalizing narrative, the history of a discipline can be reconstructed so as to "normalize" the stance of a particular faction.[128] These naturalizing histories can also be used to support "fatalizing" accounts of scientific progress: by making the unveiling of truth appear natural, history-writing can make it also appear inevitable. In both periodizing and naturalizing accounts of history, "framing" is involved. By showing how ideas and theories are related—and what problems are most basic—"framing" accounts define the meaning and significance of past evidence and discoveries. Historical figures and ideas are presented as fitting into a particular order; and the student, by studying this order, learns an interpretive framework. It is worth noting that each of these three narrative devices acts on a different aspect of the science's historicity. Periodization treats the science

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[126] T. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970), 138.

[127] For a discussion of Bachelard's idea of "recurrent history," see Chimisso, *Gaston Bachelard: Critic of Science and the Imagination*, 98-99, 147-148.

[128] For a discussion of normalization, see Kuhn, *The Structure of Scientific Revolutions*, Chapter 11.

diachronologically, creating boundaries that divide its past into characteristically distinct eras. Naturalization takes the science out of time, disassociating its knowledge from the historical context in which it was developed. Framing projects present-day meaning onto past ideas and work, anachronistically shaping their significance.

My analysis of the use of these three general historiographical forms in the early science of heredity has thus far revealed extensive heterogeneity in the first generation of genetics textbook histories—a finding that appears in conflict with Kuhn's claim that "textbook history" is standardized according to the norms of normal science. It is possible, however, that my finding has little to say against the common Kuhnian view, for it could be the case that Kuhn is right in suggesting that discrepancies are eliminated as the science becomes more established. And a brief review of second and third generation genetics textbooks does, in fact, seem to exonerate Kuhn: much of the substantial heterogeneity in the first generation of genetics textbooks was gradually eliminated through the standardization of the Mendel story.

In the 1920s-1950s, most genetics textbooks claimed that the development of modern genetics began with Mendel.[129] There were some exceptions, such as the fourth editions of Thomson's *Heredity* and H. E. Walter's *Genetics*—textbooks first written before 1915. But these were anomalies, and the 1938 edition of Walter's textbook was one of the last that presented Mendel's work as the basis for only one of five avenues by which to study genetics. Most authors presented Mendel as the "founding father" and

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[129] Mendel was presented as the founding father of genetics in T. H. Morgan, *The Physical Basis of Heredity* (Philadelphia: J. B. Lippincott, 1919); E. W. Sinnott and L. C. Dunn, *Principles of Genetics*, 1st, 2nd, and 3rd eds. (New York: McGraw-Hill, 1925, 1932, 1939); E. W. Sinnott, L. C. Dunn, and Th. Dobzhansky, *Principles of Genetics*, 4th and 5th eds. (New York: McGraw-Hill, 1950, 1958); N. Fasten, *Principles of Genetics and Eugenics: A Study of Heredity and Variation in Plants, Animals, and Man* (Boston: Ginn and Company, 1935); A. F. Shull, *Heredity*, 1st, 2nd, 3rd, and 4th eds. (New York: McGraw-Hill, 1926, 1931, 1938, 1948); A. H. Sturtevant and G. W. Beadle, *An Introduction to Genetics* (London: Saunders, 1939); L. H. Snyder, *The Principles of Heredity*, 1st, 2nd, 3rd, and 4th eds. (Boston: D. C. Heath, 1935, 1940, 1946, 1951); L. H. Snyder and P. R. David, *The Principles of Heredity*, 5th ed. (Boston: D. C. Heath, 1957); E. Altenburg, *Genetics*, 1st and Rev. eds. (New York: H. Holt, 1945, 1957); A. M. Winchester, *Genetics: A Survey of the Principles of Heredity*, 1st, 2nd, and 3rd eds. (Boston: Houghton Mifflin, 1951, 1958, 1961); A. M. Srb and R. D. Owen, *General Genetics*, 1st ed. (San Francisco: W. H. Freeman, 1952); A. M. Srb, R. D. Owen, and R. S. Edgar, *General Genetics*, 2nd ed. (San Francisco: W. H. Freeman, 1965); W. Hovanitz, *Textbook of Genetics* (New York: Elsevier Press, 1953); and E. O. Dodson, *Genetics: The Modern Science of Heredity* (Philadelphia: W. B. Saunders, 1956).

included various tributes to his work.[130] The frontispiece chosen by Cyril D. Darlington and Kenneth Mather, for example, showed a page from Mendel's notebooks on which the results of a breeding experiment and ratio were written.[131] In Sinnott and Dunn's *The Principles of Genetics*, the frontispiece was a photograph of Mendel's garden, captioned, "The birthplace of genetics."[132] Irwin H. Herskowitz's *Genetics* contained a translation of part of a letter from Mendel to Nägeli.[133] And Edward C. Colin included translations of "Mendel's Autobiography" and "page one of his paper on peas" in his *Elements of Genetics*.[134] Photographs of Mendel were standard, and were occasionally displayed in the frontispiece.[135] And it was not unusual for textbooks to include translations of Mendel's paper.[136] The general attitude towards Mendel was well stated in the opening line of William Hovanitz's *Textbook of Genetics*: "Genetics as a branch of learning had its origin in the experimental and analytical mind of Gregor Mendel."[137] By the 1940s, key disciplinary terms introduced by Wilhelm Johannsen and Bateson—such as the distinctions between phenotype and genotype, and homozygous and heterozygous—were being presented as part of Mendel's conclusions.[138]

Almost all of the second and third generations of textbooks highlighted Mendel's humble origins, the greatness of his discovery, and the justice in the fact that the world eventually came to recognize his genius. In these ways, the Mendel story became

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[130] See, e.g., Shull, *Heredity*, 3rd ed., 15: "It seems clear, therefore, that the fundamental part of Mendel's scheme was new with him, and that he is entitled to the honor commonly bestowed on him as father of modern genetics." See also Dodson, *Genetics*, 1, and Winchester, *Genetics*, 3rd ed., 32.

[131] C. D. Darlington and K. Mather, *The Elements of Genetics* (London: Allen & Unwin, 1949), frontispiece.

[132] Sinnott and Dunn, *Principles*, 2nd ed., frontispiece.

[133] I. H. Herskowitz, Genetics, 1st ed. (Boston: Little, Brown, and Company, 1962).

[134] E. C. Colin, *Elements of Genetics: Mendel's Laws of Heredity with Special Application to Man*, 3rd ed. (New York: McGraw-Hill, 1956), Appendix A and B.

[135] See, e.g., Altenburg, *Genetics*, 1st ed., 36; Shull, *Heredity*, 3rd ed., 15; Punnett, *Mendelism*, 3rd ed., frontispiece; Colin, *Elements of Genetics*, 3rd ed., 3; Fasten, *Principles of Genetics and Eugenics*, 163; E. G. White, *Principles of Genetics* (St. Louis: C. V. Mosby, 1940), frontispiece; Sinnott, Dunn, and Dobzhansky, *Principles*, 1st and 4th eds., frontispiece; and Winchester, *Genetics*, 3rd ed., 33.

[136] See, e.g., Sinnott, Dunn, and Dobzhansky, *Principles*, 4th and 5th eds.; and Dodson, *Genetics*. Mendel's works were also reprinted outside textbooks in journals, such as *Journal of Heredity* 42 (1951): 3-47, and in collections, such as C. Stern and E. R. Sherwood, *The Origin of Genetics: A Mendel Source Book* (San Francisco: W. H. Freeman, 1966).

[137] Hovanitz, *Textbook of Genetics*, v. See also, e.g., M. W. Strickberger, *Genetics*, 1st ed. (New York: Macmillan, 1968), 3, in which the first line states: "The modern science of heredity originated when Gregor Mendel discovered that hereditary characteristics are determined by elementary units transmitted between generations in uniform and predictable fashion."

[138] See, e.g., King, *Genetics*, 1st ed., 68: "Mendel's analysis of his data led to the recognition of the distinction between phenotype and genotype and homozygosity and heterozygosity."

standardized in form of the heroic historiographical tradition.[139] However, like many of the founding father histories written in the nineteenth century, the Mendel story also included a feature of the pragmatic historiography of the enlightenment: the rationality of Mendel's approach was regularly identified as the key to his success, implying that his discovery was the inevitable outcome of rational science. During the second half of the twentieth century, this fatalization of his discovery was explicitly articulated. In the widely used *Heredity and Development*, for example, J. A. Moore described the rediscovery of Mendel's laws:

> This is another example of a frequent happening in science. When the field is 'ready,' the discovery is certain to be made. If Mendel had never lived, the history of genetics would not have been greatly different. About the year 1900, either he would be rediscovered or, had he never lived, others would reach essentially the same conclusions as he had in 1866. His work was unappreciated in his own lifetime, for biologists in 1866 had neither the background nor the prescience to understand the significance of what he had accomplished.[140]

Heroic and fatalizing accounts of the founding of disciplines often exist in conflict. This is because the heroic founding father narrative creates an image of a discovery that sprang from the mind of genius working outside the realm of normal science—an image in which the discovery might not have been made without the founding father, and is therefore historically contingent. In contrast to this picture is that created by a fatalizing account of scientific discoveries, which suggests that they are inevitable—that the rationality of science governs and guarantees the progress of science. Because of this conflict, disciplinary origin narratives that rely on both types of claims are often internally incoherent. In the specific case of the Mendel story, however, this conflict is averted. What is unique about the Mendel founding father story is that it does not claim that he *actually* "founded" genetics. Mendel's status is not based on the claim that he was the prime cause of genetics, but rather that he was the first geneticist—a geneticist before the time of genetics. With the story of long neglect and simultaneous rediscovery, Mendel's heroic status of genius is justified at the same time that his discoveries are fatalized.

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[139] For a brief overview of five "epochs" of historiography—doxographic, erudite (or Humanist), pragmatic (or Enlightened), heroic (or Romantic), positivist (or Modern)—see N. Jardine, *The Scenes of Inquiry: On the Reality of Questions in the Sciences* (Oxford: Clarendon Press, 1991), 126-130.

[140] J. A. Moore, *Heredity and Development*, 1st ed. (New York: Oxford University Press, 1963), 45.

The heroic periodization and fatalized naturalization of Mendel's work became a standard feature of genetics textbooks by the mid-twentieth century. But where histories become standardized on the surface, instability can continue unseen underneath, and a brief look at some later textbooks suggests that "the history of genetics" was not as stable as it might seem. Despite the consensus about Mendel's status as founding father, the meaning of Mendel's work and the historical context in which it was situated continued to change throughout the first half of the twentieth century. In A. F. Shull's *Heredity,* for example, there were significant revisions between the first edition of 1926, and the fourth edition of 1948. In the first, Mendel's work was discussed in relation to that of Darwin, and Shull focused on the fact that Mendel had discovered laws of heredity.[141] Few changes were made in the second edition, but in the third, Darwin was replaced by the German botanist Joseph Gottlieb Kölreuter, who was presented—along with Mendel—as using hybridization experiments to study "the processes of heredity."[142] In the fourth edition, Mendel and Kölreuter were again refashioned, and presented as working on "particulate inheritance."[143] This recontextualization of Mendel corresponded with reinterpretations of the significance of his work. By the 1920s, key aspects of Bateson's presentation of Mendel were no longer included. For example: Mendel's work was no longer presented in the context of evolutionary debates about variation and the origin of new species. Rather, as stated in *Principles of Genetics*: "The results which Mendel obtained…were chiefly important in showing that inheritance…was subject to certain definite rules or laws."[144] This understanding of his work continued throughout the 1920s and 1930s. But by the 1940s, textbook authors began to focus on the gene. In the 1945 edition of Edgar Altenburg's *Genetics*, for example: "Mendel clearly demonstrated by his crosses that the hereditary material of a plant or animal consists of separable units—the units we now call genes."[145] W. R. Singleton made a similar suggestion in *Elementary Genetics*, justifying the anachronism of the claim: "We can properly say that Mendel discovered the gene. It was not so named until many years later, but Mendel

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[141] Shull, *Heredity*, 1st ed., 5-6.
[142] Shull, *Heredity*, 3rd ed., 8-9.
[143] Shull, *Heredity*, 4th ed., 2-4.
[144] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 9.
[145] Altenburg, *Genetics*, 1st ed., 74. This type of claim was common: see, e.g., Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 5th ed., 120; and Winchester, *Genetics*, 2nd ed., 65.

discovered it just as truly as Columbus discovered America, which also was not so named until many years later."[146] Similar shifts, towards an emphasis on the gene, also occurred with respect to other nineteenth century scientists. For example: in the 1925 edition of *Principles of Genetics*, a discussion of many nineteenth century scientists briefly mentioned Weismann because of his important rejection of the theory of the inheritance of acquired characteristics; by the time of the 1958 edition, however, Weismann was one of the few historical figures discussed in the textbook, his germplasm theory was presented as a precursor to the theory of the gene, and he was described as "the forerunner of modern genetics."[147] Thus, the nineteenth century work of Mendel and Weismann was reinterpreted as it was recruited into the conceptual framework of gene-oriented genetics.

The use of Mendel for disciplinary purposes has not gone unnoticed by geneticists, nor has it escaped the attention of historians. As early as 1936, statistician and geneticist R. A. Fisher criticized Bateson's "polemical use of the rediscovery," concluding:

> Each generation, perhaps, found in Mendel's paper only what it expected to find; in the first period a repetition of the hybridization results commonly reported, in the second a discovery in inheritance supposedly difficult to reconcile with continuous evolution. Each generation, therefore, ignored what did not confirm its own expectations.[148]

In this article, Fisher also advanced the controversial suggestion that Mendel had falsified some of his data. And since the 1960s, a steady flow of scholarship by geneticists and historians has addressed the topic in detail.[149] There has also been substantial disagreement about whether Mendel discovered and articulated the laws of

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[146] Singleton, *Elementary Genetics*, 34.

[147] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 10; and Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 5th ed., 9.

[148] R. Fisher, "Has Mendel's Work Been Rediscovered," *Annals of Science* 1 (1936): 137.

[149] See., e.g., G. W. Beadle, "Mendelism, 1965," in *Heritage from Mendel*, ed. R. A. Brink and E. D. Styles (Madison: University of Wisconsin Press, 1967); Dunn, *A Short History of Genetics: The Development of Some of the Main Lines of Thought, 1864-1939*, 12-13; A. W. F. Edwards, "Defending Mendel Merely Perpetuating a Myth," *Nature* 326 (1987); E. Mayr, *The Growth of Biological Thought* (Cambridge: Harvard University Press, 1982), 719-720; C. E. Novitski, "Another Look at Some of Mendel's Results," *Journal of Heredity* 86 (1995); W. W. Piegorsch, "Fisher's Contributions to Genetics and Heredity, with Special Emphasis on the Gregor Mendel Controversy," *Biometrics* 46 (1990); I. Pilgrim, "The Too-Good-to-Be-True Paradox and Gregor Mendel," *The Journal of Heredity* 75 (1984); L. M. Van Halen, "Mendel Was No Fraud," *Nature* 325 (1987); and S. Wright, "Mendel's Ratios," in *Origin of Genetics*, ed. C. Stern and E. Sherwood (San Francisco: W. H. Freeman, 1966).

inheritance that carry his name—whether or not Mendel was a Mendelian.[150] Points of significant contention include Mendel's intentions, his conclusions, and the ways in which his work has been misinterpreted and used by geneticists in the twentieth century.

The variety and persistence of arguments about the "real" Mendel are indicative of the degree to which statements about founding fathers are part of scientific discourse. Disagreements about the nature of the Mendel's discovery were not merely disagreements about the past, but rather were part of arguments about productive and legitimate modes of practice and explanation. Claims about Mendel's experiments and discoveries were used to identify and establish acceptable methods of inquiry, to attribute significance to experimental data, and to articulate procedures by which evidence could be evaluated. Representations of Mendel's work were used to define the nature and tradition of the science of genetics.[151] As I have indicated in this chapter, the meaning of Mendelism was gradually reshaped over the early decades of the twentieth century: whereas Bateson, Punnett, and the early Mendelians saw Mendelism and Darwinism in conflict, the geneticists of the synthesis made them compatible. This was not, however, the only point of conflict in this period. In addition, divergent and conflicting interests in heredity and evolution caused arguments about the purpose and future direction of genetics, the legitimacy of competing methods for gathering data, and the meaning of data collected: embryologists and geneticists disagreed about whether Mendelian genes controlled all or only superficial characteristics of the organism; and population geneticists disagreed with experimental geneticists about the value of data collected in the laboratory versus that in the field. I suspect that an analysis of textbooks will find these disagreements beneath the surface of the seemingly standardized history, reflected in subtle differences in accounts of Mendel's intentions and conclusions. Thus, I suggest that it is not the standardization of history in textbooks, as Kuhn suggested, but rather the appearance of standardization that gives history-writing its disciplining power. It is similarities in surface, belying underlying substantive differences, that imbue textbook

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[150] See, e.g., Brannigan, "The Reification of Mendel"; L. A. Callender, "Gregor Mendel: An Opponent of Descent with Modification," *History of Science* 26 (1988); F. Di Trocchio, "Mendel's Experiments: A Reinterpretation," *Journal of the History of Biology* 24 (1991); R. Olby, "Mendel No Mendelian?" *History of Science* 17 (1979); and C. Zirkle, "Gregor Mendel and His Precursors," *Isis* 42 (1951) .

[151] See Sapp, "The Nine Lives of Gregor Mendel," for an excellent discussion of the various functions performed by the continual reconstruction of Mendel throughout the twentieth century.

histories with normative weight—with the authority of tradition. By organizing the past and present into a coordinated foundation for the future of the discipline, history-writing projects a coherent purpose onto the science that defines and guides its "natural" course of progress.

Conclusion

In general, a discipline cannot be defined by reference to its subject domain alone. While some disciplines can be more or less identified by their content or methods, such clear demarcation is unusual. Disciplinary boundaries are frequently disputed and require enforcement. Thus, a discipline emerges and maintains itself by wielding power over these boundaries, defining the terms in which its relationships with other sciences are seen. Through claims about a science's past, a picture of its purpose and relation to other sciences is presented. Given that the significance of facts—the way in which data is constituted as evidence—is dependent on what counts as "the problem" of a science, history-writing allows for an articulation of the discipline's methods of investigation and modes of interpretation. The ordering of ideas and theories, hierarchically or in networks, shapes their meaning. The student who is taught to see key historical figures and ideas as fitting into a particular order is inculcated into an interpretative framework. Thus the writing of history does not only help discipline the science's past, but also provides the student with a way of seeing that will shape future interpretations of significance and meaning.

In this chapter, I have explored the histories told in three influential first generation genetics textbooks in order to study and explore the relationship between history-writing and the establishment of genetics as an independent branch of biology. I have found that history-writing situated the science within several narratives of scientific progress. The most prominent account presented the development of the science from within. A second, externalist, narrative located genetics within the development of the life sciences, characterizing it as either a branch or the future foundation of biology. And the third discussed the general nature of scientific inquiry and scientific laws, situating the principles of genetics within an account of the development of quantification and predictive models in the physical sciences. With these historical contextualizations of the discipline, textbooks defined the nature of genetics—as a field of inquiry, as a discipline within the life sciences, and as a type of science. These forms of history-writing periodized, naturalized, and framed the research, theories, and discoveries of hereditary science. They were used to recruit past theories and thinkers into modern formulations of the science, demonstrate the validity and invalidity of practices and theories, make the knowledge and progress of genetics appear inevitable, and define the conceptual coherence that unified genetics as a science. At times, these uses were motivated by controversy and factional interests. Bateson's account of history, for example, was clearly meant to discredit the biometricians on the one hand, and legitimate and naturalize the rise of Mendelism on the other. These intentionally polemical uses of history have not, however, been the focus of my analysis. Rather, I have tried to illustrate that Kuhnian claims about the functions of disciplinary histories do not go far enough in challenging the common view that history could be removed from the teaching of science. The functions of historical claims are not limited to those of truncated and distorted presentistic histories, by which past scientists appear to have worked on "the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific" and students "come to feel like participants in a long-standing historical tradition."[152] The more subtle disciplinary functions are performed by historical accounts that convey, order, and attribute significance to past facts, theories, and practices pertaining to the study of heredity—histories that position a science in relation to other sciences. Focusing on these functions, I have argued that a textbook's account of a science's history is not a frame in which the science is presented and from which it can be removed. It is, rather, deeply embedded in the teaching of the discipline, and thereby inextricably connected with scientific practice.

I have shown that there was little standardization in the first generation of "genetics" textbooks. Bateson, Thomson, and Castle each presented what could be considered as being a discipline that was distinct from that of the others. Their histories referred to many of the same historical figures and theories, but established very different

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[152] Kuhn, *The Structure of Scientific Revolutions*, 138.

visions of the basic nature of the study of heredity. The work of Weismann, Galton, and Mendel was interpreted and shaped to fit distinct and coherent visions of the history and purpose of the study of heredity. Weismann, for example, was seen as doing something different by Thomson, Bateson, and Castle: as illuminating the architecture of the hereditary material, disproving the theory of the inheritance of acquired characteristics, and identifying the causes of change in individuals and species, respectively. Galton's law of ancestral heredity was similarly read in three ways: as a conceptualization of ancestry in terms of the continuity between generations, an attempt to discover mathematical order in heredity, and a contribution to the theory of evolution. And Mendel's law of segregation was interpreted as either the identification of the particulate nature of the germplasm, the discovery of mathematical order in heredity, or a way of thinking about the mechanism of evolution. Although these various readings of what were agreed to be foundational works were not in and of themselves incommensurable, they remained distinct in the first generation of genetics textbooks. Each was created by and involved in the operation of a disciplinary framework that characterized the significance of past research, theories, and practices in ways that aligned them with a vision of the future of the science, thereby governing the path of its development. For Bateson, genetics was a science devoted to discovering laws and mathematical order. On Thomson's understanding, it was a search to identify the materiality and architecture of inheritance. And Castle saw it as an attempt to understand the process of the coming into being of new lives and types of life. These different understandings of the purpose of genetics corresponded with different ways of establishing order within the science—of deciding which evidence supported which theory, what concepts were most fundamental, and how genetics was situated in relation to other sciences.

Exploring three disciplinary frameworks in the early twentieth century, I have indicated the nature of the historical contingency and the interpretation implicit in the science that later took Mendel's laws as its foundation. Whether Mendelian segregation was a law to which there were exceptions, or a group of exceptions to the norm, was not decided by reference to features of the world, but rather by aspects of scientific culture—by ways of attributing meaning and significance to facts. Bateson recognized that there were innumerable cases of breeding that did not clearly segregate, and characterized these cases as exceptions to a rule. Thomson, looking at the same data, concluded that it was Mendelian segregation that was an exception. This difference in interpretation did not arise from disagreement about the percentage of traits that exhibited segregation, but rather from the authors' very different ideas of what genetics was about. For Bateson, genetics was a science devoted to finding constant laws in biology comparable to those in the physical sciences. On this account, Mendelian segregation naturally provided the foundation on which the discipline would be built. But for Thomson, who thought of the science as the study of the material basis of heredity, there was no compelling reason to think of Mendel's laws as the core of the discipline. Mendelian phenomena were helpful insofar as they illuminated the nature of the architecture of the germplasm.

Presentations of the science of genetics in subsequent generations included aspects of Thomson's, Bateson's, Castle's interpretive frameworks. The extent of their synthesis cannot be addressed here, but it is worth briefly noting a few points. Bateson's vision of the conceptual structure of genetics was adopted in most textbooks, which began with Mendel's principles, and proceeded step by step through apparent exceptions, modifying principles, and actual exceptions; the physical basis of heredity was often discussed only insofar as it was necessary to explain the principles of genetics. In this way, students were brought to see Mendel's laws as the conceptual foundation of the discipline. The interpretation of these laws, however, was very much informed by Thomson's Weismannism. Mendelian segregation was understood as a physical event regarding genes in the chromosomes; and the gene was conceptualized as the most basic unit in the architecture of inheritance. And with the rise of the evolutionary synthesis, Castle's emphasis on life and the "coming into being" of organisms made its way into the super-structure of many textbooks. Introductory chapters contextualized the study of genetics within discussions of the nature of life.[153]

In a general introduction to science that was published shortly after the first edition of *Heredity,* Thomson wrote that a science "is defined not by its subject matter,

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[153] See, e.g., Srb and Owen, *General Genetics*, 1st ed., which began with a chapter "Inherent Patterns in Living Things," discussing the fundamental unity of life; and Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 5th ed., in which "Heredity and the Continuity of Life" replaced "Genetics, the Science of Heredity and Variation" as the first chapter. In these textbooks, "life" became an organizing trope.

but by the categories under which it thinks of that subject matter” and by “its point of view—the particular kind of question it asks."[154] It has been an aim of this chapter to explore how the categories and field of potential questions for geneticists came into existence during the early twentieth century. My contention is that history-writing was not merely a way of presenting the discipline, but rather was an essential tool involved in its creation. It was through history-writing that proper methods of investigation and interpretation were articulated and demonstrated, and that questions were endowed with significance. This is not merely to say that historical contextualization made some questions seem more pressing than others, but also and more importantly, that it was constitutive of the reality of questions. The way in which the discipline was pictured—the way its history was seen—limited what could and could not be seen as potential questions. In the following chapter, I will further explore this theme in a study of how the students’ epistemic stance was shaped through the use of historical exemplars.

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[154] J. A. Thomson, *Introduction to Science* (New York: H. Holt, 1911), 229.

CHAPTER 2

Virtual Historical Environments:

Principles, Exemplars, and the Enculturation of Students

> Is what we call “obeying a rule” something that it would be possible for only *one* man to do, and to do only *once* in his life?
>
> Ludwig Wittgenstein, in *Philosophical Investigations*.[1]

> They have said that to make a general principle worth anything, you must give it a body. You must show in which way and how far it would be applied actually in an actual system. You must show how it has gradually emerged as the felt reconciliation of concrete instances…Finally, you must show its historic relations to other principles often of very different dates and origins.
>
> Oliver Wendell Holmes Jr., in an address to the Harvard Law School.[2]

In this chapter, I will discuss the use of a type of history-writing that did not take the form of “a history” of genetics. Looking beyond historical claims that were presented *as* history, I will investigate the writing of historical cases with which authors taught what they conceived of as their science. I will look not only at how “the principles of genetics” were taught, but also at the ways in which the student gained the tacit skills needed to understand and use these principles. Exploring the relationship between historical cases and principles, I will ask how and why history-writing became central to the pedagogical, disciplining environment created in and by textbooks.

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[1] L. Wittgenstein, *Philosophical Investigations*, trans. G. E. M. Anscombe, 3rd ed. (Oxford: Blackwell, 2001), §199 (emphasis original).

[2] O. W. Holmes Jr., "The Harvard Law School [Justice Holmes' Oration at the 'Quarter-Millennial' Celebration of Harvard University on the 5th of November, 1886]," *Law Quarterly Review* 3 (1887): 121.

In developing my argument, I will draw on Thomas Kuhn's analysis of exemplars, which is not only analytically helpful in understanding the scientific functions of history-writing, but also substantively connected to the use of historical cases in the teaching of science.[3] The arguments about exemplars articulated in *Structure of Scientific Revolutions* were inspired by Kuhn's experiences teaching cases from the history of physics at Harvard.[4] His course was part of a curriculum being developed in the 1940s by the university's president, James Conant, who believed that a realistic understanding of the "tactics and strategy of science" was essential for all citizens of a democracy, and that the nature of science could be best conveyed with the case-study method used at Harvard's Schools of Law, Medicine and Business. This method was originally introduced and developed at the Law School, by Professor Christopher Columbus Langdell during his time as Dean (1870-1895), as a solution to the problem of how to present a tangled web of legal precedents as a principled or doctrinal system of law.[5] According to Langdell, "the best, if not the only way of mastering the doctrine effectively is by studying the cases in which it is embodied."[6] This method of teaching was then adopted in the Harvard Medical School at the turn of the century, and in the Business School at its founding in 1911.[7] It was in the context of this intellectual legacy that Kuhn followed Conant's curriculum and taught physics using the case-study method; and it was from this experience that Kuhn went on to argue that shared exemplars provided the foundation of scientific communities and the empirical content of scientific laws. Thus, when I bring Kuhn's conception of exemplars to bear on the functions of history-writing in the teaching of science in the 1930s, it is genealogically related ideas

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[3] Note that Kuhn introduced the term "exemplar" in the postscript to the second edition of *The Structure of Scientific Revolutions* to clarify to clarify what he saw as "the most novel and least understood aspect of this book"—his conception of a scientific paradigm as a set of shared examples, or "concrete problem-solutions that students encounter from the start of their scientific education." See T. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970), 186-187. Much of this analysis was originally articulated in Chapter 4, Normal Science as Puzzle-Solving, and Chapter 5, The Priority of Paradigm.

[4] Kuhn, *The Structure of Scientific Revolutions*, vii.

[5] J. Forrester, "If P, Then What? Thinking in Cases," *History of the Human Sciences* 9 (1996): 15.

[6] C. C. Langdell, *A Selection of Cases on the Law of Contracts* (Cambridge, MA: Harvard University Press, 1871), vii, as quoted in Forrester, "If P, Then What? Thinking in Cases," 15, which drew my attention to the work of Langdell.

[7] Forrester, "If P, Then What? Thinking in Cases," 15-16.

that are brought together.[8] Before I develop my study, however, I will first identify some of the key points of Kuhn's analysis.

According to Kuhn, there are two forms in which students encounter exemplars. The first is in portrayals of good science. These can be model lines of though, such as Aristotle's analysis of motion, Ptolemy's computations of planetary positions, Lavoisier's application of the chemical balance, or Maxwell's mathematization of the electromagnetic field.[9] Or alternatively, they can be entire books or papers, such Lyell's *Geology,* Lavoisier's *Traité Elémentaire de Chimie*, or Newton's *Principia* and *Opticks*.[10] These models of good science not only contain theories, laws, and principles, but also show how they are used to solve important problems, particularly when the solution involves a new experimental or mathematical approach (e.g., the chemical balance in *Traité Elémentaire de Chimie*, or the calculus in *Principia Mathematica*). An exemplar is not constituted in a model experiment alone, however, but rather relies on problem-solving exercises that correspond to the model—"the problem-solutions…at the ends of chapters in science texts."[11] These exercises allow the student to practice using the law or principle derived; and more importantly, they define the types of problems that can be solved using the law or principle. Thus, although the historicity of the problems is not always explicit, they must be understood as crucial components of the historical exemplars that are used in the teaching of the science.

Kuhn identifies three general functions fulfilled by exemplars in scientific practice: they suggest new puzzles, provide a method for solving them, and serve as the standard by which the quality of a proposed solution can be measured.[12] By studying exemplars, the beginning student learns how to see a new problem as similar to the

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[8] For more on this history, see J. Harvey, "History of Science, History and Science, and Natural Sciences: Undergraduate Teaching of the History of Science at Harvard, 1938-1970," *Isis* 90 (1999); M. A. Dennis, "Historiography of Science: An American Perspective," in *Science in the Twentieth Century*, ed. J. Krige and D. Pestre (United Kingdom: Harwood Academic Publishers, 1997), 10-22; Forrester, "If P, Then What? Thinking in Cases," esp. 6-16; and E. A. Melia, "Science, Values and Education," 443-459. Although Conant focused on teaching science to non-scientists, his ideas impacted the general teaching of science to scientists; for example, Conant's *On Understanding Science* and *Science and Common Sense* were cited in the first chapter of a widely used college biology textbook: W. C. Beaver, *General Biology*, 5th ed. (St. Louis: C. V. Mosby, 1958), 31.

[9] Kuhn, *The Structure of Scientific Revolutions*, 23.

[10] Ibid., 10.

[11] Ibid., 187.

[12] Ibid., 35-42.

exemplar, and how to apply the principles used in the exemplar to the new problem. However, the judgments of similarity that provide the foundation for this exemplar-based reasoning do not correspond to inherent similarities between cases, according to Kuhn, but rather are a matter of social agreement. In support of this claim, Kuhn discusses the process by which a child learns to recognize geese, ducks, and swans.[13] He argues that the child learns "to apply symbolic labels to nature without anything like definitions or correspondence rules."[14] Concepts and objects in the world are constituted together in this process of learning by ostention; and Kuhn suggests that the learning of scientific concepts and laws is similar. To learn an exemplar is to internalize a new way of seeing: what was once seen as a stone on the end of a string becomes a pendulum. Students learn the concepts of space, time, mass, and force as they internalize the corresponding Newtonian exemplars, and in this way they come to see Newtonian objects for the first time.[15] The similarity between the various cases in which a given scientific principle or law applies is like the similarity between geese: "shared examples have essential cognitive functions prior to a specification of criteria with respect to which they are exemplary."[16] Kuhn argues that they are socially agreed-upon similarities that cannot be fully captured by criteria or rules. In this way, his account of exemplars rejects aspects of the conventional picture of scientific laws and the process by which they are applied.

On Kuhn's picture, textbooks must be understood as tools for teaching the student the similarity relationships that undergird scientific practice. One of the ways in which textbooks do this is by teaching beginning scientists how to 'simplify' a complex problem—to isolate the aspects of the complex problem that will allow it to be seen as similar to the paradigmatic case. By demonstrating how to reconceptualize a new problem in terms of an old, the textbook exemplar enables scientists to see new puzzle-situations in terms of familiar puzzles, and hence enables them to see potential solutions to their new puzzles. The student does not learn the theory by merely memorizing formulas or rules, but rather grasps the theory by learning how to apply it properly to

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[13] T. Kuhn, "Second Thoughts on Paradigms," in *The Structure of Scientific Theories*, ed. F. Suppe (London: University of Illinois Press, 1974), 473-482.
[14] Ibid., 475.
[15] Kuhn, *The Structure of Scientific Revolutions*, 118-128.
[16] Kuhn, "Second Thoughts on Paradigms," 477.

solve the standard problems.[17] Given the nature of the similarity relationships on which scientific practice is based, textbooks cannot separate facts from method of investigation; students learn both together, “as samples of accepted achievement.”[18]

Kuhn not only rejects the conventional picture of the way in which rules are learned and applied, but also the traditional presumption that theories and laws determine the empirical content of science. If the key determinant in the acceptability of a proposed puzzle-solution is its similarity to the paradigmatic puzzle-solutions—and that perception of similarity cannot be reduced to rules—the science does not consist fundamentally of its laws, but rather is defined centrally by its exemplars: "In the absence of such exemplars, the laws and theories...would have little empirical content."[19] This means that learning a science involves acquiring a new way of seeing—acquiring the ability to group problems according to the theoretical principles that are relevant to those problems. It is thus these "similarity groupings" of mature scientists that distinguish them from students. It is shared exemplars, rather than a complex system of rules, that unifies a scientific community.[20]

Kuhn’s idea of shared exemplars as the basis of a scientific community provides the conceptual starting point of this chapter, which begins with the observation that detailed accounts of Mendel’s experiments occupy a central place in almost all genetics textbooks of the second quarter of the twentieth century, and proceeds by exploring the functions and importance of these reconstructions of history that are not presented as accounts of the past.[21]

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[17] Ibid., 471.

[18] T. Kuhn, "Discussion," in Scientific Change, ed. A. C. Crombie (London: Heinemann, 1963), 391.

[19] Kuhn, *The Structure of Scientific Revolutions*, 188.

[20] For a more extensive discussion of Kuhn’s claims about scientific training, exemplars and similarity relations, see the second chapter of B. Barnes, *T. S. Kuhn and Social Science* (London: Macmillan, 1982), 16-40. See also Forrester, "If P, Then What? Thinking in Cases," esp. 8-10.

[21] On the uses and functions of statements about key experiments that *are* made in scientists’ accounts of the past, see G. N. Gilbert and M. Mulkay, "Experiments Are the Key: Participants' Histories and Historians' Histories of Science," *Isis* 75 (1984).

The Presentation of Past Experiments as a Virtual Historical Environment: Teaching rule-finding, or, how to see with the principles of geneticists

When R. C. Punnett predicted of Mendel's work, "the lucidity of the exposition will always give it high rank among the classics of biological literature," he was right.[22] Mendel's experiments with peas were described in almost all genetics textbooks published after the mid-1920s.[23] Often, these descriptions were complemented by a translation of Mendel's papers—a historical exemplar that was first used in Bateson's *Mendel's Principles of Heredity: A Defense* (1902), in which Mendel's writings accounted for almost one third of the pages, and remained a popular feature of textbooks written through the 1950s.[24] One of the common reasons authors gave for presenting Mendel's work was that it provided the student with a model of scientific ideals. Robert King, for example, wrote: "the present-day geneticist need only follow the example set by the elegant experimentation of Gregor Mendel."[25] In general, three features of Mendel's work were identified as key to his success: his simplification of the problem of heredity,[26] his organization of his experimental results,[27] and his attention to the

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[22] R. C. Punnett, *Mendelism*, 3rd ed. (London: Macmillan, 1911), 7.

[23] See, e.g., H. E. Walter, *Genetics: An Introduction to the Study of Heredity*, Rev., 3rd, and 4th eds. (New York: Macmillan, 1922, 1930, 1938); E. W. Sinnott et al., *Principles of Genetics*, 1st, 2nd, 3rd, 4th, and 5th eds. (New York: McGraw-Hill, 1925, 1932, 1939, 1950, 1958); A. F. Shull, *Heredity*, 2nd, 3rd, and 4th eds. (New York: McGraw-Hill, 1931, 1938, 1948); A. H. Sturtevant and G. W. Beadle, *An Introduction to Genetics* (London: Saunders, 1939); E. Altenburg, *Genetics*, 1st and Rev. eds. (New York: H. Holt, 1945, 1957); A. M. Winchester, *Genetics: A Survey of the Principles of Heredity*, 1st, 2nd, 3rd eds. (Boston: Houghton Mifflin, 1951, 1958, 1966); W. R. Singleton, *Elementary Genetics* (New York: D. Van Nostrand, 1962); and R. C. King, *Genetics*, 1st and 2nd eds. (New York: Oxford University Press, 1962, 1965). Note that in the first edition of some of these textbooks—e.g., Shull's *Heredity* and Walter's *Genetics*—Mendel's experiments were not described.

[24] See, e.g., W. E. Castle, *Genetics and Eugenics: A Text-book for Students of Biology and a Reference Book for Animal and Plant Breeders*, 1st ed. (Cambridge, MA: Harvard University Press, 1916), Appendix; E. O. Dodson, *Genetics: The Modern Science of Heredity* (Philadelphia: W. B. Saunders, 1956), Appendix A; and Sinnott, Dunn, and Dobzhansky, *Principles*, 4th and 5th eds., Appendix. Note that reviews generally praised authors for providing the student with access to Mendel's work. See, e.g., "A Text-Book of Genetics," Review of *Genetics and Eugenics*, by W. E. Castle, *Nature* 99 (1917): 203: "An excellent feature is an appendix containing a translation of Mendel's paper, which ought to be carefully digested by every student." See also E. Caspari, Review of *Principles of Genetics*, by E. W. Sinnott, L. C. Dunn, and Th. Dobzhansky, *Science* 112 (1950): 725: "It is to be hoped that many teachers will make use of this opportunity to introduce to their students the beautifully clear logic and presentation and the great experimental skill of Gregor Mendel."

[25] King, *Genetics*, 1st ed., 69.

[26] E.g., Sinnott and Dunn, *Principles of Genetics*, 1st ed., 37-38: "Where earlier investigators had made general observations upon the animal or plant as a whole…Mendel instead still further simplified the problem by confining his attention to a single character at a time." See also T. H. Morgan, *The Physical Basis of Heredity* (Philadelphia: J. B. Lippincott, 1919), 19.

[27] E.g., Sinnott and Dunn, *Principles of Genetics*, 1st ed., 38-39: "In tracing these characteristics from generation to generation a careful technique and a thorough system of recording observations became

importance of quantification in designing and carrying out the experiments.[28] Mendel was an exemplary scientist according to Edmund Sinnott and L. C. Dunn, who explained the general concepts "scientific law" and "scientific method" by reference to his work: "Mendel's discovery of these first laws of inheritance led through precisely the steps of observation and experiment, classification, tentative explanation or hypothesis, testing, and final deduction which have been outlined."[29]

Detailed accounts of Mendel's work were not just included to illustrate scientific ideals, however; more crucially, they were used in the teaching "the principles" of genetics. In the 1920s, textbook authors often explained their pedagogical aims in introductions to their textbooks, and in *Principles of Genetics* (1925), Sinnott and Dunn explained why they believed that an understanding of the principles could "best be gained by the student through a process which is somewhat similar to that employed in their original discovery."[30] Their use of the historical case method was connected to their understanding of the nature of the principles of genetics which, according to Sinnott and Dunn, could not be learned as facts alone: "There is a common feeling that…by virtue of 'knowing the book' one acquires all of the knowledge of the subject which it is necessary to have. Such beliefs have little to justify them."[31] They suggested that an understanding of the principles was essentially different from remembering a corpus of information: "No text is or can be complete or final; nor, if it were, would an understanding of the subject be gained by committing the whole book to memory."[32] In other words, the *discipline* of genetics could not be reproduced through the mere

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necessary…This involved the task of keeping full and precise pedigree records of all plants studied." See also Morgan, *The Physical Basis of Heredity*, 19.

[28] According to Sinnott and Dunn, *Principles of Genetics*, 1st ed., 37, Mendel was the first to "reduce the phenomena to a *measurable* basis" and employ "the exact quantitative methods used so successfully in many other science." See also Morgan, *The Physical Basis of Heredity*, 16-17; and L. H. Snyder and P. R. David, *The Principles of Heredity*, 5th ed. (Boston: D. C. Heath, 1957), 14.

[29] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 37. This explanation of the importance of including Mendel's work was made in textbooks through the 1950s. See, e.g., E. C. Colin, *Elements of Genetics: Mendel's Laws of Heredity with Special Application to Man*, 3rd ed. (New York: McGraw-Hill, 1956), xiv: "The experiments of Mendel constitute a brilliant example of the application of the scientific method to a specific problem." Note that Colin's attention to "the historical facts concerning Mendel and his work" was noted in a review of an earlier edition: E. L. Powers, Jr., Review of *Elements of Genetics*, by E. C. Colin, *The American Midland Naturalist* 26 (1941): 699.

[30] Sinnott and Dunn, *Principles of Genetics*, 1st ed., xvii.

[31] Ibid. These types of claims were also made by biologists reviewing biology textbooks: see, e.g., C. E. McClung, "Scientific Books: Elementary Biological Texts," *Science* 94 (1941): 392.

[32] Sinnott and Dunn, *Principles of Genetics*, 1st ed., xvii.

transmission of its *dogma*. True knowledge, according to Sinnott and Dunn, "*grows* in the minds of those who discover for themselves new facts and relationships."[33] Students of genetics did not need to develop a new "world-view" or "episteme," but rather an epistemic stance; genetics was a localized approach for a specific problem of knowledge, and it was with historical exemplars—with the temporality of discovery embedded into the structure of the textbook—that the student developed this ability to think with the geneticists' approach.[34]

The historical case-study approach, whereby the student re-lived the discovery of the principles, was widely adopted in the 1930s-1940s, and its value came to be seen as common sense by many geneticists. In the *Science* review of Edgar Altenburg's 1945 *Genetics*, for example, Curt Stern compared two ways of presenting a theory: (a) statement of theorem followed by experimental proof, versus (b) statement of experiment followed by deduction of theory. According to Stern, "While the information supplied is identical in both sequences, the latter seems better suited to convey the exciting pleasure of the discovery and organically makes possible a historical treatment with its humanistic implications."[35] Stern's review criticized Altenburg for choosing the former, as did the review of the textbook in *Quarterly Review of Biology*:

> To the reviewers the presentation of major theories…as cut-and-dried facts robs the book of a part of its value and interest…It seems preferable in such a course to cultivate an appreciation of scientific method by focusing attention on the actual steps by which such theories have been built up, leading up to the theory inductively rather than stating it as a premise.[36]

Textbook author Irwin H. Herskowitz agreed, and adopted the historical case-method in the 1962 and 1965 editions of *Genetics*. As he explained: "Whenever feasible, genetic principles are derived scientifically—by recognizing and stating a problem, designing

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[33] Ibid. (emphasis original). See also, e.g., the similar organic language in C. Stern, Review of *Genetics*, by E. Altenburg, *Science* 102 (1945): 515.

[34] The concept of the "episteme" comes from M. Foucault, *The Order of Things: An Archaeology of the Human Sciences* (London: Tavistock Publications, 1970), e.g., xxii; that of the "world-view" is developed throughout A. Koyré, *From the Closed World to the Infinite Universe* (Baltimore: Johns Hopkins Press, 1957).

[35] Stern, Review of *Genetics*, by E. Altenburg: 515. While geneticists' beliefs about the "humanistic implications" of teaching with historical cases lies outside the focus of this study, it is worth noting their apparent connection to several pedagogical projects advanced at Harvard in the first half of the twentieth century. For a detailed treatment of this history, see Melia, "Science, Values and Education."

[36] R. F. Kimball and E. S. Gersh, "Genetics and Cytology," Review of *Genetics*, by E. Altenburg, *Quarterly Review of Biology* 21 (1946): 80.

appropriate experiments to test hypotheses, analyzing the experimental results, and drawing conclusions."[37]

The historical case-study approach not only allowed students to learn by discovery, but also helped them develop a new way of *visualizing* organisms. The idea that genetics was a way of seeing was first articulated by Bateson in *Mendel's Principles*. It was necessary to overcome the "habit of looking on the bodies of animal and plants as *single* structures," he argued: "So soon as the mind becomes thoroughly accustomed to the fact that all individuals…are *double*"—or, made up of pairs of factors—"it becomes easier to think in Mendelian terms."[38] Bateson and the early Mendelians saw training in genetics as the internalization of a new way of seeing, in which the single structure of the organism was replaced by its underlying double gametic structure.[39] Once the mind had been trained, they argued, "the world of gametes…comes naturally and persistently before the mind." By learning to "penetrate behind the visible appearances of the individual," the student could see "the definite systems" from which it originated.[40] In the 1920s, Bateson's language of "seeing-through" was regularly used in genetics textbooks. Sinnott and Dunn's *Principles of Genetics*, for example, emphasized the visual character of geneticists' knowledge in a discussion of the nature of scientific laws and Mendel's work. Mendel was praised for his ability to find the "constant relationships" and "fundamental order" in phenomena that "appear at first sight to be irregular and unpredictable." According to this line of explanation that was developed at length, it was Mendel's ability to see order through the apparent disorder—"to *see* certain simple relationships which *underlie* the facts"—that allowed him to discover the first two

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[37] I. H. Herskowitz, *Genetics*, 1st and 2nd eds. (Boston: Little, Brown, and Company, 1962, 1965), v.

[38] W. Bateson, *Mendel's Principles of Heredity*, 1st ed. (Cambridge, England: University Press, 1909), 56 (emphasis original).

[39] See also, e.g., Punnett, *Mendelism*, 3rd ed., Chapter One, esp. 5: "Since a zygote arises from the yoking together of two separate gametes, the individual so formed must be regarded throughout its life as a double structure…But when the zygote in its turn comes to form gametes, the partnership is broken and the process reversed. The component parts of the dual structure are resolved with the formation of a set of single structures, the gametes."

[40] Bateson, *Mendel's Principles of Heredity,* 1st ed., 56. Bateson's approach is described as the "method by which the process of segregation is visualized," in R. R. Gates, "Mendelism," Review of *Mendel's Principles of Heredity*, by W. Bateson, and *Mendelism*, by R. C. Punnett, *Botanical Gazette* 48 (1909): 62.

laws of genetics.[41] Insofar as the importance of Mendel's work was conceptualized in terms of his ability to see beneath a deceptive veneer, the approach of the geneticist was thought of as distinctively and importantly visual. This emphasis on the visual character of the principles continued through the 1960s, and was manifest in the centrality of illustrations in textbooks. According to Adrian Srb and Ray Owen, for example: "The figures and tables are meant to be an integral part of our presentation of genetics; they should be considered in context and not as isolated embellishments."[42]

The figures and tables that were written as part of the Mendel case-study, and were used to inculcate the geneticists' way of seeing, were standardized early in the development of the teaching tradition. From the 1920s onwards, authors relied heavily on three types of pedigree maps—which I will identify as "abstract," "schematic," and "illustrated"—all of which were copied from the first generation textbooks of Bateson and Punnett.[43] These three types of maps were similar in that they all represented the potential genetic relationships between parental and filial generations, but there were two variables in their presentation of this potential: the manner of representing the genes, which ranged from symbolic to embodied, and the aspect of Mendel's laws that was emphasized, which ranged from specific basic ratios to general pictures of the process.

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[41] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 36; see also 42, where Sinnott and Dunn state that Mendel's key revelation was his realization that appearance could not be taken as indicative of genetic constitution.

[42] A. M. Srb, R. D. Owen, and R. S. Edgar, *General Genetics*, 2nd ed. (San Francisco: W. H. Freeman, 1965), vii. See also, e.g., A. M. Srb and R. D. Owen, *General Genetics*, 1st ed. (San Francisco: W. H. Freeman, 1952), vi. These illustrations were described as "valuable" and "particularly effective" in G. Pontecorvo, Review of *General Genetics*, by A. M. Srb and R. D. Owen, *Nature* 171 (1953): 1039.

[43] Note that the standardization of these figures and tables—the copying of Bateson's and Punnett's textbooks by later authors—is a use of history, discussed briefly in the Introduction, that is outside the scope of this chapter.

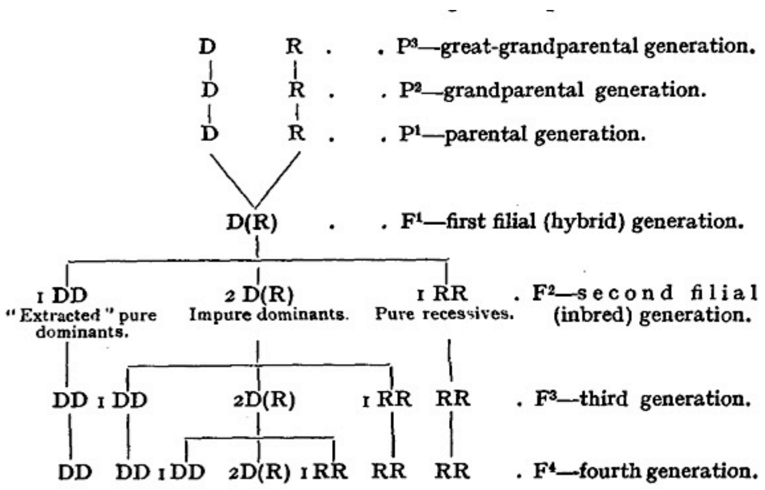

Figure 1: Abstract Pedigree Map.[44]

The "abstract" pedigree maps (see Figure 1) were unique in that they carried self-fertilization to the F_4 generation, thereby indicating the broader pattern that would emerge in the self-crossing of all subsequent generations' homozygous dominant, homozygous recessive, and heterozygous lines.[45] They showed the student how to think about the process of segregation at the scale of an infinite series of filial generations, and how to think about genes in simple symbolic terms. The next type of figure, the "schematic" map (see Figure 2), removed the amateriality and atemporality of the

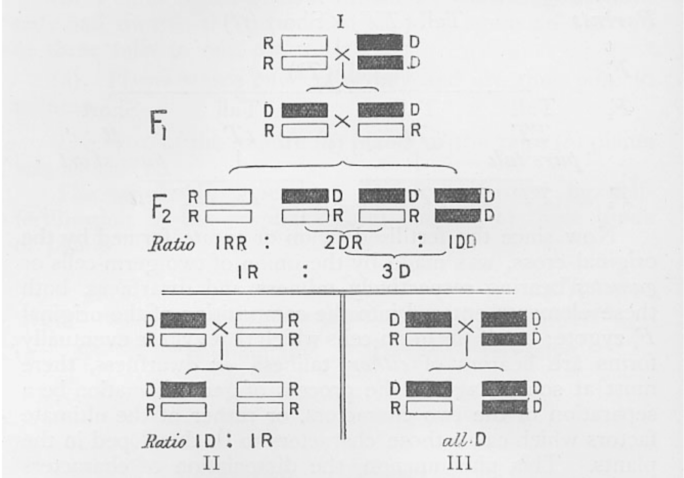

Fig. 2. Diagrams showing numerical consequences of segregation. I. The mating *DD* × *RR*, and *DR* × *DR*. II. The mating *DR* × *RR*. III. The mating *DR* × *DD*.

Figure 2: Schematic Pedigree Map.[46]

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[44] J. A. Thomson, *Heredity*, 1st ed. (London: J. Murray, 1908), 343.

[45] This type of figure, originally from Punnett's *Mendelism,* was widely used: see, e.g., Bateson, *Mendel's Principles of Heredity,* 1st ed., 11; A. F. Shull, *Heredity*, 1st ed. (New York: McGraw-Hill, 1926), 49; Walter, *Genetics*, Rev. ed., 104; and Walter, *Genetics*, 4th ed., 61.

[46] Bateson, *Mendel's Principles of Heredity,* 1st ed., 12.

abstract representation; it gave bodies to the genes, and focused on the specific moment in segregation that most interested the geneticist—the formation of the 1:2:1 genotypic ratio, with its corresponding 3:1 phenotypic ratio.[47] In addition, unlike the "abstract" map, it showed the phenotypic results of crossing heterozygous and homozygous members of the F_2 generation. With this type of pedigree map, the student visualized the genes as things combining into one of a limited number of potential patterns, creating one of two phenotypes: D, or R. The third type of figure, the "illustrated map" (see Figure 3), mirrored the schematic map's F_1 and F_2 generations, but represented incomplete dominance and located the genes and the law of segregation in the world of organisms.[48]

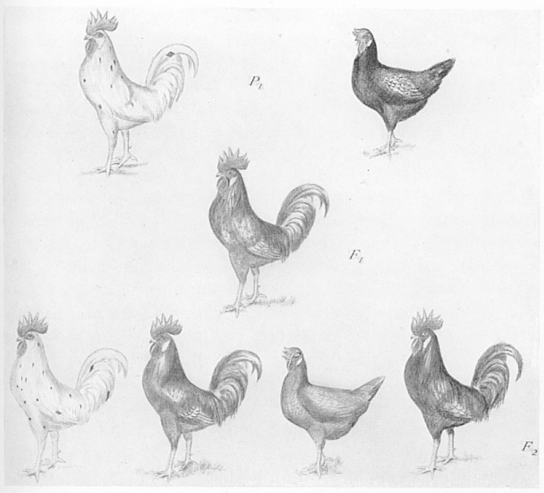

FIG. 3.—Cross between splashed-white and black, giving in F_1 Andalusian, and in F_2 one splashed-white, two Andalusian, and one black.

Figure 3: Illustrated Pedigree Map.[49]

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[47] These "schemes" (Punnett) or variations of them were regularly used in other textbooks: see, e.g., W. E. Castle, *Genetics and Eugenics: A Text-book for Students of Biology and a Reference Book for Animal and Plant Breeders*, 2nd ed. (Cambridge, MA: Harvard University Press, 1920), 141; King, *Genetics*, 1st ed., 70; and Walter, *Genetics*, 4th ed., 60.

[48] This figure or variations of it were used by many authors: see, e.g., Altenburg, *Genetics*, Rev. ed., 31, 41; N. Fasten, *Principles of Genetics and Eugenics: A Study of Heredity and Variation in Plants, Animals, and Man* (Boston: Ginn and Company, 1935), 193, 195, 219, 220; Shull, *Heredity*, 1st ed., 47, 51; Sinnott and Dunn, *Principles of Genetics*, 1st ed., 41, 42, 46, 86; Srb and Owen, *General Genetics*, 1st ed., 20; Thomson, *Heredity*, 1st ed., Figure 37; and M. W. Strickberger, *Genetics*, 1st ed. (New York: Macmillan, 1968), 154. Note that in Castle, *Genetics and Eugenics*, 2nd ed., there were more than 150 such figures, most of which were *photographs* of mice in the form of the 'Illustrated Pedigree Map.'

[49] Morgan, *The Physical Basis of Heredity*, Figure 3/Plate 2.

It connected the abstract law and its schematic representation to what the student saw in life, providing the visual surface, the "somatic conditions," of a phenomenon that had been unveiled in the previous maps. It emphasized the identity between the apparent single structures of the organisms and the underlying double structures of reality.

While students using the textbooks of Bateson and Punnett in the 1910s learned how to see through hereditary phenomena by synthesizing these three types of maps, students in the following decade had this work done for them by their textbooks. In the 1920s, new types of figures were introduced that either juxtaposed (see Figure 4) or superimposed (see Figure 5) the "schematic" and "illustrated" pedigree maps.[50]

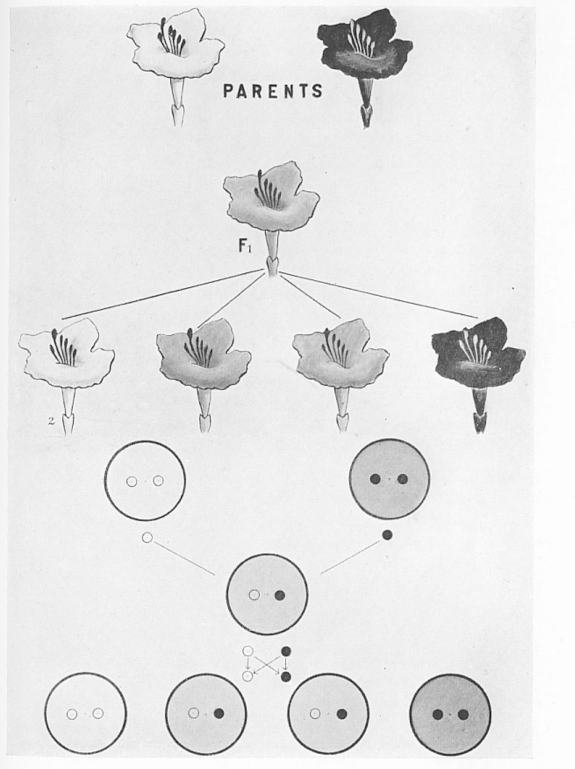

FIG. 2.—Cross between white and red flowered four-o'clocks (*Mirabilis jalapa*). In the lower part of the diagram the large circles represent somatic conditions, the included small circles the genes that are involved.

Figure 4: Juxtaposition of
Schematic and Illustrated Maps.[51]

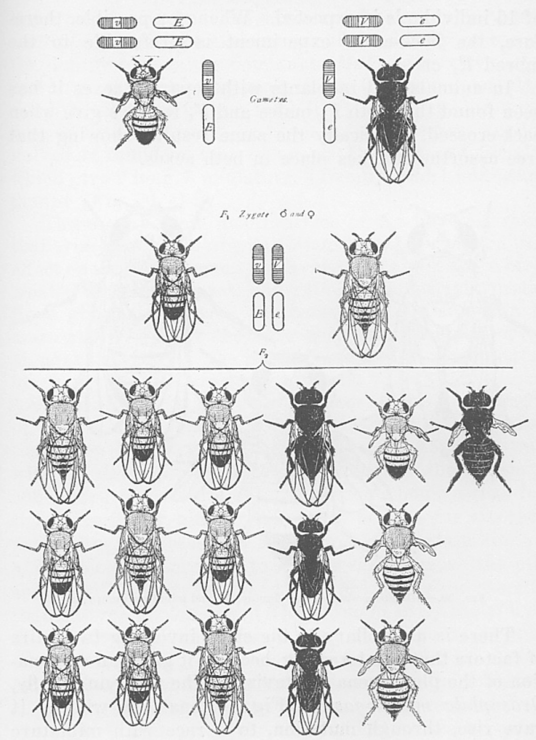

FIG. 29.—Cross between wingless and ebony vinegar fly.

Figure 5: Superimposition of
Schematic and Illustrated Maps.[52]

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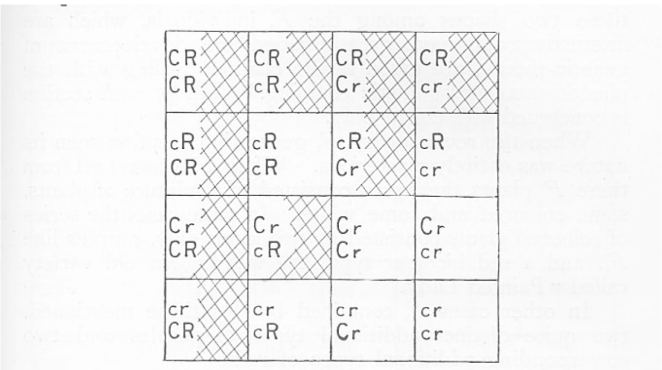
[50] This style was first used in Morgan, *The Physical Basis of Heredity*, e.g., 20, 81, 83, 89, 90. These fly diagrams were widely reproduced: see, e.g., R. C. Punnett, *Mendelism*, 5th ed. (London: Macmillan, 1919), Fig. 31, Fig. 32; Fasten, *Principles of Genetics and Eugenics*, 217, 218, 232, 233; Sinnott and Dunn, *Principles of Genetics*, 1st ed., 163, 164, 202, 204, 216; and Sturtevant and Beadle, *An Introduction to Genetics*, 25, 26. Variations of them were also adopted: see, e.g., Sinnott and Dunn, *Principles of Genetics*, 1st ed., 48, 157; and Winchester, *Genetics*, 1st ed., 60, 62, 66.

[51] Morgan, *The Physical Basis of Heredity*, Figure 2/Plate1.

[52] Ibid., 65.

I have thus far focused on the ways in which illustrations were used to help the student visualize the movement of genes according to Mendel's laws, but this was not their only function. In addition, they were employed to help the student determine the potential outcomes of a cross. Particularly when dealing with the independent assortment of two or three pairs of genes, the mathematics could be confusing. One of the most widely employed mathematical aids was the "Punnett Square."[53] This form of illustration not only simplified the process of determining the potential genotypic combinations, but also allowed for the representation of the phenotypic ratios that could be expected to result from the cross (see Figure 6, where a cross between *Cr* and *cR* is shown to result in a 9:7 phenotypic ratio—a modification of 9:3:3:1). This style of diagram, which was introduced by Punnett in 1905, soon after became ubiquitous in chapters on Mendel's laws. Although Castle referred to them as "the ingenious checkerboard method devised by Punnett," they were generally just called "Punnett Squares."[54] Thus they were not presented as "a method," but rather as the natural way of calculating the outcome of crosses, their historicity buried in the textbook.[55]



CR CR	CR cR	CR Cr	CR cr
cR CR	cR cR	cR Cr	cR cr
Cr CR	Cr cR	Cr Cr	Cr cr
cr CR	cr cR	cr Cr	cr cr

Fig. 16. Composition of the 9 coloured and 7 albino offspring in F_2 from the cross between albino *Cr* with albino *cR*, showing the ratio 9 coloured : 7 albinos.

Figure 6: "Diagram" of Inheritance, or Punnett Square.[56]

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[53] Punnett called these "diagrams" in R. C. Punnett, *Mendelism*, 1st ed. (Cambridge, England: Macmillan and Bowes, 1905), but they were later named after him. They were widely used: see, e.g., Castle, *Genetics and Eugenics*, 2nd ed., 116; Morgan, *The Physical Basis of Heredity*, 60-63, 68, 71; and Sinnott and Dunn, *Principles of Genetics*, 1st ed., 93, 97, 103, 105, 107, 109.

[54] Castle, *Genetics and Eugenics*, 1st ed., 104.

[55] Note that the Punnett Square was just one method for calculating the outcome of crosses: Sturtevant and Beadle, who wanted to emphasize the mathematical nature of genetics in their 1939 textbook, complemented Punnett Squares with a pedigree map that employed algebra to calculate the outcome of multi-gene crosses. See e.g., Sturtevant and Beadle, *An Introduction to Genetics*, 54, 55. However, this math-intensive type of diagram did not gain greater currency, and the Punnett Square remained the dominant method.

[56] Bateson, *Mendel's Principles of Heredity,* 1st ed., 89.

In the 1920s, Sinnott and Dunn further developed their function, combining them with T. H. Morgan's superimposed schematic and illustrated maps (see Figure 7). This synthesis of representations, which was widely adopted in the 1930s and 1940s, allowed the student to visualize the organisms, their underlying genotypes and potential gametes, and the theoretically predicted phenotypic and genotypic ratios.[57]

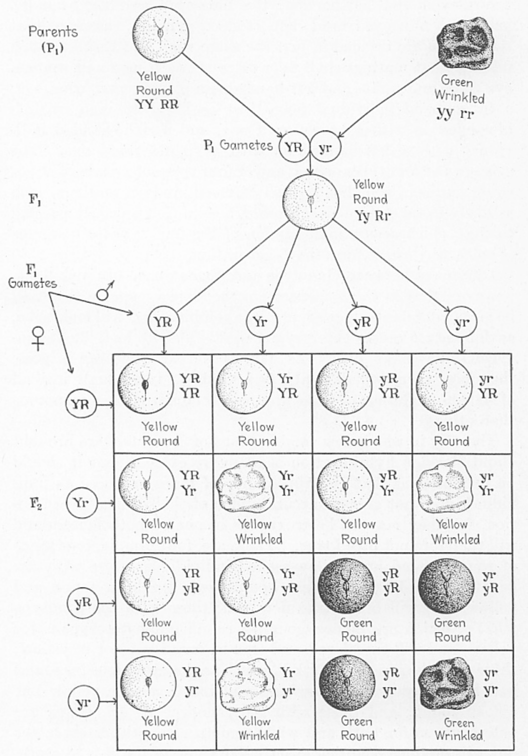

FIG. 29.—Diagram showing the independent assortment in peas of two pairs of characters in which dominance is complete. In a cross between a plant homozygous for yellow and round seeds and a green, wrinkled-seeded one, the appearance, genotype and gametes of parents and F_1 are shown. The results of random union between the four types of gametes formed by the F_1 heterozygote are presented in the F_2 checkerboard.

Figure 7: Synthesis of Various Illustrations.[58]

While the illustrations of Mendel's laws took many forms, they were all similar in that they were designed to teach the student to think and see in terms of potential phenotypic and genotypic *ratios*—a central feature of genetics, according to textbook authors and geneticists later reflecting on the history of their science. In *An Introduction to Genetics*, for example, A. H. Sturtevant and George W. Beadle stated: "Genetics is a

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[57] See, e.g., L. H. Snyder, *The Principles of Heredity*, 2nd ed. (Boston: D. C. Heath, 1940), 14, 46, 49, 50, 52, 53, 55, 57, 80, 81, 88; Sturtevant and Beadle, *An Introduction to Genetics*, 53, 57; Altenburg, *Genetics*, 1st ed., 55; Winchester, *Genetics*, 1st ed., 74, 76; and Strickberger, *Genetics*, 1st ed., 101, 111.

[58] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 69; see also 53, 54, 71, 91 146.

quantitative subject. It deals with ratios, with measurements, and with the geometrical relationships of chromosomes."[59] At the end of the textbook, the authors returned to this point: "As indicated in Chapter 1 and repeatedly thereafter in this book, genetics is to a large extent a science of ratios."[60] J. A. Detlefsen agreed in a textbook review in *Science*, criticizing the author for trying to "reduce mathematical formulae to a minimum":

> Underlying all distributions of characters in assortive matings are certain elementary principles based on probabilities and the theory of simple sampling. When the student looks upon a Mendelian population in these terms, he has the advantage of a general fundamental law rather than the knowledge of an individual case.[61]

Sinnott, Dunn, and Dobzhansky likewise thought that the student's default type of thinking should be one of ratio-finding: "In connection with answers to these problems, we should emphasize that where solutions can be stated in terms of exact ratios or probability, this should always be done, *even when it is not specifically called for*."[62] The terms "ratio" and "law" were often used interchangeably. Mendel's law of segregation was generally presented as the law that in the most simple case produced a 3:1 phenotypic or 1:2:1 genotypic ratio; and Mendel's law of independent assortment was that which produced a 9:3:3:1 phenotypic ratio. These ratios were the neutral position—the standard against which "exceptions" were defined. They could be altered by various factors, but the results were framed as mere modifications of these ratios. For example, the 3:1 phenotypic ratio could become a 1:2:1, or the 9:3:3:1 could become a 9:7. The student had to be able to see the 1:2 inside the 3, or the 3:3:1 inside the 7. Depending on variations in dominance within pairs of alleles, or interaction between allelic pairs, the ratio of phenotypes could break down along the lines of the underlying genotypes. Several authors used illustrations with a variety of columns to show how the

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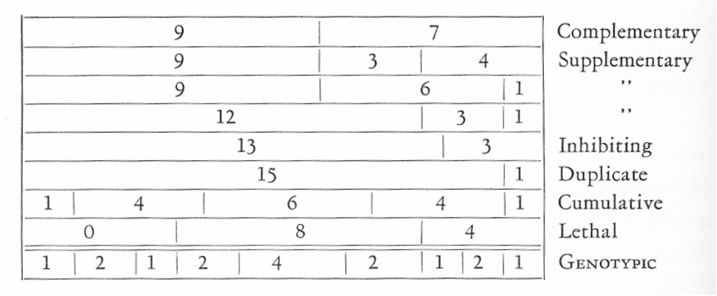
[59] Sturtevant and Beadle, *An Introduction to Genetics*, 11. See also Dunn's claims about "the ordering influence of the kind of thinking characteristic of genetics," in L. C. Dunn, *A Short History of Genetics: The Development of Some of the Main Lines of Thought, 1864-1939* (New York: McGraw-Hill, 1965), 212. Dunn also discusses the importance of "ratios" on page 32.

[60] Sturtevant and Beadle, *An Introduction to Genetics*, 367.

[61] J. A. Detlefsen, Review of *Genetics: An Introduction to the Study of Heredity*, by H. E. Walter, *Science* 56 (1922): 146.

[62] Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 5th ed., viii (emphasis added).

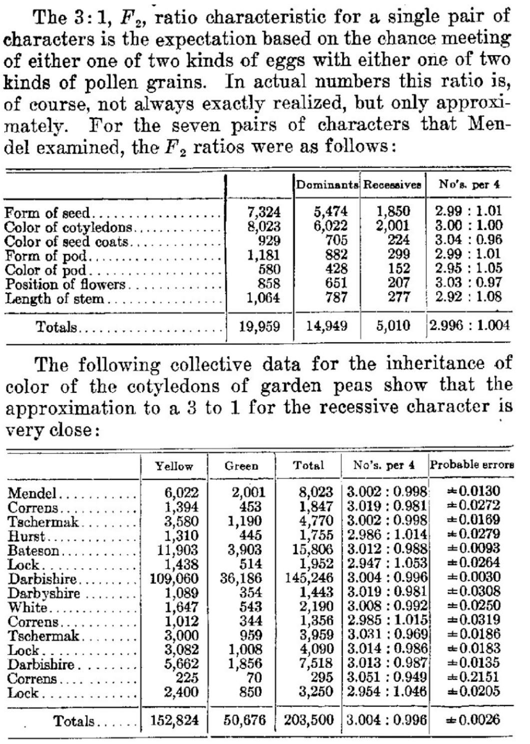
standard phenotypic ratios corresponded with the genotypic, and how the 9:3:3:1 ratio could take other forms (see Figure 8).[63]



9 | 7 Complementary
9 | 3 | 4 Supplementary
9 | 6 | 1 ,,
12 | 3 | 1 ,,
13 | 3 Inhibiting
15 | 1 Duplicate
1 | 4 | 6 | 4 | 1 Cumulative
0 | 8 | 4 Lethal
1 | 2 | 1 | 2 | 4 | 2 | 1 | 2 | 1 GENOTYPIC

Figure 8: Variations of the 9:3:3:1 ratio.[64]

In teaching the students to recognize the standard ratios, authors often referred to tables of experimental data from historically significant experiments (see Figure 9). These tables—which were presented as though they contained raw, rather than selected, historical data—played a central role in placing the student on the trajectory by which the



The 3 : 1, F_2, ratio characteristic for a single pair of characters is the expectation based on the chance meeting of either one of two kinds of eggs with either one of two kinds of pollen grains. In actual numbers this ratio is, of course, not always exactly realized, but only approximately. For the seven pairs of characters that Mendel examined, the F_2 ratios were as follows:

		Dominants	Recessives	No's. per 4
Form of seed	7,324	5,474	1,850	2.99 : 1.01
Color of cotyledons	8,023	6,022	2,001	3.00 : 1.00
Color of seed coats	929	705	224	3.04 : 0.96
Form of pod	1,181	882	299	2.99 : 1.01
Color of pod	580	428	152	2.95 : 1.05
Position of flowers	858	651	207	3.03 : 0.97
Length of stem	1,064	787	277	2.92 : 1.08
Totals	19,959	14,949	5,010	2.996 : 1.004

The following collective data for the inheritance of color of the cotyledons of garden peas show that the approximation to a 3 to 1 for the recessive character is very close:

	Yellow	Green	Total	No's. per 4	Probable errors
Mendel	6,022	2,001	8,023	3.002 : 0.998	±0.0130
Correns	1,394	453	1,847	3.019 : 0.981	±0.0272
Tschermak	3,580	1,190	4,770	3.002 : 0.998	±0.0169
Hurst	1,310	445	1,755	2.986 : 1.014	±0.0279
Bateson	11,903	3,903	15,806	3.012 : 0.988	±0.0093
Lock	1,438	514	1,952	2.947 : 1.053	±0.0264
Darbishire	109,060	36,186	145,246	3.004 : 0.996	±0.0030
Darbyshire	1,089	354	1,443	3.019 : 0.981	±0.0308
White	1,647	543	2,190	3.008 : 0.992	±0.0250
Correns	1,012	344	1,356	2.985 : 1.015	±0.0319
Tschermak	3,000	959	3,959	3.031 : 0.969	±0.0186
Lock	3,082	1,008	4,090	3.014 : 0.986	±0.0183
Darbishire	5,662	1,856	7,518	3.013 : 0.987	±0.0135
Correns	225	70	295	3.051 : 0.949	±0.2151
Lock	2,400	850	3,250	2.954 : 1.046	±0.0205
Totals	152,824	50,676	203,500	3.004 : 0.996	±0.0026

Figure 9: Experimental Data Table.[65]

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[63] See, e.g., Snyder, *The Principles of Heredity*, 2nd ed., 59; and Winchester, *Genetics*, 1st ed., 82, and 3rd ed., 96. Variations of this were used in Shull, *Heredity*, 4th ed., 117, 121, and other tabular forms of representing the modified ratios were used in Strickberger, *Genetics*, 1st ed., 191-194. Note that ratios also played an important role in the narrative structure of textbooks—a topic that I will discuss in Chapter 3.

[64] Walter, *Genetics*, 4th ed., 82.

[65] Morgan, *The Physical Basis of Heredity*, 24.

ratios, and the principles of genetics, had been discovered.[66] The authors would walk the students through Mendel's experiments, and provide them with tables of his experimental results; in this way they would demonstrate how Mendel interpreted the data, identified the 3:1 and 9:3:3:1 ratios, and formulated the principles of segregation and independent assortment. They would then present the students with data collected in the early 1900s by Correns, Tschermak, Hurst, Bateson and other historically significant figures. These tables were said to show the correspondence between the mathematical predictions and the geneticists' data. In *Principles of Genetics*, for example: "This ratio [3:1]—and the same is true of the other mendelian ratios—merely indicated what may be expected on the basis of probability. Experience, agreeing with theoretical expectation, has shown that the larger the number of individuals raised, the closer the F_2 ratio approaches ¾:¼, a fact strikingly emphasized in the table just cited."[67] In this way, tables containing data from replications of Mendel's experiments at the turn of the century were presented as verifications of his conclusions.

Textbooks also included tables of data gathered in early twentieth-century experiments testing the validity of Mendel's laws on other organisms.[68] These tables were used as part of the narrative by which the student experienced the development of the science, discovering apparent exceptions to Mendel's principles, and finding ways of resolving them. In *General Genetics*, for example, Srb, Owen and Edgar provided a detailed description of some experiments that Morgan conducted after finding a white-eyed male in a population of red-eyed *Drosophila*.[69] They explained that crossing the white-eyed male with a red-eyed female produced all red-eyed F_1 generation, which suggested that "white-eye" was a standard recessive gene, but that when Morgan crossed

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[66] See, e.g., Sinnott et al., *Principles of Genetics*, 1st ed., 44, 47; 2nd ed., 45, 48; 3rd ed., 45, 47; 4th ed., 38, 42; 5th ed., 38; Walter, *Genetics*, Rev. ed., 100; 4th ed., 60-6; Strickberger, *Genetics*, 1st ed., 101; W. Hovanitz, *Textbook of Genetics* (New York: Elsevier Press, 1953), 32-33; C. M. M. Begg, *An Introduction to Genetics* (London: The English Universities Press, 1959), 40; Srb, Owen, and Edgar, *General Genetics*, 2nd ed., 5; and Winchester, *Genetics*, 3rd ed., 71.

[67] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 47, where they also stated: "Later work on peas by other investigators has completely confirmed Mendel's results." NB: the term "mendelian" was not capitalized in the first edition of *Principles of Genetics*.

[68] See, e.g., Hovanitz, *Textbook of Genetics*, 50, 59; Begg, *An Introduction to Genetics*, 54, 57; Dodson, *Genetics*, 35; L. H. Snyder, *The Principles of Heredity*, 1st ed. (Boston: D. C. Heath, 1935), 79; Snyder, *The Principles of Heredity*, 2nd ed., 93; Srb and Owen, *General Genetics*, 1st ed., 23; and Srb, Owen, and Edgar, *General Genetics*, 2nd ed., 19, 36, 55, 57.

[69] Srb, Owen, and Edgar, *General Genetics*, 2nd ed., 35-37. See also, e.g., Snyder, *The Principles of Heredity*, 1st ed., 64-68.

the F_1 generation, he found an anomaly: "In the F_2 generation there were 3,470 red-eyed flies and 782 white-eyed ones. The data fit rather badly the ratio we might expect, but for the moment we might accept the numbers as representing a Mendelian 3:1 ratio with some shortage in the recessive class."[70] Describing Morgan's subsequent experiments, which confirmed that there was in fact a deviation from the 3:1 ratio, and presenting the data he obtained, Srb and Owen guided the student through Morgan's line of thought. It was in this way that the student learned of, or "discovered," sex-linkage.[71] According to A. F. Shull, an author of numerous popular genetics and biology textbooks, this use of experimental data was standard in the 1930s: "The traditional method has been to present experimental results," and then "formulate a scheme of gene operations which will logically explain them."[72] Herskowitz explained his use of this method in the introduction to *Genetics*: "The presentation is designed to encourage the reader to use his power of inductive reasoning to arrive at the primary generalizations of genetics on the basis of experimental evidence."[73] Thus, data tables performed an important narrative function in the historical case-studies of genetics textbooks, challenging established theory and thereby driving the student's "discovery" of new principles.[74]

Historical data did not just serve an important narrative function, however, but also and more importantly was used in teaching the student to see key ratios amidst experimental results. For example, after a discussion of one of Mendel's experiments that resulted in a 24:25:22:26 phenotypic ratio, Sturtevant and Beadle wrote: "It is clear that, in each case, the four possible classes of gametes were produced in equal numbers…giving a 1:1:1:1 ratio."[75] Elsewhere, they described a 315:108:101:32 ratio as "obviously a close agreement with the expected 9:3:3:1."[76] This type of statement—proclaiming that it was self-evident that experimentally-derived ratios corresponded with given ideal ratios—was common in genetics textbooks, and was involved in showing the student how to see ratios amidst the data. In addition, they defined what counted as

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[70] Srb, Owen, and Edgar, *General Genetics*, 2nd ed., 36.

[71] Ibid., 37.

[72] Shull, *Heredity*, 3rd ed., vii-viii. Shull's *Principles of Animal Biology* received an excellent review in McClung, "Scientific Books: Elementary Biological Texts."

[73] I. H. Herskowitz, *Genetics*, 1st and 2nd eds., v.

[74] In Chapter 3, I will discuss this *exception-overcome-by-extension* narrative device in greater depth.

[75] Sturtevant and Beadle, *An Introduction to Genetics*, 52.

[76] Ibid., 55.

"agreement" between the theoretical and experimental results. In textbooks of the 1930s, there was no explicit rule or norm that defined or quantified this relationship.[77] Rather, *agreement* was a matter of *reasonable disagreement*.[78] It was a judgment, and the knowledge needed to make such judgments was not taught explicitly. Instead, textbooks showed what counted as reasonable disagreement with examples, and it was in this capacity that data tables, which compared theoretical predictions with experimental findings, played another crucial role. The tables were models demonstrating cases of acceptable agreement, thereby conveying historically agreed-upon conventions. Thus, they did not represent a natural order, but rather presented the social order of genetics.[79] They provided the basis for the analogical reasoning by which the student could perform judgments about agreement in other cases. They taught the tacit skills of judgment needed to decide whether a specific set of data corresponded with the action of general principle—whether a given case counted as a case of a general rule. Insofar as the tables conveyed aspects of the geneticists' culture that were necessary for understanding and making use of the principles, the tables cannot be properly understood as offering mere support for them. They were, rather, essential pieces of theory. By studying them, the student learned what to expect from the principles of genetics. He came to understand how they were applied to, and supported by, data. As Kuhn suggested, "An acquaintance with the tables is part of an acquaintance with the theory itself. Without the tables, the theory would be essentially incomplete."[80]

In addition to helping define the principles of genetics, experimental data in textbooks contributed to the presentation of the disciplinary identity of genetics. Textbooks in the 1920s and 1930s highlighted the mathematical character of the principles. In Morgan's *The Physical Basis of Heredity*, for example: "They [Mendel's

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[77] It was not until the 1940s that the "Chi-square" method for testing the "goodness of fit" became a common feature of textbooks. Sinnott and Dunn added it to their third edition in 1939, and Snyder added it to his second edition in 1940. With the Chi-Square test, the students were given a statistical rule that defined whether their data followed a genetic rule. Whether this introduction of a rule to follow a rule closed the problem of judgment is a question that lies outside my present focus.

[78] In making this argument, I draw on the analyses developed in T. Kuhn, "The Function of Measurement in Modern Physical Science," *Isis* 52 (1961): 165-167; and Barnes, *T. S. Kuhn and Social Science*, 20-22.

[79] Barnes, *T. S. Kuhn and Social Science*, 21. Barnes also notes that the authority of the model of agreement does not come from its correspondence with facts about the data and predictions, but rather comes from the fact that the model is located in the textbook—that it has the collective support of the scientific community.

[80] Kuhn, "The Function of Measurement in Modern Physical Science," 166.

laws] rest on numerical data, and are therefore quantitative and can be turned into mathematical form wherever it seems desirable."[81] Morgan and his contemporaries regularly demonstrated this fact by translating crosses into algebraic terms. In *Genetics: An Introduction to the Study of Heredity*, H. E. Walter wrote of Mendel's experiments:

> These crosses may be expressed as follows:—Tall, *T*, x dwarf, *t*, = tall, *T(t)*…When now the hybrids, *T(t)*, were crossed together, *the result algebraically expressed was as follows*: —
>
> T + t (all possible egg characters)
> $\underline{\ T + t\ }$(all possible sperm characters)
> TT + Tt
> $\underline{\qquad Tt + tt}$
> TT + 2T(t) + tt
>
> That is, one of the possible four cases is dwarf, *tt*, in character and the other three are apparently tall, although only one of the three is pure tall, *TT*.[82]

The quantitative foundation of Mendel's generalizations was crucial for the identity of genetics, because it justified the geneticist's decision to refer to them as 'laws.' Morgan emphasized this in the introduction to his textbook:

> Despite the fact that the use of this word 'law' has been much abused in popular writing we need not apologize for using it here, because the postulates in question have been established by the same scientific procedure that chemists and physicists make use of, viz., by deductions from quantitative data.[83]

In these ways, genetics was presented as being grounded in quantification. But this explicit quantification of heredity relied on the authors' ability to first teach tacit skills of qualitative judgment and evaluation.[84]

To briefly summarize my analysis of the use of historical case-studies in the teaching of Mendel's principles: Mendel was presented as an exemplary, or model, scientist in most genetics textbooks, and in this capacity his work came to serve as one of the discipline's most important *exemplars*. Geneticists believed that Mendel's principles were best "illustrated by typical examples from the experiments of Mendel and later

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[81] Morgan, *The Physical Basis of Heredity*, 16. See also, e.g., Kimball and Gersh, "Genetics and Cytology," 80, which criticized Altenburg's failure to discuss the statistical treatment of data: "Since Genetics is frequently the only course in college where the student applies quantitative methods in biology, this omission seems most unfortunate."

[82] Walter, *Genetics*, Rev. ed., 99 (emphasis added). See also, e.g., Walter, *Genetics*, 4th ed., 59.

[83] Morgan, *The Physical Basis of Heredity*, 16-17.

[84] This general idea comes from Kuhn, "The Function of Measurement in Modern Physical Science," in which he argues that significant qualitative work is the prerequisite for quantification in the physical sciences.

investigators," and thus taught genetics through ostention.[85] Representations of "classic" experiments, in detailed descriptions and in illustrations, allowed the students to learn and practice not only those principles that were espoused by geneticists and explicitly taught as rules in the text, but also the tacit skills that were needed to follow, find, or understand these rules. By making students follow the steps by which the principles were discovered, textbooks taught the discipline of genetics, in addition to its dogma. With pedigree maps and tables of experimental results, which appeared to present uninterpreted data but were in fact constructed through careful historical selection, the authors brought the student into a way of seeing organisms and hereditary phenomena. They showed the student how to find a ratio in the midst of experimental data, and how to judge whether the experimental ratio agreed with the theoretical. Ratio-finding was central to the science, for it was in this skill that the quantitative and visual aspects of genetics came together. In a sense, the Mendelian exemplars taught translation. They showed the student how to translate breeding experiments into pedigree maps, schematic pedigree maps into organisms, experimental data into theoretical ratios, and Mendel's laws into algebraic equations. In teaching these forms of translation, or ways of seeing one thing as being like another, the Mendelian exemplar taught some of the family resemblances that were crucial to the culture of geneticists. In other words: the detailed descriptions of Mendel's experiments and the accompanying illustrations and data were a virtual historical environment—a reconstruction of the past in which the student was socialized, developing the experientially-based, tacit skills needed to see, discriminate, and evaluate with the geneticists' approach.

Problem-Solving in—and as—a Virtual Historical Laboratory:
Teaching rule-following, or, how to apply the principles of genetics

Thus far I have focused on how narrative and illustrative features of the textbooks were used to bring the student into the process of discovering the principles of genetics. But it was not only these features of textbooks that were used to create a virtual historical environment in which the student developed the tacit skills of the geneticist. As Kuhn

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[85] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 40. See also, e.g., Castle, *Genetics and Eugenics*, 1st ed., 88: "Mendel's law may best be explained with the aid of examples."

suggested in his analysis of exemplars, problem-solving exercises played an essential role in the historical case-study method of teaching, and most geneticists agreed with Dunn in 1965 when he wrote: "Genetics…can be learned by imitating the steps by which its principles were established, that is by deriving such results from sets of numerical data *in the form of problems*, and vice versa, by predicting the outcome of experiments from a knowledge of principles."[86] The important point here is that "imitating the steps" of discovery was seen as comparable to problem-solving—to deriving principles from data, and predicting data from principles. To show how and why problem-solving exercises functioned and were seen to function in this way, and to illustrate the broader historical significance of this understanding of their capabilities, I will turn to the origin of their use in genetics textbooks: the 1925 edition *Principles of Genetics*, by Sinnott and Dunn.

As discussed earlier in this chapter, Sinnott and Dunn articulated their pedagogical aims in the introduction to *Principles*. Learning genetics was not a matter of learning facts. It was, rather, the enculturation into an approach, which required "considerable practice of the reasoning faculty by which deductions are made, and applied or tested on many similar cases."[87] Emphasizing the importance of the repetitive practice of reasoning and thinking in cases, Sinnott and Dunn wrote: "It is only in this way that the process of inheritance can be *understood*. The learning of facts alone cannot accomplish this."[88] To bring the student into the process of discovering and working with the principles, Sinnott and Dunn created problem-solving exercises, which they explained: "As an aid to such a comprehension of the science of genetics, this book includes problems of three types, which form an integral part of the subject matter." These problems took the form of "Questions," "Reference Assignments," and "Problems." Of these, the Problems—which were problem-solving exercises—were meant to "provide opportunity for practicing and extending the methods and applying the theories outlined in the text."[89] The authors conceived of problem-solving as a form of

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[86] Dunn, *A Short History of Genetics*, 208.

[87] Sinnott and Dunn, *Principles of Genetics*, 1st ed., xvii.

[88] Ibid. (emphasis original). Castle articulated a similar goal for his textbook in his comments about knowledge and independent thinking: Castle, *Genetics and Eugenics*, 1st ed., preface.

[89] Sinnott and Dunn, *Principles of Genetics*, 1st ed., xvii.

virtual laboratory work: "Nearly all of them…may be most profitably studied *as* laboratory exercises under the guidance of an instructor."[90] And controversially, in explaining what was novel about their textbook, they suggested that these pen and paper exercises allowed for a type of laboratory practice that was in some ways better than the experimental laboratory.

> The Problems are designed chiefly for laboratory practice. Counting corn kernels, breeding fruit flies and measuring larvae are valuable laboratory exercises, but for imparting a thorough understanding and mastery of the principles of genetics, and particularly those of mendelian heredity, the experience of the authors has found nothing equal to persistent drill in solving a wide diversity of problems.[91]

This use of problem-solving marked a significant departure from the textbooks established in the first quarter of the twentieth century, which rarely included problems.[92] Sinnott and Dunn thought that the exercises were "perhaps the most novel feature" of their textbook, which they highlighted in its subtitle, *Principles of Genetics: An Elementary Text, with Problems.*

Although the review in *Nature* identified Sinnott and Dunn's problems, questions, and assignments as "an innovation in this class of book," and praised the problems as "on the whole excellent," the use of problem-solving as a teaching technique in *Principles of Genetics*—and the claim that it could provide a substitute for some types of laboratory experience—was not immediately well received in the broader community.[93] In one generally positive review of the textbook that praised it for its uniquely "balanced presentation of the subject as a whole," Francis Wenninger, zoologist at Notre Dame and associate editor for general biology at the *American Midland Naturalist*, commented: "A feature of the book that is especially desirable is the questions for thought and discussion and the reference assignments."[94] Given that Sinnott and Dunn had drawn attention to

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[90] Ibid., xviii (emphasis added).

[91] Ibid., x.

[92] Problem-solving exercises were not included in any of the standard textbooks before the late 1920s: Thomson, *Heredity*, 1st ed.; Bateson *Mendel's Principles*, 1st ed.; Castle, *Genetics and Eugenics*, 1st, 2nd, and 3rd eds.; Walter, *Genetics*, 1st and Rev eds.; and Shull, *Heredity*, 1st ed.

[93] "Our Bookshelf," Review of *Principles of Genetics*, by E. W. Sinnott and L. C. Dunn, *Nature* 117 (1926): 336.

[94] F. J. Wenninger, Review of *Principles of Genetics*, by E. W. Sinnott and L. C. Dunn, *The American Midland Naturalist* 10 (1926): 135. Although the author of the review is only identified as "F. W.", Francis Wenninger became associate editor of general biology in 1930, according to R. P. McIntosh, "American Midland Naturalist: The Life History of a Journal," *American Midland Naturalist* 123 (1990).

their use of problems, questions, and references assignments, the omission of the "problems" from the list of features that were "especially desirable" suggests that Wenninger did not see them as noteworthy—that either he did not notice them, or that he did not think they were worth criticism or praise. Other reviewers were openly skeptical about their value. Castle, under whom Dunn did his graduate studies at Harvard, disagreed about the utility and effect of engagement in problem-solving. In his review of the first edition of *Principles of Genetics,* Castle noted that "much attention" had been give to the preparation of problems, but he backed away from praising this method of teaching when he said that it was "perhaps a worth-while way to try to get the student to think."[95] This hedged praise turned to criticism when he continued: "it is doubtful whether it is in any sense a fair substitute for a laboratory course in which the student handles the live material, gathers his own data and feels his way along toward conclusions." Castle explained his reasoning: "Most biological problems involve…other powers of the mind than those used in arithmetic…For training in scientific method the student should…be given *real* problems, rather than hypothetical ones."[96] Castle wrote this review of *Principles of Genetics* less than a year after his *Outline for a Laboratory Course in Genetics* was first published, complementing the third edition of his widely used *Genetics and Eugenics*. Presumably, Castle had his "real" problems in mind when criticizing Sinnott and Dunn's problem-solving exercises and arguing that laboratory experiments were essential. On this issue, the basic assumptions of geneticists were clearly divided, as is evident from Charles Davenport's praise for Castle's manual:

> As a teacher he [Castle] has seen the need of bringing the student into firsthand contact with the phenomena of genetics and so he has prepared an outline for a laboratory course in genetics. By use of the rapidly breeding banana fly and of dried ears of corn he has been able to bring students into contact with the methods and results of genetics…This 'outline' will do much to put genetics on a proper pedagogic basis.[97]

Given that the book reviews were written by the journal's editors, it seems likely that this review was written by Wenninger.

[95] W. E. Castle, "Some New Books on Genetics," Review of *Genetics in Plant and Animal Improvement,* by D. F. Jones, *Principles of Genetics*, by E. W. Sinnott and L. C. Dunn, and *Animal Genetics* by F. A. E. Crew, *Science* 62 (1925): 568.

[96] Ibid.

[97] C. B. Davenport, Review of *Genetics and Eugenics*, by W. E. Castle, *Science* 61 (1925): 542.

Here, Davenport's position was antithetical to that articulated by Sinnott and Dunn, who had specifically identified "breeding fruit flies" and "counting corn kernels" as insufficient ways of teaching the fundamentals of genetics.[98] Thus the use of problem-solving exercises in the 1920s was a matter of significant disagreement; geneticists had very different basic conceptions of what it meant to practice and apply the principles of genetics, and of what could be gained through engagement in problem-solving.

As the case-study method of teaching science gained ground in the 1930s, the general reception of Sinnott and Dunn's problem-solving exercises showed signs of change. In a review of the second edition in *Science*, for example, Albert Blakeslee commented with surprise at the great number of problems to be found at the end of each chapter, and the addition of more than 400 problems to the appendix: "The problem method *seems to have met* with success at the hands of the authors since the number of problems given is increased over the earlier edition. They should help the student to a more thorough grasp of the subject as is the case with 'original' problems in text-books of geometry."[99] While Blakeslee's review was not an overt endorsement of the method of teaching—insofar as it was partly descriptive, rather than evaluative—it is indicative of a general shift in many authors' sense of the perceived value of problem-solving. For example: a student of Castle, H. E. Walter, whose *Genetics* had been in print since 1913, introduced a book of problems into the second printing of its third edition (1931), explaining that he only did so because his publisher had assured him that they "would not be entirely unwelcome."[100] A. W. Lindsey likewise included problem-solving in his *A Textbook of Genetics* (1932), explaining: "So far as I know, Sinnott and Dunn in their *Principles of Genetics* (McGraw-Hill, 1925) are the only writers who have previously included this type of material in a textbook; it is a pleasure to acknowledge their good example."[101] In the 1930s, Shull added problems into his *Heredity*, which was first published in 1926 without problems. And new textbooks, such as Sturtevant and Beadle's *An Introduction to Genetics*, were often written with problem-solving sections

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[98] Sinnott and Dunn, *Principles of Genetics*, 1st ed., x.

[99] A. F. Blakeslee, Review of *Principles of Genetics*, by E. W. Sinnott and L. C. Dunn and *Recent Advances in Plant Genetics*, by F. W. Sansome and J. Philip, *Science* 77 (1933): 285 (emphasis added).

[100] H. E. Walter, *One Hundred and One Problems to Accompany Genetics* (New York: Macmillan, 1931), i.

[101] A. W. Lindsey, *A Textbook of Genetics* (New York: Macmillan, 1932), viii. The problem-solving method was widely seen as an "innovation" of Sinnott and Dunn: see, e.g., "Our Bookshelf," 336.

and praised on these grounds.[102] Problem-solving was beginning to be seen as a way of *using* the principles, as Sturtevant and Beadle explained: “Genetics also resembles other mathematically developed subjects, in that facility in the use and understanding of its principles comes only from using them. The problems at the end of each chapter are designed to give this practice. The student will find that it is important that they be actually solved.”[103]

By the time Sinnott and Dunn published the third edition of *Principles of Genetics* in 1939, the language used in reviews had changed dramatically. Plant taxonomist Theodore Just, for example, wrote of this edition: “The many problems now appended to each chapter should prove a splendid means for the student to test his understanding of genetics.”[104] A review of Laurence Snyder’s popular *The Principles of Heredity* stated: “The problems given at the ends of the chapters are well chosen and together with the selected references will be very helpful to the discriminating teacher.”[105] And in a review of Altenburg’s *Genetics*, Stern wrote: “Numerous problems furnish an opportunity for applying knowledge gained from the text.”[106] The form of these claims, as descriptions of fact, suggests that the value of the problems was not in question. These statements were not an answer to the question, ‘Can the use of problems accomplish the necessary goals,’ but rather, ‘Does the text contain the problems that accomplish the necessary goals?’ It was taken as given that problem-solving furnished an opportunity for applying and working with the principles of genetics. Problem-solving was seen as “the surest way to a proper understanding of the subject.”[107] These exercises thus became a staple feature of introductory college textbooks, regardless of their

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[102] C. W. Cotterman, "Recent Books on Genetics," *The American Naturalist* 75 (1941): 598.

[103] Sturtevant and Beadle, *An Introduction to Genetics*, 11.

[104] T. Just, Review of *Principles of Genetics*, by E. W. Sinnott and L. C. Dunn, *The American Midland Naturalist* 22 (1939): 755. While the review does not clearly identify its author, McIntosh’s history of the journal suggests that it would have been Theodore Just: McIntosh, "American Midland Naturalist: The Life History of a Journal."

[105] L. J. Cole, "Genetics Texts," Review of *The Principles of Heredity,* by L. H. Snyder and *Principles of Genetics and Eugenics,* by N. Fasten, *Science* 83 (1936): 373.

[106] Stern, Review of *Genetics*, by E. Altenburg: 515. A review *Quarterly Review of Biology* agreed about the value of its “well constructed problems”: Kimball and Gersh, "Genetics and Cytology," 79.

[107] Begg, *An Introduction to Genetics*, v. See also Srb, Owen, and Edgar, *General Genetics*, 2nd ed., vii: “We continue to believe in the importance of problem-solving as a way to facilitate the student’s mastery of genetics.”

conceptualization and presentation of the nature of the science.[108] The titles of textbooks no longer mentioned the fact that they included problems.[109] Presumably this was because the inclusion of problems was not notable in a culture that saw them as essential, as was stated in a review of the 1950 edition of *Principles of Genetics:* "Today more than ever before, it is essential that a student build for himself a solid foundation of fundamental principles and their applications…An indispensable feature of any such text is the inclusion of a well-balanced set of problems which will efficiently and clearly demonstrate, by means of student participation, the workings of these principles."[110] In the preface to one of the most popular textbooks of the 1960s, *General Genetics*, and in a review of this textbook, it was agreed: "genetics is essentially a 'problem-solving' kind of science."[111]

In a scientific culture in which textbooks were thought to bring students into the experience of discovery, introductory courses did not necessarily need to be complemented by "a laboratory course in which the student handles the live material, gathers his own data and feels his way along toward conclusions."[112] The essential complement to historical case-studies was instead provided by problem-solving in the 1920s-1950s. The only laboratory manuals for introductory courses published in this period were written before 1925, and by first generation geneticists: Babcock and Collins, Castle, Hurst, and Morgan.[113] In addition, it was very rare for a textbook to

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[108] E.g., problem-solving exercises were used in the "logically" organized textbooks, such as Sturtevant and Beadle, *An Introduction to Genetics*, as well as in the traditional "historically" organized textbooks, including: Altenburg, *Genetics*, 1st and Rev. ed.; King, *Genetics*, 1st and 2nd eds.; L. H. Snyder, *The Principles of Heredity*, 1st, 2nd, 3rd and 4th eds. (Boston: D. C. Heath, 1935, 1940, 1946, 1951); and Snyder and David, *The Principles of Heredity*, 5th ed. I will further discuss this distinction between the "logical" and "historical" methods in Chapter 3.

[109] E.g., Sinnott and Dunn's 1939 edition was merely titled, *Principles of Genetics*; the 1925 and 1932 editions had been titled, *Principles of Genetics: An Elementary Text, With Problems*.

[110] S. Fogel and I. H. Herskowitz, Review of *Principles of Genetics*, by E. W. Sinnott, L. C. Dunn, and Th. Dobzhansky, *Quarterly Review of Biology* 27 (1952): 211.

[111] Srb and Owen, *General Genetics*, 1st ed., vi. See also T. W. Whitaker, "Ultra-Modern Genetics," Review of *General Genetics*, by A. M. Srb and R. D. Owen, *Journal of Heredity* 43 (1952): 188: "The author's feel, and properly so, that genetics is a problem solving science."

[112] Castle, "Some New Books on Genetics," 568.

[113] The only manuals for introductory courses published in this period that I have encountered in my research or have been able to find in the catalogues of United States Library of Congress, the British Library, and the Harvard University Library (searching titles containing "genetics" and either "laboratory," "manual," or "experiments") are: E. B. Babcock and J. L. Collins, *Genetics Laboratory Manual* (New York: McGraw-Hill, 1918); T. H. Morgan, *Laboratory Directions for an Elementary Course in Genetics* (New York: H. Holt, 1923); W. E. Castle, *Outline for a Laboratory Course in Genetics: Recommended for Use in Connection with the Text-Book Genetics and Eugenics* (Cambridge, MA: Harvard University Press,

include suggestions for laboratory work.[114] The type of hands-on experience that Castle had seen as necessary no longer provided the first step in the disciplining of the student's mind. Both authors and reviewers agreed that problem-solving exercises were the best way to "initiate aspiring students"—that they could convey knowledge that could not be explicitly articulated.[115] In the preface to the widely-used *The Principles of Genetics*, for example, Snyder iterated and reiterated that problem-solving provided the student with "much material" that was not in "the text itself."[116] Stern likewise agreed that "numerous problems…include additional information" in a review of Altenburg's *Genetics*.[117] The nature of this additional knowledge conveyed by problem-solving was identified in W. R. Singleton's *Elementary Genetics*: "The text leads the student directly into the *geneticist's approach*, working with clues and solving problems."[118] The implicit historical recreation of the laboratory in the problems was highlighted by Srb and Owen, who saw their problem-solving exercises as an experimental environment: "We have been particularly concerned to confront the student with actual experimental situations for his interpretation."[119]

To briefly summarize, my argument in this section is that the use of problem-solving exercises in genetics textbooks was the use of a form of history that performed a key function in the disciplinary reproduction of genetics. These exercises were not mere

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1924); and C. C. Hurst, *Experiments in Genetics* (Cambridge, England: University Press, 1925). After 1925, the only manuals listed for this period were written by Castle and were designed for more advanced courses: W. E. Castle, *The Genetics of Domestic Rabbits: A Manual for Students of Mammalian Genetics, and an Aid to Rabbit Breeders and Fur Farmers* (Cambridge, MA: Harvard University Press, 1930); and W. E. Castle, *Manual for a Laboratory Course in Genetics, to Accompany the Textbook Mammalian Genetics* (Cambridge, MA: Harvard University Press, 1940). Note however that in the 1960s-1970s, several manuals were again published, but these were most often for more advanced molecular genetics experiments: M. W. Strickberger, *Experiments in Genetics with Drosophila* (New York: Wiley, 1962); R. C. Clowes and W. Hayes, *Experiments in Microbial Genetics* (New York: Wiley, 1968); G. A. Hudock, *Experiments in Modern Genetics* (New York: John Wiley & Sons, 1967); D. P. Snustad and D. S. Dean, *Genetics Experiments with Bacterial Viruses* (San Francisco: W. H. Freeman, 1971); and J. H. Miller, *Experiments in Molecular Genetics* (Cold Spring Harbor, NY: Cold Spring Harbor Laboratory, 1972).

[114] Of all the textbooks in this period that I have surveyed, only two contained suggestions for laboratory work: "Laboratory Methods for Class Work on Drosophila" in Sturtevant and Beadle, *An Introduction to Genetics*, Appendix; and "Laboratory Exercises in Genetics" in Colin, *Elements of Genetics*, 3rd ed., 463-476.

[115] Walter, *Genetics*, 4th ed., 338.

[116] Snyder, *The Principles of Heredity*, 2nd ed., viii. See also Snyder and David, *The Principles of Heredity*, 5th ed., vii: "It should be re-emphasized that the problems provide valuable source of factual and thought-provoking material *supplementary* to the discussion in the text" (emphasis added).

[117] Stern, Review of *Genetics*, by E. Altenburg: 515.

[118] Singleton, *Elementary Genetics*, dust jacket.

[119] Srb and Owen, *General Genetics*, 1st ed., vi.

tools for testing and reinforcing the student's understanding of the material, nor were they created as a way to prepare the student for the types of standardized exams that were becoming more common during this period. Rather, they were introduced in the 1920s as a complement to the case histories, which were designed to allow students to experience the development of key discoveries; Sinnott and Dunn argued that problem-solving, more than laboratory work, allowed students to develop a true understanding and grasp of the principles of genetics and the geneticists' approach. This solution to the problem of how to teach genetics was at first met with skepticism and criticism, but was gradually adopted for the reasons first articulated by Sinnott and Dunn, as geneticists came to agree that these pen and paper exercises were the best way for novice students to use, practice, and interact with the principles. Thus the problems not only served the crucial functions that Kuhn identified in his analysis of exemplars, but also allowed for an important development in the reproductive mechanism of the discipline of genetics: as part of the virtual historical environments created in and by textbooks, problem-solving exercises allowed geneticists to begin socializing and enculturating their students without the physical infrastructure of a laboratory.

A Comparative Survey:
History, problem-solving, and laboratories in physics and the life sciences

To gain a sense of whether the use of virtual historical environments in introductory genetics textbooks was part of a broader trend in the teaching of science in the 1920s-1950s, I surveyed comparable textbooks from a few other fields. In the life sciences, I found very few similarities on the main points of my analysis. Botany textbooks, for example, rarely had the manifest uses of history that were characteristic of most genetics textbooks of the time: while there were a few botany textbooks that included historical details about the development of the science,[120] and at least one "unconventional" textbook that was organized along historical lines,[121] the vast majority did not present the

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[120] See, e.g., E. W. Sinnott, *Botany: Principles and Problems*, 1st, 2nd, and 3rd eds. (London: McGraw Hill, 1923, 1929, 1935); and J. W. Mavor, *General Biology* (New York: Macmillan, 1936).

[121] See, e.g., A. W. Haupt, *An Introduction to Botany*, 1st ed. (London: McGraw-Hill, 1938), viii: "The organization…represents a departure from the conventional plan followed in most textbooks of general botany," in that it "follows the historical development of botany."

material in a historical manner or even contain historical accounts of the science.[122] Instead, they were generally organized according to conceptual or analytic schemes.[123] One reason for this was that botanists had very different conceptions of the value of historical presentation. For example: A. J. Eames and L. H. MacDaniels, authors of popular textbook *An Introduction to Plant Anatomy*, explained that they did not "pretend to present the historical development," but rather provided a brief historical outline in the last chapter, because "the beginning student may best make use of it only when he has acquired an understanding of the subject matter."[124] On this point, the botanists' position was antithetical to that of most geneticists, and the distance between the two grew greater as years passed. By the time Eames and MacDaniels wrote the second edition of their textbook, they believed that even a limited treatment of history was not justified: because history was "not essential to the student gaining a working knowledge of plant anatomy," they removed the historical chapter entirely.[125] Thus, with respect to manifest uses of history, introductory botany textbooks differed significantly from their historically-laden counterparts in genetics.

The teaching of botany also differed from that of genetics in that it did not rely on problem-solving exercises in the 1920s-1950s. One of the few popular textbooks with problem-solving in the 1920s—Sinnott's *Botany: Principles and Problems*—no longer contained them in the third edition of 1935, which kept only the "Questions for Thought

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[122] See, e.g., A. G. Tansley, *Elements of Plant Biology*, 1st and 2nd eds. (London: George Allen & Unwin, 1922, 1935); G. M. Smith et al., *A Textbook of General Botany*, 3rd ed. (New York: Macmillan, 1935); W. W. Robbins and J. Isenbarger, *Practical Problems in Botany* (New York: John Wiley and Sons, 1936); F. O. Bower, *Botany of the Living Plant*, 3rd ed. (London: Macmillan, 1939); E. N. Transeau, H. C. Sampson, and L. H. Tiffany, *Textbook of Botany* (New York: Harper and Brothers, 1940); R. D. Gibbs, *Botany: An Evolutionary Approach* (Philadelphia: Blakiston, 1950); W. W. Robbins et. al, *Botany: An Introduction to Plant Science*, 1st and 2nd eds. (New York: John Wiley and Sons, 1950, 1957); and P. B. Weisz, *The Science of Biology* (New York: McGraw-Hill, 1959).

[123] Many authors stated that they used the order that they found most satisfactory for laboratory work: see, e.g., A. J. Eames and L. H. MacDaniels, *An Introduction to Plant Anatomy*, 1st ed. (New York: McGraw-Hill, 1925), preface; and Mavor, *General Biology*, v. At least one textbook progressed from the "organizational" to "operational" aspects of life—from "description" to "analysis" (see Weisz, *The Science of Biology*, preface); another attempted to "start with the simple and to work up to the complex…to follow Nature's order" (see Gibbs, *Botany: An Evolutionary Approach*, vii); and another proceeded "from the known to the unknown" (see Bower, *Botany of the Living Plant*, vii).

[124] Eames and MacDaniels, *An Introduction to Plant Anatomy*, 1st ed., preface.

[125] A. J. Eames and L. H. MacDaniels, *An Introduction to Plant Anatomy*, 2nd ed. (New York: McGraw-Hill, 1947), vii.

and Discussion."[126] And while a few other textbooks also included "questions" at the end of each chapter,[127] they were not as numerous or as valued as the problem-solving sections of genetics textbooks, and the vast majority of botany textbooks did not contain any.[128] This was perhaps related to the fact that botany textbooks were usually designed to be used in conjunction with actual laboratory experiments. Often, textbook authors wrote or collaborated on the production of laboratory manuals to accompany their textbooks.[129] When they did not, they generally included suggestions for laboratory work, either at the end of each chapter or in an appendix.[130] And the few authors who did not write lab manuals or include lab exercises in their textbooks usually stated that they expected the teacher to use the book alongside laboratory work.[131] To appreciate the difference between botany and genetics in this respect, it is important to note that laboratory work was not just seen as being an important part of a botany course. It was, rather, considered to be foundational, as was explained by the authors of *Plant Biology*: "It is assumed that the use of most of the chapters will be *preceded* by laboratory exercises adequate to give the student a feeling of the reality of the subject matter with which he is dealing and to foster the spirit of inquiry and self instruction."[132] This sentiment was shared by Eames and MacDaniels, who explained: "Training which results

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[126] For an explanation of the function of the questions, see E. W. Sinnott, *Botany: Principles and Problems*, 3rd ed. (London: McGraw-Hill, 1935), x. See also Transeau, Sampson, and Tiffany, *Textbook of Botany*, viii.

[127] See, e.g., Mavor, *General Biology*; Robbins and Isenbarger, *Practical Problems in Botany*; Gibbs, *Botany: An Evolutionary Approach*; and Weisz, *The Science of Biology*.

[128] See, e.g., Tansley, *Elements of Plant Biology*, 1st and 2nd eds.; Eames and MacDaniels, *An Introduction to Plant Anatomy*, 1st and 2nd eds.; Smith et al., *A Textbook of General Botany*; Haupt, *An Introduction to Botany*; Bower, *Botany of the Living Plant*; Transeau, Sampson, and Tiffany, *Textbook of Botany*; Weatherwax, *Plant Biology*; and Robbins et al., *Botany: An Introduction to Plant Science*, 1st and 2nd eds.

[129] See, e.g., L. Bonar, R. M. Holman, and L. Roush, *A Laboratory Guide for a Course in General Botany*, 1st ed. (New York: J. Wiley & Sons, 1925); A. T. Evans et al., *A Laboratory Manual for First Course in Botany* (Boston: Ginn and Company, 1928); C. S. Gager, *A Laboratory Guide for General Botany* (Philadelphia: Blakiston, 1916); H. C. Heath, *A Laboratory Manual of Elementary College Botany, Revised for Use in a Three-Hour Course with Transeau's General Botany* (Ann Arbor: Edwards Brothers, 1932); J. W. Mavor and L. B. Clark, *A Laboratory Manual in General Biology* (New York: Macmillan, 1936); W. J. Robbins and H. W. Rickett, *Laboratory Instructions for General Botany* (New York: D. Van Nostrand, 1930); E. W. Sinnott, *Laboratory Manual for Elementary Botany*, 1st ed. (New York: McGraw-Hill, 1927); W. N. Stewart and H. J. Fuller, *Laboratory Manual for General Botany*, Rev. ed. (New York: H. Holt, 1956); and P. B. Weisz and M. S. Fuller, *Laboratory Manual in the Science of Botany* (New York: McGraw-Hill, 1963).

[130] See, e.g., Tansley, *Elements of Plant Biology*, 1st and 2nd eds.; and W. J. Dakin, *The Elements of General Zoology* (London: Oxford University Press, 1927).

[131] See, e.g., Haupt, *An Introduction to Botany*, viii; Transeau, Sampson, and Tiffany, *Textbook of Botany*, viii; and Weatherwax, *Plant Biology*, iv.

[132] Weatherwax, *Plant Biology*, iv, (emphasis added).

in independence…is, of course, secured only by laboratory practice. On such practice the authors believe emphasis should be placed, and not on lectures, text study, nor, in the beginning, on reading."[133] It was lived experience that provided the foundation for knowledge of botany, according to the authors of *Textbook of Botany*: "The textbook has been written primarily to supplement what is observed and discussed."[134] It was not only in botany that introductory textbooks and the laboratory were interconnected. Many of the popular zoology and general biology textbooks were also written with accompanying laboratory guides.[135] For example: J. W. Mavor's "very widely used" *General Biology* and *Laboratory Exercises in General Biology* provided the teacher with a curriculum that aligned lab work with chapters in the textbook, as did the "splendid" *Principles of Biology* and its companion volume *Laboratory Studies in Biology.*[136] Occasionally, authors tried to synthesize a textbook with a laboratory guide, as was the case in the well-received *Guide to Zoological Experience* and *Practical Problems in Botany.*[137] According to one review: "Laboratory directions are organically interwoven into the *Guide*, and it would we difficult to decide where 'laboratory manual' ends and 'textbook' begins, if one wished to make such a distinction."[138] In addition, laboratory manuals for

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[133] Eames and MacDaniels, *An Introduction to Plant Anatomy*, 1st ed., preface. See also Eames and MacDaniels, *An Introduction to Plant Anatomy*, 2nd ed., ix.

[134] Transeau, Sampson, and Tiffany, *Textbook of Botany*, viii. See also E. N. Transeau, *General Botany, an Introductory Course for Colleges and Advanced Classes in Secondary Schools* (Yonkers-on-Hudson, NY: World Book Company, 1923), preface.

[135] See, e.g., W. C. Curtis and M. J. Guthrie, *Laboratory Directions in General Zoology*, 1st ed. (New York: Wiley, 1925); N. Fasten, *General Zoology Laboratory Outlines* (Corvallis, OR: OSC Cooperative Association, 1941); F. G. Hall and A. S. Pearse, *Laboratory Manual for General Zoology (Zoology 1)* (Durham, NC: Duke University Press, 1929); R. W. Hegner, *Directions for Laboratory and Field Work in Zoology, for Use in Connection with Practical Zoology* (New York: Macmillan, 1930); H. H. Newman, *Laboratory Guide and Review Manual for General Zoology* (New York: Macmillan, 1929); G. E. Potter, *Laboratory Manual for Introductory Zoology (One-Semester Course)* (St. Louis: C. V. Mosby, 1941); K. M. Roehl and H. H. Newman, *A Laboratory Manual for General Zoology* (New York: Macmillan, 1936); K. A. Stiles, *Laboratory Explorations in General Zoology* (New York: Macmillan, 1943); T. I. Storer, *Laboratory Manual for General Zoology*, 1st ed. (New York: McGraw-Hill, 1944); and H. J. Van Cleave, H. R. Linville, and H. A. Kelly, *Biological Principles in General Zoology: A Laboratory Manual* (New York: Ginn and Company, 1930).

[136] R. B. Gordon, Review of *Principles of Biology*, by W. G. Whaley et al., *The Scientific Monthly* 79 (1954): 253; and B. Glass, Review of *A Brief Biology*, by J. W. Mavor, and *A Laboratory Manual for a Brief Course in Biology*, by J. W. Mavor, *Quarterly Review of Biology* 24 (1949): 345.

[137] See, e.g., Robbins and Isenbarger, *Practical Problems in Botany*, vi, where the authors explain that the textbook was written as "a series of problems and subproblems" in which the student worked through the 144 lab exercises that were interwoven into the chapters of the textbook.

[138] R. Gillette, Review of *Guide to Zoological Experiences*, by J. L. Metcalf and C. W. Creaser, *Quarterly Review of Biology* 26 (1951): 74. See also the review of *Practical Problems in Botany*: E. B. Matzke and S. F. Trelease, "Recent Botanical Books," *Science* 85 (1937): 19.

zoology were frequently published independently of any particular textbook.[139] Thus, it seems that the sciences of botany, zoology, and general biology had few of the features of genetics that I have identified and discussed in this chapter.

A brief study of physics textbook likewise finds little in common with the virtual historical environments of genetics textbooks. Very few authors of physics textbooks attempted to employ a historical approach to teaching, although some physicists interested in pedagogy had suggested that it might prove to be a valuable approach.[140] And while physics textbooks generally included numerous problem-solving exercises,[141] and the use of problems was so pervasive that it attracted the interests of physicists interested in pedagogy,[142] the form and function of these problems were fundamentally different from those in genetics textbooks. Whereas geneticists saw quantification as an essential feature of their problem-solving exercises, physicists were interested in minimizing the quantitative aspect of theirs. For example: Thomas Cope, professor at the University of Pennsylvania and active member of *Association of Physics Teachers*, suggested that textbook authors should write problems that involved "rigorous thinking without arithmetic," or "semi-quantitative, physical problems."[143] H. K. Schilling, head of the Physics Department at Penn State, agreed and proposed that physics teachers should use "stripped" problems—"problems which, divested of nearly all mathematical, stand bare physically." He argued that this type of problem allowed students to practice

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[139] See, e.g., H. L. Bruner, *Laboratory Directions in College Zoology*, 1st ed. (New York: Macmillan, 1928); G. A. Drew, *A Laboratory Manual of Invertebrate Zoology*, 3rd ed. (Philadelphia: W. B. Saunders, 1920); L. H. Hyman, *A Laboratory Manual for Elementary Zoology*, 2nd ed. (Chicago: University of Chicago Press, 1926); H. D. Reed and B. P. Young, *Laboratory Studies in Zoology*, 1st ed. (New York: McGraw-Hill, 1930); C. P. Sigerfoos, *Laboratory Directions in General Zoology*, 9th ed. (Minneapolis: Perine, 1921); and H. V. P. Wilson and R. E. Coker, *Laboratory Guide in General Zoology* (Chapel Hill: University of North Carolina Press, 1925).

[140] J. C. Hubbard, "Trends in Physics Teaching. Some Recent Texts," *Science* 95 (1942): esp. 411.

[141] See, e.g., J. A. Eldridge, *College Physics* (New York: John Wiley and Sons, 1940); W. Winiger, *Fundamentals of College Physics* (New York: American Book Company, 1940); and N. H. Black, *An Introductory Course in College Physics* (New York: Macmillan, 1941). See also K. Lark-Horovitz, "Physics Teaching and the Text-Books," *Science* 88 (1938); and Hubbard, "Trends in Physics Teaching. Some Recent Texts."

[142] See, e.g., C. J. Lapp, "The Effectiveness of Mathematical Versus Physical Solutions in Problem Solving in College Physics," *American Journal of Physics* 8 (1940): 241: "A perusal of the catalog descriptions of the courses in general physics offered by institutions of college grade gives direct, strong evidence that problem solving is held to be one of the potent learning devices used in instruction." Lapp later carried out a two-year study to test the validity of what he saw as the long-held "but unsubstantiated" belief in the value of problem-solving in physics teaching; he concluded that problem-solving was in fact a valuable teaching tool. See C. J. Lapp, "The Effectiveness of Problem Solving in Producing Achievement in College Physics," *American Journal of Physics* 9 (1941).

[143] T. D. Cope, "Problems in Physics Textbooks," *American Journal of Physics* 5 (1937): 89.

the “thought processes involved in the application of fundamental principles and concepts to particular physical situations.”[144] The use of “stripped” problems was also advocated by C. P. Lapp, who published research on their value in *American Journal of Physics.*[145] Thus, geneticists and physicists had very different ideas about the ideal form of problem-solving exercises. In addition, and perhaps more importantly, these exercises were meant to serve very different functions. Whereas geneticists thought that problem-solving could replace laboratory work in some introductory courses, physicists continued to see actual experiments as essential.[146] Textbooks were expected to prepare students for “the usual laboratory experiments of the general physics course.”[147] Problem-solving was merely meant to complement experiments: “Each chapter concludes with a list of suggested laboratory experiments and a goodly assortment of questions and problems.”[148] On some accounts, the discipline’s reproduction was essentially rooted in the laboratory. For example: in a review of two popular manuals—*A Laboratory Manual of Experiments in Physics*, by Ingersoll and Martin, and *General Physics for the Laboratory*, by Taylor, Watson and Howe—it was suggested that “an effective course in general physics” could “be offered with a manual like either of these, perhaps reshaped a little, as the core textbook with a shelf of the usual texts in general physics at hand for collateral reading.”[149] Thus some physicists—much like some botanists and zoologists, and unlike geneticists—thought that their textbooks could be replaced by lab manuals. The laboratory was seen as a central place of teaching, as a 1942 article on trends in physics teaching concluded: “there has never been a time when the importance of laboratory points of view…have been more clearly recognized.”[150]

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[144] H. K. Schilling, "'Stripped Problems' Tests," *American Journal of Physics* 9 (1941): 124.

[145] Lapp, "The Effectiveness of Mathematical Versus Physical Solutions in Problem Solving in College Physics."

[146] G. F. Hull, "Text-Books in Physics," *Science* 89 (1939): 154: “The teaching of physics depends on three factors, the teacher, the laboratory equipment, the textbook.”

[147] Hubbard, "Trends in Physics Teaching. Some Recent Texts," 411.

[148] Ibid.

[149] T. D. Cope, "Text-Books of Physics," *Science* 97 (1943): 556. See also, e.g., the “preface” and “announcement” in L. L. Loeb, *A Laboratory Manual of Electricity and Magnetism* (Stanford: Stanford University Press, 1941).

[150] Hubbard, "Trends in Physics Teaching. Some Recent Texts," 412. This claim was not, however, entirely uncontested. Several studies were conducted in the 1950s to compare learning achieved through lecture-only versus lecture and laboratory courses; they are discussed in V. W. Miles and W. C. V. Deventer, "The Teaching of Science at the College and University Level," *Review of Educational Research* 31 (1961).

While brief, my survey suggests that genetics in the 1920s-1950s was unlike botany and zoology, from which it had emerged, and unlike physics, which geneticists emulated in differentiating their field from the life sciences. Although exemplars certainly played an important role in the teaching of these other sciences, they were not developed into virtual historical environments or used as replacements for the laboratory. It thus seems that geneticists might have developed a unique form of case-based teaching and disciplinary reproduction in the 1920s, but to draw such a conclusion would require a more extensive analysis than is feasible or suitable in this chapter.

Conclusion

Although textbooks are designed as educational tools, the specific goals they serve cannot be uniformly characterized. Textbooks can be written to convey a body of theories, facts, and ideas; or alternatively, to inculcate a way of seeing, an approach to a type of problem, or a grasp of the ways a scientific law is used. In the 1920s, the teaching of genetics was itself seen as an emerging science, and authors such as Sinnott and Dunn actively tried to develop a solution to what Castle described as the "problem of how successfully to teach genetics."[151] The problem was not just methodological, but also arose from uncertainty about the purpose of teaching genetics, according to Stern.[152]

In this chapter, I have argued that the 1920s marked the beginning of a style of teaching that later became standardized whereby two types of virtual historical environments were written into genetics textbooks—in presentations of past experiments, and in problem-solving exercises. Together, these features of the textbook constituted the exemplars with which the principles of genetics were taught. Although the historicity of the past experiments was always explicit while that of the problems was not, these two parts of the textbook were designed together to allow the student to practice the path by which the principles had been discovered, tested, applied, and modified. They placed the student in an environment wherein past events could be reenacted. In this way,

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[151] Castle, "Some New Books on Genetics," 567. See also Sinnott and Dunn, *Principles of Genetics*, 1st ed., ix.

[152] Stern, Review of *Genetics*, by E. Altenburg: 514: "The purpose of a college course in genetics is less well defined than that of many other courses." According to Stern, a course in genetics was not a "prerequisite for advanced work in fields other than genetics itself," whereas anatomy and physical chemistry were professionally useful for physicians and chemists.

geneticists taught their students how to think in cases—how to find and follow rules, and thereby *how to go on* when confronting new situations. Internalizing knowledge of a historical exemplar, the student not only grasped a principle of genetics, but also the principles of geneticists.

By writing virtual historical environments into their textbooks, geneticists removed the first steps of their discipline's reproduction from the laboratory. It was with pen and paper that novice students began to reenact the historical discovery of the principles, perform experiments with them, and gain a grasp of the shared exemplars that unified and defined the scientific community. The centrality of history in this form of disciplinary reproduction calls into question the common view that history-writing does not perform scientific functions. In the following chapter, I will continue to problematize this notion by exploring a way in which history was even further embedded into the structure of genetics textbooks. But I will first conclude my study of the Mendelian exemplar where it began—with the professor of law who created the first case-studies at Harvard, Christopher C. Langdell.

During the second quarter of the twentieth century, geneticists came to see their discipline much as Langdell had once seen his: "the best, if not the only way of mastering the doctrine effectively is by studying the cases in which it is embodied."[153] With this pedagogic shift came a significant change in the places where these doctrines were learned. When Langdell developed the case-study method of teaching in the late nineteenth century, he had seen the library and its books as "the proper workshop" for students of law: "it is to us all that the laboratories of the university are to the chemists and physicists, the museums of natural history to the zoologists, the botanical garden to the botanists."[154] By the 1940s, however, the workshops of the legal and natural sciences were connected by more than analogy. While first-year physicists still trained in laboratories and botanists in gardens, the students of genetics often joined the students of law in the library. Textbooks had replaced fly rooms and crop fields. It was in their pages that genetics was being practiced.

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[153] Langdell, *A Selection of Cases on the Law of Contracts*, vii, as quoted in Forrester, "If P, Then What? Thinking in Cases," 15.

[154] C. C. Langdell, "The Harvard Law School [Professor Langdell's Speech at the 'Quarter-Millennial' Celebration of Harvard University on the 5th of November, 1886]," *Law Quarterly Review* 3 (1887): 124.

CHAPTER 3

Historia as *Logos*:
The Development and Presentation of a Conceptual Order

The organization of principles and theories was relatively standardized in the genetics textbooks of the 1940s-1960s. In the vast majority, there was no variation in the chapters on "Mendelian" and "chromosomal" genetics: segregation, independent assortment, modified Mendelian ratios, sex-linked inheritance, linkage, cytology and crossing-over, chromosome maps, chromosomal aberrations, and mutation were almost always presented in this order. There was only slight variation in the organization of much of the remaining material. Cellular reproduction and the formation of gametes were discussed in one of two places— at the opening of the textbook, or more often after Mendel's principles and before chromosomal genetics. Sex-determination and quantitative inheritance were explained at some point after the discussion of modified Mendelian ratios. And topics of research that were seen as under development (e.g., population genetics and evolution, genetic systems, developmental genetics, and cytoplasmic inheritance) were discussed in the final third of the text.[1]

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[1] With a few exceptions—such as A. F. Shull, *Heredity*, 1st, 2nd, 3rd, and 4th eds. (New York: McGraw-Hill, 1926, 1931, 1938, 1948); A. H. Sturtevant and G. W. Beadle, *An Introduction to Genetics* (London: Saunders, 1939); and C. D. Darlington and K. Mather; *The Elements of Genetics* (London: Allen & Unwin; 1949)—my statements in this paragraph apply to almost all of the widely-circulated textbooks written in this period, including: E. W. Sinnott and L. C. Dunn; *Principles of Genetics*; 1st; 2nd; and 3rd eds. (New York: McGraw-Hill; 1925; 1932; 1939); E. W. Sinnott, L. C. Dunn, and Th. Dobzhansky, *Principles of Genetics*, 4th and 5th eds. (New York: McGraw-Hill, 1950, 1958); L. H. Snyder, *The Principles of Heredity*, 1st, 2nd, 3rd, and 4th eds. (Boston: D. C. Heath, 1935, 1940, 1946, 1951); L. H. Snyder and P. R. David, *The Principles of Heredity*, 5th ed. (Boston: D. C. Heath, 1957); E. Altenburg, *Genetics*, 1st and Rev. eds. (New York: H. Holt, 1945, 1957); A. M. Winchester, *Genetics: A Survey of the Principles of Heredity*, 1st, 2nd, and 3rd eds. (Boston: Houghton Mifflin, 1951, 1958, 1961); A. M. Srb and R. D. Owen, *General Genetics*, 1st ed. (San Francisco: W. H. Freeman, 1952); A. M. Srb, R. D. Owen, and R. S. Edgar, *General Genetics*, 2nd ed. (San Francisco: W. H. Freeman, 1965); E. J. Gardner, *Principles of Genetics*, 1st and 2nd eds. (New York: John Wiley & Sons, 1960, 1964); C. M. M. Begg, *An Introduction to Genetics*, (London: The English Universities Press, 1959); and R. C. King, *Genetics*, 1st and 2nd eds. (New York: Oxford University Press, 1962, 1965).

Although it is tempting to treat standardized presentations of a science as objects that tell us about matters of consensus in the scientific community, textbooks are not artifacts, but rather tools and sites of production. It is in them that disparate scientific practices and theories are first brought together and shown to be a unified discipline. Thus the authors of textbooks do not passively document a practice that exists elsewhere, but rather create the coherence of the science when they present and organize it.

Behind the standardized organization of genetics textbooks in the mid-twentieth century, a construction of history and a form of history-writing were at work. Their use will be the subject of the three sections of this chapter. To begin, I will explore the heterogeneity in the organization of first generation textbooks, the reasoning that subsequently led to the standardization of the "historical approach," and the outcome of attempts to develop an alternative "logical" order. I will then turn to an analysis of the form and function of the literary devices that were used to write a textbook "historically." In the third section, I will explore whether the standardization of organization affected authors' abilities to recreate the science over time. Together, these three sections will show how a sense of history was intricately involved in the formation, presentation, and reconstructions of the conceptual order of genetics.[2]

Historical vs. Logical: The Standardization of Textbook Order

In the 1910s, the organization of subject matter in genetics textbooks was generally discussed in detail, but rarely critically evaluated, in book reviews. An article in *Science*, for example, described the order in Thomson's *Heredity*: "The book starts with definitions of heredity and inheritance, and proceeds to discuss the physical basis of inheritance, the germ cells, their maturation and fertilization." In the paragraphs that followed, the review gave a thorough "summary of the contents of the book."[3] Reviews of Bateson's *Mendel's Principles of Heredity* likewise recounted the progression of subjects in detail—from pre-Mendelian theory, to "the principles of Mendelian theory,"

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[2] For a more theorized account of how the nature of scientific facts and discoveries are constituted through their textual organization, see S. Woolgar, "Discovery: Logic and Sequence in a Scientific Text," in *The Social Process of Scientific Investigation*, ed. K. D. Knorr, R. Krohn, and R. Whitley (London: D. Reidel, 1980), esp. 256-262.

[3] J. P. McMurrich, Review of *Heredity*, by J. A. Thomson, *Science* 28 (1908): 211.

to "exceptions, real or apparent, to Mendel's law."[4] Bateson's historical ordering of the subject matter was described as though it were the natural way to present a science that was only a few years old: "The present work…gives a comprehensive account *of the development* of Mendelian principles of heredity down to the present year."[5] Although these and other reviews devoted much space to discussing the organization of subject matter, they did not address the merits or weaknesses of the various forms of order. No approach was heralded as being more logical, or as allowing for better explanation. However, reviewers did critically discuss authors' interpretations of hereditary phenomena and generally identified textbooks as representing different schools of thought.[6]

As I mentioned in Chapter 1, reviews of *Heredity* often commented on Thomson's commitment to Weismannism. North American zoologist J. Playfair McMurrich, for example, wrote: "He avowedly sails…under the flag of Weismannism and nails this flag firmly to the mast."[7] In much of this review for *Science*, McMurrich elaborated on the consequences of Thomson's position, arguing that his insistence on material units "rendered him somewhat intolerant of epigenetic possibilities." McMurrich saw the phenomena of heredity as open to numerous possible interpretations, each with its own value and merit: "it must be admitted that the concept of determinants or specific material units furnishes a convenient 'notation' for the discussion of certain phenomena of inheritance; it is not, however, the only concept possible."[8] Distinguishing between the phenomena and the concepts employed to understand it, McMurrich accepted Thomson's Weismannism on instrumental grounds, stating that it

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[4] W. E. Castle, Review of *Mendel's Principles of Heredity*, by W. Bateson, *Science* 30 (1909): 482-483. See also R. R. Gates, "Heredity," Review of *Heredity*, by J. A. Thomson, *Botanical Gazette* 47 (1909): 61.

[5] Castle, Review of *Mendel's Principles of Heredity*, by W. Bateson: 482 (emphasis added).

[6] E.g., J. P. McMurrich, Review of *the Laws of Heredity*, by G. A. Reid, *Science* 32 (1910): 761-762, discussed Reid's commitment to a deductive model of inquiry; E. M. East, "Genetics," Review of *Elemente der Exakten Erblichkeitslehre*, by W. Johannsen, *Botanical Gazette* 57 (1914): 241, suggested that many of Johannsen's views were Weismannian; W. E. Castle, Review of *Mendel's Principles of Heredity*, by W. Bateson, *Science* 40 (1914): 246, and R. R. Gates, "Mendelism," Review of *Mendel's Principles of Heredity*, by W. Bateson, and *Mendelism*, by R. C. Punnett, *Botanical Gazette* 48 (1909): 61-62, discussed Bateson's Mendelian orientation; and McMurrich, Review of *Heredity*, by J. A. Thomson: 210-211, and Gates, "Heredity," 154, highlighted that Thomson was a famous advocate of Weismannism.

[7] McMurrich, Review of *Heredity*, by J. A. Thomson: 210-211. See also, e.g., Gates, "Heredity," 154: "The writer…is perhaps most widely recognized as the translator of Weismann's works and the exponent of Weismannism."

[8] McMurrich, Review of *Heredity*, by J. A. Thomson: 211.

was just one of numerous useful ways of presenting and studying genetics. He commended Thomson for not being so dogmatic as to not see the validity of alternatives: “he admits that determinants are ‘scientific fictions,’ that they are elements of a ‘symbolic notation’ to be discarded so soon as it is shown to be inconsistent with demonstrable facts.”[9]

Like Thomson’s *Heredity*, Bateson’s *Mendel’s Principles* was understood as a textbook representing a specific school of thought—in his case, Mendelism. Reginald Gates, for example, wrote: “The remarks on every page…leave no doubt as to the interpretation placed upon the phenomena described.”[10] Mendelism was seen and critically discussed as being just one of many possible interpretations of heredity, relying on the selective consideration of facts: “It is a curious blindness to other facts of heredity which leads the author to the opinion that Mendelism probably represents the only type of inheritance which exists.”[11] Emphasizing the fact that Bateson’s approach to the organism was based on “an unproven hypothesis,” Gates acknowledged the instrumental value of the theory, but suggested that it was likely to be incorrect: “The hypothesis has certainly proved useful, even though another explanation of the phenomena of segregation may ultimately be found necessary.”[12] According to Gates, Bateson misunderstood other types of approaches to the issue: “The fact that Galton’s law was designed for populations rather than for individuals seems to have been overlooked.”[13] Gates did not claim that Bateson did not understand Galton’s law, but rather that he did not understand the scope and purpose of the law—the type of law that it was.

In identifying and criticizing Thomson’s and Bateson’s schools of thought, textbook reviews of the 1910s implicitly made sense of the logic underlying the organization of subject matter in *Heredity* and *Mendel’s Principles*. Bateson’s commitment to a Mendelian interpretation explained his decisions to begin with Mendel’s experiments and discoveries, and to use Mendel’s laws as a foundation and organizing logic of the textbook. Likewise, Thomson’s commitment to understanding

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[9] Ibid.

[10] Gates, "Mendelism," 62. See also, e.g., “Bateson’s book…may be regarded as the authoritative interpretation of Mendelism,” in Castle, Review of *Mendel's Principles of Heredity*, by W. Bateson: 246.

[11] Gates, "Mendelism," 61-62.

[12] Ibid.: 62.

[13] Ibid.

inheritance in terms of determinants made sense of his decision to begin with a discussion of the physical basis of inheritance, the germ cells, and their reproduction. In the following decades, overt criticism and discussion of authors' different "interpretations" became rare in textbook reviews. However, I want to suggest that this was not because disagreements over interpretation had been resolved, but rather because they had become embedded in discussions of order and organization, as Bateson's and Thomson's approaches became prototypes of two competing approaches to teaching.

The mode of organization in *Mendel's Principles*—in which the genetics was presented as building on the foundation of Mendel's laws—became known as the "historical approach" and was seen as part of the standard method of teaching by the 1930s.[14] As stated in a review of *An Introduction to Genetics* (1939), "genetics is customarily approached through an account of its history."[15] And in a review of Snyder's widely-used *The Principles of Heredity* (1935):

> The general plan of the book does not depart greatly from the conventional. After presenting simple (monohybrid) Mendelian inheritance, there is a chapter on the physical background (cytology), after which dihybrid and modified ratios, sex-linkage, lethals, multiple allelomorphs, etc., are taken up in much their usual order.[16]

Reviewers in *Science* and *The American Naturalist* often identified and categorized textbooks in relation to this traditional "historical" organization. For example: in a review of H. S. Jennings' *Genetics*, "the general plan of the book departs from the conventional," and in a review of Adrian Srb and Edgar Owen's *General Genetics*, "the arrangement of chapters does not deviate from that usually found in genetics textbooks."[17] This historical organization had been adopted in the 1920s because it was seen as the natural order that placed the fundamental facts first. For example, when H. E.

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[14] Note that in physics, textbook authors were just beginning to experiment with the historical approach in the 1940s: J. C. Hubbard, "Trends in Physics Teaching. Some Recent Texts," *Science* 95 (1942): 411.

[15] T. Just, Review of *An Introduction to Genetics*, by A. H. Sturtevant and G. W. Beadle, *The American Midland Naturalist* 23 (1940): 752.

[16] L. J. Cole, "Genetics Texts," Review of *The Principles of Heredity,* by L. H. Snyder and *Principles of Genetics and Eugenics,* by N. Fasten, *Science* 83 (1936): 373.

[17] E. Caspari, Review of *General Genetics*, by A. M. Srb and R. D. Owen, *Science* 117 (1953): 45; and M. F. Guyer, "Genetics," Review of *Genetics*, by H. S. Jennings, *Science* 83 (1936): 576. See also, e.g., "the traditional approach to the basic principles has been abandoned," in C. N. Herndon, Review of *Medical Genetics*, by L. H. Snyder, *The American Naturalist* 75 (1941): 602.

Walter made revisions to the first edition of *Genetics* and adopted a historical order in the second, a review in *Science* praised the "radical changes," explaining:

> A chapter discussing 'Pure Lines and Selection' formerly preceded Mendelism but now more appropriately follows this subject because pure lines and selection can be interpreted best in terms of Mendelism. The basic facts of cytology were originally treated in the second chapter but now follow the *fundamental* facts of Mendelism.[18]

Here and elsewhere, reviews suggested that the "historical order" corresponded to a more natural conceptual order. It was also widely thought that this form of organization had clear pedagogical strengths. In Charles W. Cotterman's review of the fourth edition of Walter's *Genetics*, which he identified as "among the earliest accounts of the new science to appear in this country," he suggested, "Teachers of genetics and allied subjects should find some useful teaching devices," such as "the historical approach" to teaching.[19] By the 1940s, many authors agreed that knowledge of the basic foundation of the discipline could be best conveyed in historical terms. In the preface to *Textbook of Genetics*, for example, William Hovanitz wrote: "This book maintains the historical approach to genetics…It is believed best that this approach be preserved in an introductory account to the subject."[20] It was also thought that the historical approach made the subject more interesting. As the author of *Elements of Genetics* explained, "The historical approach has been adopted as the one most likely to gain the interest of the reader."[21]

It is important to note that when authors and reviewers spoke about this "historical approach," they did not mean that all of the subjects in the textbook were presented chronologically. Jennings' *Genetics*, for example, did not follow the history exactly, but Gates nevertheless stated: "the author in his treatment follows the history of

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[18] J. A. Detlefsen, Review of *Genetics: An Introduction to the Study of Heredity*, by H. E. Walter, *Science* 56 (1922): 145-146 (emphasis added).

[19] C. W. Cotterman, "Recent Books on Genetics," *The American Naturalist* 75 (1941): 599.

[20] W. Hovanitz, *Textbook of Genetics* (New York: Elsevier Press, 1953), v. This view was also expressed in E. C. Colin, *Elements of Genetics: Mendel's Laws of Heredity with Special Application to Man*, 3rd ed. (New York: McGraw-Hill, 1956), vii: "it is felt that a knowledge of the development of a science is of particular interest and importance to the beginning student." See also Gardner, *Principles of Genetics*, 1st ed., v: "This book…emphasizes basic principles and tells the story of the classical experiments which have laid the foundation for a modern science."

[21] Colin, *Elements of Genetics*, 3rd ed., vii. The idea that historical details made science more interesting for students dates at least to the nineteenth century: W. J. Sherratt, "History of Science in Education," 9-10.

the development of knowledge."[22] When Edith White's preface to *Genetics* explained that the "subject matter of this book is presented from a historical viewpoint," what she meant was that students were first introduced to "Mendelian Genetics" and then to "Drosophila Genetics."[23] And as Dobzhansky noted in a review of *The Science of Genetics*, it was often just the chapters on transmission genetics that were being presented in a roughly historical order: "Classical transmission genetics is outlined in the first two chapters, which generally follow the historical sequence of genetic discoveries (except that the concepts of allelism, homozygosity, and heterozygosity are introduced in a chapter preceding that in which an account of Mendel's experiments is given)."[24] It was a sense of historical order—a construction of history, and not an adherence to chronology—that guided the writing and organization of genetics textbooks. Thus, while the concept of the "standard historical approach" allowed geneticists to easily discuss the organization of a particular textbook, it also effaced differences between textbooks. The use of the term was not only descriptive, but also normative: it reinforced a notion of what the convention was, as well as the sense that there was an established and valued way of ordering the subject matter, influencing the reception of textbooks in both genetics and other life sciences.[25]

An alternative to the historical order was the "logical method," which shared essential features of Thomson's approach in *Heredity*, but was explicitly formulated in the late 1930s as several prominent textbook authors began to question the conventional wisdom of geneticists. A. Franklin Shull was amongst the first of these authors. In the first two editions of his textbook *Heredity*, he discussed gametes and cellular reproduction prior to Mendelian genetics. And in the third edition of 1938 he explained and defended this method, criticizing the historical approach on pedagogical grounds:

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[22] R. R. Gates, "Principles of Genetics," Review of *Genetics*, by H. S. Jennings, *Nature* 137 (1936): 801.

[23] E. G. White, *Genetics*, 2nd ed. (New York: Vantage Press, 1962), i.

[24] Th. Dobzhansky, "Genetics, the Core Science of Biology," Review of *The Science of Genetics*, by C. Auerbach, *Genetic Research*, by A. Muntzing, *Genetics on the Population Level*, by M. Rasmuson, *Human Genetics*, by C. C. Li, and *Cell Heredity*, by R. Sager and F. J Ryan, *Science* 134 (1961): 2091-2092.

[25] See, e.g., R. Gillette, Review of *Guide to Zoological Experiences*, by J. L. Metcalf and C. W. Creaser, *Quarterly Review of Biology* 26 (1951): 75: "one might suggest that it would have been more consistent to present the student with the breeding data from which the Mendelian generalizations are drawn *independently* of the cytological parallels and *later* to demonstrate the correlation between Mendelian breeding factors and chromosomal behaviour. The conclusion that the chromosomes are the material bearers of the hereditary factors could come after rather than before the Mendelian generalizations."

> The traditional method has been to present experimental results, formulate a scheme of gene operations which will logically explain them, and then—long afterward—show that these operations call for genes which are located in the chromosomes. While this has been the order of discovery, few sciences are most effectively mastered by following the sequence of their historical development.[26]

Sturtevant and Beadle agreed with Shull that genetics should not be taught historically, and in the preface to *An Introduction to Genetics* explained their logical approach: "The treatment of the material is not a historical one; the object has been rather to give a natural order."[27] They thought of genetics as a "mathematically formulated subject that is logically complete and self-contained."[28] For this reason, they "attempted to treat the subject…as a logical development in which each step depends on the preceding ones" and directed the students to read the textbook in this way: "The book should be read from the beginning, like a textbook of mathematic or physics, rather than…like a textbook of comparative anatomy or natural history."[29] In reviewing *An Introduction to Genetics*, Theodore Just noted: "Despite its rather recent origin genetics is customarily approached through an account of its history. Contrary to this practice, the authors of this 'Introduction' adopt a different arrangement."[30] Just's review identified the authors' claims about the value of this "logical" presentation, but did not say whether he agreed.[31] Similarly, the reviewer in *Nature* noted that Sturtevant and Beadle thought that their order was more natural, but did not offer an evaluation of the approach, agreeing only that it was new: "The order in which the material is presented is claimed to be a natural one and the reader will find the approach to genetics entirely novel."[32] Geneticists were evidently unsure about the value of this approach. Although many agreed that it was more "logical" to begin a textbook with chapters on the physical basis of heredity, there was uncertainty about whether this had any pedagogical advantage. In a book review of

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[26] Shull, *Heredity*, 3rd ed., vii-viii.

[27] Sturtevant and Beadle, *An Introduction to Genetics*, 11.

[28] Ibid.

[29] Ibid. See also H. S. Jennings, *Genetics* (New York: W. W. Norton, 1935), in which a similar approach was used for the same reasons.

[30] Just, Review of *An Introduction to Genetics*, by A. H. Sturtevant and G. W. Beadle: 752.

[31] Through the 1940s, reviewers were commenting on the use of the logical without evaluating it. See, e.g., W. R. Singleton, Review of *Introduction to Genetics and Cytogenetics*, by H. P. Riley, *Science* 107 (1948): 634: "The approach used by Dr. Riley is the logical one rather than a more or less historical development of genetics. The first part of the book is devoted to the physical basis of heredity."

[32] P. C. Koller, "A Synopsis of Genetics," Review of *An Introduction to Genetics*, by A. H. Sturtevant and G. W. Beadle, *Nature* 144 (1939): 1067.

Jennings's *Genetics*, Michael Guyer, author of the widely used textbook *Animal Biology*, voiced skepticism:

> The general plan of the book departs from the conventional in that the underlying mechanism of heredity—the nature of the germinal constituents—is discussed before the generalities of genetics are reviewed. While this is unquestionably the logical approach, how such a method will work out in actual class usage…is a matter that will doubtless be watched with much interest by teachers of genetics.[33]

The logical approach was, however, well received by some. Reviewers for *Quarterly Review of Biology*, for example, offered praise of Altenburg's *Genetics*: "The general plan and arrangement of material is excellent. The description of the chromosomal behavior in the first chapter lays a foundation for much of what follows."[34]

In the 1930s-1940s, geneticists discussed the logical and historical approaches as though they were merely two different ways of teaching the same science. This disagreement about organization, however, can been seen as the product of a deeper disagreement about the foundation of genetics—about whether the science was based in units of matter, or in scientific laws. With the logical order, knowledge of genes and their behavior in chromosomal reproduction provided the basis for understanding Mendelism; with the historical, Mendel's laws provided the foundation for the chromosome theory and the theory of the gene. On this view, essential features of Bateson's and Thomson's disagreement about the nature of the science were recapitulated in these disagreements about the best approach to teaching. The historical and logical methods were not merely two ways of presenting the same science. They were, rather, ways of conveying two very different conceptual orders. This was acknowledged by textbook authors Srb and Owen, who had originally written *General Genetics* using the logical method, but decided to reorganize the second edition along

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[33] Guyer, "Genetics," 576. Guyer's *Animal Biology* received an excellent review, and its success was noted, in C. E. McClung, "Elementary Biological Texts," Review of *Principles of Animal Biology*, by A. F. Shull, *Animal Biology*, by M. F. Guyer, *General Biology*, by J. W. Mavor, *Foundations of Biology*, by L. L. Woodruff, *Human Biology,* by G. A. Baitsell, *Biology and Human Affairs*, by J. W. Ritchie, *This Living World*, by C. C. Clark and R. H. Hall, *Science* 94 (1941): 392.

[34] R. F. Kimball and E. S. Gersh, "Genetics and Cytology," Review of *Genetics*, by E. Altenburg, *Quarterly Review of Biology* 21 (1946): 79.

historical lines.[35] In the preface to this second edition, they explained: "Our decision to introduce students to genetics by way of the contributions of Mendel was not merely a matter of graceful tribute on the occasion of the centennial of his work. It is a recognition that his pioneering contribution framed the terms of reference of typical genetic analysis and thinking, even as they are carried out today."[36] In adopting the historical approach, textbook authors chose to teach one conceptual order over another; they presented a picture of genetics in which Mendel's principles were a frame, holding the heterogeneous aspects of the science in place.

In the 1910s, the decision to introduce genetics with Mendel's principles was understood as a Mendelian approach to genetics; by the 1930s, it had come to be seen as a historical approach, defined against the logical; and in the 1950s-1960s, it was transformed again, as this organization became ubiquitous. In the textbooks of this period that opened with an explanation of the physical basis of heredity, this explanation served to introduce a series of many chapters that were organized much like those in the historical approach.[37] Thus, the historical approach was at least partly adopted in most textbooks. There were not any new overtly ahistorical textbooks that presented sex-linkage before Mendel's principles, as Sturtevant and Beadle had in writing their "logical" *An Introduction to Genetics*. But the reason for this was not that geneticists ceased valuing a logical approach; rather, there had been a shift in their sense of "the logical." There had been a reappropriation of the language used by Sturtevant and Beadle to justify their ahistorical organization—their claims that genetics was a "mathematically formulated subject that is logically complete" and that it should be presented "as a logical development in which each step depends on the preceding ones." In the 1950s, geneticists agreed with these statements; Edward C. Colin, for example, made almost identical claims in the introduction to his textbook: "Genetics resembles mathematics in that one topic is built upon another in logical order."[38] What it meant to

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[35] See Srb and Owen, *General Genetics*, 1st ed., v, in which the authors had explained that they had chosen to present the material logically, which "meant the abandonment of anything like a systematic historical approach to the subject."

[36] Srb, Owen, and Edgar, *General Genetics*, 2nd ed., vi.

[37] See, e.g., Altenburg, *Genetics*, 1st ed. and Rev. ed.; Winchester, *Genetics*, 1st ed.; Begg, *An Introduction to Genetics*; and King, *Genetics*, 1st ed. and 2nd ed.

[38] Colin, *Elements of Genetics*, 3rd ed., xiv.

present genetics logically, however, had changed; for in Colin's textbook, the science was presented as being based on Mendelism. This approach was no longer seen in opposition to the logical. It was, rather, adopted precisely because it was thought to reveal the logic of the science. As explained by Dunn, "The history of genetics provides an example, perhaps the clearest one among the biological sciences, of how scientific knowledge evolves. The logical connection between its ideas—its inner consistency—is readily apparent."[39] Herskowitz voiced similar thoughts when explaining his use of this approach in his textbook *Genetics*: "The aim is to present genetics as a rational, organized body of knowledge."[40] Reviewers of *Principles of Genetics*, such as Caspari, agreed: "The first ten chapters follow the *logical* sequence of earlier editions by first developing the methods and results of 'formal' genetics and then proceeding to the discussion of the chromosome theory of heredity."[41] Thus, it is my contention that in the 1950s-1960s, the "historical" organization was not only seen as conventional. Rather, the standardized use of history as an organizing logic, across several decades of textbooks, had made this order logical.

A Dialectical Narrative: Making Mendel's Laws the Foundation

Having traced and analyzed the standardization of organization in genetics textbooks, I will now explore how the historical approach was implemented in the text, with a case study of its use in *Principles of Genetics*. I have chosen this "classical textbook of Sinnott and Dunn" because it was widely considered to be one of the best textbooks in circulation.[42] Lewis Tiffany, for example, described it as "one of the two or three outstanding texts in the field of genetics."[43] Seymour Fogel and Irwin Herskowitz agreed, writing in their review in *Science*: "Beyond question, this is the best available

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[39] L. C. Dunn, *A Short History of Genetics: The Development of Some of the Main Lines of Thought, 1864-1939* (New York: McGraw-Hill, 1965), xi.

[40] I. H. Herskowitz, Genetics, 1st ed. (Boston: Little, Brown, and Company, 1962), 5.

[41] E. Caspari, Review of *Principles of Genetics*, by E. W. Sinnott, L. C. Dunn, and Th. Dobzhansky, *Science* 112 (1950): 725 (emphasis added).

[42] Ibid.

[43] L. H. Tiffany, "Edmund Ware Sinnott: President of AAAS, 1948," *Science* 107 (1948): 1.

textbook in the field of general genetics."[44] And in retrospective description, it was characterized as "the classic textbook of classical genetics."[45] Thus, Sewall Wright's 1926 prediction, "It is a book which should come into very extensive use," was right.[46] The widespread use of the textbook was not limited to the United States and Great Britain; *Principles* was, rather, "a handbook with worldwide distribution."[47] Through the publication of several translations in foreign languages and a pirated English edition in Taiwan, *Principles* was used in Continental Europe, Asia, and South America.[48] The Russian translation, which was printed in greater numbers than the English original, was widely used until 1948, when it was famously criticized by Lysenko in a report to the V. I. Lenin Academy of Agricultural Science.[49] Subsequently banned, "it came to be passed from hand to hand like a subversive tract."[50] The pedagogical influence of *Principles* was not even limited to the classrooms in which it was used, due to its impact on the content and method of teaching employed in many other popular genetics textbooks. For example: a method of teaching "three-factor mapping" that was apparently invented by Dunn and Sinnott in 1925 was widely copied in the following decades and persists in many introductory genetics textbooks today, despite the fact that deficiencies in this method have been identified.[51] In addition, several illustrations from *Principles* were slightly modified and used in James D. Watson's influential *Molecular Biology of the*

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[44] S. Fogel and I. H. Herskowitz, Review of *Principles of Genetics*, by E. W. Sinnott, L. C. Dunn, and Th. Dobzhansky, *Quarterly Review of Biology* 27 (1952): 211.

[45] J. A. Moore, *Heredity and Development*, 2nd ed. (New York: Oxford University Press, 1972), 134.

[46] S. Wright, "Genetics for the Classroom," Review of *Principles of Genetics*, by E. W. Sinnott and L. C. Dunn, *Journal of Heredity* 17 (1926): 185.

[47] W. G. Whaley, "Edmund Ware Sinnott," *Biographical Memoirs of the National Academy of Sciences* 54 (1983): 355; see also 357: "It is difficult to establish the priority among Sinnott's many contributions, but if the *Principles of Genetics* does not occupy the foremost position, it certainly comes close to it." When seen in light of the importance of Sinnott's many contributions to genetics and biology in the first half of the twentieth century, Whaley's comment must be read as a great tribute to *Principles*.

[48] Th. Dobzhansky, "Leslie Clarence Dunn," *Biographical Memoirs of the National Academy of Sciences* 49 (1978): 82.

[49] Lysenko stated: "An example of how far our native Mendelists-Morganists uncritically accept idealistic genetics is the fact that until recently the basic textbook on genetics in many of our higher institutes of learning is a translation of the strictly Morganist American textbook of Sinnott and Dunn," in T. D. Lysenko, "The Situation in Biological Science," in *Death of a Science in Russia*, ed. C. Zirkle (Philadelphia: University of Pennsylvania Press, 1949), 111. This comment was well known and was seen as an example of Lysenko's attack on classical genetics. See, e.g., R. C. Cook, "Lysenko's Marxist Genetics: Science or Religion?" *Journal of Heredity* 40 (1949): 182. I will return to this topic in Chapter 4.

[50] Dobzhansky, "Leslie Clarence Dunn," 82.

[51] M. Chalfie, "Is the Traditional Way of Teaching Three-Factor Mapping Sufficient?" *Trends in Genetics* 13 (1997): 94.

Gene.[52] And, as I discussed in the previous chapter on historical exemplars, the widespread use of problem-solving in textbooks of the 1930s-1960s was modelled on Sinnott and Dunn's pedagogical innovation in the first edition of *Principles*. Thus, it was during the life of *Principles* and long after that the method and style of teaching developed by Sinnott and Dunn shaped the teaching of genetics; and it is for this reason that I will focus on their text.

In *Principles*, the use of Mendelian genetics as the basis for the "historical" presentation of the principles of genetics—and the illustration of the science's coherence and inner logic—was achieved through an *exception-overcome-by-extension*, or dialectical, narrative structure: Mendel's principles were explained, evidence was introduced that seemed to conflict with the principles, the principles were refined or rearticulated in light of the evidence, and seemingly problematic evidence was again introduced, etc. In this manner, the heterogeneous body of knowledge conveyed by the textbook was unified on a common ground. Evidence and theory that were not discovered by Mendel were presented as "interpreting, amplifying, and modifying Mendel's principles"—and thus, as being essentially Mendelian.[53] The central argument underlying this narrative was first explicitly articulated in the conclusion to the chapter on the principle of independent assortment. Here, Sinnott and Dunn discussed the fact that the "thousands of experiments performed" in the first decades of the twentieth century had "resulted in the discovery of several entirely new principles," but characterized these as mere "amplifications" of Mendel's principles: "Perhaps the most striking result of all this activity, however, is that Mendel's main conclusions still remain, essentially unchanged, as the cornerstone of the science of genetics."[54] This claim was substantiated throughout the rest of the textbook. In roughly every other chapter that followed, a group of apparent exceptions to Mendel's principles was shown to require not a rejection of them, but rather a refinement or extension of their meaning.

The first "exceptions" to be discussed in *Principles* were the dihybrid crosses that resulted in "modifications" of the Mendelian ratios. Sinnott and Dunn introduced these

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[52] Whaley, "Edmund Ware Sinnott," 355.
[53] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 11.
[54] Ibid., 76

exceptions with a discussion of their discovery in the early twentieth century. They noted that research in this period had proven that the principles were not "sufficient to explain all the facts," but emphasized that this did not mean they were incorrect, explaining: "These principles are, indeed, the firm foundation upon which all later research has built, but they have necessarily been qualified and extended."[55] The body of the chapter was then devoted to detailing ways in which incomplete dominance, factor interaction, and epistasis could result in "modifications" of the Mendelian ratios. Each subdivision of the chapter dealt with a different modification, including the 9:7, 9:3:4, 12:3:1, 13:3, and 15:1 ratios.[56] By presenting these ratios as variations of the 9:3:3:1 ratio—as different tokens of a single form—the authors reconciled these non-Mendelian results with Mendel's principles. The apparent exceptions to Mendelism were shown to be essentially, in the sense of mathematically, Mendelian. In this way, the chapter gave new meaning to Mendel's principles. It treated them as abstract mathematical forms within which numerous unknown variations were able to reside. This recharacterization was, however, masked by the form of the presentation, which treated the modified ratios as inherent features, or natural extensions, of the standard ratios. These central claims of the dialectical narrative were reiterated in the chapter's conclusion. Emphasizing that "unsuspected complications" to Mendelism were often "so intricate and obscure as to suggest, at first, that they were unexplainable in simple terms," Sinnott and Dunn highlighted the eventual synthesis: "advancing knowledge has bought them one by one into harmony with the underlying principles enunciated by Mendel."[57] Here, the exception-to-extension narrative of this chapter ended, with the claim that Mendel's "underlying" principles remained essentially intact as the foundation of genetics.

In addition to showing a reconciliation of past exceptions with Mendelism, the dialectical presentation created a picture of the principles of genetics in which their development followed along a logical trajectory. This picture was not only of the past, but also of a future in which Mendel's principles would continue to provide the foundation of the science. Sinnott and Dunn articulated this idea explicitly in the

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[55] Ibid., 84.

[56] Ibid., 92-110.

[57] Ibid., 119.

conclusion to the chapter on modified ratios: "Since many of the puzzling cases of the past have been resolved by mendelian experiments, it is probable that the new and more difficult problems encountered in experimental breeding will, on careful analysis, yield to this method and be reconciled with already familiar principles."[58] Thus, the idea of progress embedded in the use of the dialectical narrative not only presented the past as though it had been gradually built on the foundation of Mendel's principles, but also implied that the future would continue along these lines. In the following chapters, this picture was substantiated through the step by step reconciliation of more "apparent exceptions."

The next chapter to extend the reach of Mendel's principles, "Linkage," began much as the chapter on modified ratios did: it set up a potential conflict between the principles and the experimental results of early twentieth century research. About independent assortment, the authors wrote: "Soon after the rediscovery of Mendel's principles some doubt began to be cast on the universal applicability of this principle, since it did not explain certain exceptional results."[59] They then went on to discuss the first case of linkage discovered by Bateson and Punnett in 1906 and the wide variety of cases that were subsequently identified. This discussion provided the background for an explanation of how the principle of linkage was developed by Morgan to deal with these phenomena. In discussing Morgan's work, they did not once mention Mendel. But in the conclusion to the chapter, they once again returned to Mendel in order to reconcile Morgan's "formulation of a new principle of heredity, the principle of linkage," with the principles of Mendel. According to Sinnott and Dunn, the principles were fundamentally similar:

> The history of mendelian inheritance and the history of linkage, the most important modification of mendelism, are in some respects parallel. Mendel's principles were established from careful breeding experiments with one plant, under the guidance of new methods and a new statistical notation…The principles of linkage were discovered during intensive breeding experiments with one animal, the fruit fly, under the guidance of Mendel's notation *plus* a new idea.[60]

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[58] Ibid. NB: the term "mendelian" was not capitalized in the first edition of *Principles of Genetics*.

[59] Ibid., 150.

[60] Ibid., 167 (emphasis original).

By taking the basic formula of Mendel's research as a point of substantive similarity with Morgan, and emphasizing that Morgan merely added one idea, Sinnott and Dunn characterized the discovery of the principle of linkage as fundamentally based on Mendel's methodological insights. Linkage was presented as an essentially Mendelian principle. It is important to note, however, that—as in the chapter on modified ratios—this reconciliation did not leave the reconciled entities unchanged. Whereas the previous chapter had treated Mendel's principles as malleable mathematical ratios, this chapter treated them as the embodiment of a problem and an approach to solving it.

The extension of Mendelism and assimilation of Morgan's principles under those of Mendel continued in the following chapter, "The Chromosome Theory of Inheritance," which opened with a reiteration and further explanation of the ways in which these principles were similar:

> The facts presented in the preceding chapter have led to the conclusion that linkage, instead of being a sporadic exception to Mendelian inheritance, is actually a widespread, orderly occurrence, subject to laws which are not only of the same cogency as those dealing with the segregation of factors but which also give a reasonable explanation of the *mechanism* of segregation and assortment.[61]

On this account, Morgan's and Mendel's principles were not only similar in form and nature, but also in content, as both helped explain the same phenomena. The only difference was that whereas Mendel identified the law-like nature of the phenomena, Morgan identified the mechanism behind it, showing that linkage was best explained by the chromosome theory of heredity. By opening the chapter in this way, Sinnott and Dunn set up their discussion of "the many suggestive implications" of the chromosome theory in such a way that they seemed to fall within the trajectory of Mendel's discoveries. For example, when identifying the implications of the chromosome theory, they wrote:

> It means for one thing that the genes are actual particles of living material, occupying a measurable portion of space…The nucleus of each cell is on this view to be regarded as a complex aggregation of units which are arranged and guided through the processes of inheritance and

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[61] Ibid., 177 (emphasis original).

> development by those laws which in the end are the physical and chemical laws of living matter.[62]

Here, the meaning of Mendel's principles gained a materiality that they did not have in the context of Mendel's experiments, which was the context in which the textbook had initially identified and explained them. Thus, the meaning of the principles shifted as the student read the text, just as they had shifted through the historical development of the discipline. The final section of the chapter, on the significance of the chromosome theory, reiterated this claim about the material extension of Mendelism: "Since the work of Mendel it has been known that the animal or plant may be viewed as an aggregation or separate units…From the work of Morgan and other investigators it is now known that the gene is a definite part of the chromosome with a definite location therein."[63] On this account, Morgan's work merely identified the bodies of the entities that Mendel had long before discovered.

The fourth class of hereditary phenomena brought under Mendel's principles included those quantitative characters that were influenced by multiple genes. The chapter on these phenomena, like those on the other exceptions, began with a discussion of the period following the rediscovery of Mendel's principles. Highlighting the fact that "such characters were long thought to constitute important exceptions to Mendel's principles and to require some other principle for their explanation," Sinnott and Dunn went on to state that these traits were Mendelian in the sense that they relied on the genetic mechanism identified in Mendel's principles:

> In recent years…it has been recognized that even these quantitative characters are not essentially different in their method of inheritance from the more definite ones on which Mendel based his theory. The extension of mendelian principles to this very important group of traits is one of the most notable advances which genetics has made since Mendel's day.[64]

Here, Sinnott and Dunn stated the reconciliation of a problem that they subsequently identified in the chapter. Stated briefly, the problem was that the distribution of characteristics such as height and weight—unlike binary variations in coloring or shape—could not be easily categorized and counted; and consequently, it seemed that

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[62] Ibid. Corollary principles, such as "the principle of the limitation of the linkage groups," were also brought under the Mendelian trajectory: Sinnott and Dunn, *Principles of Genetics*, 1st ed., 180.

[63] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 188.

[64] Ibid., 234.

they could not be subject to Mendel's principles. But, as in the other chapters, this was shown to be only an apparent problem, as Sinnott and Dunn explained how biometric methods were developed to study these quantitative characters and how the multiple factor hypothesis was developed to explain them. Sinnott and Dunn did not mention the significance of these developments with respect to Mendel's principles in the body of the chapter. But in the last sentence, they once again returned to Mendel. Concluding their discussion of the multiple factor hypothesis, they wrote: "Its chief contribution has consisted in bringing size characters, at first thought to be radically different in their inheritance from qualitative ones, definitely under the operation of mendelian principles."[65] In this reconciliation, it was the mechanism identified by Mendel that provided the point of contact between his principles and the extension.[66]

In general, the historical approach to teaching in *Principles of Genetics* relied on a dialectical narrative that ran throughout the chapters dealing with the "principles" of genetics. In the first edition that I have studied here, this was roughly every other chapter, and thus the narrative device spanned the entire textbook.[67] In later editions of the textbook, the authors added many chapters that were not about "principles," such as the chapters on development, population genetics, mutation, and cytoplasmic inheritance; the dialectical narrative did not run through these chapters, but other types of statements had similar effects. In the fourth edition's chapter on population genetics, for example, the authors wrote: "Modern population genetics is founded on a proposition deduced in 1908 by Hardy and by Weinberg as a corollary of Mendel's principle of segregation."[68] Moreover, when the authors discussed the addition of new chapters to later editions, they explained that the chapters on principles were the most important. In the preface to the

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[65] Ibid., 260.

[66] Note that in later editions, they strengthened this synthesis by arguing that there was no essential difference between the qualitative and quantitative traits—that these were not natural properties of different traits: "the same character, such as size, which in a population of microorganisms may be treated as quantitative at one magnification, may, at another, be classified into discontinuous categories and treated qualitatively." They argued that the distinguishing feature of nominally "quantitative" characters was "the transgressive or apparently continuous way in which the variations must be *described*" (emphasis added). In this way, they broke down the conceptual barrier that prevented some forms of inheritance from being characterized as Mendelian, stating that the Mendelian phenomena were merely difficult to recognize. See Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 4th ed., 121.

[67] Only chapters discussing applications of the principles (e.g., animal breeding, and eugenics) or special cases of the principles (e.g., sex-linkage, and inheritance in man) did not have this structure.

[68] See Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 4th ed., 307.

third edition, for example, they stated: "The heart of the book, however, continues to be general and well-established principles and the exemplification of these in problems for the student to solve."[69] Thus, it was in the core of the textbook that the dialectical narrative functioned, creating transitions between chapters dealing with principles of genetics that were developed at different times to deal with the evidence produced by different lines of research.

To briefly summarize, my argument thus far has been that the "historical approach" to teaching relied on a dialectical narrative in which the authors gradually reformulated Mendel's principles. By emphasizing a different aspect of the principles in each chapter, ranging from mathematical to mechanical features, the authors were able to create points of contact that allowed for the reconciliation of different types of exceptions that had been discovered in the twentieth century. Thus in all of the chapters, the exceptions were not reconciled with the principles Mendel articulated, but rather with a conception of their fundamental nature.[70] For this reason, it was the ideas inside the laws, rather than the laws themselves, that were highlighted as Mendel's greatest achievement: "Mendel's *greatest contribution* to genetics was the *idea* that the organism is an aggregation of separable units which are inherited in an orderly manner according to certain definite laws."[71] It was by defining Mendel's "greatest contribution" in terms of an idea and not a law, that the authors were able to turn exceptions into extensions. It was in this way that they presented Mendel's principles as "the firm foundation upon which all later research has been built" and were able to conclude: "Mendel's main conclusions still remain, essentially unchanged, as the cornerstone of the science of genetics."[72] In the picture created by this dialectical narrative, twentieth century geneticists appeared to be fundamentally similar to Mendel, and Mendel appeared to be a geneticist; the interpretations were mutually reinforcing.

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[69] Sinnott and Dunn, *Principles of Genetics*, 3rd ed., vii. See also Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 5th ed., viii, where the authors stated that individual chapters from Chapter 20 onwards could be skipped at the instructor's discretion; they explained that the most important chapters, Chapters 3-19, were those that contained "quantitative" subjects and problem-solving exercises.

[70] It is worth noting that Dunn thought that most scientific advances and discoveries merely involved the extension of existing ideas, as he explained in Dunn, *A Short History of Genetics*, 208-210. See also page xix, where he distinguished between hypotheses and ideas, concluding, "there are not very many 'ideas' in genetics altogether."

[71] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 84 (emphasis added).

[72] Ibid., 84, 76, respectively.

Although this study has focused on how the historical approach was written in the highly influential *Principles of Genetics*, the features of the text that I have identified and analyzed were common in textbooks of the 1930s-1960s. The vast majority of chapters in Dodson's *Genetics*, for example, opened with a statement about Mendel's principles or methods, thereby situating the subject matter in relation to Mendelism.[73] White explicitly identified the principle-to-extension logic in the preface to the second edition of *Genetics*.[74] In textbooks by Altenburg, Begg, King, Singleton, Srb and Owen, and Winchester, the exception-to-extension style of narrative was used to introduce and explain modified Mendelian ratios, linkage, and quantitative inheritance;[75] and as in *Principles*, it was these topics on the principles and their extensions that were said to be the most important.[76] In addition, Mendel's laws were generally described as having remained "essentially" unchanged by these modifications. For example, after explaining that "the system of Mendel had to be modified" due to the discovery of sex linkage, linkage, and gene interaction, Shull concluded: "Thus the scheme grew and developed. Yet through all these changes *the essential Mendelian feature remained*—the segregation and recombination of the units of heredity."[77] Similarly, in a discussion of multiple factors in *Genetics*, Altenburg suggested: "Certainly the more complicated cases of inheritance do not disprove Mendel. On the contrary they point to something dependent upon his principle: multiple factors."[78] A. W. Lindsey agreed in *A Textbook of Genetics*, characterizing Mendel's laws as being merely "amplified and supplemented by other discoveries," and concluding, "the whole field of genetics is replete with demonstrations of their truth."[79] There were a few textbooks in the 1930s that did not treat Mendelism as

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[73] E. O. Dodson, *Genetics: The Modern Science of Heredity* (Philadelphia: W. B. Saunders, 1956), Chapters 1-11, 17-18, 20-22.

[74] White, *Genetics*, 2nd ed., preface.

[75] See, e.g., the explanation of these topics in Altenburg, *Genetics*, 1st ed., 67-70, 82-96; Winchester, *Genetics*, 1st ed., 73-74, 78-83; Begg, *An Introduction to Genetics*, 80-91, 171-186; King, *Genetics*, 1st ed., 79-88, 94-95, 103; and Srb, Owen, and Edgar, *General Genetics*, 2nd ed., 23-26, 146-152, 442, 450-454.

[76] E.g., Hovanitz, *Textbook of Genetics*, vi: "For a shortened course…it is suggested that the first ten chapters be used with portions of the remainder." See also Dodson, *Genetics*, v, where he suggested that it was just the first fourteen chapters that were essential.

[77] Shull, *Heredity*, 4th ed., 8 (emphasis added).

[78] Altenburg, *Genetics*, 1st ed., 96.

[79] A. W. Lindsey, *A Textbook of Genetics* (New York: Macmillan, 1932), 71.

the foundation of the science.[80] But even some of these explicitly articulated the implicit argument in *Principles*. In Walter's *Genetics*, for example: "Since Mendel formulated these rules, numerous 'exceptions that prove the rule' have been discovered."[81]

This exception-to extension narrative was widely used to create a picture of genetics founded on Mendelism—an idea that was explicitly articulated by Charles Begg in *An Introduction to Genetics*: "An appreciation of this simple fact [the existence of multiple-factors] will at one and the same time demonstrate how universal in application are the fundamental Mendelian principles, and how elaborate is the fabric which may be woven with their help."[82] Srb and Owen made a similar claim in *General Genetics*, using words that were identical to those of Sinnott and Dunn: "The principle of independent assortment…is one of the cornerstones on which an understanding of genetic systems has been built."[83] And A. M. Winchester made an even stronger claim in *Genetics*, stating: "It should be kept in mind, however, that these deviations are the exception, and that the great majority of dihybrid crosses yield the 9:3:3:1 ratio."[84] As a corollary of the exception-to-extension narrative structure and the presentation of Mendelism as the foundation of genetics, many textbook authors presented twentieth century theories as being located in the mind of Mendel. Altenburg's *Genetics*, like Sinnott and Dunn's *Principles*, gave Mendel substantial credit for the development of the chromosome theory of heredity:

> It should be emphasized that no one man is to be credited with the chromosome theory…But the name most prominently associated with the development of the theory must be that of Mendel. For it was he who showed how the genes were distributed…Thus, Mendel's experiments laid the foundation of the chromosome theory.[85]

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[80] E.g., Jennings, *Genetics*, presented genetics as a system, and each chapter focused on a different aspect of the system; and H. E. Walter, *Genetics: An Introduction to the Study of Heredity*, 4th ed. (New York: Macmillan, 1938), treated genetics methodologically, organizing the text broadly in terms of "five avenues of approach."

[81] Ibid., 82; see also 83: "It may be repeated that they strengthen rather than weaken the original theory of Mendelian inheritance."

[82] Begg, *An Introduction to Genetics*, 186. See also, e.g., the section titled "The Universality of Mendel's Principle," in Altenburg, *Genetics*, 1st ed., 95-96.

[83] Srb, Owen, and Edgar, *General Genetics*, 2nd ed., 146.

[84] Winchester, *Genetics*, 1st ed., 83.

[85] Altenburg, *Genetics*, 1st ed., 76.

Here, and elsewhere, Mendel was also credited with the discovery of genes.[86] Thus, the analysis of *Principles of Genetics* that I developed in this section does not only apply to this one influential textbook, but rather identifies widespread features of the textbook teaching of genetics in the 1920s-1950s.

Although I have thus far focused on how the historical approach was related to the stability of the conceptual order of genetics, it is also important to look at how this approach affected its malleability over time. The narrative structure of the textbook placed experiments, evidence, and principles in hierarchical relationships that did not remain constant across the editions of a textbook, but rather shifted, often through the rewriting of history. To explore the form and content of these shifts in the ways the science fitted together, I will return to my case study of *Principles of Genetics* in the next section, analyzing the first four editions' presentations of the chromosome theory of inheritance.

Before returning to this case study, however, it is perhaps worth noting that geneticists in the 1940s were not unaware of the gradual transformation of the meaning of Mendel's principles. This was, in fact, explicitly identified in some textbooks' discussions of the "essential idea" of the principles.[87] In *Heredity*, for example, Shull discussed the malleability of geneticists' conception of "the essential characteristics of Mendelian heredity."[88] He identified various conflicting ways in which the nature of Mendel's principles had been defined, focusing on geneticists' disagreements about whether dominance and blending inheritance were "Mendelian." Shull explained that some geneticists saw dominance as a central part of Mendel's experiments, and therefore regarded blending inheritance as non-Mendelian because dominance was absent. He then explained that others defined Mendelian heredity as the segregation and independent assortment of genes: distinguishing Mendel's principles from other features of heredity "by the fact that they depend upon the behavior of the chromosomes," these geneticists thought that dominance was not central to Mendelism because it had "no relation to these

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[86] See, e.g., W. R. Singleton, *Elementary Genetics* (New York: D. Van Nostrand, 1962), 34.

[87] See, e.g., Shull, *Heredity*, 1st ed., 134-135; Shull, *Heredity*, 4th ed., 160; and Altenburg, *Genetics*, 1st ed., 42-43.

[88] Shull, *Heredity*, 1st ed., 134.

chromosomal movements." Shull advocated this later view, suggesting that blending inheritance should be considered Mendelian for these reasons. But he noted that this conception raised questions about other aspects of heredity, including "crossing-over" and the various "forms" of non-disjunction: "Are these peculiarities to be excluded from Mendel's law, or shall the law be defined to include them?" Answering this question, and concluding this section, he wrote: "Usage has differed greatly, but there seems to be a growing tendency to regard as Mendelian all heredity that definitely depends upon chromosomes or their behaviour."[89] Shull thought that geneticists had shifted the boundaries of Mendelian phenomena, including and excluding certain forms of heredity, to bring them in line with a chromosomal conception of Mendel's laws.[90] In later editions of *Heredity*, he articulated this claim more fully. Discussing whether linkage, the first apparent exception to Mendelism, should be thought of as falling under Mendel's laws, Shull wrote:

> Mendel never observed linkage; should linkage then be excluded from Mendelian heredity? Considering that linkage is produced by the very things (chromosomes) which are responsible for independent assortment, geneticists chose not to separate linkage from the other fundamental features of heredity and have extended the term Mendelian heredity to cover it. Once this extension was accepted, it was logical to include the other things that are dependent on the regular behaviour of chromosomes, such as sex-linked inheritance. And finally, even those phenomena which result from irregularities of chromosome conduct—nondisjunction, translocation, duplication, deficiency—were regarded as Mendelian phenomena. At the present time, any transmission that is directly dependent on chromosomes is considered Mendelian heredity.[91]

Thus, geneticists such as Shull were well aware of the malleability and historical development of geneticists' sense of the nature of Mendel's laws. And it is with their chromosomal characterization of this shift in mind that I now turn to look at textbooks' presentations of the chromosome theory of heredity.

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[89] Ibid.

[90] Note that on this conception, "non-Mendelian" inheritance was limited to cytoplasmic inheritance—the topic of the chapter in which this discussion of Mendelism was located.

[91] Shull, *Heredity*, 4th ed., 160.

A Malleable Conceptual Order: Reshaping the Chromosome Theory of Heredity

In 1925, when *Principles of Genetics* was first published, the chromosome theory of heredity was a matter of general consensus in the Anglo-American community of geneticists.[92] Stephen Brush's detailed case study of the reception of the theory identifies the types of evidence that led to this consensus.[93] Most important were the phenomena of sex-linkage and non-disjunction, the parallelism between chromosomal behavior and Mendel's laws, the correspondence between the number of linkage groups and chromosomes, and the ability to predict crossing-over frequencies from linear genetic maps.[94] In an attempt to understand how the theory "became knowledge," Brush analyzes the assimilation of the chromosome theory into textbooks of genetics, cytology, and biology in the 1910s-1920s. He finds that there was often a discrepancy between the evidence geneticists found convincing, and the evidence given as proof of the theory—that the evidence cited varied according to discipline and audience.[95] Although this study sheds light on how the chromosome theory disseminated outside the community of geneticists, it leaves open important questions about the meaning of the knowledge within the community and the ways in which this changed after the theory was widely accepted.[96] For example: when new questions and fields of research emerged in the following decades, how were older bodies of evidence and theory brought into relationships with those under development? Within textbooks using a historical approach, were chapters and their contents reorganized? What effect did any recontextualization within a volume have on the picture of the science it presented? It is

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[92] S. G. Brush, "How Theories Became Knowledge: Morgan's Chromosome Theory of Heredity in America and Britain," *Journal of the History of Biology* 35 (2002): 503-506.

[93] Ibid. On the reception of the chromosome theory outside the United States, see A. G. Cock, "William Bateson's Rejection and Eventual Acceptance of the Chromosome Theory," *Annals of Science* 40 (1983); J. Harwood, *Styles of Scientific Thought: The German Genetics Community 1900-1933* (Chicago: University of Chicago Press, 1993), 33-45; and J. Gayon and R. M. Burian, "France in the Era of Mendelism (1900-1930)," *Comptes Rendus de l'Académie des Sciences, Paris, Sciences de la Vie* 323 (2000).

[94] Brush, "How Theories Became Knowledge," 516-518.

[95] Ibid.: 510-515. He argues, for example, that although practitioners considered non-disjunction to be the most compelling proof of the theory, many textbooks—he identifies Shull's *Heredity*—did not present this evidence and focused instead on less technical evidence, such as the correspondence of linkage groups. While this is true of the first edition of Shull's *Heredity,* which he cites as evidence, he fails to note that Shull included non-disjunction in later editions; see, e.g., the fourth edition.

[96] As Peter Galison suggests in his analysis of the many ways in which Maxwell's equations have been reinterpreted since the time of their invention, the resituating of past theories can serve important disciplinary functions: P. Galison, "Re-Reading the Past from the End of Physics," in *Functions and Uses of Disciplinary Histories*, ed. L. Graham, W. Lepenies, and P. Weingart (Dordrecht: D. Reidel, 1983).

to begin to answer these questions that I now return to my case study of *Principles of Genetics*, exploring how Sinnott and Dunn gradually revised their presentation of the proof of the chromosome theory, thereby analyzing the malleability of the conceptual order of genetics.[97]

In the 1925 edition of *Principles*, Sinnott and Dunn presented evidence for the chromosome theory in a series of three chapters—"VI: The Physical Basis of Heredity," "VII: Linkage," and "VIII: The Chromosome Theory of Inheritance."[98] In the first of these chapters, they discussed early support for the chromosome theory, identifying the "striking parallels" between geneticists' accounts of the Mendelian behavior of genes and cytologists' accounts of the behavior of chromosomes in cellular meiosis. According to Sinnott and Dunn, these parallels indicated that the genes were located in the chromosomes; but they did not prove it because of an epistemological difference between geneticists' knowledge of the behavior of genes and cytologists' knowledge of the behavior of chromosomes. Cytologists' account of chromosomal behavior was "a description of fact, since it is founded on actual microscopic evidence," whereas Mendel's laws were "hypotheses which satisfactorily explain the results of breeding experiments."[99] Reiterating that the parallel was therefore not sufficient proof, Sinnott and Dunn concluded by stating that they would identify the crucial evidence in the next two chapters.[100] In the following chapter, Sinnott and Dunn discussed the development of geneticists' knowledge about linkage—from an exception to Mendel's laws, to a principle of genetics—and presented the principle as providing additional support for the chromosome theory. But in the chapter's conclusion, they once again stated that the

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[97] It is important to note that my analysis of the *revisions* to the presentation of this evidence will by definition set aside questions about the categorical exclusion of certain forms evidence. For an extensive discussion of the denigration of embryologists' work on the chromosome theory—a disciplining use of history that falls outside the scope of this chapter—see S. Gilbert, "Bearing Crosses: A Historiography of Genetics and Embryology," *American Journal of Medical Genetics* 76 (1998), which I discussed in the Introduction. For more on the relationship between genetics and embryology, see S. Gilbert, "The Embryological Origins of the Gene Theory," *Journal of the History of Biology* 11 (1978); and G. E. Allen, "T. H. Morgan and the Split between Embryology and Genetics, 1910-1935," in *A History of Embryology*, ed. T. J. Horder, J. A. Witkowski, and C. C. Wylie (New York: Cambridge University Press, 1983).

[98] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 147. This format was standard. See, e.g., Snyder, *The Principles of Heredity*, 2nd ed., 32; and Snyder and David, *The Principles of Heredity*, 5th ed., 47-48.

[99] Sinnott and Dunn, *Principles of Genetics*, 1st ed., 144; here, they also described these as being parallels "between a concrete set of facts (chromosome behaviour) and the hypotheses proposed to explain another set of facts (factor behaviour)."

[100] Ibid., 147.

"specific evidence" and proof of the theory's validity came from the discoveries they would outline in the last of the series of three chapters.[101] There, in "The Chromosome Theory of Inheritance," Sinnott and Dunn first discussed the correspondence between the number of linkage groups and the number of chromosomes (the principle of the limitation of the linkage groups), and then explained Bridges' discovery of non-disjunction—a special case of sex-linkage, in which the transmission of a sex-linked gene parallels the irregular transmission of X chromosomes. The principle of the limitation of the linkage groups offered "cogent" and "negative" evidence for the theory, according to Sinnott and Dunn, but it was Bridges' combined cytological and genetic studies of non-disjunction that provided "positive" and "conclusive" proof.[102] They did not discuss these experiments in detail, however, and focused instead on the experiments that "accomplished the more difficult feat" of showing that "the genes are arranged in a single straight line, and their distances apart are proportional to the amount of crossing-over between them."[103] This principle, the linear order of the genes, was a "secondary or supporting principle of the chromosome theory," according to Sinnott and Dunn.[104] After discussing the development of this principle, they concluded the chapter with a section on the significance of linkage and the chromosome theory for the study of genetics.

To briefly summarize the main points of my analysis thus far: in a series of three chapters, "The Physical Basis of Heredity," "Linkage," and "The Chromosome Theory of Inheritance," discoveries and work on linkage and the linear arrangement of the genes were organized to progress and culminate in an account of the chromosome theory of inheritance. The first chapter noted parallels that indicated the validity of the chromosome theory; the second discussed a principle that offered support; and the third provided cogent but negative evidence for the theory, followed by conclusive and positive proof. By presenting this evidence as a series of steps forward—from uncertainty to certainty—Sinnott and Dunn unified these chapters in a quasi-historical narrative of progress, in which progress in theory appeared to be driven by the historical

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[101] Ibid., 169.

[102] Ibid., 180

[103] Ibid., 180, 184.

[104] Ibid., 184.

accumulation of evidence presented in the text. With this ordering of experiments, data, and discovery, the authors defined the hierarchical order of theory and evidence in the science of genetics. In addition, they differentiated various forms of evidence, implicitly defining the relative weight of evidence according to the experimental practices that produced it. Equating facts with things that could be visually observed, they suggested that cytology allowed for the identification of "facts" about heredity. They emphasized that although breeding experiments produced data that could support "hypotheses" about heredity, such as Mendel's and Morgan's principles, these were not visually verifiable and therefore not facts.[105] Both breeding experiments and cytology, on their account, indicated the validity of the chromosome theory of inheritance. But it was only when Bridges brought them together in his cytogenetic research on non-disjunction that geneticists obtained proof.[106] Through this account of the history of the discovery, Sinnott and Dunn not only presented a hierarchy of evidence and theory in which linkage and Mendel's principles supported the chromosome theory of inheritance, but also a hierarchy of experimental methods with cytogenetics at the top.

The way in which Sinnott and Dunn thought about the collection of facts and theories for the chromosome theory of inheritance was not stable, however, as can be seen in the significant revision and reorganization of the text of these three chapters in the second edition in 1932. In the first of the chapters, on the physical basis of heredity, explanations of gamete cell structure and reproduction, which previously formed half the chapter, were substantially reduced and transformed into an introduction for new sections on the chromosome theory of sex determination and its "important influence on ideas concerning the physical basis of inheritance."[107] These new sections presented explanations of sex-linked factors and non-disjunction, much of which had been imported from later chapters.[108] Within these sections, parallels between transmission of sex-linked genes and X chromosomes were added as evidence of the gene's location in the chromosome. But, as in the first edition's presentation of the more general parallels

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[105] Ibid., 144.

[106] Ibid., 169.

[107] Sinnott and Dunn, *Principles of Genetics*, 2nd ed., 116.

[108] E.g., the discussion of sex chromosomes in the second edition was imported from "Chapter X: Sex and Its Inheritance" in the first edition; thus, in the second edition, this chapter had less content and was renamed, "The Determination of Sex."

between genes and chromosomes, the authors emphasized that these conclusions "remained highly probable inferences" until Bridges provided proof with nondisjunction.[109] In the chapters that followed, far less text was changed. The few modifications to the chapter on linkage had little effect on the picture it presented.[110] But the several small changes to the chapter on the chromosome theory were significant. A new opening discussion of the history of the theory emphasized that it did not develop from similar theories proposed by Weismann or investigators in the field of experimental embryology.[111] The discovery of the genes' linearity was no longer presented as a supporting principle of the chromosome theory, but rather was characterized as "an additional principle of heredity which Morgan has called the Linear Order of the Genes."[112] A new section compared cytological and genetical maps, illustrating the cytological support for chromosomal gene maps. And the conclusion no longer discussed the significance of linkage and the chromosome theory. Taken as a whole, the 1932 revisions to the three chapters constituted a new presentation of these aspects of the science. The series still culminated in a chapter on the chromosome theory of inheritance. But given that non-disjunction—the proof of the genes' location in the chromosomes—was explained in the first chapter, the three chapters were no longer organized around a narrative of accumulating evidence for the theory. Rather, they progressed towards the creation of gene maps. And unlike the first edition, in which the chapters progressed from cytological and genetic evidence to cytogenetic evidence, all three of the second edition's chapters presented steps that were achieved through a complementary use of cytology and breeding experiments. In the first, it was the discovery of non-disjunction that confirmed the genes location in the chromosomes. In the second, it was the discovery of linkage groups that confirmed the principle of linkage. In the third, it was the parallels between theoretically-formulated linkage maps and visually-formulated cytological maps that supported the development of a physical

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[109] Sinnott and Dunn, *Principles of Genetics*, 2nd ed., 125.

[110] E.g., a few cases of linkage were added, the conclusion on the widespread occurrence of linkage was replaced by one on factors affecting the strength of linkage, and the section on multiple allelomorphs was moved to an earlier chapter; see page 147. See also Sinnott and Dunn, *Principles of Genetics*, 1st ed., 165.

[111] Sinnott and Dunn, *Principles of Genetics*, 2nd ed., 149.

[112] Ibid., 153. Note that Morgan's "principle of Limitation of the Linkage Groups" was also capitalized and identified as being a principle on page 152.

representation of genes in chromosomes. This shift towards a cytogenetic characterization of the body of evidence and theory for the chromosome theory was further developed in subsequent editions, providing the basis for a representation of genes as structural features of the chromosome.

The revisions to the chapter on the physical basis of heredity for the third edition in 1939 changed very little. There was a new short section on the discovery of *Drosophila's* giant salivary gland chromosome—a discovery that made it "possible to identify the factors of heredity, the genes, with specific structures in the chromosome."[113] Here and elsewhere in this new section, the focus was on the genes' materiality.[114] And the few small additions to the next chapter had a similar purpose: the new title—"Linkage and Linkage Maps," instead of "Linkage"—drew attention to the chapter's focus on the physical representation of genes, as did new concluding sections, which highlighted the genes' structural properties and behavior as bodies within the chromosomes.[115] The only text deleted from the chapter was the section on cytological demonstrations of crossing-over; and this was actually just moved into the next chapter on the chromosome theory, which was renamed "Genes and Chromosomes." Here, the imported section accompanied several new sections on salivary gland chromosome mapping; all of these sections presented hypotheses about the material structure of genes (e.g., deficiency, inversion, and duplication) that had been confirmed visually with cytological studies. And new text in the chapter's concluding paragraph reiterated that this body of evidence supported a material picture of the gene:

> By comparing the genetical and cytological findings in a number of such cases, it has been possible to specify the locations of many genes in the chromosomes and to show that the linear order of the genes is not only a necessary inference from the data of linkage and crossing over but a fundamental fact about the structure of chromosomes.[116]

Taken together, these revisions to the 1932 text moved this series of chapters further away from their original emphasis on the chromosome theory. Although the third chapter still discussed the theory, the narrative underlying the series culminated in a

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[113] Sinnott and Dunn, *Principles of Genetics*, 3rd ed., 163.

[114] E.g., ibid., 164: "the elements in the cross bands...constitute the nearest approach to the ultimate morphological structure yet attained."

[115] Sinnott and Dunn, *Principles of Genetics*, 2nd ed., 207-208.

[116] Sinnott and Dunn, *Principles of Genetics*, 3rd ed., 240.

picture of the genes as material bodies. The importance of this feature of genetic theory was emphasized by a dramatic change in the textbook's frontispiece: where a photograph of Mendel was once presented, the third edition provided the student with a detachable fold-out poster of Bridges' maps of the giant salivary gland chromosomes of *Drosophila*. Commenting on this revision in a review, Just wrote: "This change bespeaks well the new era in genetics."[117]

In the fourth edition of 1950, a new introduction to the chapter on the physical basis of heredity explicitly identified what had over the past 25 years become the focus of this series of three chapters. This new introduction differentiated the gene concept that had been used in the first six chapters of the textbook, which dealt with the laws of gene transmission, from the gene concept that would be used in the following chapters. About the transmission gene of the earlier chapters, the authors wrote: "The gene so defined is really a symbol or abstraction, and the body of knowledge concerned with this symbolic gene has come to be known as *formal genetics*."[118] They then explained that the series of chapters on chromosomal genetics would deal with "the material, or physical" gene, which was studied through a "hybrid" of genetics and cytology.[119] Revisions to their discussion of the gene gave substance to this new gene concept. In the section on "the fundamental hereditary material," for example, there were several changes. Where the third edition had identified the gametic cells as the fundamental hereditary link—"the only physical link between one generation and the next…is the pair of gametes"—the fourth edition focused on the genes: "the inherited constitution consists of discrete units, the genes."[120] This shift in emphasis was accompanied by a new discussion of the ways in which "the theory of the gene resembles the atomic and molecular theories of physics and chemistry." The authors identified similarities in form (e.g., "all these theories are *particulate*, or *corpuscular*") and similarities in justification (e.g., "their justification lies in the fact that they make intelligible many relationships…and often permit us to predict the results of new, hitherto untried

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[117] T. Just, Review of *Principles of Genetics*, by E. W. Sinnott and L. C. Dunn, *The American Midland Naturalist* 22 (1939): 755.

[118] Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 4th ed., 151 (emphasis original).

[119] Ibid.

[120] Sinnott and Dunn, *Principles of Genetics*, 3rd ed., 157; and Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 4th ed., 150.

experiments").[121] The conclusion to this chapter on the physical basis of heredity was similarly revised: having previously focused on the fact that genes were likely to be in the chromosomes, it was rewritten to highlight the fact that meiotic divisions provided the mechanism for the movement of genes in Mendel's laws. The substantial sections of text dealing with sex chromosomes (sex-linkage, attached X chromosomes, and non-disjunction), which had been imported into this chapter in 1932 as proof of the chromosome theory, were inserted into the following chapter, "Genes and Chromosomes," which was moved before the chapter on linkage and linkage maps.[122] All of the 1939 text from "Genes and Chromosomes," however, had been removed. The chapter contained only these newly imported sections on sex chromosome studies, which were presented as confirmation that the genes were located in the chromosomes.[123] This shift was complemented by slight changes in the following chapter on linkage: instead of introducing the idea of linkage by focusing on its support for the chromosome theory, the chapter focused on its support for the theory of the linear arrangement of the genes.[124] Sections that were previously in the chapter on genes and chromosomes, supporting the chromosome theory, were brought into discussions of linkage; this shift included the sections explaining the limitation of linkage groups and the linear arrangement of the genes, which were no longer called "principles of heredity" or attributed to Morgan, as they had been in 1939.[125] The section on "Chromosome Maps" was renamed "Linkage Maps."[126] And the comparisons of linkage and cytological chromosome maps—and the corresponding cytological analysis of the chromosome, which was shown to parallel and support the theories of geneticists—were no longer included in this chapter, but rather were located in a new chapter that followed, "Chromosome Aberration and Cytological Maps of Chromosomes."

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[121] Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 4th ed., 150-151. The only other addition was the inclusion of historical detail; the discussion of general cell biology was almost identical.

[122] Ibid., 179-192.

[123] Ibid., 197.

[124] Ibid., 203-204.

[125] Sinnott and Dunn, *Principles of Genetics*, 3rd ed., 218-220, 223-224; and Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 4th ed., 225. The sections on double crossing-over, interference, and coincidence were also moved into this chapter.

[126] Sinnott and Dunn, *Principles of Genetics*, 3rd ed., 219, 223; and Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 4th ed., 225.

These 1950 revisions and reorganizations of the 1939 text thus gave new significance to a body of experiments and evidence that had once been presented as being about chromosomes. The chapter on the physical basis of heredity no longer mentioned the chromosome theory, as it focused instead on genes as material entities. The proof of the chromosome theory was located in "Genes and Chromosomes," but this chapter no longer discussed genes and chromosomes generally, focusing instead on sex linked inheritance—as was noted in the fifth edition, where the chapter was renamed "Sex-Linked Inheritance." And the chapter on linkage was used to present evidence for the linear arrangement of the genes and gene maps, rather than for the chromosome theory. Thus, a narrative that had once culminated in the presentation of "the principle of the chromosome theory of inheritance" no longer even mentioned it as a "principle." In its place stood another principle—linkage—that had previously been presented as support for the chromosome theory. Consequently, the 1950 text created a dramatically different hierarchical ordering of evidence and principles, which in turn conveyed a very different picture of genetics. The progression of topics—from the nature of the genes, to their place in the chromosome, to their linear order and ability to cross-over—formed a series of chapters about the materiality of the genes and their behavior. According to the authors' explanation of these changes in the preface, the textbook no longer presented the chromosome mechanism for its own sake, but rather discussed it in relation to the "genetical system."[127]

Although it has not been feasible to include other textbooks in this detailed comparative study of the four editions of *Principles*, a brief survey of other widely used textbooks in this period suggests that many of the changes I have identified corresponded with broader trends in the teaching of genetics. The increased focus on the materiality of the gene, for example, appeared in numerous textbooks. In the 1948 edition of *Heredity*, for example, Shull wrote: "it is now possible to look in a microscope…and point almost exactly to the spot where something lies that is responsible for an eye color, a wing shape, or the number of joints in a leg."[128] Others, such as King, followed Sinnott and Dunn and included detachable foldout posters of Bridges' maps of *Drosophila's* giant

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[127] Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 4th ed., ix.

[128] Shull, *Heredity*, 4th ed., 9. See also page 33.

salivary gland chromosomes.[129] In the preface to the 1951 edition of *Principles of Heredity*, Snyder drew attention to the new chapter on the physiology of the gene, and explained that related aspects of the 1930s language of the first edition had been replaced: "The term 'factor,' which was used synonymously with 'gene' in the earlier editions, has been replaced with the more exact term gene, and thus the concept of gene is presented to the student from the very beginning."[130] And, as in *Principles*, knowledge of the materiality of the gene was presented as being knowledge of the cause of hereditary phenomena: "Mendel's laws are essentially a description of *what* happens when we make various kinds of crosses or matings. Having found this out the next question we are likely to ask is *how* or *why* it happens."[131]

In general, my analysis in this section has tried to show how the significance of evidence and principles was dependent upon the ways in which they were used. As new questions and fields of study emerged between 1925 and 1950, the experiments and facts that were originally presented in support of the chromosome theory were recontextualized. Within the "historical" framework of the textbook, chapters and their contents were reorganized, substantially altering the meaning of a body of text that had changed relatively little on the whole. Although this case study has focused on changes in presentations of evidence for the chromosome theory of heredity, this is just one of many topics that were shaped into different conceptual orders. The study of quantitative inheritance, for example, did not have a standardized place in presentations of genetics: textbooks of the 1950s-1960s were often divided into three general sections—on Mendelian, chromosomal, and population genetics—and different authors placed quantitative inheritance in different sections by emphasizing different aspects of the study of this type of heredity.[132] Gardner and many of his contemporaries presented quantitative inheritance as an aspect of Mendelism by focusing on the quantitative traits

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[129] King, *Genetics*, 1st ed., 132-133. A non-detachable foldout map was included in Sturtevant and Beadle, *An Introduction to Genetics*, Figure 48 (between pages 130 and 131).

[130] Snyder, *The Principles of Heredity*, 4th ed., vi (emphasis original). This increased focus on the gene in genetics seems to have had a parallel in physics, in which textbooks came to be organized around "force functions." For a discussion of the organizing logic of physics textbooks, see Chapter 3 in R. N. Giere, *Explaining Science* (Chicago: University of Chicago Press, 1988), esp. 66., where he explains: "it is *force functions* that provide the chief organizing principle in most classical mechanics textbooks."

[131] Snyder and David, *The Principles of Heredity*, 5th ed., 36.

[132] Note that I will return to discuss the division of textbooks into sections on Mendelian, chromosomal, and population genetics in Chapter 4.

that formed modified Mendelian ratios.[133] Snyder and David presented it in the section on chromosomal genetics by drawing connections between the statistical study of multiple genes and the cytogenetic study of multiple alleles.[134] Srb, Owen, and Edgar presented it in the group of chapters on population genetics, where they connected it by focusing on the biometrical tools that were used to study both quantitative traits and the movement of genes in populations.[135] And Sinnott and Dunn, after moving it from the section on population genetics in the second edition to the section on Mendelian genetics in the third, made it an independent chapter at the end of the textbook in the fifth edition that was meant to be studied alongside the numerous chapters containing statistical problems.[136] Thus, it was not just the significance of evidence that was highly malleable in the hands of textbook authors, but also that of entire types of heredity and experimental methods. Throughout the textbook, in decisions about how to organize the subject matter, authors shaped the conceptual order of the science.

Conclusion

My study of the development of the conceptual order of genetics began with a historical investigation of two competing ways in which textbook authors organized their subject matter. Exploring the reception of the first generation textbooks by Bateson and Thomson revealed that the differences between these approaches were not, at first, conceived in primarily pedagogical terms. Book reviews, which provided detailed accounts of the ordering of subject matter, presented the two approaches as though they were connected to two very different pictures of the nature of hereditary science: Bateson built the science on heredity's laws, Thomson on heredity's ultimate physical units. Although substantive discussions of these differences became increasingly rare after the 1910s, I have argued that the underlying disagreements were not resolved but rather translated into discussions of pedagogical utility. The controversial features of

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[133] See, e.g., Gardner, *Principles*, 1st ed., Chapter 6; Colin, *Elements of Genetics*, 3rd ed., Chapter 6; Dodson, *Genetics*, Chapter 10; King, *Genetics*, 1st ed., Chapter 7; and Singleton, *Elementary Genetics*, Chapter 11.

[134] Snyder and David, *The Principles of Heredity*, 5th ed., Chapters 14, 15.

[135] Srb, Owen, and Edgar, *General Genetics*, 2nd ed., Chapter 14.

[136] Sinnott and Dunn, *Principles of Genetics*, 2nd ed., Chapter 11; 3rd ed., Chapter 6; and Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 5th ed., Chapter 29. The decision to make it an independent chapter is discussed on page vii of the fifth edition.

Bateson's and Thomson's conceptions of hereditary science did not disappear with this reduction, but rather were embedded and unacknowledged in later uses of these two organizational forms. Thus, the growth and eventual ubiquity of the historical approach was not only a pedagogically significant development. This norm brought about a change in geneticists' ideas of "the logical," and made Mendel's laws the general foundation of the conceptual order of the science.

The use of Mendel's laws as the basis for the "historical" presentation of the principles of genetics was often achieved with what I have identified as a "dialectical" narrative structure. Evidence that at first seemed to require a rejection of the laws was shown, chapter-by-chapter, to merely require the extension of them. In each chapter, the primary character of Mendelian phenomena was redefined in terms that created a link between them and the apparent exceptions: mathematical, methodological, and mechanistic points of contact provided the basis for a presentation of reconciliation. The steps of the narrative thus relied on different rhetorical devices. Throughout all of the steps, however, Mendel's laws were treated as though they were the manifestations of more fundamental ideas. The exceptions were not reconciled with extensions of Mendel's articulation of his principles, but rather with extensions of a conception of the principles. In this manner, Mendel's principles were imbued with meaning that they did not have in the context of his experiments, which was the context in which the textbook had initially identified and explained them. Thus, the significance of the principles shifted as the student read the text, just as they had shifted through the historical development of the discipline. A recapitulation of the past was thereby embedded in the narrative structure of the textbook, perhaps even more deeply than was intended. The step-by-step reconciliation of seemingly problematic evidence also created a picture in which past progress in theory had been driven by the accumulation of evidence as presented in the text—a positivistic picture that was further supported by the exclusion of failed theories from the presentation of these steps. The textbook connected principles that had been independently developed to deal with evidence produced by relatively unrelated lines of research, unifying Morgan's principles under those of Mendel and creating an image of genetics as a rationally organized body of knowledge.

At the same time that the dialectical historical narrative presented disciplinary coherence, it provided a framework for the conceptual order of the science. Despite the nominally "historical" approach, the hierarchical organization of facts and theories was not entirely determined by the order of their historical discovery; nor was it determined by an internal logic. Rather, the hierarchy was malleable and shaped by individual geneticists. This type of flexibility was acknowledged by authors such as Dunn, whose history of genetics highlighted the influence of Johannsen as an "orderer of ideas" and a "systematizer of thought in genetics," and discussed the importance of his work and that of others who had brought disparate aspects of practice and theory together into a cohesive discipline.[137] Although Dunn's list of influential orderers of ideas only identified individuals who made significant contributions to theory, I have suggested that the authors of textbooks were equally influential in this respect. Because textbooks are one of the few places that the heterogeneity of practice and theory are brought together to be presented as a cohesive discipline, the conceptual order of the science is particularly malleable in the hands of textbook authors. Slight changes in the organization of dogma contribute to very different pictures of the discipline.

In this chapter I have argued that history, as a logic of organization, came to function as disciplinary frame in genetics textbooks, holding disparate facts and theories in their proper places and presenting the student with a coherent and unified science. In making this argument, I have tried to illustrate the importance of a type of history-writing that is not immediately apparent in the words of the text, but rather is embedded and partly hidden in its structure. The historicity of this "history" differed significantly from that of the historical introductions and historical exemplars, as did the features of the science it presented. The conceptual order and rational structure of the science of genetics were not identified with statements about the past, but rather were conveyed through a feature of the textbook that existed only in the dialectical narrative holding together its cumulative chapters. Thus, history was constitutive of the organizational logic, narrative structure, and inner rationality of the science presented; it was, in another word, the *logos* of the textbook.

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[137] Dunn, *A Short History of Genetics*, 211.

CHAPTER 4

Senses of History:
The Conception of "Classical" Genetics

> We need not be too concerned lest "classical genetics" of the text-books be found wanting…It is a long time since any geneticist put his whole conception of genetic processes into a text-book.
>
> A. F. Shull, in "Two Decades of Evolutionary Theory."[1]

> In most textbooks of genetics, ideas are not always, or even often, ascribed to those who first presented or tested them.
>
> L. C. Dunn, in *A Short History of Genetics*.[2]

Although the term "classical" is often used unreflectively, studies of its myriad senses have suggested that it does not function as a simple description, but rather as a powerful and complex idea in need of analysis. The epochal "classical" that we project onto an era, for example, has been identified as a concept in tension—essentially static in that it designates a period that is by definition over, and necessarily changing in that it cannot have any sense or function without the dynamism of nostalgia or repetition.[3] The elevation of a text to the status of "a classic" has been shown to efface the situation in which it was created, endowing it with a degree of authority that allows the reader to

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[1] A. F. Shull, "Two Decades of Evolution Theory," *The American Naturalist* 76 (1942): 176.

[2] L. C. Dunn, *A Short History of Genetics: The Development of Some of the Main Lines of Thought, 1864-1939* (New York: McGraw-Hill, 1965), xii.

[3] S. Settis, *The Future of the 'Classical'* (Cambridge, England: Polity Press, 2006), 16. This "timeless" aspect of the concept of the classical has been analyzed in very different ways in H. G. Gadamer, *Truth and Method*, trans. J. Weinsheimer and D. Marshall (New York: Crossroad, 1989), 285-290, and in N. Crowe, "Timelessness and the Idea of the Classical," in *Nature and the Idea of a Man-Made World* (Cambridge: MIT Press, 1999).

forget the texts that came before it.[4] Various mechanisms of classicism—retrospective and prospective, and transferential and regressive—have been identified in references to "classical antiquity."[5] The aesthetic of "classicism" in art has been characterized as the naturalization of a social source of authority with which power is asserted.[6] The idea of "musical classics" that developed in eighteenth century England has been connected to the Tory-Whig political conflict and the social reformation of manners, and thus recognized as an aspect of the English elite's efforts to exert stability and maintain privilege during a period of rapid change.[7] Physicists' uses of the "classical" at the turn of the twentieth century have been analyzed as features of the worldview arguments with which "modern" physics was fashioned.[8] Thus, "classical" should be seen as a term that exerts force and in a variety of fields, and its use should raise questions.

In the 1970s, students of genetics, like students of physics, learned to conceptualize their science in terms of its classical and modern eras. With this differentiation of the classical from the modern, a sense of the past played a key role in the disciplining of the science. The idea of the classical allowed for the presentation of molecular genetics as a revolution in geneticists' understanding of heredity. But, the idea of "classical genetics" was not merely an anachronistic description projected by molecular geneticists onto the past. The use of the term in the 1970s was, rather, just one redefinition in a series of reappropriations of an idea that had developed over five decades, in which "classical" had been used by geneticists to refer to a type of past work, an ideology, a philosophy of science, a type of genetics, and a historical era. In this chapter, I will explore how the sense of disciplinary history embodied in references to the classical was conceived and conceptualized, from the first references to "classical cases" in the 1920s, to the disciplinary deployments of "classical genetics" fifty years later.

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[4] J. A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation* (Chicago: University of Chicago Press, 2003), see esp. 515-518.

[5] J. Porter, "What Is "Classical" About Classical Antiquity," in *Classical Pasts: The Classical Traditions of Greece and Rome*, ed. J. Porter (Princeton: Princeton University Press, 2005), see esp. 62.

[6] H. Zerner, "Classicism as Power," *Art Journal* 47 (1988).

[7] W. Weber, *The Rise of Musical Classics in Eighteenth-Century England: A Study in Canon, Ritual, and Ideology* (New York: Oxford University Press, 1992). The analysis of the concept of classical music by L. Finscher, "Zum Begriff der Klassik in der Musik," *Deutsches Jahrbuch der Musikwissenschaft* 11 (1966), is considered to be a key contribution to this subject.

[8] R. Staley, "On the Co-Creation of Classical and Modern Physics," *Isis* 96 (2005).

A Reference to Past Work: The Identification of Exemplary Cases and Theories

Some of the first uses of the word "classical" in the literature of genetics were references to problems that pre-dated Mendelian studies of heredity.[9] For example: when researchers at the Berkeley Division of Genetics discussed their studies of a non-Mendelian phenomenon, they did so by referring to a "classical" case of extra-gametic hereditary transmission: "The classical instance of reversion of the nectarine back to the peach can be clearly explained, as East (1921) has suggested, on the assumption that nectarines subject to this phenomenon are simply periclinal chimeras."[10] Although this article did not identify what made the case "classical," East's article had explained the conventional wisdom: Darwin had "fathered" the idea that some bud variations are not transmitted by seed and, "The usual citation is the nectarine."[11] In this sense, "classical" identified the fact that a particular phenomenon was well known, pre-dating the twentieth century study of genetics.

More often in the 1910s-1920s genetics literature, "classical" was used to identify, or to define, the paradigmatic cases of a hereditary phenomenon for which there was a generally accepted "Mendelian" explanation. A phenotypic ratio of 1:2:1 in the F_2 generation, for example, was associated with a well-established "classical" case discovered by Bateson: "Most students of 'Mendelism' would probably hold the opinion that little remains to be added to our knowledge of the classical case of blue Andalusian fowl."[12] Lethality was regularly identified with Lucien Cuénot's yellow mice, as in a

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[9] Note that throughout this chapter, my claims about first references and general trends in the use of "classical" as a description of genetics have been developed through extensive readings of primary and secondary sources. Additional support has been provided by electronic searches for the term "classical" and all of its variants (e.g., classic, classics) and typographical forms (e.g., classic-cal) in every article that was published between 1916-1970 (as well as some that were published earlier and later) in: *American Journal of Botany*, *American Midland Naturalist*, *American Naturalist*, *Annals of the Missouri Botanical Garden*, *Botanical Gazette*, *Bulletin of the Torrey Botanical Club*, *Genetics*, *Journal of Ecology*, *Journal of Heredity*, *Missouri Botanical Garden Annual Report*, *New Phytologist*, *Philosophical Transactions of the Royal Society of London: Series B*, *Proceedings of the Royal Society of London: Series B*, *Proceedings of the National Academy of Sciences of the United States of America*, *Quarterly Review of Biology*, *Science*, and *Scientific Monthly*. For journals that are not electronically archived (e.g., *Nature* pre-1950), I have read the relevant published indexes.

[10] R. E. Clausen and T. H. Goodspeed, "Inheritance in Nicotiana Tabacum. III. The Occurrence of Two Natural Periclinal Chimeras," *Genetics* 8 (1923): 104. NB: The article by East to which they refer was actually published in 1917; the title, journal and volume information are correct.

[11] E. M. East, "The Bearing of Some General Biological Facts on Bud-Variation," *The American Naturalist* 51 (1917): 143.

[12] G. H. Carpenter, "Heredity and Evolution," *Nature* 104 (1919): 81.

1914 study of linkage in *Drosophila*: "The classic example of this sort of effect is that of the yellow mice."[13] Gene interaction producing a 9:3:3:1 ratio was frequently identified with Bateson and Punnett's heterozygous walnut-combed chicken. In a study of interaction in flower color, for example: "Among animals a classical case of a compound character and of spectacular factor interactions is that of the walnut comb in fowls."[14] And mutation was generally seen in terms of Hugo De Vries' work on species of *Oenothera*, as in an article on mass mutation in *Botanical Gazette*: "The type of heredity here exemplified is shown by several mutations from *O. Lamarckiana*. *O. lata* De Vries provides the classic case."[15] In addition to these European examples, hereditary mechanisms discovered by chief advocates of Mendelism in the United States were also described as classical. For example, in reporting on a study of *O. Lamarckiana* in 1922, De Vries wrote that he had "tried to arrange the mutant types into distinct groups, analogous to those proposed by Morgan," describing Morgan's experiments as "his classical researches on *Drosophila*."[16] In these publications, the cases identified as "classical" were often those that had allowed for the original identification of a genetic principle or mechanism, highlighting the work of key contributors to the science.[17]

The early uses of "classical" in the literature of genetics served an important scientific function. In projecting "classical" onto the paradigmatic cases of genetically explicable hereditary phenomena, geneticists not only pictured them as models, but also treated them as exemplars of a *type* of hereditary phenomena; they were the foundation for reasoning by analogy. For example: the case of the walnut comb in fowl—which was first detailed in articles and textbooks by Bateson and Punnett, and was subsequently

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[13] J. S. Dexter, "The Analysis of a Case of Continuous Variation in Drosophila by a Study of Its Linkage Relations," *The American Naturalist* 48 (1914): 723. See also, e.g., L. R. Waldron, "A Study of Dwarfness in Wheat Accompanied by Unexpected Ratios," *Genetics* 9 (1924): 214; and S. Wright, "Systems of Mating. IV. The Effects of Selection," *Genetics* 6 (1921): 163.

[14] J. P. Kelly, "A Genetical Study of Flower Form and Flower Color in Phlox Drummondii," *Genetics* 5 (1920): 226. This was also the standard example in textbooks of the time. See, e.g., E. W. Sinnott and L. C. Dunn, *Principles of Genetics: An Elementary Text, with Problems*, 1st ed. (New York: McGraw-Hill, 1925), 77.

[15] H. H. Bartlett, "Mass Mutation in Oenothera Pratincola," *Botanical Gazette* 60 (1915): 442.

[16] H. De Vries and K. Boedijn, "On the Distribution of Mutant Characters among the Chromosomes of Oenothera Lamarckiana," *Genetics* 8 (1923): 233, 236.

[17] In the 1930s, Emerson's work on plant colors in maize was called classical, as was Wilson's work on the variable number of chromosomes in a species. See, e.g., respectively, M. M. Rhoades, "Effect of the Dt Gene on the Mutability of the A1 Allele in Maize," *Genetics* 23 (1938): 377; and Th. Dobzhansky, "The Y Chromosome of Drosophila Pseudoobscura," *Genetics* 20 (1935): 373. See also W. Bateson, "The Progress of Mendelism," *Nature* 104 (1919): 214.

included in most textbooks by their colleagues and the future generations of geneticists—provided the basis for thinking about other cases of gene interaction. In an analysis of white-margined flowers in the Japanese morning-glory published in *Genetics*, for example, the author noted that Bateson and Punnett's "classical experiment" had guided the discovery of a "number of similar instances."[18] The work of Morgan's lab received similar treatment in academic publications. The linkage groups in *Drosophila*, for example, provided a model that other geneticists used to interpret and describe their work: "The summaries…suggest that linkage group organization in the tomato resembles closely the classical *Drosophila* picture."[19] In these uses, "classical" provided a point of reference by which to categorize and discuss current research. The classical cases were not only scientific exemplars, but also the basis for categories of family resemblance.[20] When Dobzhansky later discussed whether the segregation, recombination, linkage, chromosome maps, non-disjunction, and translocations in bacterial genetics were "similar to those discovered by classical genetics," he pictured the principles of genetics in terms of "their *Drosophila* prototypes."[21]

In the 1930s, the types of things identified as "classical" expanded, as geneticists began to use the term to refer to theories, as well as cases. East frequently referred to Mendelian dominance "in the classical sense."[22] The cellular mechanism that caused Mendelian segregation was said to be explained "either on the classical theory of separation of pairs of chromatids…or on the chiasmatype theory."[23] Dobzhansky described Morgan's principle of the limitation of the linkage groups as "the classical assumption, according to which each linkage group corresponds to a separate

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[18] Y. Imai, "A Genetic Analysis of White-Margined Flowers in the Japanese Morning-Glory," *Genetics* 12 (1927): 203.

[19] J. W. Macarthur, "Linkage Studies with the Tomato II: Three Linkage Groups," *Genetics* 13 (1928): 415.

[20] See also, e.g., "The similarity of these described series to the classical allelic gene series of Drosophila eye color, rodent coat color, and anthocyanin color in maize or primula is striking," in R. E. Lincoln and J. W. Gowen, "Mutation of Phytomonas Stewartii by X-Ray Irradiation," *Genetics* 27 (1942): 460; and Waldron, "A Study of Dwarfness in Wheat Accompanied by Unexpected Ratios," 214, in which "the classical case of the yellow coat color of mice" is used to explain unexpected experimental results.

[21] Th. Dobzhansky, "The Theory of the Gene," *American Naturalist* 87 (1953): 123.

[22] See, e.g., E. M. East, "Heterosis," *Genetics* 21 (1936): 392.

[23] D. G. Catcheside, "Critical Evidence of Parasynapsis in Oenothera," *Proceedings of the Royal Society of London. Series B, Biological Sciences* 109 (1931): 181.

chromosome."[24] And Morgan referred to Bridges' explanation of irregular segregation, which provided the basis of the chromosome theory, as the "classical theory of secondary non-disjunction."[25] In addition, the experimental organisms on which these theories were based were often characterized as "classical" in journals, with claims such as: "For many years *Drosophila melanogaster* has been a classical object for genetic studies."[26] In his 1935 textbook *Genetics*, Jennings agreed: "*Drosophila*…is in many respects the classic organism for genetics."[27] And while I will return to this point later in the chapter, it is perhaps worth noting here that *Drosophila* experiments would later be used to define the classical era of genetics.[28]

In summary, the rapid growth in genetics in the interwar period had brought about changes in geneticists' sense of the historicity of their science. One of the first uses of "classical" in the literature of genetics had referred to a problem discovered by Darwin, which had provided a point of reference in East's 1916 studies of bud variation. By the 1930s, however, such pre-Mendelian cases of heredity had been eclipsed by those of the Mendelian era; it was, for example, East's 1916 studies that were being referred to as "classical work on quantitative characters" and "classical experiments," providing an exemplar and reference point for research in the field.[29] In the intervening years, the nature of that which was identified as classical had shifted—from individual cases of hereditary phenomena, to cases that were taken to represent types of phenomena, to theoretical accounts of these phenomena. Insofar as these uses of "classical" identified past problems, experiments, and theories, they participated in the characterization of the study of genetics as a science with a history. Unlike "Mendelian," which was a characterization of the hereditary phenomena or mechanism, "classical" was a

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[24] Th. Dobzhansky, "Translocations Involving the Third and the Fourth Chromosomes of Drosophila Melanogaster," *Genetics* 15 (1930): 385.

[25] L. V. Morgan, "Effects of a Compound Duplication of the X Chromosome of Drosophila Melanogaster," *Genetics* 23 (1938): 434.

[26] T. S. Painter, "A New Method for the Study of Chromosome Aberrations and the Plotting of Chromosome Maps in Drosophila Melanogaster," *Genetics* 19 (1934): 175. See also the references to "classical material" and "the classic genera *Oenothera* and *Drosophila*" in, respectively, H. M. Showalter, "Self Flower-Color Inheritance and Mutation in Mirabilis Jalapa L," *Genetics* 19 (1934): 568; and R. R. Gates, "International Genetics," *Nature* 124 (1929): 296.

[27] H. S. Jennings, *Genetics* (New York: W. W. Norton, 1935), 36; see also 47-48.

[28] See, e.g., J. Schultz, "Innovators and Controversies," Review of *The Gene: A Critical History*, by E. A. Carlson, *Science* 157 (1967): 298.

[29] See, e.g., E. Anderson, "Recombination in Species Crosses," *Genetics* 24 (1939): 670, 674.

description of the importance of an event in the history of the study of heredity. In the early twentieth century, the use of "classical" by geneticists was similar to that of mathematicians, physicists, botanists, physiologists, and chemists.[30] The role of these claims in the developing and malleable discipline of genetics, however, was particularly significant; for it was with this type of language that the conceptual and institutional boundaries of the science began to be defined. To the extent that the science was defined by its exemplars—by the experiments and theories that were taken to be classics—the language of the classical played a key role in its disciplining. And although the "classics" of genetics were generally identified without political intent into the early 1930s, it was not long before the political character of this language became apparent, in a shift that coincided with the emergence of "classical genetics."

A Formalistic Idealization: The Attack from Dialectical Genetics

It was in the mid to late 1930s—in reports of T. D. Lysenko's attempts to suppress the research of geneticists working in the Mendelian tradition in Russia—that "classical genetics" first entered the academic literature and public presentations of the science of genetics. The origins of this development can be traced to December 14, 1936, when *The New York Times* reported on the cancellation of the seventh international congress on genetics in Russia, with a headline proclaiming, "Prof. N. I. Vaviloff, a Famous Plant Expert, Is Arrested—Others Under Attack." The article explained:

> In the past three months T. D. Lysenko…has been attacking the "classical geneticists" in the monthly scientific magazine, *Socialist Reconstruction of Agriculture.* He challenged the validity of classical genetics, including

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[30] See, e.g., "the classical example" and "a classical example" in L. Berman, "Anthropology and the Endocrine Glands," *The Scientific Monthly* 21 (1925): 165; G. R. Bisby, "Some Observations on the Formation of the Capillitium and the Development of Physarella Mirabilis Peck and Stemonitis Fusca Roth," *American Journal of Botany* 1 (1914): 279; A. F. Blakeslee, "The Botanic Garden as a Field Museum of Agriculture," *Science* 31 (1910): 55; C. H. Browning and R. Gulbransen, "Bactericidal Properties Conferred on the Blood by Intravenous Injections of Diamino-Acridine Sulphate," *Proceedings of the Royal Society of London. Series B, Containing Papers of a Biological Character* 90 (1918): 136; C. M. Jackson, "Obstacles to Research," *Science* 42 (1915): 820; W. Libby, "Conceptual Thinking," *The Scientific Monthly* 15 (1922): 440; T. R. Merton and J. W. Nicholson, "On Intensity Relations in the Spectrum of Helium," *Philosophical Transactions of the Royal Society of London. Series A, Containing Papers of a Mathematical or Physical Character* 220 (1920): 161, 168; and H. S. Taylor and W. N. Henderson, "The Solubility of Curves of Salt Hydrates: Calcium Nitrate," *Journal of the American Chemical Society* 37 (1915): 1688. A brief search of non-scientific journals finds similar uses of "classical examples," as well as the use of very different senses of the term (e.g., "classical" as a reference to timeless values, an aesthetic form, a pedagogical cannon, and a historical era).

> the Mendelian laws and the chromosome theory and stigmatized them as "formalistic" and of no practical value, whereas his work, he said, is producing useful results. Mr. Lysenko said, "Genetics is merely an amusement, like chess or football," and he attacked the All-Union Institute of Plant Industry…as useless.[31]

This report was immediately reprinted in *Science* and translated into Russian by the Moscow newspaper *Izvestia*, sparking numerous replies from Russian geneticists defending Soviet science, which were then published in *Science* and *Journal of Heredity*, as were counter-replies by American geneticists.[32] It was in the context of this controversy that "classical genetics" entered into *Science* and *Journal of Heredity* for the first time. And within days the controversy was furthered fueled with the publication of another article in *The New York Times*, from their correspondent in Moscow:

> Classical genetics came under heavy fire at the conference of the Agricultural Academy of Genetics, which closed here tonight…Professor Lysenko, as the leader of the "practical" school of selectionists, criticized Professor Vaviloff as the leader of the classical—or, as his opponents say, "formalistic"—school for failing to take into account a vast amount of experimental data on the production of new varieties of plants through inter-variety crossings on 15,000 collective farms.[33]

While these reports gave geneticists little information about the specifics of Lysenko's attack, *Journal of Heredity* quickly published a few articles intended to shed some light on the issue.[34] In an article that discussed "Iarovization vs. 'Classical Genetics,'" A. J. Bruman of the U. S. Department of Agriculture suggested that some aspects of

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[31] "Moscow Cancels Genetics Parley," *New York Times*, December 14 1936, 18. This article first ran in *The New York Times* as a wireless transmission from a correspondent in Moscow and was then reproduced in *Science* as "Abandonment of the Moscow Meeting of the International Congress of Genetics," *Science* 84 (1936): 553-554. This article and events were also discussed in "News and Views: International Congress of Genetics and the U.S.S.R.," *Nature* 139 (1937): 142, although without the phrase "classical genetics."

[32] These first reports of the "Lysenko Affair" in *The New York Times* and the subsequent controversy were widely discussed in the literature of the natural, social, and political sciences. See, e.g., R. C., "The Genetics Congress," *Journal of Heredity* 28 (1937): 24-25; R. C. Cook, "Lysenko's Marxist Genetics: Science or Religion?" *Journal of Heredity* 40 (1949): 169-170; P. E. Mosely, "Freedom of Artistic Expression and Scientific Inquiry in Russia," *The Annals of the American Academy of Political and Social Science* 200 (1938); J. W. Pincus, "The 'Genetic Furore' in U.S.S.R.," *Journal of Heredity* 28 (1937): 57; J. W. Pincus, "The Genetic Front in the U.S.S.R," *Journal of Heredity* 31 (1940): 165; and C. Zirkle, ed., *Death of a Science in Russia* (Philadelphia: University of Pennsylvania Press, 1949), 47-48. For the later historiography of the "Lysenko Affair," see Z. Medvedev, *The Rise and Fall of T. D. Lysenko* (New York: Columbia University Press, 1969); D. Joravsky, *The Lysenko Affair* (Cambridge: Harvard University Press, 1970); and N. Roll-Hansen, *The Lysenko Effect: The Politics of Science* (Amherst: Humanity Books, 2005).

[33] H. Denny, "Geneticists Argue Work in Moscow," *New York Times*, December 28 1936, 18.

[34] See, e.g., A. J. Bruman, "The Place of Iarovization in Plant Breeding," *Journal of Heredity* 28 (1937); and C., "The Genetics Congress." The lack of scientific publications by Lysenko was also noted in "Genetics and Plant Breeding in the U.S.S.R.," *Nature* 140 (1937): 296.

Lysenko's work were "grasping in what may prove to be a field of some promise."[35] An Editor's Note that prefaced this article explained that it was submitted a week before the cancellation of the Seventh International Congress, and described Bruman's analysis as "invaluable" in helping the reader "understand the underlying motivation, which has led the 'iarovization geneticists' to brand 'classical genetics' as 'a game like chess or football'." The editor noted: "It is left to the reader to reach his own conclusion as to the point at which such exact scientific measures as probable errors leave off, and where such intangibles as 'Marxian ideology' begin to change the writings of the new iarovization school of genetics from true science to political special pleading."[36]

It is notable that Anglo-American genetics used Lysenko's terminology when referring to the two sides involved in this controversy. In the *Quarterly Review of Biology*, for example, Bentley Glass used Lysenko's phrase "Mendelian-Morganian geneticists" as a way of talking about his own scientific community: "In the second Russian conference on the genetics controversy, held in 1936 (the first was in 1929), the Mendelian-Morganian geneticists lost the day…In 1939, a third genetics conference met, and charges against the 'classical' geneticists were repeated."[37] It was not until the Marxist journal *Science and Society* published some of the proceedings from the 1939 Conference on Genetics and Selection, however, that substantive versions of the arguments made by Lysenko and his colleagues were available in English.[38] The editors of *Science and Society* explained their choice of three addresses, out of the fifty-three printed in the Russian journal *Pod Znamenem Marksizma* (1939, no. 11): "Fifty-three

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[35] Note that in the 1930s, the term "iarovization," also referred to as "vernalization," was used to refer to the manipulation of biological processes through external factors; it later became associated with Lysenko's Lamarckian theory of acquired inheritance.

[36] Bruman, "The Place of Iarovization in Plant Breeding," 31.

[37] B. Glass, "Dialectical Materialism and Scientific Research," Review of *The New Genetics in the Soviet Union*, by P. S. Hudson and R. H. Richens, *Quarterly Review of Biology* 23 (1948): 333-34. Conway Zirkle's account of Soviet "verbalism," written to help American genetics reading Lysenkoists' arguments in *Death of A Science in Russia*, is useful here. Noting that doctrines, theories, and hypotheses were personalized, Zirkle explained: "Thus, the Russian universities do not have professors of evolution but professors of Darwinism. Genetics is rarely mentioned…but instead such terms as "Mendelism," "Morganism," and "Michurinism" are current." According to Zirkle: "This personalizing is much more than the mere attaching of personal labels to impersonal ideas. The ideas themselves are distorted." He noted that while "Mendelism-Morganism" received "great condemnation" in the late 1930s, it was largely replaced by "Morganism-Weismannism" in the late 1940s. See Zirkle, ed., *Death of a Science in Russia*, 8-11.

[38] These publications in *Science and Society* were later discussed in Zirkle, ed., *Death of a Science in Russia*, 34-35.

scientists presented their positions at the open meetings of the Conference, but the principal disputants, representing widely different views on theory and methods in genetics, were Vavilov and Lysenko…Because of their prominence and leading roles in the controversy, the speeches of Vavilov and Lysenko are here presented almost in full, together with the address of I. M. Polyakov who takes perhaps the most interesting intermediate position."[39]

In Lysenko's address, he set out to respond to the Russian geneticists who were critical of his rejection of the Mendelian tradition, as he explained in the opening: "the present conference wants to hear from me, principally, why I reject Mendelism, why I do not consider formal Mendelian-Morgan genetics a science." Lysenko answered these questions with many "illustrations," explaining his break with the "Mendelian-Morganists" as a disagreement about what should be counted as exemplary work in the study of heredity—a disagreement about the "classics." For example: "The Mendelians have continually blamed us (and still do), for not appreciating the teachings of Johannsen, and for taking a critical attitude toward this 'classic' of biological science."[40] Here, and elsewhere in his speech, Lysenko characterized his conflict with the West as rooted in fundamentally different ways of choosing scientific exemplars, and therein sources of authority. He stated: "One has to know how to choose authorities in science. Only that theory which helps you in practical solution of problems undertaken or assigned, earns the right to scientific labors."[41] Lysenko's criticism of Johannsen's work was part of his attempt to redefine the classics of Soviet genetics. According to N. I. Vavilov, a well known advocate of Mendelism, Lysenko was responsible for the growing belief "that it is not only necessary to remove Mendel from the list of classics, but also to regard his work as full of harmful generalizations."[42] Lysenko discussed this point at length in his address, and it was in his introduction to this discussion that he used the phrase "classical genetics," describing a source of error. He stated: "Let us take up

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[39] "Genetics in the Soviet Union: Three Speeches from the 1939 Conference on Genetics and Selection," *Science and Society* IV (1940): 183. NB: "Vaviloff," mentioned in *The New York Times*, was also spelled "Vavilov."

[40] T. D. Lysenko, "Genetics in the Soviet Union: Three Speeches from the 1939 Conference on Genetics and Selection," *Science and Society* IV (1940): 199.

[41] Ibid.: 217.

[42] N. I. Vavilov, "Genetics in the Soviet Union: Three Speeches from the 1939 Conference on Genetics and Selection," *Science and Society* IV (1940): 190.

another example of our ‘attacks’ on ‘classical genetics.’ The question is the so-called 3:l relation.”[43] This seems to be one of the few times that Lysenko used the term “classical genetics” in an English publication, and it is difficult to know the meaning of the inverted commas.[44] But what is important is that Anglo-American geneticists did see the term as a criticism used by Lysenko, as it was in Polyakov’s defense of Lysenko. Polyakov discussed “the issues on which classical genetics dogmatized, and hastened to declare to be forbidden ground,” such as directed mutation; he noted “the conjecture” on the topic made “by classical geneticists” before Morgan “clamped down” on them and “buried” the topic.[45] In referring to “Mendelian-Morganism” as “classical genetics,” Lysenko and his supporters not only suggested that the science and its power structure were constituted in the classics it had chosen. In addition, they implied that “Mendelism-Morganism” was an artifact of the culture of the West—that the science was no more than a cultural tradition. It was this aspect of their argument that American geneticist R. Cook rejected in a lengthy article on “Lysenko’s Marxist Genetics,” in which he responded to an article that had been published in the journal *Soviet Russia Today*: “The issue is not whether ‘the *tradition* of Mendel-Morganism’ or the ‘Michurin teaching’ is doctrinally true. This issue is of the facts, which Lysenkoists deny. The principles of ‘Mendel-Morgan genetics’ are not a ‘tradition’.”[46] Muller responded to Lysenko in a similar way, arguing that there was no difference between “formal genetics” and “true genetics.”[47] On this Anglo-American picture, Western science was not one approach amongst many; it was the only scientific approach. The substantive connection between the culture of the West and “classical” genetics, and their superiority over the Soviet alternatives, was later emphasized by Caspari and Marshak, who wrote in an article on

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[43] Lysenko, "Genetics in the Soviet Union: Three Speeches from the 1939 Conference on Genetics and Selection," 203.

[44] There are at least two problems. First, there is not any notation of whether Lysenko indicated them in his address, or of who decided to use them in the transcription. Second, it is not clear whether the language is his, or if he is attributing one or both of the terms to his opponents; the situation is not helped by looking at the language of Anglo-American geneticists, who used inverted commas in a similar manner.

[45] I. M. Polyakov, "Genetics in the Soviet Union: Three Speeches from the 1939 Conference on Genetics and Selection," *Science and Society* IV (1940): 224.

[46] Cook, "Lysenko's Marxist Genetics: Science or Religion?" 196 (emphasis original).

[47] Muller’s response to Lysenko was reported in Denny, "Geneticists Argue Work in Moscow," 2.

the rise and fall of Lysenko: “the slow ‘Western’ method, based on quantitative classical genetics, is superior.”[48]

Anglo-American geneticists’ understanding of the Lysenkoists’ attack on “classical genetics” was also informed by the English translation of the 1948 *Proceedings of the Lenin Academy of the U.S.S.R.*, and by the numerous subsequent reprintings of Lysenko’s address to this session.[49] At this session of the Lenin Academy, Mendelism-Morganism was characterized as idealistic and formalistic, in its approach to heredity and thus in the way it represented the processes of life.[50] According to Lysenko’s colleague V. N. Stoletov, the problem with Mendel’s classic work was that his approach “tried to press manifestations of diverse and mobile life into line with the laws of mathematics.”[51] Lysenko agreed that the attempt to “reduce biological science to mere statistics” resulted from the fact that Mendelist-Morganists failed “to grasp the concrete content of biological processes.”[52] As described by his colleague P. F. Plesetsky, “the investigator with such a mathematical approach to the problem loses sight of the biological characteristics of the plant.”[53] The reason that this approach obscured the nature of heredity was that it was not “science,” but rather a form of “extra-experimental speculation.”[54] As articulated by Lysenko: “The scientific basis of studying biological connections is lost when studying living nature detached from nature.”[55] The mathematical “extra-experimental” analysis involved the prioritization of the mental over the material, and thus led to an “idealistic” and “metaphysical” picture of heredity.[56] In the argument advanced by Lysenko and his colleagues, this idealism was intricately connected to the non-pragmatic orientation of the science. Academician E. I. Ushakova,

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[48] E. Caspari and R. E. Marshak, "The Rise and Fall of Lysenko," *Science* 149 (1965): 276.

[49] See, e.g., T. D. Lysenko, "The Situation in Biological Science," in *Death of a Science in Russia*, ed. C. Zirkle (Philadelphia: University of Pennsylvania Press, 1949); and T. D. Lysenko, *Soviet Biology* (London: Birch Books, 1948). For an excellent analysis of these addresses, see P. Wrinch, "Science and Politics in the U.S.S.R.: The Genetics Debate," *World Politics* 3 (1951).

[50] For a discussion of a general Soviet attack on “formalism” in the arts that was written during this period, see Mosely, "Freedom of Artistic Expression and Scientific Inquiry in Russia.”

[51] V. N. Stoletov, as quoted and translated in Wrinch, "Science and Politics in the U.S.S.R.: The Genetics Debate," 502.

[52] *Proceedings of the Lenin Academy of Agricultural Sciences of the U.S.S.R., July 31-August 7, 1948, Complete Stenographic Report* (New York: International Publications, 1949), 614-615.

[53] Ibid., 111.

[54] This usage is discussed in Wrinch, "Science and Politics in the U.S.S.R.: The Genetics Debate," 496.

[55] Lysenko, "The Situation in Biological Science," 131.

[56] *Proceedings of the Lenin Academy of Agricultural Sciences of the U.S.S.R., July 31-August 7, 1948, Complete Stenographic Report*, 21-26.

for example, argued: “Just as it is hard for a normal person to understand and accept the notion that the world we see is not an objective reality existing outside of us…so it is hard to understand how the Morgan-Mendel theories can be applied in practice.”[57] It was research into “useless problems” that led classical geneticists “deeper into the quagmire of formalism,” according to Stoletov; it was “living practice” that was “the enemy of formalism” and its “cure.”[58] A similar distinction was drawn by Lysenko, who praised research that followed the Lamarckian theories of Soviet botanist Ivan Michurin: “It is not an exaggeration to state that the feeble metaphysical Morganist ‘science’ of the nature of living organisms can in no way be compared to our efficient Michurinist agrobiological science.”[59] The reason for this was that they saw pragmatically oriented research as linked to the study of development. Lysenko explained: “*The scientific solution of practical problems is the most reliable method toward a sound knowledge of the rules of development of living nature*.”[60] The study of development was “the point of departure” for all valuable work on heredity, according to Lysenko.[61] Equating “the fundamental concepts of the theory of development” with “the principles of dialectical materialism,” he wrote: “According to dialectics, every identity, every sameness always contains differences. Hence we cannot imagine two offsprings of a plant being in no way different from each other.”[62] Michurinism arose from practical concerns and dialectical materialism, and for this reason reflected the dynamic nature of life; Mendelism-Morganism was based in theory and idealism, and thus misrepresented heredity as formalistic and static.

In short, Lysenko argued that the Mendelian-Morganian research tradition, the tradition of classical genetics, was formalistic in that it was an idealistic and static model of heredity; it failed to capture the dynamic and dialectical nature of development—the

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[57] Ibid., 195.
[58] Ibid., 574.
[59] Lysenko, "The Situation in Biological Science," 105. See also, e.g., the numerous articles discussed by Wrinch, "Science and Politics in the U.S.S.R.: The Genetics Debate," 499.
[60] Lysenko, "The Situation in Biological Science," 131 (emphasis original).
[61] T. D. Lysenko, "Intravarietal Crossing and Mendel's So-Called 'Law' of Segregation," in *Agrobiology: Essays on Problems of Genetics, Plant Breeding and Seed Growing* (Moscow: Foreign Languages Publishing House, 1954), 223. Elsewhere, Lysenko spoke of “formal genetics” as being a body of theory that was in opposition to “the theory of development.” See, e.g., Lysenko, "Genetics in the Soviet Union: Three Speeches from the 1939 Conference on Genetics and Selection."
[62] Ibid.

essential nature of heredity. From this political and ideological critique, "classical genetics" entered into the common language of Anglo-American geneticists, where it was later reappropriated along with other aspects of Lysenko's terminology.[63] Before I turn to explore these reappropriations, however, it is worth noting that the few Anglo-Americans who wrote of "classical genetics" outside of the context of the Lysenko affair in the early 1940s accepted a picture of their discipline as a science founded in static mathematical relationships. For example, G. W. Beadle and E. L. Tatum, who were working on the genetic control of developmental reactions, embraced this picture; as they explained in the introduction to an address on the study of development: "During the last forty years the mechanism of the transmission of hereditary characters has been elucidated by the work collectively referred to as classical genetics. With the gradual unfolding of the rules of this 'geometry of inheritance' the concept of the gene as the heredity unit has taken form."[64] They accepted this idea of the classical gene that emerged from statistical studies, arguing that it was not incapable of dealing with the dynamics of biology: "Just as the organism may be thought of as an integrated whole, so the complement of genes that regulate its development may be thought of as a whole or unit of higher order." According to Beadle and Tatum, this conception of the developmental system did not require "abandoning the gene concept as Goldschmidt has recently argued."[65] Rather, they argued, genes could be reconceptualized as units directing the synthesis of enzymes controlling metabolic processes—a hypothesis that they characterized as "a purely gratuitous assumption," but for which they offered experimental support later that year, in an article that quickly became a widely recognized landmark, providing the foundation for their "one gene – one enzyme" theory of the gene.[66]

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[63] E.g., C. D. Darlington adopted Lysenko's terminology in 1956, referring to the principles of genetics as "the Mendelian-Morganian rules" in his critique of classical genetics: C. D. Darlington, "Natural Populations and the Breakdown of Classical Genetics," *Proceedings of the Royal Society of London. Series B, Biological Sciences* 145 (1956): 355.

[64] G. W. Beadle and E. L. Tatum, "Genetic Control of Developmental Reactions," *The American Naturalist* 75 (1941): 107.

[65] Ibid.: 107-108.

[66] G. W. Beadle and E. L. Tatum, "Genetic Control of Biochemical Reactions in Neurospora," *Proceedings of the National Academy of Sciences of the United States of America* 27 (1941).

A Static Philosophy of Science: The Dynamic Genetics of Richard Goldschmidt

While Anglo-Americans such as Beadle and Tatum found it easy to disregard Lysenko's criticisms of classical genetics as founded in Marxist ideology rather than science, similarly radical and systematic criticisms of "classical" genetics were surfacing from within their own scientific community and academic establishment in the late 1930s.[67] This internal criticism was not new, but rather had developed over the previous three decades—a period in which the study of embryology was effectively split off from the study of heredity, which came to be conceptualized in terms of geneticists' work on variation and mechanics. During these decades, those interested in embryology and development had criticized this narrow line of research, arguing that it contributed to a misconceptualization of the nature of the hereditary material.[68] As Beadle and Tatum mentioned, one of the most vocal critics was Richard Goldschmidt, who had left Germany in 1936 and joined the division of genetics at Berkeley.[69] Goldschmidt's position was, however, very different from that of Lysenko, which he vehemently criticized.[70] Coming from a scientific culture in which physiological and developmental studies were highly valued and research was not only centered on the theories of Mendel and Morgan, Goldschmidt rejected the atomistic picture of the chromosome.[71] While he

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[67] While Sonneborn attempted to address the scientific merits of Lysenko's arguments without being biased by political considerations, he noted that most of his colleagues "maintain that the controversy is not a scientific one at all," in T. M. Sonneborn, "Heredity, Environment, and Politics," *Science* 111 (1950): 529. See also Glass, "Dialectical Materialism and Scientific Research."

[68] G. E. Allen, "Opposition to the Mendelian-Chromosome Theory: The Physiological and Developmental Genetics of Richard Goldschmidt," *Journal of the History of Biology* 7 (1974); G. E. Allen, "T. H. Morgan and the Split between Embryology and Genetics, 1910-1935," in *A History of Embryology*, ed. T. J. Horder, J. A. Witkowski, and C. C. Wylie (New York: Cambridge University Press, 1983); S. F. Gilbert, "Cellular Politics: Ernest Everett Just, Richard B. Goldschmidt, and the Attempt to Reconcile Embryology and Genetics," in *The American Development of Biology*, ed. R. Rainger, K. R. Benson, and J. Maienschein (Philadelphia: University of Pennsylvania Press, 1988); and J. Sapp, "The Struggle for Authority in the Field of Heredity, 1900-1932: New Perspectives on the Rise of Genetics," *Journal of the History of Biology* 16 (1983).

[69] C. D. Darlington was a later critic of "classical genetics." See, e.g., Darlington, "Natural Populations and the Breakdown of Classical Genetics." See also his claims about "classical genetics" and "classical geneticists" in C. D. Darlington, "Control of Evolution in Man," *Nature* 162 (1958): 14, 17; and C. D. Darlington, "Axiom and Process in Genetics," *Nature* 234 (1971): 521.

[70] E.g., Goldschmidt criticized the picture of classical genetics on which Lysenko's criticisms were based: "He does not realize that atomistic classical genetics is extremely materialistic, whilst his own views border on mysticism," in R. B. Goldschmidt, "Research and Politics," *Science* 109 (1949): 223.

[71] On genetics in Germany, see J. Harwood, *Styles of Scientific Thought: The German Genetics Community 1900-1933* (Chicago: University of Chicago Press, 1993). See especially pages 49-98 (Chapter 2), where Harwood discusses developmental genetics and Goldschmidt. For a brief discussion of

articulated this as a rejection of "classical genetics" in the 1950s—arguing that the "tenets of classical genetics" need to be reconceptualized, and identifying hereditary phenomena that "must be understood in terms of a general genetic principle" other than those "underlying classical genetics"—his critique began in the 1930s with an attack on the classical theory of the gene.[72]

In his 1938 "The Theory of the Gene," one of the first pieces to target "the classical," Goldschmidt introduced his position: "What we intend to discuss is not the existing classic theory of the gene, but the reasons why we believe that this classic theory is no longer tenable and has to be superseded."[73] Drawing on the discovery that a gene's expression could be altered by either a change in its position within the chromosome (a position effect) or by crossing-over in an adjacent locus (a spontaneous chromatin rearrangement)—and further developing Muller's proposal that these effects could be understood in terms of chemical interactions among gene products—Goldschmidt proposed a new picture of the nature of hereditary material. Because a gene's location altered its function, it could no longer be seen as a structural unit that carried its function with it, according to Goldschmidt. And thus, the classical picture—which presented the gene as being, simultaneously, a unit of structure, function, mutation, and recombination—misrepresented the nature of the hereditary material. According to Goldschmidt, "the language of classic genetics" made what was actually a chromosomal rearrangement "act" like a mutant gene, and thereby created a false picture of the gene as an independent unit of the chromosome.[74] Speculating that everything that geneticists had identified as gene mutations might actually be position effects, Goldschmidt argued that a system of misleading concepts needed to be collectively dismantled. He suggested

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Goldschmidt's attitude towards the chromosome theory of heredity—which, many fail to note, he supported through the 1920s—see page 42.

[72] R. B. Goldschmidt, "Genic Conversion in Oenothera? A Critical Review," *The American Naturalist* 92 (1958): 97; and R. B. Goldschmidt, "'Repeats' and the Modern Theory of the Gene," *Proceedings of the National Academy of Sciences of the United States of America* 36 (1950): 365.

[73] R. B. Goldschmidt, "The Theory of the Gene," *The Scientific Monthly* 46 (1938). It is notable that Goldschmidt thought that these ideas would make him "an outcast in genetics" and Dunn tried to prevent him from publishing them without more evidence. For a discussion of the history of this publication, see M. Dietrich, "From Gene to Genetic Hierarchy: Richard Goldschmidt and the Problem of the Gene," in *The Concept of the Gene in Development and Evolution: Historical and Epistemological Perspectives*, ed. P. J. Beurton, R. Falk, and H. Rheinberger (Cambridge: Cambridge University Press, 2000), 96; and M. Dietrich, "On the Mutability of Genes and Geneticists," *Perspectives on Science* 4 (1996): 334.

[74] Goldschmidt, "The Theory of the Gene," 270.

that geneticists should think of the entire chromosome as the unit of heredity. On this picture, mutations were not changes in the chemical constitution of a classical gene, but rather rearrangements of the chromosome that altered its chemical properties: "The conclusion, then, is that... no genes are existing but only points, loci, in the chromosome which have to be arranged in a proper order or pattern to control normal development."[75] Goldschmidt's conviction that the gene did not exist as an individual unit became an underlying theme in his work. In the following decade he developed an alternative picture of the hereditary material, proposing a hierarchy of structural and functional genetic units ranging from barely visible structures to large segments of the chromosome.[76]

Goldschmidt's reconceptualization of the nature of the chromosome, which was based on his work in physiological and developmental genetics, challenged traditional pictures of the genetic mechanisms driving development and evolution; and his articulations of this challenge significantly influenced how geneticists in the 1940-50s thought about the nature of "classical genetics." For example: in his controversial and widely read monograph *Physiological Genetics* (1938), published in the same year as "The Theory of the Gene," Goldschmidt criticized geneticists' attention to problems of transmission at the expense of problems of development, employing language similar to that used by Lysenko. Describing studies of the mechanism of heredity and "the status of the germ plasm" as "static genetics," Goldschmidt advocated a shift in focus towards the "physiological" or "dynamic genetics" of gene action. This distinction, which he drew only in the preface to the monograph, was widely commented upon by his colleagues and was subsequently used to characterize the nature of the difference between "classical" and "developmental" genetics.[77] In Goldschmidt's mind, however, it was not only a

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[75] Ibid.: 271.

[76] Dietrich, "From Gene to Genetic Hierarchy: Richard Goldschmidt and the Problem of the Gene." For one of Goldschmidt's latter discussions of "the classical theory of the gene," see Goldschmidt, "'Repeats' and the Modern Theory of the Gene," 367. See also his discussion of "the classical corpuscular gene," in R. B. Goldschmidt, "Marginalia to McClintock's Work on Mutable Loci in Maize," *The American Naturalist* 84 (1950).

[77] Although this distinction was later often associated with Goldschmidt, it had been in circulation long before this monograph. See, e.g., C. D. Darlington, "The Internal Mechanics of the Chromosomes. I. The Nuclear Cycle in Fritillaria," *Proceedings of the Royal Society of London. Series B, Biological Sciences* 118 (1935): 34; T. H. Morgan, "The Theory of the Gene," *American Naturalist* 51 (1917): 513; and J. H.

distinction between fields of study. As he explained in his Presidential Address to the Ninth International Congress on Genetics in Bellagio (1953), the static and dynamic—the statistical and the physiological—were “different philosophies of genetics.” The former developed explanations through “the introduction of more and more units for statistical treatment,” whereas the latter referred to “genic action and developmental systems” and avoided “explanation in terms of unproved, additional systems of units” such as gene modifiers.[78] These were different “mental attitudes,” according to Goldschmidt, in the sense that he thought there was “no objective way to decide which is the correct” one.[79] In discussing the outcomes of this congress in *Science*, Michael Lerner, a population geneticist and colleague of Goldschmidt at the Department of Genetics at Berkeley, noted:

> it can be said that the most significant trend at the Bellagio Congress was the virtual abandonment of the gene in the classical sense as the object of study. Both the statistical and physiological approaches, to follow the distinction laid down by President Goldschmidt, have instead turned their attention to the properties and functions of more complex systems…This is not to say that the foundations of classical genetics constructed in the course of the last 50 years have been undermined or destroyed. It is only that some of the more naive and simple concepts, essential in their own time, have now outlived their usefulness and are being replaced.[80]

Although Lerner held Goldschmidt’s work on physiological genetics in great esteem, he strongly disagreed with Goldschmidt’s work on evolution, which was equally influential in shaping the idea of “classical genetics” that emerged in the 1940s-50s.[81]

In *Material Basis of Evolution* (1940), a widely read and radical critique of the model of classical genetics underlying the Neo-Darwinian study of evolution, Goldschmidt argued that the evolution of new species could not result from the gradual accumulation of small mutations. The “bridgeless gaps” between species could only be spanned by macromutations, which Goldschmidt suggested could occur through two different genetic mechanisms: systemic chromosome mutations, or gene mutations with

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Woodger, "The "Concept of Organism" and the Relation between Embryology and Genetics. Part III," *Quarterly Review of Biology* 6 (1931): 185.

[78] R. B. Goldschmidt, "Different Philosophies of Genetics," *Science* 119 (1954): 704.

[79] Ibid.

[80] I. M. Lerner, "The Ninth International Congress of Genetics," *Science* 118 (1953): 709.

[81] At this time, Lerner was working on *Genetic Homeostasis* (1954), a widely acclaimed monograph in which he proposed a picture of population genetics that was incompatible with Goldschmidt’s theory of evolution.

significant developmental effects. On the "systemic mutation" picture, which he saw as the phylogenetic consequence of his rejection of the classical gene, a new species emerged through large-scale rearrangements of the primary pattern and thus the reaction system of the chromosome.[82] This theory of evolution-through-systemic-mutations required that a genetic system jump from one stable developmental reaction system to another; and in the last third of *The Material Basis of Evolution,* Goldschmidt demonstrated that a single genetic change could bring about such a shift. He noted that this aspect of his argument did not require a rejection of the classical theory of the gene, given that developmental macromutations could be understood as mutations in genes or as systemic mutations. But he argued that systemic mutations were a more plausible means for producing a new species, and *Material Basis of Evolution* was widely read as a radical rejection of the classical genetics on which Neo-Darwinism was founded. In a review for *American Naturalist*, for example, Carl Hubbs stated that Goldschmidt presented his readers with a simplistic dichotomy, "classical genetics versus Goldschmidtian genetics."[83] And Shull's historical review of population genetics, "Two Decades of Evolutionary Theory," quoted *Material Basis of Evolution* when discussing the hereditary mechanisms studied through the mathematical analysis gene populations: "They are the subject matter of what Goldschmidt, in a tone of mild disparagement, calls 'classical genetics'."[84]

Few of Goldschmidt's colleagues agreed that the gene concept needed to be completely abandoned or that microevolution had no role in species formation. But by the 1950s, many did agree that the "classical" picture of the gene was a misrepresentation of the hereditary material: discussions of the nature of the gene regularly drew on Goldschmidt's ideas, and standard textbooks presented his rejection of the classical picture, often quoting from his work at great length.[85] Perhaps the reason for this was

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[82] M. Dietrich, "From Hopeful Monsters to Homeotic Effects: Richard Goldschmidt's Integration of Development, Evolution, and Genetics," *American Zoologist* 40 (2000): 739.

[83] C. L. Hubbs, Review of *The Material Basis of Evolution*, by R. Goldschmidt, *American Naturalist* 75 (1941): 274.

[84] Shull, "Two Decades of Evolution Theory," 172.

[85] E.g., Stadler used Goldschmidt's analysis of position effect to support his conclusion that "the classical theory of the corpuscular gene must be discarded," in L. J. Stadler, "The Gene," *Science* 120 (1954): 815, 817. A quotation from Goldschmidt extended across three pages of E. O. Dodson, *Genetics: The Modern*

that most geneticists did not see Goldschmidt as rejecting the foundation of classical genetics, but rather as building on it. As described by Curt Stern: "His outstanding experimental accomplishment…led to an analysis of the phenomenon of intersexuality which went far beyond the framework of classical genetics… He reached his height in his endeavors to build a dynamic physiological genetics on the static and material basis which Mendel and Morgan had laid."[86] On this picture, Goldschmidt's approach was compatible with that of classical genetics; and it was in these terms that Goldschmidt's ideas were incorporated into the teaching of the principles of genetics.

A Type of Science: The Pedagogical Differentiation of Classical Genetics

One of the first textbooks to refer to "classical genetics" was the 1945 edition of Altenburg's *Genetics*, which used the term in differentiating between two ways in which genetics was studied and conceptualized. The opening line of the textbook's preface, "In the present text emphasis has been placed on modern genetics," introduced a discussion in which Altenburg explained that the traditional definition of genetics, as the study of heredity and variation, was "no longer adequate."[87] Drawing attention to modern geneticists' studies of various aspects of the germplasm—including its origin and evolution, physical and chemical constitution, interactions with the environment, spread and distribution, and influence on development—Altenburg suggested: "Genetics in the modern sense might therefore better be defined as the science which concerns itself with the study of the germ plasm." This physical focus of modern genetics made it distinct from that of "classical genetics," according to Altenburg, who did not clearly define the latter but seemed to accept that the "definition often given of genetics as the study of heredity and variation" was fitting.[88] On this picture, classical genetics was based on breeding experiments and statistical analyses of phenotypic distributions, whereas modern genetics complemented these approaches with cytological observations.

Science of Heredity (Philadelphia: W. B. Saunders, 1956), 210-212. See also E. W. Sinnott, L. C. Dunn, and Th. Dobzhansky, *Principles of Genetics*, 5th ed. (New York: McGraw-Hill, 1958), 387.

[86] C. Stern, "Richard Goldschmidt, Biologist," *Science* 128 (1958): 1069.

[87] E. Altenburg, *Genetics*, 1st ed. (New York: H. Holt, 1945), v.

[88] Ibid.

A similar distinction was introduced in the fourth edition of *Principles of Genetics* (1950), which was later described as "the classic textbook of classical genetics."[89] Here, in the introduction to the chapter on "the physical basis of inheritance," Sinnott, Dunn, and Dobzhansky differentiated between the gene concept used in studying cytogenetics and that used in studying the laws of transmission:

> For the purposes of analyzing the inheritance of traits in crosses it is sufficient to define a gene as a unit, transmitted from parents to offspring…The gene so defined is really a symbol or abstraction, and the body of knowledge concerned with this symbolic gene has come to be known as *formal genetics*…Formal genetics was developed in essentially modern form during the first quarter of the twentieth century.[90]

The historical account that followed explained that the "union of genetics and cytology" in recent decades had allowed geneticists to study the genetic material "visually," and had thus led to a new conception of the gene. According to Sinnott, Dunn, and Dobzhansky, when geneticists "endeavored to discover where in the organism genes are located, what they are, and how they function," the gene they were studying was identical to "the material, or physical, basis of heredity."[91] Thus, it made sense to distinguish between two different types of genetics—formal genetics and cytogenetics, each with a purpose-specific conception of the gene. Although the authors never used the phrase "classical genetics," they drew these lines in the same place as Altenburg, who did use the term. This is worth noting because of the way in which these lines subsequently shifted in later textbooks of the 1950s. In this regard, it is also notable that Sinnott and Dunn had not drawn this distinction in the first three editions of *Principles of Genetics* (1925, 1932, and 1939), but introduced it only in 1950.

The introduction of a distinction between the symbolic and material gene in 1950 coincided with addition of the eminent population geneticist Dobzhansky to the authorship of *Principles of Genetics.* At the time this fourth edition was published, the textbook had already achieved extensive international circulation and had been widely

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[89] J. A. Moore, *Heredity and Development*, 2nd ed. (New York: Oxford University Press, 1972), 134: "Throughout its long history this has been the classic textbook of classical genetics."

[90] E. W. Sinnott, L. C. Dunn, and Th. Dobzhansky, *Principles of Genetics*, 4th ed. (New York: McGraw-Hill, 1950), 151 (emphasis original).

[91] Ibid.

praised by Anglo-American geneticists and famously criticized by Lysenko.[92] Dobzhansky's contributions further contributed to its popularity in the United States. A review in *Science*, for example, praised the new edition: "Teachers of genetics will be gratified to find that they have here for the first time a textbook of genetics adequately representing the facts and principles of classical genetics as well as the more recent findings and problems of our own time."[93] This review by Ernst Caspari was one of the first to mention "classical genetics," using it to refer to the essential teachings of genetics that grounded the student in the discipline—a form of presentation that was to become standard in the textbooks of the 1950s. For example: in Srb and Owen's *General Genetics* (1952)—which was described as a "leading" textbook in the 1950s, and later seen as the molecularly-oriented successor to "the definitive genetics textbook" of Sinnott, Dunn, and Dobzhansky—the structure was outlined in the preface: "The first sixteen chapters provide elementary coverage of the so-called 'classical' areas of genetics. These areas are included in almost every general course in the subject."[94] Srb and Owen differentiated classical genetics from the study of "genetic effects," in chapters 17-19, and "population genetics," in chapters 20-21. Similar divisions and language were employed by E. J. Gardner, in his widely used textbook series *Principles of Genetics*. In the third edition (1968), the preface explained: "The material has been organized into three parts: I, Basic Genetics; II, Nature and Function of Genetic Material; and III, Population Genetics and Evolution."[95] Merrell similarly divided his *An Introduction to Genetics* (1975) into three main sections, "on classical genetics, the nature of the gene, and the genetics of populations," noting that they "constitute, more or

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[92] Lysenko stated: "An example of how far our native Mendelists-Morganists uncritically accept idealistic genetics is the fact that until recently the basic textbook on genetics in many of our higher institutes of learning is a translation of the strictly Morganist American textbook of Sinnott and Dunn," in Lysenko, "The Situation in Biological Science," 111. This comment was well-known and was seen as an example of Lysenko's attack on classical genetics. See, e.g., Cook, "Lysenko's Marxist Genetics: Science or Religion?" 182.

[93] E. Caspari, Review of *Principles of Genetics*, by E. W. Sinnott, L. C. Dunn, and Th. Dobzhansky, *Science* 112 (1950): 725.

[94] A. M. Srb and R. D. Owen, *General Genetics*, 1st ed. (San Francisco: W. H. Freeman, 1952), v. Bentley Glass described it as the "leading" textbook in B. Glass, Review of *General Genetics*, by A. M. Srb and R. D. Owen, *Quarterly Review of Biology* 29 (1954): 359. According to D. L. Nanney, "Eugenics and Human Heredity," *Journal of Heredity* 77 (1986): 481, "L. C. Dunn…was coauthor through many editions of the definitive genetics text until it was finally replaced by Srb and Owen's formulation of a new and molecular center-of-gravity for genetics." The textbook was also translated into Japanese, Spanish and Polish.

[95] E. J. Gardner, *Principles of Genetics*, 3rd ed. (New York: John Wiley & Sons, 1968), v.

less, the usual subject matter for a beginning course in genetics."[96] It was not just general genetics textbooks, however, but also more specialized works that were seen as requiring a foundation in classical genetics. James Watson, for example, opened *The Molecular Biology of the Gene* with a chapter on "The Mendelian Worldview," in order to "help the reader see how our ideas about molecular genetics have developed out of the work of the classical geneticists."[97] Authors' treatments of "classical genetics" were also regularly discussed in book reviews and mentioned on textbook dust jackets. [98]

The authors of the standard general genetics textbooks of the 1960s—like Altenburg (1945) and Sinnott, Dunn, and Dobzhansky (1950)—used the gene as a point of reference in defining "classical genetics."[99] There was, however, a significant shift in the content and textual location of these claims. In the earlier uses, "classical genetics" referred to theories and knowledge developed from the statistical analysis of phenotypes, as opposed to those developed through cytogenetics. In one respect, this distinction was similar to that in Goldschmidt's work: it was used to identify different ways in which a single problem, the problem of transmission, was approached and conceptualized. By the late 1950s, however, this had changed. Geneticists did not differentiate between a

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[96] D. Merrell, *An Introduction to Genetics* (New York: W. W. Norton, 1975), preface. Note that this structure remains common today: e.g., D. P. Snustad and M. J. Simmons, *Principles of Genetics*, 4th ed. (Hoboken, NJ: John Wiley & Sons, 2005), which is divided into "Classical Genetics," "Basic Molecular Genetics," and "Quantitative, Population and Evolutionary Genetics."

[97] J. D. Watson, *Molecular Biology of the Gene*, 1st ed. (New York: W. A. Benjamin, 1965), ix. Likewise, a review criticized *Bacterial Genetics* for devoting only 15 per cent of the text to the basic principles of classical genetics: "inasmuch as genetic principles pervade all of the biological sciences, including bacteriology, it is seriously doubted that there is any substitute for a course in the principles of classical genetics for the beginning bacteriology student today," in R. W. Barratt, Review of *Bacterial Genetics*, by W. Braun, *Quarterly Review of Biology* 29 (1954): 259. Moore's overview of "classical genetics" in *Principles of* Zoology (1957), which was used to "illustrate the methodology, philosophy and strategy of science," was praised in J. E. Harris, "Basic Zoology," *Nature* 181 (1958): 1431.

[98] See, e.g., A. M. Srb, R. D. Owen, and R. S. Edgar, *General Genetics*, 2nd ed. (San Francisco: W. H. Freeman, 1965), dust jacket: "The authors present a balanced view of the science and retain a substantial part of what is called classical genetics." See also T. W. Whitaker, "Ultra-Modern Genetics," Review of *General Genetics*, by A. M. Srb and R. D. Owen, *Journal of Heredity* 43 (1952): 188; R. F. Kimball, Review of *The Science of Heredity*, by J. S. D. Bacon, and *What's All This About Genetics?* by R. Hurst, *Quarterly Review of Biology* 27 (1952): 390-91; Th. Dobzhansky, "Genetics, the Core Science of Biology," Review of *The Science of Genetics*, by C. Auerbach, *Genetic Research*, by A. Muntzing, *Genetics on the Population Level*, by M. Rasmuson, *Human Genetics*, by C. C. Li, and *Cell Heredity*, by R. Sager and F. J. Ryan, *Science* 134 (1961): 2091-92; D. H. Morgan, Review of *Genetics*, by H. Kalmus, *Nature* 206 (1965): 78; and V. Woodward, "Survey of Genetics," Review of *Genetics*, by M. W. Strickberger, *Science* 161 (1968): 1234.

[99] See, e.g., Dodson, *Genetics*, 145; Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 5th ed., 339; and Srb, Owen, and Edgar, *General Genetics*, 2nd ed., 265.

symbolic and physical gene, nor between the gene of Mendel and that of Morgan.[100] When considered alongside the molecular picture of the gene, the differences between the two seemed insignificant. And thus, they came to be defined as one—as an independent and indivisible unit of hereditary structure, function, recombination, and mutation. The symbolic gene of Mendelian breeding experiments was reconceived as a physical unit, and the physical gene of cytogenetics came to be seen as "classical."

This unification of symbolic and physical genes, and corresponding redefinition of the classical era, came with a shift in the location of textbooks' discussions of the nature of classical genetics. In the late 1950s, the idea of classical genetics was no longer introduced in between chapters on Mendelian and cytogenetic studies of heredity. It was, rather, shifted to the transition between chapters on cytogenetic and physiological studies. In its new location, it projected unity onto the chapters on Mendelism, Sex-Linkage, Linkage, Crossing Over, Genome Structure, and Mutation. "Classical genetics" functioned as an idea of the past against which to define the modern. In *Genetics: The Modern Science of Heredity* (Dodson, 1956), for example, the chapter on "The Physiology of the Gene" opened with a discussion that differentiated its subject matter from that of the first sixteen chapters: "The preceding chapters," which explained the theories and investigative methods of transmission genetics, "dealt principally with numerical relationships between various phenotypes and the genotypes upon which they are based." The student learned that physiological genetics, on the other hand, focused on the "causal sequences" between the genotype and phenotype, "in which the gene is simply the primary causal factor." This was the aspect of heredity that the classical approach failed to address. In describing this difference, Dodson drew explicitly on Goldschmidt:

> The earliest studies of heredity were directed entirely towards the problems of the mechanisms of transmission, the understanding of the genetic ratios, the "statics of genetics," as Goldschmidt has put it. The problems of dynamic genetics, or how genic effects are obtained, was

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[100] Note that there were exceptions, such as a poorly reviewed textbook by E. G. White, who distinguished between three phases of research, "Mendelian Genetics, based on entire chromosomes; Drosophila Genetics, based on gene alignment on the chromosomes; and Chemical Genetics, based on DNA as the basic component of the gene," in E. G. White, *Genetics*, 2nd ed. (New York: Vantage Press, 1962), preface.

> [*sic*.] seldom touched upon. As a result heredity was commonly regarded as more or less magical in its operation.[101]

On this account, the distinction between physiological and classical genetics had to do with differences in their investigative strategies and the types of phenomena they studied—conceptual differences that were presented in terms of historical progress from the early to the modern study of heredity. It was thus in chronological as well as methodological and conceptual terms that the chapters on physiological genetics were differentiated from those on classical genetics. The textual placement of Dodson's discussion of the nature of the gene and classical genetics—at the start of a series of chapters on the genic control of metabolism and development—was a common way of differentiating between sections of the textbook.

Statements about the nature of the gene and classical genetics were also located at the opening of chapters on physiological genetics in the 1958 edition of *Principles of Genetics*—which, notably, removed the previous edition's discussion of the symbolic and physical gene. In this fifth edition, classical and physiological genetics were described as approaches that focused on different aspects of the hereditary process and produced different types of knowledge: attempts to study the classical principles of genetics analyzed "effects," whereas physiological studies uncovered "causal relations." According to Sinnott, Dunn, and Dobzhansky, the former supported inferences, which were known as the laws of transmission. And the latter led to two different types of knowledge, corresponding to the two processes studied in physiological genetics: the genic control of development and the genic control of metabolism, both of which "are concerned with ontogeny, but deal with it at different levels."[102] They explained that the study of development began with differences in phenotypic characters and worked backward through developmental stages towards differences in genes, whereas the study of metabolism began with the nucleus and tried to identify the reactions by which compounds were synthesized. It is notable that the chapters explaining these aspects of genetics were framed by discussions of the nature of the gene and references to classical genetics. At the beginning of the chapter on developmental genetics, for example, they

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[101] Dodson, *Genetics*, 193.

[102] Sinnott, Dunn, and Dobzhansky, *Principles of Genetics*, 5th ed., 327.

identified the assumptions about the gene that provided the foundation for classical genetics and asked: "are new assumptions about genes and their effects needed to explain the genic control of development processes?"[103] In answering this question, they characterized the distinction between "classical" and "developmental" genetics as being one of setting: classical genetics was based on the analysis of phenotypic differences that were traceable to genetic differences between individuals of the same familial lineage; developmental genetics was based on the analysis of metabolic differences that were traceable to the "same set of genes" in "members of the same cell lineage." One of the purposes of studying development, according to Sinnott, Dunn, and Dobzhansky, was "to discover new ideas about genes and their functions in a setting different from that in which the problems of classical or formal genetics are studied."[104]

In the 1960s, the standard textbooks continued to define classical genetics in terms of the gene, but the textual location of these discussion was once again significantly restructured: they still followed the chapters on the "principles," but instead of being located in the introduction to chapters on physiology, they were part of an independent section of the textbook on the gene.[105] In John Moore's *Heredity and Development*, for example, a series of chapters on "Mendelism," "Chromosomal Inheritance," and "Morgan and *Drosophila*" were followed by a chapter entitled "Genetics—Old and New," which opened with a discussion of what had been accomplished by the 1930s. According to Moore, "What had been worked out were the rules governing the transmission of the genes," which he explained by listing the "generally established concepts of classical genetics."[106] These rules of transmission were characterized as universal (e.g., "their universality was impressive"), but the textbook presented them as idealizations to which there were some exceptions. In Moore's words: "the generally established concepts of classical genetics appear in *italics*;

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[103] Ibid., 339.

[104] Ibid.

[105] In addition to the textbooks that I discuss, see E. J. Gardner, *Principles of Genetics*, 1st ed. (New York: John Wiley & Sons, 1960), 257-299.

[106] Moore, *Heredity and Development*, 2nd ed., 140.

expansions or exceptions…are in roman type."[107] Thus, the chromosome theory of heredity was presented:

> 3. *Genes are situated on chromosomes.* There are few exceptions to this generalization. A fraction, possibly very small, of inheritance is dependent upon non-chromosomal structures such as mitochondria, plastids, and some virus-like bodies.[108]

Insofar as the concepts of classical genetics were presented as rules to which there were a few exceptions, they were presented as idealizations—and so were the phenomena that geneticists described with them: "From these simple propositions one can deduce most of the phenomena of classical genetics."[109] In the chapters that followed, Moore turned to discuss the questions geneticists asked "once the concepts of transmission genetics had been established"—"What is the gene? How does it act?" In these chapters that discussed the "new" study of genetics, the classical was further defined, as it became the foundation for knowledge of modern genetics. This "historical" presentation adopted by Moore was common in this period and was seen as the logical way of teaching genetics. David Merrell, for example, explained in the preface to his 1975 *An Introduction to Genetics*: "Rather than beginning by describing our current knowledge of molecular genetics, we present genetic concepts and discoveries for the most part in historical sequences…Only after one understands these laws can one logically proceed to analyses at the cellular and molecular levels of hereditary organization."[110]

The 1960s expansion of chapters on the gene and corresponding historicization of the classical era can also be seen in the widely used second edition of *General Genetics* (Srb, Owen, and Edgar, 1965). In this edition, it was at the opening of a pair of chapters on gene action—after a series of chapters on the general principles of genetics, and before the chapters on development and population genetics—that they first discussed classical genetics, in a section on "The Classical Gene." As the authors explained in the introduction to the chapter, they took the classical gene to be that used in Morgan's work on *Drosophila*: "In this chapter we will describe the 'classical' gene, as conceived, for

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[107] Ibid.

[108] Ibid., 141 (emphasis original).

[109] Ibid., 142. The idea of "classical genetics" as ideal genetics was furthered in authors' references to Mendelian ratios as "classical ratios." See, e.g., Chapter 6, "Modification of Classic Genetic Ratios," in R. C. King, *Genetics*, 1st ed. (New York: Oxford University Press, 1962).

[110] Merrell, *An Introduction to Genetics*, xiii.

example, by T. H. Morgan, and then show how the classical conception has been revised."[111] According to the authors, the "classical conception of the gene assumed it to be a unitary particle" in three different ways: a unit of physiological function, mutation, and chromosome structure—each of which corresponded to a way in which the gene had been observed.[112] Here, they defined the classical gene in terms that corresponded neatly with Benzer's cistrons, recons, and mutons, and with the philosophy of operationalism that prominent geneticists were advocating in the 1950s-1960s. L. J. Stadler, for example, had famously argued that geneticists needed to adopt the "operationalist viewpoint that has become commonplace in modern physics."[113] Bentley Glass agreed in an article on the gene: "In these days of the operational definition, we should quite properly not be speaking of 'genes' at all, but of…'mutons,' 'recons,' and 'cistrons'."[114] On the operationalist account, classical geneticists had used three distinct criteria for the gene, but had mistakenly thought that they referred to a single unit of the chromosome: "For a long time geneticists tacitly assumed that the units of mutation, recombination and function corresponded to the same physical entity, the classical 'gene'."[115] Modern geneticists, then, had to merely break the classical gene along divisions that already existed within it. In this picture, classical genetics was reducible to modern genetics; the classical gene, if it existed, was merely a special case in which the recon, muton, and cistron happened to coincide. Authors who adopted this view, such as Srb, Owen, and Edgar, were thus able to conclude: "The picture of the gene derived from formal genetic analyses agrees well with modern ideas and facts concerning the chemical nature of the gene and its mode of action."[116]

When Srb and Owen first published their textbook in 1952, several reviews suggested that it focused on the modern at the cost of the classical. *Journal of Heredity*, for example, reported: "In their enthusiasm to include the latest developments, the historical foundation of the subject has been neglected. In fact, the student may be left

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[111] Srb, Owen, and Edgar, *General Genetics*, 2nd ed., 265.
[112] Ibid.
[113] Stadler, "The Gene," 814.
[114] B. Glass, "In Pursuit of a Gene," *Science* 126 (1957): 683. See also, e.g., A. W. Ravin, "Experimental Approaches to the Study of Bacterial Phylogeny," *The American Naturalist* 97 (1963): 309.
[115] Ravin, "Experimental Approaches to the Study of Bacterial Phylogeny," 309.
[116] Srb, Owen, and Edgar, *General Genetics*, 2nd ed., 280.

with the impression that all of the important discoveries in genetics were made within the past decade."[117] Ernst Caspari agreed in his review for *Science*: "The heaviest stress of the book is on physiological genetics. This does not mean that the number of chapters dealing with this subject has been increased above that found in other textbooks. It means rather that a physiological point of view prevails throughout the book."[118] Although Caspari praised their treatment of this material, he suggested that this emphasis "led to the result that relatively little space is devoted to purely formal genetics," which he thought made it "doubtful whether the book will gain wide acceptance in the teaching of elementary genetics."[119] By the late 1960s, however, the second edition of *General Genetics* had become one of the most widely used general genetics textbooks, along with Gardner's *Principles of Genetics*, and Monroe Strickberger's *Genetics*.[120] The textual structure and treatment of "classical genetics" in *General Genetics* was even replicated in popular textbooks of the 1970s and 1980s, such as Suzuki, Griffiths, and Lewontin's *An Introduction to Genetic Analysis*.[121]

The three standard textbooks of the 1960s—Srb, Owen, and Dodson's *General Genetics*, Gardner's *Principles of Genetics*, and Strickberger's *Genetics*—made a notable break with their predecessors: they all devoted an independent section to the physiology/biochemistry of the gene, rather than treating it as an aspect of a chapter on development. The index reference for "biochemical genetics" in Gardner's 1972 edition, for example, did not identify a chapter on developmental genetics, as it had in Snyder's 1957 *Principles of Heredity*, but rather pointed to a chapter on the nature of the genetic material. In the "modern" textbooks of Gardner, Strickberger, and Srb, Owen, and

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[117] Whitaker, "Ultra-Modern Genetics," 188.

[118] E. Caspari, Review of *General Genetics*, by A. M. Srb and R. D. Owen, *Science* 117 (1953): 45-46.

[119] Ibid.

[120] In 1969, a survey of 226 genetics instructors at 217 colleges and universities found that approximately 80 percent of the participating institutions used one of these three textbooks. This study is discussed in M. J. Straney and T. R. Mertens, "A Survey of Introductory College Genetics Courses," *Journal of Heredity* 60 (1969): 224.

[121] E.g., Chapter 10 ("The Nature of the Gene") in *An Introduction to Genetic Analysis* is identical in key aspects of content and organization: the authors first characterized the gene concept used in the previous chapters as "the bead theory" of the gene, defined as a unit of structure, mutation, and function; in Chapter 10, they begin their "attack on the bead theory." See D. T. Suzuki, A. J. F. Griffiths, and R. C. Lewontin, *An Introduction to Genetic Analysis*, 2nd ed. (San Francisco: W. H. Freeman, 1981), 372-373.

Dodson, the nature of the gene was distinguished from the function of the gene.[122] And the study of gene action was presented as distinct from the study of development.[123]

The addition of a fourth section on the gene is apparent in the language of Strickberger's preface. For although Strickberger maintained the traditional division of genetics into three levels of heredity, "from the molecules of cells, through developmental stages of individuals, to populations of organisms," he identified four "areas of study," or fields of questions: "1. What and where is the genetic material? 2. How is it formed, transmitted, and changed? 3. How is it organized and how does it function? 4. What happens to it among groups of organisms as time passes?"[124] Students reading the textbook would not just learn about the genetics of transmission, development, and populations; they would also study the gene. And although most textbooks of the 1970s continued to devote just one chapter to developmental genetics and one to population genetics, the growth of research in molecular genetics resulted in a significant expansion in the number of chapters devoted to the nature of the gene and gene action.[125]

Between 1950 and 1970, the growth of research on microorganisms provided the foundation for a new way of conceptualizing "classical genetics." Instead of mapping classical genetics onto the previously established category of transmission genetics, textbook authors defined it in terms of its difference from molecular genetics. The pioneers in the field of microorganism research—Beadle, Gray, and Tatum—had framed their work in terms of its relation to "classical genetics" as early as the 1940s.[126] But this language did not gain greater currency until the 1950s-1960s, when it became common for authors of journal articles on microorganism research to refer to the techniques and

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[122] E.g., they were split into two different sets of chapters, "Part IV" and "Part V," in M. W. Strickberger, *Genetics*, 1st ed. (New York: Macmillan, 1968).

[123] See, e.g., Gardner, *Principles of Genetics*, 3rd ed.; and Srb, Owen, and Edgar, *General Genetics*, 2nd ed.

[124] Strickberger, *Genetics*, 1st ed., vii.

[125] See, e.g., M. W. Strickberger, *Genetics*, 2nd ed. (New York: Macmillan, 1976); and D. T. Suzuki, A. J. F. Griffiths, and R. C. Lewontin, *An Introduction to Genetic Analysis*, 1st ed. (San Francisco: W. H. Freeman, 1976).

[126] See, e.g., G. W. Beadle, "Biochemical Genetics," *Chemical Reviews* 37 (1945): 16, 18, 20, 56, 69, 81; Beadle and Tatum, "Genetic Control of Developmental Reactions," 107; G. W. Beadle and E. L. Tatum, "Neurospora 2: Methods of Producing and Detecting Mutations Concerned with Nutritional Requirements," *American Journal of Botany* 32 (1945): 682; and C. H. Gray and E. L. Tatum, "X-Ray Induced Growth Factor Requirements in Bacteria," *Proceedings of the National Academy of Sciences of the United States of America* 30 (1944): 408.

principles of "classical genetics" when describing new methods and discoveries.[127] As these new lines of research developed, genetics came to be seen as a science with two branches. These two branches were presented as having different scopes and aims, and consequently, as having different paradigmatic organisms. For example, in the context of molecular experiments on microorganisms, the historicity of the use of *Drosophila* became apparent. This research organism, which had been the preferred subject and tool of geneticists for decades, became a marker of a past era, which Schultz noted in 1967: "'Classical' genetics has come to be identified with the work on *Drosophila*, which proved the chromosome theory of heredity and gave us the theory of the gene."[128] The identification of eras with organisms was also articulated in genetics textbook reviews, which regularly divided the subject into classical and molecular branches.[129] A review of King's textbook, for example, stated: "This book and, of course, its author, are admittedly *Drosophila*-oriented. Yet it must not be assumed, as it so often is, that this book adopts a 'classical' approach to its subject matter, or that its treatment of 'molecular' genetics is bound to be skimpy and inadequate."[130] Even Dunn's *A Short History of Genetics* promoted the conceptual association between types of genetics and types of organisms, defining classical genetics as the study of multi-cellular organisms and molecular genetics as the study of single-cell organisms.[131] Thus, research on microorganisms not only provided the subject matter for new textbook chapters on gene

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[127] See, e.g., the uses of "classical" in G. W. Bartlett and C. Hinshelwood, "Observations on the Drug Resistance of Bacterial Recombinants," *Proceedings of the Royal Society of London. Series B, Biological Sciences* 150 (1959): 330; Dobzhansky, "The Theory of the Gene," 123; F. Jacob, "Genetics of the Bacterial Cell," *Science* 152 (1966): 1471; D. L. Nanney, "Mating Type Determination in Paramecium Aurelia, a Model of Nucleo-Cytoplasmic Interaction," *Proceedings of the National Academy of Sciences of the United States of America* 39 (1953): 115; S. Spiegelman, R. R. Sussman, and E. Pinska, "On the Cytoplasmic Nature of 'Long-Term Adaptation' in Yeast," *Proceedings of the National Academy of Sciences of the United States of America* 36 (1950): 603; and J. D. Watson and W. Hayes, "Genetic Exchange in Escherichia Coli K12: Evidence for Three Linkage Groups," *Proceedings of the National Academy of Sciences of the United States of America* 39 (1953): 416.

[128] Schultz, "Innovators and Controversies," 298.

[129] See, e.g., L. Ehrman and D. Andow, Review of *Understanding Genetics*, by N. V. Rothwell, *Quarterly Review of Biology* 52 (1977); and E. R. Katz, Review of *Genetics*, by D. J. Cove, *Quarterly Review of Biology* 48 (1973): 636.

[130] L. Ehrman, Review of *Genetics*, by R. C. King, *Quarterly Review of Biology* 41 (1966): 209. See also G. Pontecorvo, "Inheritance, Chromosomal and Otherwise," Review of *The Mechanics of Inheritance*, by F. W. Stahl, *Extrachromosomal Inheritance*, by J. L. Jinks, and *The Cytoplasm in Heredity*, by D. Wilkie, *Nature* 205 (1965): 4.

[131] Dunn, *A Short History of Genetics*, 229. A similar distinction is drawn in C. Hinshelwood, "The Royal Society: Anniversary Address," *Nature* 178 (1956): 1266. This way of defining the classical and molecular fields continues today: e.g., J. R. Preer Jr., "Sonneborn and the Cytoplasm," *Genetics* 172 (2006): 1373.

structure and action in the 1950s—the chapters that were subsequently known as the section on "molecular genetics"—but also provided the ideas with which the classical era was reconceptualized, in both the academic literature of genetics and in its historiography.

A Historiographical Category: The Writing of Disciplinary History

The gene provided a point of reference for definitions of "classical genetics" in textbooks, and in histories of genetics, the first of which was written in 1965—a year in which geneticists were celebrating the centenary year of the publication of Mendel's classical paper, and in which Watson published what would later become a "classic" textbook of molecular genetics. In *A Short History of Genetics*, Dunn told the history of genetics from the perspective of 1939: "We may say that in the preceding period back to about 1900, genetics as we know it today took a form sufficiently unified to constitute a distinctive entity within biology. I shall call this the period of 'classical genetics'."[132] While Dunn acknowledged that these beginning and end dates were somewhat arbitrary—that classical genetics was, to an extent, a historiographical artifact—he argued that research in this period was unified by "a few relatively simple ideas," such as "the theory of the gene and its extension to the physical basis of heredity and to the causes of evolutionary changes in populations."[133] In Dunn's historiography, the definition of classical genetics in terms of the theory of the gene is significant, because it allowed for a reconceptualization of the relationships between classical, developmental, and population genetics. Although he occasionally equated "classical genetics" with "transmission genetics,"[134] as geneticists often did, he generally characterized classical and molecular genetics as different ways of framing the same set of problems. Thus he did not differentiate classical genetics from studies of development and evolution, but rather from the "new" studies of molecular genetics. In classical genetics, "problems were stated in terms of historical events in organisms—reproduction, heredity,

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[132] Dunn, *A Short History of Genetics*, xix-xx. On pages xv-xvi, Dunn noted that he was guided by Alfred Barthelmess's *Vererbungswissenschaft* (1952) and Hans Stubbe's *Kurze Geschichte der Genetik* (1963), both of which had focused on pre-"classical" history.

[133] Ibid., vii.

[134] Ibid., 224.

development, evolution," as opposed to the new genetics, in which they were "stated and studied in molecular terms." The two types of genetics were presented as asking questions at different "levels."[135] On this account, which was repeated by others, classical and molecular genetics were parallel approaches to the same set of problems.[136]

The concluding chapter of Dunn's history, "Old and New in Genetics," further developed his picture of the classical era with a discussion of molecular genetics, which Dunn dated to the 1940s: "When Avery, MacLeod, and McCarty proved (1944) that the material identified was deoxyribonucleic acid…a new kind of genetics began. It was proper to call this 'molecular genetics' since questions about continuity and change could now be asked in molecular terms."[137] Posing questions such as, "Will the new knowledge cause a radical change in our perception of the problems of genetics as these had taken form in the 'classical' period?", Dunn suggested: "It will help, in answering these questions, if we keep in mind what have been the chief directions of research in classical genetics," which he identified as "formal genetics (or transmission genetics), population genetics, and physiological or developmental genetics." He suggested that these three branches were unified in that they all sought "to understand a single entity, the succession of living organisms in reproduction, heredity, evolution, and individual development." And, according to Dunn, "the chief focus through which these problems came to be viewed was the concept of the gene."[138] Thus, Dunn's historiography can be seen as inadvertently answering his opening question: molecular genetics did in fact change the ways in which classical genetics was seen. From the perspective of the modern, previously disparate fields of study were seen as unified by their non-molecular, "classical," conception of the gene.

The publication of Dunn's history coincided with the publication of Arnold Ravin's *The Evolution of Genetics* and Alfred Sturtevant's *A History of Genetics*, which were immediately followed by Elof Carlson's *The Gene: A Critical History* and Curt

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[135] Ibid., xiii.

[136] E.g., Schultz, "Innovators and Controversies," 299: "One of the impressive characteristics of the recent history of molecular genetics is the emergence, in molecular format, of the same sequence of ideas that were elaborated in 'classical genetics'."

[137] Dunn, *A Short History of Genetics*, 225.

[138] Ibid., 224.

Stern and Eva Sherwood's *The Origins of Genetics: A Mendel Source Book.*[139] Sturtevant's and Dunn's histories became sources for the "preface histories" in genetics textbooks, and Stern and Sherwood's text was suggested as a supplement for textbook teaching.[140] Commenting on this "upsurge" in history-writing, Jack Schultz, who had worked with Sturtevant in Morgan's lab and praised him as "one of the founders of classical genetics," suggested: "It is due partly, perhaps, to the circumstance of a succession of centennial celebrations, for Darwin and Mendel…to the rise of molecular genetics…and perhaps most important, to the approaching twilight of a generation whose accomplishments are embodied in this history."[141] Remarkably, these histories of the 1960s, as well as many of the accounts that were subsequently included in textbooks and journals, placed the end of the classical era in the late 1930s—the time at which, I have shown, "classical genetics" was first used as a name for a type of genetics.[142]

Geneticists' sense of the history of classical genetics, and their historiographical presentation of its basic character, was informed by their conceptions of "classical physics." Parallels had long been drawn between the studies of heredity, physics, and chemistry—between factors/genes, atoms, and molecules—in both textbooks and journal articles. In the 1953 *Textbook of Genetics,* for example: "Genetics deals with facts as fundamental to the biological sciences as the atomic theory to the chemical sciences…The gene and the chromosome theory of heredity is equivalent to the atomic and molecular theory of the constitution of matter."[143] These parallels continued to be drawn in the historiography of the discipline, as the transition from classical to molecular

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[139] E. A. Carlson, *The Gene: A Critical History* (Philadelphia: Saunders, 1966); A. W. Ravin, *The Evolution of Genetics* (New York: Academic Press, 1965); C. Stern and E. R. Sherwood, *The Origin of Genetics: A Mendel Source Book* (San Francisco: W. H. Freeman, 1966); and A. H. Sturtevant, *A History of Genetics* (New York: Harper & Row, 1965). Although Sturtevant did not refer to "classical genetics," the pioneer of biochemical genetics with whom he had written the "logically" oriented textbook *An Introduction to Genetics*—George Beadle—noted in a review for *Science*: "the book is a beautifully concise summary of the substance of classical genetics." See G. W. Beadle, "History of Genetics," Review of *A History of Genetics*, by A. H. Sturtevant, *Science* 152 (1966): 922.

[140] E. J. Gardner, *Principles of Genetics*, 4th ed. (New York: John Wiley & Sons, 1972), 26-27.

[141] Schultz, "Innovators and Controversies," 296.

[142] See, e.g., Preer Jr., "Sonneborn and the Cytoplasm," 1373: "In the late 1930s…classical studies on genetics were almost finished"; and Moore, *Heredity and Development*, 2nd ed., 4: "Classical genetics was essentially complete by 1930."

[143] W. Hovanitz, *Textbook of Genetics* (New York: Elsevier Press, 1953), vi. See also, e.g., H. De Vries, *Intracellular Pangenesis*, trans. C. S. Gager (Chicago: Open Court, 1910), 13; Stadler, "The Gene," 814; D. H. Thompson, "On Gene Starvation," *American Naturalist* 72 (1938): 53; and Watson, *Molecular Biology of the Gene*, 1st ed., x.

genetics was presented as mirroring the transition from classical to modern physics. This idea had been articulated as early as 1939, when Goldschmidt stated: "It is my opinion that the classic theory of the gene corresponds to the conception of the indivisible atom of old physics…Genetics has outgrown this." Describing genetics as being "in a condition parallel to that of physics immediately before Rutherford," he proclaimed that geneticists "shall soon be ready for our Planck and Bohr."[144] In the era of molecular genetics, the history of the classical science continued to be articulated in these terms. A review of Pontecorvo's *Trends in Genetics Analysis* (1959), for example, stated:

> When the physicist increased the resolving power of his tools and entered the atomic nucleus, a whole new world of parts confronted and confused him: so it was with the geneticist upon his entry within the classical gene…This unanticipated complex fine structure has rendered the word gene all but useless and a legion of new terms and old terms in new garb have rushed in to fill the void.[145]

Here, the reviewer not only mapped the history of genetics onto that of physics, but did so by referring to the emergence of the new terminology of "mutons," "recons," and "cistrons." As noted earlier, the shift towards this operationalist language in genetics was itself modelled on physics. Thus, operationalism in physics influenced the way geneticists formulated their research, as well as how they described their history. And it was not long before this historiographical modeling of genetics on physics impacted the teaching of genetics, through its incorporation into textbooks. In the discussion of the classical era in *Heredity and Development* (1972), for example:

> Classical genetics was essentially complete in 1930. It was in a state similar to that of physics in 1899, when A. A. Michelson said, "The more important fundamental laws and facts of physical science have all been discovered, and these are now so firmly established that the possibility of their ever being supplanted in consequence of new discoveries is exceedingly remote […]." For both sciences the period of intellectual calm was brief.[146]

In describing the discoveries that brought closure to the classical periods of genetics and physics, Moore wrote: "Physics was soon revolutionized by studies of the nucleus of

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[144] Goldschmidt, as quoted in C. H. Waddington, *An Introduction to Modern Genetics* (New York: Macmillan, 1939), 1.

[145] R. S. Edgar, Review of *Trends in Genetic Analysis*, by G. Pontecorvo, Quarterly Review of Biology 35 (1960): 89.

[146] Moore, *Heredity and Development*, 2nd ed., 4. See also, e.g., J. D. Watson, *Molecular Biology of the Gene*, 2nd ed. (New York: W. A. Benjamin, 1970), x: "I believe that biology has as sound a basis as was provided chemistry, about 1932, by the explosive development of the quantum theory of the atom."

physical matter; genetics was revolutionized by studies of the nucleus of living matter."[147] On Moore's account, it was in the mid 1940s—when the chemical composition of genes was discovered, providing the basis for studies of gene action—that the new era of genetics had begun.

Conclusion

The preface of Gardner's 1960 *Principles of Genetics* opened with a statement about the textbook's aim: "This book is written primarily for the college student taking his first course in genetics. It emphasizes basic principles and tells the story of the classical experiments which have laid the foundation for a modern science."[148] At several points in the textbook, Gardner emphasized that almost all of the principles and mechanisms discussed were supported by many more examples than the classical ones he identified.[149] Throughout the first 13 chapters of the textbook, hereditary mechanisms, principles, and anomalies were all explained by reference to a "classical example," "classical experiment," or "classical study." The chapter on incomplete dominance, for example, detailed the "early classical study" conducted by Bateson and Punnett that "demonstrated gene interaction…from the results of crosses between two varieties of sweet peas."[150] And the chapter on modified ratios discussed the "classical experiment which resulted in the 15:1 ratio," which was "reported by George H. Shull from studies on the plant called shepherd's purse."[151] Quantitative inheritance was illustrated by Emerson and East's work on maize, aneuploidy by nondisjunction in Bridges' *Drosophila*, and multiple alleles by Castle's albino rabbits.[152] These were the classic cases of classical genetics—the scientific exemplars that defined the principles and the

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[147] Moore, *Heredity and Development*, 2nd ed., 4

[148] Gardner, *Principles of Genetics*, 1st ed., v.

[149] E.g., Ibid., 174. He noted when there were only a few examples to support a generalization, as with geneticists' account of sex-determination, which was based only on the research of Bridges on flies, Goldschmidt on months, and Whiting on wasps: Gardner, *Principles of Genetics*, 1st ed., 179-180.

[150] Gardner, *Principles of Genetics*, 1st ed., 85-86.

[151] Ibid., 87.

[152] Ibid. See also page 102: "One the classical studies on quantitative inheritance…was made by R. A. Emerson"; pages 197-198: "Bridges' example…has now become a classic"; and page 241: "A classical example of multiple alleles was discovered many years ago in rabbits."

discipline, and were widely agreed upon and identified as "classical" in textbooks, journals, and histories of genetics.[153]

Many of the classical examples identified by Gardner had first gained their status as "classical" in the 1910s-1920s, when "classical" was not a description of an aspect of heredity, but rather a description that identified the historical place of the example in scientific practice. In its very earliest uses, "classical" did little more than identify the historicity of an example, situating it within an established tradition. But, as I have shown in the first section of this chapter, its significance was soon after transformed, as "classical" came to be used to identify experimental cases that were seen as being representative of types of heredity. Defining diverse groups of cases in common terms, geneticists established key categories of family resemblance.

Although geneticists in the early 1930s were seemingly unaware of the "political" character of calling something a classic, this aspect of the language was identified by the end of the decade, as the more explicitly disciplinary language of "classical genetics" emerged. In the writings of Lysenko, the classics of Weismannian-Mendelian-Morganian genetics were identified as sources of scientific authority that misguided the science of heredity; the Western tradition of genetics was described as a cultural tradition rooted in idealism. This political critique of "classical genetics" as an ideology was categorically rejected by most Anglo-American genetics. But a concurrent attack on its philosophy, articulated by Richard Goldschmidt, had greater traction. Goldschmidt's attack, like Lysenko's, was from the vantage point of physiological and evolutionary theory, characterizing "classical genetics" as a static and atomistic science that obscured the dynamic character of heredity and life. Although Goldschmidt was a harsh critic of Lysenko, there are a few similarities in their discussions of classical genetics.[154] Both defined classical genetics as a formalistic and static orientation toward hereditary phenomena that was essentially flawed; and neither used the term more than a few times. These similarities are worth noting because of the way in which classical genetics was

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[153] E.g., Dunn, *A Short History of Genetics*, 101, also identified Cuénot's yellow mice as a classical case. Sturtevant, *A History of Genetics*, 60, also identified East and Emerson's work on maize as "a classic." And Dodson used many of the same examples as Gardner, including: Mendel's peas for Mendel's laws, white-eyes in *Drosophila* for sex-linkage, the creeper trait in foul for lethality, and vestigial wings in *Drosophila* for multiple alleles: Dodson, *Genetics*, 8-14, 65-67, 85, 88-89.

[154] E.g., Goldschmidt, "Research and Politics," 223.

subsequently defended and incorporated into the language of the self-described "classical geneticists."

In the 1940s-1950s, "classical genetics" was not only a criticism, but also a defense of a dominant tradition in the Anglo-American study of heredity. The defense did not advance a radically different picture of the nature of classical genetics, but rather a different evaluation of how to treat this body of knowledge. Both its critics and its defenders saw classical genetics as an idealized picture of heredity—a mathematical model that almost never mapped perfectly onto the biological reality of heredity. But while those advocating a dialectical ideology and dynamic philosophy of biology took this idealistic simplification as grounds on which to reject classical genetics, its defenders took this as reason to use it as a foundation on which to build a more complex model. It was in the latter terms that "classical genetics" was subsequently incorporated into textbooks, obscuring the radical agenda that had motivated its initial deployments—the political foundation on which future conceptions of the discipline's past era were built.

In the textbooks of the 1950s, classical genetics was defined through its differentiation from developmental and evolutionary genetics. The difference between them was not, however, characterized as the manifestation of essentially different underlying philosophies of genetics, as they had been in Goldschmidt's work. They were, rather, presented as the product of different experimental contexts, differing only in scope: developmental genetics focused on the gene, classical genetics on the individual, and evolutionary genetics on the population. They differed because they looked at different aspects of heredity, not because they were different ways of looking; it was differences in the hereditary phenomena that were responsible for the differences in approach. Genetics had long been divided into branches dealing with three different problems. In a 1938 review of Goldschmidt's *Physiological Genetics*, for example, Coulter had stated: "the three great problems with which geneticists have concerned themselves" are "the problem of the machinery of heredity," "the problem of development," and "the problem of evolution."[155] When geneticists used "classical genetics" synonymously with "the study of hereditary mechanics"—or, "transmission" or

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[155] M. C. Coulter, Review of *Physiological Genetics*, by R. Goldschmidt, *Ecology* 19 (1938).

"Mendelian" genetics—they did not establish a new disciplinary category. But through this usage, "classical genetics" came to be seen as the study of a type of problem; and as an aspect of the structure of genetics textbooks, it was not only a body of science, but also a type of heredity. Thus, "classical" was no longer primarily about historicity.

Although textbooks of the 1950s departed radically from Goldschmidt's conception of classical genetics, their presentation was similar in one respect: the theory of the gene provided a point of reference with which to differentiate classical from physiological genetics. These discussions of the gene originally served as a transition from the classical to the physiological chapters, but expanded into entire sections of the textbook in the 1960s-1970s, as the biochemical study of gene action and structure came to be seen as an independent branch of the science. With this shift, classical genetics came to be defined by its differentiation from molecular genetics, rather than physiological and population genetics. And in the concurrent emergence of the historiography of genetics in the mid 1960s, it was this account that was given. Classical genetics was not defined by a type of problem or type of heredity, but rather by the level at which it investigated heredity. The study of transmission, development, and evolution could be approached from the perspective of either classical or molecular genetics; classical genetics looked at organisms, molecular genetics looked at genes. And it is a variation of this picture that exists in the textbooks, monographs, and journal articles of today, which often define "classical genetics" as "reverse genetics," and "molecular genetics" as "forward genetics."[156]

In this chapter, I have traced the conception and development of the language of the classical in genetics, showing that "classical genetics" cannot be properly understood as a mere idealization projected anachronistically from the modern onto the past. This epochal idea was read back onto a period in which geneticists meant something very

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[156] E.g., W. Brune, M. Messerle, and U. H. Koszinowski, "Forward with BACs," *Trends in Genetics* 16 (2000): 256-257; F. C. Kafatos, "A Revolutionary Landscape: The Restructuring of Biology and Its Convergence with Medicine," *Journal of Molecular Biology* 319 (2002): 862; P. Mormede and B. C. Jones, *Neurobehavioral Genetics* (New York: CRC Press, 1999), 70, 375; F. Vogel and A. G. Motulsky, *Human Genetics*, 3rd ed. (New York: Springer, 1997), 196; and A. J. M. Walhout and M. Vidal, "Protein Interaction Maps for Model Organisms," *Nature Reviews: Molecular Cell Biology* 2 (2002): 56-57. References to "classical" approaches, studies, and analyses are common in contemporary textbooks. See, e.g., Snustad and Simmons, *Principles of Genetics*, 10, 11, 12, 54, 101, 144, 205, 230, 234, 362, 371, 411, 475, 509, 520, 549, 550, 551, 608, 668, 726, 792.

different when describing their work as “classical,” but the “classical” examples of the 1920s and the “classical genetics” of the 1970s were nevertheless substantively connected. Genealogically, I have shown that it was through reappropriations of the idea of classical cases that classical genetics was created. And with respect to function, I have shown that the varied uses of “classical” were alike in one way: as a description of an exemplary case, a type of science, and a branch of genetics, “classical” was a powerful concept in the political language of genetics. At first used to define and establish sources of scientific authority, it was subsequently developed in arguments about the philosophical and ideological character of science, and eventually served to establish the disciplinary identity and boundaries of genetics. These genealogical and functional similarities have not, however, been the sole focus of this chapter.

Tracing the development of “classical” in the language of geneticists, I have also identified significant differences between its particular and context-specific deployments. And it is perhaps this differentiation that is most interesting and important. For when we explore the different senses in which the classical functions, we recognize the myriad ways in which history makes claims on us; and when we stop using the term honorifically, we open up rich historiographical ground. Although references to the classical make use of the authority of the past and function as statements about history, the power of the classical relies on the effacement of its historicity and the conditions in which it has been created and deployed. It is through this tension that the classical gains traction on the present. And it is by analyzing the effacement of its historicity that we understand what we inherit with our senses of the past.

CONCLUSION

The Forms and Functions of History-Writing

> Critical acumen is exerted in vain to uncover the past; the *past* cannot be *presented*; we cannot know what we are not. But one veil hangs over past, present, and future, and it is the province of the historian to find out, not what was, but what is. Where a battle has been fought, you will find nothing but the bones of men and beasts; where a battle is being fought, there are hearts singing. We will sit on a mound and muse, and not try to make these skeletons stand on their legs again.
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> Henry David Thoreau, August 9, 1841.[1]

It is often suggested that the use of history in science textbooks gives life to dry facts, theories, and equations, showing the human side of scientific practice and the excitement of discovery. This view is articulated by scientists commenting on the didactic use of history-writing in textbooks, as well as by those advocating it. In a report on how to improve the teaching of science, for example, a committee of the British Association for the Advancement of Science (BAAS) concluded:

> It is desirable…to introduce into the teaching some account of the main achievements of science and of the methods by which they have been attained…There should be more of the spirit, and less of the valley of dry bones, if science is to be of living interest…Everyone should be given the opportunity of knowing something of the lives and work of such men as Galileo and Newton…Darwin and Mendel, and many other pioneers of science. One way of doing this is by lessons in the history of science.[2]

In this passage, the authors' use of the phrase "the valley of dry bones" is an allusion to a valley described in *The Book of Ezekiel*:

> The hand of the Lord…set me down in the midst of the valley; and it was full of bones; and he caused me to pass by them round about; and behold,

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[1] H. D. Thoreau, *The Journal of Henry D. Thoreau*, ed. B. Torrey and F. H. Allen, vol. 1 (New York: Dover, 1962), 85 (emphasis original).

[2] "Report of the Committee on Science in Secondary Schools," in *Report of the British Association for the Advancement of Science, 1917* (London: John Murray, 1918), 140.

> there were very many in the open valley; and lo, they were very dry. And he said unto me, Son of man can these bones live?[3]

Alluding to this question and answering it, the authors of the BAAS report suggested the valley of dry bones *of science* could be brought to life, and that it was history-writing that could do so.

The view that history-writing in textbooks gives life to a science by breathing life into its past is one that I have challenged in this dissertation. I have shown that the dry bones of science in textbooks are not like those in the valley of *Ezekiel*, but rather like those in what I take to be Thoreau's redescription of it. Whereas the breath of God will give bodies and life to the bones in the valley and transform them into "an exceeding great army," the skeletons of soldiers on Thoreau's battlefield will not "stand on their legs again." According to Thoreau, the historian does not breath life into the past—"the *past* cannot be *presented*." In *representing* the past, we recreate it, and thus do not uncover what we often imagine. With historical reflection, we pull aside the one veil that hangs over our "past, present, and future," and in doing so see ourselves. For although "we cannot know what we are not," we can study who we are, and therein lies the "province of the historian."

If we read Thoreau's radical historiographical claims as descriptive, rather than evaluative—as statements about what history-writing does, rather than about what it can do—they identify a key feature of the types of history-writing that I have explored. In this dissertation, I have shown that formulations of the historical in genetics textbooks were not about the past. Histories of the study of heredity written by Bateson, Castle, and Thomson defined the nature of a science that had not yet cohered. Virtual historical environments were means of socializing students into the scientific culture of their time. The use of history as a logic of organization provided a stable structure in which the conceptual order of the science could be defined and recreated. Claims about "classical" genetics were used in establishing and challenging sources of scientific authority, playing a formative role in the development of the disciplinary identity of genetics. These types

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[3] *The Book of Ezekiel* 37: 1-3, in *The Holy Bible Containing the Old and New Testaments Translated out of the Original Tongues* (Oxford: University Press, 1885). NB: I have chose this version, later known as the "English Revised Version," because it was the standard version in circulation when the BAAS report was written.

of history-writing did not primarily provide accounts of what had been, but rather were arguments and tools used to shape what genetics was and would be.

In identifying and exploring these uses of history, I have suggested that history-writing played an important role in the formation and disciplinary development of genetics. This was the case, at the very least, because of the ways in which history-writing was connected to the pedagogy and reproduction of genetics: insofar as textbooks were central to the teaching of genetics, history-writing was central to the development of geneticists. But, as Fleck has suggested, it is not only the textbook's functions at the beginning of the life of a scientist that make it important. As one of the few places in which the heterogeneous aspects of a field are brought together and presented as a discipline, textbooks are also a key form of scientific literature. Thus, they should not be understood as repositories into which science is deposited, nor as artifacts that are only valuable because they allow us to trace the assimilation and acceptance of scientific advances. To develop a complete picture of where science happens, we need a symmetrical analysis that treats textbooks as sites of production. In this dissertation, I have begun to develop this line of analysis with respect to four types of history-writing. But, as is inevitable, there were many questions that I left unanswered; to conclude, I will briefly discuss some of these as potential areas for future research.

The dissemination of ideas and facts from textbooks to scientific practice is in need of further study. In my analysis of geneticists' uses of the "historical approach," I argued that a construction of history was central to the development and presentation of the conceptual order of genetics; but I did not explore if and how this conceptual order was subsequently incorporated into journal science. While it would likely be difficult if not impossible to prove that a textbook was the origin of a particular constellation of evidence and theory, a comparative study of textbooks and journals could certainly suggest some such connections. It would be valuable to know, for example, if and how the use of "Mendelian" in articles changed as the historical approach became standard in textbooks, and whether there was a reordering of facts and theories related to the chromosome theory in journal science during the second quarter of the twentieth century.

Focusing on the transfer of ideas from textbooks to journals would also help further an analysis of "classical genetics." In my investigation of the development and use of this term, I was able to show that it was shaped by geneticists who worked to pioneer new branches of genetics, as well as by those who tried to incorporate these advances into the standard textbooks of the 1950s-1970s. Given that this language with wide circulation in journal science was in part formed in textbooks, it would be worth looking at how the "classical genetics" of textbooks related to that of journal science in the late twentieth century. The relationship between the "classical" of geneticists and that of physicists also deserves attention. Considering that the historiographical use of "classical genetics" was informed by a sense of the historical development of physics, further study would not only clarify the nature and extent of this connection, but also the more general ways in which geneticists modelled their science on physics.

In addition to looking at how history-writing in textbooks influenced journal science, it would be valuable to explore how these uses of history were shaped by broader culture. This approach would be particularly useful in extending my analysis of virtual historical environments and textbook histories. With respect to the use of virtual historical environments, one of the most interesting questions that I left unanswered concerns the cultural origins of this use of history-writing. Although I found that this pedagogical development coincided with a broader trend of interest in historical and case-based approaches to teaching, I was unable to identify a casual relationship between the two. As I discussed, there has been significant research—in departments of history, sociology, and education—on the uses of historical lessons and "discovery learning" in science education in secondary schools; and the use of case-studies in the teaching of science to non-scientists at Harvard has been the focus of several studies. But I have been unable to find any historical research that identifies the pedagogical ideals that shaped the teaching of university science for scientists. This line of research would be valuable, as it would contribute to a more complete picture of the communication circuit that informed and shaped the scientific functions of history-writing in textbooks.

Regarding explicit accounts of the history of genetics, there is much work to be done in comparing textbook histories with the sources on which they were based.

Whereas authors such as Thomson and Castle regularly discussed and quoted from primary sources, most authors of later textbooks rarely did. Instead, they relied on secondary sources, and their accounts of history can thus be explored as reconstructions of historiography. By studying what went into textbook histories, and what was excluded, it would be possible to see how history-writing was used to shape the science long after accounts of its history had become relatively standardized. In this regard, it would be important to explore why most authors did not use and refer to a rich body of historical scholarship, as Thomson and Castle had. It seems likely that this was in part because the history of the science had come to be thought of as a matter of common knowledge; if so, it would be worth looking at how this common sense account was formed. In general, particular attention should be given to the textbooks written after Dunn, Sturtevant, Ravin, and Carlson published accounts of the history of genetics around 1965.[4] These monographs allow for a valuable form of comparison between the textbook histories and the historical scholarship of geneticists. Of special interest are the histories written by Dunn and Sturtevant—not only because these authors also wrote widely-circulated textbooks, but also, and more so, because their textbooks were paradigms of the competing historical and ahistorical approaches to teaching. It is not, however, just the possibility of comparative analyses that makes the histories of Dunn, Sturtevant, Carlson, and Ravin interesting sources for further study. Because these monographs were seen as a sign of the "twilight of a generation whose accomplishments are embodied in this history," a study of their reception would enrich our understanding of geneticists' attitudes about the place of history-writing in science.[5] Lastly, in addition to looking at historical accounts written by geneticists, it would be worth looking at those written by historians. Comparing geneticists' textbook histories with historians' revisionist histories would help clarify if and how textbook authors read and dealt with critical historical scholarship.

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[4] L. C. Dunn, *A Short History of Genetics: The Development of Some of the Main Lines of Thought, 1864-1939* (New York: McGraw-Hill, 1965), A. W. Ravin, *The Evolution of Genetics* (New York: Academic Press, 1965), A. H. Sturtevant, *A History of Genetics* (New York: Harper & Row, 1965), E. A. Carlson, *The Gene: A Critical History* (Philadelphia: Saunders, 1966). Note that drafts, notes, and correspondence regarding Sturtevant's history are part of the A. H. Sturtevant Papers at California Institute of Technology.

[5] J. Schultz, "Innovators and Controversies," Review of *The Gene: A Critical History*, by E. A. Carlson, *Science* 157 (1967): 296.

In concluding, I should note that I hope my analysis will not only be valuable for historians of science, but also for those interested in the general nature and functions of history-writing. As I have identified and explored the ways in which history was written in genetics textbooks, I have gradually pushed the boundaries of the concept of history-writing, challenging common sense notions of what can be involved in, and what should be counted as, the writing of history. I have shown that history-writing did not only take the form of *writing-about-history* in lessons about the past. In addition, history was *written-into* textbooks as virtual historical environments used in the enculturation of students. Authors were *writing-historically* when presenting the conceptual order of the science. And they *wrote-with-history* when using the disciplinary category of the classical. Thus, history-writing should be seen as a complex conjunction, and narrative history-writing as just one of many ways in which the past is put on paper. In recharacterizing "history-writing," I have also questioned what we often consider to be the nature of the "history" and of the "writing" of it. I have argued the *writing* of history in textbooks did not only take place in historical introductions, nor was it merely the inscription of discursive text on paper. Rather, it included the creation of the frontispiece, the diagrams and illustrations, the problem-solving exercises, the structure holding the chapters together, and the concept of the classical. In addition to providing descriptions of the past, writing was used to map and order the world in which the geneticist stood. For these reasons, we must reconceptualize the nature of the graphic component of history-writing—of where and what it is. In addition, my analysis has shown that with each type of history-writing, "history" was constituted in a different manner. In narratives about "the history" of genetics, the past was treated as a body of information. With exemplars, past experiments were turned into mirrors of present scientific practice. When authors used "the historical approach," they turned history into an ideal. And in the creation and development of the concept of the "classical," a sense of the past became part of the disciplinary identity of genetics. Thus, history like writing appeared in a myriad of ways. In identifying some of their forms and functions, I hope to have provided a starting point for further historiographical research on the subtle yet powerful ways in which the historicity of our past can make claims upon us.

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