Mutant utopias: evening primroses and imagined futures in early twentieth-century America


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Mutant Utopias

Evening Primroses and Imagined Futures in Early Twentieth-Century America

By Jim Endersby*

ABSTRACT

Hugo de Vries’s mutation theory is now little more than a footnote to the history of biology, a failed theory that briefly led a few biologists astray. However, for the first quarter of the twentieth century it attracted considerable attention from both professional biologists and laypeople. De Vries’s theory—together with the plant, Oenothera lamarckiana, that had supplied most of his evidence—became the focus of a surprising variety of imaginative hopes. Scientists and their various publics were fascinated by the utopian possibilities that the primrose seemed to offer, and their discussions shaped a public culture around biology that would help define the twentieth century as the “century of the gene.” From a conventional history of science perspective (which, in the case of twentieth-century biology, often remains focused on the content of scientific theories and the professional communities that shaped them), the mutation theory seems unimportant. However, while De Vries’s new theory of evolution ultimately failed to persuade the scientific community, it was much more important than is now realized, particularly because it helped make biology part of a wide variety of public debates. Understanding the mutation theory’s story more fully suggests that we may need to rethink much of the rest of the century of the gene’s history, to think less in terms of what happened in the lab and more about how biology came to function as public culture.

IN 1900, THREE EUROPEAN BOTANISTS “rediscovered” Gregor Mendel’s work and began what would become the “century of the gene.” One of the rediscoverers was the Dutch botanist Hugo de Vries (see Figure 1), who would probably be irate to learn that,

* Department of History, University of Sussex, Brighton BN1 9SH, United Kingdom.

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1 Evelyn Fox Keller, The Century of the Gene (Cambridge, Mass.: Harvard Univ. Press, 2000). The story of the rediscovery of Mendel’s work is much more complex than traditional histories suggest, but these complexities are not relevant to the present essay. For more details see L. C. Dunn, “Xenia and the Origin of Genetics;”

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insofar as he is remembered at all, it is for something he considered relatively unimportant, at least by comparison with his own work. He would doubtless have preferred to be immortalized as the author of *Die Mutationstheorie* (1901, 1903), in which he announced a radical new conception of evolution that he was convinced marked the completion of Charles Darwin’s work.

At the beginning of the twentieth century many biologists, including some Darwinists, were losing confidence in Darwin’s version of evolution. Darwin had argued that the small, everyday variations in plants and animals were sifted by natural selection over millions of years. Variations that were harmful to the organism were eliminated, while those that were beneficial gradually accumulated until eventually new species emerged. Some critics pointed out that late nineteenth-century physics implied that the Earth could...
not be old enough for Darwin’s stately dance of the species to have produced the variety of life we see around us. Others felt convinced that although selection might prune undesirable traits from a species, it could not generate genuine novelty; how, for example, could the small variations in the traits of a wingless species eventually transform it into a flying one? One response to such criticisms was to focus on the rare but dramatic changes, known as “sports” or “saltations,” that occasionally appear in every species; these could rapidly produce new forms that were radically different from their parents. Nevertheless, few believed that saltations could be the main source of evolutionary change, since the new types were always rare and would therefore be forced to interbreed with the more common, unimproved variety; as a result, any radical novelties would inevitably be swamped.3

De Vries’s mutation theory was a response to these objections. In an earlier book, *Intracellular Pangenesis* (1889), he had offered a hypothetical physical mechanism of heredity that drew on Darwin’s theory of pangenesis. De Vries posited the existence of what he called “pangenes,” minute particles in the cell nucleus that carried an organism’s traits down the generations. He had high hopes for his theory, but it was largely ignored; the few reviews were generally negative and noted the almost total lack of experimental evidence to support the hypothesis. This was a damning criticism at a time when speculative new ideas were expected to be based on experimental evidence derived from rigorous laboratory experiments rather than the more anecdotal field observations typical of nineteenth-century natural history.4 The mutation theory was De Vries’s second attempt to present his idea, but intracellular pangenesis—having been such a failure—was no longer mentioned explicitly. Instead, De Vries presented experimental evidence that species did in fact change rapidly and dramatically. After years of experiments with the plant species *Oenothera lamarckiana*, a member of the evening primrose genus, he argued that the plant provided clear evidence that Darwin and his latter-day followers were wrong: evolution did not happen slowly, through a long series of small, gradual steps. De Vries believed that he had shown, instead, that evolution occurred through rapid leaps: “new species originate suddenly, without preparation or intermediate forms.” As a result, natural selection played only a minor role in evolution, since “once formed, the new species are as a rule at once constant. No series of generations, no selection, no struggle for existence are needed.”5 De Vries argued that no one had noticed this important phenomenon before because organisms were stable most of the time, only rarely entering into what he called a mutation period. During this time, as *O. lamarckiana* illustrated, numerous new forms were generated, and their simultaneous appearance in comparatively large numbers meant that they avoided being swamped. It was not clear what stimulated


5 Hugo de Vries, “The Origin of Species by Mutation,” *Science*, 1902, 15(384):721–729, on pp. 723–724. During the early twentieth century, many naturalists retained the earlier practice of capitalizing species names that were derived from proper nouns—hence *Oenothera Lamarckiana* (the species having been named in honor of Jean-Baptiste Lamarck). I have retained this original capitalization in quotations, while using the modern form elsewhere.
these periods, but De Vries believed it was some factor in the external environment; in Species and Varieties (the first book-length, English-language presentation of his theory), he suggested that O. lamarckiana might have begun to mutate in response to being transplanted from America to the new environment of Europe. Whatever the unknown environmental changes were, they produced essentially random mutations, but these new species produced by mutation (as opposed to the everyday minor variations within a species) were then subject to natural selection. Of these new species, the best adapted would survive, thus producing the apparent progress visible in the fossil record. The natural tendency for organisms to mutate and then be improved by selection could be accelerated once the causes of mutation had been understood; new mutations could then be artificially induced and selected to suit human needs.

These claims caused a scientific sensation, and over the following years biologists around the world tried to repeat De Vries’s Oenothera work and apply it to other plants and animals. Historians of biology have long known that the mutation theory provoked lively debate among early twentieth-century American proponents of experimental evolution, yet they tend to describe it as a failed theory, little more than an obstacle in the path of progress toward the modern evolutionary synthesis, the central event around which histories of twentieth-century biology are usually organized. Seen from this perspective, mutation theory seems unimportant, and as a result its reception and public impact have scarcely been considered. However, if we ignore most of what went on in the labs and experimental gardens, and focus instead on what various people imagined mutation might mean, it becomes clear that the evening primrose and the theory it embodied are of vital importance in understanding the history of twentieth-century biology. During the early decades of the century, particularly in the United States, interest was so intense that De Vries became one of the world’s most famous living scientists. His claims and their significance were widely discussed in print—in both specialized and popular scientific journals, as well as in newspapers and magazines of all kinds—and much of the reporting centered on O. lamarckiana, which provided the bulk of the experimental evidence for the theory. The flower became the focal point of a rich landscape of ingenious hopes linked by a distinctively American utopianism, the drive to find rapid technological solutions to social and economic problems. The

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6 E.g., in his remarks at the opening of the Cold Spring Harbor Laboratory, De Vries commented that “it is probable that the degree of mutability . . . is more or less dependent on the outer life-conditions”; Hugo de Vries, Species and Varieties: Their Origin by Mutation: Lectures Delivered at the University of California (Chicago: Open Court, 1905), p. 48.


9 There is a huge literature on utopianism, but the American version I have in mind is discussed in Kenneth M. Roemer, The Obsolete Necessity: America in Utopian Writings, 1888–1900 (Kent, Ohio: Kent State Univ. Press, 1976); and Howard P. Segal, Technological Utopianism in American Culture (Syracuse, N.Y.: Syracuse Univ. Press, 2005).
disparate American reactions to the theory were linked by the plant’s possibilities: for a couple of decades, scientists and their various publics imagined the future in terms of what the primrose might offer. For much of the twentieth century, there was a public culture of biology, haunted by utopian hopes as much as by dystopian fears; I will argue that the evening primrose played an important and previously unrecognized part in shaping this culture.

To understand the impact of the mutation theory fully, it is helpful to ignore (temporarily at least) the distinction between “elite” and “popular” science (the very term “popular science” is problematic, as we shall see). In recent years, historians of Victorian science have shown how elite and popular science created and defined each other at a time when the boundaries between amateur and professional were fluid and ill-defined; as long as it remained unclear who the real experts were, the involvement of laypeople in science—whether as direct participants or as consumers of various genres of writing about science—remained a vital part of the making of scientific knowledge. By contrast, there is much less comparable literature on the twentieth century, perhaps because it is widely assumed that the fluidity that characterized the nineteenth century had largely ended; by the early twentieth century professionalization was complete and a stable distinction between elite and popular science existed that largely excluded lay engagement. The story of the mutation theory’s reception serves as a reminder that while the lay/expert distinction was a real (and important) one, it was anything but stable. During the early twentieth century, mutation theory was one of several rival theories of inheritance and evolution (including Mendelism, orthogenesis, neo-Lamarckianism, and biometrics), all vying to overcome the perceived deficiencies of orthodox Darwinism. No one knew which, if any, of these rivals would eventually become accepted, institutionalized sciences. A new science is constructed around a community united by a range of practices, which range from formulating new theories to working with new experimental organisms. Whenever such a science begins to assert its claim to full scientific status, the very novelty of its claims may make it difficult to know who is best qualified to judge their validity. This was precisely the situation that mutation theory helped to create: it illustrates how the boundary between experts and laypeople that had been established by the professionalization of biology remained porous and ill-defined. Indeed, it could be argued that the sort


12 Peter Bowler makes this point in the introduction to his recent book, Science for All, which provides an invaluable overview of elite scientists who continued to write for broad audiences in the twentieth century but does not address the public’s continued role in the making of scientific knowledge; see Peter J. Bowler, Science for All: The Popularization of Science in Early Twentieth-Century Britain (Chicago: Univ. Chicago Press, 2009). The narrative of the “professionalization” of science in the late nineteenth and early twentieth centuries has been subjected to considerable criticism; for an overview see Jim Endersby, Imperial Nature: Joseph Hooker and the Practices of Victorian Science (Chicago: Univ. Chicago Press, 2008), pp. 1–30.
of fluidity and confusion that characterized biology in the early decades of the twentieth century is recreated whenever a significantly novel science first presents its credentials and seeks admission into the club of respected, elite sciences. Such boundaries typically do not serve as permanent barriers but exist to regulate the ever-changing movement of people and ideas across the borderline.13

Moreover, I will argue in what follows that in the twentieth century, even more than in the nineteenth (thanks, in large measure, to the changing nature of the news media), the success of each novel science’s application for proper scientific status depended to some extent on the public’s perception of it. Was the new science exciting? What would it change? And, above all, would it make the world better—or worse? These questions had to be answered by various communities that were primarily consumers, rather than producers, of scientific knowledge—politicians, policy makers, and interested laypeople, upon whose (usually tacit) support funding often depended. These audiences generally read about recent scientific work in newspapers, popular scientific journals, or general interest magazines, rather than in specialist scientific periodicals. This growing reliance on nonspecialist media was in part a reaction to scientific periodicals becoming increasingly specialized and incomprehensible (even to scientists within other specializations). Hence, both twentieth-century scientists and their various publics were increasingly likely to come across radically new ideas in general, rather than specialist, publications. This is a further reason why elite and lay attitudes to the mutation theory need to be considered simultaneously, not least because—being proponents of a really new science—the mutationists had no specialized journals, any more than they had institutions, degree programs, university chairs, or any of the other mechanisms that demarcate the boundaries of an established science. These complexities are explored in what follows by analyzing Luther Burbank’s role in the mutation story: the U.S. nurseryman and “plant wizard” was well known to the public, so both proponents and opponents of the mutation theory tried to persuade him to endorse their view. Burbank is a useful lens through which to view these debates because his methods relied on the identification of comparatively rare new forms he called “sports”—and De Vries’s claims invariably recalled these sports, especially for American audiences. In addition, Burbank straddled the boundary between elite science and practical know-how, and his high profile made him a potentially valuable ally in an age increasingly concerned with publicity. Finally, he was an American, and he epitomized the ties to the land that, for many, exemplified the best kind of American; given that De Vries, his plant, and his theory were all somewhat foreign, allying them with Burbank’s native genius was an attractive strategy. However, the attempt to enlist the self-educated Burbank in these debates resulted in further blurring of the boundaries between lay and expert.

In analyzing the diverse uses to which mutation theory could be put, it is unhelpful to try to distinguish between “scientific” and “popular” understandings; to do so risks prejudging the answers to key questions, such as: Where, how, and by whom are the meanings of science determined? Instead, the wide variety of people with an interest in the theory and in Oenothera, which came to embody it, are better understood as being grouped into a range of overlapping communities, each characterized by one or more common practices, including growing and selling plants, making experiments, devising theories, interpreting them for various publics, reading about them, and imagining the futures such

13 I am indebted to Robert Kohler for this observation: personal communication via email, Sept. 2011.
discoveries might promise. These groupings are deliberately vague. For example, some interpreters of science could be fairly characterized as “journalists” or “science writers”; however, this would imply that they pursued writing as a paid profession, whereas in most cases we do not know why they wrote or whether they were paid. Moreover, the interpreters included some professional scientists, including De Vries himself, who either wrote for newspapers or chose to give lengthy interviews to them. Hence the choice of “interpreter” as a label, which characterizes such people by their practice—that of explaining science to non-specialists—rather than by their scientific status or how they earned their living. The growth of both science and the newspaper market increased the need for such interpreters, but, as with so many aspects of this story, no one was sure who was qualified for this work: interpreters and their audiences possessed varying degrees of scientific expertise from which they constructed their understandings of mutation. As a result, much of what was written about Oenothera and mutation may appear confused, not least in the writers’ willingness to link ideas that are now regarded as incompatible.  

Instead of trying to separate the “right” from the “wrong” approaches, it is more useful to recognize that the various kinds of writing about primroses and mutations were composed by freely combining seemingly incongruous facts and interpretations from both expert and lay sources, so that specialized scientific journals became sources for socialist speculations at the same time as the utopian hyperbole of journalists inflected researchers’ sober reports to their funding bodies. Each of these communities made its own contribution to the meanings of the evening primrose and, in the process, began to define the genres that were appropriate for writing about biology. The result was a kind of mutant medley, a symptom of biology’s mutability at the time. The mutation theory’s reception made utopianism a key part of the way biology became public culture in the twentieth century.

INTERPRETING MUTATION

Because mutation theory was a new science, no stable community of scientific experts yet existed who could judge it. As a result, both the meaning of the theory and the precise contours of the boundary between lay and expert were negotiated through the process of reporting and interpreting the new theory. The nature of these negotiations becomes clearer when we consider where the first report of De Vries’s ideas in the United States appeared: not in a scientific publication, but in the popular family magazine the Youth’s Companion, on 1 August 1901. The report was brief and is quoted here in full:

NEW LIGHT ON THE ORIGIN OF SPECIES. Prof. Hugo de Vries, the well-known botanist and biologist, is credited with a “momentous discovery” concerning the origin of species among plants. Briefly stated, his observations indicate that new species appear suddenly by mutation, never as the outcome of a progressive variation. He avers that he has been able, for the first time, to watch the formation and development of new species. A reviewer of his work in the English scientific journal, Nature, says: “The facts are so striking and convincing that an

14 E.g., many who were interested in mutation theory were equally interested in neo-Lamarckianism (the idea that acquired characteristics can be inherited). This seems particularly incoherent to historians of biology because De Vries (being one of Mendel’s rediscoverers) should be within the “winning” camp in the supposed battle against Lamarckianism. But this understanding, once again, results from the way the modern evolutionary synthesis is allowed to shape biology’s history, since it has been claimed that vanquishing “soft” Lamarckian inheritance was a key step on the way to the synthesis. See Ernst Mayr, “Prologue: Some Thoughts on the History of the Evolutionary Synthesis,” in The Evolutionary Synthesis: Perspectives on the Unification of Biology, ed. Mayr and William B. Provine (Cambridge, Mass.: Harvard Univ. Press, 1998), pp. 1–48.
outsider, like the reviewer, cannot but feel that a new period in the theories of the origin of species and of evolution has been inaugurated.”

This paragraph encapsulated many themes that were to become typical of the way mutation theory was reported: it emphasized De Vries’s stature (“the well-known botanist and biologist”); it asserted that evolution was a fast rather than a slow process (“new species appear suddenly”); it suggested that Darwin had been wrong (evolution was never “the outcome of a progressive variation”); it noted that evolution could now be observed directly (De Vries had been able, “for the first time, to watch the formation and development of new species”); and, most strikingly, it took information from a specialized scientific publication (Nature) and interpreted it for a nonspecialist audience. However, the process of interpretation cannot be described as simply “diffusing” elite scientific knowledge to a lay audience; the Youth’s Companion report makes it clear that Nature’s reviewer was “an outsider,” which potentially undermined the writer’s authority to judge the significance of De Vries’s “momentous discovery.”

Nature had to employ an outsider to write the report because there were as yet no “insiders”; De Vries’s claims were so novel that it was not yet clear who could be expected to evaluate them. Thus, from the moment the mutation theory reached the public’s attention, it forced them to question the lay/expert boundary.

However, the most obvious point to be made about the Youth’s Companion report is the assumption that an esoteric biological theory was of sufficient public interest to be reported in a nonspecialist publication. This supposition was shared by the many other newspapers and magazines that reported the theory over the next few decades, and an analysis of their accounts of the mutation theory illustrates the ways in which the new theory began to set the terms in which other issues, from anti-Darwinism to eugenics, would be debated. Many reports of the new theory included several of the themes or topics present in the Youth’s Companion piece, and these illustrate how mutation theory began to shape a biological public culture. For example, it was repeatedly claimed that Darwin was wrong: in 1904 the San Francisco Chronicle (see Figure 2) told its readers that “there is no evolution such as Darwin taught, in nature, and that the change from one species to another is immediate and abrupt. The law of nature says de Vries, is not evolution but mutation.”

The claim that De Vries had proved Darwin wrong prompted much excitement, and numerous writers made it, but—as will be shown below—they used it in the service of a diverse variety of often-contradictory claims, from bolstering Christian antievolutionism to discrediting eugenics.

Many reports illustrate a second theme: excitement about the claim that new species formed more rapidly than Darwin ever envisaged, that the changes seemed more revolutionary than evolutionary. One result was that some Marxists and other progressives interpreted De Vries as having given scientific blessing to the cause of political revolution. The socialist interpreter of science Arthur M. Lewis argued that De Vries had shown that “it is no longer necessary to assume countless millions of years for the evolution of living

15 “New Light on the Origin of Species,” Youth’s Companion, 1901, 75(31):387. This, at least, is the earliest reference I have found; I would be grateful to hear of others.

16 The report appeared as an editorial under the title “Recent Scientific Work in Holland” on 27 June 1901 (Nature, 1901, 64[1652]:208–209) and was signed simply “J.P.K.” I have been unable to identify the writer.

17 J. Theo Wilson, “Evolution’s Worst Knock,” San Francisco Chronicle, 2 Oct. 1904, p. 3. I have not been able to discover anything about J. Theo Wilson, and this appears to be the only article he ever wrote for the Chronicle.
forms. A plant enjoys a period of apparent stability, then it reaches a point where it ‘explodes’ and gives birth to new species. If a plant, why not a society?” He concluded that “there is therefore, no longer anything in biological science to contradict the Socialist position that a new society may be born of a sudden revolution.”18 The idea of overnight transformation was taken a step further by the socialist and feminist writer Charlotte Perkins Gilman, whose utopian novel *Herland* (1915) depicted a world inhabited only by women who reproduced parthenogenetically, a situation explained by one of the novel’s protagonists as resulting from “the law of mutation.”19

Meanwhile, the idea of rapid evolution created utopian hopes among the scientific community that were as dramatic as those stirred among socialists and feminists. For scientists, De Vries’s claim of rapid evolutionary change meant that it could be studied under laboratory conditions. This led directly to the Carnegie Institution of Washington founding the Station for Experimental Evolution at Cold Spring Harbor, Long Island. At the opening ceremony the station’s first director, Charles Benedict Davenport, described De Vries’s book *The Mutation Theory* as “the most important work on evolution since

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19 Charlotte Perkins Gilman, *Herland* (1915; New York: Pantheon, 1979), p. 77. Given the date, “law of mutation” must be a reference to De Vries, not Mendel; however, analyzing the extent and nature of Gilman’s debt to the mutation theory, which does not appear to have been previously recognized, would require more space than is available here. I will return to this subject, and to the wider question of politically radical readings of mutation, in a future article. See also Pittenger, *American Socialists and Evolutionary Thought, 1870–1920*, pp. 84–87.
Darwin’s ‘Origin of Species,’ a work destined to be the foundation stone of the rising science of experimental evolution.” To men like Davenport, the idea that evolution could be observed in a laboratory suggested that once the mechanism of mutation was understood, it might be possible to induce artificial mutations and thus create new types of plant or animal.20 My concern here is not with the details of the scientific work nor with the complex story of why Oenothera was eventually abandoned by the biologists—fascinating though these are—but with the ways in which they were reported; whether or not it was a realistic hope, the prospect of rapidly creating new types of plants became a third major theme in the press coverage of mutation.21 For example, the New York Times regularly reported on the work of Daniel Trembly MacDougal (see Figure 3), De Vries’s most enthusiastic U.S. supporter. According to one such report, it was “entirely within the range


21 What De Vries called the species O. lamarckiana is no longer recognized as a species by most botanists but is instead regarded as an unusual form of hybrid with a number of unusual genetic characteristics; as a result, the results obtained from Oenothera could not easily be generalized to other species. For a brief overview of the story see Jim Endersby, A Guinea Pig’s History of Biology: The Plants and Animals Who Taught Us the Facts of Life (London: Heinemann, 2007), pp. 128–169. For a detailed overview of the subsequent history of cytological and genetic research into the plant see Ralph E. Cleland, Oenothera: Cytogenetics and Evolution (London: Academic, 1972).
of possibilities that [MacDougal’s] methods may be so extended as to enable man, the conscious organism, to control and direct the evolution of the entire organic world.”

So, although the content of their specific dreams differed, expert and lay audiences shared the conviction that mutation theory promised a better world; these very different hopes were components of a single public biological culture.

The utopian prospect of new plants was repeated often, not least because the announcement of the mutation theory coincided with a period of record immigration into the United States. As Europe’s huddled masses poured in, some Americans wondered whether their country could continue to feed its rapidly growing population. Such fears may have been exaggerated (perhaps deliberately, in order to justify increased research funding), but there is no doubt that some genuinely perceived a need to improve their crop plants and animals more rapidly than traditional methods allowed. For example, in 1902 Beverly T. Galloway, head of the U.S. Department of Agriculture (USDA) Bureau of Plant Industry (recently established and charged with using botanical science to improve America’s crops), told the American Association for the Advancement of Science that “as population increases and competition in all lines of agricultural production becomes keener, the need for securing plants better adapted to certain conditions and which can be produced at a minimum expense, will become greater and greater.” The urgent need for such plants was clear because, as the association had been told repeatedly, “within a comparatively short time the United States will not be able to grow the amount of wheat, and possibly other cereals, needed for consumption.”

Galloway’s widely shared fears help us understand the widespread attention paid to Oenothera; it seemed to promise precisely what America required—a rapid route to new and improved plants—just when it was supposedly needed most.

Mass immigration may also have provided the context for a fourth theme that emerged in discussions of the promising primrose: the question of whether Oenothera lamarckiana was a native American plant. When De Vries first visited the United States in 1904 he drew his audience’s attention to the fact that “the species which yielded these important results is an American plant. It is a native of the United States.” As we shall see, Oenothera’s American identity played a role in the initial acceptance of the new theory; however, the theory’s opponents not only disputed the plant’s native origins but—by asserting that it was a foreign-born hybrid—proclaimed that it was not a species at all.

There is, of course, no necessary link between the purity of a species and its indigenousness, but it is striking how regularly contemporary sources conflated the two. Discussions of the plant and its significance frequently touched on themes of purity, hybridity, and degeneration as if these were identical. At the time, writers of all kinds regularly used the term “race” to indicate a species or variety of plant or animal, which perhaps encouraged the tendency to apply lessons drawn from Oenothera directly to people. Reading the often-heated debates regarding the plant’s ancestry and purity elicits the elusive but persistent sense of a shift back and forth between politically charged debates about the desirability of cross-breeding different races of people and those about hybridizing plants—issues united by the question of how to produce an improved stock.


Tracing the rich diversity of these responses is possible only because Oenothera itself was central to the reporting, becoming a synecdoche for the mutation theory and the various hopes it was seen to embody. The regularity with which the nonspecialist press referred to the plant suggests that the image of a revolutionary, almost magical, flower that gave birth to new species overnight had captured the public’s imagination. The advent of the mutation theory coincided with the most rapid expansion in the history of the American newspaper industry; by 1920, every home in America received on average at least one newspaper. The geographical and political range of newspapers that covered the mutation theory is striking. In addition to journals aimed at children (e.g., the Youth’s Companion) or at farmers (such as the Ranch [Washington]), reports appeared in prestigious dailies (Chicago Daily Tribune, New York Times, Washington Post) and local newspapers (Ogden Standard [Utah], Muskogee Cimeter [Oklahoma]). The theory was reported in journals with a wide variety of political and religious affiliations (the Catholic World [New York], the Reformed Church Review, the Independent, the International Socialist Review, the Nation). Naturally, reports appeared in numerous publications that explicitly aimed to publicize and explain science to lay audiences (Popular Science Monthly, Scientific American), but they were also found in general interest popular magazines (Atlantic Monthly, Harper’s Monthly Magazine). And, of course, many specialist scientific publications discussed the theory.

The coverage in these publications varied as much as their denominational, geographical, and political affiliations; some stories were little more than brief summaries, while others offered detailed expositions extending over several pages. Some publications enlisted experts (including De Vries himself) to interpret the new theory, while others simply lifted items directly from other papers. In what follows I will analyze a selection of the reports, grouping them according to the main topics that characterized accounts of the mutation theory in order to illustrate the extraordinary richness of the ideas presented. What unites many of the reports, despite their obvious diversity, is that the sources from which the stories were assembled were as diverse as the publications in which they appeared, and the arguments made by the writers were even more varied. Proponents of the mutation theory did not form a stable, professional community; instead, the theory allowed biology to function as a kind of playground within which various writers could exercise their imaginations.

25 The use of the plant’s Latin name in these publications helps the historian to track references to the plant and, thus, the theory in the press (whereas the word “mutation” occurs too often in unrelated contexts), which helps reveal the scale of interest in the theory. Oenothera thus serves as a “cultural tracer” of the kind outlined by James A. Secord, for whose work I am most grateful; see Secord, Victorian Sensation (cit. n. 11).


27 Thanks largely to digitization projects such as ProQuest Historical Newspapers and the U.S. Library of Congress Chronicling America Project (http://chroniclingamerica.loc.gov), the resources for examining the broad impact of a scientific theory have never been more readily available. The following periodicals can be added to those already mentioned: Call (San Francisco), Chicago Defender, Los Angeles Herald, New-York Tribune, Perrysburg Journal (Ohio), Intermountain Catholic (Utah), St. Louis Republic, Saint Paul Globe, Salt Lake Herald, San Francisco Chronicle, Savannah Tribune, Weekly Tallahasseean, Valentine Democrat (Nebraska), Washington Times (D.C.), Washington Herald (D.C.). Magazines include the Atlantic Monthly, Current Opinion, Forum, Harper’s Weekly, the Independent, the Open Court, the Outlook and Independent, the Scientific Monthly, and Wilshire’s Magazine. This preliminary list is doubtless incomplete, and I would be grateful to receive additional citations.
DISPROVING DARWIN

Some of the ways in which mutation theory shaped public culture are illustrated by its effect on the public’s understanding of Darwin’s theories, which had been a matter of considerable general interest in the United States ever since they were first published.\textsuperscript{28} As previously mentioned, at the beginning of the twentieth century the fact of evolution (i.e., the claim that extant species were derived from earlier ones by purely natural means) was broadly accepted, especially within the scientific community. However, there was less consensus as to the precise mechanism that caused evolution. De Vries saw himself as completing rather than replacing Darwin’s work, and some accounts of his work in the nonspecialist press made that clear.\textsuperscript{29} This was a comparatively rare interpretation, however, perhaps because the claim that Darwin had been “proved wrong” sold more newspapers. The \textit{San Francisco Chronicle}’s 1904 report that “the Darwinian theory of evolution is flatly contradicted by Hugo de Vries” was summarized by the writer as “MAN didn’t slowly evolute from the monkey after all.”\textsuperscript{30} Nothing De Vries said could conceivably support this bizarre view, yet for some U.S. journalists and, presumably, many newspaper readers the idea that “Darwin was wrong” must mean that the apes had been pruned from our family tree.

It was not just journalists who used mutation theory to challenge the validity of Darwinism. The same strategy was employed by the Catholic priest Edwin Vincent O’Hara (later to become one of the more prominent bishops in the United States). He cited the work of De Vries and others to demonstrate that biologists were rejecting natural selection and exulted, “Its foundation gone, what is the Darwinian world view but a castle in the air?” Writing in the \textit{Catholic World}, O’Hara cited various scientific authorities to show that doubts about the efficacy or sufficiency of natural selection had been around for several years even before “the greatest revolt against Darwinism, led by de Vries.” He concluded that “Darwin denied the stability of species; de Vries affirms that species are ‘like invariable unities.’ It is evident that there is no important point upon which the theories are not mutually exclusive.”\textsuperscript{31} If, as De Vries said, species were “invariable,” their sudden transformations seemed miraculous—and perhaps they were.


\textsuperscript{30} Wilson, “Evolution’s Worst Knock” (cit. n. 17), p. 3.

the scientific community (in this case, the various attempts to address Darwinism’s perceived limitations) to argue that evolution itself was under attack.

Although some members of the scientific community tried to correct what they saw as the deliberate misappropriation of mutation theory, the link between that theory and antievolutionary sentiment was not easy to break. In 1905 *Popular Science Monthly* quoted the celebrated American horticulturalist Luther Burbank (whose complex relationship with De Vries and mutation will be discussed in more detail below) as saying that “the mutation theory of the origin of species seems like a step backward towards the special creation theory.”32

The advent of the mutation theory coincided with the rise of laboratory-based experimental biology in the United States, a development that was sometimes perceived as a threat by the established community of traditional field naturalists.33 In this context, some saw the new theory as vindicating the superiority of laboratory-based experimental evolution over the sort of older, field-based natural history that Darwin exemplified; hence the perception that its opponents were anti-Darwinian, despite their having no doubts about the fact of evolution. The *San Francisco Chronicle*’s report reflected these debates when it stressed that De Vries, unlike Darwin, based his conclusions on “eighteen years of careful experiment.” Like many other articles, this one emphasized the rigor of De Vries’s work, describing his methods in considerable detail. Readers would have been left in no doubt that Darwin—together with the style of natural history he represented—was out of date and that progress in plant breeding would follow from the abandonment of an outdated dogma and the adoption of more rigorous, laboratory-based methods. Many of mutation’s proponents, such as MacDougal, were also keen to promote laboratory biology, and part of Oenothera’s attraction was that the promise of rapidly creating new types of plants dramatized the laboratory’s potential to transform the world. As Sharon Kingsland has shown, the scientific practices that were shared by mutation’s opponents illustrate how the theory was deployed in a clash between rival styles of scientific research. However, the public interest in both Darwin and mutation meant that these debates could not be confined to the specialist press. In 1904, when De Vries himself spoke at the International Arts and Science Congress (a widely publicized occasion held as part of the Louisiana Purchase Exposition, often referred to as the World’s Fair), the *St. Louis Republic* reported the meeting under the headline “Darwin’s Theories Assailed by Scientists’ Congress,” with a subheading that read “Professor Hugo de Vries of Amsterdam declares naturalist’s theory of the evolution of species has been disproven by experiments many times repeated.”34 The impact of such stories was increased by


their being republished in other newspapers at a time when syndication and the wire services were helping to create a genuinely national news market. Virtually identical stories appeared across the country: San Francisco’s paper the Call ran theirs under the headline “Famed Botanist Disagrees with Darwin’s Theory”; the story was repeated almost verbatim as “Goes Against the Theory of Darwin” in Minnesota’s Saint Paul Globe; Ohio’s Perrysburg Journal used the headline “Attacks Darwin’s Theory”; and Utah’s Intermountain Catholic (which proudly proclaimed itself to be “A Catholic paper for the Catholic home”) flatly told its readers that De Vries “Does Not Believe in Evolution.” These shifting headlines embody the process by which mutation was used to bring topicality to existing debates over evolution.

However, mutation did more than just reshape old arguments. Plant breeding was of huge economic importance, and so scientific advances were of considerable interest to farming communities. Many of the readers of the Dakota Farmer might have been dubious about Darwinism, but when the paper reported on the Second International Conference on Plant Breeding and Hybridization, held in New York in 1902, the editors sent a prominent scientific plant breeder, professor Niels Ebbesen Hansen (1866–1950), to cover the meeting and give readers the facts about the new theory. Hansen’s report was republished by the Ranch (Seattle), which told its readers that “the storm center of discussion during the whole conference was about Mendel’s law of heredity and DeVries’ theory of mutation of species. These two seem to mark an epoch in the history of plant and animal breeding.” In contrast to readers of a paper like the Intermountain Catholic, those who read the Ranch or the Dakota Farmer would have learned that De Vries’s “new theory of mutation of species” was “some-what opposed to Darwin’s theories.” Newspaper editors clearly believed that practical farmers wanted information about scientific matters such as De Vries’s work rather than anti-Darwinian propaganda; the latter was a mainstay of the Intermountain Catholic, while the Ranch praised Darwin’s work on the rare occasions that his name was mentioned.

Popular newspaper reports of the somewhat esoteric debates within the scientific community were not just used to attack Darwinism. The arguments between prominent scientists were picked up and used by interpreters not just to make sense of mutation theory but also to interpret other scientific issues, such as eugenics. As the example of the Youth’s Companion given above shows, such reports often borrowed ideas from the specialist scientific press and re-presented them. For example, in 1919 the journal Current Opinion ran a piece entitled “Violent Science in State Legislatures: The Reaction of the Experts against Eugenics.” It quoted the English geneticist William Bateson as condemning the eugenic measures then being debated in U.S. state legislatures for being “too violent” and based on outmoded science. The writer


combined Bateson’s comments with material from the British medical journal the *Lancet* and an article by Dr. J. Parton Milum in the *London Quarterly Review*, which claimed of the mutation theory that “the more it is known the more it prevails.” According to Milum, “it is established, then, that species arise and are arising by mutation. We go further, and doubt if they have ever arisen by slow transition as evolutionists have almost universally believed.” The anonymous writer in *Current Opinion* developed this claim to argue that eugenicists were applying an outdated and faulty evolutionary theory to the human stock: “if the mutation theory be true, then the possibility of producing a superman by selection is excluded.” However, the author went on to note that although much nonsense was being written about eugenics, “it by no means follows from all this that there is no such science as eugenics.”

It is difficult to know what *Current Opinion*’s readers would have made of this complex assemblage, but the claim that mutation theory had overturned traditional wisdom about breeding, thus forcing a rethinking of eugenic policies, is clear. While more research is needed to clarify the mutation theory’s impact on eugenics, it is evident that the widely accepted view that natural selection was too slow appeared to make selection-based eugenics impracticable. For those unwilling to wait, the mutation theory’s promise of accelerated evolution offered an alternative; that may well have been its appeal to Davenport, who, as we shall see, saw the science of experimental evolution, founded on De Vries’s work, as the key to improving the human race.

The possibility that what was true of primroses was also true of people helped establish in the public mind the idea that biology was the science that promised (or threatened) to transform humanity—and to transform it rapidly. Most reports of the mutation theory emphasized its swiftness. In 1905 the *Atlantic Monthly* published a review of De Vries’s *Species and Varieties* (1905), which noted that “in Lamarck’s evening primrose, [De Vries] has seen the origin of a really new species by one clean jump. . . . Somewhere between one generation and the next the change occurs complete and final.” De Vries himself described the basics of this theory in the popular *Harper’s Magazine*, emphasizing “the sudden production of new forms from an old stock.” The significance attached to the swiftly mutating primrose could not be controlled either by the scientists or by their patrons. As mentioned previously, several socialist writers seized on De Vries’s claimed botanical revolution as evidence for the possibility of a political one, partly to defend Marxism’s scientific status. To many scientists, however, Oenothera’s rapid changes meant that evolution could at last be studied experimentally—and thus also offered the hope of attracting funding from those whose main concern was feeding the country’s growing population.

**GARDENING IN UTOPIA**

In 1902, when the USDA’s Beverly Galloway had stressed the urgency of feeding the country’s growing population, he had been forced to admit that America “has not as
yet been able to take front rank in the way of original discoveries.” Nevertheless, Americans were effective at utilizing “discoveries of all kinds where there was promise of practical results.” This was especially true of plant breeding, where much had been achieved thanks to the “far reaching intuitive knowledge” of such men as Luther Burbank, whose work “has shown great possibilities, and the improvement made in many crops will, no doubt, in time, prove of more value than even the present seems to indicate.” The way mutation theory was reported and received created complex connections between government scientists like Galloway, practical plant breeders like Burbank and Hansen (who was sometimes referred to as the “Burbank of the Plains”), and the theory’s promoters, especially De Vries himself. The utopian hope that the mutation theory might help feed the world’s expanding population actually began with De Vries, as Bert Theunissen has shown. In the first full-length exposition of his theory to reach American readers, De Vries had claimed that “if it should once become possible to bring plants to mutate at our will and perhaps even in arbitrarily chosen directions, there is no limit to the power we may finally hope to gain over nature.” Such claims were a key aspect of the mutation theory’s appeal to Americans, both within and beyond the professional scientific community; they seemed to offer a technological solution to potential problems.

The agricultural prospects associated with the primrose were reported regularly in both the scientific press and among wider readerships. For example, the Savannah Tribune reprinted a piece by De Vries from Scientific American in which he had written, “Underlying and directing all the efforts should be the hope of obtaining such a knowledge of the phenomenon as would enable us to take the whole guidance of it into our own hands.” As mentioned previously, fears of a shortfall in food may have been exaggerated, but the supposed applications of mutation theory became central to the way De Vries’s work and ideas were reported. When he visited the United States in 1912, the Washington Post took note of one of his speeches under the headline “Fears World Famine: Prof. de Vries Fears Land Will Become Exhausted.” De Vries and his theory were taken very seriously; he met President Theodore Roosevelt during this visit, and his speech to the Botanical Society of Washington was followed by addresses by Willard M. Hayes, Assistant Secretary of Agriculture, and by Dr. Edwin F. Smith, one of Galloway’s colleagues at the USDA Bureau of Plant Industry.

It is no surprise that the U.S. press invariably emphasized mutation theory’s practical implications: in 1912, the New York Times reported De Vries’s visit under

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40 Garland Allen and Sharon Kingsland have argued that the most tantalizing possibility offered by the theory was that mutations might eventually be produced to order, allowing dramatically increased crop yields. See Allen, “Hugo de Vries and the Reception of the ‘Mutation Theory’” (cit. n. 2); and Kingsland, “Battling Botanist” (cit. n. 32). See also Zevenhuizen, “Hugo de Vries” (cit. n. 8), p. 417. On U.S. agriculture and the promise of engineering life see Philip J. Pauly, Controlling Life: Jacques Loeb and the Engineering Ideal in Biology (New York: Oxford Univ. Press, 1987); and Pauly, Fruits and Plains: The Horticultural Transformation of America (Cambridge, Mass.: Harvard Univ. Press, 2007).

the headline “Noted Holland Expert Tells How to Double Our Crops.” And in the same year, the Chicago Daily Tribune told its readers that De Vries believed that “unless the science and practice of agriculture developed, the world will face famine within a few centuries.” Whether or not there was a basis for such fears, there were clearly some newspaper editors who either believed them or saw them as a way to sell newspapers. However, if the mutation theory were, literally, to bear fruit, it would need to be transformed from speculation into application. The Washington Post’s “Fears World Famine” story described De Vries as “one of the most eminent botanists in the world,” but he had nevertheless acknowledged to the assembled scientists and politicians he was addressing that “I have nothing better than a theory at present, but I hope to prove it practicable” and so “breed a plant which will produce much more than the form of life nature achieves.”

De Vries’s admission that he had “nothing better than a theory” is crucial to the way his work was reported: his American audiences were predominantly interested in the practical applications of science, and many looked to American plant breeders to produce the plants that promised “much more than . . . nature achieves.” The American horticulturalist Luther Burbank (see Figure 4) was central to this contest between (foreign) theory and (American) practice. The Chicago Tribune commented in 1912 that while “the principles” involved in improving plants were “being studied by De Vries,” the practical work involved was already under way, being done by “experimenter like our own Burbank.” The contrast between highfalutin European theories and American practicality was implicit in Galloway’s address and was one the press often pointed to when they compared De Vries and Burbank, popularly described as the “plant wizard” of Santa Rosa, California (a reference to Thomas Edison’s status as the “Wizard of Menlo Park”). As Katherine Pandora has shown, Burbank was an almost mythical figure in the American popular imagination who, like Edison, was presented as possessing a largely untutored and thus “native” inventiveness; he was often described as having an almost magical ability to reshape flowers and fruits at will by selecting unusual forms from which to create his new varieties. This ability was believed by many to offer the solution to feeding America’s population, with improved varieties like the still-popular Burbank potato. As De Vries’s mutations began to be publicized, the question inevitably arose as to whether they were different from the rarities that Burbank habitually called “sports.” Kingsland has argued persuasively that the data from breeders like Burbank became one focus of the contest over the expertise of field-based naturalists and laboratory-based biologists.

Building on Kingsland’s insight, my interest is in the way mutation theory inflected discussions of what it meant to be American by considering the relationship between two different questions: How were American farmers going to turn De Vries’s theory into profitable practice? Was Burbank’s Americanness linked to claims about Oenothera’s indigenousness?

In reporting De Vries’s imminent visit to California, the San Francisco paper the Call noted that “when Professor de Vries comes here next summer he will meet a
Californian as learned as himself in the secrets of plant life—Luther Burbank of Santa Rosa. Professor de Vries represents the theoretical aspect of that which Mr. Burbank has accomplished practically in his experimental gardens.” De Vries himself drew a similar contrast when he told the *Salt Lake Herald* that “the methods of Professor Burbank and my own are somewhat similar . . . with this difference: his is practical
work, while mine is theoretical. I find the way and let others secure the results. He secures the results.” And the San Francisco Chronicle accorded Burbank and De Vries comparable stature when it reported De Vries’s return from “a consultation . . . with Luther Burbank” under the headline “Famous Plant Experts Confer at Santa Rosa” (see Figure 5).44

Despite (or perhaps because of) his celebrity, Burbank was not generally held in high esteem by the scientific community. Nevertheless, his reputation was such that numerous scientific figures wished to connect themselves with him, and De Vries was no exception. During his first visit to the United States in 1904 he lectured at the University of California at Berkeley, which provided what he claimed was a long-wished-for opportunity to study Californian fruit-growing techniques. He was especially excited to visit Burbank, since, as he told the readers of Popular Science Monthly in 1905, “Burbank is the man who creates all the novelties in horticulture, a work which every one can not do. It requires a great genius.” However, De Vries wrote that while he had visited to find out “what secret method” allowed Burbank to produce such novelties as a stoneless plum, he discovered that Burbank had in fact simply procured some samples of an old and obscure French variety of “prune sans noyau” and crossed it with his own varieties—and so “there has been no real production of a new character but we have had a case of the general American principle: ‘try everything.’” This was, De Vries admitted, “to a certain degree, a disappointment.” He had “expected to learn a great deal about this point, the fundamental idea, if not the ultimate aim . . . the question of the nature and origin of new characters,” but had been forced to conclude that “Burbank’s experience did not throw any light on this question.”45 For De Vries, the absence of a new scientific principle was a disappointment, but for Burbank’s admirers it could scarcely have mattered; if “the general American principle” of “try everything” worked for Burbank, what was the use of fancy European theorizing?

Yet, despite his disappointment, De Vries tried to interpret Burbank’s work in ways that supported his own ideas. As he noted in a later popular article about Burbank, horticultural terminology was frustratingly vague, so De Vries attempted to clarify it, noting that “the jumps or leaps, however, which in ordinary terminology are called ‘sports’ embrace a number of phenomena of which the mutations are only one instance.” He distinguished the sports Burbank made use of from “mutations proper”; it was only the latter that resulted in the “production of really new, progressive, or retrogressive characters.” In trying to distinguish Burbank’s everyday “sports” from true mutations, De Vries was also trying to create a parallel distinction between his science and Burbank’s everyday plant breeding. He hoped that the evidence from Burbank’s well-known experiments would lend support to his new science, helping to ensure its acceptance. At the same time, David Starr Jordan made an attempt to coopt

Burbank to the antimutationist camp: Burbank’s remarks describing mutation as “a step backward towards the special creation theory” come from an article by Jordan in the *Popular Science Monthly* that presented Burbank as criticizing De Vries’s ideas.  

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The hybrid nature of Burbank’s reputation, and the resultant contest over it, was apparent in De Vries’s *Plant-Breeding* (1907), a collection of university lectures and scholarly papers that bore the subtitle “Comments on the Experiments of Nilsson and Burbank.” Simply by discussing Burbank’s experiments alongside those of the Swedish scientist Hjalmar Nilsson, De Vries implicitly cast him as a scientific worker. Yet despite this, and although the volume was clearly intended for an audience of academic scholars and expert plant breeders, De Vries reprinted his earlier account of visiting Burbank from the *Popular Science Monthly* and commented (as he did in everything he wrote about Burbank) that most of the popular reporting of his work was misleading, if not downright false. He had, therefore, “prepared myself for these visits by studying the magazine articles on his work published during the last few years, . . . among which those of E. J. Wickson in *Sunset Magazine* may be cited as the most complete and the most reliable.” Given De Vries’s repeated criticisms of popular accounts of Burbank, this is a surprising source to cite: *Sunset Magazine* was founded in 1898 by the Southern Pacific Transportation Company to promote travel to and settlement in California. However, the articles De Vries refers to so approvingly were themselves a curious hybrid, having been written not by a professional journalist but by a professor in the Department of Horticulture at the University of California.

Edward J. Wickson wrote four articles about Burbank for *Sunset* (December 1901 and February, April, and June 1902), which were then collected as a pamphlet that “attained wide circulation in all parts of the world.” However, a few years later, the editors of *Sunset* claimed that the pamphlet had created “an amount of effusiveness” and “unconscious exaggeration” that obscured Burbank’s true merits and left “the reader in confusion.” So Wickson was invited to contribute a further article, “The Real Luther Burbank” (see Figure 6), to the May 1905 issue, in order to “help the general reader to a better understanding” of the significance of Burbank’s work. Wickson highlighted Burbank’s curious status by noting the apparent paradox that the Carnegie Institution of Washington, “the greatest establishment for the promotion of original research in the world,” was funding his work—even though he had never “had a day’s scientific training.” This was the essential paradox that Burbank embodied: he was a native genius, in the sense of being both untutored and a native-born American. These qualities would have made him a potentially attractive figurehead for mutation theory; his endorsement would have made it both American and practical. However, Burbank’s lack of scientific training was also problematic; Wickson quoted De Vries as saying that Burbank used scientific terms but gave them entirely idiosyncratic meanings, yet Wickson himself denied any desire to “punish” Burbank for these misuses. He closed by stating that Burbank is “a horticulturalist, and not a scientist nor a philosopher, and they who are attempting to drag him into these fields are not wise.”

Despite Wickson’s attempt to use his academic authority to clarify the scientific

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meaning of Burbank’s work, numerous competing interpretations of it—including effusive, exaggerated, and confusing ones—continued to appear. Indeed, Wickson’s own article was accompanied by a poem in praise of Burbank that claimed humanity had called to God to reveal “the hidden wine / Hitherto hid from eyes of men,” in response to which “straightaway there arose a man, / born to interpret soil and sod, /
Burning with love for God’s own Plan.” Whatever one thinks of this as poetry, it is a long way from the impersonal language of scientific discourse.

In the same year in which he cited Wickson’s articles in Plant-Breeding, a book for a largely expert audience, De Vries also wrote a more explicitly popular account of “Burbank’s Ideas on Scientific Horticulture” for the Century Illustrated Monthly Magazine. It echoed both Wickson’s language and his arguments in several ways, not least by suggesting that Burbank’s supporters were in some respects his worst enemies. De Vries commented that in the case of the majority of articles about Burbank, “it is evident to the scientific readers that the author was not a scientist himself.” This statement implied that only scientists were qualified to comment on such matters—and would thus seem to exclude Burbank from commenting on his own work, since, as De Vries noted, Burbank possessed the “temperament . . . of an artist” and a “natural endowment for his work.”

A delicate balance was being struck here. De Vries sought to honor Burbank while also confining him to a properly subordinate role in the genuinely scientific hierarchy. Burbank was an “artist,” not a scientist, and “natural,” as opposed to educated; hence his results, however valuable, were practical rather than theoretical.

Both Wickson and De Vries avoided criticizing the “plant wizard” directly, not least because, like Jordan, they wished to make use of Burbank, to stabilize his significance by confining him to a role that they defined. Yet—simply by making the argument in popular magazines—they were virtually acknowledging the impossibility of their task. Wickson tried to label Burbank as a “horticulturalist,” as did De Vries (once he realized that Burbank was not going to endorse the mutation theory compliantly). But attempting to define Burbank was as difficult as defining the audience for his work—or for the mutation theory; each was as indefinable as the genre to which Wickson’s and De Vries’s articles should be assigned. Burbank’s “proper” role and meaning were no clearer than Oenothera’s; each remained stubbornly mutable.

Meanwhile, Burbank had his own ideas about evolution, and so, taking advantage of the increased permeability of the expert/lay boundary that Oenothera had precipitated, he tried to claim scientific status for himself and his ideas. Like De Vries and Jordan, he was to be disappointed, but the episode demonstrates the ways in which Oenothera opened up biology, almost as if the discipline itself had entered a mutation period. De Vries and Jordan competed for the public recognition that went with Burbank’s name, yet Burbank was not (in their eyes at least) a scientist. Hence it became impossible to identify the appropriate genre within which to discuss Burbank’s ideas: popular or elite? Moreover, the uses to which Burbank’s name might be put brought up the wider issue of defining an American. It was no coincidence that J. Hector St. John Crevecoeur asked the question “What then is the American, this new man?” in a series of Letters from an American Farmer (1782). Farming and the land have been central to Americans’ sense of themselves for centuries, and Burbank was only the latest in a long line of figures who embodied the idea of Americans as deeply connected to the land they cultivated. As De Vries himself


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put it, Burbank “loves his plants, and is enthusiastic over his work and plans. To accomplish something great for his country is his ideal”; moreover, “he does most of the work personally.” According to De Vries, “Two days each week Burbank spends on the farm, riding there on his bicycle; the rest of the week he is at home. Here are all the more delicate crossings, and it is here that every new experiment is started.”51 The image of Burbank on his bicycle, tending his experiments himself in an effort to help his country, embodies an ideal of Americanness that contrasted sharply with the image of De Vries—foreign, theoretical, and leaving the practicalities to others. For mutation theory to fulfill its promise, it needed to become more practical and thus more American.

RETURN OF THE NATIVE?

The mutation theory appeared at a time when U.S. biologists were working to establish an independent American biological tradition: the Carnegie Institution founded the Cold Spring Harbor Laboratory in order to help create just such a tradition, and, as we have seen, many American biologists hoped De Vries’s theories would provide the underpinning for this new biology that would create new plants to order. And even those who remained agnostic about De Vries’s broad evolutionary ideas were attracted by the promise of new experimental techniques for studying questions such as adaptation. However, American mutationists may have been somewhat embarrassed by the fact that their new biology was based on a European theory. One way of building American enthusiasm for the new theory was to emphasize that, while the theory might be foreign, O. lamarckiana itself was indigenous. De Vries certainly made this claim, telling the readers of Harper’s Monthly, for example, that “the whole genus Oenothera is of American origin.” And Daniel MacDougal, De Vries’s most important U.S. supporter, also stressed that the plant was a native one: in a technical monograph on Oenothera he noted that all his experiments had been made with “guarded pedigree-cultures [that] have been made from pure seeds of native species of evening-primroses.”52 Once again, we see purity and indigenousness conflated; whether this conflation was deliberate or unconscious, the vexed question of whether Oenothera itself was an American helps us understand another aspect of the plant’s impact. The context of immigration, especially debates over whether and how it should be restricted, may have given added importance to a relatively obscure question about the geographical origin of an experimental organism. It is difficult to find a direct link between the Oenothera debate and that over immigration and race; however, the language in which the biologists discussed the issue seems to be modulated by that of the immigration debate. The link is a subtle one, but it suggests another way in which mutation contributed to the coalescence of a public culture of biology, as the scientists and their publics came to share a common language.

The Oenothera workers’ claims about the plant’s indigenousness had at least two audiences and two different significances. Their main purpose was to persuade biologists of a key claim of the mutation theory. De Vries had argued that traditionally defined species were essentially arbitrary groupings, made up of several genuine “elementary” species whose distinctiveness

was obscured by minor, “fluctuating” variations that were not heritable. “The real units,” he wrote, “are the elementary species,” and only laboratory experiments could identify them. Isolating pure species was supposedly the key to improving them, since De Vries had argued that most of the so-called species available to the hybridizer were themselves hybrids, in which case further crosses must, inevitably, be unpredictable. The mutation theory would (albeit in some largely unspecified way) allow pure, “elementary” species to be identified and used as reliable raw material for future experiments in crop improvement.

However, the mutationists’ claim that the plant was an American native may have also helped persuade their various audiences that a new, American biology was emerging. The San Francisco Chronicle’s account proclaimed that De Vries had been “Studying California Plants in Holland,” adding that *Oenothera lamarckiana* was “very common in California and it is open to all to test the conclusions of Professor Hugo de Vries.” As Kingsland has noted, many Americans interpreted the mutation theory as a democratizing idea that made science accessible to everyone, from the professor to the gardener. When Charles A. White of the Smithsonian Institution claimed to have proved De Vries’s theories using tomatoes, the Washington Post reported his work under the headline “Variation in Species: Scientific Investigation Pursued in a Washington Back Yard,” emphasizing that the work had been done “in a little patch of ground hardly six feet by three” yet had been hailed by De Vries “as an important and startling confirmation of his theory.” However, if Americans were really to test the mutation theory in their gardens, it was essential that the Chronicle be correct in claiming that *O. lamarckiana* was “very common in California,” an assertion that many doubted.

Opponents of the mutation theory claimed that the “species” *O. lamarckiana* was nothing of the sort, merely an artificial hybrid produced in Europe. Among the most tenacious critics was Bradley Moore Davis (see Figure 7). He was originally a field naturalist, having been a friend of David Starr Jordan’s since boyhood and having followed Jordan from the University of Indiana to Stanford. He then did doctoral work at Harvard and became an assistant professor of botany at the University of Chicago, but lost his job after eleven years there. He went back to Cambridge, Massachusetts, and supported himself writing botanical textbooks until he got another position, at the University of Pennsylvania, in 1911. It was while in Cambridge, between about 1906 and 1911, that Davis became interested in Oenothera. As he explained in an unpublished memoir, “at this time Professor Hugo de Vries was actively pushing by popular lectures and extensive writings his Mutation Theory of Evolution.” Davis’s sense that De Vries had been “pushing” mutation, and doing so via “popular lectures and extensive writings,” may partly explain his hostility to the theory; perhaps he suspected that it rested on too much (self-) promotion and not enough scientific evidence.

With the help of the Canadian-born Harvard professor of botany Edward Charles Jeffrey, who provided seeds of wild Oenotheras, Davis conducted a long series of experiments to test

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53 De Vries, *Species and Varieties* (cit. n. 6), pp. 11–12 (quotation); and Allen, “Hugo De Vries and the Reception of the ‘Mutation Theory’” (cit. n. 2), p. 82.


O. lamarckiana. He became convinced that its “parentage is far from pure, that it is in fact of hybrid origin. We are dealing with the product of the garden.” His mentor, Jordan, joined in the attack, telling readers of the *Popular Science Monthly* that “the species called by de Vries *Oenothera Lamarckiana* is not known in its wild state anywhere in North America.” Davis returned repeatedly to his claim, arguing in 1917 that “there is no evidence that this plant is a wild species native to the American continent.” Trying to prove his case, Davis hybridized native American species in an effort to replicate *O. lamarckiana*’s unusual characteristics. For

*Figure 7.* Bradley Moore Davis. (Courtesy: Frank Boyden.)
complex technical reasons, he never succeeded, but he never gave up his attempts (his grandson has told me that he continued growing Oenotheras in his retirement, long after De Vries was dead). Davis remained convinced that the mutation theory’s crucial evidence derived not from a native American plant but from an unstable, unnatural, European hybrid—an “impure species,” as he dubbed it. As we saw in the case of MacDougal—who was one of the theory’s supporters—opponents like Davis also chose words that erased the difference between purity and indigenousness: the plant’s “parentage is far from pure”; it was never a native, but an “impure species.” As we shall see, similar, slightly emotive language was used regularly by those who attacked the mutation theory.

The claim that their flagship flower was neither American nor a species clearly stung MacDougal and his collaborators. In the 1905 monograph Mutants and Hybrids of the Oenotheras, MacDougal rehearsed the evidence for the plant’s North American origins in detail. It included a plate from the impeccably American William Barton’s Flora of North America (1821) that pictured a “Large Flowered Evening Primrose,” “found in South Carolina and Georgia, which is apparently identical to Oenothera Lamarckiana.” Numerous similar examples were given; but the more exciting news was that De Vries himself, during his visit to America in summer 1904, had “joined in the quest for specimens” and had “called attention to a sheet of material in the herbarium of the Philadelphia Academy of Sciences, collected by C. W. Short near Lexington, Ky., which he considered as O. lamarckiana, and which was grown wild in the locality recorded.” Botanists in Kentucky had gone hunting for the species, without success; nevertheless, MacDougal was confident that “the above evidence makes it fairly conclusive, however, that the large-flowered evening-primrose which formed the basal material for the experimental researches of de Vries, is, or was, a component part of the flora of North America.”

The mutationists speculated that O. lamarckiana might have died out in America after being introduced to Europe. No living O. lamarckiana plants had been found until the “apparently identical” species described by Barton eighty-five years earlier was rediscovered at the indisputably American location of Dixie Landing, on the Alabama River. The New York Times told its readers that, during his 1912 U.S. visit, De Vries would “visit the place in Alabama where the original evening primrose with which he has performed so many of his experiments, is said to grow.” Sadly for De Vries and his followers, this turned out to be a dead end; the Dixie Landing primroses were not O. lamarckiana but hybrids of O. grandiflora and another species. As De Vries had to acknowledge after visiting the location, “no Oenothera from Dixie Landing . . . can be cleared of the suspicion that it may be of hybrid origin.” However, he emphasized that the strain he used in Amsterdam “is unmixed, since no other species of Oenothera grows at that locality.” There was, moreover, a positive aspect to the Dixie Landing discovery: it cast doubt on the work of Bradley Davis, since his specimens were now under “suspicion”—having been grown from seeds originally collected at Dixie Landing.

As a representative of traditional Darwinism and the naturalist tradition, Davis (like Jordan)
had good reasons to dispute the supposed evidence in favor of mutation, not least because *O. lamarckiana* had become the standard-bearer of an attempt to Americanize the newfangled, European, laboratory-based practice of experimental evolution. So, whatever disappointment he might have felt over the revelation that his Oenothera stocks were not themselves pure may have been mitigated by a degree of Schadenfreude at the sight of his opponents being forced to revert to traditional natural history practices like field collecting and herbarium searches in order to bolster their position.

However, the claim that Oenothera was a native plant had wider implications for the imaginative uses of mutation theory. Oenothera’s indigenousness was invariably connected to the claim that it was a “pure” species, a term whose resonances often suggested that the lessons learned from the primrose might be applied to humans. When MacDougal’s work was discussed in the *New York Times* in 1905, the writer (prompted no doubt by MacDougal himself, who was quoted extensively in the article) pointed out that it had been done using “the evening primrose of the Adirondacks and Northern New England” as well as “the great-flowered evening primrose of the Southern States.” However, it was the nature of *O. lamarckiana* itself that became the focus of attention, and the *Times* emphasized that “Lamarck’s evening primrose has been carefully guarded against hybridizations since 1884 and perhaps there is no plant in existence in which the purity of the parental strain has been so critically examined.” A later piece noted that De Vries regarded obtaining a pure strain as vital if yields were to be increased. It commented that current breeding practices entailed an “endless battle against mediocrity” that could never be won, because existing crop plants, being “more or less mixed, tend to degenerate.” However, thanks to “the discoveries of de Vries,” this tiresome battle was over, “for by the careful selection of the ‘mutants’ . . . a new species springs into being of a pure type, never degenerating, and perhaps of double or triple the yield of the parent plant.”

Given concerns over immigration at the time, many of the *Times*’s readers might well have reflected on the degenerative effects of mixed human breeds as they read this, wondering about the impact that such hybrids would have on America’s progress and whether it might not be desirable to maintain “a pure type, never degenerating.” This language of purity, so closely linked to questions of indigenousness, had very wide circulation at the time.

The idea that primroses could provide a model for people may seem absurd, yet such a claim was sometimes made. In 1902, the *Independent* described De Vries’s work as “one of the most illuminating announcements of modern science” and suggested that the new theory explained how it was that, despite the poverty and discrimination that affected the Jewish people, they nevertheless gave birth to so many exceptional figures, whether financiers, poets, or philanthropists. Someone like the British Prime Minister Benjamin Disraeli must be considered “a glorified Jewish Primrose,” the writer announced—but then added that “Jews are not that different from other people,” just as it was presumed that Oenothera was unlikely to be the only plant that “sports” to create new types.

The slippage from primroses to people may perhaps explain the rather heated tone that occasionally surfaced in the biologists’ supposedly dispassionate scientific discussions of

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Oenothera’s disputed indigenousness. One of the most aggressive attacks on *O. lamarckiana*’s purity came from Davis’s mentor, Edward Charles Jeffrey, whose 1914 article “The Mutation Myth” argued that it was time to reject the theory because “recently a number of American students of plant genetics have cast doubt on the genetical purity of De Vries’ *Oenothera Lamarckiana.*” He explained that studying wild species of Oenothera (grown from the same stock of seeds Davis used) “has led me to the apparently inevitable conclusion that spontaneous hybridism is extremely common in the genus,” resulting in “high genetical impurity.” He concluded that “there appears in fact to be every reason to believe that the bar sinister has been crossed and double crossed in our American evening primroses.” (“Bar sinister,” as the *Oxford English Dictionary* explains, is “in popular, but erroneous phrase, the heraldic sign of illegitimacy.”) And so, Jeffrey went on, “it follows of course that no genus or group of plants could have been more unfortunately chosen to illustrate the origin of species by mutation or saltatory evolution.”61

It is difficult not to detect a note of moral disapproval in Jeffrey’s comments. He observed, for example, that since the mutation theory rested so largely on *Oenothera lamarckiana*, it was essential that it and the entire plant family to which it belonged “must like Caesar’s wife, be beyond suspicion.” “Like Caesar, Oenothera has become a name of authority and its family affairs accordingly, should be beyond suspicion, when subjected to the most searching investigation. It is apparently just in this direction that the weak spot of the mutation hypothesis lies.”62

This apparent aspersion cast on the plant’s sexual behavior strengthens the sense that there are anxieties in play here that go beyond primroses, an idea reinforced by Jeffrey’s conclusion that De Vries’s ideas “may apparently be now relegated to the limbo of discarded hypotheses”: “It is now high time, so far as the so-called mutation hypothesis, based on the conduct of the evening primrose in cultures, is concerned, that the younger generation of biologists should take heed lest the primrose path of dalliance lead them imperceptibly into the primrose path to the everlasting bonfire.”63

The utopian hopes that surrounded De Vries’s claims about the economic importance of mutation brought him much attention and gave the question of Oenothera’s purity considerable significance. Whether or not their claim was justified, both supporters and opponents believed that proving *Oenothera lamarckiana* to be a native American would also provide evidence of its purity, and for supporters it would also help Americanize the otherwise foreign idea of mutation. However, the meaning of the mutation theory was neither created nor settled by debates within the scientific community. Nonscientists interpreted mutation theory according to their own interests and ideas, but this was not a one-way process; the debate over Oenothera’s indigenousness suggests that the language of the scientists may have been influenced by that of wider debates over racial purity.

**CONCLUSION**

The scientific community lost interest in Oenothera almost as rapidly as it had initially taken up the plant; by about 1915, attention was shifting to other, more cooperative organisms such

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63 Jeffrey, “Mutation Myth” (cit. n. 61), p. 491.
as *Drosophila melanogaster*. However, the wider public remained interested in De Vries, his primrose, and his theory for at least another decade. In anticipating the Scopes trial in 1925, the *New York Times* noted that, once it began, “erudite headlines will confront the nation,” and alongside the names of Charles Darwin and August Weismann would be that of Hugo de Vries, and in addition to discussions of Pithecanthropus, Piltdown, Cro-Magnon, and human embryology would be considerations of “the mutations of the evening primrose.” And in 1924 the *Chicago Defender* announced the “discovery” of some pale-skinned, fair-haired Native Americans in Panama. The explorer responsible, Richard O. Marsh, told the paper that “we consider the white Indians most probably examples of the Law of Mutation, as worked out by De Vries, the eminent scientist.”

Both the prominence and the persistence of the mutation theory require explanation. As noted above, concerns (even if exaggerated) over feeding the country’s expanding population attracted many to the mutationists’ claims of rapid, controlled evolution, especially American scientists looking to establish (and obtain funding for) new disciplines. Awareness of the mutation theory was certainly helped by the growth of the U.S. newspaper market in the early twentieth century, and the impact of the reports that appeared in the American press was increased by syndication—and perhaps at times by outright plagiarism. Men like De Vries and MacDougal, eager to gain attention for their new ideas, were greatly helped by the fact that there were many more column inches to fill than had been the case a decade or two earlier. However, the scientists’ need for publicity often resulted in their ideas being appropriated in ways they neither anticipated nor desired. While he was no socialist, De Vries was a political liberal and might therefore not have been too concerned at the socialist uses being made of his theory (if, indeed, he ever knew of them). On the other hand, the claim that he had overturned Darwin created headlines that sold newspapers—but they were headlines De Vries could not possibly have been happy about.

Given the importance of mutation theory, it is worth asking why historians have hitherto not generally given it the attention it warrants. One reason is that the resources for studying its reception have only recently been created. Newspapers like the Utah *Intermountain Catholic* covered De Vries’s work once, in a few short paragraphs, during the first twenty-five years of the century. Even the most diligent (and long-lived) researcher could not hope to discover such reports without the ability to search digitized newspaper archives, such as the Library of Congress Chronicling America Project, which has been vital to this research. Such archives make it possible to begin considering that most intriguing but elusive historical question: What did the general public think? They cannot, of course, provide a full answer, and many other kinds of sources are needed to flesh out the hints that newspapers and magazines provide; nevertheless, the much-hyped advent of the “digital humanities” does give historians an unprecedented opportunity to rethink the ways we tell our stories. For perhaps the first time, we can investigate what twentieth-century scientists thought and did alongside the question of how their various publics thought about and used their discoveries.

The ways in which mutation theory was appropriated by antievolutionary writers in the first quarter of the twentieth century perhaps contributed to the hostility some contemporary scientists expressed toward the upstart theory; at a time when biologists were trying to

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establish the credibility of new laboratory-based and experimental approaches, the mutationists’ seemingly inflated claims could be interpreted as providing ammunition to the enemies of science. The antievolutionary rhetoric often entailed taking scientific critiques of evolution out of their original context and appropriating them to attack the idea of evolution itself; this genre has, of course, persisted in the writings of those who call themselves scientific creationists or proponents of intelligent design. A concern not to give comfort to the creationists may explain why traditional naturalists tended to confine their remarks to specialized scientific publications. By contrast, the mutationists were more willing to write for or give interviews to the nonspecialist press, not least because—as is obvious from the case of Luther Burbank—it was not clear which were the appropriate genres and periodicals. Burbank’s role also suggests that, just as Oenothera became a synecdoche for mutation theory (with meanings that ranged from exciting to foreign, from revolutionary to theoretical), so Burbank came to stand for everything from down-to-earth, native American practicality to raw genius. But neither the plant nor the man could be pinned down. Like Oenothera lamarckiana, Burbank himself was an unstable hybrid; his indigenousness was indisputable, but his scientific status could not be determined.

Another factor that explains historians’ neglect of the mutation theory is that the history of twentieth-century biology remains somewhat fixated on scientists’ ideas and their work. One effect of this residual internalism is that histories of the century of the gene remain rather focused on the modern evolutionary synthesis. If one assumes the synthesis to be the central event, mutation theory is not merely irrelevant but an obstacle in the path of progress. As Ernst Mayr wrote in his prologue to The Evolutionary Synthesis, “One almost has to go so far as saying that the generation which had become imprinted with the de Vriesian mutation concept had to die out before the new concept of mutation, introduced by Morgan, could take hold.”

Analyzing the reception of the mutation theory provides some important correctives to the established history; at the very least, it is now impossible simply to dismiss the theory as unimportant or a failure. However, the methodological approach employed here may also shed light on a wider historical question: Who determines the meaning, and thus the success or failure, of a modern scientific theory? Of course, the specialized scientific community plays a vital role, and in many cases that role may be decisive, but one has only to consider continuing popular opposition to such scientific advances as (to use just a few recent examples) genetically modified foods, the MMR vaccine, or evolution itself to realize that even a strong consensus within the scientific community may not be sufficient to quell public skepticism. And the way the word “evolution” is consistently used by advertisers as synonymous with “progress” clearly shows that the public’s acceptance of a scientific idea does not mean that they have understood it in the same terms as the scientists do. Reconsidering the reception of

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66 This may be another reason why mutation theory has been written out of the history of twentieth-century biology: the modern synthesis greatly increased the authority and credibility of evolutionary biology, and those who participated in it (some of whom were also to become its earliest historians) did not wish to be reminded of how confused and contradictory early twentieth-century biology had been.

Oenothera suggests that analyzing how various publics use (or fail to use) scientific theories may be a more fruitful way of asking how the meanings of theories take shape.

The century of the gene began in the fly room at Columbia, among Harvard’s rats and guinea pigs, in America’s Corn Belt, and in the Oenothera beds of Cold Spring Harbor. But it also began in the daily newspapers and popular magazines of the early twentieth century. The reporting of mutation theory reminds us that, as with those other organisms, it was the utopian hope of controlling nature that drew people to the evening primrose. As the New-York Tribune put it in 1907: “The dream of Bacon, who saw in the New Atlantis gardens a land devoted to the modification of animals and plants at man’s will, is being realized by the Carnegie Institution at its new ‘Station for Experimental Evolution’ at Cold Spring Harbor, Long Island.” This report described the work that had been done during the laboratory’s first few years under the headline “Creation of Species.” As the reporter noted, “The discovery of the laws of organic evolution is the prime function of this interesting institution,” a research project directly inspired by De Vries’s theories. Discovering these laws would have a “direct bearing upon man,” as the paper demonstrated by quoting the laboratory’s director, Charles Davenport: “Since when we know the law we may control the process, the principles of evolution will show the way to an improvement of the human race. A knowledge of the principles of evolution is advantageous in still another way. It shows how organisms can best be modified to meet our requirements of beauty, food, materials and power.” This may sound like hyperbole: Davenport trying to meet a journalist’s expectations of the dramatic claims that must be made to justify the space devoted to his lab by the newspaper. However, the quotation was not given to the journalist; it was taken from Davenport’s annual report to his funding body, the Carnegie Institution, which—among other things—listed the thousands of Oenotheras that the station had grown that year. Davenport’s report had originally been intended for a small, elite audience, the institution’s twenty-five trustees, yet it was presented in language that was equally suited to a tabloid readership; the biologists’ utopian hopes and claims had already become part of the way in which it was expected that biology was to be discussed. This is just one example of the ways in which the public helped shape the meanings of a scientific theory.

The themes that recurred whenever the evening primrose was discussed implied various political and scientific agendas; they helped to define a public conversation that encompassed both the scientists and their various audiences. These early debates shaped the public’s expectations of what biology might be able to do, for better or worse, and—equally significantly—they shaped the scientists’ sense of what was legitimate science and how it should be presented. Thanks, initially at least, to Oenothera lamarckiana and the mutation theory, these themes would become part of the way biology would be envisaged throughout the twentieth century.
