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The Nature of Tomorrow:
Inbreeding in Industrial Agriculture and Evolutionary Thought
in Britain and the United States, 1859-1925

By

Theodore James Varno

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

History

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor John E. Lesch, Chair
Professor Cathryn L. Carson
Professor Stephen E. Glickman

Fall 2011

The Nature of Tomorrow:
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Abstract

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Historians of science have long recognized that agricultural institutions helped shape the first generation of geneticists, but the importance of academic biology to scientific agriculture has remained largely unexplored. This dissertation charts the relationship between evolutionary thought and industrial agriculture from Charles Darwin's research program of the nineteenth century through the development of professional genetics in the first quarter of the twentieth century. It does this by focusing on a single topic that was important simultaneously to evolutionary thinkers as a conceptual challenge and to agriculturalists as a technique for modifying organism populations: the intensive inbreeding of livestock and crops.

Chapter One traces zoological inbreeding and botanical self-fertilization in Darwin's research from his articles published in *The gardeners' chronicle* in the 1840s and 1850s through his *The effects of cross and self fertilisation in the vegetable kingdom* of 1876. In doing so, it demonstrates how Darwin metaphorically linked natural selection to methodical selection in order to authorize the naturalist to become an experimental evolutionist. It also explores the potential of Darwin's program as an ideology for actors intent on transforming the political economy of agriculture.

Chapter Two moves away from scholarly discussions of evolution to consider how intensive inbreeding was pioneered as a method for modifying crops by the Bureau of Plant Industry of the U.S. Department of Agriculture between 1897 and 1907, the decade following William Jennings Bryan's loss to William McKinley in the election of 1896. By analyzing both Robert Bakewell's livestock inbreeding system of the eighteenth century and Archibald Shamel's Connecticut River tobacco self-fertilization program of the early twentieth, the chapter explains the political economy of intensive inbreeding. It concludes by exploring the broader context of the experiments devoted to livestock inbreeding that George Rommel initiated at the Bureau of Animal Industry of the U.S. Department of Agriculture in the early 1900s.

Chapter Three combines the intellectual history of Chapter One with the political and economic history of Chapter Two by illustrating how the research program of experimental evolution that Darwin formulated in the nineteenth century mapped onto the large-scale industrial agriculture projects of the USDA between 1903 and 1925. The chapter follows ideas on inbreeding as they moved across various academic and professional communities, paying particular attention to institutional arrangements like the American Breeders' Association and the Bussey Institution of Harvard University that facilitated these transfers. It also examines various inbred organism populations that became prototypes for industrial production and the scientists who became their shepherds: the Wistar rats of Helen Dean King, the hybrid corn of Edward Murray East and Donald Forsha Jones, and the guinea pigs of Sewall Wright.

For Mom & Dad

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Acknowledgments

This dissertation is the product of two scholarly communities separated by nearly 3,000 miles. While it was being prepared, I drove between them on ten different occasions, taking as many looping, inefficient cross-country routes as I could discover. I accumulated far more debts on those journeys, both intellectual and personal, than I could possibly enumerate here; I have been a very lucky person.

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In 2008, I received a generous Dissertation Writing Fellowship from the Philadelphia Area Center for History of Science (PACHS) that allowed me to relocate to the East Coast and become a part of another community of historians. At PACHS, I could always count on Babak Ashrafi to be a calm, supportive presence in my day; his genuine interest in all facets of the history of science and the thoughtful consideration he gives to the historians he meets continue to impress me. In addition, Bonnie Clause helped to introduce me to the city of Philadelphia and passed on her great knowledge of the Wistar rat. Without the assistance, advice, and recommendations of the staff of the American Philosophical Society Library, especially Roy Goodman, Charles Greifenstein, Valerie-Ann Lutz, and Earle Spamer, this dissertation could not have possibly included the wide range of scientists as subjects that it does; preliminary research for my dissertation was also made possible by a Library Resident Research Fellowship.

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This dissertation is dedicated to my parents, Rosey and Ted Varno. Wherever I wandered, I knew that they would be there for me with their unconditional love. No words can express what I feel for you both. Thank you.

Introduction

At Berkeley, mid-century, in the hills of northern California, Professor William E. Castle began an investigation of the golden hues of the Palomino horse. He had built a career as a geneticist by studying rodents at the Bussey Institution, Harvard University's graduate school of applied biology, but when the Bussey closed its doors in 1936 he retired and headed west to the University of California, where his friend and colleague Ernest B. Babcock had carved a department of genetics out of the school's old college of agriculture. Set loose from his professorial duties, Castle drifted bit-by-bit away from mice and rats and rabbits and toward horses; fascinated by their diversity of colors and markings, he let his ties to the academic genetics community atrophy as he made new connections in the horse associations, the breed clubs, the experimental farms. When his son Edward, himself a plant physiologist at Harvard, wrote him in January 1952 to ask what had initially brought him to the study of biology, Castle reminisced not about the laboratory but about his childhood in southern Ohio:

As a boy I was interested in outdoor life on the farm, particularly the wild spring flowers blooming in the woods. I gathered and put together the whitened skeletons of sheep which died in the pastures and whose bones were cleaned up by maggots, turkey-buzzards, and rain. I transplanted seedling apple trees and grafted them to varieties of apples growing on the place.

It was, he suggested, immersion in this rural countryside, a site of simple wonders, that sparked his early curiosity about the living world.¹

In *Phenomenology of perception*, Maurice Merleau-Ponty celebrates a state of raw consciousness like that of Castle's remembered youth as the foundation upon which we make any sense of the universe. "All my knowledge of the world, even my scientific knowledge," he notes, was "gained from my own particular point of view, or from some experience of the world without which the symbols of science would be meaningless." In those years when Castle first began his career, when he moved to Ottawa, Kansas, and taught Latin at a small Baptist college, he roamed the countryside on his afternoons off and compiled and published a list of the hundreds of ferns and flowering plants of Franklin County. In these prairie years, the connection between Castle's physical embodiment in the world and his scientific explorations was not difficult to discern.²

¹ On E.B. Babcock and Berkeley's Department of Genetics, see Vassiliki Betty Smocovitis, "The 'Plant *Drosophila*': E.B. Babcock, the genus *Crepis*, and the evolution of a genetics research program at Berkeley, 1915-1947," *Historical studies in the natural sciences* 39 (2009): 300-355. Basic biographical information for William E. Castle can be found in L.C. Dunn, "William Ernest Castle, October 25, 1867 – June 3, 1962," *Biographical memoirs of the National Academy of Sciences* 38 (1965): 33-80. William E. Castle to Edward Castle, January 28, 1952, WCP-APS, Box 1, Folder: Castle, William Ernest - Autobiographical and genealogical notes.

² Maurice Merleau-Ponty, *Phenomenology of perception*, trans. Colin Smith (London: Routledge & Kegan Paul, 1967), viii. Dunn, "Castle," 36, 65.

In the decades that followed, however, matters became more complex. In the fall of 1892, in a season when Kansans elected a third-party candidate on the Populist ticket as their governor, Castle left the state and followed some of his former classmates east to Massachusetts, where he intended to study botany at Harvard. Instead, he “found the Zoology courses better organized and more interesting,” became an assistant in Charles Davenport’s animal research laboratory, and graduated after three short years with a doctorate in zoology. From that time onward, with the exception of a rare and occasional trip into the field, as when he led an expedition to Peru to collect wild cavies for breeding experiments in captivity, Castle vanished into what Robert Kohler has described as the “placelessness” of the laboratory, a space congenial to undertaking research of a universalistic character:

Generic places sustain the illusion that their inhabitants’ beliefs and practices are everyone’s beliefs and practices. We credit knowledge and practices that are universal and mistrust what is merely local and particular, and laboratories are meant to seem universal, the same everywhere. The variability and unexpected occurrences of nature have no place in labs. Such things would only undermine the reason why we trust experiments, which is that they turn out the same wherever they are performed.

This move into the laboratory world not only physically removed Castle from the land of his youth, but committed him to producing a type of knowledge that obscured its particularistic origins, concealing the specific time and place within which it originated.³

Nonetheless, we find Castle in 1952 recalling his youthful attachment to the land, the exact spot where he grew up with all its peculiarities, as the wellspring of his later success as a laboratory scientist. This seems a contradiction, but Castle resolved it by writing to his son about another formative experience of his youth, this one a classroom encounter at adolescence with a charismatic scholar and a new doctrine of nature:

A smattering of biology was taught to us by a really competent and inspiring personality, Clarence L. Herrick, but his teaching load included geology, zoology, and botany, and consequently we got only the rudiments of each. Herrick made his subjects interesting and alive and growing. He himself was contributing to them by studies of the local geology ... Herrick imparted to us the inspiring concept of organic evolution, [à la] Darwin, which was only beginning to receive wide attention, in the face of strong theological opposition, voiced locally by the President in the ethics course. But the students all sided with Darwin and Herrick. The discussion was always friendly, not marred by Fundamentalism and fury, as it came to be a generation later, when the Scopes trial occurred in Tennessee.

³ William E. Castle to Edward Castle, January 28, 1952, WCP-APS, Box 1, Folder: Castle, William Ernest - Autobiographical and genealogical notes. Robert Kohler, *Landscapes & labscapes: Exploring the lab-field border in biology* (Chicago: University of Chicago Press, 2002), 7.

Through Darwin, Castle identified with Herrick; he incorporated the theory of evolution into a sense of self that oriented him to the adult social world he was beginning to enter, a sense that structured how he would recognize and differentiate his peers, spot potential allies, know his opponents.⁴

Along with positioning Castle within the social sphere, Darwinian evolution also provided him a new orientation to the landscape, a new way of knowing place. Gone were the days of wandering a strange and curious countryside where each new discovery beckoned an awakened consciousness; instead, the trees and streams and birds and insects presented themselves as products of a long, cosmic pageant. There was still a sense of wonder in this world, to be sure, but it was displaced from the things themselves to the process that had brought them into being: "It is interesting to contemplate a tangled bank," Darwin concluded *Origin of species*, "clothed with many plants of many kinds, with birds singing on the bushes, with various insects flitting about, and with worms crawling through the damp earth, and to reflect that these elaborately constructed forms, so different from each other, and dependent on each other in so complex a manner, have all been produced by laws acting around us." From this perspective, the Darwinian viewpoint, the various details of the countryside ought to draw our attention because they were examples from which one might derive universal laws, "laws acting around us," that had created and were still in the process of remaking the living world.⁵

This transformation of the particular into the universal, the specific object into an exemplar of cosmic happenings, is precisely what made Darwinism so threatening to the faithful and so inspiring to its adherents. *Origin of species* does not claim to have uncovered the process by which a particular organism was brought into being, but to have discovered the process that created virtually all life everywhere on the planet. This elevation of the universal over the local in *Origin* is also what made it possible for Castle and biologists of his generation to believe they could leave the places where life abounded, reposition themselves in the placelessness of laboratories, and still remain committed to the natural historical tradition that had been perhaps most famously symbolized by Darwin's very much embodied circumnavigation of the earth aboard the *Beagle*. Observing rodents day-in, day-out, in controlled and regulated indoor rooms on the outskirts of dense urban Boston, Castle could imagine himself answering the focused and crucial questions that would carry Darwin's project into the new century, keeping the natural historical tradition alive. Its sense of the uncanny had been relocated from nature's specifics themselves to the universal processes which had brought them into being.⁶

What this relocation threatened to extinguish, however, was the awareness of position and the sense of place so integral to the kind of embodied experiences Castle remembered from his youth. The spectator looking down

⁴ William E. Castle to Edward Castle, January 28, 1952, WCP-APS, Box 1, Folder: Castle, William Ernest - Autobiographical and genealogical notes.

⁵ Charles Darwin, *On the origin of species by means of natural selection, or the preservation of favoured races in the struggle for life* (London: John Murray, 1859), 489.

⁶ This "new natural history" of the 1890s and early 1900s, positioned in laboratories, field stations, and biological farms, is described in detail in Kohler, *Landscapes & labscapes*, 23-59.

upon Darwin's tangled bank was an omniscient eye, watching nature pass unaffected by its other-worldly presence. The experimental evolutionist post-Darwin assumed that vantage too, and in so doing failed to consider that the science he or she was undertaking might be subtly re-orienting his or her relationship to the land.

II

William Castle was not the only person to leave the state of Kansas during the 1890s. In that final decade of the nineteenth century, there was a net out-migration of 149,000 people from the state. Only neighboring Nebraska, which shed 154,000 more residents than it gained, could match Kansas's population losses during that difficult decade.⁷

Nationwide, the number of farms and the number of farmers in the United States continued to increase through the 1890s, but the same sense of nervous anxiety that might have been found in small towns throughout Kansas pervaded much of rural America. Deep structural transformations of the continent's political economy were underway, revealing themselves gradually, month-to-month, year-to-year. Agricultural productivity was on the rise. Between 1880 and 1920, the number of farms in the United States increased from 4,009,000 to 6,454,000. The amount of land under cultivation almost doubled from 536,082,000 acres to 958,677,000 acres. The farm population rose from 21,973,000 to 31,974,000. Relative to the growth of urban America, however, these increases were modest. The Census of 1880 found that 43.8% of Americans lived in the countryside; by 1920, the Census estimated that rural Americans made up only 30.1% of the population. During these forty years, many farmers also experienced considerable economic reverses; while 74.4% fully owned their farms in 1880 and only 25.6% were tenants, in 1920 farm ownership was down to 52.2% and tenancy had risen to 38.1%. At the same moment that agricultural productivity was sharply increasing, the stability of farm life seemed to be in rapid decline.⁸

From Ocala to Omaha, this conundrum haunted the thought of rural Americans. The continent's grand cities, especially New York and Chicago, were experiencing unprecedented growth and demanding more raw materials and agricultural commodities than ever before, and yet those regions that increased production to meet urban needs seemed to be declining in wealth and community stability. With the cities booming, how was it possible that the rural places tied to them could be facing such hard times?

To those who gathered at the first convention of the National People's Party in Nebraska in July 1892, the answer was clear: "The fruits of toil of millions are boldly stolen to build up colossal fortunes for a few," facilitated by the "[c]orruption [that] dominates the ballot-box, the legislatures, the Congress, and touches even the ermine of the bench." The crisis of the producing classes,

⁷ As a result of an abundance of births, however, the population of Kansas did increase from 1,428,000 to 1,470,000 during the 1890s. See United States Bureau of the Census, *Historical statistics of the United States, colonial times to 1957* (Washington: Government Printing Office, 1960), 12, 44.

⁸ Don E. Albrecht and Steve H. Murdock, *The sociology of U.S. agriculture: An ecological perspective* (Ames: Iowa State University Press, 1990), 43, 54.

agreed the reformers and representatives of the farmers' alliances and labor organizations, was at its root a social crisis; self-interested private parties, the Eastern financiers and the corporations, had been allowed to manipulate the government and rig the markets. The delegates met "in the midst of a nation brought to the verge of moral, political, and material ruin," in which "most of the States [had] been compelled to isolate the voters at the polling places to prevent universal intimidation and bribery." The Populists' solution was an expansion of "the power of government ... as rapidly and as far as the good sense of an intelligent people and the teachings of experience shall justify": currency regulation, a graduated income tax, and the nationalization of the railroads, telephones, and telegraphs. The social crisis required an expansion of the power of the federal government.⁹

Education, along with government intervention, held a central place in the Populists' program for combating the decline of rural America. The Farmers' Alliance, the national umbrella organization that coordinated the efforts of its various regional members, promoted a lecture circuit that reached hundreds of thousands of farmers in thirty different states. The National Reform Press Association, working in conjunction with the Alliance, built a network of over a thousand newspapers, magazines, and journals, distributing information throughout rural regions. The type of education the movement supported was pragmatic, aimed at producing individuals who could respond to new, unpredictable developments; the Alliance hoped to train farmers who had the social resources necessary to cope with the political and economic changes being set into motion by the dynamism of the cities. Charles Macune, one of the Alliance's most influential leaders, campaigned for a program of business education, a program that could turn farmers into accountants, marketers, or amateur economists. Populist reformers dreamed of agricultural producers who would not be content to merely grow their crops and then pass them on unquestioningly into the distribution chain, but would negotiate effectively with buyers, participate in collective struggles, and tackle the power imbalances that drove social inequities.¹⁰

These two Populist concerns, enlisting government in the cause of the growers and implementing a system of education that could produce a new type of farmer, intersected in the land-grant colleges, state colleges dedicated to agriculture, engineering, and industrial pursuits that had proliferated after passage of the federal Morrill Act of 1862. The land-grant colleges, explicitly dedicated to practical knowledge and vocational training, seemed to be precisely the types of institutions the agrarian movement might have supported, and yet Alan I. Marcus has found an astounding volume of criticism of the colleges in the rural press:

Farmers persistently voiced dissatisfaction, even disgust, with the condition of American agricultural colleges in the 1870s and 1880s ... they continually harped on the low attendance at these schools, complained about courses of study, and objected to individual faculty members. They

⁹ Quoted from the Omaha Platform, reproduced in William D.P. Bliss, ed., *The encyclopedia of social reform* (New York: Funk & Wagnalls, 1897), 954-955.

¹⁰ Charles Postel, *The populist vision* (Oxford: Oxford University Press, 2007), 49-50, 61-62.

often argued that these institutions did little more than absorb public funds and provide sinecures for those otherwise unable to find employment ... farmers frequently maintained that these colleges had not aided American agriculture and that the schools had not decreased the stream of farm children fleeing to the cities. In short, farmers contended that the colleges had failed to live up to their Morrill mandate; they regularly pronounced these schools unfit, not entitled to further federal aid.

The root cause of this criticism, according to Marcus, was a clash between two different approaches to what the American farm should become, two irreconcilable visions of "what the new farmer ought to be." The administrators and faculty of the land-grant colleges placed their faith in scientific agriculture, a research program that would employ modern scientific methods to analyze and improve agricultural techniques. The colleges would be sites for the training of a relatively small number of elite scientific investigators, who could then advise the farmers (and farming institutions, such as the federal and state departments of agriculture) about the best practices available; in this vision, "successful modern farming required agriculturalists to rely on others outside their cohort." The farmers, however, distrusted the academics; they recognized them as outsiders, members of a distinctly different class, and suspected their goals and allegiances. Instead of relying on scientists to alter farming methods, the agrarian movement envisioned the colleges becoming "places at which farmers who had studied and exhibited their mastery of the business and organizational principles of modern agriculture trained the next generation," where students would be instructed in practical techniques of farm management and operation honed through experience.¹¹

An essential component of the agrarians' college would be the demonstration farm, a small-scale fully-functional farm where instruction would take place. Students would be expected to devote several hours of each day to manual labor, working the farm and its implements to improve their practical aptitude; as Marcus notes, the colleges would attempt to "introduce mind into the agricultural equation and give it the role of directing the back, muscles, and hands." The administrators and professors of the colleges, by and large, opposed this form of immersive learning; they found manual labor "beneath the dignity of scientists" and a "[hindrance] to scientific study, either weakening the constitutions of young scholars or detracting from the time that should be used studying science." Much as field biologists and laboratory biologists disputed the importance of place and the uses of placelessness during this period, agrarian reformers sparred with scientific agriculturalists about the proper role for embodied experience in agricultural education.¹²

What was at stake in these pedagogical contests was who would control the modernization of American agriculture, whether it would be directed by those who worked the land or by a cadre of professionals constituted by their

¹¹ Alan I. Marcus, "The ivory silo: Farmer-agricultural college tensions in the 1870s and 1880s," *Agricultural history* 60 (Spring 1986): 22, 25, 28. For a description of the farmers' vision, see pp. 25-28. For a description of the scientific agriculturalists' vision, see pp. 28-31.

¹² *Ibid.*, 27, 30.

technical expertise. To the Populists, this distinction was crucial; the nature of one's relationship to the countryside determined what sorts of values one held. The Omaha Platform declared the land the repository of "all the natural sources of wealth" and proclaimed it ought remain "the heritage of the people"; alienation from the land threatened the very possibility of a rural livelihood, and a central plank of the People's Party platform was the prohibition of alien ownership of land. It was, ultimately, the land that bound urban America to rural America; it was on the produce of the land that the cities depended for their sustenance and the farmers for their livelihood. From the perspective of the urban commodity markets, though, the countryside was best envisioned as a series of transformable spaces, spaces which might be rationally remade through capital investment to maximize efficiency. To those who lived in rural America, these transformations threatened the obliteration of the places that gave them their means of subsistence, their community ties, their familial identities.¹³

In the midst of such a struggle, during a contest in which one's connection to the land held such heightened importance for social group membership, even discussions of the non-human world could become charged with broader socio-political meaning. As William Cronon observed in his history of nineteenth-century Chicago and its ties to its hinterlands, "[j]ust as our own lives continue to be embedded in a web of *natural* relationships, nothing in nature remains untouched by the web of *human* relationships that constitute our common history." Gaining knowledge about nature requires an observer to stake a position in relation to nature, to become a certain type of participant in nature, and any engagement with nature in those rural places in those years of strife could become a move, whether conscious or not, in a political chess match.¹⁴

III

In the autumn of 1901, William Castle and several of his students at Harvard began a series of experiments on the inbreeding of fruit flies. Pairs of flies, brother and sister, were placed in cylindrical battery jars, fed on fermented grapes or bananas, and left to reproduce. This was well before Thomas Hunt Morgan had turned *Drosophila* into a regular habitué of the genetics laboratory, so when, late in 1903, Castle petitioned the Carnegie Institution of Washington for funding to keep these experiments running, he did not even think it important to specify the organism with which he was working, referring to it vaguely as a "species of insect which breeds throughout the year and whose life cycle is completed within three weeks or less."¹⁵

As the experiment wound down thirty-three incestuous generations later, toward the end of 1905, Castle was surprised by its results. He had expected,

¹³ Bliss, *Encyclopedia of social reform*, 955.

¹⁴ William Cronon, *Nature's metropolis: Chicago and the Great West* (New York: W.W. Norton, 1991), 19.

¹⁵ W.E. Castle *et al.*, "The effects of inbreeding, cross-breeding, and selection upon the fertility and variability of *Drosophila*," *Proceedings of the American Academy of Arts and Sciences* 41 (May 1906): 733-735. On the early use of *Drosophila* in genetics laboratories, see Garland E. Allen, "The introduction of *Drosophila* into the study of heredity and evolution: 1900-1910," *Isis* 66 (September 1975): 327-329. William E. Castle to the Trustees of the Carnegie Institution, December 1, 1903, CAF-CIW, Box 9, Folder 11: Castle, W.E.

like previous investigators of close inbreeding, to find “decreased fertility, ... lack of vigor, diminution in size, partial or complete sterility, and pathological malformations” in his later generations, and at first it looked as though the flies might be headed that way. The inbred pairs rarely produced more than one hundred offspring per generation, while “[c]ontrol cultures made under identical conditions ... had a productiveness two or three times as great and showed no signs of sterility.” In the fifty-third generation, however, an inbred pair suddenly produced over two-hundred larva and showed signs of renewal for several generations. Castle now suspected that something besides the practice of inbreeding alone had driven the earlier deterioration, and after repeating the experiment with several new brother-sister pairs, each pair unrelated to the next, and finding that some pairs showed no signs whatsoever of decline, he concluded that the “low productiveness must have been present in the [original] stock at the beginning of our experiments and have persisted.” The *Drosophila* results seemed to demonstrate that “particular degrees of fertility are transmitted in certain families irrespective of the consanguinity of the parents”; the act of inbreeding was not itself the cause of physiological decline.¹⁶

These were, indeed, unexpected findings. The wide consensus among biologists of the second half of the nineteenth century, backed by abundant experimental evidence, was that inbreeding would lead inevitably to physiological deterioration. As early as *Origin of species*, Charles Darwin had “collected so large a body of facts, showing ... that close interbreeding diminishes vigour and fertility” that he was convinced there was a “general law of nature ... that no organic being self-fertilises itself for an eternity of generations”. Two more decades of information gathering and intermittent botanical experimentation only strengthened his belief in the “good effects of crossing, and ... the evil effects of close-interbreeding or self-fertilisation.” Between 1880 and 1900, several prominent European researchers, including August Weismann and Jan Ritzema-Bos, had also produced results in controlled laboratory experiments that indicated that inbreeding mice and rats reduced their fertility in the long run. And the biologists were not the only group at the turn of the century who were drawing these conclusions. “It is the opinion of most experienced animal breeders,” Castle thought, “that close inbreeding should be avoided because it has a tendency to decrease the size, vigor and fecundity of the race in which it is practiced.” His evidence drawn from the fruit flies, it seemed, stood alone.¹⁷

To change the view that inbreeding is inherently harmful, a view deeply held by not only scientists but the general public as well, Castle would need to mount a campaign of continuous outreach. Late in 1910, he delivered a public lecture at the Lowell Institute on “The effects of inbreeding”. Though he acknowledged in it that intensive inbreeding could under certain circumstances lead to unwanted outcomes, his central message was clear: “We may not ...

¹⁶ Castle *et al.*, “Effects of inbreeding,” 732-733, 739-740.

¹⁷ Darwin, *Origin of species*, 96-97. Charles Darwin, *The effects of cross and self fertilisation in the vegetable kingdom* (London: John Murray, 1876), 27. On Weismann, Ritzema-Bos, and other inbreeding experimenters of the 1880s and 1890s, see W.E. Castle, *Genetics and eugenics: A textbook for students of biology and a reference for animal and plant breeders* (Cambridge: Harvard University Press, 1916), 222, and Helen Dean King, *Studies on inbreeding* (Philadelphia: The Wistar Institute of Anatomy and Biology, 1919), 87-91. Castle, *Genetics and eugenics*, 219.

lightly ascribe to inbreeding or intermarriage the *creation* of bad racial traits, but only their manifestation." Crossing non-related individuals "tends to *hide* inherent defects, not to exterminate them," while inbreeding makes it possible to spot the individual carrying poor genes; as a consequence, "any racial stock which maintains a high standard of excellence under inbreeding is certainly one of great vigor, and free from inherent defects." Castle thought the wide prevalence of defective genes among humans justified the continuation of laws against marrying close relatives, but livestock managers should rethink their practices:

The animal breeder is ... amply justified in doing what human society at present is probably not warranted in doing, - viz. in practicing close inbreeding in building up families of superior excellence and then keeping these pure, while using them in crosses with other stocks. For an animal of such a superior race should have only vigorous, strong offspring if mated with a healthy individual of any family whatever, within the same species. For this reason the production of "thoroughbred" animals and their use in crosses is both scientifically correct and commercially remunerative.

Castle imagined herds of horses overseen like *Drosophila* in their jars, selected, sorted, purified.¹⁸

The lecture on inbreeding was one of eight public presentations to an urban audience that Castle gave in the winter of 1910 on "heredity in relation to evolution and animal breeding." Those lectures were delivered in Boston, but they had first been developed the previous summer when Castle lead a course at Iowa State's Graduate School of Agriculture at Ames, in the dead-center of the state. In the same way that Castle had shuttled from the metropolitan seaboard to the agricultural heartland, the topics of his lectures wandered from the city to the countryside, from the laboratory to the fields. There was, he asserted in his lectures, an inseparable connection between the rural places where he'd grown up and the burgeoning cities in which he and most of his colleagues now lived and worked:

The existence of civilized man rests ultimately on his ability to produce from the earth in sufficient abundance cultivated plants and domesticated animals. City populations are apt to forget this fundamental fact and to regard with indifference bordering at times on scorn agricultural districts and their workers. But let the steady stream of supplies coming from the land to any large city be interrupted for only a few days by war, floods, a railroad strike, or any similar occurrence, and this sentiment vanishes instantly. Man to live must have food, and food comes chiefly from the land.

A Bostonian standing near an open door to the auditorium of the Lowell Institute might well have mistaken the speaker inside for William Jennings Bryan.¹⁹

¹⁸ William E. Castle, *Heredity in relation to evolution and animal breeding* (New York: D. Appleton, 1911), 151-152.

Unlike Bryan, though, who would famously campaign to keep Darwin out of Tennessee, Castle vigorously argued the case that “the theory of organic evolution” needed to be carried into middle America. Darwin, he thought, could in two ways re-order the relationship between rural and urban spaces. First, Darwin’s understanding of evolution would drive human society to more conscientiously manage its affairs by bringing into being a new form of social awareness:

The evolutionary idea has forced man to consider the probable future of his own race on earth and to take measures to control that future, a matter he had previously left largely to fate. With a realization of the fact that organisms change from age to age and that he himself is one of these changing organisms man has attained not only a new ground for humility of spirit but also a new ground for optimism and for belief in his own supreme importance, since the forces which control his destiny have been placed largely in his own hands.

Understanding that they had emerged from a long cosmic material process, humans might now finally recognize their own ability to seize control of that process and use it to refashion their world.²⁰

Second, Darwin’s theory of natural selection, along with its more recent refinements, provided the new technical knowledge necessary for this re-engineering of nature for human betterment. Until recently, according to Castle, modifying organisms had been a haphazard project:

The production of new and improved breeds of animals and plants is historically a matter about which we know scarcely more than about the production of new species in nature. Selection has been undoubtedly the efficient cause of change in both cases, but how and why applied and to what sort of material is as uncertain in one case as in the other. The few great men who have succeeded in producing by their individual efforts a new and more useful type of animal or plant have worked largely by empirical methods. They have produced a desired result but by methods which neither they nor any one else fully understood or could adequately explain.

Breeders had traditionally worked diligently, but with little consciousness of what they were doing.²¹

A rigorous understanding of selection, however, would catapult both breeding practice and evolutionary theory into “an age of science,” an age in which “we are not satisfied with rule-of-thumb methods, we want to know the *why* as well as the *how* of our practical operations.” Pre-industrial economies were inefficient, employing “superfluous steps and roundabout methods,” but “the industrial history of the last century is full of instances in which a knowledge of causes in relation to processes, i.e. a *scientific* knowledge, has

¹⁹ *Ibid.*, 2.

²⁰ *Ibid.*, 1-2.

²¹ *Ibid.*, 3-4.

shortened and improved practice in quite unexpected ways.” In the realm of agriculture, “[b]efore Darwin the practices of animal and plant breeders were ... based on unreasoned past experience, just as was in antiquity the practice of metallurgy”; Darwinian evolution promised “a knowledge of causes in relation to processes” that could push breeding toward a more advanced industrial state, that could push production in agriculture to the scale of the mass-manufacturing concerns in the cities.²²

IV

By 1960, when Castle had retired to horses in California, the Midwest hardly resembled the countryside of his youth. The Census of that year found that the number of rural farmers in the United States had declined to 13.4 million, only 7.5% of the total population. Agricultural production had become concentrated in the largest, usually corporate-owned, farms; in 1959, the fifty percent of farms with the smallest acreage were responsible for only 8% of total production, while the twenty percent of farms with the largest acreage held about 74% of the total. Through large swaths of the country, human labor in the fields had been replaced by expensive fossil-fueled machinery. Across the Corn Belt, small lonely towns, fragments of what they’d once been, sat surrounded by vast fields of nearly-identical plants in perfect grid patterns, efficiently ordered for the mechanized combines.²³

“Long after the atomic age becomes commonplace,” wrote Henry A. Wallace in 1956, “men will still concern themselves with the nature of life and of the forces which change it.” And change it he had. As an early enthusiast for inbreeding, as the editor of an influential progressive farm journal, as the founder of one of the Midwest’s largest corn seed companies, and as Secretary of Agriculture throughout the New Deal, Wallace had done more than any other single individual to produce, protect, and promote the inbred strains of corn that were annually filling the fields with their seed. Hybrid corn, the product of crossings between these intensely-inbred lines, was custom-engineered for the new agribusiness climate of mid-century: the stalks that sprouted from Wallace’s seeds were sturdier and more uniform, could be planted in rows more closely together, and responded more favorably to chemical fertilizers than open-pollinated seed. These new corn plants were not only easier to harvest using machine combines than their predecessors, but they were actually more difficult for human labor to handle. As the largest producers invested heavily in new technologies, the percentage of American corn acreage planted in hybrid seed exploded from less than one-tenth of 1% in 1930 to 94% in 1960.²⁴

Creating hybrid seed required a long collaboration between the U.S. Department of Agriculture, state experiment stations, agricultural colleges, and commercial growers. And yet when Wallace reflected on the plant’s history, he

²² *Ibid.*, 3-5.

²³ Albrecht and Murdock, *Sociology of U.S. agriculture*, 157. Bruce L. Gardner, *American agriculture in the twentieth century: How it flourished and what it cost* (Cambridge: Harvard University Press, 2002), 67.

²⁴ Henry A. Wallace and William L. Brown, *Corn and its early fathers* (East Lansing: Michigan State University Press, 1956), 19. Jack Ralph Kloppenburg, *First the seed: The political economy of plant biotechnology, 1492-2000* (Cambridge: Cambridge University Press, 1988), 116-123.

chose a nineteenth-century naturalist, not a twentieth-century agricultural scientist, as “the Great Grandfather of Hybrid Corn”:

Charles Darwin did far more than propound the theory of evolution, the doctrine of natural selection and the descent of man. In 1876 he came out with a book on cross- and self-fertilization in plants, containing ideas which were destined to reach into the heart of the Corn Belt and change the nature of the corn plant for all time. Strangely enough, scarcely one in a hundred modern scientists has ever read the book in its entirety or knows that Darwin first observed hybrid vigor in corn in 1871 or thereabouts ... Today Darwin’s corn experiment would be looked on as utterly inadequate, in fact almost pitiable. And yet Darwin planted an idea which sprouted in Michigan, grew in Illinois, expanded in Connecticut and Long Island and finally found fruition to the amount of half a billion bushels of corn a year in the Corn Belt.

As Castle had half a century previously, Wallace continued to link Darwin’s research program to the industrialization of American agriculture.

At mid-century, he was not the only person celebrating a Darwinian heritage. While Darwinian natural selection had fallen out of favor among academic evolutionists at the turn of the century, by the 1950s, following the Modern Synthesis, academic biologists were again touting natural selection as the motive force in evolution. Darwin had returned as a celebrated icon within the scientific community, and science teachers in the United States were championing neo-Darwinism as the best exemplar of “the deep self-understanding of which humankind was capable if freed from the oppression of blind ideology.”²⁵

Was this re-emergence of Darwin as an icon among both academic biologists and scientific agriculturalists merely a coincidence? What was it about Darwin’s research program, sidelined by the 1890s, that made it so attractive in the new century? Why did a nineteenth-century natural historian suddenly become relevant to both university biologists and the pioneers of industrial agriculture?

We go a long way toward answering these questions if we open with this presupposition: Darwinism in its first century was not a dead academic discourse but a living transformative practice. As a practice, it oriented itself toward existing social and material relations, and it sought to reorder those relations based on its own set of valuations. Contrary to its self-presentation, Darwinism did not develop (to borrow from Pierre Bourdieu) in the imagined “situation of *skholè*, the free time, freed from the urgencies of the world, that allows a free and liberated relation to those urgencies and to the world,” but

²⁵ On the nadir of Darwinian selection in the evolutionary biology community, see Peter J. Bowler, *Evolution: The history of an idea*, rev. ed. (Berkeley: University of California Press, 1989), 246-281. John L. Rudolph, *Scientists in the classroom: The Cold War reconstruction of American science education* (New York: Palgrave, 2002), 149.

rather emerged out of an engaged dialogue, a protracted struggle, with existing social and material conditions.²⁶

The first generation of Darwinists was keenly aware of this, and the place of thought and the mind within the material and social environment was of prime importance to them. Consider, for example, an earlier Wallace, Alfred Russel Wallace, the famous co-discoverer of evolution by natural selection. Before the Anthropological Society of London in March 1864, he provided one of the earliest statements of what the appearance of a self-reflective mind meant for the Darwinian evolutionary process:

At length ... there came into existence a being in whom that subtle force we term *mind*, became of greater importance than his mere bodily structure ... Though unable to compete with the deer in swiftness, or with the wild bull in strength, *this* gave him weapons with which to capture or overcome both ... Though less capable than most other animals of living on the herbs and the fruits that unaided nature supplies, this wonderful faculty taught him to govern and direct nature to his own benefit, and make her produce food for him when and where he pleased ... From the moment when the first skin was used as a covering, when the first rude spear was formed to assist in the chase, the first seed sown or shoot planted, a grand revolution was effected in nature, a revolution which all the previous ages of the earth's history had had no parallel, for a being had arisen who was no longer necessarily subject to change with the changing universe – a being who was in some degree superior to nature, inasmuch, as he knew how to control and regulate her action, and could keep himself in harmony with her, not by a change in body, but by an advance of mind.

The mind was not a space separate from the evolutionary process, but an actor within it; the mind would unavoidably both *comprehend* and *transform* the world. To Wallace, the mind's appearance in humans marked a new stage in the history of the world and reoriented the course of historical development:

[T]his victory which he has gained for himself gives him a direct influence over other existences. Man has not only escaped "natural selection" himself, but he actually is able to take away some of that power from nature which, before his appearance, she universally exercised. We can anticipate the time when the earth will produce only cultivated plants and domestic animals; when man's selection shall have supplanted "natural selection"; and when the ocean will be the only domain in which that power can be exerted, which for countless cycles of ages ruled supreme over all the earth.

To Wallace, mind was a new force in the universe, paradoxically distinct from the power of nature and yet itself a force of nature. In this universe, thought was inseparable from action.²⁷

²⁶ Pierre Bourdieu, *Pascalian meditations*, trans. Richard Nice (Stanford: Stanford University Press, 2000), 1.

Throughout Wallace's presentation we find a glorification of mind, a celebration of the mind liberated from the body, the body liberated from nature. There is a sense throughout the talk of the mind as the *shaper*, not the *shaped*. Alfred Russel Wallace, though, was a body as well as a mind, and that body was situated in a particular place, and places, as Edward S. Casey reminds us, ultimately shape our thinking:

[I]t remains the case that where we are – the place we occupy, however briefly – has everything to do with what and who we are (and finally, *that* we are). This is so at the present moment: where you are right now is not a matter of indifference but affects the kind of person you are, what you have been doing in the past, even what you will be doing in the future. Your locus deeply influences what you perceive and what you expect to be the case ... Place itself is concrete and at one with action and thought.

Wallace's privileging of mind over body, mind over nature, was a decision fraught with material and social consequences, an act that, like many other formulations of the early Darwinists, advocated a particular course of action in the world.²⁸

By the middle of the twentieth century, the place of the mind in the world had largely dropped out of evolutionary inquiry. The primary problem of place that occupied academic evolutionary biologists now was how the theorist related to the field, not how the mind related to nature. A key concern was demonstrating that the quantitative modeling of Sewall Wright and R.A. Fisher accurately described natural selection as it occurs in the wilderness, and this epistemological quandary was taken up by E.B. Ford and Theodosius Dobzhansky in a manner that convinced most academic biologists. There was, however, also a second dimension of place, one which all evolutionary theorists post-Darwin had to confront but which was less frequently addressed consciously or worked out in careful analysis. This was the connection between the theorist and the *fields*, the sites of agricultural production. Charles Darwin made the analogy between natural selection and the selection practiced by humans on domesticated plants and animals a central, perhaps *the* central, pillar of his argument in *Origin of species*, and henceforward evolutionists needed to conceptualize how this form of selection related to their research programs as much as how natural selection did.

While the question of the relation of the theorist to the field remained a scholarly question, resolvable in the bounded space of *skholè*, the question of the relation of the theorist to the fields was unavoidably implicated in a larger political and economic struggle. The fields were a contested terrain, site of numerous social and environmental conflicts, not the least of which was the

²⁷ Alfred Russel Wallace, "The origin of human races and the antiquity of man deduced from the theory of 'natural selection'," *Journal of the Anthropological Society of London* 2 (1864): clxvii-clxviii. On the many ways in which Darwin and his followers tackled the nature-mind quandary, see Robert J. Richards, *Darwin and the emergence of evolutionary theories of mind and behavior* (Chicago: University of Chicago Press, 1989).

²⁸ Edward S. Casey, *Getting back into place: Toward a renewed understanding of the place-world* (Bloomington: Indiana University Press, 1993), xiii.

fierce dialectical clash between urban and rural forces. That academic biologists who utilized the fields for their research purposes so infrequently recognized, discussed, or consciously acted to influence these contests, even as their research programs played a major part in transforming the fields, suggests that they largely shared a set of values by mid-century that made their position in the fields seem natural, unchangeable, and generally unremarkable.

When the twentieth century opened, diverse theories of evolution, some incorporating and others downplaying the importance of natural selection, were guiding the research of life scientists. By the middle of the century, neo-Darwinism was well on its way to becoming a key orthodoxy of modern Anglo-American biology. During those same five decades, industrial agriculture, supported by new methods in plant and animal breeding, strikingly transformed the power dynamic between urban and rural America. Each of these two transitions, one involving ideas within a professional academic community and the other material relationships spanning a continent, has been well mapped by historians. Heretofore, these two stories have been treated largely in isolation from each other. For the next three chapters, we will examine key moments when these two stories overlapped, considering how and why so many of the important theorists of neo-Darwinism were also early architects of industrial breeding practices.

We will proceed using as our guiding light a vision of science sketched by Merleau-Ponty in *Phenomenology of perception*: “The whole universe of science is built upon the world as directly experienced, and if we want to subject science itself to rigorous scrutiny and arrive at a precise assessment of its meaning and scope, we must begin by reawakening the basic experience of the world of which science is the second-order expression.” That “world as directly experienced” is the “world which precedes knowledge, of which knowledge always *speaks*, and in relation to which every scientific schematization is an abstract and derivative sign-language, as is geography in relation to the countryside in which we have learnt beforehand what a forest, a prairie or a river is.” We cannot, of course, return first-hand to the world that the early neo-Darwinists experienced; that world has long since vanished from our senses. What we can do, though, is reconstruct that world based on extant evidence. Ideas are always forged in specific *places*, and by plumbing the second-hand accounts of a wide range of those who inhabited these places, we can form a more sophisticated (though always imperfect) representation of the places where neo-Darwinian theory was forged.²⁹

One difficulty, however, that we will quickly encounter is that places are possessed of infinite qualities. What a place *is* will be determined in relation to the sensory apparatuses of the living beings who inhabit it; further, the sensations that build place are filtered through the complex memories and second-order symbolic codes of those who experience them. Our trip back into the fields and laboratories of the first half of the twentieth century could explode into a meaning-free succession of sights, smells, sounds, and emotions that ultimately signify nothing. Our journey, therefore, will concentrate on those elements of place that are relevant to evolutionary ideas; it will examine how Darwinian and neo-Darwinian conceptions of the history of life structure the

²⁹ Merleau-Ponty, *Phenomenology of perception*, viii-ix.

relationships of their adherents to the non-human living world and to the human social order. Its goal will be to challenge the biologists' frequent invocation of the placelessness of the laboratory and the universality of experiments undertaken therein, to reject the bodiless vantage point that Darwin constructed in portions of *Origin of species*, and to remain cognizant of the importance of the fields as sites where battles between urban and rural forces over how to structure society were being waged. We will privilege place in order to see how different the story of ideas might appear when it is present, and, like Edward S. Casey, we will assume that "[h]owever lost we may become by gliding rapidly between places, however oblivious to place we may be in our thought and theory, and however much we may prefer to think of what happens in a place rather than of the place itself, we are tied to place undetachably and without reprieve."³⁰

Neo-Darwinism and industrial agriculture are both products of long, sprawling histories, so to keep our analysis focused we will examine in detail a narrower topic that runs through both: the history of inbreeding, in both theory and practice. During the first decade of the twentieth century, inbreeding, the mating of close relatives, became an important method by which scientific agriculturalists could produce new populations of plants and animals. These inbred organism populations proved remarkably useful for state and corporate agencies as they attempted to deal with farmers and manufacturers. Among evolutionary theorists, inbreeding remained a topic of research from the middle of the nineteenth century through the middle of the twentieth century. Charles Darwin wrote extensively about in-and-in breeding, as did most of the important contributors to population genetics in the 1920s and 1930s.

We open by tracing Charles Darwin's thoughts on inbreeding from the 1840s through the 1870s. Darwin first became interested in inbreeding while observing bees pollinating flowers in his garden, and over the next several decades the topic took on great theoretical importance for his work on evolution. One of his final publications was *The effects of self and cross fertilisation in the vegetable kingdom*, which examined in detail the impact of intense botanical inbreeding. Along with providing an introduction to mid-nineteenth century scientific understandings of inbreeding, our visitation at Down will demonstrate how Darwin constructed a unique vision of how natural selection was related to the selection practiced on crops and livestock by scientific breeders.

In our second chapter, we leave Darwin behind and turn from ideas about inbreeding to the actual practice of inbreeding as a technology of political ecology in the United States during the first decade of the twentieth century. Around 1903, breeders working for the Bureau of Plant Industry of the U.S. Department of Agriculture pioneered the use of intensive inbreeding to create crops that more efficiently linked crop growers to manufacturers and consumers. Recognizing the political utility of this method, an ambitious staff member of the Bureau of Animal Industry by the name of George Rommel promoted a scientific program to make the same results possible with livestock and poultry. By the second decade of the century, the U.S. Department of Agriculture had become a major patron of research into genetics and scientific breeding.

Our third chapter will move from government offices to university laboratories, following the lives and careers of the academic scientists who took

³⁰ Casey, *Getting back into place*, xiii.

up inbreeding research between 1903 and 1925. Here we will find that our two previous chapters converge: as these theorists pondered what new developments in genetics meant for evolution, they discovered a way to link the Darwinian research program of the nineteenth century to the scientific agricultural advances of the twentieth. These Neo-Darwinian biologists, especially those associated with Harvard's Bussey Institution, built academic careers while advancing the practical interests of the Department of Agriculture. These theorists were so successful within the community of academic biologists that the debt their work owed to practical breeding problems, the world outside of *skholè*, was often entirely forgotten by their successors.

What we will observe across this century's stretch, from Charles Darwin in his garden at Down to Henry Wallace touring Iowa's cornfields, is the elaboration of a new and potent form of political agro-ecology, one that by the end of the twentieth century would be entrenched in the United States and much of the rest of the world. What we will also observe is how the Darwinian research program, tied closely from its inception to scientific agriculture, emerged in a mutually-enriching relationship with this new political and economic form. Far from establishing itself as a pure academic discipline in the free space of *skholè*, neo-Darwinian evolutionary studies remained lodged in the corporate and government practices from which it had drawn much of its strength.

Chapter One

“An experiment on a gigantic scale”: Charles Darwin on domesticated nature, inbreeding, and the inevitable unfolding of human history, 1859-1876

In the spring of 1869, nearly a decade after the publication of *Origin of species* and more than thirty years after he'd returned from the voyage of the *Beagle*, Charles Darwin made an observation in his greenhouse at Down in the Kent countryside that threatened one of the central pillars of his theory of the transmutation of species. Under a fine white cotton mesh, in place to keep out stray insects, Darwin had planted two common morning glories on opposite sides of a single large pot. The vines had climbed up two poles under the mesh, coiling in loops as their flowers of purple and white blossomed in the warmth of May. Darwin untangled the two long stems from their poles and measured the length of each: the first was 7 feet and 2½ inches long, while the second was 7 feet and 3 inches long. This half-inch difference, so seemingly insignificant, perturbed Darwin so much that he set the taller plant aside and began formulating new experiments to try out on it to see if this result might be an anomaly. He even gave the vine its own name to distinguish it from all his other morning glories: the Hero.³¹

What so unsettled Darwin about Hero was that the vine had emerged, tall and healthy, in the sixth generation of a line of intensely-inbred plants that had been otherwise unimpressive, a stunted lineage that he had hoped would provide incontrovertible empirical evidence of the physiological perils brought on by close inbreeding. The experiment with morning glories was just one of a number of similar horticultural experiments underway in 1869 in the greenhouse at Down. This particular experiment, the most troublesome to date, had begun in the autumn of 1866 when Darwin planted a store-bought morning glory seed in a pot. After this foundational vine grew and began blooming, Darwin introduced pollen from a different, unrelated morning glory to some of its flowers; he allowed several other flowers on the vine to fertilize themselves with their own pollen. What resulted were two different categories of new seed, the first cross-fertilized and the second self-fertilized. After letting both types of seed germinate in a damp sandy tumbler atop the mantle of a chimney in one of the warmer rooms at Down House, Darwin placed one self-fertilized seed and one cross-fertilized seed on opposite sides of five different pots, keeping the two sides of each pot apart with thin dividing partitions. He made certain that the conditions on both sides of each pot, from soil composition to watering time to light intensity, were identical. In all five pots, morning glory vines quickly began to inch upward, and as soon as one of the pair in a pot reached the top of its pole

³¹ Charles Darwin, *The effects of cross and self fertilisation in the vegetable kingdom* (London: John Murray, 1876), 37. In this chapter of *Effects of cross and self fertilisation*, Darwin provides height results for his plants, but he does not supply a chronology that would allow us to determine when his experiments took place. For details on when Darwin undertook his inbreeding experiment with morning glories, see DMC-CUL 78.72 to DMC-CUL 78.111, his notes on the undertaking. For information specifically about the sixth generation of the experiment, Hero's generation, see DMC-CUL 78.92 and DMC-CUL 78.93.

Darwin terminated the experiment and measured the heights of the two plants to see how different their heights were.³²

The results in the first generation were precisely what he had hoped they would be: in all five pots, the plants rising from cross-fertilized seed grew taller and produced more seed capsules than the plants grown from self-fertilized seed. The differences in their heights were non-trivial: for every 10.0 inches of cross-fertilized stem, there were only 7.6 inches of self-fertilized stem. Encouraged by these initial findings, Darwin and his gardener, Henry Lettington, carried the experiment into a second generation. Once again, the selfed plants impregnated themselves, while Darwin or Lettington introduced to each crossed member of the first generation pollen from one of its siblings. When the next growing season came to a close, each of the cross-fertilized vines was taller than its self-fertilized cohabitant, with a significant height ratio of 10.0 to 7.9.³³

For eight more generations, Darwin and Lettington continued these controlled matings. In each of these generations, the experiment produced quantitative evidence that suggested that heavily inbred morning glories were inferior by most measures of survival to their cross-bred peers. Surveying the results years later, Darwin noted that his data might “be best appreciated by an illustration: If all the men in a country were on an average 6 feet high, and there were some families which had been long and closely interbred, these would be almost dwarfs, their average height during ten generations being only 4 feet 8¼ inches.”³⁴

While the numbers supported Darwin’s contention that close inbreeding brought on harmful consequences, the physical changes that had taken place in the self-fertilized line offered even more compelling testimony that something unusual was afoot. The flowers of the later inbred generations were more likely to fall off their vines before bearing seed, a sign, Darwin thought, of impending sterility. On average, the flowers on the inbred vines also had shorter, thinner stamens, the “male” botanical organs on which pollen is stored, and the amount of pollen on each of these stamens was, according to Darwin, “as far as could be judged by the eye, about half of that contained in one from a crossed plant.” Most striking of all was the color change that had occurred in the petals of the flowers of the inbred vines. During the seventh generation of the experiment, Lettington informed Darwin that it was no longer necessary to label which of the plants had been self-fertilized, for while the flowers on the crossed morning glories, like those grown from commercial seed, varied in color from pink to deep purple to bright white, the inbred vines were only producing flowers of one single peculiar “tint, namely, of a rich dark purple.” In just seven generations, color variation had leached from the flowers of the self-fertilized vines.³⁵

With ten generations of nearly unambiguous evidence in his notebooks, Darwin was just about ready to declare intensive inbreeding a complete biological hazard, but one firm anomaly stood in his way: Hero the self-fertilized morning glory. During the sixth generation, Hero had grown half an inch taller

³² DMC-CUL 78.72. Darwin, *Effects of cross and self fertilisation*, 11-13, 28-29.

³³ Darwin, *Effects of cross and self fertilisation*, 29, 31.

³⁴ *Ibid.*, 53.

³⁵ *Ibid.*, 59.

than the crossed vine with which it shared its pot, making it the first inbred plant in the experiment to beat its opponent in, as Darwin phrased it, a “victory [that] was fairly won after a long struggle.” Darwin might, of course, have dismissed this single contest as a chance accident, but Hero, it turned out, had mighty offspring. In the next generation, Hero’s descendants, all self-fertilized, not only grew taller than the other selfed morning glories by a ratio of 10.0 to 8.4, but even exceeded the cross-fertilized plants of the seventh generation by a ratio of 10.0 to 9.5. To make matters more complex, Hero’s self-fertilized grand-children, members of a line that had been inbred for eight generations, grew taller by a ratio of 10.0 to 9.4 than vines produced by cross-fertilizing Hero’s descendants of the previous generation. Reluctantly, Darwin was forced to conclude that, somehow, Hero had “transmitted to its offspring a peculiar constitution adapted for self-fertilisation.”³⁶

This was not a happy admission for Darwin, for it opened the possibility that inbreeding, rather than always being deleterious, might under certain circumstances become advantageous, and that the ability to endure intense inbreeding might perhaps even be an adaptive trait of some populations. And this would be a troubling conclusion indeed, for since the first publication of *Origin of species* Darwin had been constructing a complicated argument about the relationship between artificial selection and natural selection that required perpetual self-crossing to lead always to senescence and sterility. Hero and its kin challenged Darwin’s initial findings about inbreeding, and as a result undermined what he had thought was one of the most persuasive lines of evidence for his grand theory of evolution. The tendrils in the greenhouse at Down creeping up their rods were supposed to generate the quantitative data that would complete a complex line of argument that Darwin had been assembling for decades about the relationship between species in the wild and the domesticated varieties that had emerged during human history. Instead, showing total disregard for the great evolutionist’s plans, these inbred rebels twined themselves upward in their pots and threatened his version of the theory of evolution by natural selection, along with the history of domestication he had built around it. To understand the challenge that these bizarre plants posed, we must return to Darwin’s first public enunciation of his theory, and to a decision that he made then, on the cusp of his celebrity, that, though it appeared trivial at the time, proved to be fraught with consequences for the coming century.

II

When in June of 1858 Darwin unexpectedly received in the post a draft manuscript on natural selection from Alfred Russel Wallace, his immediate response was panic: here in front of him in another man’s hand was his own theory, which had been percolating in his mind and in scattered form in his notebooks for decades. In a letter to his close friend and colleague Charles Lyell, he wrote in exasperation that he had never seen “a more striking coincidence,” and he worried that “all [his] originality, whatever it may amount to, will be smashed.” As Lyell and Joseph Hooker, Darwin’s other close confidante, hastily prepared a response to the incident that would preserve their friend’s priority,

³⁶ *Ibid.*, 37, 48-50.

they made the assumption, based on Darwin's reaction, that the two theories of natural selection were substantively the same, differing only on trivial matters. And when, on July 1st, the two separate versions of the theory were read together before the Linnaean Society of London, they were submitted as proof that these two thinkers, each working without the knowledge of the other, had "conceived the same very ingenious theory to account for the appearance and perpetuation of varieties and of specific forms on our planet."³⁷

There was, however, at least one crucial aspect of the theory of evolution put forward in London in 1858 on which Darwin and Wallace openly and irreconcilably disagreed: whether the varieties and breeds of animals and plants that had been brought into being by breeders during human history had been produced by the same force that formed species over very long spans of time in the wild. By late June of 1858, Darwin had recognized this, noting in a letter to Lyell that he and Wallace "differ only, [in] that I was led to my views from what artificial selection has done for domestic animals," while Wallace stated clearly in his manuscript that he believed "that no inferences as to varieties in a state of nature can be deduced from the observation of those occurring among domestic animals." Darwin and his close circle overlooked this disagreement as they organized the joint presentation because they believed the mechanism driving evolution, natural selection, was for the most part the same in both formulations of the theory; their concern was to secure for their friend recognition among naturalists. What they missed, though, was how radically different the implications arising from each theory were for those making sense of all the organisms on the planet, not just those in a state of the "wild". Both theories agreed on what nature before human history was like, but they were fundamentally opposed when it came to conceptualizing domesticated nature within human civilizations.³⁸

Wallace, the field naturalist, the former collector of exotic specimens in the Amazonian rainforest, traveling in 1858 throughout the Malay Archipelago, thought domesticated animals to be strange creatures "which never occur and never can occur in a state of nature." An animal in the wild depended "upon the full exercise and healthy condition of all [its] senses and physical powers," with "no muscle of its body that is not called into daily and hourly activity," while its pampered domestic cousin, fed and sheltered by humans, cut a much less impressive figure: "Half of its senses and faculties are quite useless; and the other half are but occasionally called into feeble exercise, while even its muscular system is only irregularly called into action." If reintroduced into a state of

³⁷ Francis Darwin, ed., *The life and letters of Charles Darwin* (New York: D. Appleton, 1904), 1: 473. Charles Darwin and Alfred Wallace, "On the tendency of species to form varieties; and on the perpetuation of varieties and species by natural means of selection," *Journal of the proceedings of the Linnean Society of London, Zoology* 3 (20 August 1858): 45. The response of Darwin, Hooker, and Lyell to Wallace's letter has been described in numerous biographies; a clear narration of the events of June and July 1858 can be found in Janet Browne, *Charles Darwin: The power of place* (Princeton: Princeton University Press, 2002), 3-42. For an account of the incident from Wallace's perspective, see Ross A. Slotten, *The heretic in Darwin's court: The life of Alfred Russel Wallace* (New York: Columbia University Press, 2004), 141-178.

³⁸ For a discussion of how this difference affected the level of selection that each naturalist believed to be most important, see Jean Gayon, *Darwinism's struggle for survival: Heredity and the hypothesis of natural selection*, trans. Matthew Cobb (Cambridge: Cambridge University Press, 1998), 19-59. Darwin, *Life and letters*, 474. Darwin and Wallace, "On the tendency," 61.

nature, the domestic animal “must return to something near the type of the original wild stock, or become altogether extinct.”³⁹

This strong distinction between wild and domesticated varieties was a necessary element of Wallace’s argument for natural selection. How species, natural varieties, and domesticated varieties differed from each other was a question of great importance to naturalists of the mid-nineteenth century, and the manuscript that Wallace sent to Darwin tried to find a way to coherently relate these groupings in order to argue for evolutionary transmutation. The line of reasoning that Wallace would bring forth in the manuscript was first hinted at in a short note that he published in a zoology journal early in 1858. How, he asked, do we tell the difference between a species and a variety in nature? Each naturalist, he answered, uses a different, idiosyncratic set of criteria to draw this distinction, deciding arbitrarily what degree of variation is needed before an organism can be assigned to a variety under the larger species instead of to a species of its own. He hinted that the way out of this taxonomic morass, which had created a great deal of confusion and left naturalists talking past each other, was “by considering the permanence, not the amount, of the variation from its nearest allies” to characterize species and the “instability, not the smaller quantity, of variation to mark the variety.” The numerous criteria that naturalists had been using to distinguish species from varieties in effect assumed that species and varieties are “of exactly the same nature, and differ only in degree,” while the criterion that Wallace posited “define[d] the two things by a difference in their nature.” Hypothetically, species are fixed over time, while varieties are in a state of constant flux.⁴⁰

Once a naturalist accepted these reasonable assumptions, however, Wallace argued that it would, paradoxically, become very difficult for him to continue to maintain that species are the unvarying result of a special creation! If, after all, varieties “have been produced by ordinary generation from a parent species,” and often physically differ from species themselves by such a minor degree that observers of long experience have trouble agreeing on which animals belong to varieties and which do not, then “why should a special act of creation be required to call into existence an organism differing only in degree from another which has been produced by existing laws? If an amount of permanent difference, represented by any number up to 10, may be produced by the ordinary course of nature, it is surely most illogical to suppose, and very hard to believe, that an amount of difference represented by 11 required a special act of creation to call it into existence.” To Wallace, the best argument for the transmutation of species is that varieties in nature, which most naturalists agreed had emerged from species and changed over time, were so difficult to distinguish from purportedly-fixed species.⁴¹

Explaining *how* these natural varieties become new species was the main goal of the theory that arrived on Darwin’s doorstep in June 1858. In a state of the wild, species exist side-by-side with their many varieties of finely-graded difference; as environmental conditions change some of these natural varieties

³⁹ Darwin and Wallace, “On the tendency,” 59-61.

⁴⁰ Alfred Russel Wallace, “Note on the theory of permanent and geographical varieties,” *The zoologist* 16 (1858): 5887.

⁴¹ *Ibid.*, 5888.

become better survivors than others, and as the weaker varieties fade the species as a whole comes more-and-more to resemble the stronger varieties. At the end of this process, “the variety would now have replaced the species, of which it would be a more perfectly developed and more highly organized form. It would be in all respects better adapted to secure its safety, and to prolong its individual existence and that of the race.” What guaranteed that the process would continue, that the new species would birth new varieties, was a “tendency in nature to the continued progression of certain classes of varieties further and further from the original type – a progression to which it appears no reason to assign any definite limits,” a tendency which, Wallace believed, would allow his selection mechanism to be indefinitely renewed.⁴²

The greatest threat, Wallace thought, to acceptance of this theory was the widespread belief that “varieties occurring in a state of nature are in all respects analogous to or even identical with those of domestic animals.” Wallace believed that domesticated breeds, unlike wild varieties, would in time revert to their parental form; the forward evolutionary progress driven in nature by the tendency of species to generate new varieties was nowhere to be found among organisms that had come into close relations with human civilizations. Domestication had increased the variation in traits in many plants and animals, but nowhere had it brought into being the radically new forms that Wallace’s evolutionary theory projected. A domesticated variety had never become a new species. If domesticated and natural varieties were the same things, Wallace knew his theory would be challenged by the vast amount of empirical evidence that could be provided by livestock, crops, and pets, and he was not prepared to move into that largely uncharted realm. It would be better to assert, he thought, that domesticated animals are “abnormal, irregular, artificial,” keep a solid boundary between the domain of the naturalist and the domain of the breeder, and found a theory of evolution by selection on the connection between species and varieties in the wild.⁴³

Charles Darwin, country gent, entertained a position on domestication wholly opposed to Wallace’s. His conception of evolution by natural selection had emerged dialogically, bit-by-bit, from his encounters, both first- and second-hand, with domesticated plants and animals. In the years following his circumnavigation of the globe, as he scribbled his loose thoughts into notebooks and as his theory began to take form, Darwin regularly consulted the animal breeding literature, seeking exemplary cases that would help to sharpen his ideas; the cattle breeders, especially John Wilkinson and Sir John Saunders Sebright, proved especially influential. In 1839, as he became immersed in the practical arcana of the art of animal improvement, Darwin composed a series of questions about animal breeding and distributed them to as many practicing breeders as he could find; while this particular survey brought few responses, Darwin would in time cultivate personal connections with experienced breeders that would allow him to build a global correspondence network from his isolated country estate. His experience with domesticated animals was not limited, though, to these second-hand accounts. In the mid-1850s, after toying briefly with cabbages and ducks, Darwin (and his children living with him at Down)

⁴² Darwin and Wallace, “On the tendency,” 58, 62.

⁴³ *Ibid.*, 53, 61.

plunged fully into the world of pigeons: he began keeping the birds in a small house near his garden (as many as ninety individuals by 1857) for experimentation and observation, he mingled with the pigeon fanatics and observed the many bizarre ornithological specimens that they had brought into being at meetings of the exclusive Philoperisteron Society, he kept abreast of the latest developments in the fanciers' periodicals, and he corresponded with those who knew pigeons best, particularly with the journalist William B. Tegetmeier. To Darwin, domesticated animals were familiar, ordinary, and perhaps even comforting.⁴⁴

Perhaps unsurprisingly, then, Darwin's formulation of evolution by natural selection treated varieties in nature and domesticated varieties as essentially the same things. In the excerpt from one of his early manuscripts that Lyell and Hooker transmitted to the Linnaean Society in 1858, Darwin described a Malthusian nature in which "yearly more are bred than can survive," where "the smallest grain in the balance, in the long run, must tell on which death shall fall, and which shall survive," a nature in which various environmental forces constantly select the favored few that will engender the next generation. Instead, however, of explaining the means by which a variety in nature might through this state of differential survival overtake its parent species, as Wallace had, Darwin brought into his service the example of two of Britain's best known livestock improvers: "Let this work of selection on the one hand, and death on the other, go on for a thousand generations, who will pretend to affirm that it would produce no effect, when we remember what, in a few years, [Robert] Bakewell effected in cattle, and [Charles Callis] Western in sheep, by this identical principle of selection?" Central to Darwin's theory was the idea that the process in nature that generated new varieties was the same process followed by the breeder; domestic breeds were one specific type of variety, no different in essence than the varieties that had for so long vexed naturalists, though considerably easier to observe and comprehend.⁴⁵

Darwin believed so strongly in this link between natural and domesticated varieties that he made it the opening gambit in the famous treatise on evolution by natural selection that he rapidly assembled between August 1858 and May 1859. In the first chapter of that monograph, which would reach the general public in Britain in November 1859 as *On the origin of species by means of*

⁴⁴ The precise details of Darwin's engagement with domestic organisms from the voyage of the *Beagle* through the publication of *Origin* have been carefully recorded in L.T. Evans, "Darwin's use of the analogy between artificial and natural selection," *Journal of the history of biology* 17, no. 1 (Spring 1984): 113-140. A discussion of specific issues, such as prepotency, reversion, and telegony, relating to inheritance in Darwin's work with domesticates can be found in Mary M. Bartley, "Darwin and domestication: Studies on inheritance," *Journal of the history of biology* 25, no. 2 (Summer 1992): 307-333. On the influence of Sebright and Saunders on Darwin's early thoughts about the transmutation of species, see Evans, "Darwin's use," 119-120. On the 1839 pamphlet, see R.B. Freeman and P.J. Gautrey, "Darwin's *Questions about the breeding of animals*, with a note on *Queries about expression*," *Journal of the society for the bibliography of natural history* 5, no. 3 (1969): 220-225. On Darwin and pigeons, see James A. Secord, "Nature's fancy: Charles Darwin and the breeding of pigeons," *Isis* 72 (1981): 163-186.

⁴⁵ Darwin and Wallace, "On the tendency," 49. For an account of how Robert Bakewell's breeding work was used by Darwin, see Roger J. Wood, "Robert Bakewell (1725-1795): Pioneer animal breeder and his influence on Charles Darwin," *Folia mendeliana: Papers relating to Mendel and to the early development of genetics* 8 (1973): 231-242.

natural selection, the pigeon is the first organism a reader meets in detail. "Altogether at least a score of pigeons might be chosen, which if shown to an ornithologist, and he were told that they were wild birds, would certainly, I think, be ranked by him as well-defined species," Darwin noted, and yet each of these remarkable domesticated varieties had descended from the dour grey rock dove from the wild. What had brought about this extraordinary transformation was the principle of selection, and selection was simply the breeder's method of choosing which individuals might give birth to the next generation; invoking the words of the veterinarian William Youatt, Darwin called selection "the magician's wand, by means of which he [the livestock breeder] may summon into life whatever form and mould he pleases."⁴⁶

Darwin first introduced his ideas to his audience of new readers in the domesticated realm, which he sensed they would find familiar and unproblematic, before leading them to his primary destination, selection in the wild, a bitter, controversial, potentially disorienting concept. If, he suggested, animals in a state of nature vary in their traits and capacities as their domesticated cousins do, and if, in a state of nature, more animals are produced in each generation than can possibly survive, doesn't Nature, by choosing which organisms live and die, perform an operation directly analogous to the breeder's? If, he continued, "man can produce and certainly has produced a great result by ... means of selection, what may not nature effect?" The principle followed by the breeders, what we might call *artificial selection*, is but one case of a universal principle that has been at work throughout the history of life on the planet earth, *natural selection*. With this principle of selection in hand, Nature, like a master breeder, "is daily and hourly scrutinising, throughout the world, every variation, even the slightest ... silently and insensibly working, whenever and wherever opportunity offers, at the improvement of each organic being."⁴⁷ What Nature has done is no different than what Robert Bakewell did with his herds and flocks.

As *Origin of species* sold out at the bookstand and was re-released through multiple editions (three by 1861), Darwin's version of the theory of evolution by natural selection became *the* theory of evolution by natural selection. In the book's introduction, Darwin pays tribute to Wallace as the catalyst that drove him to publish, the naturalist who "arrived at almost exactly the same general conclusions that [he had] on the origin of species." And so Wallace would be remembered. But in June 1858 there were two distinct versions of the theory of evolution by natural selection, one that depended on artificial selection and domesticated breeds and one that dodged them. For the rest of his career, Darwin would need to devise a way to defend from its opponents not only the theory of evolution by natural selection, but also his particular formulation of it, with its heavy reliance on artificial selection.⁴⁸

⁴⁶ For an account of this period, see Browne, *Darwin: Power of place*, 47-63. Darwin, *Origin of species*, 22, 31.

⁴⁷ Darwin, *Origin of species*, 83-84.

⁴⁸ *Ibid.*, 2.

III

Charles Robert Bree, a physician and amateur ornithologist from Essex, was not won over by Darwin's argument. In an attempt to refute *Origin of species* and to defend a worldview in which life was the product of "distinct acts of special creation, by Him whose wisdom our finite minds are too apt to interpret and criticise," Bree directly challenged Darwin's use of artificial selection to stand in for natural selection:

That domestic animals of the same genus will modify, as Mr. Darwin has shewn, no one ever doubted ... But the pigeon reared by the fancier is still a pigeon; the short-horned ox and the Devon are still most unmistakably bovine; the racer and the cart-horse still proclaim their brotherhood; the greyhound and the spaniel are still dogs! ... [H]ow immensely different is the question of such a modification from that which by any process of natural change, could convert the water-breathing fish into the air-breathing mammal, or the bird with air-filled bones ... and its complex flying apparatus, into the crawling reptile, the fish, or the quadruped?

While he accepted that selection might bring about great changes in domesticated organisms, Bree drew a sharp distinction between *species* and *varieties* (which, like Darwin, he envisioned as domesticates) and refused thereby to allow Darwin's extensive evidence for artificial selection to become evidence for natural selection.⁴⁹

Similar critiques of Darwin's doctrine surfaced repeatedly in 1859 and 1860 as reviewers granted the power of selection to modify domesticated varieties while denying its ability to alter species. A piece in the *Daily News* noted that, "Man has never converted his pigeon into any other bird than an undoubted pigeon," while an anonymous voice in *The Record* declared that one of the "axiomatic truths of Mr. Darwin's peculiar science" was the odd assumption that "'varieties' and 'species' are but terms for the same thing," an assumption for which "not a shadow of proof is given." Another anonymous writer, "eminent in the world of science" (Darwin later determined it was the geologist Adam Sedgwick, his former teacher in Cambridge), complained that "the only facts [Darwin] pretends to adduce, as true elements of proof, are the *varieties* produced by domestication, or the human *artifice* of cross-breeding." While one might be impressed by "how very unlike are poodles and greyhounds," the creation of a new species required "the operation of a power quite beyond the powers of a pigeon-fancier, a cross-breeder, or hybridizer." Darwin read each of these reviews and recognized that to defend his project he needed to demonstrate convincingly, using widely-accepted facts, that natural varieties, species, and domesticated varieties differ only in degree, not in kind.⁵⁰

⁴⁹ C.R. Bree, *Species not transmutable, nor the result of secondary causes* (London: Groombridge and Sons, 1860), 9, 13-14.

⁵⁰ See DMC-CUL 226.1.37-39, DMC-CUL 226.1.139-140, and DMC-CUL 226.1.161. Darwin scribbled "Sedgwick" in one of the margins of his copy of the review. These reviews and many others can be found in a large scrapbook that Darwin kept that is currently a part of the Darwin Manuscript Collection of the Cambridge University Library, listed as number 226.

He thought the best line of reasoning might be constructed using hybrids, organisms like mules or ligers that are produced by the crossing of two distinct species. His argument about the importance of hybrids, which at times descended into byzantine complexity, was outlined fully in the eighth chapter of *Origin of species*. It was subsequently ignored or dismissed by almost all of the commentators on the book. Nevertheless, Darwin returned to the argument frequently in the final two decades of his scientific career, hoping that with every new fact he gathered he might marshal the evidence into a more robust form to shore up his understanding of species and varieties.

The eighth chapter of *Origin* opens with a question: What precisely is the difference between a species and a variety? Darwin answers that, traditionally, naturalists have believed that species, when crossed, “have been specially endowed with the quality of sterility, in order to prevent the confusion of all organic forms,” while the different varieties of a given species are generally capable of mating to produce viable offspring. Varieties can cross, but species cannot, and the sterility between species is part of the Creator’s plan to keep species distinct. By turning to hybrids, Darwin sought to poke holes in this line of reasoning, first by challenging whether crossed species are wholly sterile and secondly by showing that the barriers to reproduction between both species and varieties were caused by a law of reproduction common to both, not some divine decree applicable only to species.⁵¹

In 1859, Darwin had limited first-hand experience with hybridization, so he depended for his evidence on the published findings of the botanical hybridizers of the past hundred years (animals were a poor choice of subject, he thought, because “much fewer experiments have been carefully tried than with plants”). Joseph Gottlieb Kölreuter, an eighteenth-century botanist who had studied tobacco hybrids in detail and undertaken over five hundred experiments, provided a vast amount of data for contemplation, while Darwin’s contemporary Carl Friedrich von Gärtner summarized in an 1849 monograph the great deal of research undertaken since Kölreuter’s time and added to it the results of thousands of experiments of his own. Darwin faced a considerable difficulty in using Kölreuter’s and Gärtner’s findings, for both hybridizers, like virtually all of their collaborators, believed in Divine Creation, the immutability of species, and the sharp division between species and varieties. He surmounted this by pitting the two botanists against each other: both agreed on what constituted a species and what a variety, both had devoted their lives to studying their plants with the utmost care, and yet frequently the two disagreed about which specimens belonged to distinct species and which were merely varieties. If, Darwin asked, “the two most experienced observers who have ever lived ... arrived at diametrically opposite conclusions in regard to some of the very same forms,” then the evidence must not consist only of obviously cross-fertile varieties and obviously cross-sterile species. It must be possible, on some occasions, for organisms from what we recognize as two different species to mate to produce hybrids. Likewise, while domesticated varieties can almost always be crossed, when “we look to varieties produced under nature, we are immediately involved in hopeless difficulties; for if two hitherto reputed varieties be found in

⁵¹ Charles Darwin, *The Origin of species: A variorum text*, ed. Morse Peckham (Philadelphia: University of Pennsylvania Press, 1959), 424.

any degree sterile together, they are at once ranked by most naturalists as species." With a touch of sarcasm, Darwin noted that, "If we thus argue in a circle, the fertility of all varieties produced under nature will assuredly have to be granted." His review of the evidence from botanical hybridization experiments was clear and emphatic: "It can ... be shown that neither sterility nor fertility affords any clear distinction between species and varieties."⁵²

Few of Darwin's critics were convinced by this line of reasoning. "There is no evidence offered," wrote one, "beyond the mere fact that naturalists sometimes differ in their calculations, one esteeming that to be a variety which another ranks as a species." Another noted that the fact that "there may have been very many blunders among naturalists, in the discrimination and enumeration of species" was hardly a sufficient reason to question "the grand truth of nature, and the continuity of species." Darwin needed a more convincing line of argument, preferably grounded in abundant empirical evidence, to defend his reinterpretation of Gärtner's and Kölreuter's work.⁵³

What he chose to do, very ambitiously, was to explain in clear terms *why* sterility developed across both species and varieties. If he could demonstrate this, then he could show that species and varieties are not in essence different, but rather are marked by differing degrees of fertility and sterility as determined by a shared underlying law of nature. And once this law was made apparent, he could gather material evidence to support it, thereby justifying his use of domesticated varieties as stand-ins for natural varieties and species.

The task was a difficult one, a form of explanation different from most provided in *Origin of species*. Darwin had become experienced at explaining why a physical trait or behavioral instinct might be adaptive for an organism; he had discussed at length how natural selection might favor a structure that aided an organism in its survival or reproduction. What he had to do now, however, was explain how the inability of species to cross had arisen, and clearly the "sterility of hybrids could not possibly be of any advantage to them, and therefore could not have been acquired by the continued preservation of successive profitable degrees of sterility." Those who opposed evolution saw sterility as a designed element intended to keep species apart, but Darwin proposed instead that "sterility is not a specially acquired or endowed quality, but is incidental on other acquired differences"; it was an unintended side-effect of some other law of nature at work in the universe.⁵⁴

To elucidate that law, Darwin noted that there were actually two situations in nature that could bring about sterility. The first and most obvious was the crossing of different species or varieties. The more morphologically different the two were the less likely it would be that they could cross. An enterprising breeder might dream of mating a lion to a tiger, but nobody seriously entertained the idea of crossing an elephant with a mouse. On the

⁵² *Ibid.*, 432. The monograph by von Gärtner was *Versuche und Beobachtungen über die Bastardzeugung im Pflanzenreich*. For a brief introduction to the work of Kölreuter, see Thomas L. Hankins, *Science and the Enlightenment* (Cambridge: Cambridge University Press, 1985), 137-139. On Gärtner, see Lois N. Magner, *A history of the life sciences*, 3rd ed. (New York: Marcel Dekker, 2002), 375-376. Darwin, *Origin of species: Variorum*, 427, 459-460.

⁵³ See DMC-CUL 226.1.139-140 and DMC-CUL 226.1.161.

⁵⁴ Darwin, *Origin of species: Variorum*, 424-425.

opposite extreme, however, close relatives often encountered major complications when they paired. Darwin had “collected so large a body of facts, showing that close interbreeding lessens fertility” that he could not “doubt the correctness of this almost universal belief among breeders.” Considering these two sterility scenarios side-by-side (extremely different individuals and extremely similar individuals both being unable to mate), he thought he was on to something: “I cannot persuade myself that this parallelism is an accident or an illusion. Both series of facts seem to be connected together by some common but unknown bond, which is essentially related to the principle of life.”⁵⁵

That principle of life would, for the time being, have to remain vague, but it was related to “an old and almost universal belief, founded ... on a considerable body of evidence, that slight changes in the conditions of life are beneficial to all living things.” Breeders of both plants and animals had shown “that a cross between very distinct individuals of the same species, that is between members of different strains or sub-breeds, gives vigour and fertility to the offspring.” A moderate amount of difference between parents was optimal. Major differences between the two parents could prevent the creation of an embryo from ever occurring, or would so disrupt the growing hybrid organism that its reproductive system never properly developed. On the other hand, “close interbreeding continued during several generations between the nearest relations, especially if these be kept under the same conditions of life, always induces weakness and sterility in the progeny.” A moderate amount of constitutional and reproductive difference was beneficial between two parents; too much or too little difference would endanger the offspring.⁵⁶

And so, in Chapter VIII of *Origin of species*, Darwin laid in the world of hybrids the foundations for an argument to bridge the gap between species and varieties: a law of nature dictated that parents should be neither too similar nor too different, that law operated on all individuals to determine who could and could not mate, and this law (and not the dictate of a Creator) is what gave shape to those collections of organisms that naturalists labeled either species or varieties. Hence “there is no fundamental distinction between species and varieties.” There was no good reason to assume that the species of birds in nature had been formed by a process different than the varieties of pigeons in the fanciers’ coops.⁵⁷

This detailed chain of arguments about hybridism had very little impact on the early evolutionary debates. Hardly anyone seriously engaged it in 1859 or 1860. Bree lampooned Darwin’s understanding of inbreeding as “among the most improbable speculations contained in [his] book.” Nonetheless, Darwin was not yet ready to abandon the strategy. *Origin of species* was anchored in the process by which artificial selection generated new domestic varieties, and Darwin still needed to find a way to explain why this process compared to change at the species level in nature. To provide more empirical support for his claims about inbreeding and hybridism, in his next major publication Darwin

⁵⁵ *Ibid.*, 428, 459.

⁵⁶ *Ibid.*, 452.

⁵⁷ *Ibid.*, 474.

turned to a topic that had captivated him for decades: the mysterious relationship between colorful flowers and their insect pollinators.⁵⁸

IV

In May 1862, John Murray unveiled Darwin's latest literary project, a meticulously detailed treatise on the reproductive anatomy of the many families of orchids. The book, which bore the title *On the various contrivances by which British and foreign orchids are fertilised by insects, and on the good effects of intercrossing*, was a curious successor to the controversial *Origin of species*. Rather than arguing openly for the theory of evolution, Darwin promised to patiently guide his readers through the wonderful world of the orchids, examining the flowers' "many beautiful contrivances [that] will exalt the whole vegetable kingdom in most persons' estimation." In order to introduce readers to the orchid tenders' technical language, *Contrivances* opened with a definition of terms, from the elementary botanical concepts of stamen (the "male", pollen-producing organ in plants) and pistil (the "female", ovule-producing organ) to a more esoteric vocabulary associated exclusively with the orchid families. From this basic beginning, the work moved on chapter-by-chapter through detailed dissections of the forms that flowers take in each of the families of orchids, dropping here and there odd observations. *Contrivances* has the feel of a conversation among specialists, long-conducted and long-developed, pitched to a new, general audience bound to be challenged by the experience.⁵⁹

In the midst of all this weighty detail, however, Darwin did develop one organizing concept related to his theory of evolution via natural selection. As he moved from family to family, Darwin showed how each of the strange forms that orchids had taken, so diverse and so bizarre, facilitated one primary aim: insect pollination. In the genus *Catasetum*, for example, the male and female organs of several species are found on separate plants. When a flying insect lands on one of the male plants, its flower launches an adhesive glob of pollen in the direction of the arthropod. The insect, startled, flies off to another plant, and if it lands on the flower of a *Catasetum* female and assumes the same position it had on the male orchid, the mass of pollen is deposited in the plant's stigma, achieving fertilization. The diversity of forms across the orchid families provided substantial evidence of a long, gradual co-evolution of plants and their pollinators.⁶⁰

But Darwin saw an even more important use for the information he had compiled than just providing another example of adaptation. It was obvious to him how insects benefited from these co-adaptive relationships; they collected nectar and pollen, which they consumed for nutrition. But what were the flowering plants getting out of this arrangement? They were, obviously, using the pollinators to help them reproduce, but there was not any clear reason why they required these intermediaries. Why did they not simply produce flowers that contained both male and female organs, thereby allowing them to fertilize

⁵⁸ Bree, *Species not transmutable*, 78.

⁵⁹ Charles Darwin, *On the various contrivances by which British and foreign orchids are fertilised by insects, and on the good effects of intercrossing* (London: John Murray, 1862), 2.

⁶⁰ *Ibid.*, 212-213.

themselves whenever the conditions were opportune? With his argument about inbreeding and hybridism from *Origin of species* in mind, Darwin drew a conclusion:

Considering how precious the pollen of Orchids evidently is ... [i]t is an astonishing fact that self-fertilisation should not have been an habitual occurrence. It apparently demonstrates to us that there must be something injurious in the process. Nature thus tells us, in the most emphatic manner, that she abhors perpetual self-fertilisation. This conclusion seems to be of high importance, and perhaps justifies the lengthy details given in this volume. For may we not further infer as probable, in accordance with the belief of the vast majority of the breeders of our domestic productions, that marriage between near relations is likewise in some way injurious, - that some unknown great good is derived from the union of individuals which have been kept distinct for many generations?

Here in his extended treatment of the orchids was the defense of his views on inbreeding that he had wished to deliver in *Origin of species*; here "Nature," rather than Charles Darwin, "tells us, in the most emphatic manner, that she abhors perpetual self-fertilisation."⁶¹

Once again, however, Darwin's argument about the importance of cross-breeding left little impression. Professional botanists like Asa Gray and Joseph Hooker adored *Contrivances*, but they praised Darwin for his skilled observation and experimentation, not for his evolutionary theorizing. Other reviewers of the book, most of whom knew Darwin as the author of a travelogue about the *Beagle* who had recently made a splash by writing a book on evolution, were puzzled by what the naturalist was up to. "All that a perusal of his pages enables us to affirm," wrote one anonymous critic in *The Athenæum*, is "that in the gardens of green and gladsome Kent Mr. Darwin has been peacefully and pleasantly engaged in studying the fertilization of Orchids."⁶²

The ten months that Darwin devoted to preparing *Contrivances* were, however, no deviation from his researches; far from it, they perhaps better represented Darwin's method of the past two decades than *Origin of species* had. In his later years, Darwin recalled those pleasant days with the orchids as the culmination of an exploration of pollination begun in the summer of 1839 and carried out intermittently "more or less during every subsequent summer," an exploration that gained momentum as the most germane facts accumulated in the late 1850s. The book on orchid fertilization was just one product of Darwin's deep, long-term engagement with the horticulturalists, with whom he would share his ideas until the end of his life.⁶³

⁶¹ *Ibid.*, 359-360.

⁶² For an overview of the reception of *Contrivances*, see Browne, *Darwin: Power of place*, 192-195. On Darwin's *Athenæum* critic, see DMC-CUL226.1.11-12.

⁶³ Traditionally, Darwin has been remembered as a zoologist, but throughout his life he worked seriously and systematically on plants as well as animals. For a strong argument about the centrality of Darwin's botanical work to his evolutionary ideas, see Robert Ornduff, "Darwin's botany," *Taxon* 33, no.1 (February 1984): 39-47. On Darwin's memories of his orchid project, see *The autobiography of Charles Darwin, 1809-1882*, ed. Nora Barlow (New York: Norton, 1958), 127.

The first public evidence of this engagement surfaced, sandwiched between advertisements for conservatories and want ads for landscapers, in an 1841 issue of *The gardeners' chronicle*, a weekly newspaper devoted to rural issues. In late July, a controversy had opened in the periodical's pages when a farmer, writing under the pseudonym "Ruricola," called for the eradication of wild bees throughout Britain. Made deliriously drunk by the honey of thistle flowers, these insects, who were "often seen reeling about as if intoxicated, throwing out their legs in a very grotesque manner," had rampaged through Ruricola's bean plants, tearing holes through their sepals to get at the nectarine inside. The bean grower estimated that he might have lost up to eighty percent of his crop, and encouraged readers to "destroy the humble-bees' nests at the end of summer, and employ children to catch and kill the females in the Bean-fields as soon as the first blossoms have expanded." Two weeks later, the paper published a rebuttal from another farmer, who defended the bee as part of a Divine Plan: "The Providence that gave the Bean for the service of man, also, in infinite wisdom, provided within its blossoms a rich repast for certain tribes of insects; ... if examined with a microscope, not a single pod will be found injured in a large field."⁶⁴

Darwin, who had been receiving *The gardeners' chronicle* from its initial issue, responded to this exchange in an attempt to steer the conversation in a new direction. The holes the bees drilled into the flowers were not much more than a nuisance, he claimed, for hybridizers often modified petals and sepals in significant ways without preventing their plants from reproducing. The nectar-robbing behavior was, though, causing another potentially serious problem. In his 1793 examination of plant reproduction, the German botanist Christian Konrad Sprengel had demonstrated that many plants required the intervention of an insect to carry their pollen from anther to stigma. "What unworthy members of society are these humble-bees," wrote Darwin, "thus to cheat, by boring a hole into the flower instead of brushing over the stamens and pistils, the, so imagined, final cause of their existence!"⁶⁵

Darwin's inquiry into inbreeding, which would later take on greater theoretical meaning, began in his garden. In the garden was a world, palpable and earthy, in which Darwin might incubate his thoughts; it was a world in which his evolutionary vision, elsewhere expressed in decontextualized abstractions, was thoroughly embodied, where conjecture might lead quickly to action and thought to consequences. *The gardeners' chronicle* provided him a wealth of information, and he contributed to it occasional musings from his reading and experience at Down: on manure, on how to produce humane traps for vermin, on the impact that salt water might have on the survival of seeds. The periodical, edited by the botanist John Lindley of University College, London, was intended for a wide audience of the horticulturally curious; it

⁶⁴ Ruricola [pseudo.], "Humble bees," *The gardeners' chronicle* 30 (1841): 485. An Old Farmer [pseudo.], "Humble bees," *The gardeners' chronicle* 32 (1841): 517.

⁶⁵ The work was *Das entdeckte Geheimnis der Nature im Bau und in der Befruchtung der Blumen*. Curiously, though Darwin makes reference to Sprengel's work and is fairly conversant about it in his August 1841 article in *The gardeners' chronicle*, he notes in his autobiography that he did not read the book until Robert Brown advised him to in November 1841. See *Autobiography Darwin*, 127. Charles Darwin, "Humble bees," *The gardeners' chronicle* 34 (1841): 550.

provided a forum for both laborers, like “the Gardener, the Forester, the Rural Architect, the Drainer, the Road-Maker, and the Cottager,” and their employers, like Darwin. Scientists were invited to contribute their knowledge, but Lindley did not want *The gardeners’ chronicle* to become primarily a journal of scientific botany:

[T]he art of Gardening would soon be deprived of all novelty and interest, if it were not for the daily discoveries of science, and the application of them as they arise to the practice of cultivation. For these reasons Vegetable Physiology, Systematic Botany as far as handsome or useful plants are concerned, and Vegetable Chemistry are more especially matters upon which information may be constantly expected. Let not our readers, however, fear lest we oppress them with too much learning. We perfectly understand that our general duty is to write for those who have little acquaintance with science, and to instruct the uninformed rather than to gather information for men of science, who can always collect it for themselves from its original sources.

Ideally, a practicing farmer like Ruricola, watching the humble bees swarm over his bean plants, could expect in the newspaper for a naturalist like Charles Darwin to provide him with useful advice.⁶⁶

And this Darwin did, regularly, both in short dispatches published by Lindley and in occasional correspondence with the *Chronicle’s* readers. One commentator, surveying in 1875 the full sweep of Darwin’s contributions to the art of gardening, praised the naturalist’s ability to effortlessly bridge the gap between horticulture and scientific botany. He had introduced gardeners to the hybridization studies of Kölreuter and Gärtner and the pollination research of Sprengel, very useful work known previously primarily in botanical circles. In the other direction, his theory of evolution, with its central emphasis on domestication, had demonstrated to scientific botanists that “a new variety raised by man ... is a more interesting subject for study than one more species added to the crowded lists.” Darwin did more, though, than act as a vector for transmitting information from one community to another; he also undertook his own experiments on “the great subjects of fertilisation by insects, of cross-fertilisation, of hybridisation,” subjects that were simultaneously botanical and horticultural. While Darwin was widely known by the 1870s as a naturalist, *The gardener’s chronicle* lauded this “great physiologist of our day [who] has supplied the thoughtful cultivator with innumerable facts, careful observations, and suggestive inferences.”⁶⁷

When, in December 1856, Darwin published an open appeal to farmers for any information related to cross-breeding in legumes, his intention was both to continue a long-standing, practical horticultural conversation about pollination and also to collect useful information relating to hybridism to help defend his incipient theory of evolution. Since the 1840s, he had watched the bees in his garden each summer, and he was even more convinced than when he wrote his first article to the *Chronicle* that Sprengel had been right, that insects were

⁶⁶ John Lindley, “Prospectus of *The gardeners’ chronicle*,” *The gardeners’ chronicle* 1 (1841): 3.

⁶⁷ [“On Charles Darwin,”] *The gardeners’ chronicle* 62 (1875): 308-309.

required for the reproduction of flowering plants. However, legumes, one of the largest families of flowering plants, seemed to pose a challenge to this generalization, for the shape of their flowers generally facilitated self-fertilization. When Darwin published a year later a more specific appeal for information about crossing in kidney beans, he hoped that one of the *Chronicle's* regular readers might furnish some evidence that legumes do sometimes cross without a breeder's intervention.⁶⁸

He was not disappointed. In November 1857, Henry Coe, a gardener employed by a mental asylum in Hampshire, sent to Down a packet of beans that he thought might be just what Darwin was looking for. Coe had planted some black kidney beans amidst rows of white plants and brown plants, and the beans that these black plants produced, which Darwin gazed at in his study, "presented an extraordinary mixture ... of all shades between light brown and black, and a few mottled with white; not one-fifth of the Beans, perhaps much less, were pure Negroes." Clearly, these black kidney bean plants had not self-fertilized. Throughout 1858, Coe and Darwin experimented on the strange beans, planting them in new arrangements to determine what outcomes might result. Finally, convinced that legumes too sometimes cross-fertilized when insects spread pollen from plant to plant, Darwin presented to *The gardeners' chronicle* an account of what he suspected might be going on:

It is, I think, well ascertained that very close interbreeding tends to produce sterility, at least amongst animals ... May we not then suppose in the case of Leguminous plants, after a long course of self-fertilisation, that the pollen begins to fail, and then, and not till then, the plants are eagerly ready to receive pollen from some other variety? Can this be connected with the apparently short duration and constant succession of new varieties amongst our Peas, and as is stated to be the case on the Continent with Kidney Beans?

Darwin's understanding of inbreeding and his belief in its inherent dangers were developing, step-by-step, from his experiences in the garden and in his encounters with gardeners.⁶⁹

Similarly, Darwin's theory of natural selection appeared in *The gardeners' chronicle* not as an import from a distant intellectual realm, but as the obvious, though ingenious, insight of a reflective horticulturalist. Shortly after the publication of *Origin of species*, John Lindley summarized the treatise for his readers, explaining its argument in simple, familiar terms. "Suppose," he wrote, "that it be desired to raise ... a new kind of Wheat, better suited to a particular soil or climate than that in ordinary cultivation." As every nurseryman knows, the obvious way to do this is "to select from the kind that already grows best in

⁶⁸ Charles Darwin, "Cross breeding," *The gardeners' chronicle and agricultural gazette* 49 (1856): 806. Charles Darwin, "Bees and fertilisation of kidney beans," *The gardeners' chronicle and agricultural gazette* 43 (1857): 725.

⁶⁹ Frederick Burkhardt and Sydney Smith, eds., *The correspondence of Charles Darwin. Volume 6, 1856-1857* (Cambridge: Cambridge University Press, 1990), 480. Charles Darwin, "On the agency of bees in the fertilisation of papilionaceous flowers, and on the crossing of kidney beans," *The gardeners' chronicle and agricultural gazette* 46 (1858): 829.

that locality the strongest and most prolific individuals, and to propagate from these." Over time, the field will be filled with these new, superior plants, the first stock of a new variety of wheat. To this point, Lindley had described nothing that would seem particularly unusual to a gardener, but next he introduced the novelty of Darwin's theory: "[I]n effecting this rapid change man has only powerfully aided Nature; for it is quite clear that the quality which man has thus so prominently brought out is one which ... must ultimately [in nature] have asserted its superiority in a similar manner." The horticulturalist who brings a new variety into the botanical world is only accelerating a transformation that would inevitably have been brought into being anyways: "[I]t matters not in the long run whether Nature or man sows or garners." Nature was the universe's most accomplished gardener.⁷⁰

There was no discussion in *The gardeners' chronicle* of the species question that obsessed the naturalists, no turmoil over the challenge that Darwin's cosmology posed to the theologians. The central lesson of *Origin of species* was that the world of wild nature operated like the world of domesticated nature. While Darwin was calling on the naturalists to look to the farm and the stockyard for their material, Lindley hoped the horticulturalists would analyze wild nature to discover new, more effective techniques of plant manipulation. Darwin's theory promised to steer naturalists into work of practical value to the world of botanical cultivation. Lindley was filled with enthusiasm by the prospects opening before him: "In the present state of science, we cannot doubt the wisdom of [Darwin's] proposal whatever its results may; it is an application of the principle [of selection] which Mr. Darwin believes Nature to have followed in producing species of plants profitable to man and the lower animals, and which a perusal of his work assures us is, in the existing infant condition of experimental agriculture, capable of indefinite application and undreamt-of results."⁷¹

Throughout 1860 and 1861, Darwin sent Lindley brief notices about orchids to publish in his journal, requests for unusual specimens or speculations about how fertilization took place that he could float as trial balloons before a knowledgeable audience. The orchid cultivators were happy to assist, for they recognized the problems that Darwin was working on to be their own. Darwin's challenge remained convincing those who were naturalists, and those who were following second-hand the controversy he had stirred up, that what was happening amidst domesticated varieties was properly representative of what had happened to species in the wild.

V

While Darwin worked at Down to strengthen his theory, his antagonists in the natural theological tradition regrouped and began a frontal assault on the analogy between artificial and natural selection. On a December evening in 1866 in Salem, Massachusetts, Paul Chadbourne, a professor of natural history at Williams College who would shortly become the president of the State

⁷⁰ John Lindley, ["Review of Darwin's *Origin of species*,"] *The gardeners' chronicle and agricultural gazette* 3 (1860): 46.

⁷¹ *Ibid.*

Agricultural College at Amherst, rose before the Commonwealth's Board of Agriculture in Lyceum Hall to deliver a lecture on variation in plants. In every plant, Chadbourne assured his audience of agriculturalists, we can discern "what may be called the *creative idea* – that is, a certain purpose, often dimly sketched in the wild plant, but which is more perfectly developed by all the changes produced under cultivation." This creative idea animates each of the species of plants that humans have domesticated; some species are inherently designed to produce tasty, nutritious, abundant fruit, while others, like the beautiful orchids, are aesthetically pleasing. Horticulturalists, who have produced many new and distinct plant varieties, provide no direct evidence that species have evolved in the wild, for these "varieties simply unfold and exhibit the creative idea in the species." The careful botanical cultivator is following a plan inscribed in nature by the Creator: "Men and animals do not make use of plants because they happen to be what they are; but the plants are constituted as they are for the sake of the animal kingdom, and many of them with direct reference to man as an intellectual and moral being."⁷²

As Chadbourne took his seat, he was replaced at the rostrum by the eminent Professor Louis Agassiz, the founder and director of the Harvard Museum of Comparative Zoology and a prominent critic of Darwinian evolution. Gazing into a sea of farmers and stockmen, Agassiz picked up where Chadbourne had left off. With the skill of a judoka, he turned Darwin's argument from domesticates against itself, noting that "this practical business of cultivating the ground and raising plants for special purpose is an awful fact to theorists." The aging professor repeated the same critique of *Origin of species* that had been ubiquitous since 1859, that we have no evidence of a domesticated variety ever becoming a new species, and then he urged his experienced listeners to engage the naturalists' debate:

[S]ome of the most important problems that enter into the decision of questions of abstract science are left for you to work out. Only let the farmer, when he goes to work to examine his proofs, do it with a particular knowledge of what he is to report upon in reference to this question. If he tells us that he can raise wheat out of oats, that he can raise corn out of rice, that he can raise hemp out of nettles, then he will have shown just what the doctrine of transmutation assumes; but if, on the contrary, the farmer tells us that he is ever moving in a circle, which is returning upon itself, and that within that circle there is, in one case, nothing but apples, in another nothing but pears, in another nothing but cherries, in another nothing but grapes, in another nothing but wheat, in another nothing but corn, however great the varieties of corn, the varieties

⁷² The Massachusetts Board of Agriculture, *Fourteenth annual report of the Secretary of the Massachusetts Board of Agriculture, with an appendix containing an abstract of the finances of the county societies, for 1866*, Public Document No. 4 (Boston: Wright & Potter, 1867): 61, 66, 70. Chadbourne's natural theology is more fully presented in P.A. Chadbourne, *Lectures on natural theology; or, nature and the Bible from the same author* (New York: Putnam, 1867). For an account of Chadbourne's involvement with agricultural education and academic leadership, see Leverett Wilson Spring, *A history of Williams College* (Boston: Houghton Mifflin, 1917), 227-242.

of apples, and so on, may be, then, he tells us that he does not make species, but only unfolds to the utmost all their inherent properties.

What Agassiz envisioned was a branch of scientific agriculture turned *against* selectionist evolution, where the wide gulf that separated a variety from a species would be the central line of investigation. Darwin and his followers could assert as often and loudly as they liked that varieties were incipient species, but Agassiz promised that the experience of the farmer would continue to contradict them, for he did “not suppose that by any particular witchcraft agriculture is to do in the next five years very materially different things from what it has done before.” And as his speech wound to its finale, he prophesied that “we shall not have from agriculture evidence of the correctness of the Darwinian doctrine; we shall have no support for this transmutation theory from it, but only a succession of severe blows, which are coming so rapidly that I trust the doctrine will not live much longer.”⁷³

Back at Down, Darwin received a short account of the meeting in Massachusetts. Domesticated organisms were very much on his mind, so he clipped the article on Agassiz’s performance from the newspaper in which he’d found it and pasted it into one of his research scrapbooks.⁷⁴

As Alfred Russel Wallace had foreseen in 1858, the connection between wild and domesticated creatures, between Darwin’s theory of natural selection and his deep understanding of artificial selection, remained a soft spot that might be exploited by evolution’s opponents. In January 1860, just months after *Origin* had been published, Darwin began organizing the notes he’d collected for decades on cultivated plant varieties and animal breeds, hoping to assemble a book addressing the process of domestication, the process by which a wild population becomes a domesticated population, to respond to his critics. He was sidetracked, however, by other projects, like his work on the orchids, and throughout the 1860s suffered numerous bouts of debilitating illness that kept him from his research. The manuscript on domestication remained in preparation for a long time, and Darwin had ample opportunity to supplement, bit-by-bit, each section with new observations he’d made and new accounts from the world of the breeders that he’d come across. When the exhausting project was finally published in January 1868, after four years and two months of dedicated effort, *The variation of animals and plants under domestication* occupied two mammoth volumes.⁷⁵

Together, these two volumes presented, in overwhelming detail, Darwin’s magisterial vision of how wild nature in its encounter with humankind became transformed into a new form of life. The first volume was a compendium of the domestic organisms. It surveyed the mammals and birds that would have been familiar in the British rural setting: dogs, cats, horses, donkeys, cattle, pigs, sheep, goats, chickens, ducks, and geese, with a sharpened focus on rabbits and pigeons, two types of animals to which the breeders had paid special attention. Wheat, rye, barley, oats, vegetables, fruit trees, and cultivated flowers received

⁷³ Massachusetts Board of Agriculture, *Fourteenth annual report of the Secretary of the Massachusetts Board of Agriculture*, 71, 73-74.

⁷⁴ See DMC-CUL 226.1.32.

⁷⁵ Darwin, *Autobiography*, 126-127, 129-130.

some attention as well, though not as much as the animals. In the second volume, Darwin returned in greater detail to themes he had first introduced in *Origin*: why organisms exhibit variation in their traits, how and why some varieties and species can intercross but others cannot, the power and limitations of selection, how heredity operates. Animating the project from beginning to end was an assumption about domestication that Darwin stated clearly in his opening pages: “Man ... may be said to have been trying an experiment on a gigantic scale; and it is an experiment which nature during the long lapse of time has incessantly tried.”⁷⁶

An experiment on a gigantic scale. But who, exactly, has been conducting this experiment? Who has been observing its results? And who will put this gargantuan experiment’s findings to use?

To flesh out his description of the history of domesticated life as a massive experiment, Darwin divided selection into three different forms, methodical, unconscious, and natural, and then narrated a tale of how these three forms of selection have related to each other over time. This tripartite division made its initial appearance in the opening chapter of the first edition of *Origin of species*, but it was not until *Variation under domestication* that Darwin enunciated its terms in explicit detail:

Methodical selection is that which guides a man who systematically endeavours to modify a breed according to some predetermined standard. *Unconscious selection* is that which follows from men naturally preserving the most valued and destroying the less valued individuals, without any thought of altering the breed; and undoubtedly this process slowly works great changes ... Lastly, we have *Natural selection*, which implies that the individuals which are best fitted for the complex, and in the course of ages changing conditions to which they are exposed, generally survive and procreate their kind.

What distinguishes each of these forms of selection from each other is the awareness of the human mind in relation to it. The actual process of selection that takes place in each is identical; there is, for all functional purposes, one single “great principle of Selection.”⁷⁷

And for as long as there has been life on earth, Darwin declared, that principle of selection has been in operation. For the long stretch of time before the dawn of human civilization, the only type of selection possible was, of course, natural selection, but from the time the first humans appeared unconscious selection was at work, as “man preserves the animals which are most useful or pleasing to him, and destroys or neglects the others.” Methodical selection emerged from unconscious selection gradually, the two processes easily bleeding into each other, as breeders became aware of the power they held to remake animal and plant populations, and Darwin suggested that the first methodical selection probably began very early in human history. The book of

⁷⁶ Charles Darwin, *The variation of animals and plants under domestication*, vol. 1 (London: John Murray, 1868), 3.

⁷⁷ On the first published description of methodical, unconscious, and natural selection, see Darwin, *Origin of species*, 34-37. Darwin, *Variation under domestication*, Vol. 2, 193-194.

Genesis records both “speckled and dark breeds” of sheep, yet “[b]y the time of David the fleece was likened to snow.” The Old Testament includes many useful guidelines for livestock breeding, while the surviving texts of ancient Greece and Rome demonstrate that those civilizations understood the rudiments of animal husbandry. In more recent memory, Charlemagne devoted considerable care to determining which of his stallions to mate to his mares, and pigeon fanciers acknowledge Emperor Akbar Khan as having made significant improvement to his birds before 1600. Jesuit accounts of the eighteenth century detail a remarkable degree of cultivation in the rice, fruit trees, and other plants of China, a cultivation that had been fostered through long centuries. Darwin was convinced that both unconscious and methodical selection had been constantly remaking domesticates throughout human history.⁷⁸

And yet, though it had already been a force on earth for thousands of years, methodical selection had taken a new turn in recent decades. The process was accelerating; Europe’s breeds were being transformed at a rate previously unimagined. Darwin described what had been lately unfolding in Britain in startling terms:

What methodical selection has effected for our animals is sufficiently proved ... by our Exhibitions. So greatly were the sheep belonging to some of the earlier breeders, such as Bakewell and Lord Western, changed, that many persons could not be persuaded that they had not been crossed. Our pigs, as Mr. Corringham remarks, during the last twenty years have undergone, through rigorous selection together with crossing, a complete metamorphosis. The first exhibition of poultry was held in the Zoological Gardens in 1845; and the improvement effected since that time has been great. As Mr. Baily, the great judge, remarked to me, it was formerly ordered that the comb of the Spanish cock should be upright, and in four or five years all good birds had upright combs; it was ordered that the Polish cock should have no comb or wattles, and now a bird thus furnished would be at once disqualified; beards were ordered, and out of fifty-seven pens lately (1860) exhibited at the Crystal Palace, all had beards. So it has been in many other cases ... The steady increase of weight during the last few years in our fowls, turkeys, ducks, and geese is notorious; “six-pound ducks are now common, whereas four pounds was formerly the average.”

The age of slow, gradual, unconscious selection was fading into the past, and the methodical selection that had emerged alongside it was beginning to overwhelm it. British breeders were becoming conscious of the immense powers they had to refashion life on earth.⁷⁹

If Darwin was at all disturbed by the transformations he listed, there was nothing in *Variation under domestication* to suggest it. And why should he be? By his account, what methodical selection was bringing into being at the middle of the nineteenth century was, in principle, no different from what Charlemagne had attempted with his mounts in the 9th century or the Israelites had done to

⁷⁸ Darwin, *Variation under domestication*, Vol. 2, 201, 211.

⁷⁹ *Ibid.*, 198-199.

their livestock in ancient times. It was the same universal principle of nature that had been generating new species from before human civilization existed. All that was different now was that humans had begun to understand how to harness this power and direct it toward their own ends.

Darwin's historical narrative imbued the new developments of the recent past with the long, irreversible qualities of natural history; it naturalized the intense selection practices of livestock breeders and fanciers of the past fifty years. Simultaneously, it provided a justification for seeing the entire history of human relation with domesticates as one immense experiment. Direct observations of natural selection were nearly impossible to make, and it was "difficult to offer direct proofs of the results which follow from" unconscious selection, but serious breeders kept meticulous records of their undertakings. If "[u]nconscious selection so blends into methodical that it is scarcely possible to separate them," and if unconscious selection is just a human application of the principle of selection carried out by nature, then it follows logically that methodical selection should be a near-perfect model of the principle of natural selection. *Variation under domestication* furnished a rationale for conceiving of scientific breeding as experimental evolution.⁸⁰

At a time when Agassiz and other anti-evolutionists were contesting the notion that domestic varieties resembled incipient species, Darwin's domestication narrative provided his defenders with a comprehensive worldview in which artificial selection and natural selection were fundamentally the same process. Why an uncommitted observer should prefer Darwin's account to Agassiz's, however, was still uncertain. To argue for his version of the relation of domesticated to wild populations, Darwin devoted five chapters of *Variation under domestication* to a discussion of inbreeding, cross-breeding, and sterility. As it had in *Origin*, his argument revolved around hybridism, but this time he incorporated a much greater amount of material evidence drawn from the literature on methodical selection, in which he had undoubtedly been immersed.

As he had in 1859, Darwin began again by noting that naturalists collectively had underestimated "how difficult, or rather how impossible it often is, to distinguish between races and sub-species ... and again between sub-species and true species." Once again, he argued that the main criterion most have used to show that varieties and species are different natural categories, the general rule that varieties can easily be crossed while species cannot, overlooked the wide diversity of actual outcomes that resulted when this was tried. And just as in *Origin*, he attempted to demonstrate that all crossing is governed by the same laws of nature; what naturalists call species differ only in degree in their ability to cross from what naturalists call varieties. What he added now to this argument in *Variation under domestication* was an explanation, gleaned from the many cases he presented in the book's first volume, of why domestic varieties generally could cross and why species generally could not.⁸¹

To start, Darwin returned to the scheme he'd developed in *Origin of species*, noting that moderate differences between two parents promote fertility while extreme difference or similarity results in sterility. What's important,

⁸⁰ *Ibid.*, 210.

⁸¹ Darwin, *Variation under domestication*, Vol. 1, 4.

however, is not the entire anatomies of the parents, but rather their reproductive structures. In the wild, natural selection works on all parts of an organism at all times; any slight changes in any structure can have an impact on survival. Under domestication, however, unconscious and methodical selection very rarely act as forces on reproductive anatomy; breeders care about the body weight, plumage, or disposition of the animals they're tending, not how pretty their genitalia look. As a result, there are only slight differences in the reproductive structures of domesticated varieties, even if the resulting breeds appear to be radically different from each other in other physical respects. Because species vary more widely in their reproductive apparatuses than varieties, which retain moderately different organs and habits of mating, varieties cross more easily than species.

Speciation in the wild took a very long time, and Darwin was no more able to experiment directly on the genitalia of wild creatures than he was able to induce the creation of new species through selection. What he could do, however, is provide plentiful testimony from the breeding and horticultural communities about what happens at the opposite extreme when domesticated organisms become too uniform. Intensive inbreeding offered an experimental means for better understanding the underlying law of nature that Darwin thought governed the fertility and sterility of crosses.

While inbreeding had been briefly discussed in *Origin of species*, it received a much more thorough treatment in *Variation under domestication*. Darwin was able to muster the argument about self-fertilization and cross-pollination that he had developed so carefully in his work on orchids, and he described some initial self-fertilization experiments he had begun in his greenhouse. In *Origin*, Darwin had alluded to the "almost universal belief among breeders" that inbreeding would lead to sterility, and now he provided a long and detailed account of the many exemplary cases provided by methodical selection: the shorthorn cattle, inbred extensively in Britain since the late eighteenth century, that produced more malformed offspring than other livestock; the expert sheep breeder who learned from long experience to keep five largely-unrelated flocks and to cross them regularly to prevent too much inbreeding; the isolated herds of deer kept for centuries in British parks that required a constant infusion of new blood to perpetuate themselves; the well-known deterioration of the Scotch deerhound brought on by too close breeding; the enfeebled lineage that Lord Western raised from one Neapolitan boar and one sow, that produced the Improved Essex breed when it was finally crossed with the unrelated Essex pig; the Bantam chickens, their mating kept within the family since they were first established in the early 1800s, that "are now notoriously bad breeders." All lines of evidence from both the plant and animal kingdoms pointed to the same conclusion:

The consequences of close interbreeding carried on for too long a time, are ... loss of size, constitutional vigour, and fertility, sometimes accompanied by a tendency to malformation. Manifest evil does not usually follow from pairing the nearest relations for two, three, or even four generations; but several causes interfere with our detecting the evil – such as the deterioration being very gradual, and the difficulty of distinguishing between such direct evil and the inevitable augmentation of any morbid tendencies which may be latent or apparent in the related parents.

Careful breeders had provided Darwin with all the evidence he needed.⁸²

In his attempt to order the relationship between artificial and natural selection, Darwin had tumbled into a tautological word game. Methodical selection yielded the evidence that could justify thinking of it as a form of experimental evolution. The rationale for conceiving of methodical selection as experimental evolution depended on the assumption that methodical selection was experimental evolution. The responses of his critics, which he took quite seriously, were forcing Darwin not only to argue that selection was one universal principle, but also to emphasize natural selection as the novel feature of his theory and artificial selection as a familiar, routine, and obvious process unworthy of critical inquiry except as a stand-in for something else. Darwin had created a worldview in which a systematic breeder could think of herself as an experimental evolutionist, or a horticulturalist imagine himself to be a naturalist.

But this was a game fraught with material consequences. *Variation under domestication* collected the findings of the vast literature on methodical selection in two convenient volumes; even though it was heavily weighted toward the animal kingdom, a review in *The gardeners' chronicle* thought it "of such importance to both the practical and theoretical gardener ... that it must claim a large share of our attention ... for its special merits, and the stores of information it contains." It was a compilation of knowledge about plant and animal breeding as comprehensive as any that had been attempted, and it not only reported the results of what methodical selectors had wrought in the recent past but attempted to determine how they had been able to pull it off. Darwin had never been a pure theorist; his knowledge of nature had come from his experience with it and his contact with the various communities that tended it. The narrative he wove in *Variation under domestication*, however, cast the illusion that it was possible to be an observer outside the system, that one could read the results of a long and fitful history of human immersion in the natural world as one big experiment.⁸³

And, even as it presented copious evidence of how drastically plants and animals had begun to change, *Variation under domestication* depicted the intensive inbreeding practices of the recent past as an accelerated natural process, not a transformative event in historical time that needed itself to be conceptualized and reacted to. Naturalists who took Darwin seriously were being encouraged to move into breeding circles, to help push artificial selection to its extremes to see what experimental support for natural selection might surface; they were not being encouraged to observe critically the impact this process might be having on the wider world. Darwin's domestication narrative and the text it supported served this ideological function for the biologist it attracted: it made it possible for him to imagine himself merely an observer of breeding practice, even as he himself participated in that practice.

An experiment on a gigantic scale. One of the advantages of an experiment, of course, is that it can be repeated, and its conditions modified, to produce more refined results. If the metaphor worked, and horticulturalists and breeders had already generated a copious outpouring of results, then it stood to

⁸² *Ibid.*, 115, 124.

⁸³ ["Review of Darwin's *The variation of animals and plants under domestication*."] *The gardeners' chronicle and agricultural gazette* 8 (1868): 184.

reason that the naturalist should be able to perform controlled breeding experiments of her own, on her own terms. And this is precisely what Darwin did during his final decade of productive research and writing.

VI

In the early summer months, along abandoned low brick walls or in fields untended, the toadflax blooms throughout rural England. The bundles of brilliant yellow-and-white flowers that appear atop the plants' stems imbue the verdant landscape with golden hues. At mid-century, John Lindley noted in his encyclopedia of the botanical world that dyers sometimes used the flowers to produce a yellowish effect, while Londoners boiled the bitter plant in milk to drive away the flies.⁸⁴

While composing *Variation under domestication*, "for the sake of determining certain points with respect to inheritance," Darwin sowed side-by-side two plots of common toadflax. The seeds for each plot came from the same parent plant, but one batch had been the result of self-fertilization while the other of cross-fertilization with pollen from another toadflax flower. Though, as he recalled later, he had planted the toadflax "without any thought of the effects of close interbreeding," he discovered that "the crossed plants when fully grown were plainly taller and more vigorous than the self-fertilised ones." The suddenness with which inbreeding's effects had made themselves manifest surprised him, for "no instance was known with animals of an evil appearing in a single generation from the closest possible interbreeding, that is between brother and sisters." He reasoned that "the fertilisation of a flower by its own pollen corresponds to a closer form of inter-breeding than is possible with ordinary bi-sexual animals," and, inspired by his dual beds of toadflax, began concocting a much larger experiment.⁸⁵

His plan was simple. Take a plant, any plant that can be easily grown and tended, and mate it in two different ways to produce two different sets of seeds. One set of seeds, the self-fertilized seeds, will be produced by introducing pollen from the plant itself to its own flowers; the other set of seeds, the cross-fertilized seeds, will be produced by introducing pollen from a distinct individual of the same variety to the experimental plant's flowers. Place both sets of seeds on opposite sides of an easily observed container under hospitable circumstances and allow them to germinate. As the seeds sprout, transplant a self-fertilized seedling and a cross-fertilized seedling of about the same age to opposite sides of another pot, with two vertical rods placed next to the two seedlings. Continue doing this "until from half-a-dozen to a score or more seedlings of exactly the same age [are] planted on the opposite sides of several pots." At this point, the experimenter would have a room full of pots of growing plants, with half of each pot containing a self-fertilized plant and the other half a cross-fertilized plant. Conditions would be identical for each side of the pot, a situation made possible by the controlled setting of the greenhouse behind Darwin's house. As the season wore on, each plant would grow up its rod, and when one reached the

⁸⁴ John Lindley, *The vegetable kingdom; or the structure, classification, and uses of plants, illustrated upon the natural system*, 3rd ed. (London: Bradbury & Evans, 1853), 684.

⁸⁵ Darwin, *Effects of cross and self fertilisation*, 8-9.

top the experiment would be terminated and, because “the eye alone was never trusted,” several measurements of each plant would be recorded so that the difference between the self-fertilized and cross-fertilized might be quantified.⁸⁶

Initially, Darwin expected to conduct his experiment for a single generation, but the pronounced evidence of inbreeding’s impact that showed up after the first round convinced him that the experiment would be worth carrying into later generations. In subsequent stages, Darwin chose one strong self-fertilized plant to be parent to the next self-fertilized generation and mated two of the healthy cross-fertilized plants to obtain the next generation of cross-fertilized seed. Using this method, Darwin and his gardening staff maintained the experiment for up to ten generations for many of the plants.

What began as a single, almost accidental, trial snowballed into a decade of obsession. Darwin had obtained his first toadflax seeds from a wild plant growing near his home, but he soon turned to mail-order seed catalogs and commercial nursery gardens to supply his base stock. He scoured Down for eligible subjects; from his outdoor garden, he drew varieties of lettuce, oregano from the herbs destined for his kitchen, and a bright red canna lily from his hothouse. As he had in his work with orchids, he tapped into his far-flung network of associates to provide him with more exotic specimens for controlled breeding: a weed from Calcutta, a shrub with drooping red flowers from southern Brazil, cultivated candytuft from Algiers. From Kew Gardens, he received some tobacco seeds. Whenever he could keep a plant alive in his greenhouse, he subjected it to ten generations of intensive inbreeding. By the mid-1870s, Darwin had drawn over fifty different botanical species, including ornamental flowers, legumes, cash crops, and pesky weeds, into his orbit.

In 1876, he published the results of these experiments as *The effects of cross and self fertilisation in the vegetable kingdom*. One year later, translations of the work were produced in German and French, while John Murray brought out an accompanying second, revised edition of Darwin’s book on orchids, which had fallen out of print. In the twilight of his long career, Charles Darwin was producing one final, definitive statement of what his continuous dabbling for decades in the world of inbreeding and self-fertilization had taught him.

For a reader who had been following the development of Darwin’s thought since *Origin of species*, this new synthesis provided few surprises. “The first and most important of the conclusions which may be drawn from the observations given in [*The effects of cross and self fertilisation*],” Darwin emphasized, was “that cross-fertilisation is generally beneficial, and self-fertilisation injurious,” the same point he had developed using an alternate form of evidence in 1862 in *Contrivances in orchids*. At the end of ten generations of inbreeding, the majority of the plants in self-fertilized lines were shorter, weighed less, produced fewer seeds, and engendered less fertile offspring than their cross-fertilized kin. Across the vast majority of species and genera that Darwin had sampled, this seemingly-universal rule held sway.⁸⁷

Even with the great numerical evidence that he had accumulated, however, Darwin was forced to qualify his statements and to provide more detailed justifications of some of his conclusions. Unexpected difficulties had

⁸⁶ *Ibid.*, 12-13.

⁸⁷ *Ibid.*, 436.

arisen during the experiments, and these needed to be addressed and resolved. One of the more fundamental was that, as Darwin learned in time, the term *cross-fertilized* lost a great deal of its integrity after the first generation. His method for self-fertilizing plants left no ambiguity in the resulting specimens; after ten generations, the self-fertilized line was “as closely interbred as was possible.” And the first cross-fertilized offspring were also unproblematic, for they were the product of the mating of two unrelated individuals. In subsequent generations, however, Darwin had paired his cross-fertilized parents with individuals to whom they were related; technically, the resulting generations could be said to be cross-fertilized, but what this meant in practice was unclear, for each subsequent generation was becoming more and more inbred as more closely-related plants were paired. The degree of inbreeding of the plants in the cross-fertilized pool became uncertain as the experiments progressed: “In the second generation a large number of seedlings would be what may be called whole or half-cousins, mingled with whole and half-brothers and sisters, and with some plants not at all related ... The relationship will thus have become more and more inextricably complex in the later generations; with most of the plants in some degree and many of them closely related.” Another way to conceptualize the situation is to think of the self-fertilized line as descended from one single individual and the cross-fertilized line as descended from a slightly larger, though still limited, population of four or six founders. With each passing generation, the cross-fertilized line was becoming less and less cross-fertilized.⁸⁸

Darwin acknowledged that he “ought to have crossed the self-fertilised plants of each generation with pollen taken from a non-related plant,” but he was too far into the project when he had this realization to change course. To compensate, he occasionally introduced to the later inbred generations pollen from a new batch of plants of the same variety, preferably grown away from Down; in the ninth generation of his morning glory experiment, for example, he crossed one of his inbred plants with a vine that had been raised in a garden in Colchester. The offspring of these pairings proved even heartier than the later cross-fertilized generations, and Darwin believed that this reinforced one of the cardinal claims about crossing that he had made in *Variation under domestication*: “the benefit from cross-fertilisation depends on the plants which are crossed having been subjected during previous generations to somewhat different conditions.” Both self-fertilization and the uniform environment of the greenhouse were driving the deterioration of the inbred lines of descent.⁸⁹

A second complication that Darwin had to contend with was how to account for the occasional individuals and varieties in his experiments that did not seem to be harmed by self-fertilization. As the project took shape in his mind, it developed under the naïve belief that the numbers would simply speak for themselves, and for as long as each of the cross-fertilized plants grew taller and healthier than its self-fertilized match, this belief could remain unchallenged. It was not long, however, before evidence contrary to Darwin’s expectations began surfacing. There was the intensely inbred morning glory vine, Darwin’s Hero, that not only outgrew its cross-bred counterpart, but seemed able to pass on an aptitude for inbreeding to its descendants. A similar individual appeared

⁸⁸ *Ibid.*, 20-21.

⁸⁹ *Ibid.*, 20, 444.

among the yellow monkey-flowers, and curiously “this victorious self-fertilised plant consisted of a new white-flowered variety, which grew taller than the old yellowish varieties.” The bright orange California poppy did not seem to become more vigorous at all after cross-breeding, although it might produce more seeds, while the wild mignonette did not look like it was affected much one way or the other by repeated selfing. And the common pea plant, reputed in Britain to be the product of constant self-fertilization for over sixty years, did markedly better when selfed than crossed.⁹⁰

As the pressing weight of his evolutionary theory was pushing him to demonstrate that inbreeding leads to sterility, Darwin had to explain away the anomalies he’d generated as resulting from unusual circumstances, particularistic instances that did not threaten his main point. What he lacked was a statistical protocol for analyzing the numbers he was generating, a technique for transforming raw quantitative data into meaningful statements. He knew that for any given species or variety his sample size was quite small, but he thought that the degree of control his greenhouse allowed over environmental variables and the degree of relationship of his plants compensated for that:

[I]f we took by chance a dozen or score of men belonging to two nations and measured them, it would I presume by very rash to form any judgment from such small numbers on their average heights. But the case is somewhat different with my crossed and self-fertilised plants, as they were of exactly the same age, were subjected from the first to last to the same conditions, and were descended from the same parents.

He sent a small sampling of his records to his cousin Francis Galton, who mailed back “graphical representations which he had made of the measurements,” showing that they “evidently form fairly regular curves,” but this was the limit of Darwin’s aggregate number-crunching. Without an art of statistics to allow him to establish a norm and tag outliers as deviants, Darwin was forced to temper his strong conclusions about inbreeding with a reluctant caveat: “[I]t is difficult to avoid the suspicion that self-fertilisation is in some respects advantageous; though if this be really the case, any such advantage is as a rule quite insignificant compared with that from a cross with a distinct plant, and especially with one of a fresh stock.”⁹¹

Hero and its compatriots did spur Darwin to recommend to commercial florists that they consider inbreeding some of their stock. “[I]f they will fertilise the flowers of the desired kind with their own pollen for half-a-dozen generations,” he mused, “they have the power of fixing each fleeting variety of colour.” For all others, though, the lesson he drew from his experiments was to cross dissimilar individuals. Stock breeders should pair animals that were not only unrelated, but had been reared at “two or more distant and differently situated farms,” while cultivators of the soil ought to obtain their seed from diverse locales. For humans, the marriage of close relatives was to be avoided, although those, like Darwin himself, who had married first cousins might be

⁹⁰ *Ibid.*, 79.

⁹¹ *Ibid.*, 15-16, 18, 352.

exonerated, for this slight degree of interrelatedness “would be much less injurious than that of persons who had always lived in the same place and followed the same habits of life.” What was most important for children, regardless of their species, genus, order, class, or kingdom, was that their parents should be a little different from each other, and whether that difference came from heredity or environment did not much matter.⁹²

Providing advice to the methodical selector was not, however, Darwin’s driving concern while writing *The effects of cross and self fertilisation*, even if his experiments were themselves a form of methodical selection. Even as his hands (and his gardeners’) tended the greenhouse, his eyes remained on natural selection, and he thought that his results “[threw] light on ... the whole subject of hybridism, which is one of the greatest obstacles to the general acceptance and progress of the great principle of evolution.” There was in the closing pages of the work the same litany of hybrids, varieties, and species that had taken a draft form in *Origin of species* in 1859, been elaborated in *Contrivances* in 1862, expanded in *Variation under domestication* in 1868, and that now, finally, reached its apotheosis in 1876. Eleven years of experimentation across fifty genera had proven conclusively that intensive inbreeding sends a line toward sterility, “we have ... no right to maintain that the sterility of species when first crossed and of their hybrid offspring, is determined by some cause fundamentally different from that which determines the sterility of” the inbred plants, and finally and most importantly “it is quite unjustifiable to assume that the sterility of species when first crossed and of their hybrid offspring, indicates that they differ in some fundamental manner from the varieties or individuals of the same species.” The major point of *The effects of cross and self fertilisation* was that domesticated varieties can be models for species in the wild, methodical selection a stand-in for natural selection.⁹³

Despite all its talk of hybridism and speciation, Darwin’s new volume had almost no resonance among the naturalists. In the pages of *The gardeners’ chronicle*, however, it sparked a greater controversy than *Origin of species* had. The journal’s new editor, as had the late Lindley before him, praised Darwin for the services he rendered to the art of horticulture:

[T]he great value of Mr. Darwin’s last work ... consists in the practical applications which follow from the author’s very numerous, protracted, and laborious experiments. Seed growers and hybridisers will find ... that much that was mere haphazard and of a tentative nature in their practice has been by Mr. Darwin reduced to rule and method. Uncertainty and loss of time are thus to a considerable extent replaced by certainty and confidence as to result.

But a far more substantial review in the magazine, published in eight installments between January and May of 1877, ferociously attacked Darwin’s central project. “I maintain,” wrote its author, the Reverend George Henslow, “that, as a broad general principle, self-fertilisation in the vegetable kingdom is not ‘injurious’ in any ordinary sense of the term.” Later that autumn, a revised

⁹² *Ibid.*, 459-461.

⁹³ *Ibid.*, 27, 466-467, 469.

critique of *The effects of cross and self fertilisation* in hand, he assured the Linnean Society in London that “self-fertilisation was a great principle in Nature; that plants naturally so raised are as healthy as any others; ... [and] that such plants are the most widely dispersed and are in every way ‘the best fitted to survive in the struggle for life’.”⁹⁴

The Reverend Henslow was no stranger to Charles Darwin. His father was John Henslow, the Cambridge University professor of botany who had introduced the young Darwin in the late 1820s to the world of natural history, “a circumstance which,” Darwin later reminisced, “influenced my whole career more than any other.” He was also the brother-in-law of Joseph Hooker, Darwin’s personal friend and botanical ally at Kew Gardens. In 1865, on the recommendation of Hooker, Henslow had opened a correspondence with Darwin on various botanical subjects; the two exchanged letters on-and-off for the next couple of years. At one point, Darwin even loaned Henslow a book on hybridism.⁹⁵

But the younger Henslow had never become a committed Darwinist. At Cambridge he studied divinity alongside natural science, and through the 1860s he’d worked at various schools as headmaster and in various parishes as curate. On the side, he lectured in botany and dabbled in horticulture. In the early 1870s, he began to write books on scientific topics for a popular audience, and one of his recurrent themes was the reconciliation of theology with developments in natural history; in *The theory of evolution of living things* of 1873, he posited “God to be the Author of Creation and believe[d] Him to have adopted Evolution as the method by which He chose to bring about the existence of successive orders of beings until Man appeared upon the scene of Life.” While Henslow believed that zoological, botanical, and geological lines of evidence supported the idea of evolution, he was not convinced that selection was the motive force behind the process.⁹⁶

In his long, aggressive review of *The effects of cross and self fertilisation*, he did not explicitly address Darwin’s hybridism argument, but he steadfastly refused to imagine Darwin’s plants as universal models for all organisms or to envision Darwin’s experimental manipulations as analogous to the force of natural selection. Darwin had, he conceded, made several convincing, potentially very useful points; he had demonstrated that “the colouring of self-fertilised plants always tends to become extremely uniform in successive generations,” that crossing a botanical line long-cultivated with fresh stock could significantly increase vigor, and that there is little advantage gained from crossing two different flowers from the same plant. These were good rules to

⁹⁴ [“Review of Charles Darwin’s *The effects of self and cross fertilisation in the vegetable kingdom*,”] *The gardeners’ chronicle* VII, no. 166 (1877): 276. George Henslow, [“‘The fertilisation of plants,’ a review of Charles Darwin’s *The effects of self and cross fertilisation in the vegetable kingdom*”], Part 7. *The gardeners’ chronicle* VII, no. 174 (1877): 534. “Self fertilisation of plants,” *The gardeners’ chronicle* VIII, no. 202 (1877): 586.

⁹⁵ Darwin, *Autobiography*, 64. Bernard Lightman, *Victorian popularizers of science: Designing nature for new audiences* (Chicago: University of Chicago Press, 2007), 89-90.

⁹⁶ George Henslow, *The theory of evolution of living things and the application of the principles of evolution to religion considered as illustrative of the “wisdom and beneficence of the Almighty* (Cambridge: Cambridge University Press, 1873), 31. On Henslow’s biography and beliefs about evolution, see Lightman, *Victorian popularizers*, 87-94.

guide the creative horticulturalist. But in Henslow's eyes, the greenhouse at Down, the rows of ceramic pots of crossed and selfed plants, the controlled regime of watering and light and soil, were artificial impositions, strange contrivances that put the seed into circumstances it would rarely, if ever, encounter in nature. Some plant species in the wild are self-fertile, others are self-sterile, and some alternate between the two states depending on environmental conditions. It made little sense to say that inbreeding was injurious to all plants:

The fact appears to me to be this:- the word "injurious" is a purely relative term. If a plant is so highly differentiated that it has not only become adapted to insect agency but also to be self-sterile, then, of course, to put its own pollen upon the stigma of a flower, rather than that of another plant, may be said to be, relatively to the latter process, at least useless if not injurious.

To Henslow, self-fertilization could be a survival strategy that some species had adopted in response to their external surroundings.⁹⁷

How could one draw any generalized lesson from these experimental plants without first understanding the specific conditions under which the experiment had been conducted? Henslow granted legitimacy to Darwin's results, so long as they were understood within the horticultural context in which they had been generated, but he was unwilling to allow those results to speak for all flowering plants, let alone all organisms. He was also unwilling to accept Darwin's interpretation of those results. Week after week, in his serialized response, he dissected Darwin's data, delving into the details to divulge how Darwin had dismissed evidence that might suggest self-fertilization was not such a poor state of affairs. There were, of course, the many counter-examples with which Darwin had wrestled, Hero, the poppies, the pea plants, and Henslow rehabilitated them, arguing they were not deviants from a norm, but a legitimate, alternate class all their own. The majority of flowering plants, he pointed out, were self-fertile, and there was no reason to think that the plants Darwin had enlisted in his experiments were a good representative sample of all the plants out there. And individuals like Hero, that were themselves capable of thriving under intensive inbreeding even though they came from species which seemed not to do well under self-fertilization, illustrated just how malleable organisms were when faced with a fluctuating environment:

[W]hen we see a plant thus usually self-sterile becoming, under changed circumstances, self-fertile, this power of self-fertilisation is actually so much positive gain ... Now it is clear that many plants have become highly differentiated by adaptation to insects, and in becoming so, the pollen has physiologically changed correlatively to such a degree as to be useless on the flower's own stigma. This therefore is so much absolute loss to the

⁹⁷ George Henslow, ["The fertilisation of plants," a review of Charles Darwin's *The effects of self and cross fertilisation in the vegetable kingdom*"], Part 1. *The gardeners' chronicle* VII, no. 159 (1877): 42. Henslow, ["The fertilisation of plants,"] Part 7, 534. Henslow, ["The fertilisation of plants,"] Part 3, 204.

plant, as it now has to depend, with less certainty, upon the chance visit of insects; but when the plant can recover its lost power, and especially when it can acquire fresh and new constitutional elements ... the first result is the absolute re-gain of self-fertilisation ... accompanied by an equality with, or even a superiority in vigour to, the intercrossed.

What these plants were up to could not be understood without reference to the context in which they were developing.⁹⁸

For Henslow, there was nothing *intrinsically* wrong with close inbreeding. In some situations, it might prove beneficial, and in others harmful. It was not possible to create an experimental situation that was not at the same time situated somewhere, under some particular circumstances, and to pretend otherwise merely masked *those* circumstances. So much of the evidence of inbreeding's injuriousness furnished by methodical selection made the conditions under which selection had been applied invisible. Take, for instance, the varieties of peas that the British horticulturalist Andrew Knight fixed in the late eighteenth century, that were cultivated under perpetual self-fertilization and improved later by his devotee Thomas Laxton. These lines persisted in health for half a century without a cross, yet Darwin believed them to, in the end, have buckled under the accumulating weight of the practice. "Was the dying out of these varieties," asked Henslow, "due to degeneracy, or mainly to fresh varieties competing with them in the market and superseding them?" And if self-fertilization were recognized as a stratagem recently adopted, a response to current changing conditions instead of an evolutionary hangover, some contemporary situations might be better explained. Most botanical species that had passed from Britain along trade routes and had taken root in the colonies were highly self-fertile; their independence from insect pollinators likely made it possible for them to colonize spaces distant from where they had originated, while plants heavily dependent on cross-fertilization had to remain close to their arthropod coevals. Self-fertilization might be a characteristic of weeds, and it might be on the rise as a result of recent historical developments.⁹⁹

Henslow's review was confrontational in tone, unyielding in its opposition to the conclusions about inbreeding drawn in *The effects of cross and self fertilisation*. No contributor to *The gardeners' chronicle* came forward to defend Darwin, and he himself only wrote a brief letter to the editor to correct some minor errors in Henslow's account. "I have long been convinced that controversy is a mere waste of time," he explained in one of his final dispatches to the newspaper, and "I will, therefore, not make any other remarks on Mr. Henslow's criticisms, though I think that I could answer them satisfactorily." The two exchanged a few personal letters on cross-fertilization after the article was published, but they clearly had little useful to offer each other. Darwin was, at long last, finished with the topics of self-fertilization and inbreeding, while for Henslow, not everything in biology needed the light of Darwinian evolution in order to make sense.¹⁰⁰

⁹⁸ Henslow, ["The fertilisation of plants,"] Part 3, 204.

⁹⁹ Henslow, ["The fertilisation of plants,"] Part 8, 560.

¹⁰⁰ Charles Darwin, ["Response to George Henslow's review of *The effects of cross and self fertilisation in the vegetable kingdom*"], *The gardeners' chronicle* VII, no. 165 (1877): 246.

VII

When Charles Darwin passed away in the spring of 1882, his widow Emma planned to have him buried, per his final wish, near the humble parish church of St. Mary's in Down where two of his children and his older brother were already at rest. Word of his passing, though, spread quickly through London, and his friends and colleagues in the metropolis arranged instead to have him interred at Westminster Abbey. On April 26th, at a grand ceremony attended by thousands, including scientific luminaries, civic representatives, and the assorted Victorian celebrity, respects were paid to Britain's most famous naturalist, who was placed in the nave not far from the remains of Sir Isaac Newton.¹⁰¹

Among the pallbearers keeping Darwin's casket aloft was Alfred Russel Wallace, who had maintained through the years collegial, congenial ties to the man who had often loomed so large over him. In recent years, Wallace's financial situation had become precarious; he was unable to find any scientific employment, and he depended on the occasional returns from his writings to get by. Though Wallace's reputation in elite scientific circles was dodgy, Darwin, after discovering his correspondent's circumstances, had attempted in 1879 to secure a yearly civil pension for him for the services he'd rendered to natural history. The first person Darwin contacted with the idea, Joseph Hooker, thought it was atrocious: "Wallace has lost caste considerably, not only by his adhesion to Spiritualism, but by the fact of his having deliberately and against the whole voice of the committee of his section of the British Association, brought about a discussion of Spiritualism at one of its sectional meetings." The sharp, disapproving tone of Hooker's response initially deterred Darwin from further pursuing the matter, but he later put his own reputation on the line and successfully worked with some of Wallace's friends to get the grant from the government. A year after his funeral, a thankful Wallace eulogized his late benefactor: "[F]or long years to come the name of Darwin will stand for the typical example of what the student of nature ought to be. And if we glance back over the whole domain of science, we shall find none to stand beside him as equals."¹⁰²

Wallace lived this curious double life, at once scientific naturalist *and* spiritualist radical. The same man who authored *The geographical distribution of animals*, praised by the members of the British Association for the Advancement of Science as one of the century's most important works of biogeography, would indulge in sessions of spirit rapping, spectral photography, the midnight séance. This strange dissonance bred among the more orthodox a great distrust of Wallace, though he and those close to him saw no contradictions in his behavior. On the contrary, the way in which Wallace had conceptualized natural selection legitimized his journeys into the netherworld.¹⁰³

How these two realms were reconciled in Wallace's mind can be discerned in the pages of *Darwinism*, a long defense of evolution by natural selection that Wallace spent most of 1888 writing. Having returned home from a year-long

¹⁰¹ Browne, *Darwin: Power of place*, 495-497.

¹⁰² Quoted in Sloten, *Heretic in Darwin's court*, 357-358, 376.

¹⁰³ On *The geographical distribution of animals*, see *Ibid.*, 320-325.

speaking tour of the United States, Wallace decided to rework his lecture notes into publishable form; the resulting product, which was ostensibly a tribute to Darwin and his legacy, reads like Wallace's own version of *Origin of species*. Most of the same themes are present in the two books, but Wallace's includes the fruit of thirty years of further research, debate, and reflection. It also, and more subversively in a publication bearing Darwin's name, unfolds from the premise of Wallace's 1858 essay that Darwin did not accept: the idea that domesticated organisms are not very good models for what natural selection has done. "It has always been considered a weakness in Darwin's work," notes Wallace early in *Darwinism*, "that he based his theory, primarily, on the evidence of variation in domesticated animals and cultivated plants." Because of this, Wallace proposes that "[a] full exposition of the facts of variation among wild animals and plants is the more necessary, because comparatively few of them were published in Mr. Darwin's work." Where "Darwin was accustomed to appeal to the facts of variation among dogs and pigeons," Wallace has "endeavoured to secure a firm foundation for the theory in the variations of organisms in a state of nature." Darwin's careful descriptions of differing breeds and varieties are replaced in *Darwinism* by "a series of diagrams, to exhibit to the eye the actual variations as they are found to exist in a sufficient number of species." As he proposed it would be in 1858, the primary line of evidence for natural selection in Wallace's 1889 synthesis is transition from varieties *in a state of nature* to species *in a state of nature*.¹⁰⁴

Artificial selection is not absent from *Darwinism*, but it has been displaced from the central position it occupied in Darwin's project. Wallace opens by introducing the fundamentals of his theory of evolution, and then, instead of discussing the breeds of pigeons and their fanciers, he marshals examples from the zoology and botany literature to illustrate how widely species vary in the wild. Domestication does not make its first appearance until the fourth chapter, and here Wallace's intent is not to use the process as a model for natural selection, but rather to use domesticates as another line of evidence for how widely a species is capable of varying. "[I]t is not at all surprising that it should be so," he notes, "since all the species were in a state of nature when first domesticated or cultivated by man, and whatever variations occur must be due to purely natural causes." The domesticate is a refracted form of wild nature, not a model of it.¹⁰⁵

But what boundary are those organisms crossing that forces their refraction? In the same manner that Darwin required a foundational narrative to legitimize his conclusions in *Variation under domestication*, Wallace required one as well for *Darwinism*. Darwin's story had been one of *continuity*, of selection persisting as an unchanged process from the inception of life through the present-day, a tale wherein the domesticated and the wild, though they may appear to be dissimilar, were in essence the same. Wallace had a different story entirely.

By his account, the earth's history had been punctuated by three *ruptures* in unfolding time, three moments "in the development of the organic world

¹⁰⁴ On the circumstances under which *Darwinism* was produced, see *Ibid.*, 408-409, 416. Alfred Russel Wallace, *Darwinism*, 2nd ed. (London: Macmillan, 1891), vi, 81.

¹⁰⁵ Wallace, *Darwinism*, 101.

when some new cause or power must necessarily have come into action." To make these moments intelligible, he introduced an analogy from the physical world. At one time, geologists thought that all the features of the earth's surface, including "moraines and other gravel deposits, boulder clay, erratic boulders, grooved and rounded rocks, and Alpine lake basins," could be explained as the result of "denudation by wind and frost, rain and rivers," ordinary, oft-repeated, natural processes. The dawning realization that glaciers, overwhelming bleak white sheets of slow-moving ice, had once covered Europe caused geologists to realize that another powerful force, one with which they had not reckoned, was responsible for the changes as well. The glacier "was no breach of continuity, no sudden catastrophe; the cold period came on and passed away in the most gradual manner, and its effects often passed insensibly into those produced by denudation or upheaval; yet none the less a new agency appeared at a definite time, and new effects were produced which, though continuous with preceding effects, were not due to the same causes." His ruptures in the history of life were moments in which natural selection could not explain the course of evolution, even as the process remained widely in operation.¹⁰⁶

The first moment of rupture, which could not have been predicted from the natural forces that had preceded it, was the transition from "inorganic to organic, when the earliest vegetable cell, or the living protoplasm out of which it arose, first appeared." Natural selection came into force soon after, generating endless variations from these first life forms. The second moment was the "introduction of sensation or consciousness, constituting the fundamental distinction between the animal and vegetable kingdoms." Selection had produced the body in which this consciousness became possible, but it had not created consciousness itself, for it would be "altogether preposterous to assume that at a certain stage of complexity of atomic constitution, and as a necessary result of that complexity alone, an *ego* should start into existence, a thing that *feels*, that is *conscious* of its own existence." Into this world of thought and sensation came natural selection once more, acting upon these new elements to shape and perpetuate some animal forms over others. The third rupture was the sudden introduction into anthropoid apes of mathematical ability, musical talent, artistic skill, the appreciation of beauty, an understanding of metaphysics, wit, and humor; these traits carried no discernable survival value in the wild, yet they could be found to some degree in any human. These "special faculties," brought to expression only when favorable social conditions made them possible, "clearly point[ed] to the existence in man of something which he ha[d] not derived from his animal progenitors," something that could not have been molded by the gradual operation of natural selection, something belonging to another agency, another power.¹⁰⁷

To Darwin in *Variation under domestication*, no solid line divided one era from the next, no radical leap separated artificial selection from everything that had preceded it. For Wallace, though, artificial selection, especially its methodical variant, belonged to that time after the third rupture, when humans could no longer be said to be exactly like other animals, but carried some extra-animal quality within themselves. Domesticated organisms were fundamentally

¹⁰⁶ *Ibid.*, 463, 474.

¹⁰⁷ *Ibid.*, 474-475.

different from their wild counterparts; they were still plants and animals, but they had also been altered, through their immersion in human societies, by this other power, this other agency, which was *not* natural selection. In this sense, the narrative spun in *Darwinism* in 1889 was consonant with the way Wallace had conceptualized natural selection in 1858, when he had insisted that “[d]omestic animals ... are subject to varieties which never occur and never can occur in a state of nature.”¹⁰⁸

Though they largely agreed about how species in the wild had come into being, Wallace and Darwin had elaborated two distinct visions of how nature and human civilization related to each other. These visions were out of sync; they suggested differing pathways for how society should proceed, for what form future evolutionary research projects ought to take. In the decades since 1858, Darwin had reoriented his position within the breeding and horticultural communities to more explicitly transform them, at least in his mind, into an experimental evolution. The organism in its environment began in his writings to evaporate as domesticates themselves, especially those breeds recently created by the most intensive methods, became stand-ins for the wild nature out there. Wallace believed more firmly in the wild nature out there, away, and in his vision the domesticated organism remained something of a mystery, half-natural half-cultivated, an unsettled category still awaiting its explanation in terms derived from elsewhere. The odd personal connection between the two men, the interdependence of their lives and the way each used the other while they forged a mutual legacy, was a tactical marriage of convenience, without consummation.

To convince his audience to accept his vision, Darwin embarked on a long, meticulous journey into hybridism, inbreeding, self-fertilization, and the history of domestication. He was struggling, through material demonstration, to prove his worldview. Wallace took another tack. Structurally, the agency that had risen at the third rupture in the history of life, that had given us those parts of the human mind that had no animal antecedents, might hypothetically have been any force, so long as it was not natural selection: social organization, cultural transmission, language, advanced tool manufacture, the intervention of Martians. Wallace, though, was adamant that it, along with the two earlier moments of rupture, had an identifiable cause: “These three distinct stages of progress from the inorganic world of matter and motion up to man, point clearly to an unseen universe – to a world of spirit, to which the world of matter is altogether subordinate ... [T]hose progressive manifestations of Life in the vegetable, the animal, and man ... depend upon different degrees of spiritual influx.” And what Wallace offered those who accepted his worldview was not a rational explanation of how the universe operated, but rather the hope of a meaningful life:

They will ... be relieved from the crushing mental burthen imposed upon those [like Darwin] who – maintaining that we, in common with the rest of nature, are but products of the blind eternal forces of the universe ... have to contemplate a not very distant future in which all this glorious earth – which for untold millions of years has been slowly developing forms of life and beauty to culminate at last in man – shall be as if it had

¹⁰⁸ Darwin and Wallace, “On the tendency,” 61.

never existed; who are compelled to suppose that all the slow growths of our race struggling towards a higher life, all the agony of martyrs, all the groans of victims, all the evil and misery and undeserved suffering of the ages, all the struggles for freedom, all the efforts towards justice, all the aspirations of virtue and the wellbeing of humanity, shall absolutely vanish, and, "like the baseless fabric of a vision, leave not a wrack behind."

Darwin projected an evolving material universe where the naturalist-become-breeder might only watch passively as selection unfolded inevitably. Wallace invited a defiance of that universe, a hope that the collective cooperation of human minds might redirect that force of nature toward new ends.¹⁰⁹

In the years after Darwin's death, the fate of his vision was uncertain. Evolution was widely accepted in the scientific community, even if there was still significant opposition outside, but there was no consensus around Darwin's version of evolution, with its emphasis on natural selection. Even those like Wallace who considered themselves Darwinians and granted selection a central role in the evolutionary process still deviated in major ways from the program of experimental evolution that Darwin had enunciated and in part enacted in the 1860s and 1870s. There was no agreement among biologists about what the relationship between domesticated and wild organisms was. Nor at the end of the century was there an institutional home for the research program that Darwin had spent his life elaborating. New political and economic arrangements, however, were beginning to coalesce outside of the scientific community that would find the Darwinian worldview quite useful. We turn to these in the next chapter.

¹⁰⁹ Wallace, *Darwinism*, 475-476.

Chapter Two

“500 plants so nearly alike that you could not tell them apart”: Self-fertilization and intensive inbreeding at the United States Bureaus of Plant and Animal Industry, 1897-1907

When in the first half of the nineteenth century a prominent London veterinary surgeon by the name of William Youatt, a contemporary of Darwin, toured central England to survey the country's livestock, he found throughout Leicestershire “the most valuable of long-woolled sheep.” Short, squat animals with torsos as round as wooden barrels, held aloft by weak thin-boned wobbly legs, the New Leicesters gazed vacantly about themselves from “eyes prominent, but with a quiet expression.” Their value came not from their wool, which was unremarkable, but from their “early maturity, and a propensity to fatten, equalled by no other breed,” which produced a meat that was “tender and juicy,” though, in “the opinion of many persons, somewhat insipid.” These rotund ruminants dotted the pastoral landscape, efficiently converting grass and clover into fatty mutton; “[t]hey are sooner prepared for the butcher than any other description of sheep,” noted Youatt, “and the pastures left ready for other purposes.” Their anatomies had been engineered to maximize the proportion of cuts that could be roasted instead of boiled, their bones made fine rather than sturdy to take up less space on the leg, and if as a consequence their lambs were “weakly, and unable to bear the occasional inclemency of the weather,” Youatt decided this was a small price to pay for a breed that provided the grazier with the “return of most money for the quantity of food consumed.”¹¹⁰

The New Leicester was not only common in central England, but might be found throughout the British Isles; Youatt observed with some admiration that “[s]uch, indeed, have proved to be their merits, that at the present day there are very few flocks of long-woolled sheep existing in England, Scotland, or Ireland, which are not in some degree descended from” them. By the second decade of the nineteenth century, the breed would have been familiar to a farmer in any region of rural Britain. And yet, despite this far-flung diffusion and ready adoption by pastoralists, the New Leicesters were not a traditional British breed, but had come into being quite recently and under unprecedented circumstances. They had originated only fifty years previously on a single farm of roughly 450 acres, the Dishley Grange outside of Loughborough, where Robert Bakewell, a third-generation tenant farmer, had begun in the late 1740s to experiment with a breeding technique that had only previously been attempted systematically with pigeons and fowl. With the aid of hedgerows, fences, and an elaborate irrigation works, Bakewell divided Dishley into numerous enclosures, one acre each closer to his house and ten acres each further out, that allowed him to sort, segregate, and scrupulously observe his livestock. His interests ranged widely; by 1771,

¹¹⁰ William Youatt, *Sheep: Their breeds, management, and diseases* (New York: Orange Judd & Co., 1867), 25-26, 30-32.

Arthur Young, the agrarian travel writer, counted at Dishley “sixty horses, four hundred large sheep and one hundred and fifty beasts of all sorts.”¹¹¹

Bakewell aimed to produce new types of cattle, sheep, pigs, and horses custom-tailored for burgeoning new urban markets; his meat animals would fatten faster in the right places than traditional livestock, and his draught and harness equines would provide sturdier labor. The suddenness with which he accomplished these goals both impressed and startled his countrymen. The thick round New Leicesters looked nothing like the tall, flat-sided sheep that had grazed the shire throughout living memory; likewise, in mere decades Bakewell’s Longhorn cattle became “unrivalled for the soundness of [their] form, the smallness of [their] bone and [their] aptitude to acquire external fat.”¹¹²

These rapid transformations were brought on by a controversial new mating plan, what became known as a system of in-and-in breeding. Bakewell first, as Youatt recalled, surveyed sheep throughout Leicestershire and “selected from the different flocks in his neighborhood, without regard to size, the sheep which appeared to him to have the greatest propensity to fatten, and whose shape possessed the peculiarities which he considered would produce the largest proportion of valuable meat, and the smallest quantity of bone and offal.” Traditionally, in order to maximize the number of head produced in a season, English stockmen kept all of their animals, regardless of sex, together, allowing them to mate at will. After making his selections, however, Bakewell broke with this practice and housed each of his rams and bulls in separate enclosures and kept his ewes and cows far from them. This arrangement allowed him to personally select which pairs of animals mated and when. And as he paired ewe to ram and bull to cow, he broke with another standard practice because he “did not object to breeding from near relations, when, by so doing, he put together animals likely to produce a progeny possessing the characteristics that he wished to obtain.” Indeed, the pairing of near relatives became the centerpiece of Bakewell’s system; a favorite ram might be induced to mate with his own sister, or with his own daughter, granddaughter, and other descendents down many generations. Bakewell became so convinced of the effectiveness of this method of inbreeding that he even carried it over to the cabbages on Dishley, which he grew in isolated patches throughout fields of wheat so that unrelated cabbages were far enough apart that they could not cross-pollinate each other.¹¹³

In time, Bakewell’s inbred creations became known not only for their propensity to fatten, but also for their reliability as breeders. His bulls and rams passed their most favored characteristics on to their descendents with an unparalleled effectiveness. When he first offered to loan out one of his rams for studding for a small fee at the start of his breeding program, in 1760, Bakewell found few takers; his neighbors could not understand why they would want to pay for a service their own rams were pleased to provide for free. The highest

¹¹¹ *Ibid.*, 28. Roger J. Wood and Vítězslav Orel, *Genetic prehistory in selective breeding: A prelude to Mendel* (Oxford: Oxford University Press, 2001), 61-62, 67. H. Cecil Pawson, *Robert Bakewell: Pioneer livestock breeder* (London: Crosby Lockwood & Son, 1957), 19.

¹¹² Pawson, *Bakewell*, 56.

¹¹³ Youatt, *Sheep*, 26. On the novelty of Bakewell’s pairing practice, see Harriet Ritvo, “Possessing Mother Nature: Genetic capital in eighteenth-century Britain,” in John Brewer and Susan Staves, eds., *Early modern conceptions of property* (London: Routledge, 1995), 416. Youatt, *Sheep*, 27. Wood and Orel, *Genetic prehistory*, 62.

fee he collected for a ram that season was seventeen shillings. After his system of in-and-in breeding had been rigorously followed and his sheep fleshed out, however, Bakewell discovered that he could charge staggeringly high prices for a visit from one of his rams. In 1784, he received as much as £100 for his most famous animals, and by the 1790s he might expect the enormous sums of £300 or £400 per stud per season. Livestock breeders believed, with good reason, that Bakewell's animals were capable of transmitting their traits to other herds and flocks.¹¹⁴

Careful selection followed by close inbreeding produced striking results. As the years passed, Robert Bakewell sold fewer of his animals on the open meat market and instead took his income from loaning his bulls and rams out during the breeding season. The Dishley Grange became primarily a stud farm, one of the first in England to be composed primarily of livestock other than horses. Bakewell had perfected a method for sculpting the heredity of groups of large organisms; “[i]n effect,” writes Harriet Ritvo, “he was selling a template for the continued production of animals of a special type.” With this method, he had found a way to substantively transform agricultural life forms within a single human generation. While regional differences in British livestock had developed gradually over the centuries, spanning many human lifetimes, the Longhorn and the New Leicester were crafted primarily by one breeder in the much shorter length of decades. What was dawning in Leicestershire, within the limits of vision of a man who dreamed of producing an animal that more efficiently converted pasture into table meat, was the possibility of substantively re-engineering crops and livestock to provide for the needs and desires of an exploding urban population. Close inbreeding might become a tool for binding agricultural spaces more tightly to urban centers or, viewed from a slightly different vantage, a weapon that could be used by the metropolis to conquer intransigent landscapes.¹¹⁵

By converting Dishley into a sizable stud-production center, Bakewell bolstered the scale at which his new breeds could be dispersed into the surrounding countryside. He recognized, however, that the very effectiveness of his program threatened its ability to continue generating profit. The fees for borrowing original Longhorns or New Leicesters were steep, and those willing to pay such high costs did so because they had long-term plans to go into commercial breeding themselves. Inbred livestock transmitted their characteristics reliably from generation to generation, so their heirs could become their functional replacements. Farmers with flocks or herds descended from the originals at Dishley could enter the studing business as new competitors without any further need to pay for Bakewell's services.

This business dilemma, inherent to pure-breeding, vexed Bakewell, and he actively sought ways to overcome it. One solution was the Dishley Society, an organization that Bakewell established with his neighbors in 1783. Anyone who wanted access to Dishley studs was required to join the Society, which imposed stringent rules upon its membership. The first and foremost of these was that members were prohibited from utilizing rams that did not belong to either

¹¹⁴ Ritvo, “Possessing Mother Nature,” 416.

¹¹⁵ *Ibid.*

Bakewell or others within the Society. The Society imposed a limit on the numbers of rams that a member could rent out to non-members in a given season, and required that no ram ever be allowed to service more than two different farms. None within the Society were permitted to sell their breeding sheep outside the Society, unless they sold their entire flock, and the new owner would presumably be under pressure to himself become a member of the Society. To control, however crudely, the way in which his new inventions became distributed across the countryside, Bakewell in essence made all of his associates' sheep part of one virtual super-flock, which produced meat for the market but did not provide reproductive services to outsiders.¹¹⁶

The plan worked, but only for as long as the Society's founder lived. After the bachelor Bakewell passed away in October 1795, leaving behind no heirs and no clear successor, the Dishley Society unraveled. Without strict rules to regulate their reproduction, the curious new livestock of Leicestershire spread throughout Britain. Commercial breeders far from the Dishley Grange carried the animals into new regions, and from these beachheads their descendants diffused into the hills and valleys. Without Society regulation, though, the flocks and herds lost some degree of their resemblance to their progenitors at Dishley with each new introduction. The breeds' new caretakers were not concerned about preserving the forms that Bakewell had crafted; instead, they were interested in producing livestock that would flourish under new local conditions. As New Leicester rams moved south into Oxfordshire, for example, they were no longer mated to their own relatives but were instead paired with ewes from established, long successful local varieties. These crosses created new hybrids that married the New Leicesters' rapid fattening to physical qualities helpful for robust survival; a cross with the traditional Cotswold, for instance, produced the Improved Cotswold, which became established in the region, while a later cross with the Southdown resulted in the successful Oxford Down.¹¹⁷

As quickly as Bakewell had created his pure breeds, these new cross-breeds supplanted their predecessors. By the 1820s, Bakewell's Cotswold had become scarce in Oxfordshire, and when Youatt visited Leicestershire in 1833 he was surprised to discover that Bakewell's cattle breed had been superseded by a new inbred Shorthorn breed: "There are not a dozen pure [Longhorns] within a circuit of a dozen miles of Dishley. It would seem as if some strange convulsion of nature, or some murderous pestilence, had suddenly swept away the whole of this valuable breed." Without a constant, attentive program of regulation to maintain it, a pure breed would fade and vanish. This became the fate of both the New Leicesters and the Longhorns. Without long-range efforts to tend them, the two varieties melted into diverse flocks and herds around the world yet themselves largely evaporated as recognizable types. The intense inbreeding program at Dishley was like a pebble dropped into a pond, sending ripples across the still surface that expanded their circumference but lost their intensity the farther they moved from their center.¹¹⁸

¹¹⁶ Pawson, *Bakewell*, 73.

¹¹⁷ John R. Walton, "The diffusion of improved sheep breeds in eighteenth- and nineteenth-century Oxfordshire," *Journal of historical geography* 9, no. 2 (1983): 181-187.

¹¹⁸ Pawson, *Bakewell*, 58.

A single breeder and his intimate circle of collaborators, no matter how devoted, then, were simply not capable by themselves of imposing long-term, fundamental change on the agrarian biosphere. At the same moment that intensive inbreeding showed its potency for adapting animal populations to fluctuating market conditions, it also showed how fragile its creations could be without support structures to shepherd them. The gains of one generation, however impressive, could be lost in the next. Lasting change, anatomies fixed across time, required extra-individual efforts, stable institutions that might persist far longer than mortal human beings.

At their simplest, these institutions might look like enlarged Dishley Societies, voluntary but formal associations of livestock producers who recognized the benefits of limited cooperation. These sorts of institutions might succeed in tapping into the hereditary materials of an extended animal population and marshalling them toward some fixed goal. By the end of the nineteenth century, loose associations of this type, devoted to pure-breeding, had formed around many animal breeds in Europe and North America. They maintained public pedigrees that allowed individual breeders to know the lineage of any purchased or rented animals, in effect allowing broader communities of breeders to work together, and they monitored the heredity of their members' animals and plants.¹¹⁹

Before these associations could produce *new* breeds, however, they would also need to produce new breeders. They could not depend in a happenstance fashion on the next Robert Bakewell to one day show up; they needed to train a generation of Robert Bakewells. This would require, overtly, forging connections through lobbying and outreach to social, cultural, and educational institutions, which could furnish promising young candidates. The effort would also be immensely aided, more subtly, by the accretion of ideologies that made breeders' interventions into the agricultural environment seem natural, benign, and even progressive.

In the first decade of the twentieth century, the U.S. Department of Agriculture began to take an interest in systematic breeding, both of animals and plants. By the middle of the century, it had brought these disparate breeding communities in the United States together, linking them to producers, to the states, to the universities, to the fields. The complex web of relations it wove, which funneled plants and animals into urban markets and steadily transformed rural ecosystems into efficient industrial production centers at a scale Bakewell could only have imagined, did not unfold according to a master conspiratorial plan; it was the end result of thousands of individuals, each acting from her or his own peculiar perspective, nudged along by bureaucrats, businessmen, and boosters always toward one final goal: profit. In the opening years of the twentieth century, along the verdant banks of a New England river, one federal bureau offered the first demonstration of just how powerful intensive inbreeding might be for building this program when it stamped an institutional vision into the flesh of nature, where it could remain lodged for generations.

¹¹⁹ Margaret E. Derry, *Bred for perfection: Shorthorn cattle, collies, and Arabian horses since 1800* (Baltimore: The Johns Hopkins University Press, 2003), 9.

II

Across the tobacco fields of the Connecticut River Valley, giant white cheesecloth tents sprouted throughout the early summer of 1902. From Hartford north into Massachusetts, almost 700 acres of land, nearly a tenth of the state's total tobacco acreage, disappeared beneath woven cotton gauze. Under the high tents, where the sweet odor of tobacco clung to the soupy humid air, laborers set new seedlings into the plowed soil, lining the plants up in narrow rows as straight as could be managed. This season, the tobacco plants were different from any other that had been sown in the valley, the prized Sumatra variety of Indonesia, and the protective shade covering of the tents would shield the delicate plants from direct exposure to the sun's rays and allow them to flourish in this new and unaccustomed climate.¹²⁰

Or at least that was the idea.

The largest growers of the valley were taking a great risk on this new variety, but they had been encouraged in their venture by the Bureau of Soils of the U.S. Department of Agriculture, which had circulated a bulletin earlier in the year suggesting that the cultivation of shade-grown Sumatra tobacco in New England would be not only possible, but also potentially highly profitable. The bureau's chief tobacco researcher, Marcus L. Floyd, had been advocating the shade-grown method ever since he arrived in Connecticut in December of 1899, and the results of two years of experimentation in the region's fields, published in detail in the bulletin, won over most of the largest land-owners to his way of thinking.¹²¹

According to Floyd, the only thing keeping the Connecticut Valley from becoming one of the world's premier tobacco-cultivating regions was its traditional dependence on the broadleaf and Havana varieties. These plants, which had been grown along the Connecticut River since the colonial era, had by the mid-nineteenth century established the region's reputation for producing fine cigar wrappers, and many local small growers raised this tobacco alongside other crops on diversified farmsteads. Though it had "long been recognized by the trade as the most desirable domestic tobacco for wrapper purposes," Floyd argued that at the turn of the century "[t]he one great trouble with the Connecticut tobacco is that it [has] not conform[ed] to the present requirements of the tobacco trade." The manufacturers needed to produce as uniform a cigar as possible for the market, and the Connecticut leaf, with its "unevenness of color and ... poor grading as to color, length, and quality," "forced [the manufacturer] to purchase a large amount of ... leaf from which to select," most of which would be wastefully discarded. That, along with the plants' other significant drawbacks (it was "too large for an ideal wrapper," its "veins [were] very large, and only the tip of the leaf [was] suitable for high-priced cigars,"), explained why New

¹²⁰ My estimate of the scope of shade-grown tobacco cultivation in 1902 comes from two sources. An August 28, 1904, article in the *Springfield Republican* claimed 700 acres had been devoted to shade-grown cultivation, while E.H. Jenkins, the director of the Connecticut Agricultural Experiment Station, estimated in 1905 that 8,000 acres were devoted to all forms of tobacco growing. See E.H. Jenkins, "The growing of tobacco in Connecticut," *The Connecticut magazine* ix, no. 2 (1905): 341.

¹²¹ Milton Whitney, "Growing Sumatra tobacco under shade in the Connecticut Valley," United States Department of Agriculture, Bureau of Soils bulletin 20 (1902).

England tobacco was selling between \$0.18 and \$0.20 per pound while imported Sumatra was going for somewhere between \$0.65 and \$1.15 per pound, even before a \$1.85 federal tariff pushed the price of a pound of the foreign import up to between \$2.50 and \$3.00.¹²²

What Floyd envisioned was a thorough transformation of the valley's cropland, a replacement of its tobacco plants that would require a concomitant remaking of the economic and social order. For while the broadleaf and Havana varieties were sprouting in the fields of many farmers on the edge of subsistence, shade-grown Sumatra tobacco cultivation would require a heavy investment of capital, the concentration of land into the hands of a smaller number of well-heeled growers, and an agrarian labor force more characteristic of tropical plantations than Yankee family farms. Floyd had spent time in the cigar-leaf producing region along the Florida-Georgia border, and he was intent on creating a tobacco-growing regime in Connecticut modeled on its development. During the 1850s and 1860s, the Florida spotted leaf variety had been popular with manufacturers, but the imported Sumatra leaf had taken a considerable chunk of that plant's market share. To revive their local industry, some of the Florida growers pooled their resources and began in the 1880s to experiment with raising various types of seed from Cuba. After their first successful test crop reached a purchaser in New York City, who put the leaves through a battery of stringent examinations, word spread through the industry that new possibilities had opened in northern Florida, and a rush of capital investment from New York began flowing into the southern fields. The growers continued their experiments, introducing new seed from many regions, and in 1892, after tinkering with their method of harvesting, the Floridians managed to produce a crop of Sumatra leaf that the manufacturers found an acceptable substitute to the Indonesian import. Some of the best leaves out of these early yields, brighter and thinner than the others, came from plants that had grown under the protective shadow of tall trees, and in 1896 a New York company in Gadsden County, Florida, covered an acre with a simple shed to emulate this microclimate. The leaves they produced were superb, better than any that had yet come from the Florida fields, and over the next couple of years shade-growing, abetted by new devices developed through trial-and-error like the cheesecloth tent, spread widely through the region.¹²³

In Connecticut, Floyd found all the resources he thought were necessary to replicate the Floridian case, and in the spring of 1900 he set the wheels of his plan in motion. Since the early 1890s, several major growers had collaborated to fund the Connecticut Tobacco Experiment Company, devoted to improving methods for raising and preparing tobacco in the valley, and the state experiment station had been providing regular assistance to the project. Floyd convinced the company's overseer, John A. Du Bon, to devote a third of an acre on the company's plot at Poquonock, out near Windsor Locks, to cultivating shade-

¹²² James O'Gorman, *Connecticut Valley vernacular: The vanishing landscape and architecture of the New England tobacco fields* (Philadelphia: University of Pennsylvania Press, 2002), 22-23. Whitney, "Growing Sumatra tobacco," 6.

¹²³ Marcus L. Floyd, "Results of tobacco experiments conducted in various parts of the United States," in Milton Whitney, *Field operations of the Division of Soils, 1900* (Washington, DC: Government Printing Office, 1901), 456-457.

grown tobacco; he, along with some of the Bureau's staff, provided guidance and Sumatra seed from the Gadsden County fields. The New England growers were initially reluctant to try the Sumatra, since the climate of their region was so different from the semi-tropical South, but Floyd argued that "during the actual growing season the difference [was] slight," and that "by the use of cheese-cloth shade ... the difference in the climatic conditions could be overcome."¹²⁴

The results obtained at the end of 1900 from the initial experiment seemed to support his enthusiasm. The cloth tents kept the humidity on the third of an acre high at all times, shielded the plants from the heaviest windstorms, and prevented most pests from reaching the crop. While the average tobacco plant in an open field in the valley grew to be four or five feet tall, the shade-grown Sumatra plants reached heights of eight or nine feet apiece. Most importantly, the crop grown in Poquonock looked to be commercially-viable; over one hundred pounds of leaves were shipped out to dealers, who cut smaller samples and passed them on to manufacturers, who proclaimed them, according to Floyd, "quite equal in every way to the finest leaf imported from the island of Sumatra."¹²⁵

As the 1901 growing season opened, the Bureau found that producers were decidedly more eager to learn how to cultivate tobacco under shade than they had been the year before. Floyd worked out an agreement with thirteen of the valley's largest farms: if they would put forward the land, labor, and capital to grow Sumatra, the Bureau would provide seed from Florida and the necessary expert supervision to implement the new method. The final crop produced would be marketed and sold by the Department of Agriculture, but all proceeds from the sale would be turned over to the producers. The growers collectively invested between \$20,000 and \$25,000 in the project, and Bureau personnel, under Floyd's direction, spread across Massachusetts and Connecticut to erect tents over 41 scattered acres of tobacco soil.¹²⁶

The growers were not alone in taking interest in the project. Several important figures within the Department of Agriculture kept a hopeful watch on what Floyd was up to. For the past couple of years, one of the major initiatives of the Bureau of Soils had been a series of expensive soil analyses conducted throughout the United States. Milton Whitney, the chief of the bureau, Floyd's immediate superior, thought this new tobacco work could be directly tied to this major research investment, and he repeatedly reminded reporters that the Sumatra project was "the result of the soil survey made in the Connecticut Valley two years ago." Whitney also brought the project to the attention of his superior, Secretary of Agriculture James "Tama Jim" Wilson. Wilson, who would remain in office longer than any other U.S. cabinet member, from 1897 to 1913, across three Republican presidencies, was seeking evidence of the practical results his department might deliver. In July, he paid a well-publicized visit to some of the shade tents in Tariffville, about five miles from the original site at Poquonock.¹²⁷

¹²⁴ *Ibid.*, 462.

¹²⁵ *Ibid.*, 467.

¹²⁶ Whitney, "Growing Sumatra tobacco," 8.

¹²⁷ "New Sumatra tobacco," *New York Times*, December 25, 1900. "Shade grown tobacco loses local farmers a million," *Springfield Republican*, August 28, 1904. On Whitney's political strategy in regard to

Funding for the bureau's work originated in Congress, so Whitney also actively courted Congressional support whenever possible. With this in mind, Marcus Floyd brought Congressman E. Stevens Henry of Connecticut's First District onto one of the Sumatra project's committees. With the prospect that his work might validate the earlier surveys and win the Bureau of Soils a supporter in the House of Representatives, Floyd was able to proceed with the full blessings of his boss Milton Whitney.¹²⁸

By the fall, the expanded experiment of 1901 looked to be as successful as the Poquonock project of the previous year. All thirteen growers reported a substantial yield with minimal losses; Whitney boasted that "the wrappers [were] all perfect leaves, without tear or puncture, and the color and grain [were] sufficiently uniform, and the veins [were] sufficiently small to allow the entire side of the leaf to be used for wrapping cigars." The new crop would not reach the market until May 1902, but the Connecticut Valley quite suddenly began to attract serious national attention. The *New York Times* predicted that the growing season of 1902 would be "the most interesting and perhaps the most valuable in the history of tobacco farming in New England."¹²⁹

Then, right at that moment of its greatest publicity, the big tobacco gambit of the Bureau of Soils began lurching towards disaster. Speculators descended on Hartford, some more honest than others, in the hope of cashing in on the new venture. One promoter, promising investors "[rich] dividends of 40 per cent" in the first year and a "reasonable expectation" of 80 percent in the second, shopped around a "large, finely-printed and excellently-illustrated book, with an embossed cover decorated with an attractive specimen of tobacco plant, over a part of which was stamped in a field of gold the shield emblematic of the United States government." One of the main selling points of his proposal was that the land on which he intended to grow Sumatra was "close ... to the United States government experiment station for raising tobacco." The bulletin that Whitney had issued at the end of the year, touting the results of 1901's experiments and providing practical, how-to information for would-be shade-growers, spread widely and convinced many of the new cultivators that they had the protection of the federal government for the expensive plan on which they were about to embark. Bureau staff, too, were drawn into the speculative frenzy. Marcus Floyd, aware that the planters and not the government had been the big winner in Florida, quit his job with the Bureau in December and took a new position, with twice the salary, as general manager of the newly-formed Connecticut Tobacco Producing and Trading Company out of Tariffville. As events began spiraling out of the Bureau's control, Milton Whitney was left to deal with them without the technical assistance of his main tobacco expert.¹³⁰

the soil surveys, see Douglas Helms, Anne B.W. Effland, and Patricia J. Durana, eds., *Profiles in the history of the U.S. Soil Survey* (Ames: Iowa State Press, 2002), 23.

¹²⁸ On the involvement of Congressman Stevens in shade-grown tobacco cultivation, see Whitney, "Growing Sumatra tobacco," 8-9.

¹²⁹ *Ibid.*, 30. "Tobacco in Connecticut," March 22, 1902.

¹³⁰ "Shade grown tobacco loses local farmers a million," *Springfield Republican*, August 28, 1904. "To grow Sumatra leaf on a large scale under shade, company organized for the purpose of planting in Connecticut," *New York Times*, December 12, 1901. See also Augustus Stabler, "The tobacco habit," *Friends' intelligencer* lix, no. 29 (July 1902): 452.

What had been touted as the most important year in the history of New England tobacco turned into a disaster for the industry. The growing season began inauspiciously when the shade-grown crop of the previous year, sold at a widely-observed Hartford auction in May, fetched prices far below the Bureau's enthusiastic hopes; some of the leaves sold for as little as 20 cents per pound. Undaunted, most of the large growers continued to expand their shade-grown acreage, but the weather complicated these plans: there was an "unprecedented amount of rain, falling in very severe showers," accompanied by "cool spells and especially cool nights," with windstorms "so strong in places that the posts [were] lifted from the ground." The Bureau had provided detailed estimates of how much it would cost in terms of labor and materials to switch to shade-growing, but the growers soon discovered that those were best-case estimates, quickly exceeded in actual practice. Worst of all, the tobacco that resulted at the end of the year was not a fit substitute for the imported Indonesian leaf; John Du Bon, who had overseen the Poquonock experiment of 1900, complained that the new Sumatra leaves would not properly burn, were not flexible enough, were sometimes brittle, often had an ugly dull coloration, and stunk "like those held in discredit by the fastidious smoker." One reporter found that "[c]igar manufacturers who have been asked why they did not use Connecticut shade-grown instead of imported Sumatra have smiled as if the question was too absurd to think about." Rather than sell their shade-grown product at the ridiculously low prices the manufacturers offered, many of the farmers stockpiled their leaves in warehouses in the hope that market conditions might significantly improve, or at least just change a little.¹³¹

By 1903, Milton Whitney was ready to throw in the towel. He withdrew employees of the Bureau of Soils from the Connecticut Valley, dispersing them to other projects around the United States, and declared against all evidence that the experiment had been a success: "It remains now for the growers to put the shade-grown Sumatra industry on a substantial basis, toward which condition great progress has already been made." What he left behind, along with lonely tents flapping in the breeze and piles of discarded cloth and lumber, was a deep distrust of the technical expertise of the U.S. Department of Agriculture. A front-page Sunday story in the *Springfield Republican* declared that "[i]n view of what has happened" the publications of the Bureau of Soils "appear unscientific to say the least. Government reports will never mean so much again to the New England tobacco growers." What the Bureau had engineered was "worse than a crop failure; it has a look to-day as if the farmers and their backers had been exploited."¹³²

Whitney also left behind his former employee, Marcus Floyd, who remained devoted to the shade-grown method, the Connecticut Tobacco Producing and Trading Corporation, the plan to bring plantation monoculture to

¹³¹ "Shade-grown tobacco sale," *New York Times*, May 1, 1902. United States Department of Agriculture, *Yearbook of the United States Department of Agriculture. 1902* (Washington, DC: Government Printing Office, 1903), 73. "Shade grown tobacco loses local farmers a million," *Springfield Republican*, August 28, 1904.

¹³² United States Department of Agriculture, *Yearbook of the United States Department of Agriculture. 1903* (Washington, DC: Government Printing Office, 1904), 65. "Shade grown tobacco loses local farmers a million," *Springfield Republican*, August 28, 1904.

New England, and his substantial new annual salary. Though most of the large growers in the region were disillusioned with the Department of Agriculture, Floyd still hoped to use his connections within the scientific establishment there to support his project. In mid-summer 1903, he invited two USDA plant specialists who did not work directly under Milton Whitney, Herbert J. Webber and Archibald D. Shamel, to tour his fields outside Tariffville, where he still maintained 40 acres of Sumatra under shade tents. The two saw an opportunity to move in where the Bureau of Soils had failed. Webber thought he understood what had gone wrong, and before long he convinced Shamel to remain in Connecticut to solve Floyd's problems.¹³³

What Webber diagnosed as the root cause of the failure of 1902 was "the variable nature of the plants grown under shade from foreign-grown seed." Unlike his USDA predecessors from the Bureau of Soils, who had hoped to transform the region's tobacco by creating humid new microclimates beneath cloth tents, Webber, an employee of the Bureau of Plant Industry, thought that the only way to make Sumatra succeed was to radically transform the plants themselves, to bring into being new varieties of tobacco descended from the Sumatra that might thrive in the more temperate climate of New England. Webber had duties he needed to return to in Washington, so Archibald Shamel, with the full support and encouragement of his superiors, remained in Connecticut. What he developed over the next couple of years was an inbreeding program that could rival Robert Bakewell's in the suddenness and intensity of its results.¹³⁴

III

It did not take long for Archibald Shamel to become entranced by the fields around Tariffville. He had just begun his career with the federal government; he was new to Connecticut and new to tobacco. He had grown up on a homestead in Illinois, a "prairie farm ... about thirty miles southeast of Springfield and 3 miles southwest of the railroad village of Stonington," where his family raised oats and corn on approximately eighty acres. The landscape there was dominated by "several species of tall prairie grasses the seed stalks of which often grew taller than a man," though "practically all of this prairie land [had] soon become so valuable for growing corn and other crops that it was quickly plowed and put into cultivation." The Connecticut River Valley offered Shamel the chance to immerse himself in a new kind of agriculture and a new environment.¹³⁵

Though he was very young, Shamel had already impressed many of his colleagues, and expectations of him ran high at the USDA. He had spent several years working on scientific projects at the agricultural experiment station of the University of Illinois, where he had worked under Professor Cyril Hopkins and had become involved in several of the most promising plant breeding experiments being undertaken at the time. The Secretary of Agriculture, who

¹³³ A.D. Shamel, "Tobacco breeding," *Proceedings of the American Breeders' Association* VI (1911): 268-269.

¹³⁴ *Ibid.*, 268.

¹³⁵ Autobiographical manuscript of Charles H. Shamel, CHS-UIA, Box 2, Folder: Autobiography, pts 1-2.

had discovered and hired him in 1902 to work for the Bureau of Plant Industry, thought Shamel had “the practical side of corn down pretty fine, and probably [knew] more about [it] than anybody else living.” For his part, Shamel was ecstatic about the possibilities working for the Department of Agriculture might open to him: “It is the thing,” he wrote to his brother, “I have been looking for all this time. I have the greatest opportunity of any young man in the U.S.”¹³⁶

With Floyd’s guidance, Shamel now spent long hours under the tents bringing the discriminating eye that he had so long honed on corn to bear on the tall thin stems and broad green leaves of the tobacco plant. Among the Sumatra, he counted and numbered eleven distinct physical types, only one of which produced a leaf that resembled the Indonesian leaf the cigar manufacturers sought. Of the 50,000 or so plants growing near Tariffville from Sumatran seed under shade, he estimated that only around 2,500, or about 5% of the total, were of this type. Shamel also strolled through some acreage shade-grown from new seed imported from Cuba, and he divided the plants he found there into five definite types, only two of which appeared to be of any real value to the cigar industry.¹³⁷

This diversity in shapes and textures among the plants, Shamel reckoned, had doomed the ’02 venture; it was “the cultivation of such a mixed and variable crop [that had] entailed a great loss to the growers on account of the small proportion of high-priced tobacco obtained and the increased cost of sorting this irregular product.” If the shade-grown investment were going to be saved, what was required, and quickly, was morphological uniformity among the plants. Shamel estimated that if this could be brought about, “the yield and value of the tobacco crop would be greatly increased and the expense of handling the crop would be reduced, so that the profit to the grower would be at least double that obtained at the present time.” The task at hand was to turn every Sumatra plant in Connecticut into a Type No. 3 and every Cuban plant into a Type No. 13.¹³⁸

Into the fields spread the small staff of researchers that Shamel had brought up from Washington, surveying the narrow crop rows for individual specimens that matched the desired types. By the end of that summer, they had located twenty-eight Type No. 3 Sumatra plants and thirty-two Type No. 13 Cubans.

Insects swarmed in the hot late summer air, and the frequent strong windstorms of the valley spread pollen across great distances; to keep his prize tobacco types from being fertilized by these wayward pollinators, Shamel

¹³⁶ Frank William Scott, *The semi-centennial alumni record of the University of Illinois* (Urbana-Champaign: University of Illinois Press, 1918), 114-115. James Wilson to Beverly Galloway, September 12, 1902, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1904-1906. A.D. Shamel to C.H. Shamel, date unknown, CHS-UIA, Box 2, Folder: 1900, 1902, 1904, 1908, etc.

¹³⁷ On the percentage of the 1902 crop that produced a leaf resembling the Sumatra, see U.S. Department of Agriculture, *Yearbook of the United States Department of Agriculture. 1904*. (Washington, DC: Government Printing Office, 1905), 438. For an estimate of the number of plants growing on Marcus Floyd’s 40 acres in 1903, see U.S. Department of Agriculture, *Yearbook of the United States Department of Agriculture. 1906*. (Washington, DC: Government Printing Office, 1907), 390. USDA, *Yearbook 1906*, 392.

¹³⁸ USDA, *Yearbook 1904*, 438. U.S. Department of Agriculture, *Yearbook of the United States Department of Agriculture. 1905* (Washington, DC: Government Printing Office, 1906), 378.

obtained common manila bags from local hardware and grocery stores and covered the flowers at the tops of the chosen tall plants' slender stalks with them. This simple bagging method, easily and inexpensively replicated by any grower on any plot, virtually guaranteed self-fertilization, and in the next summer, after one-hundred seeds from each of these bagged plants were grown in test plots, Shamel could write to Beverly Galloway, the chief of the Bureau of Plant Industry, of the remarkable transformation of affairs in the Connecticut Valley:

While I was thoroughly interested and believed in our principles of breeding which we inaugurated and carried out in tobacco, I was unprepared for the success of our work as shown in our fields. The selections to type which I made last fall have come almost absolutely true. The individuality of the parent plants in each type are marked, and in the progeny from each parent there is almost absolute uniformity of type, the growers here with whom I have been associated and who I have allowed to inspect our selections, have been astonished and gratified beyond measure with our success. In my experience with the breeding of corn, and later observations of experiments with cotton and other crops, I have never seen results so striking.

Galloway, pleased to hear that his agency was succeeding where the Bureau of Soils had failed, passed the letter on to the Secretary of Agriculture, who "expressed great interest in it and gratification at Mr. Shamel's good work under your direction."¹³⁹

Secretary Wilson depended critically on scientific workers like Shamel to carry out the department's mission. Though he was a member of the President's cabinet and was closely involved in the political work of the federal government, the Secretary of Agriculture actually had remarkably little control over what went on inside the Department of Agriculture. Real power within the department was parceled out into its many bureaus, and these received their mandates and their funding directly from Congress. Each was organized around a specific set of agricultural problems, and its scientists pursued research designed specifically to solve those problems. For both Galloway as chief of the Bureau of Plant Industry and Wilson as Secretary of Agriculture, then, Shamel's work was of considerable importance.¹⁴⁰

In flower-bagging, Shamel now had a reliable means for reproducing varietal types, and he soon realized that the method also provided him greatly-expanded powers for manipulating the physical characteristics of tobacco plants. Numerous useful traits could be transmitted to future generations through controlled self-fertilization. The number of leaves on a Sumatra plant, for example, varied from four to forty, and through bagging it became possible to

¹³⁹ USDA, *Yearbook 1904*, 448. A.D. Shamel to Beverly Galloway, July 16, 1904, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1904-1906. R.M. Reese to Beverly Galloway, undated 1904, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1904-1906.

¹⁴⁰ A. Hunter Dupree, *Science in the federal government: A history of policies and activities* (Baltimore: Johns Hopkins University Press, 1986), 157-161.

produce seed that would grow only plants with a large number of leaves. If “this method of selection is carefully pursued,” Shamel prophesied, “the average number of leaves in the tobacco crop of the country can doubtless be greatly increased, so that the yield will be correspondingly increased.” Leaf shape and size seemed to be amenable to selection as well, which might allow growers to adapt their products to fluctuating industrial demands. Virtually any characteristic of the plants, however minute, seemed to be open to modification. One individual plant under Floyd’s tents “was observed which ripened several days before the general crop was ready to be primed,” and the plants sown from its self-fertilized seed “were ready to be harvested about two weeks earlier than the remainder of the field of the same variety.” The nicotine content of the plants ranged from 0.85% to 3.0%, and Shamel produced some results that suggested it might be possible to create a low-nicotine cigar that retained the flavor, burn, and aroma that the smoker sought. One of the Sumatra plants identified during the summer of 1903 and its descendents in 1904 showed a much greater ability to stave off root disease than its compatriots, sparking Shamel to predict that “the development of disease-resistant strains of tobacco will probably become one of the most important features of tobacco breeding.”¹⁴¹

For all its potential, however, the bagging method did have one significant limitation: regardless of how long a breeder worked at his or her creation, a self-fertilized botanical line could only reproduce traits that had either been present in the original parent plant or had arisen through an unpredictable, highly unlikely spontaneous mutation. Self-fertilization might be used to create disease-resistant or high-yield seed, but it could only instill disease-resistance and high-yielding capacity in the same strain if that self-fertilized line originated from a plant that carried both these characteristics from the beginning. Locating the super individuals who had multiple favorable traits would be exceedingly costly in terms of departmental labor, so Shamel enlisted the support of some of the larger growers in an exploratory hybridization project. Down in Hockanum, just outside Hartford, he crossed some prime local broadleaf plants on Norman S. Brewer’s farm with some of the best imported Cuban plants he had so far identified. His hope was to produce a test field of plants with the “size and shape of leaves, grain, and texture” of the imports and the “habit of growth, adaptability to Connecticut Valley conditions, [and] burn” of the vigorous broadleaf. From this field, he could select the individuals that best expressed the favorable traits of both varieties while minimizing weaknesses, and then begin submitting these individuals to a regime of self-fertilization under bags. Varietal hybridization made it easier to find a good candidate for future self-fertilization, and after the technique demonstrated promise in Hockanum Shamel headed north to Granby near the Massachusetts border to attempt a similar crossing between the imported Sumatra and local Havana varieties.¹⁴²

Under Shamel’s direction, self-fertilization became in the Connecticut Valley a simple, inexpensive, widely-practiced, and frightfully effective technique for turning the botanical world itself into an agent of the U.S.

¹⁴¹ USDA, *Yearbook 1904*, 444-446. United States Department of Agriculture, *Annual reports of the Department of Agriculture, for the year ended June 30, 1908* (Washington, DC: Government Printing Office, 1909), 344. USDA, *Yearbook 1904*, 450.

¹⁴² USDA, *Yearbook 1906*, 394, 396.

Department of Agriculture. One healthy tobacco plant might furnish between 500,000 and 700,000 seeds in a single growing season, so even with the limited facilities available to the bureau near Tariffville Shamel could supply enough self-fertile seed for the entire valley. By the end of 1905, just two years after his arrival, the Bureau of Plant Industry was prepared to distribute four new tobacco varieties to growers. Two of them, the Uncle Sam Sumatra and the Hazlewood Cuban, were simple modifications of the plants growers had imported in 1902. These were, however, taller, finer, and had larger leaves than their recent ancestors. The other two, the Brewer Hybrid from Hockanum and the Cooley Hybrid from Granby, were descendants of Shamel's early experiments with hybridization. They were designed to produce high yields, up to 1,800 pounds per acre, in the climatic conditions specific to the Connecticut River.¹⁴³

Shade-grown tobacco, which had nearly disappeared from New England in 1903 and 1904, quite suddenly exploded in popularity in 1905. White tents sprung up once again across the landscape. Though it had been "very dry, in fact the most unfavorable season, according to report, since 1897," Shamel reported back to Washington that "the growers have overwhelmed us with work, bagging plants and asking us to attend town meetings." This was a valley that looked quite different than the one from which the Bureau of Soils had so recently fled in disgrace:

There has never been such interest evinced by the growers about any line of tobacco experiments, as is being shown in the results of our work ... [T]he growers are adopting our methods of seed separation and seed selection ... The local papers have presented our work fairly and assisted in bringing our work before every grower. The manufacturers and buyers are deeply interested in this work and it has opened a new field to them. The young men are getting interested and working in the field observing the plants in a way they had not thought about heretofore.

Congressman Henry, who sat on the House Committee on Agriculture, once more became a strong advocate of the program. By 1906, the Bureau of Plant Industry had received 226 requests for improved and hybrid seed from growers in the Connecticut Valley.¹⁴⁴

Neither Shamel nor his handiwork remained exclusively in New England. As the bureau's program in Tariffville wound down, Galloway frequently summoned Shamel back to Washington for advice or dispatched him to some other farming region in need of assistance. One of his annual obligations became

¹⁴³ A.D. Shamel, "The improvement of cigar-wrapper tobacco," *Proceedings of the American Breeders' Association* I (1905): 118. For a detailed description of each of these four varieties, see USDA, *Yearbook 1906*, 389-397.

¹⁴⁴ A.D. Shamel to Beverly Galloway, July 26, 1905, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1905. A.D. Shamel to Beverly Galloway, August 4, 1905, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1905. House Committee on Agriculture, *Hearings before the Committee on Agriculture of the Hon. Secretary of Agriculture and Chiefs of Bureaus and Divisions of the Department of Agriculture on the estimates of appropriations for the Department of Agriculture for the fiscal year ending June 30, 1907*, 59th Cong., 1st sess., 1906, 145.

working with the bureau's Office of Seed Distribution to decide what tobacco seed to acquire. Each year, the Bureau of Plant Industry made free seed available to the public, and because members of the public obtained the seed by writing to their Congressmen, Galloway recognized the political gains that might be won by offering seed of special quality and widely publicizing the annual distribution. As soon as the new Connecticut varieties could be produced on a wide scale, the Office of Seed Distribution began disseminating them instead of purchasing more common commercial seed. In 1906, Galloway promised members of Congress that "[t]he seed of the improved varieties of tobacco which our experts have personally selected during the past year will be sent to any of your constituents who apply for it, and our experts will take pains to select in each case the type of tobacco most desirable ... in each locality"; he instructed Shamel, in his official dealings, to "emphasize the fact that [this] is high grade seed, secured by careful selection work." In 1908 alone, the office mailed out over 10,000 seed packets to tobacco farmers around the United States, and filled special, specific requests for around 300 larger growers.¹⁴⁵

When Shamel departed Connecticut, even for short trips, it was with great reluctance. He had become absorbed in the early mornings in the fields, the close daily attachment to his tobacco plants, the workday that did not end until the setting of the sun. Galloway at one point suggested that Shamel might be overdoing it, that he was too vested in his work; his reply: "I do not want to work under too great strain as you suggest but I do want to be intensely busy all of the time. I could not be satisfied any other way." On the occasions when he did leave the valley, like in the winters when the fields were barren, he became an evangelist of close inbreeding, an apostle of heavy self-fertilization. He spoke to students at agricultural colleges, farmers in town hall meetings, tourists at open exhibitions, breeders at shows, about the selection methods he'd implemented for the Department of Agriculture. One February he hopped the tobacco train as it left Richmond and picked up 500 copies of an article he'd written entitled "Improvement of tobacco by breeding and selection" from a rendezvous point in Danville; at each short stop throughout rural Virginia, he gave his spiel about the merits of the bagging method and spread his literature to the curious crowds of tobacco cultivators.¹⁴⁶

The breeders, he discovered, were often skeptical of the technique. Many were convinced that continual self-fertilization would lead over time to a decline in vigor, and while "[t]he benefits to be derived from crossbreeding in the production of new races or varieties [were] well understood," the "use of

¹⁴⁵ Beverly Galloway to members of Congress, November 6, 1906, USDA-BPI, Division of Tobacco and Plant Nutrition Investigations: General Correspondence, 1901-1926, Folder 2: A-B-C-D-E Chief BPI, 1906-1914. Beverly Galloway to A.D. Shamel, November 13, 1906, USDA-BPI, Division of Tobacco and Plant Nutrition Investigations: General Correspondence, 1901-1926, Folder 2: A-B-C-D-E Chief BPI, 1906-1914. USDA, *Annual reports 1908*, 348-349.

¹⁴⁶ A.D. Shamel to Beverly Galloway, September 4, 1907, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1907. Beverly Galloway to A.D. Shamel, February 16, 1906, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1906. Beverly Galloway to George Hill, February 19, 1906, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1906.

inbreeding in the fixation of type and the propagation of desirable characters [was] not fully appreciated by plant breeders.” Shamel argued instead that the impact of inbreeding needed to be understood on a species-by-species basis; some plants, like tobacco, constantly self-fertilized in nature, and there was no need to fear inbreeding them, but others, like corn, required cross-fertilization to remain vital. On the topic of inbreeding, Shamel thought the great Charles Darwin had overstated his case in *The effects of cross and self fertilization in the vegetable kingdom*. He frequently cited passages from Darwin’s “classical work” in his publications, but he thought the inbred tobacco strains in Connecticut were living, visual proof that self-fertilization need not always be injurious. Perhaps unsurprisingly, Shamel’s favorite character from *The effects of cross and self fertilization* was Hero the morning glory, the heartily-inbred plant that could outperform the cross-breeds. Hero gave him hope that even “[i]n the case of corn, as well as other cross-fertilized crops, it is not beyond the limits of possibility that by continuous inbreeding an individual plant adapted for self-fertilization might be found, which would revolutionize and greatly simplify the work of corn breeding.”¹⁴⁷

When local conditions seemed right, Galloway would also dispatch Shamel by train to tobacco-growing regions where it seemed it might be possible to replicate the Connecticut project. In 1905, he traveled south to Florida with some of the first Uncle Sam Sumatra he had produced, and he found that the shade-growers there were eager to test it out. The variety flourished, growing even more vigorously than it had in New England, and when Shamel returned the following year he discovered that U.S. Sumatra was “being grown extensively on several plantations,” and had yielded between 300 and 500 more pounds per acre than the traditional Florida Sumatra seed. “I tell you[,] Doctor,” he wrote back to Galloway, “it makes one feel good to see such results.” Shamel’s improved inbred variety was replacing the plant population from which it had originally sprung: “Several of the largest planters are bagging more than 1000 plants of this type for future planting. Little else will be planted next season.”¹⁴⁸

The Florida fields, through Marcus Floyd, had been connected to the Connecticut experiment from its start, but Shamel also began to build new links to other sites. From Guthrie, Kentucky, on the Tennessee border at the heart of one of the largest-tobacco producing regions in the United States, Shamel reported that “[t]he folks here are so worked up over the importance of our work, and naturally take so kindly to the practicality of our ideas and suggestions, that we will receive hearty and earnest support.” Shamel visited the state agricultural experiment station and identified a researcher who was particularly enthusiastic about the project: “He will probably be willing to follow the plan we are now carrying out in Connecticut.” By 1908, the Bureau of Plant Industry was collaborating in tobacco selection programs with the state experiment stations in Ohio, Virginia, and Maryland, along with Connecticut

¹⁴⁷ USDA, *Yearbook 1905*, 378, 384, 391.

¹⁴⁸ A.D. Shamel to Beverly Galloway, July 10, 1906, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1906.

and Kentucky, and was working directly with growers in Florida, Georgia, Texas, Alabama, and upstate New York.¹⁴⁹

Galloway saw no reason for Shamel to stop at tobacco. With the resources of the Bureau at his command, he thought it would be prudent to introduce Shamel's techniques to other commercial crops. Here, however, he encountered a peculiar resistance. Shamel himself did not want to leave tobacco. The young breeder was happy to try out his method on hairy vetch, a crop in the Connecticut Valley that might be grown in rotation with tobacco to restore nitrogen to the soil, but he moved only slowly and reluctantly when Galloway tried to send him to Massachusetts to breed rust-resistant asparagus. Shamel had developed a powerful psychic investment in his plants. "Our Cooley and Brewer tobacco are simply the finest tobacco ever grown in the valley," he wrote to Galloway at the end of one growing season, "[and] [l]ast Spring, when I was sick, it seemed to matter little to me whether I lived or died, but I am now thankful that I lived to see the fruition of my hopes. This may seem extravagant language to you, but I am terribly in earnest about it." He called the Uncle Sam Sumatra growing in Florida "the most beautiful tobacco ever grown here," and described his daily routine in the Connecticut fields as "spending from five o'clock in the morning until dark ... intensely interested in what I found." In one especially detailed letter, he offered Galloway his reflections on what he had learned in the valley:

As I go along I am more and more convinced of the value of trained selection in the improvement of our crops. When we tried to breed plants ... according to some preconceived theory, it frequently happen[ed] that years of work and effort [were] wasted, while all about us to the seeing eye are individual plants in crop, which if saved for seed and carefully propagated may mean increased value of thousands or millions of dollars to the producers. On the other hand if the breeder goes at these problems with fixed ideas, or theories, he is not likely to see anything but what he has in his mind, and miss the many important things that nature has prepared for him. Not only is this true but if he is burdened with an intricate system of note taking ... he is left to spend the important time in this comparatively unimportant part of the work, leaving but little time for the real work, i.e. the study of the plants. Not only is this true, but I am coming to believe that the selection of important individual plants is a matter of intuition, based on an intimate experience and study of the plants or crops.

It was, suggested Shamel, the private dialogue between plant and breeder, a careful inter-species communication, that made improvement possible.¹⁵⁰

¹⁴⁹ A.D. Shamel to Beverly Galloway, July 6, 1905, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1905. A.D. Shamel, "Report of Committee on Breeding Tobacco," *Proceedings of the American Breeders' Association* IV (1908): 246.

¹⁵⁰ A.D. Shamel to Beverly Galloway, August 15, 1907, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1907. A.D. Shamel to Beverly Galloway, June 19, 1907, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding

He was not seeking promotion. He did not hope to climb into the next rank of the department's hierarchy. "Personally what I want," he wrote to Galloway, "is the opportunity to do individual work in the selection of plants, and to study the crops I work with so that I may become an expert judge in a practical way of the plants I select." He made a more personal commitment to the region and its prime industry when in 1908 he married Agnes Fay Brewer, the daughter of the grower in Hockanum after whom the Brewer Hybrid had been named.¹⁵¹

And Galloway, though he frequently called Shamel back to Washington to speak to Congressmen or assist in the seed distribution program, granted his star tobacco breeder a good deal of leeway. He did not have much of a choice. The department had a tremendously difficult time retaining its plant breeders. After a season in which four of his staff, like Marcus Floyd before them, left government employment for better-compensated positions with the large growers, Shamel implored Galloway to be more generous with wages, because "the commercial world in the districts in which our work is being conducted" fully understood the value of a worker who understood selection and "as a consequence, we lose, frequently, our best men who cannot afford, by reason of the low salaries paid them, to remain in work of an investigative nature." Despite the success the bureau had had in Connecticut, Galloway thought this tight labor situation would make it difficult to expand the tobacco breeding program further: "Increased funds would help us very little, if at all, as [our obstacle] seems to be a dearth of men rather than anything else." In this economic situation, a skilled and experienced employee like Shamel who would stick with the government over a long duration was of the utmost importance to making the project possible; his motivation remained simple: "I am in this work because I love it, and because I feel that I am working under sympathetic and helpful men for whom I have the highest respect and admiration. Otherwise I would have left it long ago."¹⁵²

Between 1903 and 1907, the Bureau of Plant Industry succeeded marvelously where the Bureau of Soils had failed. Shamel and his team made shade-growing in the Connecticut Valley a practical, and profitable, proposition. But what exactly happened during those four years? The bureau later touted those four years as a model of the importance of plant *improvement*. By 1903,

Experiments, 1905-1908, Folder: 1907. A.D. Shamel to Beverly Galloway, June 9, 1907, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1907. A.D. Shamel to Beverly Galloway, June 9, 1907, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1907.

¹⁵¹ A.D. Shamel to Beverly Galloway, September 4, 1907, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1907. Scott, *Alumni record*, 115.

¹⁵² A.D. Shamel to Beverly Galloway, May 28, 1907, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1907. Beverly Galloway to A.D. Shamel, September 3, 1907, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1907. A.D. Shamel to Beverly Galloway, September 4, 1907, USDA-BPI, Division of Cotton and Tobacco Breeding Investigations: Correspondence of A.D. Shamel relating to Tobacco Breeding Experiments, 1905-1908, Folder: 1907.

tobacco producers had developed “highly improved machinery and methods of cultivation ... in order to increase the profits from the crop,” but “[m]ethods of selection [had] not kept pace with the improvements along other lines, and ... [were] essentially the same as those used by the pioneer tobacco growers.” Selection followed by close self-fertilization, the bureau assured farmers, was one more progressive industrial technology, no different than “such practices as the application of \$100 worth of commercial fertilizer per acre, covering the fields with slat or cheese-cloth shade, [or] the installation of extensive systems of irrigation.”¹⁵³

Intensive breeding might *improve* a tobacco population in innumerable ways. It might lead to disease resistance, reduced or boosted nicotine content, a change in leaf shape and size, or the production of a more substantial root system to decrease topsoil erosion. The Bureau of Plant Industry, though, had an openly-stated, quite narrow definition of what it considered *improvement*: “It [should cost] no more to grow an improved variety of tobacco giving a higher yield of better quality than to grow unimproved and irregular varieties.” What the bureau was trying to create was a more efficient plant. Shamel’s new strains were each subjected to careful field-testing before they would be made available to the growers:

The results of these experiments, to be of value, must show the profit under practical conditions of field culture of the new varieties compared with the established varieties; or, in sections where tobacco has not been grown, the profit that may be expected by growers under normal conditions. A record of the actual cost of all of the operations in the production and handling of the crop, the yield, and the selling value of the product is necessary in order to determine the comparative or actual value of the new varieties.

The ultimate test of these new varieties was their profitability under current market conditions.¹⁵⁴

In the name of *improvement*, Archibald Shamel and his team toiled long hours for several hot summers in the humid fields, making tobacco more lucrative for the largest producers, bringing Marcus Floyd’s vision of a plantation system of agriculture in New England closer to fruition with each careful selection. “[T]he most important problem for the practical consideration of the plant breeder,” Shamel declared, is “[t]he production of uniform races of crops adapted to special purposes,” and the main purpose in this case was to shorten the distance between the grower and the manufacturer. Eliminated from the fields, first in Connecticut but soon in Florida and the other regions where USDA seed dispersed, were the “many strange types of tobacco,” the unpredictable “branching or freak plants that appeared,” those which produced leaves that were “small and of little or no value for any purpose.” The tobacco seed that resulted was a clear step-up, from the perspective of the profit-driven producers, but the deeper changes that accompanied the transition to improved seed locked the region into a closer dependence on the USDA and, later,

¹⁵³ USDA, *Yearbook 1904*, 436-437.

¹⁵⁴ USDA, *Yearbook 1906*, 402, 404.

commercial seed producers, limiting the possible ends and accompanying systems of agriculture to which the valley's crop might be adapted in the future. Called before Congress to justify the increased appropriations headed to Connecticut tobacco breeding, the most impressive observation that the department's advocate could make of Shamel's work was that "[i]f you plant 500 [of his] seeds you get 500 plants so nearly alike that you could not tell them apart," and this won the admiring support of most of the Representatives.¹⁵⁵

IV

Ten miles north of Capitol Hill on an experimental farm in Bethesda, Maryland, George McCullough Rommel had discovered by 1907 that it was far more challenging to attain uniformity in livestock than tobacco. An expert in husbandry employed by the Bureau of Animal Industry, something like Archibald Shamel's zoological equivalent, Rommel had persistently pushed his agency for years to undertake a venture like Shamel's with horses, sheep, cattle, chickens, or any other economically-useful animals available. He had made significant headway: Congressional appropriations were flowing in his direction, state agricultural stations were eager to collaborate, the Bureau's 50-acre experimental dairying and husbandry farm had acquired another 60 adjacent acres, and an impressive brick two-story turreted Neo-Renaissance laboratory and administrative headquarters was under construction on the farm. But he had no immediate prospect of remaking the animal production industries (and consequently increasing their dependence on the Department of Agriculture) as Shamel had in the Connecticut River Valley. Animals reproduced slowly, and were maintained at great expense, and no matter how hard a bureau tried they could not be made to self-fertilize with cheap burlap sacks. Rommel needed a different, longer-term game plan to do for the Bureau of Animal Industry what Shamel had done for the Bureau of Plant Industry.¹⁵⁶

One significant obstacle that stood in his way was the deep skepticism among many in the livestock business about the practice of close inbreeding. Bakewell had left behind an ambiguous legacy. His advocates and imitators, like the Colling Brothers or Thomas Bates, continued throughout the nineteenth century to produce new breeds of sheep and cattle, many of which attracted the same celebrity and high value on the market as the Dishley rams and bulls had. Other successful commercial breeders, however, like Amos Cruickshank in Aberdeen, eschewed inbreeding and profitably practiced their trade without resorting to the method. What inbreeding wrought remained an open, and controversial, question in stock-raising circles. When American wool growers held their annual meeting in Philadelphia in 1881, almost a hundred years after the founding of the Dishley Society, one visitor observed that among the

¹⁵⁵ USDA, *Yearbook 1905*, 378. A.D. Shamel, "Tobacco breeding," *Proceedings of the American Breeders' Association II* (1906): 19. House Committee on Agriculture, *Hearings before the Committee on Agriculture ... appropriations ... 1907*, 142.

¹⁵⁶ U.G. Houck, *The Bureau of Animal Industry of the United States Department of Agriculture: Its establishment, achievement and current activities* (Washington, D.C.: Hayworth Printing Company, 1924), 49. For a detailed architectural description of the laboratory, see Alden K. Watts, "Bureau of Animal Industry Building," Inventory no. 35-119, Maryland Historical Trust, Inventory of Historical Properties, 2008.

conventioneers “no question caused so much earnest inquiry as that of the effect of consanguineous alliances, or as it is called by English and American writers, ‘in-and-in breeding.’”¹⁵⁷

The idea that Bakewell had done *something* spectacular at Dishley was never questioned, and even his most severe critics granted that some degree of inbreeding was unavoidable in the early stages of creating a new type. There was something, though, about Bakewell’s creations that made many farmers uncomfortable. Surveying the sheep husbandry literature in 1880, the Tennessee Commissioner of Agriculture discussed why so many continued to oppose close inbreeding:

Many object to it from religious or moral considerations. Others contend that this method tends to weaken the constitution and debilitate the sheep, and the general appearance of the Leicesters originated by Mr. Bakewell, of England, by in-and-in breeding tends to confirm this objection. The small head, prominent, glassy eye, small bones, we say attenuated, their delicate skin, and general tendency to scrofulous diseases, would seem to be the result of too close and too long continued in-breeding.

Well into the twentieth century, there remained concerns among livestock breeders about the long-term impact of a system of in-and-in breeding.¹⁵⁸

George Rommel, though, had few qualms about close inbreeding, so long as the method was employed carefully and selectively. When he arrived in the capital to work for the USDA in July of 1901, twenty-five years old, a small-town Iowan by birth and rearing, his personal heroes were the selective breeders of the Agricultural Revolution; “[o]ut of the dark ages of ignorance and of the scrub,” he would soon write in one of the bureau’s bulletins, “by leaps and bounds, using what material he had at hand and molding it to his will, the English farmer developed the modern breeds of cattle.” He reached D.C. after a brief stint as the director of an experimental station in Walla Walla, Washington, that belonged to a subsidiary of the Union Pacific; the railroad, which owned huge amounts of grazing land along its many lines, was trying to produce new range grasses at the station through selection and hybridization that could be used to replenish land decimated by over-stocking. From that short experience, he gained a faith in the power of breeders to alter organisms, and he trusted that he would be able in time to wield this same power for the Department of Agriculture. In his early praise of the doyen of Dishley, we can see reflections of the new role the young man envisioned for himself at the Bureau of Animal Industry:

Around the name of Robert Bakewell those of all great improvers of live stock group themselves, and from the lessons that he taught by example, if not by precept, every breeder learns the fundamentals of his art. Previous to him we find a class of cattle of no uniformity, of little value as

¹⁵⁷ On the Colling Brothers, Cruickshank, and the breeding of shorthorn cattle in the nineteenth century, see Derry, *Bred for perfection*, 17-47. John L. Hayes, *Consanguinity in breeding* (Cambridge: Cambridge University Press, 1882), 3.

¹⁵⁸ J.B. Killebrew, *Sheep husbandry. A work prepared for the farmers of Tennessee* (Nashville: Tavel, Eastman & Howell, 1880), 63.

high-class meat producers, late maturing, and without quality. After his time, we see an era of wonderful growth and improvement. A man of marked attainments, striking out for himself, he achieved results, by close study of anatomical structure and heredity, that changed breeding methods the world over. He was the first man to practice systematic inbreeding ... He was a constant student – a great man – learning new facts by means of experiment and comparison, always keeping in view the most economical utilization of every force and product of the farm. Though he kept his methods to himself to a great extent, the great fact of them all – that the surest way to improve stock is by the use of inbreeding in the hands of a master – serves to perpetuate his name.

Rommel called Bakewell's experiments "the Awakening," and he prophesied a new awakening just around the bend.¹⁵⁹

What would usher in this new era, he promised in a circular distributed by the Department of Agriculture, was a new breeding program that could be effectively administered by the Bureau of Animal Industry. This would be a cooperative venture of "great magnitude, in which both the laboratory investigator – the student of pure science – and the animal husbandry worker in the agricultural colleges – the student of applied science" might join to tackle the "abstract problems of heredity and their practical application to the animal industry." The Bureau would need to construct new laboratories, with "every facility for the utmost freedom of study," where "breeding experiments [might] be carried on with the smaller animals which breed rapidly and are highly prolific." These alone, however, would not be sufficient; the plan would also require "breeding farms in the same localities as these laboratories, and operated in connection with them where the results obtained could be tested with larger animals under field conditions." The central laboratories would be "under the charge of men whose positions are secure and whose ambitions will lead them to make this work a life study," while the experimental farms would be "under the charge of men thoroughly trained in animal husbandry," who would "be able to go into a strong show ring if necessary and fill creditably the position of judges." The interplay between these laboratories, steeped in the latest scientific understanding of heredity, and these breeding farms, "in touch with the practical side of the industry," would produce the knowledge necessary to improve American livestock.¹⁶⁰

Throughout 1903 and early 1904, Rommel attended stock exhibitions, spoke at agricultural meetings, and visited state experiment stations to generate support for his vision. To the skeptics, and Rommel could expect many of them, since scientific studies of animal breeding had not produced many practical results, he pointed to what had been going on in the botanical world with the

¹⁵⁹ George M. Rommel, "American breeds of beef cattle with remarks on pedigrees," United States Department of Agriculture, Bureau of Animal Industry bulletin 34 (1902): 9-10. Houck, *The Bureau of Animal Industry*, 218. National Live Stock Association, *Proceedings of the fourth annual convention of the National Live Stock Association* (Denver: The Smith-Brooks Printing Company, 1901), 349-352.

¹⁶⁰ United States Department of Agriculture, Bureau of Animal Industry, *Twentieth annual report of the Bureau of Animal Industry for the year 1903* (Washington, D.C.: Government Printing Office, 1904), 322. George Rommel's proposals closely parallel those of the American Breeders' Association, which held its first meeting in 1903. The American Breeders' Association will be addressed in Chapter Three.

support of the Bureau of Plant Industry: “The most striking features of agricultural progress at the present time are the intense study which agriculturalists, botanists, and horticulturalists are devoting to the subject of systematic and practical plant breeding and the very remarkable results which have been reached.” Plant breeders had “increased directly the production of agricultural wealth,” and there was good reason to expect that their new methods, when adapted to animals, might produce similar results. One need only attend a livestock show to note “the variety of types – the striking lack of uniformity – among the exhibits of the same breed that may be seen in any show ring of importance,” and eliminating this diversity and reproducing a single type was precisely where plant breeders were becoming expert.¹⁶¹

At the same moment that Rommel was extolling recent advances in plant breeding to the stock industry, he reported to his own bureau chief, Daniel Salmon, that the stock industry was urging him to create a new research program: “Breeders, Experiment Station officers, stock yards officials, and herd book officers are among those who urge the importance of this step. The demand is almost universal.” Without mentioning where that demand might be coming from, he suggested that the bureau’s prime constituents were disappointed in the agency: “During my travel throughout the country ... I have been repeatedly approached by men who stand high in the live stock business of the country and asked why the Bureau of Animal Industry was not paying more attention to the study of the production of domestic animals ... They even draw comparisons between the field work done by the Bureau of Plant Industry and that of this Bureau.” Rommel was pushing from the lower ranks of the bureau’s hierarchy to convince those at the top to act, and an organized and vocal livestock industry might provide him the leverage he needed to succeed.¹⁶²

Despite Rommel’s enthusiasm, Daniel Salmon moved slowly and reluctantly. Traditionally, breeding had not been a central concern of the Bureau of Animal Industry. When the agency was created by Congress in 1884, it was an outgrowth of the Department of Agriculture’s Veterinary Division, a small operation overseen by Salmon that consisted of some simple laboratory space, a couple of stables for holding sick livestock, and an autopsy room. Its first task was to eradicate contagious pleuropneumonia, a bacterial disease that had stricken eastern cattle herds and threatened to spread west. When its reach was extended by significant Congressional funding, the agency accomplished this task surprisingly quickly, and then targeted other infectious diseases, such as hog cholera and the dreaded Texas tick fever, that could be spread from state-to-state through the trade in livestock. Under Salmon’s direction, the Bureau of Animal Industry earned a reputation as an effective, competent public veterinary health agency, and it gained wide political support within the livestock industry that benefited from these services.¹⁶³

¹⁶¹ *Ibid.*, 316-317.

¹⁶² George Rommel to D.E. Salmon, November 4, 1903, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 1, Folder 20: Annual Reports – Also establishing a Division of Animal Husbandry.

¹⁶³ Houck, *The Bureau of Animal Industry*, 48. On the early history of the Bureau of Animal Industry, see pp. 1-32.

With the mandate to stamp out pleuropneumonia came from Congress a wide suite of powers. Salmon's bureau had the authority to prevent infected livestock from leaving the United States or from being transported by railroad or steamship across state lines, and his agents, who were stationed in every major American port, could charge those who broke the law with a misdemeanor fine of up to \$5,000 or one year of imprisonment. By 1891, the bureau had expanded to include four divisions: one still devoted to veterinary pathology, another responsible for inspecting circulating livestock, a division that governed field investigations, and a unit responsible for quarantining suspected pathogen-bearers. In time, Congress also made the agency responsible for assessing taxes on imported animals according to a tariff schedule that discriminated by lineage and pure-bred degree; the bureau sent livestock pedigrees over to the Treasury Department. By the early twentieth century, the Bureau of Animal Industry had become something of an executive body for the American livestock industry, monitoring its products, guaranteeing their health and quality, and representing it in relations with foreign governments.¹⁶⁴

As the bureau's duties expanded, its simple laboratory on Benning Road, just a quarter of a mile from the northeastern boundary of the District, became increasingly crowded. In July 1894, the facility was placed under the superintendence of Ernest Schroeder, a veterinarian, and three years later he oversaw its relocation to a larger location further north of the city in Bethesda, Maryland. The new Bethesda experimental farm could accommodate more livestock and included a breeding house for small mammals that could produce large quantities of rodents for use in veterinary investigations.¹⁶⁵

The challenge facing George Rommel, then, was to convince Salmon that his veterinary bureau would be well-served by a new program in animal breeding. He assured his boss in November of 1903 that "the problems confronting the stock breeder are such that ample funds and a long series of investigations are necessary to establish facts," and then he began to spread the word throughout the husbandry community that the Bureau was contemplating his new plan. Charles Mills, an organizer and promoter of livestock exhibitions, thought Rommel's proposal would "increase the popularity and usefulness of the Bureau of Animal Industry more than one hundred per cent," and with Rommel's assistance he orchestrated in March 1904 a letter-writing campaign by agricultural leaders to urge the Secretary of Agriculture to create a new animal husbandry division within the bureau.¹⁶⁶

¹⁶⁴ *An act for the establishment of a Bureau of Animal Industry, to prevent the exportation of diseased cattle, and to provide means for the suppression and extirpation of pleuro-pneumonia and other contagious diseases among domestic animals, U.S. Statutes at Large 23 (1884): 31-33.* Houck, *The Bureau of Animal Industry*, xi.

¹⁶⁵ Houck, *The Bureau of Animal Industry*, 48-49.

¹⁶⁶ George Rommel to D.E. Salmon, November 4, 1903, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 1, Folder 20: Annual Reports – Also establishing a Division of Animal Husbandry. Charles F. Mills to George Rommel, December 17, 1903, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 1, Folder 20: Annual Reports – Also establishing a Division of Animal Husbandry. See Charles Mills to James Wilson, February 10, 1904; Andrew Boss to James Wilson, March 5, 1904; Andrew M. Soule to James Wilson, March 7, 1904; W.L. Carlyle to James Wilson, March 8, 1904; E.W. Major to James Wilson, March 9, 1904; Oscar Erf to James Wilson, March 10, 1904; Thomas I. Mairs to James

Rommel didn't get his new division immediately, but when Secretary Wilson wrote back to his petitioners in early April 1904 he informed them that "the conference committee on the agricultural appropriations bill met on Thursday of last week and agreed upon an appropriation of \$25,000.00 for the Bureau of Animal Industry to conduct experiments in animal breeding in cooperation with the State experiment stations." With new opportunities on the horizon, Salmon and Rommel began contacting some of the more prominent names in American agricultural science, both to seek advice about what lines of investigations to open up and to sound out the possibility of cooperative work. What these scientists, most of whom had spent their careers around the state experiment stations, found to be the greatest strength of the bureau was the grand scale of research it might undertake and the possibility of stretching its experimental projects out over multiple decades. Willet Hays in Minnesota thought that "[t]he improvement of breeds, the formation of breeds, and theoretical experiments in breeding and feeding [were] such long time propositions that long tenures of office, long continued efforts close to the work in hand, [were] absolutely necessary," and these were the kinds of experiments a well-funded bureau would be able to undertake. Liberty Hyde Bailey, the founder of Cornell's College of Agriculture, cautioned against working only with small animals "merely because they are inexpensive and breed rapidly"; instead, the bureau should take advantage of its size and longevity and work with "the particular animal that it is desired eventually to improve." He proposed one hypothetical experiment in which a low-grade herd of cows might be studded for multiple generations with a high-quality purebred of known lineage, thorough records being kept each breeding season; this would only "require a farm of ordinary size and a man of good ability to manage the herd and the experiment should continue for at least twenty years." From the University of Missouri College of Agriculture, Frederick Mumford stressed the great difficulty of devising experiments in the "principles and methods" of breeding and offered the USDA a list of projects his station was overseeing; included among them was a study of "the real results of in and in-breeding," with early evidence indicating that "[i]n-breeding does not necessarily result in diminished fecundity nor weakened constitution."¹⁶⁷

To Rommel, this initial appropriation was not to be the final fruit of all his lobbying but just a seed that could grow a larger vine. "It seems to me," he wrote to Salmon, "that it is only a question of time until the work in animal husbandry will be greatly enlarged." The funds should be dispersed, he argued,

Wilson, March 19, 1904; USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 1, Folder 20: Annual Reports – Also establishing a Division of Animal Husbandry.

¹⁶⁷ James Wilson to the Director of the Agricultural Experiment Station of the University of Tennessee, April 6, 1904, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 1, Folder 20: Annual Reports – Also establishing a Division of Animal Husbandry. W.M. Hays to George Rommel, May 3, 1904, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 1, Folder 2: Authorizations. L.H. Bailey to D.E. Salmon, June 13, 1904, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 1, Folder 2: Authorizations. F.B. Mumford to James Wilson, June 27, 1904, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 1, Folder 2: Authorizations.

across the country, roping every large region of the United States into the work of the Bureau of Animal Industry and producing a diverse crop of supporters in the House of Representatives. His new program could improve range sheep in the Rocky Mountains, horses for the prairie, beef cattle and hogs for the Corn Belt; in the South and in New England, it might establish demonstration farms to encourage new local breeding. The cooperating stations should foot the bills for facilities and common labor, while the USDA ought to purchase (and retain rights to) the experimental livestock and appoint and pay the salaries of researchers. By making the researchers government employees, Rommel expected that “a good class of bright young men would be attracted to the work, an esprit [de] corps would be established and the Bureau would have material available to draw upon for advancement.”¹⁶⁸

By and large, Salmon approved of Rommel’s vision. The Congressional appropriation actually called for research in both animal breeding and animal feeding, so the bureau opened up some cooperative work in Alabama and Texas in beef feeding and continued funding a long-standing animal nutrition experiment in Pennsylvania. The bulk of the new money, though, flowed into breeding research, the ultimate justification of which was the increase in “efficiency of the breeds that are now established.” Salmon, echoing Rommel, argued that breeding was a more appropriate topic for the bureau to examine because unlike nutrition “it is not greatly affected by local conditions, and very few investigations have been undertaken by the experiment stations or Department.”¹⁶⁹

Three breeding projects, each devoted to a different animal in a different region of the country, were chosen initially to receive funding from the appropriation. The price of work horses had been increasing steeply for the past decade in the United States, so the bureau purchased eighteen mares and a stallion and shipped them off to the Colorado Agricultural College, where they were to be used as the foundation stock for a new breed of American carriage horses. The best-suited sheep for the American market would have a “profitable carcass, a good clip of wool, and should stand flocking in large numbers”; breeds had been developed with two of the three characteristics, so the bureau teamed up with the Iowa Experiment Station to produce a new breed with all three traits. And in Orono, the Maine Agricultural Experiment Station had begun work with the poultry industry on the “development of [chicken] strains which will lay 200 eggs annually per hen and the study of the amount of floor space required per fowl.” The station had already produced some of these lines, but “when this is done it usually happens that in the succeeding year the egg yield is very greatly diminished, and in some cases the hens have died, apparently from exhaustion.” The bureau hoped its support could make these new strains commercially viable.¹⁷⁰

All these new lines of research, Rommel hoped, justified a new Division of Animal Husbandry, and he made the request in person to the Secretary of Agriculture. “It appears that he does not deem such action as advisable as yet,”

¹⁶⁸ George Rommel to D.E. Salmon, May 10, 1904, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 1, Folder 2: Authorizations.

¹⁶⁹ USDA, *Yearbook 1904*, 527, 538.

¹⁷⁰ *Ibid.*, 528-535.

Rommel told Salmon afterwards, “although he seems to be favorable to it at some future time.” Rommel figured that “the principal reason for holding off at this time was due to the fact that he does not desire to give Congress the impression that he is taking advantage of the new appropriation act, placing scientists in the lump fund, by making wholesale promotions and increases in salary.” If he wanted his wholesale promotions and increases in salary, Rommel needed his organization to produce concrete, practical results that could be pitched to Congress.¹⁷¹

One half of his original plan was now in place; the bureau was linked to experimental farms around the country where agricultural scientists could breed large animals. The other half of his plan, though, which called for laboratories devoted to breeding small mammals in order to determine the general principles of heredity, remained unfunded. Congress had allocated money for cooperative research with state stations, not for a new federal research facility. If the bureau were going to have a central breeding laboratory, Rommel would have to assemble it from elements that the USDA already owned.

He discovered his key component out on the veterinary farm in Bethesda. Each year, lab technician Ernest Schroeder raised around 3,000 guinea pigs for use in medical experimentation. This cavy population had been with the department for almost as long as Daniel Salmon. When Schroeder became the superintendent of the original veterinary farm just outside the District in the summer of 1894, he found 250 to 300 guinea pigs living in cages in the laboratory, and it looked to him as though “some attempts had been made to breed special varieties, such as curly haired guinea pigs, white guinea pigs with black-smudged muzzles, long-haired guinea pigs, etc.” In 1895, Schroeder had no interest in creating new breeds; instead, he introduced many purchased “plain, ordinary male guinea pigs” into the population in order to make “of the breeding pens a strict business project, with no other purpose in mind than the production of a sufficient number of satisfactory animals for the technical work of the bureau.” When the agency acquired its larger farm in Bethesda in 1897, Schroeder packed between 800 and 850 guinea pigs onto a wagon and sent them north. It was a chilly November day, and in the midst of the animals’ journey “a sudden, unexpected, heavy, cold shower of rain occurred, and many of the guinea pigs, though they were in cages and in a covered wagon, got thoroughly wet.” For two weeks afterwards, something vile raged through the population in Bethesda, “a combination of inflammation of the bowels and lungs,” killing between thirty and fifty animals a day; by the time the disease ran its course, only sixty-three remained, “and of them 9 were in such hopeless condition that they were killed.” As late as 1922, Schroeder could still boast that all the bureau’s guinea pigs “trace their ancestry back to the 54 which were left after the disastrous outbreak of disease in the year 1897.”¹⁷²

¹⁷¹ George Rommel to D.E. Salmon, June 14, 1905, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 1, Folder 20: Annual Reports – Also establishing a Division of Animal Husbandry.

¹⁷² U.S. Department of Agriculture, Bureau of Animal Industry, *An outline of the work of the Animal Husbandry Office* (Washington, D.C.: Government Printing Office, 1906), 5. Sewall Wright, “The effects of inbreeding and crossbreeding on guinea pigs,” United States Department of Agriculture bulletin 1090 (November 1922): 3-4.

These closely-related cavies, George Rommel decided, could become miniature stand-ins for Bakewell's livestock at Dishley. Intensive inbreeding rapidly transformed herds and flocks. Understanding how the system of in-and-in breeding worked, and why it so often failed, would significantly enhance the many cooperative ventures the bureau had entered into and open new future possibilities for production. In July 1906, Bureau of Animal Industry Project Number A.H. 13, an investigation of "the effects of close breeding in guinea pigs and other small animals," commenced in Bethesda; its ultimate aim was to "test the results thus obtained with larger domestic animals under farm conditions." E.H. Riley, Rommel's assistant in charge of day-to-day operations, chose "[t]wenty-four females of uniform size and conformation" from the bureau's guinea pig pool, placed them in separate cages, and then introduced a different male to each cage. After this founding generation produced its offspring, Riley henceforward chose the two "best individuals in the litter," one female and one male, and mated them, brother to sister (the surplus guinea pigs of each generation could be conveniently returned to the general stash). A second experiment was begun at the same time, identical in every way to the first except that each pair in the founding generation was obtained from a different dealer, making them less-related to each other than the animals in the first experiment. Rommel predicted that the project, after churning through several inbred generations, would produce usable information by 1910.¹⁷³

And so, in the summer of 1906, the U.S. Department of Agriculture was well on its way to adding a new practice to its storehouse of eco-political armaments. The Bureau of Plant Industry already had its agents, like Archibald Shamel, out in the field transforming crops through self-fertilization, while in Bethesda all the elements were gathering to make George Rommel's national stock breeding vision a reality. Under the banner of *improvement*, these bureaus promised to revitalize agricultural industry and increase the efficiency of producers.

What they would also do in the process is insert themselves tightly into the relationship between manufacturers, producers, and consumers. Improved seeds and superior studs would come from the center, from a bureau plot at an agricultural station or from an experimental farm just outside the capital. The grower struck a Faustian bargain with the USDA; adopting the department's improved varieties meant returning, year-after-year, to the bureau to reproduce one's livelihood. This dependence guaranteed that, as time passed, the bureaus would find vocal support among the growers, influential allies in industry, and reliable guardians in the House of Representatives.

Not all producers, however, were to benefit equally. The programs that the department was initiating would generate crops and stock that were of greatest value to the largest growers and stock raisers. Shamel entertained a belief that his work with self-fertilization would benefit all the farmers of Connecticut, but who his seeds were most useful to were those with sufficient

¹⁷³ "Close Breeding of Animals," A.H. No. 13, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 3, Folder 13: Close Breeding and Selection of Animals (Bethesda, Maryland Station). Wright, "Effects of inbreeding", 5. "Close Breeding of Animals," A.H. No. 13, USDA-BAI, Central Office, General Correspondence of the Division of Animal Husbandry, 1901-1914, Box 3, Folder 13: Close Breeding and Selection of Animals (Bethesda, Maryland Station).

capital to invest in poles, tents, expensive irrigation works, commercial fertilizers, and mass labor. His improved varieties brought to Connecticut the kind of plantation agriculture that Marcus Floyd had observed in the South. George Rommel, first trained at a field station of a major railroad line, for his part, understood fully who his breeding program was aimed at. He made no appeal to a broader public beyond the livestock industry.

What the USDA was catalyzing by 1906 was a thoroughgoing transformation of the American agricultural landscape, the eradication of an elaborate web of local relations that sustained crop and stock diversity and its replacement by a near-clonal monoculture tied to powerful distant experimental centers. In Washington, bureau chiefs received dispatches from agents spread throughout the country and made plans for what the next growing season would look like. In Bethesda, veterinarians watched the guinea pigs, paired brothers and sisters, measured and weighed, filled note cards with numbers. And along the Connecticut River, the wind rustled through row after monotonous row of identical tobacco leaves.

Chapter Three

“The theory of the two studies must inevitably go together”: Inbreeding, industrial agriculture, and the study of evolution in the United States, 1903-1925

In a 1936 entry in *Encyclopaedia sexualis*, sandwiched between one article on “Illicit Relations” and another on the “Incubus and Succubus,” Helen Dean King promised that in the future “[m]any of the ills to which man is at present subject will vanish. Superior and desirable traits will appear in an ever increasing number of individuals and eventually become the heritage of the race.” This projection, with its utopian overtones, differed little from those that had filled the eugenic publications of the past three decades. What was different now, though, was *how* King proposed to make this future a reality: “The race can ... be vastly improved through consanguineous marriages in families in which the members show exceptional mental and physical endowment in ways that are of value to themselves and to the community at large.” Inbreeding, the *bête noire* of the nineteenth century, was now being pitched as a remedy to the social ills of the twentieth.¹⁷⁴

King had, throughout her career, participated enthusiastically in the eugenics movement, pitching her proposal for human improvement to popular journalists as well as the movement’s leaders. Her belief in the positive transformative power of inbreeding, however, had very little to do with her involvement with eugenics. It was, rather, drawn from two important developments within the community of academic biologists: the accumulation of data from a range of breeding experiments using small animals and commercial plant crops, and the interpretation of that data using basic Mendelian genetics.¹⁷⁵

The first of these new experiments was William Castle’s fruit fly inbreeding study, which was completed and published by 1906. “The results of this carefully controlled series of experiments, so contrary to the generally accepted views regarding the effects of inbreeding on fertility,” wrote King, “reopened the whole fundamental aspect of the problem of inbreeding.” Influenced by Castle’s discovery, researchers now initiated breeding experiments on other organisms: Sewall Wright on guinea pigs, George Shull and Edward Murray East on corn, a series of scientists at agricultural experiment stations on poultry, King herself on albino rats. What all of these projects suggested is that intensive inbreeding, though not universally beneficial to a population, can under many circumstances avoid the detrimental effects with which it had traditionally been associated.¹⁷⁶

To explain why inbreeding was sometimes harmful and sometimes not, King turned to Mendelian genetics: “Before Mendel’s discovery of the laws of heredity became known there was no answer to the perplexing problem as to

¹⁷⁴ Victor Robinson, ed. *Encyclopaedia sexualis: A comprehensive encyclopaedia-dictionary of the sexual sciences* (New York: Dingwall-Rock, 1936), 399.

¹⁷⁵ On Helen Dean King and eugenics, see Marilyn Bailey Ogilvie, “Inbreeding, eugenics, and Helen Dean King (1869-1955),” *Journal of the history of biology* 40 (2007): 467-507.

¹⁷⁶ Robinson, *Encyclopaedia sexualis*, 386-387.

why some inbred families were uniformly excellent and others inclined to defectiveness. Now, with the knowledge of the mechanism of inheritance, we are able to interpret the conflicting data." What inbreeding did is concentrate alleles in a given population. Intensive inbreeding as a practice reduced the genetic diversity of a population, making all members of the population in the future more uniform. If the foundational stock of a population possessed undesirable alleles, perhaps initially hidden as recessive traits, inbreeding would concentrate these alleles and the practice would look harmful from the perspective of the breeder. If a population's founders, however, were superior organisms whose genes matched their physical vigor, inbreeding was the most rapid means by which to diffuse those superior genes through a population. Inbreeding could change the frequency of alleles in a population; it did not itself, as a practice, lead to either a decline or improvement of vigor. As King noted, "crossbreeding conceals and inbreeding reveals the true nature of the stock."¹⁷⁷

By 1936, then, Helen Dean King confidently believed that the mysterious effects of inbreeding had been adequately explained, that the "significance of the problem of inbreeding, and its final solution, has come through various long continued experiments on animals and plants which show that the results of inbreeding ... are all explicable according to the laws of heredity as defined by Mendel." Inbreeding was no longer an interesting topic to scientists, at least not to those who wanted to be on the cutting-edge of modern research; it was now a matter to be taken up by the practical breeders, by those who could apply these new truths in new methods. Throughout King's article runs a belief that with the physiological effects of inbreeding effectively explained there was no longer a need to address the topic of inbreeding, that the series of experiments mounted by investigators had ended the discussion. In 1936, though, King still maintained her colonies of albino rats in Philadelphia, and inbred organism populations were proliferating in scientific research communities and in government-sponsored agriculture. Though one question had been solved by Mendelian genetics, there were many questions about inbreeding that were simply not being asked.¹⁷⁸

King's description of how the problem of inbreeding had been solved, systematic experimentation mixed with a Mendelian conception of heredity, concealed the wider context out of which these "experiments" emerged. Some, to be sure, like Castle's bottled fruit flies, were explicitly created as tests to determine what might result from long-practiced matings between close relatives. Most, though, emerged from other projects, not designed explicitly to answer the kind of question that King described as *the* most important problem of inbreeding. These projects, undertaken by stable institutions capable of setting and working toward very long-term goals, were intended not simply to answer questions about heredity and breeding, but rather to bring new organism populations into existence to meet institutional goals. These projects, too, brought new researchers into being, created new experts to tend, study, protect, and speak on behalf of the organisms to which they had become attached.

Consider the career of Helen Dean King. King was introduced to academic biology as a student at Vassar in the early 1890s. In 1895, she was

¹⁷⁷ *Ibid.*, 382, 398.

¹⁷⁸ *Ibid.*, 386.

awarded a scholarship to Bryn Mawr College, out in the Philadelphia suburbs, where she pursued a doctorate and, in 1899, produced a dissertation on reproduction in the common American toad under the supervision of Thomas Hunt Morgan, who was still at that time a decade away from his famous work on *Drosophila* at Columbia. She became a research assistant at Bryn Mawr for five years before moving on to a temporary research position at the University of Pennsylvania. Because opportunities for career advancement for women within academia were limited, King in 1908 left the university and moved to the Wistar Institute, a private biological research institution just off the campus of the University of Pennsylvania, where she “came as a volunteer assistant to pursue her own researches and to aid ... with ... technical work.” In the next several years, King made herself indispensable to the research at the institute and was made an assistant professor of embryology in December 1912.¹⁷⁹

When King arrived at Wistar, the institution was in the midst of a major transition. It had been established in 1892 to house a collection of anatomy and pathology artifacts that Caspar Wistar started gathering in 1808, and as late as 1904 its chief claim to public service was that it attracted about 20,000 visitors per year, “the greater part [of whom] ... were attracted by curiosity to see what is here while a small part came here to study the demonstration cases.” In 1905, however, the Board of Managers elected a new director, Milton J. Greenman, and he took office with a clear goal: to turn the Wistar Institute into “the central anatomical institute of this country. The clearing house of anatomy, so to speak.” Breaking with the institution’s museum tradition, Greenman hired Henry Donaldson, a neurologist who specialized in the anatomy and physiology of the brain, at an annual salary of \$5,000 to steer research. To cover this new expense, Greenman made no new hires for other services at the institute.¹⁸⁰

Adding a respected scientist like Henry Donaldson to the staff was a good start, but Greenman recognized another missing component of his research program: “We shall need vivaria and aquaria in close connection with our laboratories and biological farms, not too far distant, where living forms may be observed under normal conditions. At present there is no anatomical institute in America equipped to meet these inevitable demands.” Initially, he attempted to obtain as diverse a supply of material for his researchers as possible. Donaldson had been working with albino rats at the University of Chicago, and he brought these with him when he moved to Philadelphia. Greenman provided quarters for them, and supplemented them with a breeding population of opossums. To pursue “certain studies on seasonal changes in the nervous system,” the Wistar Institute made arrangements for “Mr. C.C. Worthington of Shawnee-on-Delaware [to] set aside a large piece of swamp on his estate so that he may maintain a colony of frogs there, of which we shall have exclusive control.” Responsibility for maintaining these animal colonies was shouldered by the

¹⁷⁹ Marilyn Bailey Ogilvie, *Women in science: Antiquity through the nineteenth century* (Cambridge: The Massachusetts Institute of Technology Press, 1986), 108. ARD-WIL, 1908, 2. ARD-WIL, 1912, 4.

¹⁸⁰ Bonnie Tocher Clause, “The Wistar rat as a right choice: Establishing mammalian standards and the ideal of a standardized mammal,” *Journal of the history of biology* 26, no. 2 (Summer 1993): 333-341. ARD-WIL, 1904, 18. ARD-WIL, 1905, 1-2, 13.

curator of the museum, but Greenman hoped “that we may soon relieve him of some of this work in order that he may give more attention to the museum.”¹⁸¹

Increasingly, though, Greenman moved away from his plan to provide the Wistar with a range of animal types in order to concentrate on a single organism: the white rat. The white rat, a creature of “known qualities and quantities,” had many advantages over the alternatives: “[i]ts convenient size and intellectual qualities, together with its ability to breed rapidly ... [afford] us the opportunity of observing the transmission of characters through numerous generations.” Donaldson provided the Wistar with its starting population of the rodents, but Greenman added to the colony by purchasing individuals from local dealers. It proved quite difficult to keep the animals alive and, especially, to establish a breeding colony that could replenish its numbers. Greenman enumerated the difficulties to his Board of Managers:

The past year’s experience has taught us several important points; first, that mortality is very high among animals which have reached the period of adolescence, about 150 days; second, that the greater number die of a pulmonary affection about which we know little; and third, the ability to reproduce is seriously interfered with when animals are kept in large numbers together. From one pair kept apart from all other animals with considerable freedom, not even confined in a cage, 64 young were born in less than a year while among 20 pairs isolated in as many cages for nine months, only one pair produced as many as three litters.

With some disappointment, he concluded that the “animal colony seemed at first a comparatively simple proposition but we soon discovered that it was somewhat of a problem to get even a rat to breed rapidly and live happily in its usual environment.”¹⁸²

When Helen Dean King arrived at the Wistar Institute, trained as a morphologist with experience in invertebrate embryology, her official status was as a “technician with the privilege of devoting a portion of her time to research.” She kept up the lines of inquiry she’d been investigating for the past decade, performing experiments in amphibian sex determination, but she now became involved as well in the animal colony, working closely with Greenman, Donaldson, and J.M. Stotsenburg, the museum’s curator, to create a population of rats that could produce an annual surplus to be culled for laboratory work. The team was highly successful. In 1910, after the rats were moved from the institute’s hot third floor to its cooler basement and a new ventilation system was added, the animal colony supplied around 800 live specimens to Wistar researchers. By 1913, the colony produced over 11,000 rats, and 8,000 of them were put to use in experiments. Soon, the project was supplying many more rodents than the Wistar Institute needed.¹⁸³

Rather than slowing down, though, Greenman expanded production. In 1915, experiments at the Wistar laboratories in west Philadelphia consumed

¹⁸¹ ARD-WIL, 1908, 19. Clause, “Wistar rat,” 335-336. ARD-WIL, 1906, 10. ARD-WIL, 1908, 8. ARD-WIL, 1907, 3.

¹⁸² ARD-WIL, 1908, 12. ARD-WIL, 1909, 11.

¹⁸³ ARD-WIL, 1909, 11. ARD-WIL, 1910, 23. Clause, “Wistar rat,” 343.

around 1,100 rodents, while another 5,000 were sold to 21 different scientific institutions around the United States. In 1921, Wistar rats were provided to 59 different laboratories, and by 1927 the Wistar Institute was linked to 120 scientific institutions as a rodent supplier. The income generated from these sales was modest; proceeds rarely covered the expense of operating the production facilities. Instead, what Greenman got from the program was brand recognition; the Wistar Institute, in the 1890s an obscure local museum dedicated to medical curiosities and anatomical monstrosities, was by the 1920s an important hub in the world of biomedical research, linked to established national and international centers of scientific innovation. Like the Bureau of Plant Industry, which had pioneered the use of improved seed to create a space for itself in an emerging economy, the Wistar Institute discovered how useful organism production might be for achieving institutional aims.¹⁸⁴

In 1922, the rats were moved out of the home they'd occupied since 1913, an out-of-use former police station, into a modern new building designed specifically with them in mind. When describing the new structure, "constructed of brick, concrete, steel, and glass," Milton Greenman emphasized that it was "isolated from all other buildings," and *isolation* was a central concept in the new animal colony's design. The cage rooms "must be protected against wild mice and wild rats which may bring parasites and disease," and a "cleaning and sterilizing room is an essential." Greenman chose his building materials because he thought they offered "less harbor for dirt and vermin if properly put together." The whole complex was a miniature Dishley Farm, Milton Greenman a new Robert Bakewell, with inbred lines of white rodents replacing the cattle, sheep, and horses of the English countryside. Like his predecessor, Greenman was meticulous about keeping records, and included in the design of his new building an "office where records and all information regarding the colony may be found. These records ... indicate the various strains of rat on hand, their ages, number, location, and any other information of importance." The key to the Wistar's success in breeding rats for biomedical research was no different than the scientific agriculturalist's.¹⁸⁵

Onto this Bakewellian system, Greenman grafted the philosophy of a fellow Philadelphian: the scientific management methods of Frederick Winslow Taylor. "For some years past," Greenman wrote in 1908, "I have been impressed with the time extravagance and disorder with which we do the very ordinary things pertaining to our work. While struggling with the question, I came upon [Taylor's] work ... touching the fundamental principles of time economy." In order that "the same work [would] be accomplished on an expenditure of from 20% to 30% less money," Greenman redesigned the administration of the institute, initiating new methods like a voucher-check system "whereby about \$100 per year [will be] saved in labor this year and every year hereafter." As Bonnie Clause notes, the "influence of Taylor's principles was evident in Greenman's conceptualization and development of the Wistar rat colony." When he announced in 1909 that "Dr. Stotsenburg has succeeded during the past

¹⁸⁴ Mark A. Suckow, Steven H. Weisbroth, and Craig L. Franklin, eds., *The laboratory rat* (Burlington, MA: Elsevier Academic Press, 2006), 12.

¹⁸⁵ Milton J. Greenman and F. Louise Duhring, *Breeding and care of the albino rat for research purposes*, 2nd ed. (Philadelphia: Wistar Institute of Anatomy and Biology, 1931), 14, 16-17.

year in putting the care of the rat colony on a scientific basis," what he meant by *scientific basis* was that the colony was being run efficiently and producing a quality standardized product. One could hear the echo of this sentiment a year later, in a lecture hall in Boston, when William Castle explained to his audience during a lecture on evolution and heredity that "an age of science" is an age in which "a knowledge of causes in relation to processes, i.e. a scientific knowledge, has shortened and improved practice in quite unexpected ways," eliminating "superfluous steps and roundabout methods."¹⁸⁶

Helen Dean King became interested in inbreeding primarily as a technician and only secondarily as a morphologist. A significant number of the rats being produced in the colony in 1910 and 1911 were unhealthy, and Greenman thought this "group represent[ed] the inbred product with brain weight 10% to 20% below normal," an "animal [that was] no longer useful for research work because of its abnormal brain weight and possibly altered other conditions." Like Archibald Shamel, who thought that Connecticut tobacco growers were wasting their resources producing crops with diverse types, many of which would not be salable, Greenman hoped to eliminate these abnormal rodents to streamline the production process. He could not give up inbreeding – that was too useful a practice for a breeder to forgo – so Shinkishi Hatai, one of the Wistar's scientists, began an inbreeding experiment to "determine primarily whether inbreeding causes a deterioration of the central nervous system." King joined him in the project.¹⁸⁷

In its opening stage, the experiment seemed to provide support for the commonly-held belief that inbreeding leads to degeneration. Hatai and King chose four albino rats from the general stock, two females and two males, and then isolated each mating pair. In the next and subsequent generations, they selected two of the "largest and most vigorous animals" from the previous generation's offspring, creating two distinct inbred lines. Each generation was more disappointing than the one before it. By the sixth, "many females were sterile, and those that did breed produced small litters that contained many stillborn young; most of the animals were undersized; and a number showed malformations, particularly deformed teeth."¹⁸⁸

Many of the rats in the general stock, however, showed symptoms similar to the two inbred lines, so King began investigating the possibility that there was another cause of the physiological changes besides inbreeding. What she discovered was that all the rodents at the Wistar Institute were suffering from malnutrition. The primary diet of the rats was corn and bread soaked in milk, a diet of "too much starch, too little protein." "[P]rompted by economical reasons," Greenman suggested the institute start "the feeding of these animals on garbage freshly collected from nearby restaurants," and this turned out to solve the colony's nutritional (as well as the Wistar's fiscal) problem. The seventh generation of the two inbred lines looked no different from the general population, and for the next six or seven years King carefully maintained and

¹⁸⁶ ARD-WIL, 1908, 2. Clause, "Wistar rat," 342. ARD-WIL, 1909, 9. Castle, *Heredity in relation to evolution*, 3.

¹⁸⁷ ARD-WIL, 1911, 32. ARD-WIL, 1910, 12.

¹⁸⁸ Helen Dean King, "Is inbreeding injurious?," *Eugenics, genetics, and the family I* (1923): 271.

monitored the inbred lines. By 1923, she had collected detailed records on over 7,000 litters and 50,000 rats.¹⁸⁹

King first published the results of these experiments in four installments between May and August of 1919 in the *Journal of experimental zoölogy*, though they might have as easily fit in a magazine of scientific agronomy or an agricultural bulletin. The data was not ambiguous. The inbred lines showed no decline in body weight, fertility, or “constitutional vigor” when compared to the general stock. King could confidently assert that “inbreeding is not necessarily injurious, even when continued for forty generations of brother and sister matings – a period that, assuming three generations to a century, would cover 1300 years of human life.” And, employing a Mendelian lens, King could conclude that “[i]nbreeding invariably brings to light the latent characters that were hidden by outbreeding,” but it “cannot, from its very nature, introduce any new characters into the stock.”¹⁹⁰

II

Inbreeding might not, as King insisted, be “necessarily injurious” to the rat, but that inbreeding in the laboratory *changed* the rat was undeniable. The albino Wistar rat looked and behaved nothing like the wild ancestor, the Norway rat, from which it was descended. While the Norway rat, a common pest in Philadelphia, lived inside the walls of old structures and scrounged garbage cans to survive, raiding homes with disarming boldness, Greenman noted that “[a]lbino rats placed in dark and unfrequented corners of the building become timid and are easily frightened. They do not eat well.” He recommended that breeding technicians play “[r]ecords of certain types of music produced by stringed instruments reproduced by the phonograph,” as the rats “seem to listen intently and click their teeth while the music is being played.” The standardized laboratory rat was a new kind of organism.¹⁹¹

How would the academic biologists respond to this new type of creature? What would they make of the manufacturing process that had brought it into being? Darwin and Wallace had already staked out positions on this very question in the debate they opened in 1858. For Wallace, domesticated organisms were “abnormal, irregular, artificial,” and domestication was not analogous to natural selection. The Wistar rat would have been a product of human ingenuity, not natural forces. For Darwin, methodical selection by scientific breeders was simply the application of natural selection once the human mind had reached a stage at which it could comprehend how nature works. The project to create the standardized albino rat would have been an appropriate model for understanding evolutionary change. Darwin devoted a great deal of effort in the final decades of his career to defending this conceptualization of natural selection through his argument from hybridism, which depended on inbreeding leading to a decline in vigor.¹⁹²

¹⁸⁹ King, *Studies on inbreeding*, 5. ARD-WIL, 1911, 33. King, “Is inbreeding injurious?,” 271.

¹⁹⁰ King, “Is inbreeding injurious?,” 275.

¹⁹¹ Greenman and Duhring, *Breeding and care*, 63, 68.

¹⁹² Darwin and Wallace, “On the tendency,” 61.

We find little discussion of evolution in the writings of Helen Dean King. What we do find, though, is a detailed discussion of Darwin's *The effects of cross and self fertilisation in the vegetable kingdom*, the work in which Darwin thought he had demonstrated decisively through experimental means that inbreeding has damaging physiological consequences. King thought otherwise. The data that Darwin presented, she thought, proved that he "clearly recognized the value of inbreeding in fixing type." He was, however, blinded by "a pronounced prejudice against close inbreeding which prevailed all through the Middle Ages and is held by many livestock breeders even at the present time," and so he "failed to realize that the results of his own work did not prove this assumption, since there was little loss of vigor in his inbred lines after the first generation." King and her contemporaries were producing many lines of inbred plants and animals to disprove Darwin's thesis, and thereby also hobble his argument from hybridism to support evolution by natural selection.¹⁹³

And yet, paradoxically, these new strains would have the opposite effect, bolstering Darwin's evolutionary project at the same time they demolished his work on breeding. Even as the scientific breeders created hearty new inbred strains, Darwin's reputation among both agriculturalists and evolutionary biologists experienced an unexpected renaissance. To understand how this was possible, it's necessary to consider some of the intellectual and institutional transitions through which academic biology was headed during the first decade of the twentieth century.

By the turn of the century, evolution was widely accepted in the biological community, but the Darwinian conception of it, which had dominated into the 1880s, was a minority view. Naturalists could choose from many different, conflicting theories about how evolution takes place. Biologists in diverse fields, ranging from botany to paleontology to psychology, incorporated a hereditary model based on use-inheritance that came, in time, to be associated with the spirit (if not direct inspiration) of Jean-Baptiste Lamarck. The Darwinian emphasis on selection as a prime force in evolution persisted in the work of August Weismann and in the biometrical school, where Karl Pearson and W.F.R. Weldon attempted to show that selection might bring about gradual change in small animal populations. Others, such as Theodor Eimer and Henry Fairfield Osborn, adopted orthogenetic theories of evolution that saw variation as being controlled internally by organisms, thereby placing some agency within plants and animals for directing their own evolution and greatly reducing the power of natural selection to shape speciation. The disagreements among these scientists appeared interminable, and with good reason. As Elihu Gerson notes, these "debates over evolution arose because each attempt to solve an evolutionary problem implied difficult or implausible constraints on the solution of one or more of the other problems. In consequence, any proposed solution to one problem would bring objections from researchers concerned with a different, but overlapping problem." The theoretical landscape promoted intellectual conflict rather than conciliation.¹⁹⁴

¹⁹³ Robinson, *Encyclopaedia sexualis*, 383, 385.

¹⁹⁴ For more detailed discussions of each of these competing ideas, see Bowler, *Evolution*, 246-281. Elihu M. Gerson, "The American system of research: Evolutionary biology, 1890-1950" (PhD diss., University of Chicago, 1998), 156-7.

One of the greatest sources of controversy was how to understand variation, an idea of critical importance to nearly all the evolutionary theories of the period but one that was often poorly defined or understood in vague terms. As Gerson writes,

Some naturalists looked exclusively at inter-specific variation, others confounded inter-specific and individual variation, and still others concentrated on individual variation alone. Scientists did not incorporate the different notions of variation into their work consistently. Similarly, debates often were confounded by incommensurate concepts of variation.

In the final years of the century, understanding variation became an explicit concern of many biologists, and variation among individuals in a population became a new focus.¹⁹⁵

When, then, the work of Gregor Mendel was rediscovered in 1900, several evolutionary biologists were prepared to make use of it. Three scientists, Hugo de Vries in Holland, Carl Correns in Germany, and Erich Tschermak in Austria, each independently came across Mendel's publications while conducting experiments in botanical hybridization. Mendel, an Augustinian monk, had undertaken breeding experiments on pea plants in the middle of the previous century. After analyzing his data, he concluded that each physical trait of the pea plant is controlled by two distinct elements. Some of these elements dominated others while they were in combination, meaning that a pea plant might carry an element for a trait that it did not express in its visible anatomy. Mendel had no interest in evolution, and when he published his results and their interpretation he intended them for an audience of scientific breeders, not naturalists; this was not unusual, for "[b]efore the 1890's," as Gerson has observed, "the subject of individual variability from a biological point of view was primarily the concern of breeders, fanciers, and agricultural scientists." At the turn of the century, though, academic evolutionary biologists like William Bateson, who became a staunch advocate of Mendelism shortly after its rediscovery, were beginning to ask questions traditionally asked by the breeders.¹⁹⁶

When Bateson showed up in New York City for the Second International Conference on Plant Breeding and Hybridization in September 1902, his *Mendel's principles of heredity: A defense* having just been published, he was staggered by the enthusiasm with which he was received. These conference attendees were not, by and large, academic biologists; they were horticulturalists, commercial growers, and scientific breeders. Eleven of the seventy-two participants at New York were employees of the U.S. Department of Agriculture. They found Mendel exciting for the same reason that Bateson did: his principles provided them a practical way to systematically analyze individual variation. Unlike Bateson, though, they were unconcerned with the various competing theories of evolution being bandied about by naturalists; they saw in Mendel the possibility of immediately improving breeding practice. And many of the conference

¹⁹⁵ Gerson, "American system," 169.

¹⁹⁶ Daniel J. Kevles, *In the name of eugenics: Genetics and the uses of human heredity* (Cambridge: Harvard University Press, 1995), 41-43. Gerson, "American system," 173.

attendees, unlike Bateson, who had at his disposal tiny breeding populations of chickens and assorted small animals, had the right connections to work with experimental farm plots spread across the United States. As early as 1901, the USDA began publishing information about the rediscovery of Mendel in its *Experimental station record*, and within a year there was a greater excitement about Mendelian analysis in America's agricultural colleges and experiment stations than any other set of institutions in the world.¹⁹⁷

Along with Mendelism, participants at the New York conference discussed another development in the world of agricultural research: the formation of a new national association dedicated to scientific breeding. At the First International Conference on Hybridization, which had been held in London in 1899, three leaders in American agricultural science – Liberty Hyde Bailey at Cornell's College of Agriculture, Willet M. Hays of the Minnesota Agricultural Experiment Station, and Herbert J. Webber of the U.S. Department of Agriculture – had begun to sketch the outlines of a national organization that might organize the community of breeders. Secretary of Agriculture James Wilson approved of the project and became its honorary head, while a committee of the American Association of Agricultural Colleges and Experiment Stations, composed of each of its state chairmen, recruited members. When the new organization, the American Breeders' Association, held its first meeting in St. Louis in December 1903, the vast majority of its 650 members were personnel of agricultural experiment stations, state colleges, or the USDA; the remainder were representatives of "independent breeders, large growers, seed companies, agricultural journals, and state and regional breeders' and growers' associations." The ABA gave the community of American breeders a stronger organization than it had ever had before, and it would in time become an increasingly important venue for the confirmation of Mendelism: "Papers mentioning Mendel increased from one-ninth to one-fifth of the total presentations per year between 1903 and 1909, and while most involved only a brief acknowledgment, virtually all such citations corroborated the theory."¹⁹⁸

Willet M. Hays became the first chairman of the ABA's organization committee, so when, in 1904, he also became the Assistant Secretary of Agriculture under Secretary James Wilson, he was well-positioned to build "a close relationship between the USDA, the stations, and the Association." To achieve this end, the ABA was divided into around fifty committees, each dedicated to a specific topic of inquiry; some were devoted to particular types of plants (corn, cereals, forage crops, sugar crops, fiber crops, tree and vine fruits, cotton, tobacco, tea) or animals (sheep, goats, poultry, horses, wild birds, fish, bees and other insects), while others addressed subjects of study (eugenics, pedagogics, plant and animal introduction, animal hybridization, theoretical research in heredity). Each committee was responsible for surveying the field

¹⁹⁷ Information on the reception of Mendel by the American scientific agriculture establishment was drawn from Diane B. Paul and Barbara A. Kimmelman, "Mendel in America: Theory and practice, 1900 – 1919," in *The American development of biology*, ed. Ronald Rainger, Keith R. Benson, and Jane Maienschein (Philadelphia: University of Pennsylvania Press, 1988), 281-310. See also Barbara A. Kimmelman, "A progressive era discipline: Genetics at American agricultural colleges and experiment stations, 1900-1920" (PhD diss., University of Pennsylvania, 1987).

¹⁹⁸ Barbara Ann Kimmelman, "The American Breeders' Association: Genetics and eugenics in an agricultural context, 1903-13," *Social studies of science* 13, no. 2 (May 1983): 167-168, 172.

under its purview, and then finding ways to promote “technical investigations where needed,” cooperation between private and public institutions, “improvements in practical business methods of breed and variety improvement and breed and variety formation,” and to “[f]ind leaders in the investigations ... and where practical bring about division of labor and cooperation.” Along with coordinating research, the association had a goal of “bringing into the widest use all valuable [breed] types, as by introduction from one country or district to another” in order that “truly superior stocks of plants and animals ... may more generally replace the less desirable forms.” The American Breeders’ Association gave the USDA a new means of control of the direction of the scientific breeding community, one of suggestion rather than of fiat.¹⁹⁹

With its wide membership and endorsement by the Department of Agriculture, the American Breeders’ Association provided a forum at which the American community of scientific breeders could resolve intellectual controversies and establish its orthodoxies. And the presentations given at its first two annual meetings make it clear that, as early as 1903 or 1904, while Helen Dean King was still a morphologist and the Wistar Institute a museum of medical oddities, the establishment view among American scientific breeders was that intensive inbreeding did not itself cause harm to organisms. At the St. Louis conference, Eugene Davenport, Dean of the College of Agriculture of the University of Illinois, complained that the idea that “inbreeding is certain to end in loss of constitution and breeding powers” was one of those “ideas that have become fixed as general principles when they rest on very insufficient data”; he felt it necessary to teach his students that “infertility and lack of vigor are besetting weaknesses in many individuals in most species and that inbreeding is certain to intensify undesirable characters as well as valuable ones,” an interpretation that, though lacking a Mendelian gloss, foreshadowed King’s. At the ABA’s second meeting, numerous speakers addressed inbreeding in both plants and animals. Archibald Shamel left the tobacco fields of the Connecticut River Valley to explain how he had used perpetual self-fertilization to improve cigar wrappers. N.W. Gentry, a pig breeder from Sedalia, Missouri, apologized because he was “not competent to discuss the subject from a scientific standpoint,” but asserted that, based on his experience with Berkshires, “there [was] little or nothing to fear from kinship of animals mated if they are suited to be mated together. I have watched results of inbreeding in my herd for years, and until I can discover some evil effects from it – and I have not yet – I shall continue to practice it.” William Castle, who had yet to publish the results of his fruit fly breeding experiment, was still ready to declare that “[i]nbreeding is not invariably an evil,” and that it is “often necessary to cause the reappearance of a vanished recessive character, and is indispensable in the formation of races which will breed true.”²⁰⁰

¹⁹⁹ *Ibid.*, 170. Willet Hays to William Bateson, April 5, 1906, BLC-JIC, Box M, M.1 – American Breeders’ Association. W.M. Hays, “The new magazine,” *American breeders magazine* I, no. 1 (1910): 65.

²⁰⁰ Eugene Davenport, “Teaching thremmatology,” *Proceedings of the American Breeders’ Association* I (1905): 56. See Shamel, “Improvement of cigar-wrapper tobacco,” 115-120. N.W. Gentry, “Inbreeding Berkshires,” *Proceedings of the American Breeders’ Association* I (1905): 169-170. William E. Castle, “Recent discoveries in heredity and their bearings on animal breeding,” *Proceedings of the American Breeders’ Association* I (1905): 126.

A lone, yet very important, voice continued at the 1905 meeting to stand by the idea that inbreeding was harmful. Cyril G. Hopkins, a professor at the University of Illinois, had for the past nine years overseen a large-scale breeding experiment on corn at the state's experiment station. The project had the support and collaboration of many of the commercial seed producers of Illinois, who incorporated the experiment's findings into their practice. Hopkins continued to believe in the "well-known principle established by the investigations of Darwin and others, that injurious effects are produced from the self-pollination of plants which are naturally cross-pollinated," and he claimed that the "most important improvement which [the experiment station has] thus far made in the system of corn breeding is that which relates to the prevention of inbreeding." Nothing that Hopkins said about inbreeding outright contradicted the growing consensus about it (no researcher was claiming that inbreeding would never cause harm), but his devotion to an older way of looking at things placed a strain (as we will soon see) on his relationship with the younger researchers on his staff.²⁰¹

By 1910, American scientific breeders were so enamored with intensive inbreeding methods that, when the ABA issued its first magazine, it chose to honor Amos Cruikshank as "our greatest example of a master breeder of animals." What made Cruikshank worthy of recognition, along with his being a "man of sterling worth, simple in his tastes and of lofty character," was that he had worked as a breeder with a single superior bull, Champion of England, "the blood of which was projected with such high efficiency into his progeny, and this blood so well endured rather narrow inbreeding that it was capable of serving as the basis of a prepotent sub-breed of Shorthorn cattle." Alongside Cruikshank, a livestock breeder in the tradition of Robert Bakewell, the magazine selected two other historical figures as the three "chief pillars in a structure upon which rests the modern science of heredity and breeding": Gregor Mendel and Charles Darwin. Mendel, who had "given a new point of view and a new inspiration to biological thought and research," had of course long been a favorite of the American breeders, but Darwin was an unusual choice. Because his later research had been so heavily invested in demonstrating the dangers of inbreeding, the most recent generation of horticulturalists had mostly ignored it, and his botanical legacy was by now largely forgotten. Breeders undoubtedly believed in the power of selection, but they also recognized its limitations and still did not expect to be able to create new species. William Spillman's observation in 1910 is typical: "One of the most important things accomplished by scientific discovery, in relation to the art of the breeder, is the dissipation of the old idea that practically unlimited improvement can be made by selection. It is now the consensus of opinion, amongst the leaders in this line of work, that selection can only accomplish the segregation of the best strains in the populations selected." This is precisely the kind of evidence *against* evolution by natural selection that Darwin's opponents like Louis Agassiz had predicted that scientific agriculture would furnish. Yet, there he was, the sage of Down House, in a portrait in profile in the first pages of the ABA's magazine, staring off the

²⁰¹ Cyril G. Hopkins, "Inbreeding of corn and methods of prevention," *Proceedings of the American Breeders' Association* I (1905): 148.

page toward the right with a thoughtful mien, his scraggly white beard hiding his chin.²⁰²

Charles Darwin also made an appearance at the first conference of the American Breeders' Association in St. Louis in 1903, in spirit if not in body, and this manifestation was a telling indicator of what his role would be in the new organization. After the meeting was called to order, its first speaker and main organizing force, Willet Hays, called on his audience to remember Darwin, who "found much in the study of the practical breeding of domestic species which aided him in broadening out the world's view of natural evolution." It was now time, Hays suggested, for "scientists in biological lines [to] turn ... from the interesting problems of historical evolution to the needs of artificial evolution," and to become familiar with "the conditions of the living organisms under improvement, and the practices and the problems of the practical breeder, that they may apply their scientific methods to the solution of these problems." The moment had arrived for evolutionary biologists to "emerge from the cloister of species and genus grinding in the study of historic evolution, and coöperate with practical breeders in the study of breed and variety formation and improvement" in a noble quest to "mak[e] a possible annual increase of ten or more per cent in the billions of dollars' worth of American plant and animal products." It was an odd pitch, an appeal to the spirit of scientific inquiry and to the spirit of scientific management, to experimental evolution and to industrial efficiency, and the argument leaned heavily on the legacy of Charles Darwin.²⁰³

Two distinct groups were coming together, one in the midst of industrializing American agriculture through the application of mass production techniques, the other a faction caught in the middle of the intellectual turmoil of a biological science without a governing paradigm, each rapidly seeing the value in systematically analyzing individual variation. Or, as Hays chose to describe it, using the original Darwinian conception of domestication, these were "[t]wo schools of men ... trying to solve problems of evolution, both using methods which may roughly be denominated statistical methods." Whatever their common interests, though, these were two different groups, and their cooperation might lead to tension over their differences and, for those not a part of the partnership, controversy that this alliance should be made. The legacy of Darwin, the rough historical narrative he had pioneered in which methodical selection became a variant of natural selection, the scientific breeder an experimental evolutionist, could serve as a useful foundational myth for this association. Charles Davenport, who had supervised William Castle's doctoral work at Harvard, recognized this:

To the scholastic biologist of our universities the work of the "breeder" has for long been regarded with contempt. Although recognized as a department of commerce, it has been regarded in many quarters as the least dignified department, associated in mind with the cowboy, the stable

²⁰² "Darwin, Mendel, and Cruikshank," *American breeders magazine* I, no. 1 (1910): 6, 13-14. W.J. Spillman, "Effect of recent discoveries on the art of breeding," *American breeders magazine* I, no. 1 (1910): 70.

²⁰³ Willet M. Hays, "Address by the chairman of the organization committee," *Proceedings of the American Breeders' Association* I (1905): 9-11.

boy, the “hayseed,” the country jay, the peasant of Europe. “What do you do at the meeting of the Association,” says my university colleague, “inspect ‘hawgs,’ pass around ‘pertaters’ and show up your biggest ears of corn?” But that attitude is changing and changing fast. It is interesting to note the reasons. For one thing, the factors of evolution were always regarded as worthy subjects of research; and the old method of discussing evolution without facts had fallen into disrepute ... Meanwhile the work of the Agricultural Experiment Stations ... made pure biologists acquainted with the valuable experimental material offered by such organisms ... And so ... the scientific investigation of biological problems involving experimental breeding began.

As long as it could be called evolution, as long as it could be imagined as “pure research” belonging to the world of *skholè*, the research program could retain its social standing.²⁰⁴

Darwin did not give these new Darwinists a research program. He didn’t give them an evolutionary model to work from or a protocol for developing and evaluating new experiments. His attempt to himself become a botanical breeder turned out to be a bust, for the most part. What he gave them instead was a way to imagine themselves as a shared community, a foundational narrative about methodical selection that he’d first sketched in *Origin of species* and then elaborated in detail in *Variation under domestication*. As he’d linked the elite naturalists of Britain to the pigeon fanciers and the working-class readers of Lindley’s *The gardeners’ chronicle*, he now bound a small circle of academic biologists in American universities to a massive project of agricultural industrialization. This time, though, unlike the first, the scientific breeders were tightly organized and carried along by the enthusiastic patronage of the national government. The volume of detailed information this national project might provide the biologists, along with the impact that the biologists’ recommendations might have on the agricultural environment, was on an enormous new scale. Darwinism had come back to life again not just as an academic exercise, but as a living transformative practice.

III

February of 1905 was one of the coldest recorded months in the history of Illinois. In Champaign, on the campus of the University of Illinois, the five acres of corn cultivated in the middle of the quad, surrounded by classroom buildings, passed daily and largely unnoticed by undergraduate students, was very likely covered in a thick layer of snow. The corn plants had been on this same spot since 1876, when George Morrow, who would go on to become the first Dean of the College of Agriculture, returned from a visit to the Rothamsted Experimental Station in England and was inspired by that institution to establish a long-term experimental field devoted to measuring crop yield. By 1905, Morrow was gone, but the experiments continued, and they seemed to prove definitively that corn produced by the crossing of two distinct varieties (and which varieties the

²⁰⁴ *Ibid.*, 10-11. Charles B. Davenport, “The relation of the Association to pure research,” *American breeders magazine* I, no. 1 (1910): 66.

breeder chose seemed not to matter) outperformed either of its parental varieties, at least during the first hybrid generation. A short walk down the quad from the Morrow Plots, in warmer accommodations in the Agricultural Building, the American Breeders' Association, sponsored this year by the Illinois Live Stock Breeders' Association and the Illinois Corn Growers' Association, held its annual conference.²⁰⁵

In attendance was Edward Murray East, a twenty-five year old chemist who would have been an obscure figure to the scientific luminaries, experienced experiment station hands, established breeders, and government officials who the association had been created to bring together. East had spent the past several years nearby analyzing the material composition of corn samples as part of one of the largest projects at the Illinois Agricultural Experiment Station. His work had brought him into close contact with scientific breeders, and before the year's end he would leave Illinois to take up a position as a plant breeder at the Connecticut Agricultural Experiment Station, which maintained close ties to Yale University.²⁰⁶

In Connecticut, East continued working with corn, adjusting to the climatic conditions of New England after his initiation in the Midwest, and he supplemented this research with experiments on potatoes and tobacco. In November of 1907, though, he published an extended review essay as an agricultural bulletin of the station that illustrated, if not an actual debt to the American Breeders' Association, then a strikingly coincidental sympathy with its ideological aims. Ostensibly written for the common farmer, the essay promised to provide "a short outline of the current belief in the most important theories and principles of variation, evolution and heredity, with their practical application to methods of breeding farm crops." East invoked Darwin, who "himself obtained much of his evidence from domestic animals and cultivated plants," and proposed to merge the work of the academic evolutionary biologists with the work of the scientific breeders, "for the theory of the two studies must inevitably go together." This was to be the promised intellectual fruit of the institutional coordination that had begun in 1903.²⁰⁷

East's handling of the biological literature in his 1907 article demonstrates a mastery of the evolutionary theory of his day, yet up to the time of the article's publication he had built a conventional career as a scientific agriculturalist. A native of Du Quion, a small town in southern Illinois, closer to St. Louis than

²⁰⁵ Jeff L. Bennetzen and Sarah C. Hake, eds., *Handbook of maize: Its biology* (New York: Springer, 2009), 95. "Minutes of the meeting," *Proceedings of the American Breeders' Association* I (1905): 104.

²⁰⁶ Reconstructing the life of Edward M. East is a challenge for the historian. Unlike many of his colleagues in the early genetics community, East did not leave his papers to any historical repository, so we have no central archive to consult. One detailed source on East's work with hybrid corn is A. Richard Crabb, *The hybrid-corn makers: Prophets of plenty* (New Brunswick: Rutgers University Press, 1947), 21-92. Crabb, a journalist, interviewed most of the principal figures involved in the early production of hybrid corn, but it's difficult to corroborate many of his statements with documentary evidence produced before the 1930s. In some cases, Crabb's details conflict with the existing record. In this section, I've depended on extant letters written by East that have survived in the papers of other geneticists (especially in the collections of the American Philosophical Society in Philadelphia) and Crabb's account, along with East's many publications, in order to tell the story of his professional life.

²⁰⁷ Edward M. East, "The relation of certain biological principles to plant breeding," Connecticut Agricultural Experiment Station bulletin 158 (November 1907): 5-6.

Chicago, East entered the University of Illinois during the fall of 1898 with an aptitude for the physical sciences. He spent most of his time during the next several years taking courses in physics and chemistry, writing his Master's thesis on a method to purify water in running streams.²⁰⁸

In June 1900, after graduating from the university, he was hired as an assistant chemist by Cyril G. Hopkins, who directed studies of soil and crops at the Illinois Agricultural Experiment Station. Like East, Hopkins was a chemist by training, but his major venture at the time was a large-scale experiment in corn breeding. Most of the corn in Illinois was not being grown for human consumption, but rather for the fattening of livestock, a major industry for the state. Eugene Davenport, who had succeeded George Morrow as Dean of Agriculture in 1895 and was seeking new projects for his institution, recognized that feedlot corn did not provide adequate protein for large animals and had to be supplemented by expensive additives imported from out-of-state. He proposed in March 1896 to initiate a breeding experiment to produce a new type of corn for the livestock industry, a corn high in protein, and Hopkins, who had studied the chemical composition of corn, took control of what was, in the words of A. Richard Crabb, to be "no classical study of corn, but rather ... a practical corn-breeding job with an assigned objective and a yardstick for measuring results."²⁰⁹

The project began in the spring of 1896 when Hopkins oversaw the planting of 163 rows of corn, each row planted from the kernels of a different ear of Burr White variety corn (Hopkins chose white corn because the farms surrounding the university were growing yellow corn, so he would be able to easily spot ears that had been contaminated by incidental cross-pollination). The original ears of corn, which retained most of their kernels, were carefully labeled and kept so that they could later be tested against future generations. At the end of the growing season, Hopkins had all the ears produced in each row harvested and placed in a separate sack, providing him 163 sacks of corn for analysis.²¹⁰

The initial results were encouraging; ears of corn varied in their protein and fat content, and much of this variation seemed to be transmitted from generation to generation. Hopkins expanded his operations, and as he did he brought new young technicians to the project who would remain for varying lengths of time before moving on to other occupations. Archibald Shamel, at the time an undergraduate at the university, joined the station's staff in 1898 and remained there until being hired by the Bureau of Plant Industry in 1902. When East was hired in 1900, Hopkins put the chemist, who was described by one of his professors as "a rather retiring pleasant sort of a fellow" who "constructed his sentences with great care" and was "studious to the point of being preoccupied," to work analyzing the composition of corn kernels on the ears in the sacks that had piled up in the laboratory.²¹¹

The world in which East found himself was one where the boundaries between the university and agricultural corporations were minimal. Personnel

²⁰⁸ On East's academic record, see Edward M. East transcript, SLC-UIA, record series 25/3/17.

²⁰⁹ Crabb, *Hybrid-corn makers*, 15-16, 22.

²¹⁰ *Ibid.*, 17-20.

²¹¹ Deborah Fitzgerald, *The business of breeding: Hybrid corn in Illinois, 1890-1940* (Ithaca: Cornell University Press, 1990), 17. Crabb, *Hybrid-corn makers*, 24.

passed back and forth from the experiment station to seed companies to growers' associations, and all those involved seemed to speak a common language and seemed to be working toward a common goal; this was a place, as Deborah Fitzgerald has noted, where geneticists and breeders "received virtually the same training, often at the same schools, and most shared a common set of skills, procedures, and professional knowledge." Perry G. Holden, for example, became an assistant professor of agricultural physics at the experiment station in 1896, working to produce new strains of sugar beet, but left to oversee the Peoria Sugar Refining Company in 1900 when he was offered a \$4,000 annual salary (the station paid him only \$1,600, though he made it known that he would remain if they raised it to \$2,500). Nonetheless, he remained a frequent presence at the station and maintained close ties with many of the staff there. East was given a small office on the third floor of the Agricultural Building, a couple of doors down from Hopkins, and he shared the space with H.H. Love, a young researcher who maintained close ties to Eugene Funk of the Funk Brothers Seed Company, a corporation that greatly influenced the research direction of corn breeding in Illinois. In their office, a "room ... so small that when they both leaned back in their desk chairs, their heads would bump," Love introduced the chemist East to the practical problems with which corporate plant breeders were struggling.²¹²

At the station, East thrived. He began to expand his work beyond the chemical analysis of corn kernels, and Hopkins soon provided him with a small experimental plot on which to pursue research of his own. When a senior staff member left for a trip to Europe in June 1904, Hopkins placed East in charge of the plant breeding work of the station.²¹³

The events that would transpire in the year following this promotion created a breach between Hopkins and East that would eventually push the young breeder to leave Illinois. We have no direct surviving evidence of what happened during that year, but two second-hand accounts, each providing a different interpretation of the event in key ways, survive from the 1940s. The first account comes to us from A. Richard Crabb, whose triumphalist history of hybrid corn casts East in a starring role in the product's invention. With Hopkins away from the station and experienced personnel absent, East and his friend Love saw an opportunity in June 1904 to finally pursue new lines of research that had been stymied by Hopkins. After analyzing the past several years of data compiled by the corn breeding project, East had concluded two things: all the high-protein varieties of corn that the project had created were exhibiting declining yield with each passing generation, and all of the highest protein lines were the descendent of a single ear of corn from the original batch of 163 ears. He decided that what the station needed to do was open an experiment on inbreeding in corn in order to understand both why the best strains had come from a single founder and why each generation saw a progressive decline in yield. Luckily for him, Hopkins had been carrying on an inbreeding experiment for over a decade as a control against which he could compare the high-protein

²¹² Fitzgerald, *Business of breeding*, 3. On Holden's connection to sugar refining, see Winton U. Solberg, *The University of Illinois, 1894-1904: The shaping of the university* (Urbana: University of Illinois Press, 2000), 133. Crabb, *Hybrid-corn makers*, 26.

²¹³ Crabb, *Hybrid-corn makers*, 24.

corn breeding project that was his primary focus. These strains, of the yellow Leaming variety in order to differentiate them from the white corn of the larger project, had undergone at least four generations of inbreeding. After getting a nod of vague approval from Hopkins, East and Love, with the assistance of a field technician, planted a plot of the inbred corn in the spring of 1905 (just months after East attended the American Breeders' Association meeting at which intensive inbreeding as a method received support from so many different quarters), and by mid-summer the two researchers were startled and impressed by the fact that "all of the inbreds were remarkably uniform in comparison with the open pollinated corn in the check rows."²¹⁴

A second account, provided in 1948 by Perry G. Holden partially in response to Crabb, suggests that Hopkins was not responsible for the inbred Leamings at Illinois and had a limited connection to the corn breeding program as a whole. Holden and Eugene Davenport both studied under and worked with the corn breeder W.J. Beal in Michigan during the 1880s. When Davenport took charge of the College of Agriculture at the University of Illinois in 1895, he hired Holden to take over the station's work in crops. The two men planned a series of corn breeding experiments based on their experiences with Beal; one of these was to be an experiment on the effect of intensive inbreeding on yield, and a field technician was tasked with self-pollinating some corn by covering the stalks with sacks. The experiment continued for years, providing a good deal of information about inbreeding. When Holden left the experiment station in 1900 to work on sugar beets with a private corporation, the corn inbreeding project passed into the hands of a research assistant whom Holden had hired in 1898, none other than Archibald Shamel. In this version of the story, Cyril Hopkins, an agricultural chemist by training, had little to do with the many strains of inbred and crossbred corn with which the station was working; he was brought into the project for his analytical skills, and it was only the fact that he published the results of his chemical work and popularized it at conferences that connected him in the public mind to the breeding work of the station. By this account, then, East in the spring of 1905 picked up a line of research on inbreeding that had been begun by Holden and Davenport and carried on by Shamel.²¹⁵

Holden's version of events, if accurate, would explain why Archibald Shamel, upon arriving in Connecticut, was prepared to begin a program of self-fertilization of tobacco through bagging in 1903. It would also explain how Hopkins, with no previous experience with plant breeding, could have become involved in the station's corn projects. While the origin of the inbred corn strains in Illinois remains in dispute, what happened when Hopkins finally visited East and Love's experimental plot at the end of the growing season does not. Hopkins was clearly unimpressed by the inbred stalks and ordered East to terminate his foray into inbreeding. One famous, though uncorroborated, version of the exchange has Hopkins thundering at East amidst the stalks, "We know what inbreeding does and I do not propose to spend the people's money to learn how to reduce corn yields."²¹⁶

²¹⁴ This account can be found in Crabb, *Hybrid-corn makers*, 26-32.

²¹⁵ See Perry G. Holden, "Corn breeding at the University of Illinois, 1895 to 1900," in HMC-UIA, Box 1.

²¹⁶ Crabb, *Hybrid-corn makers*, 33. "A hearsay story," HMC-UIA, Box 1.

In addition to having a divergent research agenda with his supervisor, East found it increasingly difficult to engage with Hopkins personally. Crabb claims that “East and Love ran their little office as a sort of retreat from the atmosphere of formal discipline maintained rigorously by Professor Hopkins throughout his agronomic domain, evidence of his uncompromising nature.” Archibald Shamel had even less happy memories of his time with the agricultural chemist in Illinois:

I found Hopkins to be a selfish, greedy, jealous, unscrupulous, crafty creature. East confirmed my conclusions when I talked with him at New Haven. I think that Hopkins was evil all through and I learned to hate and despise him, as did East.

By the summer of 1905, it seems, Edward M. East was ready to leave the experiment station.²¹⁷

Edward H. Jenkins, the director of the Connecticut Agricultural Experiment Station, now gave him the opportunity to do so. Jenkins had traveled to Champaign in February to attend the ABA meeting and to inspect the university’s corn breeding program, which he hoped to replicate in New England. While there, and on the recommendation of Hopkins, he dined in an Urbana hotel with Edward East, who left a lasting impression. When funds became available in Connecticut at mid-summer to hire a new researcher at the station, Jenkins offered the position to East, promising him that he would be “subject only to my general oversight and direction in matters of station policy” and “responsible to no other of the station staff in planning and executing your work.” East accepted without hesitation, for along with these advantages the position carried, as Hopkins noted in a letter to Davenport, a “considerably higher salary (\$1700) than he is being paid here or than could possibly be paid here for any position which could be offered to [him]”.²¹⁸

At the Connecticut station, East built a corn breeding program that rivaled the one he left behind in Illinois. He had everything there he needed except the corn, and in time he got that, too; he didn’t dare ask Hopkins for any of the strains that belonged to the Illinois station, but he convinced his friend Love to surreptitiously swipe a few kernels from each of the ears of the inbred lines in the plant laboratory and mail them to him with labels. With the yellow Leamings as a foundation, East opened a wide range of new experiments, turning at times to potatoes and tobacco as well as corn. As the lead agronomist at the station, he saw his primary mission as the production of new strains of a super-productive seed corn that might “double the yield without increasing the land at present devoted to its culture by a single acre.” It would not be feasible for strains of this type to be grown for seed by every corn farmer in the state, so East recommended that “the man who grows from one to ten acres of corn should purchase his seed from some *neighboring corn breeder* who is producing a variety adapted to the soil of that locality.” It was the job of the station, East

²¹⁷ Crabb, *Hybrid-corn makers*, 26. “P.G. Holden Collection,” HMC-UIA, Box 2, Folder: Holden, Perry, 1941.

²¹⁸ Crabb, *Hybrid-corn makers*, 30-31, 34-35. Cyril Hopkins to Eugene Davenport, August 23, 1905, LB-UIA, Box 5, Volume 17.

thought, to assist these regional commercial breeders in the production of new high-yielding varieties, not to directly assist smaller farms. When the program was complete, East predicted, "Connecticut could furnish seed for the whole of New England, as it is possible to breed only very early varieties successfully in latitudes more northern than our own state."²¹⁹

In 1908, East performed a series of corn breeding experiments that suggested to him a new method for creating this high-yield seed. Technicians at the Connecticut station made thirty different crosses using a wide range of commercial corn varieties and experimental inbred lines. After performing the crosses, they planted the resulting seeds and compared the productivity of the cross-bred stalks to the productivity of stalks grown from the two parent ears of each cross. Though the researchers fought a losing battle against the "crows and chipmunks [that] played havoc with the 'stand,'" the experiment produced a surprising result. A cross between a commercial variety (121 bushels per acre) and an inbred strain (62 bushels per acre) resulted in a seed that could yield 142 bushels per acre, a clear improvement on either parent. A cross between two different commercial varieties, one yielding 121 bushels per acre and the other 72, produced a plant that could yield 124 bushels per acre, a slight improvement. When two low-yielding unrelated inbred strains were crossed, one delivering 65 bushels per acre and the other 62, something unexpected happened: their hybrid produced 202 bushels per acre! "[C]asual observation," East noted, "was sufficient to show that [the hybrid] soared far beyond each parent in vigor of plant and size of ear."²²⁰

The new method of hybridization that was suggested by the experiment, the commercial possibilities of which East was already speculating on in 1909, broke in a significant way with the earlier botanical hybridization of the nineteenth century. Traditionally, horticulturalists and breeders had sought to cross distinct varieties of plants, especially those that possessed contrasting positive characteristics, in order to produce new hybrids. This was the type of varietal hybridization that Darwin had written about in *Origin of species* and *Variation under domestication*, and it was the kind of hybridization that was discussed regularly at meetings of the American Breeders' Association. It was widely accepted that this form of crossing would produce particularly hearty offspring, recipients of a quality known as hybrid vigor; the second cross performed by the station confirmed this. What East was now proposing, though, was a system of intensive inbreeding followed by the crossing of inbred lines. The plant breeder would now have the power to isolate a favorable trait through perpetual self-fertilization, and to then mix different isolated homozygous lines to produce hybrids with great strength and productive potential. East imagined that, when his method was perfected, it would be possible for corn breeders to specialize in the creation and mass production of heavily inbred strains and for the corn grower to purchase two different seeds each season, depending on her

²¹⁹ An account of Love's acquisition of the Illinois station's corn can be found in Crabb, *Hybrid-corn makers*, 35-37. Edward M. East, "Report of the agronomist," *Annual report of the Connecticut Agricultural Experiment Station* 31 (1908): 397-98.

²²⁰ Edward M. East, "The distinction between development and heredity in inbreeding," *American naturalist* 43 (1909): 178-179.

preferences, and to then “grow the [hybrid] generation of the cross between them.”²²¹

As East was beginning to imagine the possibilities inherent in crossing inbred lines, he found an important collaborator just across the Long Island Sound. George Harrison Shull, one of the few botanical researchers at Charles Davenport’s Station for Experimental Evolution at Cold Spring Harbor, first came to East’s attention when he presented a paper in January 1908 in Washington, D.C., at the ABA’s annual gathering. “[A]n ordinary cornfield,” Shull proposed, “is a series of very complex hybrids.” While intensive inbreeding seemed to do little damage to crops like tobacco, it almost always left corn stunted and malformed. This was because “[s]elf-fertilization soon eliminates the hybrid elements and reduces the strain to its elementary components.” Most corn under cultivation, Shull suggested, was already benefiting from the great hybrid boost for which East was finding empirical support. What was required of the corn breeder in the future was not only the creation of new inbred lines, but “the development and maintenance of that *hybrid combination* which possesses the greatest vigor.”²²²

Shull, like East, was the type of biological researcher who fit comfortably in the institutional framework that the ABA fostered. He hailed from rural Ohio, and largely educated himself while working as a farm laborer; a typical entry from the diary of his youth, drawn from March 1893, reads as follows: “Loaded a load of hay and helped haul it out to the Avenue this morning. Made wood this afternoon. Studied Agriculture and Natural Philosophy.” After saving some money, he attended Antioch College in Yellow Springs, Ohio, and then earned his doctorate at the University of Chicago, where he became close to Charles Davenport. He went to work for a year as a plant breeder with the USDA’s Bureau of Plant Industry, but then Davenport offered him a research position with a higher salary at the new Station for Experimental Evolution (“I am not mercenary,” Shull explained, “but have struggled so long against financial difficulties that a difference of several hundred dollars per year, exerts a telling appeal”), and he accepted. At Cold Spring Harbor, he found himself in a situation similar to East’s, free to pursue the research he chose and constrained only by the oversight of the station’s director.²²³

After Shull’s 1908 presentation, East sent a letter to the scientific breeder at Cold Spring Harbor to tell him that “I agree entirely with your conclusion, and wonder why I have been so stupid as to not see the fact myself.” Shull responded with a cordial letter of his own, and this exchange initiated an intellectual partnership and sometimes rivalry that would last through the next decade. As early as the next meeting of the ABA, which took place in Omaha in December 1909, we find evidence of a shared program; Shull proposed a plan for experimentation in the creation of pure lines of corn and their hybridization

²²¹ *Ibid.*, 181.

²²² The first encounter between East and Shull is described in Crabb, *Hybrid-corn makers*, 45-48. A more detailed account of the relationship between the two scientists can be found on pp. 49-67. George Harrison Shull, “The composition of a field of maize,” *Proceedings of the American Breeders’ Association* IV (1908): 299-300.

²²³ *Diary of Geo. H. Shull for the year 1893*, GHS-APS. George H. Shull to Charles B. Davenport, January 18, 1904, CBD-APS, Folder 1: Corr. Shull.

that resembled the proposal that East had already put forward. At the Connecticut station, however, East held at least one distinct advantage over Shull on Long Island. With the exception of the contacts he had made through meetings of the American Breeders' Association, Shull was largely cut off from the world of scientific agriculture. While he could propose great plans for large-scale experiments, he knew he was dealing with "practical questions which lie wholly outside my field of experimentation," and he could only hope that the "Agricultural Experiment Stations in the corn-belt will undertake some experiments calculated to test the practical value of the pure-line method" he had outlined. He was also largely isolated from the botanical world at large. While the mission of the Station for Experimental Evolution included investigation of the heredity of both animals and plants, the real emphasis of the institution was on zoology and, increasingly, human eugenics. Shull found his research out of sync with that of most of his colleagues, and he felt Davenport treated his research program as secondary. "I wonder whether you know," he queried the director of the station in one petulant letter, "that the blasting up on the hill is seriously damaging my cultures ... It appears to me that such work as blasting in such close proximity to culture-fields should be attended to when those fields are empty. Do you not think so?"²²⁴

In New Haven, on the other hand, Edward East could make use of the Connecticut Agricultural Experiment Station's close ties to the state's largest growers, many of whom were eager to receive a free consultation with the station's chief agronomist. In addition, he retained a considerable freedom to research and publish on topics that traditionally belonged to the academic biologists. Though East had no formal training in theoretical biology, he had become acquainted in Urbana with Charles Hottes, a botanist who had traveled through Europe and was familiar with the recently rediscovered theories of Mendel, and East became well-versed in the debates surrounding heredity and evolution that had characterized the first decade of the century. The 1907 essay that he published as a bulletin of the station, the one that unequivocally asserted that "the theory of the two studies [plant breeding and evolution] must inevitably go together," was his first formal venture into evolutionary studies, and it illustrates well an approach he would follow for the rest of his career.²²⁵

The essay, entitled "The relation of certain biological principles to plant breeding," opens with an historical survey of "the idea of organic evolution." We learn that the first important evolutionary theorist was Lamarck, who proposed stimulating new ideas but failed to attract a sufficient following; he was followed by Darwin, who put his theory of evolution forward with "such brilliancy and with so many data" that "before his death practically the whole thinking world was converted from the orthodox Jewish belief in the special creation of every species, to that of the development of all organisms from very

²²⁴ Crabb, *Hybrid-corn makers*, 46. George Harrison Shull, "A pure-line method in corn breeding," *Proceedings of the American Breeders' Association* V (1909): 57-59. George H. Shull to Charles B. Davenport, May 20, 1913. CBD-APS, Folder 4: Corr. Shull.

²²⁵ There is some dispute about the exact relationship between East and Hottes. According to A. Richard Crabb, East took courses with Hottes while at the University of Illinois. As Winton U. Solberg has noted, however, the only botany courses that appear on East's official transcript were taken during the time span when Hottes was in Germany. See Crabb, *Hybrid-corn makers*, 23, and Solberg, *University of Illinois*, 149.

primitive types.” Though Darwin was influential, his theory had serious flaws, and so in the next generation a significant alternative emerged: the mutation theory of Dutch botanist Huger De Vries, which emphasized the appearance of new mutant traits rather than selection from pre-existing variation as the primary driving force of evolution. East identified his work most closely with that of De Vries; “[t]he writer is well aware,” he apologized, “that he can be accused of a pronounced De Vriesian view of this paper.” Curiously, however, he openly acknowledged that his reason for choosing De Vries over Darwin or Lamarck did not have to do with an inherent correctness to any of the theories, but rather for the bridge that De Vries provided between plant breeding and evolutionary theory:

[M]y view is from the standpoint of the principles and theories that give at present the most practical and efficient help in actual plant breeding. As was stated in the beginning, the studies of the evolution student and of the plant breeder should be and are parallel, but only in so far as theories are proposed that can be experimentally demonstrated ... We may admit, for instance, that the believer in Lamarckian factors as agents in evolution can say that experiments concerning the inheritance of acquired characteristics have been carried on for only a period of time that would be negligible in a geological epoch; we may admit the justness of the same criticism of our conclusions regarding the ineffectiveness of the selection of fluctuations in permanently changing characters: but we are justified in retorting that only such theories can be of use to us that produce results within the span of a human life.

East was making the odd recommendation that plant breeders ought to study evolution so that they might learn from the work of Huger De Vries, which was essentially plant breeding work hidden beneath an evolutionary facade.²²⁶

Why not, then, skip evolutionary studies entirely and just read De Vries as a plant breeder? One answer, which would be enunciated three years later by William Castle in front of a Boston lecture hall, was that in an age of science “we are not satisfied with rule-of-thumb methods, we want to know the *why* as well as the *how* of our practical operations.” East and Castle, though not yet directly working together, were already in 1907 part of the same research tradition. To them, and the many other researchers tied together under the roof of the American Breeders’ Association, evolutionary theory was not so much a true account of the origins of life but more a generalized explanation of methodical selection. East recommended that farmers and plant breeders pick up two volumes by Darwin, *Cross and self-fertilisation in the vegetable kingdom* and *Variation under domestication*, and he had very specific recommendations about how these ought to be read. For the former, one should “[r]ead chapters one and twelve, giving the plans of the experiments and the results,” conveniently skipping Darwin’s torturous and detailed explanations of what was going on. In regard to the latter, the “historical parts of the first volume” of *Variation under domestication* were “still of great interest,” but the “theoretical discussions in volume two will only be confusing to the reader and had best be omitted.” The

²²⁶ East, “Relation of certain biological principles,” 7, 18, 90.

general plan of Darwin's huge volume, in which he had envisioned the scientific breeder becoming an experimental evolutionist, was still sound, but "[o]ur views concerning the explanation of the phenomena brought together in volume one have entirely changed since Darwin's time." What East was doing was retaining the foundation and structural beams that Darwin had put into place, the narrative in which the work of the breeder was a directed form of natural evolution, while gutting the interior of Darwin's edifice, all the convoluted models of hybrids and pangenes that he had used to make his evolutionary theory comprehensive.²²⁷

With this space now cleared, East could assume the role of experimental evolutionist and return with new eyes to the volumes of data that scientific breeders had accumulated. The program that Hopkins had overseen in Illinois to produce corn with a high-protein content became an experiment in the "selection of fluctuations" (a term also used to describe how Darwin believed evolution took place), and, like a good De Vriesian, East noted that in time the project "seems to have reached its limit"; no mention was made of the program's economic *raison d'être*, to cheapen the feeding of the state's livestock. The same was true for Mendel's pea plant crosses, which were in the process of becoming models for *genetic* research rather than *scientific breeding* research. And when East traveled north into the Connecticut tobacco fields in the summer of 1908, where he found himself surrounded by "the descendents of a cross made by A.D. Shamel between the types known ... as 'Havana' and 'Sumatra'," he discovered that "[t]his plant satisfies the conditions which are requisite for material used in pure line studies." The experiment that he designed there with the support of the Bureau of Plant Industry of the USDA was to be, by East's account, a test of one of Wilhelm Johannsen's mutationist theories of evolution, regardless of the uses to which the Bureau might wish to put his results.²²⁸

East increasingly thought and published in a lexicon provided by the evolutionary theorists, even as he continued to approach the same breeding problems that had followed him from Champaign to New Haven. This shift in perspective seems to have had a limited impact upon his career as an agronomist, but it did attract the attention of an institution that would have had little interest in most other experiment station researchers: early in 1909, Edward East was offered a position as a professor at Harvard University's Bussey Institution. The Bussey Institution, located in Jamaica Plain, several miles from Cambridge, had been Harvard's school of agriculture since 1871. The university closed the college in 1908, and William Castle, who had been in residence at the Zoological Laboratory of the Museum of Comparative Zoology since 1897 as an instructor and later a professor, lobbied successfully to have the Bussey Institution turned into a graduate school of applied biology. He was appointed as professor in the new institution; by this time, he had graduated from his fruit flies to guinea pigs, and he moved a colony of cavies along with assorted other rodents he'd been using in selection experiments out to Jamaica Plain. He hoped

²²⁷ Castle, *Heredity in relation to evolution*, 3. East, "Relation of certain biological principles," 92.

²²⁸ East, "Relation of certain biological principles," 50. Edward M. East and Herbert K. Hayes, "Hybridization and selection experiments with tobacco," USDA-BPI. Division of Tobacco and Plant Nutrition Investigations. Box 4: General correspondence, 1901-26. Folder 8: Grain & Hybridization of Tobacco.

to be able to devote more time to his research in evolution and heredity, so he convinced Harvard to halve his teaching load by hiring a second professor to share the oversight of graduate education at the Bussey Institution. A query to William Bateson, who was by now something of an elder statesmen of the nascent field of genetics, turned up Edward East's name as a potential candidate; Castle, in addition, was especially taken with East's 1907 essay, which he thought was of "outstanding excellence as a discussion of mutation, Mendelism, selection and evolution."²²⁹

East accepted, but not before working out an arrangement with Edward Jenkins that would allow him to continue his work in New Haven. During the academic term, East would live in Jamaica Plain, teaching at the Bussey; in the summer months, he would return to Connecticut and his work in corn and tobacco breeding. In addition, the station would employ promising young untrained researchers to assist East with the breeding program; these new staff members would only need to remain resident at the station from May to November, allowing them to follow East to Harvard during the winter to pursue their doctorates. For the next decade, East lived this dual life, and forged ahead with his program for hybrid corn research even as he took on new professorial duties at the Bussey. Through this arrangement, East not only created new strains of corn, but also found "an opportunity ... to project his plant breeding concepts all over the United States"; East mentored so many young agronomists, A. Richard Crabb noted as he traveled around the Midwest interviewing maize scientists in the 1940s, that "today almost every plant breeder of note is a former student of East's or has been stimulated by one of East's students."²³⁰

At the Bussey Institution, though, East was not supposed to be a plant breeder. The "dour old stone building" in Jamaica Plain where East had his office was supposed to be a scholarly space, *skholè* incarnate, not an industrial laboratory for the development of production processes. Castle, East's closest colleague, was building a program in experimental evolution and heredity, and East was expected to contribute. We frequently find, then, in East's publications after 1909 an evolutionary gloss applied to what was essentially traditional research in scientific agriculture. In a 1912 bulletin for the Bureau of Plant Industry of the U.S. Department of Agriculture, for example, East provided a detailed survey of the literature in plant and animal breeding on heterozygosis, the "hybrid vigor" that seemed to result from the crossing of inbred strains of corn. He included detailed discussions of how the knowledge provided by scientific breeders might be harnessed in forestry and in the production of truck crops like tomatoes and eggplants. He also, though, speculated on the importance of heterozygosis for evolution. Some plant species, he suggested, were naturally cross-fertile because they had taken advantage of reproductive mechanisms that promoted cross-pollination, thereby benefiting from hybrid vigor and increasing their chances of survival. On the other hand, self-fertilized plant species survived and adapted based only on the strength of their own genes, without the extra boost that heterozygosis provides. "The result," East suggested, is "that self-fertilized strains that have survived competition are

²²⁹ William B. Provine, *Sewall Wright and evolutionary biology* (Chicago: University of Chicago Press, 1986): 44-45.

²³⁰ Crabb, *Hybrid-corn makers*, 69-70.

inherently stronger than cross-fertilized strains.” Though an evolutionary theorist might find some inspiration in these speculations, they are much better suited to explain why Archibald Shamel had so much of an easier time inbreeding tobacco in Connecticut than he did inbreeding corn in Illinois.²³¹

For reasons of social and institutional positioning, it was important for many life scientists of that first decade of the twentieth century to think, speak, and publish as evolutionary biologists, regardless of the context out which their work emerged. In their early years of intellectual exchange, Shull and East imagined themselves not so much as pioneering breeders but as evolutionary De Vriesians, forging forward in the construction of a new mutationist theory of the origin of species. Though their choice of words and their self-presentation might matter to their colleagues and to their patrons, their debate about whether the inbred lines of plants they were bringing into being were “elementary species” or “biotypes” or “homozygous strains” made little difference for what they were actually creating. When East or anyone else visited the experimental fields in Connecticut in 1911, they could see with their own eyes that “there stood those Leaming hybrid plants, all the same height, every one with a big yellow ear ... so uniform that they all looked alike.”²³²

IV

The Bussey Institution was, as Leslie C. Dunn recalled it to an interviewer in the late 1950s, an imposing old mansion, one that “should have been in Edinburgh or Glasgow,” standing “in the middle of a big farm.” Its graduate students “lived very intimately together” in an old dormitory on the grounds; the winters were “very bitter – very cold indeed – and the heat seldom got above the second floor,” so Dunn would climb up to third floor “on very cold days, because they had a gas plate, and they had a tea-pot brewing.” The Bussey was a “self-supporting, self-feeding institution”; the researchers “fed our animals the crops that we raised ourselves.” Most of its biologists, Dunn thought, appreciated the distance they kept from Harvard proper: “they chose to work separately, because they needed space,” greenhouses for the botanists and quarters for animal husbandry for the zoologists.²³³

This isolation seems to have created a sense of unity and common identity among its students, but it also created a sense of distinction between the scientists in Jamaica Plain and those in Cambridge. Dunn perceived a “war between the faculties of the Bussey Institution and the academic departments in Harvard,” which “took the form of invasion by Harvard faculty ... every time there was a student being examined at Bussey.” The Harvard professors would “appear in force, and several times they succeeded in flunking him”; as a student, Dunn “was well

²³¹ The brief description of the architecture of the Bussey Institution comes from “Oral History Transcript,” p. 49. LCD-APS, Box 27, Folder: Oral History Transcript – Index. Edward M. East and Herbert K. Hayes, “Heterozygosis in evolution and in plant breeding,” United States Department of Agriculture, Bureau of Plant Industry bulletin 243 (June 1912): 45.

²³² Crabb, *Hybrid-corn makers*, 77.

²³³ “Oral History Transcript,” pp. 49-50, 55, 59. LCD-APS, Box 27, Folder: Oral History Transcript – Index.

aware of this, and warned.” Students at the Bussey, he thought, “became a little bit tarred with the stigma of ‘applied’ biology.”²³⁴

With Castle and East responsible for pedagogy, the initiate at the Bussey could anticipate a detailed introduction to contemporary thought in genetics and evolution. The two “used to alternate, and they used to have to give the general lectures in Cambridge, so Castle would go over one term and East would go over another term.” Before his move to Massachusetts, East had never before taught a class, and when it came time to plan a course of general instruction he turned to Charles Darwin for his general outline:

East ... put us through *Variation of Animals and Plants Under Domestication*, which was the book in which Darwin gave his evidence for the theory of natural selection ... East told us one term (we met, you see, in a weekly seminar, all the students and most of the staff) ... the best thing we could do for ourselves was to read very thoroughly the *Variation of Animals and Plants Under Domestication*, which we did.

We know from his 1907 article that East had little use for Darwin’s theoretical speculations in *Variation under domestication*, so what students were engaging was most likely the historical material in the volumes, the discussions of scientific breeding and the narrative that turned methodical into natural selection. As a result, Dunn found that at the Bussey the study of inbreeding “was important mainly from the standpoint of evolutionary theory”:

Animals and plants have to be adapted to small niches, habitats, and when the population becomes very small, one would have to study the conditions, as to whether a small population could adapt itself to a small niche, if by the same isolation close relatives were forced to breed together.

Instruction at the Bussey, like the later botanical experiments that Darwin undertook at Down, was a venture in scientific breeding seen through a pair of spectacles that colored the world in terms of natural evolution.²³⁵

While East and Castle were institutionalizing the Darwinian outlook at Harvard, the organizational bonds that had first linked academic biologists to scientific agriculture were disintegrating. Beset with tension among its leadership for several years, the American Breeders’ Association formally disbanded in 1913. Willet Hays, who had been instrumental in keeping the association together, resigned from his position in the federal government when the election of President Woodrow Wilson forced Secretary of Agriculture Jim Wilson to end his twenty year regime; within a short time, Hays had left the United States entirely, headed for Argentina where he would work to reform rural education. The remaining members of the ABA’s leadership decided it was time for a major overhaul of the organization’s mission, so they transformed it into the American Genetic Association, which now catered mostly to academic geneticists. The association’s once eclectic magazine became the more

²³⁴ *Ibid.*, 49-50.

²³⁵ *Ibid.*, 61-62, 67.

professional *Journal of heredity*. Other constituencies within the American Breeders' Association turned to new forms of institutional support. The next generation of federal and state agricultural scientists created new modes of exchange within the experiment station community, which was now being bound together by the USDA's new *Journal of agricultural research*. The ABA's eugenics section, which had grown so large that it threatened to engulf the entire association, followed Charles Davenport into politics. Officially, the partnership between scientific breeding organizations and the academic biology establishment seemed to have ended.²³⁶

It would be a mistake, though, to read the formal demise of the ABA as a sign that the program it had been coordinating was dead. The main challenge that its organizers faced in its final years was finding a way to orchestrate the tremendous energy being generated by its membership. This was not an association shriveling from a lack of interest; its leadership, rather, struggled to keep its many rapidly expanding constituencies united under one single agenda. Even as its factions separated from each other, the geneticists to their universities and the agriculturalists to their experiment stations, the eugenicists into politics and the breeders to their fields and ranches, the association's original vision of a united practice and theory continued to become a reality. Among the founding generation, this vision had been concentrated in a single coordinating body, where it might be debated and made a topic of explicit conversation; now, though, it sprawled over many specialized, semi-isolated communities. It was only sporadically a topic of consideration. Within any of these communities, a specialized researcher might no longer see the whole complex web of associations, might imagine himself or herself as solving specific questions belonging to a bounded discipline, but important exchanges were still being transacted across the groups that allowed the entire program to move forward. The American Breeders' Association had been an incubator for the resurrected Darwinian program, not the program itself.

Those who entered the scientific profession through the gates of the Bussey Institution made their careers by skillfully navigating this new terrain. They passed from one community to the next while cultivating wide and diverse personal and intellectual connections, connections that allowed them to peer across the disciplinary and professional boundaries within which many of their colleagues operated. They found their way into many different occupations and social roles, as mathematical theorists or animal breeders or public intellectuals, but what united them was that each remained committed to an inbred line or inbred lines of plants or animals and used the legacy of the later Darwin, the Darwin of *Variation under domestication*, in order to make sense of them.

Consider, for example, Donald F. Jones, a graduate of Kansas State College who read Edward East's 1912 article on plant breeding and evolution and decided to study with the professor at Harvard. East welcomed Jones to the Bussey Institution during the summer of 1914, and in February of 1915 arranged with Edward Jenkins to have him begin work as a plant breeder at the Connecticut station. Jones was introduced to the inbred lines of corn that East had brought from Illinois, and in 1916 he devised a series of experiments to push East's work on hybrid vigor forward. The technical barrier to mass-producing

²³⁶ Kimmelman, "American Breeders' Association," 189-191.

hybrid seed for widespread use was the physical weakness of the inbred parent lines of corn. However impressive the cross-bred generation might be, its stunted inbred parents simply could not produce enough seeds to meet the needs of a scale of agriculture beyond small experimental farms. The hybrid plants could not themselves be used to produce hybrid seed, for the seed collected from them after another round of mating would not benefit from the hybrid vigor that comes from crossing two inbred lines. After a couple of seasons of trial-and-error testing, Jones made an important discovery: crossing two distinct hybrid plants that had themselves been produced by four distinct inbred lines could in some rare combinations of inbred lines result in another generation of hybrid vigor. Because the hybrid parents in this scheme were themselves large, strong plants, they could be used to mass produce this new high-yielding double-crossed hybrid seed. On a relatively small plot of land equipped with the right combination of four inbred lines of corn, then, a public or private breeder could now generate a spectacular amount of high-yielding hybrid seed. The new technique that Jones engineered, which became known as the double-cross method, was quickly absorbed by the community of corn breeders, who now set off in a search to find which breeding permutations of four inbred lines could be employed to manufacture the most valuable seed.²³⁷

Almost immediately, Jones and East recognized that the double-cross method had some other very important economic implications. The technique was far too sophisticated and required too much capital investment for any but the largest agricultural interests to pursue. In addition, while hybrid seed would grow into a high-yielding corn plant, the next generation raised from the seed of the hybrid plant, a non-hybrid generation, could not produce high-yield hybrid seed; corn producers would need to return each year to the seed-producing agency to obtain new high-yield seed drawn from a combination of the four inbred lines. Consequently, those who purchased hybrid seed could not easily become plant breeders themselves. What this meant, East and Jones would soon write, was that for “the first time in agricultural history ... a seedsman is enabled to gain the full benefit from a desirable origination of his own or something that he has purchased.” The method that Jones had developed in Connecticut would allow “the originator to keep the parental types and give out only the crossed seeds, which are less valuable for continued propagation.” The biological dimensions of corn hybridization resolved for the private breeder many of the challenges that Robert Bakewell had organized his Dishley Society to address.²³⁸

Jones published his findings in detail in several venues of scientific agriculture – as a bulletin of the Connecticut station, in *Journal of the American Society of Agronomy* and *Wallace’s farmer* – but they reached non-breeders in an abbreviated and quite different form. In 1918, Jones returned to the Bussey Institution to reflect on his work at the experiment station and to produce a doctoral thesis under East, which eventually took the title “The effects of inbreeding and crossbreeding upon development”. East found that Jones’s conclusions were in accord with the program he’d been developing for the past fifteen years, and he asked his student to collaborate with him on the writing of a

²³⁷ Crabb, *Hybrid-corn makers*, 81-88.

²³⁸ Kloppenburg, *First the seed*, 99. Edward M. East and Donald F. Jones, *Inbreeding and outbreeding: Their genetic and sociological significance* (Philadelphia: J.B. Lippincott, 1919), 224.

comprehensive new manuscript on genetics, evolution, eugenics, and breeding. The result was *Inbreeding and outbreeding: Their genetic and sociological significance*, a trade book published by J.B. Lippincott in 1919 in a series of “monographs on experimental biology”. *Inbreeding and outbreeding* was intended for a non-specialist audience; it did not advertise itself as inherently a work in agriculture or eugenics, but rather as a survey of the literature in the life sciences for the “non-biological worker interested in problems of human welfare” who might be turned on to “some new thoughts and pertinent suggestions in the compelling logic of the controlled experiments described throughout [its] pages.” Along with addressing the topic of the book’s title, East and Jones discussed structuring concepts of contemporary biology, such as Mendel’s Laws and the new understanding of chromosomes, in a simple manner that would have made *Inbreeding and outbreeding* accessible to readers who had not been following intellectual developments in the biological profession previously.²³⁹

Corn seed production makes a late appearance in the monograph, in the eleventh of thirteen chapters, entitled “The value of inbreeding and outbreeding in plant and animal improvement.” By the time it surfaces, so late in the text, it feels as though it belongs, like this was an inevitable endpoint for the story the two biologists were telling. We learn first of the origins of sexual reproduction in a far distant evolutionary past:

In the midst of strenuous competition for place, those organisms which were able to cross with others, at least occasionally, held such an advantage over those which were compelled to continue through one single line of descent that their descendants have persisted in greater numbers. They have dominated the organic world.

Later, we are told of how early human civilizations harnessed this fact of nature when they learned to crossbreed their livestock, though “it [was] hardly likely that their practice was of anything more than rule-of-thumb adopted after a variety of casual observations.” East and Jones promise instead to take us beyond these simple methods, to disregard “[u]ncontrolled experiments, casual observations of stock breeders, data on human marriages between near relatives,” in order to explain the operations of systems of breeding, the *why* as well as the *how*. We are, in other words, following the developmental path that Darwin described in *Variation under domestication* from natural to unconscious to methodical selection. “What is needed,” East and Jones suggest, “is controlled experimentation to determine just what inbreeding involves, and interpretation of the results in keeping with general biological knowledge. Darwin was the first to appreciate this.” The controlled experiments that they have in mind would have probably seemed obscure to many of the readers of their book, but they would have been very familiar to anyone who regularly attended meetings of the American Breeders’ Association: we encounter discussions of Helen Dean King’s rats, of George Shull’s corn in Long Island, of East’s own corn in Connecticut, of George Rommel’s guinea pigs at the USDA, of William Castle’s fruit flies.²⁴⁰

²³⁹ Crabb, *Hybrid-corn makers*, 89. East and Jones, *Inbreeding and outbreeding*, 7.

²⁴⁰ East and Jones, *Inbreeding and outbreeding*, 18-19, 35, 100, 118.

With natural selection equated to methodical selection equated to their own corn hybridization project, Jones and East freely mapped the results of their crosses in Connecticut onto an imagined evolutionary space free of the tracings of human history. Their breeding technique, which depended on intensive inbreeding, was just another natural force shaping populations:

The only injury proceeding from inbreeding comes from the inheritance received ... If undesirable characters are shown after inbreeding, it is only because they already existed in the stock and were able to persist for generations under the protection of more favorable characters which dominated them and kept them from sight. The powerful hand of natural selection was thus stayed until inbreeding tore aside the mask and the unfavorable characters were shown up in all their weaknesses, to stand or fall on their own merits.

The commercial corn breeder was merely facilitating a transformation that natural selection in time would have seen to; for creating weakened lines, inbreeding was “no more to be blamed than the detective who unearths a crime. Instead of being condemned it should be commended.” At the heart of this project is not merely a description of the force of natural selection but an *identification* with it, a will to see its judging of individuals and transformation of populations pushed forward. Note the similarities between their description of the evolutionary process and their description of the production of improved seeds:

Experiments with maize show that undesirable qualities are brought to light by self-fertilization which either eliminate themselves or can be rejected by selection. The final result is a number of distinct types which are constant and uniform and able to persist indefinitely. They have gone through a process of purification such that only those individuals which possess much of the best that was in the variety at the beginning can survive.

Here the breeder becomes the force of natural selection, choosing the “best” types for survival, the same as nature does. What is left unmentioned, however, is what constitutes the “best” types of individuals chosen for survival. When Darwin first published *Origin of species*, John Lindley in the *Gardeners' chronicle* almost immediately described plant breeding as speeding up Darwinian evolution, making natural selection happen faster. East and Jones continued to present themselves in the same way, conveniently hiding all the historical contingencies, the political economic sticks and carrots, the large institutions and the landscapes they were attempting to subdue, that had made one particular strain of plant “better” than the next. By conceptualization how breeding related to evolution in this way, they were also advocating a particular politics of agriculture.²⁴¹

There were other students besides Jones who wandered out of the Bussey and into the political economy of agriculture. Sewall Wright arrived in Jamaica

²⁴¹ *Ibid.*, 139-140, 216.

Plain in the middle of 1912 to study animal genetics. Raised in Galesburg, Illinois, he had come under the spell of evolutionary biology when he took a course his senior year at Lombard College taught by Wilhelmine Marie Key, who had taken her doctorate from the University of Chicago after studying in part with Charles Davenport. Key introduced him to the evolutionary and genetic debates of the first decade of the century, put him on a steady reading regimen of the classics of the Darwinian tradition, including Wallace's *Darwinism* and Vernon Kellogg's more recent *Darwinism to-day*. Using her connections, she arranged for Wright to spend the summer of 1911 at the Station for Experimental Evolution at Cold Spring Harbor, where he met many of the leading names in the discipline of genetics and toured George Shull's experimental corn field with its tall, uniform stalks. The following autumn, he matriculated at the University of Illinois in Urbana to undertake graduate studies in zoology. While there, he attended a presentation by William Castle on mammalian genetics over at the College of Agriculture. He was very taken with the kind of research that Castle described, attracted perhaps by the way Castle's experiments on selection in hooded rats spoke to broader questions in theoretical evolution, and after completing a master's degree in Illinois he followed Castle to Massachusetts.²⁴²

At the Bussey Institution, Wright distinguished himself from other students by his commitment to advanced statistical techniques. Even more than the experienced geneticists around him, he was a skilled practitioner of the biometrical approach; when a problem seemed insoluble by existing quantitative methods, he would forge new ones. While analyzing data on inheritance of traits in rabbits, he invented what would become known as the method of path coefficients, an incredibly useful way to "quantify the causal chains in an already definite causal scheme." Leslie Dunn later remembered Wright as a "very shy man," with a "great deal of timidity in speaking in public," with "no show of authority in what he says," absorbed in his own world. "His nights were spent in theoretical work and computation," Dunn recollected, and "[t]hinking in symbolic terms appeared to be a perfectly natural exercise for [him]."²⁴³

Along with his studies and his research, Wright was responsible for maintaining the Bussey's large guinea pig colony, which Castle had been tapping for his work in mammalian genetics for years. There were a wide array of inbred lines within it, with cavies of multiple colors and shapes. Most were derived from individuals that Castle had obtained from fanciers, and these were supplemented with populations that he had carried back from Peru and Brazil for hybridization experiments. Wright was as much an animal technician as he was a geneticist in these years; overseeing the colony was not a light commitment. "Wright did everything himself," according to Dunn, and "his colleagues regretted very much that he spent so much of his time worrying over whether he would have cabbages or carrots for his guinea pigs this week, and fighting disease, and working out technical methods for measuring skins."²⁴⁴

²⁴² Provine, *Sewall Wright*, 23-24, 27-29, 30-31, 65.

²⁴³ Provine, *Sewall Wright*, 77-80. "Oral History Transcript," pp. 150, 152-153. LCD-APS, Box 27, Folder: Oral History Transcript – Index.

²⁴⁴ Provine, *Sewall Wright*, 83-84. "Oral History Transcript," pp. 150. LCD-APS, Box 27, Folder: Oral History Transcript – Index.

Down the Eastern seaboard, another large colony of covies was at this time still being maintained in Beltsville, Maryland, where the Bureau of Animal Industry had relocated its work in dairying and animal husbandry to a 475-acre experimental farm in January 1911, just a mile away from a station of the Baltimore & Ohio Railroad and roughly twelve miles from the USDA's main administrative complex in Washington, D.C. These inbred guinea pigs, which comprised twenty-three distinct lines, were the descendents of those which George Rommel, by now no longer a husbandman but the head of a Division of Animal Husbandry, had begun inbreeding years before as an experimental prototype for a larger Bakewellian program with livestock. The colony of covies, the traits of its members carefully observed and recorded by technicians year after year, had generated a mountain of data, and Rommel's staff was at a loss for how to analyze it. Early inbreeding and self-fertilization projects had proceeded under the assumption that it was easy to define what it meant to be inbred; there were, on the one hand, experimental inbred lines and, on the other, control crossbred lines. But it quickly became apparent that different genealogies possessed differing intensities of inbreeding and crossbreeding, and, as Raymond Pearl, who was at the time working on a project to increase egg production in chickens at the Maine Agricultural Experiment Station, noted, "there seems not have been worked out any *adequate general method of measuring quantitatively the degree of inbreeding which is exhibited in a particular pedigree.*" Until these basic theoretical considerations were addressed, it would be fruitless to try to draw legitimate conclusions from complex inbreeding experiments.²⁴⁵

In December 1914, Rommel wrote to Charles Davenport in Long Island, asking him if he knew of any candidate who might "fill a vacancy in charge of our animal breeding work," hoping to find someone with "at least three years' experience ... in teaching or research ... in animal or plant genetics, in a college, university, State experiment station, or similar institution." Davenport recommended Sewall Wright along with five other candidates. A similar inquiry to William Castle, who had visited the colony at Beltsville in April 1913, likewise turned up Wright's name. Rommel formally offered the young geneticist a position as experimental husbandman at the Bureau of Animal Industry, and Wright, who was finishing his doctoral thesis at the time and had only one other offer of employment, from a small Southern college, accepted. In September of 1915, he left Jamaica Plain for Washington.²⁴⁶

Edward East, cheerful soul that he was, predicted that Wright would be isolated in Washington from professional genetics. It's true that Wright, when he arrived in the capital, found few academic geneticists in the area, but the relocation did put him at the hub of a large network of connections that few cities could have made available. Through Guy N. Collins, a USDA plant breeder, he was introduced socially to the top scientists of the Bureau of Plant Industry. His

²⁴⁵ Houck, *The Bureau of Animal Industry*, 219-222. Raymond Pearl, "The measurement of the intensity of inbreeding," *Maine Agricultural Experiment Station bulletin* 215 (August 1913): 124. For context on Pearl's work, see Kathy J. Cooke, "From science to practice, or practice to science? Chickens and eggs in Raymond Pearl's agricultural breeding research, 1907-1916," *Isis* 88, no. 1 (1997): 62-86.

²⁴⁶ George Rommel to Charles B. Davenport, December 29, 1914. CBD-APS, Folder: Correspondence with George M. Rommel. Charles B. Davenport to George Rommel, January 4, 1915. CBD-APS, Folder :Correspondence with George M. Rommel. Provine, *Sewall Wright*, 99.

job put him into contact with the organizers and most active members of the Society of Animal Production, and he regularly attended the International Livestock Show with which it was associated. The quantitative analysis for which he became noted had many uses at the Department of Agriculture beyond animal breeding. Henry A. Wallace, the son of the Secretary of Agriculture at the time and himself a future Secretary of Agriculture, consulted with Wright regularly, having a keen interest in his method of path coefficients. In order to keep in touch with developments in academic genetics, Wright regularly participated in meetings of the American Association for the Advancement of Science. Like Darwin at Down, Wright was enmeshed in a web of diverse relationships in agriculture, academic science, and animal and plant production, able from his position to see how these groups might speak to each other.²⁴⁷

There was, though, a major difference between Darwin's situation and Wright's. While the horticulturalists and the breeders found Darwin a compelling thinker, selectively extracting from his work ideas that suited them, they generally considered him a mediocre breeder. When he visited the orchid cultivators or the pigeon fanciers, it was generally clear who the expert was and who the dabbler. Wright, on the other hand, bore the stamp of approval of the modern research university and could speak officially in the voice of the Department of Agriculture. While he was with the USDA, he spent about half his time on research in Beltsville and half his time in the main administrative building off the Mall, where he would answer letters from the public and make recommendations on behalf of the Department. His correspondents were diverse. Some were commercial livestock breeders, some were experiment station personnel, some were eugenicists, some raised small animals for biomedical research, many were cranks. The secretary of a livestock company in Idaho found discrepancies in the literature on scientific breeding and wrote to ask "what the prevailing opinion is in regard to inbreeding in livestock and poultry." A dairy breeder in Minnesota sent in the pedigrees of his herd because he had "been inbreeding with apparent success as far as size is concerned but with a great reduction in milk yield," and he was looking for a suggestion on what to do next. They were seeking from Wright professional advice, trusting his expertise as a natural scientist. He was a conduit of ideas from the genetics profession to the general public. The knowledge he dispensed, which came from the manipulation, observation, and description of nature, ended up becoming a blueprint for the creation of new types of organisms nationwide.²⁴⁸

When he wasn't answering letters, Wright returned to what had initially attracted him to the USDA, the inbred guinea pigs in Beltsville. The trip from Union Station downtown to Beltsville, Maryland, by train took around half an hour. When he first arrived in 1915, he found that the wooden pens in which the cavies were paired had gotten "into rather bad condition ... and had become infested with bedbugs," so he had them replaced with metal cages. The guinea

²⁴⁷ Provine, *Sewall Wright*, 99-104.

²⁴⁸ *Ibid.*, 107-111. Charles Hardy to U.S. Department of Agriculture, Division of Animal Husbandry, April 12, 1923. E. Masson to Sewall Wright, received July 26, 1922. These two letters, along with many, many more requests for information, can be found in USDA-BAI, Central Correspondence, 1913-1953. Animal Husbandry, 1922-26. Box 522. Folder 4.691: Inbreeding, Crossbreeding, Selection, etc. – July 1922 – Dec. 1926.

pigs' diets varied by season, "[g]reen oats and fresh grass in spring and summer, and cabbage and kale in fall and winter." Individuals were divided into pairs as soon as they had finished weaning, on average thirty-three days after birth, though in some unavoidable cases "[f]emales occasionally [were] sufficiently mature at 33 days to bear litters sired by their own sire." As he had at the Bussey, Wright made an obsession of the guinea pigs in Beltsville; Dunn suggested that "for his temperament ... it takes a certain amount of physical, mechanical business to the keep the mind on its track, and produce ... freedom."²⁴⁹

Analyzing the data the project was producing required Wright to develop a new mathematical formula for the degree of inbreeding in a population, and it also forced him to develop a new quantitative theory of inbreeding, one that depended on the system of path coefficients he'd developed earlier. During his years in Beltsville, Wright thought his way around his inbred cavies, literally using them as guinea pigs to test his speculations and to refine his mathematical models. They were, however, also to him what the white rats at the Wistar Institute were to Helen Dean King: a component of an industrial production process onto which other imagined meanings, given the right metaphor, might be projected.²⁵⁰

The first metaphor that Wright needed to apply to his guinea pigs, the one that he was being employed by the Bureau to engineer, was the one that turned them into miniature cattle and swine. This was why George Rommel had started the experiment in the first place, and it was a natural outgrowth from Wright's other duties as a federal husbandman. After immersing himself in existing publications on livestock and considering what had been written in light of his own experiences as a geneticist, Wright penned a manuscript that became "Principles of livestock breeding," published as a scientific bulletin of the USDA in December 1920. Rommel adapted Wright's manuscript into a new form "written in simple language for the man who breeds farm animals, who wants to learn the rudiments of the science of breeding," and published it, with "[c]ontroversial subjects ... avoided so far as possible," as a popular farmers' bulletin in November 1920.²⁵¹

These bulletins went into national circulation through the Department's web of connections. When Wright and his successors received general letters of inquiry from the public about breeding, they often responded by sending out copies of one of the two bulletins. Jay Lush, an animal breeder at the Texas Experiment Station, requested multiple copies of "Principles of livestock breeding" so that he could use them as a textbook for a "general course in genetics for [around eighty] agricultural students." One of Wright's later bulletins, a detailed summary of the results of the guinea pig inbreeding project, caught the attention of animal breeders in the same way the work of East, Shull,

²⁴⁹ On the length of time to travel from Washington to Beltsville, see Sewall Wright to Emilia Vicari, May 4, 1925. USDA-BAI, Central Correspondence, 1913-1953. Animal Husbandry, 1922-26. Box 520. Folder 1.691: Experiments – Guinea Pigs, Rabbits, etc. – Miscellaneous – July 1922 – Dec. 1926. Sewall Wright, "The effects of inbreeding and crossbreeding on guinea pigs," United States Department of Agriculture bulletin 1090 (November 1922): 9.

²⁵⁰ On Wright's attempt to produce a new theory of inbreeding, see Provine, *Sewall Wright*, 126-142.

²⁵¹ George M. Rommel, "Essentials of animal breeding," United States Department of Agriculture farmers' bulletin 1167 (November 1920): 2.

and Jones had excited plant breeders. “I am glad,” one corporate farm owner wrote to his chief breeder, “you regard the Wright bulletins as distinctly encouraging ... It begins to look as though poultry were a favorable subject for our work.” The metaphor of scientific animal breeding that Wright imposed upon his guinea pigs was accepted and helped steer many breeders.²⁵²

With new mathematical tools at his disposal, Wright next turned his attention to the paradigmatic founding figure of scientific animal breeding, Robert Bakewell. He collected the pedigree charts of the Duchesses, a line of relatively inbred Shorthorns that “became the aristocrats of the cattle world,” and analyzed the breeding regimen that had been used upon them with his new definition of inbreeding and the statistical methods he had developed to measure its degree of intensity. These held a special importance, for the originator of the family, Charles Colling, had proceeded only after he made “a prolonged study of Bakewell’s methods at Dishley in 1783”; the Duchesses were perhaps the longest running lineage of cattle sculpted by the Dishley system. After determining the degree of relationship of various animals in the pedigrees, Wright concluded that Thomas Bates, who had continued breeding the Duchesses in the mid-nineteenth century, had actually inherited from Colling animals that were only about 40% inbred; for the rest of his life, he continually maintained that degree of inbreeding by occasionally introducing unrelated animals into the herd, though this was not widely recognized. Wright was beginning to understand precisely how Bakewell and his descendents were able to change their herds.²⁵³

This was information of considerable value to livestock breeders. For Wright, though, it held a special significance of another sort. Wright opened his reinterpretation of Bakewell’s work not with a discussion of its political economy or significance for modern animal industries, but with a brief history of domestication and breeding. Even among primitive peoples, he notes, “there must have been modification of the wild types through the retention of those animals which were most tractable,” but real innovations in breeding were held back by an inability to understand what was happening: “beliefs, partially true or false, such as those concerning the injurious effects of matings between close relatives ... and so forth, contributed to the traditional lore of breeding.” Robert Bakewell and his Dishley Society were the first true innovators in breeding practice (no mention was made of the larger political, economic, and institutional dimensions of Bakewell’s work), though they succeeded through trial and error and failed to understand what they were really doing. Now, though, “[w]ith accurate knowledge of the principles of heredity ... genetics has an important contribution [to make] to practical breeding in the insight which it gives into the results of the long-known mass methods of breeding.” This is, certainly, a pitch for scientific breeders to pay attention to genetics, but it’s also a set-up for Wright to impose an entirely different meaning on his guinea pig research. The narrative Wright sketched was a familiar one, unconscious selection bleeding into methodical selection, and the unstated conclusion to the narrative was that a

²⁵² Jay L. Lush to Sewall Wright, September 19, 1921. JLL-ISU, Box 15, Folder 15/1: Wright, Sewall – Correspondence, 1921-27, 1929. E. Parmalee Prentice to H.D. Goodale, January 13, 1923. HDG-APS, Folder: Correspondence with Prentice, Ezra Parmalee.

²⁵³ Sewall Wright, “Mendelian analysis of the pure breeds of livestock, II: The Duchess family of Shorthorns as bred by Thomas Bates,” *Journal of heredity* 14 (1923): 405, 407, 418.

conscious understanding of methodical selection became an understanding of natural selection. This was Darwin's narrative from *Variation under domestication*, which had a renaissance of sorts at the Bussey. Even without mentioning natural selection in the paper, Wright has himself taken the crucial step from unconscious to methodical selection by demystifying Bakewell. For those familiar with the Darwinian tradition, it would be an inevitable conclusion. "Principles of livestock" opened with a similar underlying narrative structure, but Rommel cut it from "Essentials of animal breeding," perhaps not seeing any particular relevance in it to the practical breeder.²⁵⁴

In the evenings or in his daydreams while feeding his guinea pigs, Sewall Wright was imagining his selecting and sorting of individuals as metaphorically the force of nature. He had not yet published on evolution in nature, but in 1925 he was completing a long type-written manuscript that was meant to be a comprehensive account of how populations evolve in nature. As Will Provine notes, "[r]easoning from his theory of animal breeding to his theory of evolution in nature, Wright proceeded upon the plausible but wholly unproved assumption that evolution in nature proceeded primarily by the three-level process utilized by the best animal breeders: (1) local mass selection and inbreeding, (2) dispersion of the more successful local populations, and (3) transformation of the whole species or breed." Without an Alfred Russel Wallace to challenge his metaphor, Wright could operate with an understanding that had been shared by the majority of geneticists since the institutional structure of the American Breeders' Association and the active collaboration of its members had made it a communal norm. When Wright was offered a job as a professor in the Department of Zoology at the University of Chicago in February 1925, he was tempted by the move into the university, where he could publish in the context of natural evolution rather than scientific breeding, but he had one major concern:

The most serious effect of a change ... would probably be in the interruption to the problems which I have under way ... Guinea pigs are of course rather slow and expensive material. It takes years to work out a problem or even to get ready to start on it properly. I have 8-factor recessive and 8-factor dominant strains which took a good many years to develop ... The five inbred families, with behavior and characteristics analyzed since 1906 are also ideal for certain experiments.

Wright accepted after it was arranged that the guinea pigs could come with him to Chicago, where he installed them in the basement of the chapel.²⁵⁵

In the late summer of 1932, Wright presented a paper at the Sixth International Congress of Genetics in Ithaca, New York, that would become a foundational document of both the discipline of theoretical population genetics and the Modern Synthesis. The conference organizer, Edward East, knew that Wright's abstruse thinking was often difficult for even professional geneticists to follow, so he asked his former student at the Bussey to give a presentation that

²⁵⁴ Sewall Wright, "Mendelian analysis of the pure breeds of livestock, I: The measurement of inbreeding and relationship," *Journal of heredity* 14 (1923): 339-340.

²⁵⁵ Provine, *Sewall Wright*, 161, 236.

got to the essence of his work but that could be appreciated by most of those in attendance. Wright opened with a nod to East's work on the evolution of sexual systems, and then outlined the "roles of mutation, inbreeding, crossbreeding, and selection in evolution" in a form that was clear and comprehensive. The paper soon spread throughout the community of academic biologists, but it was accessible to others. Shortly after the conference, Wright received a letter from an industrial poultry breeder who had been at the conference too: "I want to congratulate you upon the splendid paper you presented Tuesday morning. The whole Congress was on a very high plane and there were a number of papers each of which might have marked the high point of an ordinary Congress, but to me your paper topped them all." It would, in fact, have been surprising if scientific breeders could *not* understand and appreciate Wright's work. Beneath the abstractions in his talk were Rommel's pens of inbred guinea pigs. From a perspective slightly off from the academic geneticists' Wright's presentation might look like a patchwork of repurposed breeding fragments woven harmoniously together to create a new quilt, eerie fragments of a new world dawning outside the limited confines of *skholè* and being rationally ordered within its walls.²⁵⁶

²⁵⁶ Provine, *Sewall Wright*, 283-287. H.D. Goodale to Sewall Wright, September 3, 1932. HDG-APS, Folder: Correspondence with Sewall Wright.

Conclusion

On January 27th, 1940, twenty-four research scientists from around the United States gathered in Washington, D.C., to discuss the status of the nation's stock of inbred animals and plants. As the conference opened, each scientist in turn described the organisms that his facility maintained; the litany that unfolded sounded like the inventory of a strange and fantastic menagerie. There were, of course, many mentions of the small animals that had proliferated in the early years of experimental genetics. By now, the Wistar Institute had seventeen different strains of inbred rat on hand, and one of Helen Dean King's albino lines had reached its 103rd inbred generation. The Jackson Laboratory at Bar Harbor counted twenty-four or twenty-five different types of inbred mice. Alfred Sturtevant told the gathering that in Pasadena, where Thomas Hunt Morgan would soon retire, there were over 1,500 different strains of fruit fly, representing as many as forty-nine different species. Spread across its many research station affiliates, the Bureau of Animal Industry had access to sixty-four lines of sheep, forty-six lines of pigs, and ten lines of chickens. Along with these prosaic creatures, the researchers also rattled off the names of many other less common animals that had fallen under the regime of intensive inbreeding: axolotls, opossums, fish of varying sorts, doves, rabbits, a strain of hermaphroditic pigeons. One entomologist even passed around some preserved specimens from a population of inbred pygmy locusts. And as if these were not enough, the researchers expressed a desire for new strains to be created from other animals: dogs, goldfish, cats, monkeys, and apes.²⁵⁷

There was a consensus among those gathered that the great advantage of these strains was that, in the words of Milton Veldee, they would remain "uniform throughout the particular experiment or in duplicate experiments." Clarence Little voiced a concern that also appeared to be shared by most: "The biggest handicap at present is lack of assurance that the sources of supply will be maintained." Keeping these inbred lines going was difficult, but not because of the inherent biology of the animals and plants involved; these were fiscally expensive programs, requiring not only space and supplies but a great application of careful labor. "The maintenance of a given strain by a given laboratory," noted a medical researcher, "is, in a sense, a commitment." Several suggestions for collective action, perhaps with the support of national organizations, were floated. Like Robert Bakewell and his partners in the Dishley Society had one hundred and fifty years before, the committee found itself struggling with the political and economic constraints that determined whether a pure strain of inbred organisms would be perpetuated.²⁵⁸

Less than a mile away from the conference, just off the National Mall, stood the central administrative complex of the Department of Agriculture. We cannot say with certainty what the scientists at the conference would have made

²⁵⁷ "Conference on maintenance of pure genetic strains," a transcript of a meeting held under the direction of the Division of Biology and Agriculture of the National Research Council, CPF-NAA, Folder: Committee on the Maintenance of Pure Genetic Strains, 1941-1945. Special thanks to Bonnie Clause for providing this reference.

²⁵⁸ *Ibid.*, 8-9, 11.

of the USDA had they passed it while taking a walk during a break in proceedings, but we do know what struck Frank Engledow, a young agricultural scientist who toured the United States on fellowship, when he visited Washington, “capital no less of agricultural science than of government,” in 1924. Engledow found it difficult to even fathom the scope of the institution; “[l]iterary impressions alone,” he thought, could “convey a striking idea of [its] vastness ... It occupies, in part or wholly, forty-eight widely-dispersed buildings.” Two aspects of the Department’s scientific work seemed especially noteworthy to him. First, it had assumed a commanding oversight role for agricultural science, with a devoted “corps of specialists, drawn from all states, who travel widely in conducting their own work or supervising that of others” and whose “presence with and in the central administration is an interesting feature.” Second, it had, especially in its Bureau of Plant Industry, taken on a “frankly and rather inflexibly economic cast. Its staff is very well posted with the economic situation and tends in the first instance to grasp at the immediate economic possibilities of new theories and facts.”²⁵⁹

In January 1940, the Secretary of Agriculture, Henry A. Wallace, was nine months away from resigning his position so that he could run for Vice President on a ticket with Franklin D. Roosevelt. He had headed the Department of Agriculture through most of the Great Depression, and against scattered opposition had firmly advocated for an expansion in scientific agricultural research. Despite the fact that most scientific breeding programs were attempting to increase crop yields while the crisis in agriculture was primarily driven by overproduction, Wallace argued that further scientific research could solve the dire problems facing rural America. In 1935, the Bankhead-Jones Act, described by Jack Ralph Kloppenburg as “the product of an articulate scientific elite allied with private interests and represented by agricultural journals and corporations,” provided \$20 million to the Department for scientific research. In 1936 and 1937, the Secretary’s Committee on Genetics published a survey of agricultural genetics programs in the United States and their recent findings, a map by which to allocate new resources. Here was an agency with far greater means to perpetuate inbred organisms than the academic geneticists and their colleagues engaged in medical research, who were struggling to pull together any form of cooperation.²⁶⁰

Henry Wallace was no stranger to the issues surrounding the inbreeding and hybridization of crops and livestock. While he was an undergraduate at Iowa State College, he regularly read the publications of the American Breeders’ Association, and as early as 1913 he had begun planting his own experimental inbred corn plots on his family’s large farm. He was personally acquainted with both Edward East and Donald F. Jones, and in 1919 Jones provided him some inbred corn lines that were being kept at the Connecticut Agricultural Experiment Station so that he might found his own strains. These lines had a long history; they were derived from the old yellow Leaming stock that East had taken without permission from the University of Illinois. In May 1920, Frederick Richey, a breeder with the Department of Agriculture, provided him with some

²⁵⁹ F.L. Engledow, “Report on a tour in North America, 14 July – 12 September 1924, undertaken by means of a Travelling Research Fellowship in Plant Genetics awarded in 1924.” FLE-SJC, Box 4, pp. 3, 21-22.

²⁶⁰ Kloppenburg, *First the seed*, 84-88.

inbred lines of deep-red Bloody Butcher corn as a public service. Wallace crossed these Leaming and Bloody Butcher inbreds, and he sold one of the resulting lines in 1923 to the Iowa Seed Company, the first company to bring hybrid corn into mass production for sale; the first advertisement for the strain boasted of its association with “Dr. East, now of Harvard.” In 1926, Wallace himself founded the first corporation devoted only to hybrid corn production, the Hi-Bred Corn Company, which would soon become Pioneer Hi-Bred. And his interests were not limited to plants; in 1928, he wrote to Dunn at Storrs, introducing himself as someone who has “been cooperating more or less informally for a number of years with Dr. D.F. Jones ... in the inbreeding of corn,” and requested some inbred poultry because “[f]inally I am in position to do some of this same kind of work with chickens.” Wallace was creating the kind of regional hybrid seed production facility that East had envisioned fifteen years before, and he was already devising plans to replicate the program with animals.²⁶¹

What East had not foreseen, however, is what might happen when these regional seed companies received the backing of the powerful federal agricultural bureaucracy. In 1920, Henry C. Wallace, the younger Henry’s father, became Secretary of Agriculture in the Warren Harding administration. In 1922, and with the counsel of his son, he replaced the director of corn breeding research at the Bureau of Plant Industry, a prominent critic of hybrid corn, with Frederick Richey, a breeder to whom his son was close. Under Richey’s watch over the next decade, the Bureau encouraged corn inbreeding among its scientists and at the state experiment stations with which it collaborated, and the number of inbred corn lines in the Midwest rapidly proliferated as the search for strains suitable for use in the double-cross method intensified. Adoption of the new seed depended as well on policies of the federal government. Throughout the 1920s, farmers showed little interest in this new heavily-endorsed seed, preferring to retain the diverse seed they had traditionally produced on their own farms, but this situation changed dramatically during the Great Depression. Adoption of the new hybrid corn increased exponentially. What drove this switch was not so much the improvement in seed quality that the USDA’s patronage had brought in the past decade, but rather the subsidy policies of the Agricultural Adjustment Administration. As the USDA paid farmers to reduce their acreage, those farmers responded by planting high-density, highly productive crops on their remaining land, and hybrid corn had been custom-engineered for just this mode of intensive industrial agriculture. As Kloppenburg concludes, the rapid spread of hybrid corn “was the product of political machination, a solid decade of intensive research effort, and the application of human and financial resources that, as breeder Norman Simmonds writes, ‘must have been enormous by any ordinary plant breeding standards.’”²⁶²

By the end of the Second World War, the Department of Agriculture had accomplished with hybrid corn what Archibald Shamel and the Bureau of Plant Industry had attempted decades earlier on a smaller scale with his improved

²⁶¹ Crabb, *Hybrid corn-makers*, 145-147, 152-153, 157-158. The contract between Henry Wallace and the Iowa Seed Company may be found in HAW-UI, Box R2, Folder: Corn – Hi-Bred Corn Company. Henry A. Wallace to L.C. Dunn, December 6, 1928. LCD-APS, Folder: Correspondence Wallace, Henry A.

²⁶² Kloppenburg, *First the seed*, 102, 104, 119.

tobacco in the Connecticut River Valley. First seizing control of the plant itself through intensive self-fertilization, the USDA had next engineered a political and economic juggernaut that broadcast its seeds into the landscape and created bonds of dependency between farmers and corporate growers and the experimental corn production plots overseen in Washington. This was a new geography, replacing farmers' traditional retention of their own seed and leapfrogging any imagined system of small local breeding concerns; it was the Dishley Society on a continental scale. It resolved the long-standing struggle between urban and agrarian forces on terms wholly favorable to the new cities and the corporate interests nestled in them. When these seed lines later passed out of the hands of the federal government and into the hands of private companies like Pioneer and Monsanto, decision-making moved from Beltsville to corporate board rooms, but the fundamental geography that the Department of Agriculture had forged, vast agricultural production linked to an experimental center, remained in place.

This was a prototype for the nature of the future. As the human population multiplied in the last half of the century and wilderness fell before ever increasing cultivation, this was how those new agricultural spaces would be filled, near-clonal standardized lines replacing the long-evolved diverse ecosystems that had preceded them, the promise of the Green Revolution. American biologists watched this unprecedented transformation of nature from front-row seats. Many were invited to the drawing board to help design the blueprints. Some even set up the drawing board itself.

The members of the first generation of American geneticists were, like East, frequently children of the Corn Belt. They had come from rural America, from crossroad towns in Kansas, fading family farms in southern Illinois, the close-cropped cornfields of central Iowa, but they had used the university system to move into that other America, into a new social class, into new life circumstances. E. Parmalee Prentice, the wealthy owner of Mount Hope Farm in Williamstown, Massachusetts, knew when he hoped to attract the geneticist Hubert Goodale to his chicken breeding operation how to advertise the home he was going to provide: "The house I speak of," he assured Goodale, "is in the college neighborhood very near the K A Society house. Your surroundings would be professorial – not agricultural." By the end of the 1920s, when they had built successful careers in the nation's most prestigious universities, these geneticists wanted to see themselves as pure scientists, as living in the free space of untainted *skholè*, but their discipline was unavoidably attached to the governmental and corporate regime that was reordering the relationship between rural and urban America. "We must," Barbara Kimmelman has emphasized, after surveying the early history of geneticists in several European countries and the United States, "stop thinking of the agricultural context as convenient but essentially incidental to the disciplinary development of genetics and recognize that context as materially constitutive of the discipline itself." Genetics began as a bud off the branch of scientific industrial breeding, not the

other way around, however much the geneticists would later wish to forget this fact.²⁶³

What ultimately allowed the geneticists to distinguish themselves from the corporate and public breeders, to carve out a space that could be recognized by others as pure science, was evolutionary theory. The quest to produce a comprehensive theory of nature divorced from human strivings, a complete theory that might even eventually *explain* human strivings, vested their work with new meaning, allowed them to perform as minds without bodies. It charged them with what Bourdieu describes as a “dream of omnipotence, which tends to arouse fits of bedazzled identification with great heroic roles,” a dream that blinded them from “the limits of thought and of the powers of thought” and allowed them to “overstep the limits of a social experience that [was] necessarily partial and local, both geographically and socially.” By entering into a scholarly conversation of long-standing that had originally emerged out of the tradition of natural history, geneticists could make a claim on the space of *skholè*, could see themselves as professional academics.²⁶⁴

The Founding Father with whom this generation identified was Charles Darwin, not so much the naturalist Darwin of *Origin of species* or the Darwin who pondered human origins in *The descent of man*, but rather the naturalist-turned breeder of *Variation under domestication*. The community was, like any, divided into factions, especially during its first decade; some associated themselves with the mutationism of De Vries, some thought a biometric approach was the way forward, and others, who have in retrospect been termed the ‘Darwinists,’ still stuck with the notion that natural selection drove evolution. What these labels hide, though, is that virtually everyone involved in the genetics community considered themselves Darwinists. They had all, almost across the board, even the “neo-Darwinists,” tossed out the complicated mess of theoretical ideas they found in *Variation under domestication*, but they retained its historical material and its vision. Theirs was not the Romantic Darwin who had sailed around the world aboard the *Beagle*, but the gentleman-scholar who retained his rank and class privilege even as he absorbed knowledge from the fanciers and breeders and husbandmen, setting up elaborate experimental simulations at Down of what was to his contemporaries real labor.²⁶⁵

Darwin provided the evolutionary biologists and scientific agriculturalists of the early twentieth century with an historical vision and a crucial metaphor. It was a metaphor that could turn scientific breeding, despite its deep entrenchment in the political economy of the age, into natural selection, a force that transcends humanity itself. It could seize an existing material reality and impose upon it a new imagined meaning. It is a testament to this metaphor’s power that it still appears to us today, unlike so much else of Darwin’s theory, as so uncontroversial. It wasn’t as widely accepted while Darwin lived, at a time

²⁶³ Ezra Parmalee Prentice to Hubert Dana Goodale, January 29, 1923. HDG-APS, Folder: Prentice, E. Parmalee #2, 1923 (Jan.–Feb). Barbara Kimmelman, “The influence of agricultural practice on the development of genetic theory,” *Journal of the Swedish Seed Association* 107, no. 4 (1997): 186.

²⁶⁴ Bourdieu, *Pascalian meditations*, 2.

²⁶⁵ On the ubiquity of identification with Darwin among those researching evolution and genetics, see Marsha L. Richmond, “The 1909 Darwin celebration: Reexamining evolution in the light of Mendel, mutation, and meiosis,” *Isis* 97 (2006): 447-484.

before the urban world had conquered the rural hinterland with its system of industrialized agriculture. We can describe metaphors as powerful because they are neither passive nor neutral; they are, as Greil Marcus once argued, “transformations, proofs of the arbitrary nature of language, grants of mystery to ordinary things – they are in other words incipient utopias.” Darwinism was never *just* an academic discourse; it couldn’t survive unless it linked itself to the world outside *skholè*, unless it joined with scientific breeding and all its accompanying ideological baggage to become a living transformative practice, unless it sought to refashion the world. The rapid progress of evolutionary studies in the United States in the first third of the twentieth century was fueled by its entanglement with the industrial agriculture complex, the fact that theorists’ “intellectual powers ... [were] exercised in the same direction as the immanent tendencies of the social world,” that they “redouble[d] ... the effects of the forces of the world, which [were] expressed through them.”²⁶⁶

In 1919, Edward Murray East approached the 15,000 acre Sibley Estate on the prairie in central Illinois and personally offered to begin mass-producing hybrid corn; he mailed a copy of *Inbreeding and outbreeding* to explain and advertise his proposal. We have no record of what exactly East had in mind, but we know he was not especially concerned about what impact his program might have on the prairie. “The one thing modern science has done,” he later wrote, “whether it be building bridges or producing new breeds of apples, is to teach us how to save time.” His inbred strains would “be obtained by methods similar to those followed by nature, but ... in shorter time.” What he did find remarkable, though, was a gathering of farmers’ representatives that Warren Harding convened in Washington in 1922. These agriculturalists seemed eager to cooperate with the establishment, to listen seriously to the recommendations that the Department of Agriculture urged upon them. “To those who remembered the undisciplined wild scream of populism some thirty years ago,” East happily observed, “it was an almost unbelievable evolution.”²⁶⁷

²⁶⁶ Greil Marcus, *Lipstick traces: A secret history of the twentieth century* (Cambridge: Harvard University Press, 1989), 369. Bourdieu, *Pascalian meditations*, 3.

²⁶⁷ Crabb, *Hybrid-corn makers*, 90. Edward M. East, *Mankind at the crossroads* (New York: Charles Scribner’s Sons, 1923), 166, 189.

Abbreviations

- ARD-WIL: Annual Reports of the Director, Wistar Institute Library. Philadelphia, Pennsylvania.
- BLC-JIC: The Bateson Letters Collection, John Innes Centre. Norwich, United Kingdom.
- CAF-CIW: Carnegie Administration Files, Carnegie Institution of Washington. Washington, D.C.
- CBD-APS: Charles B. Davenport Papers, American Philosophical Society. Philadelphia, Pennsylvania.
- CHS-UIA: Charles H. Shamel Papers, University of Illinois Archives. Urbana, Illinois.
- CPF-NAA: Central Policy Files, National Academies Archive. Washington, D.C.
- DMC-CUL: Darwin Manuscript Collection, Cambridge University Library. Cambridge, United Kingdom.
- FLE-SJC: The Personal Papers of Sir Frank Leonard Engledow, St. John's College, University of Cambridge. Cambridge, United Kingdom.
- GHS-APS: George Harrison Shull Papers, American Philosophical Society. Philadelphia, Pennsylvania.
- HAW-UI: Henry Agard Wallace Papers, Special Collections of the University of Iowa Libraries. Iowa City, Iowa.
- HDG-APS: Hubert Dana Goodale Papers, American Philosophical Society. Philadelphia, Pennsylvania.
- HMC-UIA: Historical Material Collection, 1904-1975, University of Illinois Archives. Urbana, Illinois.
- JLL-ISU: Jay L. Lush Papers, Iowa State University Library. Ames, Iowa.
- LB-UIA: Letterbooks, 1899-1919, University of Illinois Archives. Urbana, Illinois.
- LCD-APS: Leslie Clarence Dunn Papers, American Philosophical Society. Philadelphia, Pennsylvania.
- SLC-UIA: Student Ledger Cards, University of Illinois Archives. Urbana, Illinois.

- SW-APS: Sewall Wright Papers, American Philosophical Society. Philadelphia, Pennsylvania.
- USDA-BAI: Record Group 17 – Records of the Bureau of Animal Industry. The United States National Archives II. College Park, Maryland.
- USDA-BPI: Record Group 54 – Records of the Bureau of Plant Industry, Soils, and Agricultural Engineering. The United States National Archives II. College Park, Maryland.
- WCP-APS: William Ernest Castle Papers, American Philosophical Society. Philadelphia, Pennsylvania.

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