— SCIENCE IS FUNDAMENTALLY DIFFERENT from art, it is often claimed, because the sensibility of the scientist isn't involved. Had X not made the discovery, Y would eventually have gotten to the same place. Made into a caricature, this is the view of scientists in white coats engaged in a selfless quest for truth. At the opposite extreme is the postmodernist idea that science is a social construct—that no independent, objective reality exists outside of one's perceptions. While both views are clearly absurd, I confess that I am more sympathetic with the former than the latter. The consequences of denying the existence of an objective scientific reality were dramatically illustrated in the Soviet Union in the period between 1933 and 1953 when the ideas about genetics that had been built up over the previous century were undermined and then outlawed, plunging Soviet agriculture into free fall.

While it may not matter in the long run how a scientific idea came into being, nonetheless learning about the thoughts, feelings, and interactions of

the people who worked on it can illuminate the science in unexpected ways. This may be particularly true of the study of heredity, which is both the most mathematical and abstract of the life sciences and at the same time the most personal. Intimately connected with the origin of statistics, the early study of inheritance also touched on age-old philosophical questions about free will and determinism, the relationship of parent and child, and the extent to which human beings can be reduced to the sum of their parts. Fueled by their concern with these elemental themes, I believe, the early geneticists were a particularly passionate group, pathologically competitive in some instances and utterly selfless in others, prone to intense loyalties as well as overwhelming hatreds, singularly idealistic and ruthlessly pragmatic.

For these reasons I have chosen to take a biographical approach to the history of genetics, and my research was greatly facilitated by the fact that many of the players who made the key discoveries were skilled, prolific letter writers. Darwin's epistolary output was staggering, as was that of his cousin Francis Galton, who was in many ways the pioneer of the modern theory of heredity. Similarly, Galton's disciples Frank Weldon and Karl Pearson, and their nemesis William Bateson, all of whom played leading parts in the battle over Mendelism, were devoted correspondents. Most of the papers of Gregor Mendel himself, who managed to divine the essence of modern genetics in 1865 working alone in a monastery in Moravian Silesia, were destroyed, but enough survived to give a sense of the man behind the scientist. The sad story of Hugo de Vries, who played a key role in the restitution of Mendel's work in 1900, can be pieced together from De Vries's correspondence with Darwin, Bateson, and others. The standard view of the post-1900 development of genetics, which largely took place in the lab of Thomas Hunt Morgan at Columbia, was shaped by Morgan's favorite student, A. H. Sturtevant, in his 1965 *History of Genetics*, but an account entirely at odds with the one in that book later came to light in a few surviving letters on the topic by Morgan as well as in the remembrances of several other geneticists who were members of the fly room. This alternative view is given full expression in the correspondence of Morgan's most celebrated student, Hermann J. Muller, which is held at the Lilly Library at In-

diana University, and in a previously unknown collection of 550 letters donated by Muller's son to the Cold Spring Harbor Laboratory Archives.

From these letters and other primary source material, it is possible to trace the idea of the gene as it left one consciousness and entered another. Of course many people were thinking about the same thing, and it is necessary to ignore a host of offshoots in the service of following the main thread. Sometimes ideas were passed from one scientist to another just as the originator intended. Other times, it was a terrible confusion in one person's mind that resulted in a brilliant illumination in the next. Still other times, those who found the correct path did so entirely by accident after setting off on the wrong one or, conversely, began with a brilliant flash of insight and proceeded to wander deep into the wilderness.

The story of the gene begins with Darwin's arcane and deeply flawed "provisional hypothesis of pangenesis," which reflected his recent conversion to another highly problematic theory, that of Lamarckian inheritance. Despite its many problems, Darwin's theory posited the existence of microscopic hereditary particles that would play a crucial role in the development of the modern ideas about the gene. No sooner had Darwin proposed his model than his cousin Francis Galton commandeered it to support a theory of inherited intelligence that was itself based on a host of erroneous assumptions and ad hoc arguments. Nonetheless, Galton's hereditary theory was much closer to the truth than that of his far more emotionally balanced, reasonable, and steadfast cousin.

The Dutch botanist Hugo de Vries also tried to enlist Darwin's theory of pangenesis in the service of his own theory, hoping to displace Darwin as the central figure in modern biology. In the course of his work De Vries stumbled on Mendelism, but he failed to properly appreciate that he had uncovered the discovery of the century and instead spent the remainder of his life devaluing Mendel's achievement and promoting his own erroneous theory, the mutation theory, in its place. It was the English zoologist William Bateson who took up the cause of Mendelism, driven to a feverish pitch in its defense by his desire to expose the folly of his once best friend Frank Weldon, whom Bateson felt had betrayed both him and the cause of truth.

In 1909, Morgan, who stood opposed to all three of the major developments in contemporary biology—Darwinism, the chromosome theory, and Mendelism—began to study the common fruit fly. The following year, despite his doubts about both Mendel's factors and the importance of the chromosomes, Morgan discovered the first definitive proof that a Mendelian factor was associated with a particular chromosome—the sex-linked X chromosome. But Morgan's continued equivocation, about Darwinism and the implications of the chromosome theory, led to a falling out with Muller, who had already begun to see his way toward a new synthesis of Mendel and Darwin. Ten years later, in 1926, Muller proved that X-rays could be used to artificially induce mutations in flies, a discovery that would ultimately win him the Nobel Prize. Though Morgan and Sturtevant partially succeeded in tainting Muller's reputation, Muller would go on to develop the modern theory of the gene, which served as the foundation for modern molecular biology.

In my view, discussing the history of genetics without describing actual experiments would be like analyzing the history of art without looking at any of the paintings. Accordingly, although this book is intended for a lay audience, I have described many of the key experiments in considerable detail. Some research papers read like finished masterpieces and others like works in progress. Many of the papers of the frenetic Morgan, for example, were vague and unformed. Often in a muddle over the meaning of his discoveries, Morgan was nonetheless a good enough scientist to recognize something new and important when he stumbled on it. At the other extreme was Mendel, whose paradigm-shifting paper "Experiments on Plant Hybrids" was an exquisitely polished argument, supported by a vast array of data that confirmed his model with astonishing precision.

— THE LONGER I STUDIED this material, the more the history of the developing concept of the gene began to coalesce into a coherent narrative. While I tried to emphasize the continuities and recurrent patterns, I was also acutely aware of the need to remain faithful to the facts. As a result, I excluded observations that seemed to fit with my sense of the characters but could not be independently verified, and I provided documentation for

each of the more controversial claims in this book. Further danger lurked in the accounts of the scientists themselves, who often attempted to shape the history to their own ends, as when Sturtevant wrote in his biographical memoir of Morgan for the National Academy of Sciences that Muller was often in and out of the fly room but never actually had a desk there. But to a remarkable degree, I think, it was possible to get beyond the distortions. One thing that helps is the system itself—no matter what prejudices a scientist might bring to his work, each new idea, in order to survive, must be tested in controlled experiments and must hold up to the scrutiny of other scientists. As the scientific facts ultimately sort themselves out, I believe, so do the facts about the scientists who produced them, and little by little it is possible to move closer to an undistorted view of the history as it unfolded.