CHAPTER 21

GENERAL REMARKS

There is a widespread view that scientific discoveries are more or less inevitable, and that it makes little difference whether or not a particular individual makes a discovery at a given time: if the time is not ripe for it, it will not be understood and will have little or no effect on future events; if the time is ripe, then someone else will soon make the discovery anyhow.

The history of Mendelism is one of the often-cited examples here. According to this interpretation, Mendel's paper was not understood in 1866 because the time was not ripe; in 1900, when the time was ripe, the principles were discovered independently by three different people. To me, this account seems greatly oversimplified—though it must be admitted that the development of the subject would probably have been much the same, even to the dates, if Mendel's paper had never been written.

It is true that the paper was ahead of its time, but it was not difficult to understand, and it seems unlikely that it would have remained unappreciated for so long if it had appeared in a less obscure journal, or if Mendel himself had published the further cases that he reported in his letters to Nägeli. It must be remembered that Nägeli's failure to appreciate the paper in 1866 can be matched by Pearson's failure in 1904. Both were outstanding men, and both were actively studying heredity, but to both of them Mendel's results appeared as trivial cases involving a few superficial characters, obviously neither useful nor illuminating for any general theory of inheritance. It does not follow that no biologist was likely to have appreciated the paper if he had seen it before 1900—I have suggested above that Galton, for one, would very likely have done so.

As for the simultaneous discovery in 1900, I have pointed out in Chapter 4 that it seems likely that the independent discovery was the finding of Mendel's paper, and that the actual working out of the principles without knowledge of Mendel's work was only accomplished once—by Correns—and even here it is not possible to be certain how clearly he understood the principles before he read Mendel.

In connection with the idea of the inevitability of scientific discoveries, it seems necessary to inquire into the meaning of the expression "when the time is ripe." The state of knowledge and opinion at a given time is obviously the result of individual intellectual efforts and can scarcely be thought of as inevitable. It does make a difference whether a discovery is made now, or next year, since the whole course of events in the intervening year is altered by the discovery; if it is not made now, there is a chance that the time may be overripe next year, since attention may have shifted to a quite different field.

There are other examples of a widespread failure to appreciate first-rate discoveries in genetics, and it is perhaps worthwhile to examine some of these briefly. Perhaps the most remarkable examples are the work of Cuénot on multiple alleles, of Renner on Oenothera, and of Garrod on biochemical genetics. These were all accessible and were often referred to, and all were written by men with established reputations—yet they were not fully understood nor was their importance realized until several years later. This neglect seems to have arisen in part, in all three cases, from failure to understand the terminology used. All three authors wrote in a simple, direct style, and their ideas were not inherently difficult to understand.

Cuénot used a set of symbols for the genes that was unorthodox and confusing, and he seems not to have realized that the multiple allelism that he demonstrated was unusual or unexpected. Renner was dealing with a complex situation, and he developed a useful simplifying terminology to describe it—with the result that the later beautifully clear and illuminating papers were unintelligible unless this terminology was first learned. Garrod was concerned with biochemical processes, and few geneticists were well enough grounded in biochemistry to be willing to make the moderate effort required to understand what he was talking about.

Mendel worked alone, and some of the more recent geneticists have also been rather solitary. Correns, for example, was inclined to look for new material and new problems as soon as others began to work at the problems that concerned him. Johannsen was also a rather isolated person. But it has become more and more usual for geneticists to work in closely collaborating groups, a tendency the subject shares with most scientific disciplines. The first such group was organized by Bateson, initially at Cambridge, and then at the John Innes Horticultural Institution. This group, unlike most of the later ones, used a wide variety of experimental objects—fowl, rabbits, stocks, peas, sweet peas, Primula, and many other forms. It was, however, a closely collaborating group, with much exchanging of ideas and mutual stimulation. The more recent schools have tended rather to concentrate on particular forms. A few examples are: the group organized by Castle at the Bussey Institution at Harvard, working on rodents; Emerson's maize group, at first at Nebraska but more especially later at Cornell; Morgan's Drosophila group at Columbia, and later at the California Institute of Technology with offshoots at Texas, Indiana, Columbia again, Moscow, Edinburgh, and elsewhere; Beadle's and Tatum's Neurospora group at Stanford, and many similar groups. As a rule, people working in separate laboratories on similar problems are in close contact through correspondence, temporary residence at each other's institutions, and frequent specialized symposia.

The development of genetics is one of the striking examples of the interaction between different disciplines. After 1900, the first such interaction was with cytology, which led to a very rapid development of both subjects. Later interactions were with statistics, practical breeding, evolution theory, immunology, and biochemistry. All of these have led to the utilization of new ideas and new techniques, and to rapid—sometimes spectacular—advances in genetics and in the other fields concerned.

The history of a science is primarily a history of ideas and, as such, I have treated it largely from a biographical point of view. It is also possible to treat it with emphasis on the development of new techniques—in the case of genetics, such things as the study of chromosomes, use of statistical methods, of irradiation, or of biochemical methods—or on the introduction of new kinds of organisms that are especially favorable for the study of particular problems—such as Drosophila, Neurospora, Paramecium, bacteria, or bacteriophages.