

until a better one be advanced, it will serve to bring together a multitude of facts which are at present left disconnected by any efficient cause. As Whewell, the historian of the inductive sciences, remarks:—"Hypotheses may often be of service to science, when they involve a certain portion of incompleteness, and even of error." Under this point of view I venture to advance the hypothesis of Pangenesis, which implies that every separate part of the whole organisation reproduces itself. So that ovules, spermatozoa, and pollen-grains,—the fertilised egg or seed, as well as buds,—include and consist of a multitude of germs thrown off from each separate part or unit.<sup>1</sup>

In the First Part I will enumerate as briefly as I can the groups of facts which seem to demand connection; but certain

<sup>1</sup> This hypothesis has been severely criticised by many writers, and it will be fair to give references to the more important articles. The best essay which I have seen is by Prof. Delpino, entitled 'Sulla Darwiniana Teoria della Pangenese, 1869,' of which a translation appeared in 'Scientific Opinion,' Sept. 29, 1869 and the succeeding numbers. He rejects the hypothesis, but criticises it fairly, and I have found his criticisms very useful. Mr. Mivart ('Genesis of Species,' 1871, chap. x.) follows Delpino, but adds no new objections of any weight. Dr. Bastian ('The Beginnings of Life,' 1872, vol. ii. p. 98) says that the hypothesis "looks like a relic of the old rather than a fitting appanage of the new evolution philosophy." He shows that I ought not to have used the term "pangenesis," as it had been previously used by Dr. Gros in another sense. Dr. Lionel Beale ('Nature,' May 11, 1871, p. 26) sneers at the whole doctrine with much acerbity and some justice. Prof. Wigand ('Schriften der Gesell. der gesamt. Naturwissen. zu Marburg,' Bd. ix., 1870) considers the hypothesis as unscientific and worthless. Mr. G. H. Lewes ('Fortnightly Review,' Nov. 1, 1868, p. 503) seems to consider that it may be useful: he

makes many good criticisms in a perfectly fair spirit. Mr. F. Galton, after describing his valuable experiments ('Proc. Royal Soc.' vol. xix. p. 393) on the intertransfusion of the blood of distinct varieties of the rabbit, concludes by saying that in his opinion the results negative beyond all doubt the doctrine of Pangenesis. He informs me that subsequently to the publication of his paper he continued his experiments on a still larger scale for two more generations, without any sign of mongrelism showing itself in the very numerous offspring. I certainly should have expected that gemmules would have been present in the blood, but this is no necessary part of the hypothesis, which manifestly applies to plants and the lowest animals. Mr. Galton, in a letter to 'Nature' (April 27, 1871, p. 502), also criticises various incorrect expressions used by me. On the other hand, several writers have spoken favourably of the hypothesis, but there would be no use in giving references to their articles. I may, however, refer to Dr. Ross' work, 'The Graft Theory of Disease; being an application of Mr. Darwin's hypothesis of Pangenesis,' 1872, as he gives several original and ingenious discussions.