

gard to the beak, and the œsophagus with regard to the liver, should have positions corresponding to those in vertebrates; but the positions of these organs are exactly contrary to the hypothesis. How, then, can you say," he asks, "that the cephalopods and vertebrates have *identity of composition, unity of composition*, without using words in a sense entirely different from their common meaning?"

This argument appears to be exactly of the kind on which the value of the hypothesis must depend.¹⁴ It is, therefore, interesting to see the reply made to it by the theorist. It is this: "I admit the facts here stated, but I deny that they lead to the notion of a different sort of animal composition. Molluscous animals had been placed too high in the zoological scale; but if they are only the embryos of its lower stages, if they are only beings in which far fewer organs come into play, it does not follow that the organs are destitute of the relations which the power of successive generations may demand. The organ A will be in an unusual relation with the organ C, if B has not been produced;—if a stoppage of the developement has fallen upon this latter organ, and has thus prevented its production. And thus," he says, "we see how we may have different arrangements, and divers constructions as they appear to the eye."

It seems to me that such a concession as this entirely destroys the theory which it attempts to defend; for what arrangement does the principle of unity of composition *exclude*, if it admits unusual, that is, various arrangements of some organs, accompanied by the total absence of others? Or how does this differ from Cuvier's mode of stating the conclusion, except in the introduction of certain arbitrary hypotheses of developement and stoppage? "I reduce the facts," Cuvier says, "to their true expression, by saying that Cephalopods have several organs which are common to them and vertebrates, and which discharge the same offices; but that these organs are in them differently distributed, and often constructed in a different manner;

¹⁴ I do not dwell on other arguments which were employed. It was given as a circumstance suggesting the supposed posture of the type, that in this way the back was colored, and the belly was white. On this Cuvier observes (*Phil. Zool.* pp. 93, 68), "I must say, that I do not know any naturalist so ignorant as to suppose that the back is determined by its dark color, or even by its position when the animal is in motion; they all know that the badger has a black belly and a white back; that an infinity of other animals, especially among insects, are in the same case; and that many fishes swim on their side, or with their belly upwards."