

three conditions reducing the five coefficients to two independent ones. It will be found that $\overline{m^2} = \overline{m'}^2$, as in the ordinary theory.

I doubt not that Boltzmann's minimum theorem can with some modification be applied to this system, at all events if he will take up the theory of dense gases himself.

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On Skew Probability Curves.

IN a memoir, entitled "Contributions to the Mathematical Theory of Evolution. II. Skew Variation in Homogeneous Material" (*Phil. Trans.* 186, A, pp. 343-414), and noticed in your columns by Mr. Francis Galton (January 31, 1895), I have dealt with four types of skew frequency curves.

Last Tuesday, Prof. Edgeworth drew my attention to the fact that a portion of my results has been anticipated by Mr. E. L. De Forest in vols. vi., ix., and x. of *The Analyst*, an excellent American mathematical journal, the acquaintance of which, I am ashamed to say, I have only to-day made for the first time.

So far as Mr. De Forest's priority is concerned, it covers the special class of curve I have in my memoir termed Type III. He has fully worked out the geometry of this type, and I consider his deduction of it, if somewhat more lengthy than mine, to have the advantage of greater generality. So far as my own memoir is concerned, a knowledge of Mr. De Forest's memoir would not have led me to rewrite pp. 373-6 of mine, which deal with this type, because my discussion there is only a branch of my general treatment of a series of skew frequency curves. I should, however, have referred to Mr. De Forest's priority and the excellency of his work. In particular I should have cited the whole of his numerical table iii. x. p. 69, which gives the values of the frequency in excess and defect of the mode, and the probable errors in excess and defect, for a considerable range of values. These results are only given by algebraic or empirical formulæ in my paper. The statisticians among your readers, who may be proposing to deal with skew frequency, would find a copy of Mr. De Forest's Table III. of considerable service should they come across a curve of Type III.

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University College, London, July 24.

Evolution, or Epigenesis?

IN the English translation of Prof. Hertwig's book "The Cell," it is stated (p. 295), "When the female gamete of the Alga *Ectocarpus* comes to rest, for a few minutes it becomes receptive. If the egg is not fertilised at this time . . . parthenogenetic germination begins to make its appearance . . . It may be accepted as a *law of nature* (italics mine) for mammals, and for the majority of other organisms, that their male and female sexual cells are absolutely incapable of development by themselves." Thus, what occurs in the lower organisms is no criterion of what occurs in the higher, and *vice versa*. Then why does Hertwig remark (p. 348), "It is quite sufficient for our purpose to acknowledge, that in the plants and lower animals, all the cells which are derived from the ovum contain *equal quantities of the hereditary mass*. . . All idioblasts must divide and must be transmitted to the daughter-cells, in *equal proportions both as regards quality and quantity*" (italics mine). According to the above, it is "quite sufficient" for Hertwig's purpose of discrediting Weismann's contention for differentiated distribution of hereditary elements among somatic cells, to show that there is undifferentiated distribution in the case of plants and lower animals. But, reverting to the earlier quotation, if it is not sufficient to prove sexual reproduction in the case of the higher organisms, in order to disprove parthenogenesis in the case of the lower organisms, why should it be "quite sufficient," in order to disprove distribution through germ-cells, in the case of the higher organisms, to show that, in plants and the lower animals one cell contains the same hereditary constituents as another? It is permissible to infer that differentiation in regard to germ-cells, in the higher animals, is no more disproved by the assumed demonstration that, in plants and the lower animals, there is no such differentiation, than that asexuality in lower is disproved by sexuality in higher organisms. Weismann, in my opinion, has proved to rational satisfaction that differentiation of germ from other cells must occur in the higher organisms, and he has offered a rational explanation, conformable with the theory of germ-plasm, of the apparently summational distribution of hereditary elements through somatic cells. Until Weismann's

position is seriously undermined, which, so far, is not even a likely contingency, we must decline to accept Hertwig's assumed demonstrations in regard to plants and lower animals as invalidating the theory of germ-plasm. Similarly, that environment may affect the hereditary character of a primitive organism is no more evidence that it may so affect a mammal, than sexuality in the latter is evidence against parthenogenesis in the former. On page 348 we are told: "Johannes Müller has raised the question, 'How does it happen that certain of the cells of the organised body, although they resemble both other cells and the original germ-cell, can produce nothing but their like, *i.e.* cells which are (in-?) capable of developing into the complete organism?' Thus epidermal cells can only, by absorbing material, develop new epidermal cells, and cartilage cells only other cartilage cells, but never embryos or buds.' To which he has made answer: 'This may be due to the fact that these cells, even if they possess the power of forming the whole, have, by means of a particular metamorphosis of their substance, become so specialised, that they have entirely lost their germinal properties, as regards the whole organism, and when they become separated from the whole, are unable to lead an independent existence.'" The above is simply a restatement of Weismann's doctrine regarding the origin of germ-cells. All cells which have not, as Müller states, "lost their germinal properties, as regards the whole organism," are Weismann's germ-cells.

So far as regards the essential question of heredity, Hertwig agrees with Weismann. Special units (idioblasts) are the bearers of hereditary qualities. This is "evolution," and no superstructural epigenetic thesis attributing modifying effects by environment, as the cause of a somatic cellular development, can affect the point that differentiation, through hereditary units, is the fundamental condition of morphological development. To accept "hereditary units," in my opinion, excludes "hereditary effect through environment," never mind to what matter-system the latter assumption be applied, whether the systems be, for instance, unicellular organisms or somatic cells. On the other hand, if we accept "hereditary extraneous influence," we need not trouble ourselves with "hereditary units." If "extraneous influences" have hereditary effect, "hereditary units" have no logical existence. All we then need for a theory of heredity are primordial homogeneous matter and environment. Mr. Herbert Spencer's earlier hypothesis, in which he attributed all variation to extraneous influence, would have been logical had he excluded "physiological units." With these, it became illogical. For this reason: if all organic variability depended on the effect of extraneous influences, why should such influences not have produced the differentiations called physiological units? Why should the only logical "unit" not be homogeneous *primordium*? That the conception "hereditary unit" shall be logical, involves that the "unit" shall be as unchangeable as an "atom." If, on the contrary, we have a variable "unit," it is not a genuine "hereditary unit," but merely the equivalent of any later variable "unit." Hertwig's "hereditary units," or "idioblasts" (p. 340), "are the smallest particles of material into which the hereditary mass or idioplasm can be divided, and of which great numbers and various kinds are present in this idioplasm. They are, according to their different composition, the bearers of different properties." They are not indivisible, like atoms, but assimilate food, grow and divide, as do Weismann's "biophors," from which they appear to differ only to the extent that they are complex organisms. The hereditary factor in Weismann's theory which corresponds with these "idioblasts" of Hertwig appears to be the "determinant." All the functions of the latter seem to be performed by the former. These "idioblasts" (p. 343) "must evolve in regular sequence during the process of development." As sentences are formed from words, so are organisms formed from these "idioblasts." We can attain a clear conception of the formation of sentences from words, but Hertwig does not enable us to apprehend how organisms can arise from "idioblasts." As he very truly observes (p. 344), "this portion of the theory is the most difficult to understand."

Hertwig, like Spencer, takes his stand on epigenesis. It may be asked, wherein is the epigenetic character of his (Hertwig's) theory? Unlike Spencer's "physiological units," Hertwig's "idioblasts" are intrinsically differentiated organisms with specific tendencies. Now, for a genuine epigenetic theory, hereditary units must merely compose a plastic mould to take the impress of environment, whereas these "idioblastic" cells are composed of elements with predetermined peculiarities. Accordingly they must function in a predetermined manner, and