IV.

# THE CONTINUITY OF THE GERM-PLASM AS THE FOUNDATION OF A THEORY OF HEREDITY.

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# CONTINUITY OF THE GERM-PLASM, &c.

## PREFACE.

The ideas developed in this essay were first expressed during the past winter in a lecture delivered to the students of this University (Freiburg), and they were shortly afterwards—in February and the beginning of March—written in their present form. I mention this, because I might otherwise be reproached for a somewhat partial use of the most recent publications on related subjects. Thus I did not receive Oscar Hertwig's paper—'Contributions to the Theory of Heredity' (Zur Theorie der Vererbung), until after I had finished writing my essay, and I could not therefore make as much use of it as I should otherwise have done. Furthermore, the paper by Kölliker on 'The Significance of the Nucleus in the Phenomena of Heredity' (Die Bedeutung der Zellkerne für die Vorgänge der Vererbung), did not appear until after the completion of my manuscript. The essential treatment of the subject would not, however, have been altered if I had received the papers at an earlier date, for as far as the most important point—the significance of the nucleus—is concerned, my views are in accordance with those of both the above-named investigators; while the points upon which our views do not coincide had already received attention in the manuscript.

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Freiburg i. Breisgau,
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## CONTENTS.

# IV.

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### Introduction.

When we see that, in the higher organisms, the smallest structural details, and the most minute peculiarities of bodily and mental disposition, are transmitted from one generation to another; when we find in all species of plants and animals a thousand characteristic peculiarities of structure continued unchanged through long series of generations; when we even see them in many cases unchanged throughout whole geological periods; we very naturally ask for the causes of such a striking phenomenon: and enquire how it is that such facts become possible, how it is that the individual is able to transmit its structural features to its offspring with such precision. And the immediate answer to such a question must be given in the following terms:—'A single cell out of the millions of diversely differentiated cells which compose the body, becomes specialized as a sexual cell; it is thrown off from the organism and is capable of reproducing all the peculiarities of the parent body, in the new individual which springs from it by cell-division and the complex process of differentiation.' Then the more precise question follows: 'How is it that such a single cell can reproduce the *tout ensemble* of the parent with all the faithfulness of a portrait?'

The answer is extremely difficult; and no one of the many attempts to solve the problem can be looked upon as satisfactory; no one of them can be regarded as even the beginning of a solution or as a secure foundation from which a complete solution may be expected in the future. Neither Häckel's [1], 'Perigenesis of the Plastidule,' nor Darwin's [2] 'Pangenesis,' can be regarded as such a beginning. The former hypothesis does not really treat of that

[1] Häckel, 'Ueber die Wellenzeugung der Lebenstheilchen etc.,' Berlin, 1876.

[2] Darwin, 'The Variation of Animals and Plants under Domestication,' vol. ii. 1875, chap. xxvii. pp. 344-399.

part of the problem which is here placed in the foreground, viz. the explanation of the fact that the tendencies of heredity are present in single cells, but it is rather concerned with the question as to the manner in which it is possible to conceive the transmission of a certain tendency of development into the sexual cell, and ultimately into the organism arising from it. The same may be said of the hypothesis of His [1], who, like Häckel, regards heredity as the transmission of certain kinds of motion. On the other hand, it must be conceded that Darwin's hypothesis goes to the very root of the question, but he is content to give, as it were, a provisional or purely formal solution, which, as he himself says, does not claim to afford insight into the real phenomena, but only to give us the opportunity of looking at all the facts of heredity from a common standpoint. It has achieved this end, and I believe it has unconsciously done more, in that the thoroughly logical application of its principles has shown that the real causes of heredity cannot lie in the formation of gemmules or in any allied phenomena. The improbabilities to which any such theory would lead are so great that we can affirm with certainty that its details cannot accord with existing facts. Furthermore, Brooks' [2] well-considered and brilliant attempt to modify the theory of Pangenesis, cannot escape the reproach that it is based upon possibilities, which one might certainly describe as improbabilities. But although I am of opinion that the whole foundation of the theory of Pangenesis, however it may be modified, must be abandoned, I think, nevertheless, its author deserves great credit, and that its production has been one of those indirect roads along which science has been compelled to travel in order to arrive at the truth. Pangenesis is a modern revival of the oldest theory of heredity, that of Democritus, according to which the sperm is secreted from all parts of the body of both sexes during copulation, and is animated by a bodily force; according to this theory also, the sperm from each part of the body reproduces the same part [3].

[1] His, 'Unsre Körperform etc.,' Leipzig, 1875.

[2] Brooks, 'The Law of Heredity,' Baltimore, 1883.

[3] Galton's experiments on transfusion in Rabbits have in the mean time really proved that Darwin's gemmules do not exist. Roth indeed states that Darwin has never maintained that his gemmules make use of the circulation as a medium, but while on the one hand it cannot be shown why they should fail to take the

If, according to the received physiological and morphological ideas of the day, it is impossible to imagine that gemmules produced by each cell of the organism are at all times to be found in all parts of the body, and furthermore that these gemmules are collected in the sexual cells, which are then able to again reproduce in a certain order each separate cell of the organism, so that each sexual cell is capable of developing into the likeness of the parent body; if all this is inconceivable, we must enquire for some other way in which we can arrive at a foundation for the true understanding of heredity. My present task is not to deal with the whole question of heredity, but only with the single although fundamental question—'How is it that a single cell of the body can contain within itself all the hereditary tendencies of the whole organism?' I am here leaving out of account the further question as to the forces and the mechanism by which these tendencies are developed in the building-up of the organism. On this account I abstain from considering at present the views of Nägeli, for as will be shown later on, they only slightly touch this fundamental question, although they may certainly claim to be of the highest importance with respect to the further question alluded to above.

Now if it is impossible for the germ-cell to be, as it were, an extract of the whole body, and for all the cells of the organism to despatch small particles to the germ-cells, from which the latter derive their power of heredity; then there remain, as it seems to me, only two other possible, physiologically conceivable, theories as to the origin of germ-cells, manifesting such powers as we know they possess. Either the substance of the parent germ-cell is capable of undergoing a series of changes which, after the building-up of a new individual, leads back again to identical germ-cells; or the germ-cells are not derived at all, as far as their essential and characteristic substance is concerned, from the body of

favourable opportunities afforded by such a medium, inasmuch as they are said to be constantly circulating through the body; so on the other hand we cannot understand how the gemmules could contrive to avoid the circulation. Darwin has acted very wisely in avoiding any explanation of the exact course in which his gemmules circulate. He offered his hypothesis as a formal and not as a real explanation.

Professor Meldola points out to me that Darwin did not admit that Galton's experiments disproved pangenesis ('Nature,' April 27, 1871, p. 502), and Galton also admitted this in the next number of 'Nature' (May 4, 1871, p. 5).—A. W. 1889.

the individual, but they are derived directly from the parent germ-cell.

I believe that the latter view is the true one: I have expounded it for a number of years, and have attempted to defend it, and to work out its further details in various publications. I propose to call it the theory of 'The Continuity of the Germ-plasm,' for it is founded upon the idea that heredity is brought about by the transference from one generation to another, of a substance with a definite chemical, and above all, molecular constitution. I have called this substance 'germ-plasm,' and have assumed that it possesses a highly complex structure, conferring upon it the power of developing into a complex organism. I have attempted to explain heredity by supposing that in each ontogeny, a part of the specific germ-plasm contained in the parent egg-cell is not used up in the construction of the body of the offspring, but is reserved unchanged for the formation of the germ-cells of the following generation.

It is clear that this view of the origin of germ-cells explains the phenomena of heredity very simply, inasmuch as heredity becomes thus a question of growth and of assimilation,—the most fundamental of all vital phenomena. If the germ-cells of successive generations are directly continuous, and thus only form, as it were, different parts of the same substance, it follows that these cells must, or at any rate may, possess the same molecular constitution, and that they would therefore pass through exactly the same stages under certain conditions of development, and would form the same final product. The hypothesis of the continuity of the germ-plasm gives an identical starting-point to each successive generation, and thus explains how it is that an identical product arises from all of them. In other words, the hypothesis explains heredity as part of the underlying problems of assimilation and of the causes which act directly during ontogeny: it therefore builds a foundation from which the explanation of these phenomena can be attempted.

It is true that this theory also meets with difficulties, for it seems to be unable to do justice to a certain class of phenomena, viz. the transmission of so-called acquired characters. I therefore gave immediate and special attention to this point in my first publication on heredity [1], and I believe that I have shown that the

[1] Weismann, 'Ueber die Vererbung.' Jena, 1883; translated in the present volume as the second essay 'On Heredity.'

hypothesis of the transmission of acquired characters—up to that time generally accepted—is, to say the least, very far from being proved, and that entire classes of facts which have been interpreted under this hypothesis may be quite as well interpreted otherwise, while in many cases they must be explained differently. I have shown that there is no ascertained fact, which, at least up to the present time, remains in irrevocable conflict with the hypothesis of the continuity of the germ-plasm; and I do not know any reason why I should modify this opinion to-day, for I have not heard of any objection which appears to be feasible. E. Roth [1] has objected that in pathology we everywhere meet with the fact that acquired local disease may be transmitted to the offspring as a predisposition; but all such cases are exposed to the serious criticism that the very point that first needs to be placed on a secure footing is incapable of proof, viz. the hypothesis that the causes which in each particular case led to the predisposition, were really acquired. It is not my intention, on the present occasion, to enter fully into the question of acquired characters; I hope to be able to consider the subject in greater detail at a future date. But in the meantime I should wish to point out that we ought, above all, to be clear as to what we really mean by the expression 'acquired character.' An organism cannot acquire anything unless it already possesses the predisposition to acquire it: acquired characters are therefore no more than local or sometimes general variations which arise under the stimulus provided by certain external influences. If by the long-continued handling of a rifle, the so-called 'Exercierknochen' (a bony growth caused by the pressure of the weapon in drilling) is developed, such a result depends upon the fact that the bone in question, like every other bone, contains within itself a predisposition to react upon certain mechanical stimuli, by growth in a certain direction and to a certain extent. The predisposition towards an 'Exercierknochen' is therefore already present, or else the growth could not be formed; and the same reasoning applies to all other 'acquired characters.'

Nothing can arise in an organism unless the predisposition to it is pre-existent, for every acquired character is simply the reaction of the organism upon a certain stimulus. Hence I should never have thought of asserting that predispositions cannot be trans-

[1] E. Roth, 'Die Thatsachen der Vererbung.' 2. Aufl., Berlin, 1885, p. 14.

mitted, as E. Roth appears to believe. For instance, I freely admit that the predisposition to an 'Exercierknochen' varies, and that a strongly marked predisposition may be transmitted from father to son, in the form of bony tissue with a more susceptible constitution. But I should deny that the son could develope an 'Exercierknochen' without having drilled, or that, after having drilled, he could develope it more easily than his father, on account of the drilling through which the latter first acquired it. I believe that this is as impossible as that the leaf of an oak should produce a gall, without having been pierced by a gall-producing insect, as a result of the thousands of antecedent generations of oaks which have been pierced by such insects, and have thus 'acquired' the power of producing galls. I am also far from asserting that the germ-plasm—which, as I hold, is transmitted as the basis of heredity from one generation to another—is absolutely unchangeable or totally uninfluenced by forces residing in the organism within which it is transformed into germ-cells. I am also compelled to admit that it is conceivable that organisms may exert a modifying influence upon their germ-cells, and even that such a process is to a certain extent inevitable. The nutrition and growth of the individual must exercise some influence upon its germ-cells; but in the first place this influence must be extremely slight, and in the second place it cannot act in the manner in which it is usually assumed that it takes place. A change of growth at the periphery of an organism, as in the case of an 'Exercierknochen,' can never cause such a change in the molecular structure of the germ-plasm as would augment the predisposition to an 'Exercierknochen,' so that the son would inherit an increased susceptibility of the bony tissue or even of the particular bone in question. But any change produced will result from the reaction of the germ-cell upon changes of nutrition caused by alteration in growth at the periphery, leading to some change in the size, number, or arrangement of its molecular units. In the present state of our knowledge there is reason for doubting whether such reaction can occur at all; but, if it can take place, at all events the quality of the change in the germ-plasm can have nothing to do with the quality of the acquired character, but only with the way in which the general nutrition is influenced by the latter. In the case of the 'Exercierknochen' there would be practically no change in the general nutrition, but if such a bony growth could reach the size of a carcinoma, it is conceivable that a disturbance of the general nutrition of the body might ensue. Certain experiments on plants—in which Nägeli showed that they can be submitted to strongly varied conditions of nutrition for several generations, without the production of any visible hereditary change—show that the influence of nutrition upon the germ-cells must be very slight, and that it may possibly leave the molecular structure of the germ-plasm altogether untouched. This conclusion is also supported by comparing the uncertainty of these results with the remarkable precision with which heredity acts in the case of those characters which are known to be transmitted. In fact, up to the present time, it has never been proved that any changes in general nutrition can modify the molecular structure of the germ-plasm, and far less has it been rendered by any means probable that the germ-cells can be affected by acquired changes which have no influence on general nutrition. If we consider that each so-called predisposition (that is, a power of reacting upon a certain stimulus in a certain way, possessed by any organism or by one of its parts) must be innate, and further that each acquired character is only the predisposed reaction of some part of an organism upon some external influence; then we must admit that only one of the causes which produce any acquired character can be transmitted, the one which was present before the character itself appeared, viz. the predisposition; and we must further admit that the latter arises from the germ, and that it is quite immaterial to the following generation whether such predisposition comes into operation or not. The continuity of the germ-plasm is amply sufficient to account for such a phenomenon, and I do not believe that any objection to my hypothesis, founded upon the actually observed phenomena of heredity, will be found to hold. If it be accepted, many facts will appear in a light different from that which has been cast upon them by the hypothesis which has been hitherto received, —a hypothesis which assumes that the organism produces germ-cells afresh, again and again, and that it produces them entirely from its own substance. Under the former theory the germ-cells are no longer looked upon as the product of the parent's body; at least as far as their essential part—the specific germ-plasm—is concerned: they are rather considered as something which is to be placed in contrast with the *tout ensemble* of the cells which make up the parent's body, and the germ-cells of succeeding generations stand in a similar relation to one another as a series of generations of unicellular organisms, arising by a continued process of cell-division. It is true that in most cases the generations of germ-cells do not arise immediately from one another as complete cells, but only as minute particles of germ-plasm. This latter substance, however, forms the foundation of the germ-cells of the next generation, and stamps them with their specific character. Previous to the publication of my theory, G. Jäger [1], and later M. Nussbaum [2], have expressed ideas upon heredity which come very near to my own [3]. Both of these writers started with the hypothesis that there

[1] Jäger, 'Lehrbuch der allgemeinen Zoologie,' Bd. II. Leipzig, 1878.

[2] M. Nussbaum, 'Die Differenzirung des Geschlechts im Thierreich,' Arch. f. Mikrosk. Anat., Bd. XVIII. 1880.

[3] I have since learnt that Professor Rauber of Dorpat also expressed similar views in 1880; and Professor Herdman of Liverpool informs me that Mr. Francis Galton had brought forward in 1876 a theory of heredity of which the fundamental idea in some ways approached that of the continuity of the germ-plasm ('Journal of the Anthropological Institute,' vol. v; London, 1876).—A. W., 1888.

[A less complete theory was brought forward by Galton at an earlier date, in 1872 (see Proc. Roy. Soc. No. 136, p. 394). In this paper he proposed the idea that heredity chiefly depends upon the development of the offspring from elements directly derived from the fertilized ovum which had produced the parent. Galton speaks of the fact that 'each individual may properly be conceived as consisting of two parts, one of which is latent and only known to us by its effects on his posterity, while the other is patent, and constitutes the person manifest to our senses. The adjacent and, in a broad sense, separate lines of growth in which the patent and latent elements are situated, diverge from a common group and converge to a common contribution, because they were both evolved out of elements contained in a structureless ovum, and they, jointly, contribute the elements which form the structureless ova of their offspring.' The following diagram shows clearly 'that the span of each of the links in the general chain of heredity extends from one structureless stage to another, and not from person to person:—

| | | |
|---|---|---|
| Structureless elements in Father | ... Adult Father ...<br>... Latent in Father ... | structureless elements in Offspring.' |

Again Galton states—'Out of the structureless ovum the embryonic elements are taken ... and these are developed (*a*) into the visible adult individual; on the other hand ..., after the embryonic elements have been segregated, the large residue is developed (*b*) into the latent elements contained in the adult individual.' The above quoted sentences and diagram indicate that Galton does not derive the whole of the hereditary tendencies from the latent elements, but that he believes some effect is also produced by the patent elements. When however he contrasts the relative power of these two influences, he attaches comparatively little importance to the patent elements. Thus if any character be fixed upon, Galton states that it 'may be conceived (1) as purely personal, without the concurrence of any latent equivalents, (2) as personal but conjoined with latent equivalents, and (3) as existent wholly in a latent form.' He argues that the hereditary power in the first case is must be a direct connexion between the germ-cells of succeeding generations, and they tried to establish such a continuity by supposing that the germ-cells of the offspring are separated from the parent germ-cell before the beginning of embryonic development, or at least before any histological differentiation has taken place. In this form their suggestion cannot be maintained, for it is in conflict with numerous facts. A continuity of the germ-*cells* does not now take place, except in very rare instances; but this fact does not prevent us from adopting a theory of the continuity of the germ-*plasm*, in favour of which much weighty evidence can be brought forward. In the following pages I shall attempt to develope further the theory of which I have just given a short account, to defend it against any objections which have been brought forward, and to draw from it new conclusions which may perhaps enable us more thoroughly to appreciate facts which are known, but imperfectly understood. It seems to me that this theory of the continuity of the germ-plasm deserves at least to be examined in all its details, for it is the simplest theory upon the subject, and the one which is most obviously suggested by the facts of the case, and we shall not be justified in forsaking it for a more complex theory until proof that it can be no longer maintained is forthcoming. It does not presuppose anything except facts which can be observed at any moment, although they may not be understood,—such as assimilation, or the development of like organisms from like germs; while every other theory of heredity is founded on hypotheses which cannot be proved. It is nevertheless possible that continuity of the germ-plasm does not exist in the manner in which I imagine that it takes place, for no one can at present decide whether all the

exceedingly feeble, because 'the effects of the use and disuse of limbs, and those of habit, are transmitted to posterity in only a very slight degree.' He also argues that many instances of the supposed transmission of personal characters are really due to latent equivalents. 'The personal manifestation is, on the average, though it need not be so in every case, a certain proof of the existence of latent elements.' Having argued that the strength of the latter in heredity is further supported by the facts of reversion, Galton considers it is safe to conclude 'that the contribution from the patent elements is very much less than from the latent ones.' In the later development of his theory, Galton adheres to the conception of 'gemmules' and accepts Darwin's views, although 'with considerable modification.' Together with pangenesis itself, Galton's theory must be looked upon as *preformational*, and so far it is in opposition to Weismann's theory which is *epigenetic*. See Appendix IV. to the next Essay (V.), pp. 316–319.—E. B. P.]

ascertained facts agree with and can be explained by it. Moreover the ceaseless activity of research brings to light new facts every day, and I am far from maintaining that my theory may not be disproved by some of these. But even if it should have to be abandoned at a later period, it seems to me that, at the present time, it is a necessary stage in the advancement of our knowledge, and one which must be brought forward and passed through, whether it prove right or wrong, in the future. In this spirit I offer the following considerations, and it is in this spirit that I should wish them to be received.

## I. The Germ-plasm.

I must first define precisely the exact meaning of the term germ-plasm.

In my previous writings in which the subject has been alluded to, I have simply spoken of germ-plasm without indicating more precisely the part of the cell in which we may expect to find this substance—the bearer of the characteristic nature of the species and of the individual. In the first place such a course was sufficient for my immediate purpose, and in the second place the number of ascertained facts appeared to be insufficient to justify a more exact definition. I imagined that the germ-plasm was that part of a germ-cell of which the chemical and physical properties—including the molecular structure—enable the cell to become, under appropriate conditions, a new individual of the same species. I therefore believed it to be some such substance as Nägeli [1], shortly afterwards, called idioplasm, and of which he attempted, in an admirable manner, to give us a clear understanding. Even at that time one might have ventured to suggest that the organized substance of the nucleus is in all probability the bearer of the phenomena of heredity, but it was impossible to speak upon this point with any degree of certainty. O. Hertwig [2] and Fol [3] had shown that the process of fertilization is attended by a conjugation of nuclei, and Hertwig had even then distinctly said that fertilization generally

[1] Nägeli, 'Mechanisch-physiologische Theorie der Abstammungslehre.' München u. Leipzig, 1884.

[2] O. Hertwig, 'Beiträge zur Kenntniss der Bildung, Befruchtung und Theilung des thierischen Eies.' Leipzig, 1876.

[3] Fol, 'Recherches sur la fécondation, etc.' Genève, 1879.

depends upon the fusion of two nuclei; but the possibility of the co-operation of the substance of the two germ-cells could not be excluded, for in all the observed cases the sperm-cell was very small and had the form of a spermatozoon, so that the amount of its cell-body, if there is any, coalescing with the female cell, could not be distinctly seen, nor was it possible to determine the manner in which this coalescence took place. Furthermore, it was for some time very doubtful whether the spermatozoon really contained true nuclear substance, and even in 1879 Fol was forced to the conclusion that these bodies consist of cell-substance alone. In the following year my account of the sperm-cells of *Daphnidae* followed, and this should have removed every doubt as to the cellular nature of the sperm-cells and as to their possession of an entirely normal nucleus, if only the authorities upon the subject had paid more attention to these statements[1]. In the same year (1880) Balfour summed up the facts in the following manner—'The act of impregnation may be described as the fusion of the ovum and spermatozoon, and the most important feature in this act appears to be the fusion of a male and female nucleus[2].' It is true that Calberla had already observed in *Petromyzon,* that the tail of the spermatozoon does not penetrate into the egg, but remains in the micropyle; but on the other hand the head and part of the 'middle-piece' which effect fertilization, certainly contain a small fraction of the cell-body in addition to the nuclear substance, and although the amount of the former which thus enters the egg must be very small, it might nevertheless be amply sufficient to transmit the tendencies of heredity. Nägeli and Pflüger rightly asserted, at a later date, that the amount of the substance which forms the basis of heredity is necessarily very small, for the fact that hereditary tendencies are as strong on the paternal as on the maternal side, forces us to assume that the amount of this substance is nearly equal in both male and female germ-cells. Although I had not published anything upon the point, I was myself inclined to ascribe considerable

[1] Kölliker formerly stated, and has again repeated in his most recent publication, that the spermatozoa ('Samenfäden') are mere nuclei. At the same time he recognizes the existence of sperm-cells in certain species. But proofs of the former assertion ought to be much stronger in order to be sufficient to support so improbable a hypothesis as that the elements of fertilization may possess a varying morphological value. Compare Zeitschr. f. wiss. Zool., Bd. XLII.

[2] F. M. Balfour, 'Comparative Embryology,' vol. i. p. 69.

importance to the cell-substance in the process of fertilization; and I had been especially led to adopt this view because my investigations upon *Daphnidae* had shown that an animal produces large sperm-cells with an immense cell-body whenever the economy of its organism permits. All *Daphnidae* in which internal fertilization takes place (in which the sperm-cells are directly discharged upon the unfertilized egg), produce a small number of such large sperm-cells (*Sida*, *Polyphemus*, *Bythotrephes*); while all species with external fertilization (*Daphnidae*, *Lynceinae*) produce very small sperm-cells in enormous numbers, thus making up for the immense chances against any single cell being able to reach an egg. Hence the smaller the chances of any single sperm-cell being successful, the larger is the number of such cells produced, and a direct result of this increase in number is a diminution in size. But why should the sperm-cells remain or become so large in the species in which fertilization is internal? The idea suggests itself that the species in this way gains some advantage, which must be given up in the other cases; although such advantage might consist in assisting the development of the fertilized ovum and not in any increase of the true fertilizing substance. At the present time we are indeed disposed to recognize this advantage in still more unimportant matters, but at that time the ascertained facts did not justify us in the assertion that fertilization is a mere fusion of nuclei, and M. Nussbaum[1] quite correctly expressed the state of our knowledge when he said that the act of fertilization consisted in 'the union of identical parts of two homologous cells.'

Pflüger's discovery of the 'isotropism' of the ovum was the first fact which distinctly pointed to the conclusion that the bodies of the germ-cells have no share in the transmission of hereditary tendencies. He showed that segmentation can be started in different parts of the body of the egg, if the latter be permanently removed from its natural position. This discovery constituted an important proof that the body of the egg consists of a uniform substance, and that certain parts or organs of the embryo cannot be potentially contained in certain parts of the egg, so that they can only arise from these respective parts and from no others. Pflüger was mistaken in the further interpretation, from which he concluded that the fertilized ovum has no essential relation to the

[1] Arch. f. mikr. Anat., Bd. 23. p. 182, 1884.

organization of the animal subsequently formed by it, and that it is only the recurrence of the same external conditions which causes the germ-cell to develope always in the same manner. The force of gravity was the first factor, which, as Pflüger thought, determined the building up of the embryo: but he overlooked the fact that isotropism can only be referred to the body of the egg, and that besides this cell-body there is also a nucleus present, from which it was at least possible that regulative influences might emanate. Upon this point Born [1] first showed that the position of the nucleus is changed in eggs which are thus placed in unnatural conditions, and he proved that the nucleus must contain a principle which in the first place directs the formation of the embryo. Roux [2] further showed that, even when the effect of gravity is compensated, the development is continued unchanged, and he therefore concluded that the fertilized egg contains within itself all the forces necessary for normal development. Finally, O. Hertwig [3] proved from observations on the eggs of sea-urchins, that at any rate in these animals, gravity has no directive influence upon segmentation, but that the position of the first nuclear spindle decides the direction which will be taken by the first divisional plane of segmentation. These observations were however still insufficient to prove that fertilization is nothing more than the fusion of nuclei [4].

A further and more important step was taken when E. van Beneden [5] observed the process of fertilization in *Ascaris megalocephala*. Like the investigations of Nussbaum [6] upon the same subject, published at a rather earlier date, van Beneden's observations did not altogether exclude the possibility of the participation of the body of the sperm-cell in the real process of fertilization; still the fact that the nuclei of the egg-cell and the sperm-cell do not

[1] Born, 'Biologische Untersuchungen,' I, Arch. Mikr. Anat., Bd. XXIV.

[2] Roux, 'Beiträge zum Entwicklungsmechanismus des Embryo,' 1884.

[3] O. Hertwig, 'Welchen Einfluss übt die Schwerkraft,' etc. Jena, 1884.

[4] [Our present knowledge of the development of vegetable ova (including the position of the parts of the embryo) is also in favour of the view that it is not influenced by external causes, such as gravitation and light. It takes place in a manner characteristic of the genus or species, and essentially depends on other causes which are fixed by heredity, see Heinricher 'Beeinflusst das Licht die Organanlage am Farnembryo?' in Mittheilungen aus dem Botanischen Institute zu Graz, II. Jena, 1888.—S. S.]

[5] E. van Beneden, 'Recherches sur la maturation de l'œuf,' etc., 1883.

[6] M. Nussbaum, 'Ueber die Veränderung der Geschlechtsprodukte bis zur Eifurchung,' Arch. Mikr. Anat., 1884.

coalesce irregularly, but that their loops are placed regularly opposite one another in pairs and thus form one new nucleus (the first segmentation nucleus), distinctly pointed to the conclusion that the nuclear substance is the sole bearer of hereditary tendencies—that in fact fertilization depends upon the coalescence of nuclei. Van Beneden himself did not indeed arrive at these conclusions: he was prepossessed with the idea that fertilization depends upon the union of two sexually differentiated nuclei, or rather half-nuclei—the male and female pronuclei. He considered that only in this way could a single complete nucleus be formed, a nucleus which must of course be hermaphrodite, and he believed that the essential cause of further development lies in the fact that, at each successive division of nuclei and cells, this hermaphrodite nature of the nucleus is maintained by the longitudinal division of the loops of each mother-nucleus, causing a uniform distribution of the male and female loops in both daughter-nuclei.

But van Beneden undoubtedly deserves great credit for having constructed the foundation upon which a scientific theory of heredity could be built. It was only necessary to replace the terms male and female pronuclei, by the terms nuclear substance of the male and female parents, in order to gain a starting-point from which further advance became possible. This step was taken by Strasburger, who at the same time brought forward an instance in which the nucleus only of the male germ-cell (to the exclusion of its cell-body) reaches the egg-cell. He succeeded in explaining the process of fertilization in Phanerogams, which had been for a long time involved in obscurity, for he proved that the nucleus of the sperm-cell (the pollen-tube) enters the embryo-sac and fuses with the nucleus of the egg-cell: at the same time he came to the conclusion that the body of the sperm-cell does not pass into the embryo-sac, so that in this case fertilization can only depend upon the fusion of nuclei[1].

[1] Eduard Strasburger, 'Neue Untersuchungen über den Befruchtungsvorgang bei den Phanerogamen als Grundlage für eine Theorie der Zeugung.' Jena, 1884.

[It is now generally admitted that, in the Vascular Cryptogams, as also in Mosses and Liverworts, the bodies of the spermatozoids are formed by the nuclei of the cells from which they arise. Only the cilia which they possess, and which obviously merely serve as locomotive organs, are said to arise from the surrounding cytoplasm. It is therefore in these plants also the nucleus of the male cell which effects the fertilization of the ovum. See Göbel, 'Outlines of Classification and Special Morphology,' trans-

Thus the nuclear substance must be the sole bearer of hereditary tendencies, and the facts ascertained by van Beneden in the case of *Ascaris* plainly show that the nuclear substance must not only contain the tendencies of growth of the parents, but also those of a very large number of ancestors. Each of the two nuclei which unite in fertilization must contain the germ-nucleoplasm of both parents, and this latter nucleoplasm once contained and still contains the germ-nucleoplasm of the grandparents as well as that of all previous generations. It is obvious that the nucleoplasm of each antecedent generation must be represented in any germ-nucleus in an amount which becomes less as the number of intervening generations becomes greater; and the proportion can be calculated after the manner in which breeders, when crossing races, determine the proportion of pure blood which is contained in any of the descendants. Thus while the germ-plasm of the father or mother constitutes half the nucleus of any fertilized ovum, that of a grandparent only forms a quarter, and that of the tenth generation backwards only $\frac{1}{1024}$, and so on. The latter can, nevertheless, exercise influence over the development of the offspring, for the phenomena of atavism show that the germ-plasm of very remote ancestors can occasionally make itself felt, in the sudden reappearance of long-lost characters. Although we are unable to give a detailed account of the way in which atavism happens, and of the circumstances under which it takes place, we are at least able to understand how it becomes possible; for even a very minute trace of a specific germ-plasm possesses the definite tendency to build up a certain organism, and will develope this tendency as soon as its nutrition is, for some reason, favoured above that of the other kinds of germ-plasm present in the nucleus. Under these circumstances it will increase more rapidly than the other kinds, and it is readily conceivable that a preponderance in the quantity of one kind of nucleoplasm may determine its influence upon the cell-body.

Strasburger—supported by van Beneden's observations, but in opposition to the opinions of the latter—had already explained, in a manner similar to that described above, the process by which the hereditary transmission of certain characters takes place, and to this

lated by H. E. F. Garnsey, edited by I. B. Balfour, Oxford, 1887, p. 203, and Douglas H. Campbell, 'Zur Entwicklungsgeschichte der Spermatozoiden,' in Berichte d. deutschen bot. Gesellschaft, vol. v (1887), p. 120.—S. S.]

extent our opinions coincide. The nature of heredity is based upon the transmission of nuclear substance with a specific molecular constitution. This substance is the specific nucleoplasm of the germ-cell, to which I have given the name of germ-plasm.

O. Hertwig[1] has also come to the same conclusion: at an earlier date he had looked upon the coalescence of nuclei as the most essential feature in the process of fertilization. He now believes that this former opinion has been confirmed by the recent discoveries which have been shortly described above.

Although I entirely agree with Hertwig, as far as the main question is concerned, I cannot share his opinions when he identifies Nägeli's idioplasm with the nucleoplasm of the germ-cell. Nägeli's idioplasm certainly includes the germ-plasm, if I may retain this expression for the sake of brevity. Nägeli in forming his hypothesis did indeed start with the germ-cells, but his idioplasm not only represents the nucleoplasm of the germ-cells, but also that of all the other cells of the organism; all these nucleoplasms taken together constitute Nägeli's idioplasm. According to Nägeli, the idioplasm forms a network which extends through the whole body, and represents the specific molecular basis which determines its nature. Although this latter suggestion — the general part of his theory—is certainly valid, and although it is of great importance to have originated the idea of idioplasm in this general sense, in contrast to the somato-plasm ('Nährplasma'), it is nevertheless true that we are not justified in retaining the details of his theory.

In the first place the idioplasm does not form a directly continuous network throughout the entire body; and, secondly, the whole organism is not penetrated by a single substance of homogeneous constitution, but each special kind of cell must contain the specific idioplasm or nucleoplasm which determines its nature. There are therefore in each organism a multitude of different kinds of idioplasm. Thus we should be quite justified in generally speaking of Nägeli's idioplasm as nucleoplasm, and *vice versa*.

It is perfectly certain that the idioplasm cannot form a continuous network through the whole organism, if it is seated in the nucleus and not in the cell-body. Even if the bodies of cells are

[1] O. Hertwig, 'Das Problem der Befruchtung und der Isotropie des Eies.' Jena, 1885.

everywhere connected by fine processes (as has been proved in animals by Leydig and Heitzmann, and in plants by various botanists), they do not form a network of idioplasm but of somato-plasm; a substance which, according to Nägeli, stands in marked contrast to idioplasm. Strasburger has indeed already spoken of a 'cyto-idioplasm,' and it is certainly obvious that the cell-body often possesses a specific character, but we must in all cases assume that such a character is impressed upon it by the influence of the nucleus, or, in other words, that the direction in which the cell-substance is differentiated in the course of development is determined by the quality of its nuclear substance. So far, therefore, the determining nuclear substance corresponds to the idioplasm alone, while the substance of the cell-body must be identified with the somato-plasm ('Nährplasma') of Nägeli. At all events, in practice, it will be well to restrict the term idioplasm to the regulative nuclear substance alone, if we desire to retain the well-chosen terms of Nägeli's theory.

But the second part of Nägeli's theory of the idioplasm is also untenable. It is impossible that this substance can have the same constitution everywhere in the organism and during every stage of its ontogeny. If this were so, how could the idioplasm effect the great differences which obtain in the formation of the various parts of the organism? In some passages of his work Nägeli seems to express the same opinion; e.g. on page 31 he says, 'It would be practicable to regard—although only in a metaphorical sense—the idioplasms of the different cells of an individual as themselves different, inasmuch as they possess specific powers of production: we should thus include among these idioplasms all the conditions of the organism which bring about the display of specific activity on the part of cells.' It can be clearly seen from the passages immediately preceding and succeeding the above-quoted sentence, that Nägeli, in speaking of these changes in the idioplasm, does not refer to material, but only to dynamical changes. On page 53 he lays special stress upon the statement that 'the idioplasm during its growth retains its specific constitution everywhere throughout the organism,' and it is only 'within these fixed structural limits that it changes its conditions of tension and movement, and thus alters the forms of growth and activity which are possible at each time and place.' Against such an interpretation weighty objections can be raised. At present I will only mention that the meaning of the phrase 'conditions of tension and movement' ought to be made clear, and that we ought to be informed how it is that mere differences in tension can produce as many different effects as could have been produced by differences of constitution. If any one were to assert that in *Daphnidae*, or in any other forms which produce two kinds of eggs, the power of developing only after a period of rest, possessed by the winter-eggs, is based upon the fact that their idioplasm is identical with that of the summer-eggs, but is in another condition of tension, I should think such a hypothesis would be well worth consideration, for the animals which arise from the winter-eggs are identical with those produced in summer: the idioplasm which caused their formation must therefore be identical in its constitution; and can only differ in the two cases, as water differs from ice. But the case is quite otherwise in the stages of ontogeny. How many different conditions of tension ought to be possessed by one and the same idioplasm in order to correspond to the thousand different structures and differentiations of cells in one of the higher organisms? In fact it would be hardly possible to form even an approximate conception of an explanation based upon mere 'conditions of tensions and movement.' But, furthermore, difference in effect should correspond, at any rate to some extent, with difference in cause: thus the idioplasm of a muscle-cell ought to differ more from that of a nerve-cell and of a digestive-cell in the same individual, than the idioplasm of the germ-cell of one individual differs from that of other individuals of the same species; and yet, according to Nägeli, the latter small difference in the effect is supposed to be due to difference of quality in the cause—the idioplasm, while the former fundamental difference in the histological differentiation of cells is supposed to follow from mere difference 'of tension and movement.'

Nägeli's hypothesis appears to be self-contradictory; for, although its author recognizes the truth of the fundamental law of development, and explains the stages of ontogeny as an abbreviated recapitulation of phyletic stages, he nevertheless explains the latter by a different principle from that which he employs to explain the former. According to Nägeli, the stages of phylogeny are based upon true qualitative differences in the idioplasm: the germ-plasm of a worm is qualitatively different from that of *Amphioxus*, a frog, or a mammal. But if such phyletic stages occur crowded together in the ontogeny of a single species, they are said to be based upon different 'conditions of tension and movement' of one and the same idioplasm! It seems to me to be necessary to conclude that if the idioplasm, in the course of phyletic development, undergoes any alteration in specific constitution, such alterations must also take place in ontogeny; so far at least as the phyletic stages are repeated. Either the whole phyletic development is based upon different 'conditions of tension and movement,' or if this—as I believe—is impossible, the stages of ontogeny must be based upon qualitative alterations in the idioplasm.

Involuntarily the question arises—how is it that such an acute thinker fails to perceive this contradiction? But the answer is not far to seek, and Nägeli himself indicates it when he adds these words to the sentence quoted above: 'It follows therefore that if a cell is detached as a germ-cell in any stage of ontogenetic development, and from any part of the organism, such a cell will contain all the hereditary tendencies of the parent individual.' In other words, if we are restricted to different 'conditions of tension and movement' as an explanation, it seems to follow as a matter of course that the idioplasm can re-assume its original condition, and therefore that the idioplasm of any cell in the body can again become the idioplasm of the germ-cell; for this to take place it is only necessary that the greater tension should become the less, or *vice versa*. But if we admit a real change in constitution, then the backward development of the idioplasm of the cells of the body into germ-cells appears to be very far from a matter of course, and he who assumes it must bring forward weighty reasons. Nägeli does not produce such reasons, but considers the metamorphosis of the idioplasm in ontogeny as mere differences in the 'conditions of tension and movement.' This phrase covers the weak part of his theory; and I look upon it as a valuable proof that Nägeli has also felt that the phenomena of heredity can only find their explanation in the hypothesis of the continuity of the germ-plasm; for his phrase is only capable of obscuring the question as to how the idioplasm of the cells of the body can be re-transformed into the idioplasm of germ-cells.

I am of the opinion that the idioplasm cannot be re-transformed, and I have defended this opinion for some years past [1], although I have hitherto laid especial stress on the positive aspect of the question, viz. on the continuity of the germ-plasm. I have attempted to prove that the germ-cells of an organism derive their essential nature from the fact that the germ-plasm of each generation is carried over into that which succeeds it; and I have tried to show that during the development of an egg into an animal, a part of the germ-substance—although only a minute part—passes over unchanged into the organism which is undergoing development, and that this part represents the basis from which future germ-cells arise. In this way it is to a certain extent possible to conceive how it is that the complex molecular structure of the germ-plasm can be retained unchanged, even in its most minute details, through a long series of generations.

But how would this be possible if the germ-plasm were formed anew in each individual by the transformation of somatic idioplasm? And yet if we reject the 'continuity of the germ-plasm' we are compelled to adopt this latter hypothesis concerning its origin. It is the hypothesis adopted by Strasburger, and we have therefore to consider how the subject presents itself from his point of view.

I entirely agree with Strasburger when he says, 'The specific qualities of organisms are based upon nuclei'; and I further agree with him in many of his ideas as to the relation between the nucleus and cell-body: 'Molecular stimuli proceed from the nucleus into the surrounding cytoplasm; stimuli which, on the one hand, control the phenomena of assimilation in the cell, and, on the other hand, give to the growth of the cytoplasm, which depends upon nutrition, a certain character peculiar to the species.' 'The nutritive cytoplasm assimilates, while the nucleus controls the assimilation, and hence the substances assimilated possess a certain constitution and nourish in a certain manner the cyto-idioplasm and the nuclear idioplasm. In this way the cytoplasm takes part in the phenomena of construction, upon which the specific form of the organism depends. This constructive activity of the cyto-idioplasm depends upon the regulative influence of the nuclei.' The

[1] This opinion was first expressed in my lecture, 'Ueber die Dauer des Lebens,' Jena, 1882, translated as the first essay in the present volume.

nuclei therefore 'determine the specific direction in which an organism developes.'

The opinion—derived from the recent study of the phenomena of fertilization—that the nucleus impresses its specific character upon the cell, has received conclusive and important confirmation in the experiments upon the regeneration of Infusoria, conducted simultaneously by M. Nussbaum[1] at Bonn, and by A. Gruber[2] at Freiburg. Nussbaum's statement that an artificially separated portion of a *Paramaecium*, which does not contain any nuclear substance, immediately dies, must not be accepted as of general application, for Gruber has kept similar fragments of other Infusoria alive for several days. Moreover, Gruber had previously shown that individual Protozoa occur, which live in a normal manner, and are yet without a nucleus, although this structure is present in other individuals of the same species. But the meaning of the nucleus is made clear by the fact, published by Gruber, that such artificially separated fragments of Infusoria are incapable of regeneration, while on the other hand those fragments which contain nuclei always regenerate. It is therefore only under the influence of the nucleus that the cell substance re-developes into the full type of the species. In adopting the view that the nucleus is the factor which determines the specific nature of the cell, we stand on a firm foundation upon which we can build with security.

If therefore the first segmentation nucleus contains, in its molecular structure, the whole of the inherited tendencies of development, it must follow that during segmentation and subsequent cell-division, the nucleoplasm will enter upon definite and varied changes which must cause the differences appearing in the cells which are produced; for identical cell-bodies depend, *ceteris paribus*, upon identical nucleoplasm, and conversely different cells depend upon differences in the nucleoplasm. The fact that the embryo grows more strongly in one direction than in another, that its cell-layers are of different nature and are ultimately differentiated into various organs and tissues,—forces us to accept the conclusion that the nuclear substance has also been changed in nature, and that such changes take place during ontogenetic development

[1] M. Nussbaum, 'Sitzungber. der Niederrheinischen Gesellschaft für Natur- und Heilkunde.' Dec. 15, 1884.

[2] A. Gruber, 'Biologisches Centralblatt,' Bd. IV. No. 23, and V. No. 5.

in a regular and definite manner. This view is also held by Strasburger, and it must be the opinion of all who seek to derive the development of inherited tendencies from the molecular structure of the germ-plasm, instead of from preformed gemmules.

We are thus led to the important question as to the forces by which the determining substance or nucleoplasm is changed, and as to the manner in which it changes during the course of ontogeny, and on the answer to this question our further conclusions must depend. The simplest hypothesis would be to suppose that, at each division of the nucleus, its specific substance divides into two halves of unequal quality, so that the cell-bodies would also be transformed; for we have seen that the character of a cell is determined by that of its nucleus. Thus in any Metazoon the first two segmentation spheres would be transformed in such a manner that one only contained the hereditary tendencies of the endoderm and the other those of the ectoderm, and therefore, at a later stage, the cells of the endoderm would arise from the one and those of the ectoderm from the other; and this is actually known to occur. In the course of further division the nucleoplasm of the first ectoderm cell would again divide unequally, e. g. into the nucleoplasm containing the hereditary tendencies of the nervous system, and into that containing the tendencies of the external skin. But even then, the end of the unequal division of nuclei would not have been nearly reached; for, in the formation of the nervous system, the nuclear substance which contains the hereditary tendencies of the sense-organs, would, in the course of further cell-division, be separated from that which contains the tendencies of the central organs, and the same process would continue in the formation of all single organs, and in the final development of the most minute histological elements. This process would take place in a definitely ordered course, exactly as it has taken place throughout a very long series of ancestors; and the determining and directing factor is simply and solely the nuclear substance, the nucleoplasm, which possesses such a molecular structure in the germ-cell that all such succeeding stages of its molecular structure in future nuclei must necessarily arise from it, as soon as the requisite external conditions are present. This is almost the same conception of ontogenetic development as that which has been held by embryologists who have not accepted the doctrine of evolution:

for we have only to transfer the primary cause of development, from an unknown source within the organism, into the nuclear substance, in order to make the views identical.

It appears at first sight that the knowledge which has been gained by studying the indirect division of nuclei is opposed to such a view, for we know that each mother-loop of the so-called nuclear plate divides longitudinally into two exactly equal halves, which can be stained and thus rendered visible.

In this way each resulting daughter-nucleus receives an equal supply of halves, and it therefore appears that the two nuclei must be completely identical. This at least is Strasburger's conclusion, and he regards such identity as a fundamental fact, which cannot be shaken, and with which all attempts at further explanation must be brought into accord.

How then can the gradual transformation of the nuclear substance be brought about? For such a transformation must necessarily take place if the nuclear substance is really the determining factor in development. Strasburger attempts to support his hypothesis by assuming that the inequality of the daughter-nuclei arises from unequal nutrition; and he therefore considers that the inequality is brought about after the division of the nucleus and of the cell. Strasburger has shown, in a manner which is above all criticism, that the nucleus derives its nutrition from the cell-body, but then the cell-bodies of the two *ex hypothesi* identical daughter-nuclei must be different from the first, if they are to influence their nuclei in different ways. But if the nucleus determines the nature of the cell, it follows that two identical daughter-nuclei which have arisen by division within one mother-cell cannot come to possess unequal cell-bodies. As a matter of fact, however, the cell-bodies of two daughter-cells often differ in size, in appearance, and in their subsequent history, and these facts are sufficient to prove that in such cases the division of the nucleus must have been unequal. It appears to me to be a necessary conclusion that, in such an instance, the mother-nucleus must have been capable of splitting into nuclear substances of differing quality. I think that, in his argument, Strasburger has over-estimated the support afforded by exact observations upon indirect nuclear division. Certainly the fact, discovered by Flemming, and more exactly studied by Balbiani and Pfitzner, that, in nuclear division, the loops split longitudinally, is of great and even of fundamental importance. Furthermore, the observations, conducted last year by van Beneden, on the process of fertilization in *Ascaris*, have given to Flemming's discovery a clearer and more definite meaning than could have been at first ascribed to it. The discovery proves, in the first place, that the nucleus always divides into two parts of equal quantity, and further that in every nuclear division, each daughter-nucleus receives the same amount of nuclear substance from the father as from the mother; but, as it seems to me, it is very far from proving that the quality of the parent nucleoplasms must always be equal in the daughter-nuclei. It is true that the fact seems to prove this; and if we remember the description of the most favourable instance which has been hitherto discovered, viz. the process of fertilization in the egg of *Ascaris*, as represented by van Beneden, the two longitudinal halves of each loop certainly impress the reader as being absolutely identical (compare, for instance, loc. cit. Plate XIX, figs. 1, 4, 5). But we must not forget that we do not see the molecular structure of the nucleoplasm, but something which we can only look upon (when we remember how complex this molecular structure must be) as a very rough expression of its quantity. Our most powerful and best lenses just enable us to make out the form of separate stainable granules present in a loop which is about to divide: they appear as spheres and immediately after division as hemispheres. But according to Strasburger, these granules, the so-called microsomata, only serve for the nutrition of the nuclear substance proper, which lies between them unstainable, and therefore not distinctly visible. But even if these granules represent the true idioplasm, their division into two exactly equal parts would give us no proof of equality or inequality in their constitution: it would only give us an idea of their quantitative relations. We can only obtain proofs as to the quality of the molecular structure of the two halves by their effect on the bodies of the daughter-cells, and we know that these latter are frequently different in size and quality.

This point is so important that I must illustrate it by a few more examples. The so-called polar bodies (to be treated more in detail below) which are expelled during maturation from the eggs of so many animals, are true cells, as was first proved by Bütschli in Nematodes: their formation is due to a process of undoubted cell-division usually accompanied by a typical form of indirect nuclear division[1]. If any one is still in doubt upon this point, after the observations of Fol and Hertwig, he might easily be convinced of its truth by a glance at the figures (unfortunately too little known) which Trinchese[2] has published, illustrating this process in the eggs of certain gastropods. The eggs of *Amphorina coerulea* are in every way suitable for observation, being entirely translucent, and having large distinct nuclei which differ from the green cytoplasm in colour. In these eggs two polar bodies are formed one after the other: and each of them immediately re-divides: hence it follows that four polar bodies are placed at the pole of the egg. But how is it that these four cells perish, while the nucleus, remaining in the yolk and conjugating with the sperm-nucleus, makes use of the whole body of the egg and developes into the embryo? Obviously because the nature of the polar body is different from that of the egg-cell. But since the nature of the cell is determined by the quality of the nucleus, this quality must differ from the very moment of nuclear division. This is proved by the fact that the supernumerary spermatozoa which sometimes enter the egg do not conjugate with the polar bodies. According to Strasburger's theory, the objection might be urged that the different quality of the nuclei is here caused by the very different quantity of cytoplasm by which they are surrounded and nourished; but on the one hand the smallness of the cell-bodies which surround most polar globules must have some explanation, and this can only be found in the nature of the nucleus; and on the other hand the quantity of the cell-body which surrounds the polar globules of *Amphorina* is, as a matter of fact, somewhat larger than the sphere of green cytoplasm which surrounds the nucleus of the egg! The difference between the polar bodies and the egg-cell can thus only be explained on the supposition that, in the division of the nuclear spindle, two qualitatively different daughter-nuclei are produced.

There does not seem to be any objection to the view that the

[1] According to the observations of Nussbaum and van Beneden, the egg of *Ascaris* departs from the ordinary type, but I think that the latter observer goes too far when he concludes from the form of the nuclear spindle (of which the two halves are inclined to each other at an angle) that we have before us a process entirely different from that of ordinary nuclear division.

[2] Trinchese, 'I primi momenti dell' evoluzione nei molluschi,' Atti Acad. Lyncei (3) vii. 1879, Roma.

microsomata of the nuclear loops—assuming that these bodies represent the idioplasm—are capable of dividing into halves, equal in form and appearance, but unequal in quality. We know that this very process takes place in many egg-cells; thus in the egg of the earth-worm the first two segmentation spheres are equal in size and appearance, and yet the one forms the endoderm and the other the ectoderm of the embryo.

I therefore believe that we must accept the hypothesis that, in indirect nuclear division, the formation of unequal halves may take place quite as readily as the formation of equal halves, and that the equality or inequality of the subsequently produced daughter-cells must depend upon that of the nuclei. Thus during ontogeny a gradual transformation of the nuclear substance takes place, necessarily imposed upon it, according to certain laws, by its own nature, and such transformation is accompanied by a gradual change in the character of the cell-bodies.

It is true that we cannot gain any detailed knowledge of the nature of these changes in the nuclear substance, but we can very well arrive at certain general conclusions about them. If we may suppose, with Nägeli, that the molecular structure of the germ-idioplasm, or according to our terminology the germ-plasm, becomes more complicated according to the greater complexity of the organism developed from it, then the following conclusions will also be accepted,—that the molecular structure of the nuclear substance is simpler as the differences between the structures arising from it become less; that therefore the nuclear substance of the segmentation-cell of the earth-worm, which potentially contains the whole of the ectoderm, possesses a more complicated molecular structure than that of a single epidermic cell or nerve-cell. These conclusions will be admitted when it is remembered that every detail in the whole organism must be represented in the germ-plasm by its own special and peculiar arrangement of the groups of molecules (the micellae of Nägeli), and that the germ-plasm not only contains the whole of the quantitative and qualitative characters of the species, but also all individual variations as far as these are hereditary: for example the small depression in the centre of the chin noticed in some families. The physical causes of all apparently unimportant hereditary habits or structures, of hereditary talents, and other mental peculiarities, must all be contained in the minute quantity of germ-plasm which is possessed by the nucleus of a germ-cell;—not indeed as the preformed germs of structure (the gemmules of pangenesis), but as variations in its molecular constitution; if this be impossible, such characters could not be inherited. Nägeli has shown in his work, which is so rich in suggestive ideas, that even in so minute a space as the thousandth of a cubic millimetre, such an enormous number (400,000,000) of micellae may be present, that the most diverse and complicated arrangements become possible. It therefore follows that the molecular structure of the germ-plasm in the germ-cells of an individual must be distinguished from that of another individual by certain differences, although these may be but small; and it also follows that the germ-plasm of any species must differ from that of all other species.

These considerations lead us to conclude that the molecular structure of the germ-plasm in all higher animals must be excessively complex, and, at the same time, that this complexity must gradually diminish during ontogeny as the structures still to be formed from any cell, and therefore represented in the molecular constitution of its nucleoplasm, become less in number. I do not mean to imply that the nucleoplasm contains preformed structures which are gradually reduced in number as they are given off in various directions during the building-up of organs: I mean that the complexity of the molecular structure decreases as the potentiality for further development also decreases, such potentiality being represented in the molecular structure of the nucleus. The nucleoplasm, which in the grouping of its particles contains potentially a hundred different modifications of this substance, must possess far more numerous kinds and far more complex arrangements of such particles than the nucleoplasm which only contains a single modification, capable of determining the character of a single kind of cell. The development of the nucleoplasm during ontogeny may be to some extent compared to an army composed of corps, which are made up of divisions, and these of brigades, and so on. The whole army may be taken to represent the nucleoplasm of the germ-cell: the earliest cell-division (as into the first cells of the ectoderm and endoderm) may be represented by the separation of the two corps, similarly formed but with different duties: and the following cell-divisions by the successive detachment of divisions, brigades, regiments, battalions, companies, etc.; and as the groups become simpler so does their sphere of action become limited. It must be admitted that this metaphor is imperfect in two respects, first, because the quantity of the nucleoplasm is not diminished, but only its complexity, and secondly, because the strength of an army chiefly depends upon its numbers, not on the complexity of its constitution. And we must also guard against the supposition that unequal nuclear division simply means a separation of part of the molecular structure, like the detachment of a regiment from a brigade. On the contrary, the molecular constitution of the mother-nucleus is certainly changed during division in such a way that one or both halves receive a new structure which did not exist before their formation.

My opinion as to the behaviour of the idioplasm during ontogeny, not only differs from that of Nägeli, in that the latter maintains that the idioplasm only undergoes changes in its 'conditions of tension and movement,' but also because he imagines this substance to be composed of the preformed germs of structures ('Anlagen'). Nägeli's views are obviously bound up with his theory of a continuous network of idioplasm throughout the whole body; perhaps he would have adopted other conclusions had he been aware of the fact that the idioplasm must only be sought for in the nuclei. Nägeli's views as to ontogeny can be best seen in the following passages: 'As soon as ontogenetic development begins, the groups of micellae in the idioplasm which effect the first stage of development, enter upon active growth: such activity causes a passive growth of the other groups, and an increase in the whole idioplasm, perhaps to many times its former bulk. But the intensities of growth in the two series of groups are unequal, and consequently an increasing tension is produced which sooner or later, according to the number, arrangement, and energy of the active groups, necessarily renders the continuation of the process impossible. In consequence of such disturbance to the equilibrium, active growth now takes place in the next group, leading to fresh irritation, and this group then reacts more strongly than all the others upon the tension which first stimulated its activity. These changes are repeated until all the groups are gone through, and the ontogenetic development finally reaches the stage at which propagation takes place, and thus the original stage of the germ is reached.'

Hence, according to Nägeli, the different stages of ontogeny arise out of the activities of different parts of the idioplasm: certain groups of micellae in the idioplasm represent the germs ('Anlagen') of certain structures in the organism: when any such germ reacts under stimulation it produces the corresponding structure. It seems to me that this hypothesis bears some resemblance to Darwin's theory of pangenesis. I think that Nägeli's preformed germs of structures ('Anlagen') and his groups of such germs are highly elaborated equivalents of the gemmules of pangenesis, which, according to Darwin, manifest activity when their turn comes, or, according to Nägeli, when they react under stimulation. 'When a group of such germs, by their active growth or by their 'irritation,' have caused a similar active growth or a similar irritation in the next group,' the former may come to rest, or may remain in a state of activity together with its successor, for a longer or shorter period. Its activity may even last for an unlimited time, as is the case in the formation of leafy shoots in many plants.'

Here, again, we recognize the fact that Nägeli's whole hypothesis is intimately connected with the supposition that the entire mass of idioplasm is continuous throughout the organism. Sometimes one part of the idioplasm and sometimes another part is irritated, and then produces the corresponding organ. But if, on the other hand, the idioplasm does not represent a directly continuous mass, but is composed of thousands of single nucleoplasms which only act together through the medium of their cell-bodies, then we must substitute the conception of 'ontogenetic stages of development of the idioplasm' for the conception of germs of structure ('Anlagen'). The different varieties of nucleoplasm which arise during ontogeny represent, as it were, the germs of Nägeli ('Anlagen'), because, by means of their molecular structure, they create a specific constitution in the cell-bodies over which they have control, and also because they determine the succession of future nuclei and cells.

It is in this sense, and no other, that I can speak of the presence of preformed germs ('Anlagen') in the idioplasm. We may suppose that the idioplasm of the first segmentation nucleus is but slightly different from that of the second ontogenetic stage, viz.

that of the two following segmentation nuclei. Perhaps only a few groups of micellae have been displaced or somewhat differently arranged. But nevertheless such groups of micellae were not the germs ('Anlagen') of a second stage which pre-existed in the first stage, for the two are distinguished by the possession of a different molecular structure. This structure in the second stage, under normal conditions of development, again brings about the change by which the different molecular structure of the third stage is produced, and so on.

It may be argued that von Baer's well-known and fundamental law of development is opposed to the hypothesis that the idioplasm of successive ontogenetic stages must gradually assume a simpler molecular structure. The organization of the species has, on the whole, increased immensely in complexity during the course of phylogeny: and if the phyletic stages are repeated in the ontogeny, it seems to follow that the structure of the idioplasm must become more complex in the course of ontogeny instead of becoming simpler. But the complexity of the whole organism is not represented in the molecular structure of the idioplasm of any single nucleus, but by that of all the nuclei present at any one time. It is true that the germ-cell, or rather the idioplasm of the germ-nucleus, must gain greater complexity as the organism which arises from it becomes more complex; but the individual nucleoplasms of each ontogenetic stage may become simpler, while the whole mass of idioplasms in the organism (which, taken together, represent the stage in question) does not by any means lose in complexity.

If we must therefore assume that the molecular structure of the nucleoplasm becomes simpler in the course of ontogeny, as the number of structures which it potentially contains become smaller, it follows that the nucleoplasm in the cells of fully differentiated tissues—such as muscle, nerve, sense-organs, or glands—must possess relatively the most simple molecular structure; for it cannot originate any fresh modification of nucleoplasm, but can only continue to produce cells of the same structure, although it does not always retain this power.

We are thus brought back to the fundamental question as to how the germ-cells arise in the organism. Is it possible that the nucleoplasm of the germ-cell, with its immensely complex molecular structure, potentially containing all the specific peculiarities of an individual, can arise from the nucleoplasm of any of the body-cells,—a substance which, as we have just seen, has lost the power of originating any new kind of cell, because of the continual simplification of its structure during development? It seems to me that it would be impossible for the simple nucleoplasm of the somatic cells to thus suddenly acquire the power of originating the most complex nucleoplasm from which alone the entire organism can be built up: I cannot see any evidence for the existence of a force which could effect such a transformation.

This difficulty has already been appreciated by other writers. Nussbaum's[1] theoretical views, which I have already mentioned, also depend upon the hypothesis that cells which have once become differentiated for the performance of special functions cannot be re-transformed into sexual cells: he also concludes that the latter are separated from all other cells at a very early period of embryonic development, before any histological differentiation has taken place. Valaoritis[2] has also recognised that the transformation of histologically differentiated cells into sexual cells is impossible. He was led to believe that the sexual cells of Vertebrata arise from the white blood corpuscles, for he looked upon these latter as differentiated to the smallest extent possible. Neither of these views can be maintained. The former, because the sexual cells of all plants and most animals are not, as a matter of fact, separated from the somatic cells at the beginning of ontogeny; the latter, because it is contradicted by the fact that the sexual cells of vertebrates do not arise from blood corpuscles, but from the germinal epithelium. But even if this fact had not been ascertained we should be compelled to reject Valaoritis' hypothesis on theoretical grounds, for it is an error to assume that white blood corpuscles are undifferentiated, and that their nucleoplasm is similar to the germ-plasm. There is no nucleoplasm like that of the germ-cell in any of the somatic cells, and no one of these latter can be said to be undifferentiated. All somatic cells possess a certain degree of differentiation, which may be rigidly limited to one single direction, or may take place in one of many directions. All these cells are widely different from the egg-cell from which they originated: they are all separated from it by many generations of

[1] M. Nussbaum, 'Archiv für Mikroskopische Anatomie,' Bd. XVIII und XXIII.
[2] Valaoritis, 'Die Genesis des Thier-Eies.' Leipzig, 1882.

cells, and this fact implies that their idioplasms possess a widely different structure from the idioplasm, or germ-plasm, of the egg-cell. Even the nuclei of the two first segmentation spheres cannot possess the same idioplasm as that of the first segmentation nucleus, and it is, of course, far less possible for such an idioplasm to be present in the nucleus of any of the later cells of the embryo. The structure of the idioplasm must necessarily become more and more different from that of the first segmentation nucleus, as the development of the embryo proceeds. The idioplasm of the first segmentation nucleus, and of this nucleus alone, is germ-plasm, and possesses a structure such that an entire organism can be produced from it. Many writers appear to consider it a matter of course that any embryonic cell can reproduce the entire organism, if placed under suitable conditions. But, when we carefully look into the subject, we see that such powers are not even possessed by those cells of the embryo which are nearest to the egg-cell—viz. the first two segmentation spheres. We have only to remember the numerous cases in which one of them forms the ectoderm of the animal while the other produces the endoderm, in order to admit the validity of this objection.

But if the first segmentation spheres are not able to develope into a complete organism, how can this be the case with one of the later embryonic cells, or one of the cells of the fully developed animal body? It is true that we speak of certain cells as being 'of embryonic character,' and only recently Kölliker[1] has given a list of such cells, among which he includes osteoblasts, cartilage cells, lymph corpuscles, and connective tissue corpuscles: but even if these cells really deserve such a designation, no explanation of the formation of germ-cells is afforded, for the idioplasm of the latter must be widely different from that of the former.

It is an error to suppose that we gain any further insight into the formation of germ-cells by referring to these cells of so-called 'embryonic character,' which are contained in the body of the mature organism. It is of course well known that many cells are characterized by very sharply defined histological differentiation, while others are but slightly differentiated; but it is as difficult to imagine that germ-cells can arise from the latter as from the former. Both classes of cells contain idioplasm with a structure

[1] Kölliker, 'Die Bedeutung der Zellkerne,' etc.; Zeitschr. f. wiss. Zool. Bd. XLII.

different from that which is contained in the germ-cell, and we have no right to assume that any of them can form germ-cells until it is proved that somatic idioplasm is capable of undergoing re-transformation into germ-idioplasm.

The same argument applies to the cells of the embryo itself, and it therefore follows that those instances of early separation of sexual from somatic cells, upon which I have often insisted as indicating the continuity of the germ-plasm, do not now appear to be of such conclusive importance as at the time when we were not sure about the localization of the idioplasm in the nuclei. In the great majority of cases the germ-cells are not separated at the beginning of embryonic development, but only in some one of the later stages. A single exception is found in the pole-cells ('Polzellen') of Diptera, as was shown many years ago by Robin [1] and myself [2]. These are the first cells formed in the egg, and according to the later observations of Metschnikoff [3] and Balbiani [4], they become the sexual glands of the embryo. Here therefore the germ-plasm maintains a true unbroken continuity. The nucleus of the egg-cell directly gives rise to the nuclei of the pole-cells, and there is every reason to believe that the latter receive unchanged a portion of the idioplasm of the former, and with it the tendencies of heredity. But in all other cases the germ-cells arise by division from some of the later embryonic cells, and as these belong to a more advanced ontogenetic stage in the development of the idioplasm, we can only conclude that continuity is maintained, by assuming (as I do) that a small part of the germ-plasm persists unchanged during the division of the segmentation nucleus and remains mixed with the idioplasm of a certain series of cells, and that the formation of true germ-cells is brought about at a certain point in the series by the appearance of cells in which the germ-plasm becomes predominant. But if we accept this hypothesis it does not make any difference, theoretically, whether the germ-plasm becomes predominant in the third, tenth, hundredth, or millionth generation of cells. It therefore follows that cases of early separation of the germ-cells afford no proof of a direct

[1] 'Compt. rend.' Tom. LIV. p. 150.
[2] 'Entwicklung der Dipteren.' Leipzig, 1864.
[3] 'Zeitschr. f. wiss. Zool.' Bd. XVI. p. 389 (1866).
[4] 'Compt. rend.' Nov. 13, 1882.

persistence of the parent germ-cells in those of the offspring; for a cell the offspring of which become partly somatic and partly germ-cells cannot itself have the characters of a germ-cell; but it may nevertheless contain germ-idioplasm, and may thus transfer the substance which forms the basis of heredity from the germ of the parent to that of the offspring.

If we are unwilling to accept this hypothesis, nothing remains but to credit the idioplasm of each successive ontogenetic stage with a capability of re-transformation into the first stage. Strasburger accepts this view; and he believes that the idioplasm of the nuclei changes during the course of ontogeny, but returns to the condition of the first stage of the germ, at its close. But the rule of probability is against such a suggestion. Suppose, for instance, that the idioplasm of the germ-cell is characterized by ten different qualities, each of which may be arranged relatively to the others in two different ways, then the probability in favour of any given combination would be represented by the fraction $\left(\frac{1}{2}\right)^{10} = \frac{1}{1024}$: that is to say, the re-transformation of somatic idioplasm into germ-plasm will occur once in 1024 times, and it is therefore impossible for such re-transformation to become the rule. It is also obvious that the complex structure of the germ-plasm which potentially contains, with the likeness of a faithful portrait, the whole individuality of the parent, cannot be represented by only ten characters, but that there must be an immensely greater number; it is also obvious that the possibilities of the arrangement of single characters must be assumed to be much larger than two; so that we get the formula $\left(\frac{1}{p}\right)$, where $p$ represents the possibilities, and $n$ the characters. Thus if $n$ and $p$ are but slightly larger than we assumed above, the probabilities become so slight as to altogether exclude the hypothesis of a re-transformation of somatic idioplasm into germ-plasm.

It may be objected that such re-transformation is much more probable in the case of those germ-cells which separate early from the somatic cells. Nothing can in fact be urged against the possibility that the idioplasm of (e.g.) the third generation of cells may pass back into the condition of the idioplasm of the germ-cell; although of course the mere possibility does not prove the fact. But there are not many cases in which the sexual cells are separated so early as the third generation: and it is very rare for them to separate at any time during the true segmentation of the egg. In *Daphnidae* (*Moina*) separation occurs in the fifth stage of segmentation [1], and although this is unusually early it does not happen until the idioplasm has changed its molecular structure six times. In *Sagitta* [2] the separation does not take place until the archenteron is being formed, and this is after several hundred embryonic cells have been produced, and thus after the germ-plasm has changed its molecular structure ten or more times. But in most cases, separation takes place at a much later stage; thus in Hydroids it does not happen until after hundreds or thousands of cell-generations have been passed through; and the same fact holds in the higher plants, where the production of germ-cells frequently occurs at the end of ontogeny. In such cases the probability of a re-transformation of somatic idioplasm into germ-plasm becomes infinitely small.

It is true that these considerations only refer to a rapid and sudden re-transformation of the idioplasm. If it could be proved that development is not merely in appearance but in reality a cyclical process, then nothing could be urged against the occurrence of re-transformation. It has been recently maintained by Minot [3] that all development is cyclical, but this is obviously incorrect, for Nägeli has already shown that direct non-cyclical courses of development exist, or at all events courses in which the earliest condition is not repeated at the close of development. The phyletic development of the whole organic world clearly illustrates a development of the latter kind; for although we may assume that organic development is not nearly concluded, it is nevertheless safe to predict that it will never revert to its original starting-point, by backward development over the same course as that which it has already traversed. No one can believe that existing Phanerogams will ever, in the future history of the world, retrace all the stages of phyletic development in precise inverse order, and thus return to the form of unicellular Algae or Monera; or that existing placental mammals will develope into Marsupialia,

[1] Grobben, 'Arbeiten d. Wien. Zool. Instituts,' Bd. II. p. 203.
[2] Bütschli, 'Zeitschrift f. wiss. Zool.' Bd. XXIII. p. 409.
[3] 'Science,' vol. iv. No. 90, 1884.

Monotremata, mammal-like reptiles, and the lower vertebrate forms, into worms and finally into Monera. But how can a course of development, which seems to be impossible in phylogeny, occur as the regular method of ontogeny? And quite apart from the question of possibility, we have to ask for proofs of the actual occurrence of cyclical development. Such a proof would be afforded if it could be shown that the nucleoplasm of those somatic cells which (e.g. in Hydroids) are transformed into germ-cells passes backwards through many stages of development into the nucleoplasm of the germ-cell. It is true that we can only recognise differences in the structure of the idioplasm by its effects upon the cell-body, but no effects are produced which indicate that such backward development takes place. Since the course of onward development is compelled to pass through the numerous stages which are implied in segmentation and the subsequent building-up of the embryo, etc., it is quite impossible to assume that backward development would take place suddenly. It would be at least necessary to suppose that the cells of embryonic character, which are said to be transformed into primitive germ-cells, must pass back through at any rate the main phases of their ontogeny. A sudden transformation of the nucleoplasm of a somatic cell into that of a germ-cell would be almost as incredible as the transformation of a mammal into an amoeba; and yet we are compelled to admit that the transformation must be sudden, for no trace of such retrogressive stages of development can be seen. If the appearance of the whole cell gives us any knowledge as to the structure of its nuclear idioplasm, we may be sure that the development of a primitive germ-cell proceeds without a break, from the moment of its first recognizable formation, to the ultimate production of distinct male or female sexual cells.

I am well aware that Strasburger has stated that, in the ultimate maturation of the sexual cells, the substance of the nuclei returns to a condition similar to that which existed at the beginning of ontogenetic development; still such a statement is no proof, but only an assumption made to support a theory. I am also aware that Nussbaum and others believe that, in the formation of spermatozoa in higher animals, a backward development sets in at a certain stage; but even if this interpretation be correct, such backward development would only lead as far as the primitive germ-cell, and would afford no explanation of the further transformation of the idioplasm of this cell into germ-plasm. But this latter transformation is just the point which most needs proof upon any theory except the one which assumes that the primitive germ-cell still contains unchanged germ-plasm. Every attempt to render probable such a re-transformation of somatic nucleoplasm into germ-plasm breaks down before the facts known of the Hydroids, in which only certain cells in the body, out of the numerous so-called embryonic cells, are capable of becoming primitive germ-cells, while the rest do not possess this power.

I must therefore consider as erroneous the hypothesis which assumes that the somatic nucleoplasm may be transformed into germ-plasm. Such a view may be called 'the hypothesis of the cyclical development of the germ-plasm.'

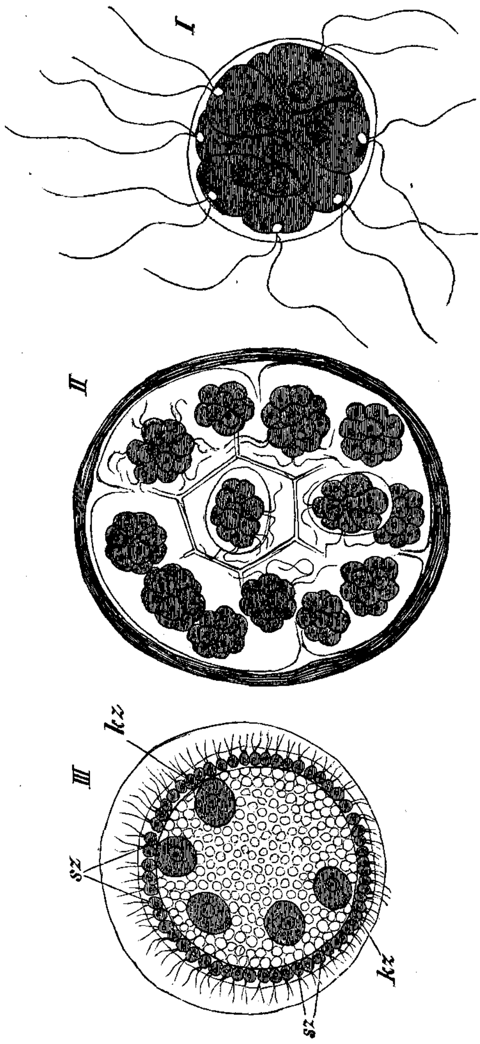
Nägeli has tried to support such an hypothesis on phyletic grounds. He believes that phyletic development follows from an extremely slow but steady change in the idioplasm, in the direction of greater complexity, and that such changes only become visible periodically. He believes that the passage from one phyletic stage to another is chiefly due to the fact that 'in any ontogeny, the very last structural change upon which the separation of germs depends, takes place in a higher stage, one or more cell-generations later' than it occurred in a lower stage. 'The last structural change itself remains the same, while the series of structural changes immediately preceding it is increased.' I believe that Nägeli, being a botanist, has been too greatly influenced by the phenomena of plant-life. It is certainly true that in plants, and especially in the higher forms, the germ-cells only make their appearance, as it were, at the end of ontogeny; but facts such as these do not hold in the animal kingdom: at any rate they are not true in the great majority of cases. In animals, as I have already mentioned several times, the germ-cells are separated from the somatic cells during embryonic development, sometimes even at its very commencement; and it is obvious that this latter is the original, phyletically oldest, mode of formation. The facts at our disposal indicate that the germ-cells only appear, for the first time, after embryological development, in those cases where the formation of asexually produced colonies takes place, either with or without alternation of generations; or in cases where alternation of generations occurs without the formation of such colonies. In a colony of polypes, the germ-cells are produced by the later generations, and not by the founder of the colony which was developed from an egg. This is also true of the colonies of Siphonophora, and the germ-cells appear to arise very late in certain instances of protracted metamorphosis (Echinodermata), but on the other hand, they arise during the embryonic development of other forms (Insecta) which also undergo metamorphosis. It is obvious that the phyletic development of colonies or stocks must have succeeded that of single individuals, and that the formation of germ-cells in the latter must therefore represent the original method. Thus the germ-cells originally arose at the beginning of ontogeny and not at its close, when the somatic cells are formed.

This statement is especially supported by the history of certain lower plants, or at any rate chlorophyll-containing organisms, and I think that these forms supply an admirable illustration of my theory as to the phyletic origin of germ-cells, as explained in my earlier papers upon the same subject.

The phyletic origin of germ-cells obviously coincides with the differentiation of the first multicellular organisms by division of labour[1]. If we desire to investigate the relation between germ-cells and somatic cells, we must not only consider the highly developed and strongly differentiated multicellular organisms, but we must also turn our attention to those simpler forms in which phyletic transitions are represented. In addition to solitary unicellular organisms, we know of others living in colonies of which the constituent units or cells (each of them equivalent to a unicellular organism) are morphologically and physiologically identical. Each unit feeds, moves, and under certain circumstances is capable of reproducing itself, and of thus forming a new colony by repeated division. The genus *Pandorina* (Fig. I), belonging to the natural order *Volvocineae*, represents such 'homoplastid' (Götte) organisms. It forms a spherical colony composed of ciliated cells, all of which are exactly alike: they are embedded in a colourless gelatinous mass. Each cell contains chlorophyll, and possesses a red eye-spot, and a pulsating vacuole. These colonies are propagated by the

[1] Among unicellular organisms, encysted individuals are often called germs. They sometimes differ from the adult organism in their smaller size and simpler structure (*Gregarinidae*), but they represent the same morphological stage of individuality.

sexual and asexual (Fig. II) methods alternately, although in the former case the conjugating swarm-cells cannot be distinguished with *certainty as male or female. In both kinds of reproduction*, each cell in the colony acts as a reproductive cell; in fact, it behaves exactly like a unicellular organism.




I. *Pandorina morum* (after Pringsheim), a swarming colony. II. A colony divided into sixteen daughter colonies: all the cells alike. III. A young individual of *Volvox minor* (after Stein), still enclosed in the wall of the cell from which it has been parthenogenetically produced. The constituent cells are divided into somatic cell (*sz*), and germ-cells (*kz*).

It is very interesting to find in another genus belonging to the same natural order, that the transition from the homoplastid to the heteroplastid condition, and the separation into somatic and reproductive cells, have taken place. In *Volvox* (Fig. III) the spherical colony consists of two kinds of cells, viz. of very numerous small ciliated cells, and of a much smaller number of large germ-cells without cilia. The latter alone possess the power of producing a new colony, and this takes place by the asexual and sexual methods alternately: in the latter a typical fertilization of large egg-cells by small spermatozoa occurs. The sexual differentiation of the germ-cells is not material to the question we are now considering; the important point is to ascertain whether here, at the very origin of heteroplastid organisms, the germ-cells, sexually differentiated or not, arise from the somatic cells *at the end of ontogeny*, or whether the substance of the parent germ-cell, during embryonic development, is *from the first* separated into somatic and germ-cells. The former interpretation would support Nägeli's view, the latter would support my own. But Kirchner [1] distinctly states that the germ-cells of *Volvox* are differentiated during embryonic development, that is, before the escape of the young heteroplastid organism from the egg-capsule. We cannot therefore imagine that the phyletic development of the first heteroplastid organism took place in a manner different from that which I have previously advocated on theoretical grounds, before this striking instance occurred to me. The germ-plasm (nucleoplasm) of some homoplastid organism (similar to *Pandorina*) must have become modified in molecular structure during the course of phylogeny, so that the colony of cells produced by its division was no longer made up of identical units, but of two different kinds. After this separation, the germ-cells alone retained the power of reproduction possessed by all the parent cells, while the rest only retained the power of producing similar cells by division. Thus *Volvox* seems to afford distinct evidence that in the phyletic origin of the heteroplastid groups, somatic cells were not, as Nägeli supposes, intercalated between the mother germ-cell and the daughter germ-cells in each ontogeny, but that the somatic cells arose directly from the former, with which they were previously identical, as they are even now in the case of *Pandorina*. Thus the continuity of the germ-plasm is established at least for the beginning of the phyletic series of development.

[1] Compare Bütschli in Bronn's 'Klassen und Ordnungen des Thierreichs,' Bd. I. p. 777.

The fact, already often mentioned, that in most higher organisms the separation of germ-cells takes place later, and often very late, at the end of the whole ontogeny, proves that the time at which this separation of the two kinds of cells took place, must have been gradually changed. In this respect the well-established instances of early separation are of great value, because they serve to connect the extreme cases. It is quite impossible to maintain that the germ-cells of Hydroids or of the higher plants, exist from the time of embryonic development, as indifferent cells, which cannot be distinguished from others, and which are only differentiated at a later period. Such a view is contradicted by the simplest mathematical consideration; for it is obvious that none of the relatively few cells of the embryo can be excluded from the enormous increase by division, which must take place in order to produce the large number of daughter individuals which form a colony of polypes. It is therefore clear that all the cells of the embryo must for a long time act as somatic cells, and none of them can be reserved as germ-cells and nothing else: this conclusion is moreover confirmed by direct observation. The sexual bud of a *Coryne* arises at a part of the Polype which does not in any way differ from surrounding areas, the body wall being uniformly made up of two single layers of cells, the one forming the ectoderm and the other the endoderm. Rapid growth then takes place at a single spot, and some of the young cells thus produced are transformed into germ-cells, which did not previously exist as separate cells.

Strictly speaking I have therefore fallen into an inaccuracy in maintaining (in former works) that the germ-cells are themselves immortal; they only contain the undying part of the organism—the germ-plasm; and although this substance is, as far as we know, invariably surrounded by a cell-body, it does not always control the latter, and thus confer upon it the character of a germ-cell. But this admission does not materially change our view of the whole subject. We may still contrast the germ-cells, as the undying part of the Metazoan body, with the perishable somatic cells. If the nature and the character of a cell is determined by the substance of the nucleus and not by the cell-body, then the immortality of the germ-cells is preserved, although only the nuclear substance passes uninterruptedly from one generation to another.

G. Jäger[1] was the first to state that the body in the higher organisms is made up of two kinds of cells, viz., ontogenetic and phyletic cells, and that the latter, the reproductive cells, are not a product of the former (the body-cells), but that they arise directly from the parent germ-cell. He assumed that the formation of germ-cells takes place at the earliest stage of embryonic life, and he thus believed the connexion between the germ-plasm of the parent and of the offspring had received a satisfactory explanation. As I have previously mentioned in the introduction, Nussbaum also brought forward this hypothesis at a later period, and also based it upon a continuity of the germ-cells. He assumed that the fertilized egg is divided into the cells of the individual and into the cells which effect the preservation of the species, and he supported this view by referring to the few known cases of early separation of the sexual cells. He even maintained this hypothesis when I had proved in my investigations on Hydromedusae that the sexual cells are not always separated from the somatic cells during embryonic development, but often at a far later period. Not only is the hypothesis of a direct connexion between the germ-cells of the offspring and parent broken down by the facts known in the Hydroids, and in the Phanerogams[2] which resemble them in this respect, but even the instances of early separated germ-cells quoted by Jäger and Nussbaum do not as a matter of fact support their hypothesis. Among existing organisms it is extremely rare for the germ-cells to arise directly from the parent egg-cell (as in Diptera). If, however, the germ-cells are separated only a few cell-generations later, the postulated continuity breaks down; for an embryonic cell, of which the offspring are partly germ-cells and partly somatic cells, cannot itself possess the nature of a germ-cell, and its idioplasm

[1] Gustav Jäger, 'Lehrbuch der Allgemeinen Zoologie,' Leipzig, 1878; II. Abtheilung. Probably on account of the extravagant and superficial speculations of the author, the valuable ideas contained in his book have been generally overlooked. It is only lately that I have become aware of Jäger's above-mentioned hypothesis. M. Nussbaum seems to have also arrived at the same conclusion quite independently of Jäger. The latter has not attempted to work out his hypothesis with any degree of completeness. The above-mentioned observations are followed immediately by quite valueless considerations, as, for instance, that the ontogenetic and phyletic groups are in concentric ratio! The author might as well speak of a quadrangular or triangular ratio!

[[2] Facts of the same kind are also known in the Vascular Cryptogams, Muscineae, Characeae, Florideae, etc.—S. S.]

cannot be identical with that of the parent germ-cell. In order to prove this, it is only necessary to refer to the arguments as to the ontogenetic stages of the idioplasm. In the above-mentioned instances, the continuity from the germ-substance of the parent to that of the offspring can only be explained by the supposition that the somatic nucleoplasm still contains some unchanged germ-plasm. I believe that the fundamental idea of Jäger and Nussbaum is quite correct: it is the same idea which has led me to the hypothesis of the continuity of the germ-plasm, viz., the conviction that heredity can only be understood by means of such an hypothesis. But both these writers have worked out the idea in the form of an hypothesis which does not correspond with the facts. That this is the case is also shown by the following words of Nussbaum—'the cell-material of the individual (somatic cells) can never produce a single sexual cell.' Such production undoubtedly takes place, not only in Hydroids and Phanerogams, but in many other instances. The germ-cells cannot indeed be produced by any indifferent cell of embryonic character, but by certain cells, and under circumstances which allow us to positively conclude that they have been predestined for this purpose from the beginning. In other words, the cells in question contain germ-plasm, and this alone enables them to become germ-cells.

As a result of my investigations on Hydroids [1], I concluded that the germ-plasm is present in a very finely divided and therefore invisible state in certain somatic cells, from the very beginning of embryonic development, and that it is then transmitted through innumerable cell-generations, to those remote individuals of the colony in which sexual products are formed. This conclusion is based upon the fact that germ-cells only occur in certain localized areas ('Keimstätten') in which neither germ-cells nor primitive germ-cells (the cells which are transformed into germ-cells at a later period) were previously present. The primitive germ-cells are also only formed in localized areas, arising from somatic cells of the ectoderm. The place at which germ-cells arise is the same in all individuals of the same species; but differs in different species. It can be shown that such differences correspond to different phyletic stages of a process of displacement, which tends to remove the

[1] Weismann, 'Die Entstehung der Sexualzellen bei den Hydromedusen.' Jena, 1883.

localized area from its original position (the manubrium of the Medusa) in a centripetal direction. For the purposes of the present enquiry it is unnecessary to discuss the reasons for this change of position. The phyletic displacements of the localized areas are brought about during ontogeny by an actual migration of primitive germ-cells from the place where they arose to the position at which they undergo differentiation into germ-cells. But we cannot believe that primitive germ-cells would migrate if the germ-cells could be formed from any of the other young cells of indifferent character which are so numerous in Hydroids. Even when the localized area undergoes very slight displacement, e. g. when it is removed from the exterior to the interior of the mesogloea [1], the change is always effected by active migration of primitive germ-cells through the substance of the mesogloea. Although the localized area has been largely displaced in the course of phylogeny, the changes in position have always taken place by very gradual stages, and never suddenly, and all these stages are repeated in the ontogeny of all existing species, by the migration of the primitive germ-cells from the ancestral area to the place where the germ-cells now arise. Hartlaub [2] has recently added a further instance (that of *Obelia*) to the numerous minute descriptions of these phyletic displacements of the localized area, and ontogenetic migrations of the primitive germ-cells, which are given in my work already referred to. The instance of *Obelia* is of especial interest as the direction of displacement is here reversed, taking place centrifugally instead of in a centripetal direction.

But if displacements of the localized areas can only take place by the frequently roundabout method of the migration of primitive germ-cells, we are obliged to conclude that such is the only manner in which the change can be effected, and that other cells are unable to play the rôle of the primitive germ-cells. And if other cells are unable to take this part, it must be because nucleoplasm of a certain character has to be present in order to form germ-cells, or according to the terms of my theory, the presence of germ-plasm is

[[1] I adopt this term, suggested by E. Ray Lankester and G. C. Bourne, as the name of the supporting lamina of Coelenterata. See 'Quart. Journ. Microsc. Sci.' Jan. 1887, p. 28.—E. B. P.]

[2] Dr. Clemens Hartlaub, 'Ueber die Entstehung der Sexualzellen bei Obelia.' Freiburg, Inaugural Dissertation: see also 'Zeitschrift für wissenschaftliche Zoologie.' Bd. XLI. 1884.

indispensable for this purpose. I do not see how we can escape the conclusion that there is continuity of the germ-plasm; for if it were supposed that somatic idioplasm undergoes transformation into germ-plasm, such an assumption would not explain why the displacement occurs by small stages, and with extreme and constant care for the preservation of a connexion with cells of the ancestral area. This fact can only be explained by the hypothesis that cell-generations other than those which end in the production of the cells of the ancestral area, are totally incapable of transformation into germ-cells.

Strasburger has objected that the transmission of germ-plasm along certain lines, viz. through a certain succession of somatic cells, is impossible, because the idioplasm is situated in the nucleus and not in the cell-body, and because a nucleus can only divide into two exactly equal halves by the indirect method of division, which takes place, as we must believe, in these cases. 'It might indeed be supposed,' says Strasburger, 'that during nuclear division certain molecular groups remain unchanged in the nuclear substance which is in other respects transformed, and that these groups are uniformly distributed through the whole organism; but we cannot imagine that their transmission could only be effected along certain lines.'

I do not think that Strasburger's objections can be maintained. I base this opinion on my previous criticism upon the assumed equality of the two daughter-nuclei formed by indirect division. I do not see any reason why the two halves must always possess the same structure, although they may be of equal size and weight. I am surprised that Strasburger should admit the possibility that the germ-plasm, which, as I think, is mixed with the idioplasm of the somatic cells, may remain unchanged in its passage through the body; for if this writer be correct in maintaining that the changes of nuclear substance in ontogeny are effected by the nutritive influence of the cell-body (cytoplasm), it follows that the whole nuclear substance of a cell must be changed at every division, and that no unchanged part can remain. We can only imagine that one part of a nucleus may undergo change while the other part remains unchanged, if we hold that the necessary transformations of nuclear substance are effected by purely internal causes, viz. that they follow from the constitution of the nucleoplasm. But that one part may remain unchanged, and that such persistence does, as a matter of fact, occur is shown by the cases above described, in which the germ-cells separate very early from the developing egg-cell. Thus in the egg of Diptera, the two nuclei which are first separated by division from the segmentation nucleus, form the sexual cells, and this proves that they receive the germ-plasm of the segmentation nucleus unchanged. But during or before the separation of these two nuclei, the remaining part of the segmentation nucleus must have become changed in nature, or else it would continue to form 'pole-cells' at a later period instead of forming somatic cells. Although in many cases the cell-bodies of such early embryonic cells fail to exhibit any visible differences, the idioplasm of their nuclei must undoubtedly differ, or else they could not develope in different directions. It seems to me not only possible, but in every way probable, that the bodies of such early embryonic cells are equal in reality as well as in appearance; for, although the idioplasm of the nucleus determines the character of the cell-body, and although every differentiation of the latter depends upon a certain structure of its nucleoplasm, it does not necessarily follow that the converse proposition is true, viz. that each change in the structure of the nucleoplasm must effect a change in the cell-body. Just as rain is impossible without clouds, but every cloud does not necessarily produce rain, so growth is impossible without chemical change, but chemical processes of every kind and degree need not produce growth. In the same manner every kind of change in the molecular structure of the nucleoplasm need not exercise a transforming influence on the cytoplasm, and we can easily imagine that a long series of changes in the nucleoplasm may appear only in the kind and energy of the nuclear divisions which take place, the cell-substance remaining unchanged, as far as its molecular and chemical structure is concerned. This suggestion is in accordance with the fact that during the first period of embryonic development in animals, the cell-bodies do not exhibit any visible differences, or only such as are very slight; although exceptional instances occur, especially among the lower animals. But even these latter (e.g. the difference in appearance of the cells of the ectoderm and endoderm in sponges and Coelenterata) perhaps depend more largely upon a different admixture of nutritive substances than upon any marked difference in the cytoplasm itself. It is obvious that, in the construction of the embryo, the amount of cell-material must be first of all increased, and that it is only at a later period that the material must be differentiated so as to possess various qualities, according to the principle of division of labour. Facts of this kind are also opposed to Strasburger's view, that the cause of changes in the nucleoplasm does not lie within this substance itself but within the cell-body.

I believe I have shown that theoretically hardly any objections can be raised against the view that the nuclear substance of somatic cells may contain unchanged germ-plasm, or that this germ-plasm may be transmitted along certain lines. It is true that we might imagine *a priori* that all somatic nuclei contain a small amount of unchanged germ-plasm. In Hydroids such an assumption cannot be made, because only certain cells in a certain succession possess the power of developing into germ-cells; but it might well be imagined that in some organisms it would be a great advantage if every part possessed the power of growing up into the whole organism and of producing sexual cells under appropriate circumstances. Such cases might exist if it were possible for all somatic nuclei to contain a minute fraction of unchanged germ-plasm. For this reason, Strasburger's other objection against my theory also fails to hold; viz. that certain plants can be propagated by pieces of rhizomes, roots, or even by means of leaves, and that plants produced in this manner may finally give rise to flowers, fruit and seeds, from which new plants arise. 'It is easy to grow new plants from the leaves of *Begonia* which have been cut off and merely laid upon moist sand, and yet in the normal course of ontogeny the molecules of germ-plasm would not have been compelled to pass through the leaf; and they ought therefore to be absent from its tissue. Since it is possible to raise from the leaf a plant which produces flower and fruit, it is perfectly certain that special cells containing the germ substance cannot exist in the plant.' But I think that this fact only proves, that in *Begonia* and similar plants, all the cells of the leaves or perhaps only certain cells contain a small amount of germ-plasm, and that consequently these plants are specially adapted for propagation by leaves. How is it then that all plants cannot be reproduced in this way? No one has ever grown a tree from the leaf of the lime or oak, or a flowering plant from the leaf of the tulip or convolvulus. It is insufficient to reply that, in the last-mentioned cases, the leaves are more strongly specialized, and have thus become unable to produce germ-substance; for the leaf-cells in these different plants have hardly undergone histological differentiation in different degrees. If, notwithstanding, the one can produce a flowering plant, while the others have not this power, it is of course clear that reasons other than the degree of histological differentiation must exist; and, according to my opinion, such a reason is to be found in the admixture of a minute quantity of unchanged germ-plasm with some of their nuclei.

In Sachs' excellent lectures on the physiology of plants, we read on page 723[1]—'In the true mosses almost any cell of the roots, leaves and shoot-axes, and even of the immature sporogonium, may grow out under favourable conditions, become rooted, form new shoots, and give rise to an independent living plant.' Since such plants produce germ-cells at a later period, we have here a case which requires the assumption that all or nearly all cells must contain germ-plasm.

The theory of the continuity of the germ-plasm seems to me to be still less disproved or even rendered improbable by the facts of the alternation of generations. If the germ-plasm may pass on from the egg into certain somatic cells of an individual, and if it can be further transmitted along certain lines, there is no difficulty in supposing that it may be transmitted through a second, third, or through any number of individuals produced from the former by budding. In fact, in the Hydroids, on which my theory of the continuity of the germ-plasm has been chiefly based, alternation of generations is the most important means of propagation.

## II. The Significance of the Polar Bodies.

We have already seen that the specific nature of a cell depends upon the molecular structure of its nucleus; and it follows from this conclusion that my theory is further, and as I believe strongly, supported, by the phenomenon of the expulsion of polar bodies, which has remained inexplicable for so long a time.

[1] English translation, by H. Marshall Ward. Oxford, 1887, Clarendon Press.