

# THE GERM-PLASM

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## A THEORY OF HEREDITY

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TO THE MEMORY  
OF  
CHARLES DARWIN



## NOTE BY TRANSLATOR

IN preparing the English edition of the present work, I have had the great advantage of being able to consult Professor Weismann personally with regard to many of the more *difficult* passages. Only those who have attempted to make a translation of an abstruse work from German manuscript, can appreciate the difficulties of rendering such a work into good English, and at the same time of keeping closely to the text. As the time in which the translation had to be prepared was a comparatively short one, I have been unable to revise the style as thoroughly as I could have wished, but trust that the author's meaning has been expressed with tolerable accuracy.

In the case of special technical terms which have no recognised English equivalents, I have in all cases added the German word in brackets the first time they are used. For the extremely useful and untranslatable word 'Anlage,' the somewhat awkward term 'primary constituent' has been used when it refers to the concrete vital units: in other cases, it has been rendered by 'rudiment;' or, when it has a more abstract meaning, by 'pre-disposition.' The words 'Eigenschaft,' 'Charakter,' 'Merkmal,' and 'Qualität,' are often used synonymously by the author, and have therefore been indiscriminately translated by 'characteristic,' 'character,' 'peculiarity,' and 'quality.'

I must express my thanks to Dr. G. H. Parker, of Harvard University, Cambridge, Mass., who kindly undertook a first revision of Chapters XIII. and XIV., and thereby rendered an earlier publication of the book possible; as well as to my friend and colleague Mr. Franck Arnold, for help in elucidating some of the more complicated sentences, and for many suggestions.

W. N. PARKER.

CARDIF, Nov. 28th, 1892.

“ Naturgeheimniss werde nachgestammelt.” — GOETHE.

## PREFACE

ANY attempt at the present time to work out a theory of heredity in detail may appear to many premature, and almost presumptuous: I confess there have been times when it has seemed so even to myself. I could not, however, resist the temptation to endeavour to penetrate the mystery of this most marvellous and complex chapter of life as far as my own ability and the present state of our knowledge permitted.

Even though the present attempt may be very imperfect and incomplete, I cannot regard it as premature. Our knowledge has increased during the last twenty years to such an extent, that it does not seem to be altogether a hopeless task to inquire into the actual processes on which the phenomena of heredity depend. It is, moreover, very essential that we should possess a theory of heredity, worked out in such a manner as to suggest new problems, which in their turn will lead to new solutions.

Previous hypotheses have been insufficient in this respect, owing to the fact that they have not been worked out in detail. They are rather to be regarded as paving the way to future theories, by merely formulating explanatory principles without professing to apply them to all the different groups of phenomena which come under the head of heredity, by which means alone their true value can be tested. Even Darwin's theory of 'pangenesis' was inade-



quate in this respect: owing to the comparatively limited number of facts then at his disposal, it could not but be what we may call an *ideal* theory; that is to say, it is founded upon certain principles without inquiring how far they are based upon actual facts. In themselves, such theories can hardly be looked upon as suggestive, for *if once the assumed principle is accepted*, all the phenomena are thereby explained, and the matter is open to no further doubt.

Let us assume that the germ contains millions of the primary constituents ('Anlagen') of all the most minute portions of the body; moreover, that these constituents are always present at the right place and in the right combination during the process of development; and, further, that they are capable of giving rise in their turn to the parts or organs to which they severally correspond. Such a theory explains everything, or nothing — the premises alone can be attacked. No new problems can arise from it till it has been placed upon a sound basis; the premises must be shown to be correct, and it must be proved that the germ is actually composed of primary constituents, which by some means or other become combined into groups and are capable of giving rise to the various parts and organs in question. Then, and then only, would the theory serve as an incentive to further investigations into the phenomena of heredity of all kinds, and experiments might be made which would support or contradict it.

There is no doubt a natural tendency to base experiments upon certain preconceived ideas; but it is one thing to be guided solely by such phenomena as may at the moment appear of especial importance, and another to base operations upon the completed outline of a theory founded upon

the principal data bearing upon the question. I have myself more than once abandoned a line of research undertaken in connection with the problem of heredity, because I felt that to proceed without the guidance of a theory more or less complete in itself, and developed on a basis of ascertained facts, would be little better than groping in the dark. The importance of such a theory lies primarily in its suggestiveness, by which alone it becomes a step towards the ideal at which we aim, viz., the formulation of *the true and complete theory*.

The growth of this book has been very gradual. What first struck me when I began seriously to consider the problem of heredity, some ten years ago, was the necessity for assuming the existence of a special organised and living *hereditary substance*, which in all multicellular organisms, unlike the substance composing the perishable body of the individual, is transmitted from generation to generation. This is the theory of *the continuity of the germ-plasm*. My conclusions led me to doubt the usually accepted view of the *transmission of variations acquired* by the body (soma); and further research, combined with experiments, tended more and more to strengthen my conviction that in point of fact no such transmission occurs. Meanwhile, the investigations of several distinguished biologists—in which I myself have had some share—on the process of fertilisation and conjugation, brought about a complete revolution in our previous ideas as to the meaning of this process, and further led me to see that the germ-plasm is composed of vital units, each of equal value, but differing in character, containing all the primary constituents of an individual. These '*ancestral germ-plasms*' ('Ahnentplasmen'), or '*ids*,' as I now prefer to call them, afforded additional matter where-

with to construct a theory of heredity, though much was wanting to render it complete.

In my last essay I certainly suggested the possibility of solving one of the most difficult problems in heredity — viz., the co-operation of the hereditary substance of the parents in sexual reproduction — by assuming the existence of these 'ids'; but I did not for a moment suppose that in doing so I had propounded a *complete and elaborated* theory of heredity, as some of my readers have thought to be the case; much still remained to be done first. I had as yet not touched upon such phenomena of heredity as have no direct bearing on the question of sexual reproduction, and had also abstained from any mention of the fundamental point of my theory of heredity — namely, the *constitution of the ids*. Although I pointed out that they must possess a complex structure which undergoes gradual and regular changes during the development of the individual from the egg-cell, I did not enter into any further details. This question remained in abeyance, for I was by no means sure whether the conception that I had formed on *à priori* grounds of the minute structure of the ids would prove tenable when viewed in the light of all the many phenomena of heredity. No conclusion could be arrived at respecting the structure of the ids till these phenomena had been individually considered.

All my investigations on the problem of heredity were so far only links, to be some day united into a chain which had as yet no existence. The question of the ultimate elements on which to base the theory was the very point on which I remained longest in doubt. The 'pangensis' of Darwin, as already mentioned, seemed to me to be far too independent of facts, and even now I am of the opinion that the

very hypothesis from which it derives its name is untenable. There is now scarcely any doubt that the entire conception of the production of the 'gemmules' by the body-cells, their separation from the latter, and their 'circulation,' is in reality wholly imaginary. In this regard I am still quite as much opposed to Darwin's views as formerly, for I believe that all parts of the body do not contribute to produce a germ from which the new individual arises, but that, on the contrary, the offspring owes its origin to a peculiar substance of extremely complicated structure, viz., the 'germ-plasm.' This substance can never be formed anew; it can only grow, multiply, and be transmitted from one generation to another. My theory might therefore well be denominated '*blasto-genesis*'—or origin from a germ-plasm, in contradistinction to Darwin's theory of '*pangenesi*s'—or origin from all parts of the body.

My doubts as to the validity of Darwin's theory were for a long time not confined to this point alone: the assumption of the existence of *preformed* constituents of all parts of the body seemed to me far too easy a solution of the difficulty, besides entailing an impossibility in the shape of an absolutely inconceivable aggregation of primary constituents. I therefore endeavoured to see if it were not possible to imagine that the germ-plasm, though of complex structure, was not composed of such an immense number of particles, and that its further complication arose subsequently in the course of development. In other words, what I sought was a substance from which the whole organism might arise by *epigenesis*, and not by *evolution*.\* After repeated attempts,

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\* The theory of 'evolution' or 'preformation' of the early physiologists supposed that all parts of the fully-formed animal or plant were present, in a minute form, in the germ. The rival theory of 'epigenesis'

in which I more than once imagined myself successful, but all of which broke down when further tested by facts, I finally became convinced that an epigenetic development is an *impossibility*. Moreover, I found an actual *proof of the reality of evolution*, which will be explained in the chapter on the structure of the germ-plasm. It is so simple and obvious that I can scarcely understand how it was possible that it should have escaped my notice so long.

It is gratifying to me to find myself at one with the great English naturalist Darwin, — as well as with de Vries and Wiesner, — at all events in the main point at issue ; and this agreement seems to me to point to the possibility of solving in the end the problem of heredity, which might seem to be open only to the wildest speculations : we may now perhaps hope to succeed in recognising the *probable* explanations among the many *possible* ones, and in finally selecting from among these the *real* solution of the problem. This will assuredly be the work of time, and our approach to the truth will be a very gradual one. But our path is marked out ; *reasoning supported by observation* will lead us to the goal. We are led by the observation of facts to form an opinion as to their bearing on each other. This gives rise to further problems and fresh investigations, which in their turn lead to a new interpretation. In this way light has before now been thrown on many a problem that seemed to baffle explanation. I need only mention the insight that we have now gained into the phenomenon of sexual reproduction.

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taught that there is no preformation of parts in the germ, but that the fully-formed organism is produced by a gradual process of differentiation. It will be seen that the word 'evolution,' as here used, has no connection with the doctrine of descent with which it is usually connected. — W. N. P.

In the same way we shall succeed in obtaining a firmer and firmer grasp of the problem of heredity, which but a short while ago appeared so utterly unapproachable.

What in this particular question appears to afford additional promise of success is the fact that we can in a sense approach it from two sides ; — namely, by observations, firstly, on the *phenomena* of heredity, and secondly, on the hereditary *substance* itself, with which we are now of course acquainted. We can now form an estimate as to whether an explanation of any particular phenomenon of heredity is of a merely hypothetical nature, or whether it may attain to the value of an established fact, inasmuch as we are in a position to judge, within certain limits at all events, whether it is consistent with the actual behaviour of the hereditary substance. Hitherto this has not been possible, and hence all previous theories, including both that of Darwin's gemmules and of Herbert Spencer's units, were up to a certain point purely speculative. We are now better off in this respect ; and I have no doubt that further research will enable us to penetrate far more deeply still into the complicated processes connected with the idioplasm, if we are prepared to reason on the results of our observations, and to utilise every theoretical advance as an incentive to fresh questions regarding the processes in connection with the distribution of the mysterious nuclear substance.

We are still far from having attained a complete insight into the matter, but I trust nevertheless that the present attempt at a theory of heredity is no mere work of the imagination ; and though it still be no more than an attempt, which will be followed by better ones, I venture to believe that time will prove it to contain more definite points, forming the centre of numerous possibilities, than many will

for the present be prepared to admit. Nevertheless I am well aware that it is but the beginning of a theory, and for this reason I have presented it in the form of an inquiry rather than of an established system. My plan has been not so much to *advance doctrines* as to *propound questions*, and to answer them with a greater or lesser degree of certainty, or in some cases even to leave them to be decided by future researches. I do not regard my theory as a complete and perfect one, but trust that it is of such a nature as to be capable of improvement and further development.

It has been my endeavour to write as simply and intelligibly as possible ; not as a specialist writing for specialists, but as one who desires to make his case clear to all interested in biological problems. For this reason a number of figures have been inserted, which, though perhaps superfluous for specialists, will, I trust, assist all who are less conversant with the subject, such as physiologists, medical men, and indeed all interested in natural science, to a clearer conception of the matters under discussion.

As a zoologist, I have naturally, in the first instance, considered the phenomena in their relation to animals, for every one must base his ideas on the facts most familiar to him. I have, however, done my best to lay due weight on the data afforded by the study of plants, and to take into account the views of botanists. It will be seen that the very facts which are furnished by certain hereditary phenomena in plants afford a strong support to certain fundamental points in my theory, and that even those which are at first sight in apparent contradiction, are in reality in perfect accordance with it.

It may perhaps be considered by medical men that I ought to have brought forward more evidence with regard

to diseases. We certainly possess a rich material on which observations concerning the transmission of diseases might be based, and this I have made use of so far as seemed expedient. It must, however, not be forgotten that the transmission of so-called hereditary diseases is not always due to a true process of heredity, but in some cases, at any rate, results from an infection of the germ. Unfortunately, we are not always able to distinguish between these two causes; and as long as this is the case, the data furnished by diseases can only be used with great caution, as will be shown in Chapter XII.

The manuscript of this book was practically completed by the end of April last, but as the translation had then to be made, its publication was delayed for some months. This will account for the fact that no mention, or only a brief one, has been made of researches which have appeared in the interval. My sincerest thanks are due to the translator — Professor W. N. Parker, — whose task has been by no means an easy one: apart from the mere knowledge of the two languages, an intimate acquaintance with the facts treated of and with the whole science of biology is essential in order to render the meaning of this complicated subject clear, and at the same time to reproduce the *original text* with anything like accuracy. I am of course unable to judge how far Mr. Parker has succeeded in clothing my ideas in good English, but am glad to state that they have been given very correctly, so far as I can judge from those parts which we have discussed together.

In conclusion, I must express my warmest thanks to the Government under which I have the good fortune to live, for the efficient way in which they have seconded my endeavours, by releasing me from my academical duties



during two winter sessions. My hearty thanks are also due to my friends and colleagues Professors Baumann, Lüroth, Wiedersheim, and Ziegler, as well as to Professor Goebel, of Munich, for information of various kinds ; and I am no less indebted to Miss Else Diestel, who, in addition to much help of a technical nature, has also been at the great pains of preparing an alphabetical index.

I thus venture to bring into the light of day a work which is the fruit of many years labour and of many doubts ; and even though but few of my results should remain unmodified, I hope nevertheless that my work has not been in vain ; for even error, if it originate in correct deductions, must become a step towards truth.

AUGUST WEISMANN.

FREIBURG, I/BR.,  
*May 19th. 1892.*