Intracellular Pangenesis

INCLUDING A PAPER ON

FERTILIZATION AND HYBRIDIZATION

BY

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TRANSLATED FROM THE GERMAN

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FOREWORD

The *Intracellulare Pangenesis,* of Hugo de Vries, was such a source of stimulation to me at the time of its appearance that I feel greatly indebted to its author. By creative imagination Hugo de Vries predicted much in his book that gained a material basis only through the histological research of the following decades. That is what makes the study of his book to-day as interesting as it is instructive.

In his paper, entitled *Befruchtung und Bastardirung,* a translation of which is included in this volume, de Vries has shown the same faculty of utilizing our present knowledge from every point of view, and of looking prophetically into the future. For in this paper also, on the ground of theoretical considerations, he predicted phenomena which were to furnish the basis for our conceptions of fertilization and heredity, but which have become actually known to us only through later works on the most intimate processes of nuclear division.

Therefore I gladly comply with the wish of the translator to introduce his translation with a few words. I say expressly "to introduce," for works of Hugo de Vries do not need a recommendation.

Bonn,
June, 1908.

E. STRASBURGER.

my well-abused hypothesis of pangenesis"

Charles Darwin, *Autobiography*.

TRANSLATOR'S PREFACE

Every student of heredity is brought face to face with the problem of some mechanism of inheritance. Pangenesis was Darwin's solution of this problem. But it was not in the form in which Darwin left it that pangenesis became directly fruitful of results; and no one felt the insufficiency of his hypothesis more keenly than Darwin himself. Writing to Asa Gray in 1867 he said: "The chapter on what I call Pangenesis will be called a mad dream . . . . . but at the bottom of my own mind I think it contains a great truth."[1] And to J. D. Hooker, in 1868, he wrote: "I feel *sure* if Pangenesis is now still born it will, thank God, at some future time reappear, begotten by some other father, and christened by some other name."[2]

Many men discerned the weak features of the hypothesis, but to Hugo de Vries belongs the credit of having detected the "great truth" it contained. He became its "other father," and rechristened it with another name —a name more nearly like the original, no doubt, than Darwin could have imagined.

The pangenesis of Darwin was hardly susceptible of experimental verification, except to the extent that a more intimate acquaintance with the facts showed that the assumption of a transportation of "gemmules" was super-

[1]Darwin, C. Life and Letters. **2**: 256. New York, 1901.
[2]*Loc. cit.* p. 261.

fluous. But it contained the germ of de Vries's intracellular pangenesis, the direct progenitor of the mutation-theory. It was primarily because of this genetic relationship, together with the masterful way in which the hypothesis is developed, and the accompanying wealth of illustration, that the little German volume, here done into English, was deemed worthy of translation at the present time.

As those who have followed the more recent literature of theoretical biology well know, Delage has argued against accepting any of the micromeric theories of the structure of protoplasm. His argument is based upon the idea that, by the law of probabilities, no one can ever, by pure imagination, correctly conceive of the ultimate structure of protoplasm in detail. Kellogg[3] cites this criticism of Delage as "a sufficient reason against accepting any one of these highly developed theories of the structural and functional capacity of invisible life units." Possibly this is correct, but that depends upon what the given hypothesis is to be accepted for. Of course no unverified hypothesis should be accepted for truth. As soon as the hypothesis can be so accepted it ceases to be a hypothesis, or even a theory, and passes into the rank of ascertained fact.

But that the argument of Delage can be advanced as a reason for rejecting any hypothesis, not inherently improbable or absurd, as a working hypothesis, a point of departure for further experiments, serving to orient a whole body of investigators, seems to me entirely to miss the point of the purpose of a hypothesis. Hypotheses are not statements of truth, but instruments to be used in the ascertainment of truth. Their value does not de-

[3]Kellogg, V. L., Darwinism To-day. p. 223. New York, 1907.

pend upon ultimate verification, but is to be measured by their effect upon scientific research. All this is now a truism.

What does it argue that no one, as Delage insists, ever anticipated by imagination the striation of muscle fibers, the existence of chromosomes and centrosomes, or any other fact of minute structure revealed by the microscope. May it not be asked in reply how long we should have had to wait for the discovery of the inert gases of the atmosphere, the accessory chromosome, and the ion, had they not first been conceived in imagination and formally embodied in working hypotheses? It is not pleasant to contemplate what the effect on the development of chemical science would have been had Dalton's (micromeric) hypothesis of indivisible units been rejected on the *a priori* grounds that the ultimate structure of matter is beyond the power of the human intellect to imagine in detail.

The hypothesis of intracellular pangenesis can never be absolutely demonstrated as true—can never advance beyond the rank of a theory—because the hypothetical pangens are conceived to be invisible, ultra-microscopic units, whose existence can never be more than inferred; but the formulation of the hypothesis marks the beginning of the greatest and most important forward step in the study of the origin of species since 1859. The notion of pangens became the parent-idea of unit-characters, offered a simple mechanism for the disjunction of characters in hybrids, and for continuous and discontinuous variation, and thus lead up directly to the conception of mutation as one method of the origin of species.[4] And, most important and significant of all, it resulted in per-

[4]Cf. footnote, p. 74 *infra*.

manently removing the entire question of organic evolution from the realm of ineffective speculation, and establishing it upon the firm basis of experimentation.

The term pangen is employed in its original sense by Strasburger in his paper on "Typische und allotypische Kertheilung."[5]

Recognizing the existence of some material entities as the ultimate units of heredity, conceiving of them as invisible, and accepting for them the name pangen, he interprets the chromatin granules (chromomeres), which can be directly seen, as larger or smaller pangen-complexes, and suggests that we designate them "pangenosomes." The pangenosomes, owing to a "certain elective affinity," he considers as combining into ids, (from the idioplasm, of Nägeli), and the ids, in turn, into chromosomes.[6]

Referring to de Vries's supposition, that the pangens influence the cytoplasm by wandering out into it from the nucleus and thus changing from an inactive to an active state, Strasburger[7] records his failure to detect any visible evidence that the bodies which he calls pangens thus wander out from the nucleus into the cytoplasm, but refers to the period in cell-division when the nuclear membrane disappears and the spindle forms, as serving to bring the chromosomes into direct contact with the cytoplasm, and thus establishing a condition favorable for the "formative influencing" of the cytoplasts by the nucleoplasts. A similar influence might also result from extranuclear nucleoli distributed in the cytoplasm. In the fertilization

[5]Jahrb. Wiss. Bot. **42**: 1-71. 1905.

[6]Mottier's use of the word pangen to designate the visible chromomeres (*Ann. Bot.* **21**; 307-347. 1907.), employs the term in a sense entirely at variance with that for which it was originally proposed (*cf.* p. 49.)

[7]*loc. cit.* p. 74.

of the egg he postulates a fusion of maternal with paternal pangens.[8] Thus, in the gametophytic generation, the pangens must be considered as univalent (*haploid*), in the sporophytic as bivalent (*diploid*). This would lead us to look for larger nuclei in the cells of the sporophyte than in those of the gametophyte. This hypothesis was verified in a number of plants, widely separated systematically. In *Taxus baccata,* for example, the nuclei of the prothallus were noticeably smaller than those of the sporophyte: and in nuclei with equally marked granulation, Strasburger counted fifty granules in an optical section of the nuclei of the nucellus, and only one-half that number in the nuclei of the adjacent prothallus.

But I cite this paper of Strasburger's chiefly to show how the hypothesis of intracellular pangenesis, in other hands that its author's, may assist in forming some comprehensible picture of the mechanism of matter in the living state. The idea and the term pangen are also adopted by Pfeffer in his Physiology of Plants.[9]

At the suggestion of Professor de Vries, a translation of his Haarlem *Vortrag* on *"Befruchtung und Bastardierung"* is included in this volume, for the purpose of showing the bearing of more recent research on the hypothesis of intracellular pangenesis, and of thus bringing the problem more nearly down to date. The translation of this *Vortrag* also appeared in "The Monist," for November, 1909.

It is a pleasure to record my profound gratitude to Professor de Vries for his careful reading and annotation of the manuscript of the translation, and for his interest and encouragement throughout the undertaking.

[8] *loc. cit.* p. 61.

[9] Pfeffer W. The Physiology of Plants. Eng. Trans. by Alfred J. Ewart. **1**: 49. Oxford, 1900.

I am deeply indebted to Professor Strasburger for his kindness in preparing an introductory note, and wish, also to express my sincere thanks to Miss Marie Onuf, whose invaluable assistance rendered the completion of the work possible.

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Nov. 13, 1909.

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