SOME REMARKS ABOUT UNITS IN HEREDITY

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THE Problem of Heredity has been treated in somewhat different ways by the several lines of biological research. The interests and points of view in the different branches of the sciences concerning Living Nature are not identical; hence we find rather different conceptions of the processes and materials operating in Heredity. The experimental Biology and Physiology of modern times have reached conceptions differing in principle from the conceptions of the mere descriptive conventional »Natural History» of old.

The description of organisms, gathered in nature or marshalled in collections, has created the terminologies of Botany and Zoology and the art of such terminology is essentially morphological. The »characters» of the organism's, i. e. forms, special development, presence or absence of composing »organs» and more or less autonomic structure-elements in animals or plants, are the units with which classical Natural History has mostly been operating. The anatomical and, later, the histological analyses of organisms have mostly been carried through in a morphological spirit; and the Cell-Theory gave in its time to the cell the rank of an ultimate and (relative) independent »morphological unit» of the bodies. The modern cytological nuclear analyses, the chromosome-researches, have at any rate in their starting point a pure morphological nature.

Morphology dissects the organisms into special *Parts,* proceeding towards a desintegration into smallest possible independent units of the body. Regarding Heredity and Variability we see, for instance, WEISMANN operating with the notion of smallest independently varying parts of the organism (»selbständig variierende Teile»). For such alleged units of the fully developed organism this prominent »morpho-biologist» supposed special organic representatives in the »germplasm» (»Keimplasma»), i. e. the total of those structural or constitutional elements in the sexual cells or fertilized ova etc., which for the zygote in question determine the possibilities of development.

This view is in its main points of very ancient origin. The Hippocratic school and a long series of authors including CHARLES DARWIN (»Pangenesis») have promulgated such conceptions. Their purely morphological nature can be emphasized in these words: *Constituent Parts of the Individual* represented through special *Particles in the Sexual Cells.* WEISMANN in his speculations as to »Germinal-selection» proceeded to absurdity in assuming independently living and competing »Biophores» — as yet the most ultra-morphological standpoint in the literature of Genetics. WEISMANN continued to vindicate the *parts* of the body as units in Variation and Heredity, even after full appreciation of Mendelism was attained.

Of course parts were the most popular hereditary units of old: the nose of your father, the eyes of your mother and the expressive mouth of a grandfather may be elements of your natural inheritance. But even *Qualities* have been regarded as units, especially those localised in special organs: The red colour of your grandmother's hair and her delicate complexion may be inherited as well as musical endowment and other probably brain-localised mental qualities. In such cases *parts* and *qualities* might be regarded as inseparable characters (»Merkmale»), i. e. as determined by the same »elements» in the zygote. We need not enter the discussions of ARISTOTLE as to differences between homogeneous parts (tissues) and composite parts (hands, feet, face and so on); some analogies between his views and the ideas of aggregated or particulate inheritance in GALTON's publications may only be pointed out here en passant. The profound accordance between GALTON's Stirp-theory, WEISMANN's primary Germplasm-teachings and ARISTOTLE's old original idea of continuity in Heredity (all in their turn rightly discrediting the over and over alleged heredity of »acquired qualities») has been mentioned by the present author on several occasions. Here it is only of interest to emphasize the purely morphological nature of these teachings.

But besides a morphological analysis or »dissection» of organisms in their parts there is a somewhat different view of analysing the nature of an organism, viz. separating its more physiological features, its different faculties or as we say »properties». A most conspicuous example is furnished already in 1826 by SAGERET when hybridizing two melons, one with yellow and sweet pulp and another with white and acid pulp. The progeny of this hybrid exhibited the four combinations: yellow-sweet, yellow-acid, white-sweet and white-acid — an elegant pre-Mendelian case. The said, morphologically speaking, homogeneous

organ, the pulp — or its cells or groups of cells — cannot here be represented in the fertilized ovum by one »unit» in the Weismannian sense, but the four different properties: white, yellow, sweet and acid seem to be »*unit-characters*» in the same sense in which primary Mendelism has later used this term. Already NÄGELI and in more recent time HUGO DE VRIES have, perhaps more or less conscious of the contradiction to the Weismannian »representation of parts», regarded such different Properties (»Einzeleigenschaften») as analytical elements in the hereditary nature of organisms.

The properties or rather the possibility of their realisation (I should say: the genotypical factors in question) may be represented throughout the individual; the local conditions in the different regions of an organism may prevent their appearance — and in many cases a special property can only be obviously manifest in specialized organs, for instance the colour of the iris in the eyes, the special negro-pigments in the surface of the body and so on.

But here a note should be introduced. WEISMANN and so far I see, ex parte also DE VRIES have assumed that in Ontogenesis the dividing cells during the whole routine of development must totally use a good deal of their assumed representative elements (be it representatives of parts or of properties). Hence the different parts of the mature organism might not have kept the same representatives; at any rate they could not have a complete (active) set of these assumed elements. It is easily seen, that such views indicate a fundamental (I should say genotypical) difference between the several parts of an organism. Also these views are mainly morphologically stamped. Morphology operates with »descriptive differences» (I should say phenotypical differences), taking such differences as the essentials. Therefore it does not much matter whether we speak of unit-parts or unit-characters, both concepts are equally morphological ideas — neither physiological nor chemico-biological. In old times a morphological spirit governed also ex parte Chemistry and, as we can easily understand, Mineralogy — this science with Botany and Zoology being the main »(o-)logies» of the »Natural kingdom». The characters and properties of natural objects were regarded somewhat as special entities; for instance the qualities »yellow», »hard», »fusible», »combustible» and so on were inherent principles, elements of the nature of sulphur.

Primary Mendelism operated in a similar way with characters, which in the experiments obviously behaved as units. Almost the whole bulk of »Mendelian» experiences from the first enthusiastic

years after the rediscovery of MENDEL's laws coincided splendidly with the conception of one independently separable representative in the gamete for each »Mendelian unit-character» in the mature organism. The morphological stamp of this initiated »analytical dissection» of the collective character of an organism into alleged unit-characters through hybridization and continued breeding, has obtained a pregnant expression in BATESON's term »Allelomorphs» for the units in »Mendelian inheritance».

It was undoubtedly a step forward to leave the notion of unit-*parts* in favour of the notion of unit-*characters*. Now this notion too is absolutely untenable. Nowadays each of BATESON's allelomorphs are not regarded as a kind of germ (»Anlage») for a corresponding unit-character. My term »*gene*» was introduced and generally accepted as a short and unprejudiced word for unit-factors in the — as to heredity — essential constitution of gametes and zygotes, but originally I was somewhat possessed with the antiquated morphological spirit in GALTON's, WEISMANN's and MENDEL's viewpoints. From a physiological or chemico-biological standpoint we must a priori in characters or developed parts of organisms see *Reactions* of the (I should say genotypical) constitution belonging to the zygote in question; and from this point of view *there are no unit-characters at all!* Undoubtedly all scientific geneticists now are or ought to be in accord as to this matter. But in the language of Genetics we meet with some unhappy old-fashioned expressions, relics of obsolete conceptions — the worst of all these relics is probably the expression *Transmission* where no transmission exists but where continuity is found! »Transmission» is here a kind of Hippocratic-Lamarckian slang-word, very misleading. Here however we shall only try to exterminate in Genetics (perhaps a hard task!) the term *unit-character* as indicating a notion that is totally inadequate and hence noxious for Genetics, for words too often govern thoughts!

Descriptive Natural History operates legitimately with such notions, and when we compare the different individuals and generations in our breeding series, we of course use methods of zoological, botanical or chemical description. Here we are dealing with the realised *Phenotypes,* i. e. the reactions, direct or indirect (hormones etc.) of the genotypes with the ambient conditions. We may in some way »dissect» the organism descriptively, using all the tricks of terminology as we please.

But that is not allowed in Genetical explanation. Here, in the pre-

sent state of research, we have especially to do with such genotypical units as are separable, be it independently or in a more or less mutual linkage. Certainly by far the most comprehensive and most decisive part of the whole genotype does not seem to be able to segregate in units; and as yet we are mostly operating with »characters», which are rather superficial in comparison with the fundamental Specific or Generic nature of the organism. This holds good even in those frequent cases where the characters in question may have the greatest importance for the welfare or economic value of the individuals.

We are very far from the ideal of enthusiastic Mendelians, viz. the possibility of dissolving genotypes into relatively small units, be they called genes, allelomorphs, factors or something else. Personally I believe in a great central »something» as yet not divisible into separate factors. The pomace-flies in MORGAN's splendid experiments continue to be pomace-flies even if they lose all »good» genes necessary for a normal fly-life, or if they be possessed with all the »bad» genes, detrimental to the welfare of this little friend of the geneticists.

Disregarding this (perhaps only provisional?) central »something» we should consider the numerous genes, which have been segregated, combined or linked in our modern genetic work. What have we really seen? The answer is easily given: We have only seen *Differences.* The famous relation 3 : 1 (1 : 2 : 1) indicates one single point of difference, the ratio 9 : 3 : 3 : 1 two points, and so on. Dominance does not at all indicate the presence of some positive unit, just as little as Recessivity indicates the lack of any unit. This is clearly seen, for instance in NILSSON-EHLE's oats-crossings, where one Mendelian unit may be responsible for one dominant and one or two recessive characters, also in such cases where dominance or recessivity is dependent upon external conditions, as in some *Drosophila*-experiments.

In the beginning of our modern »Mendelian era» one unit might be regarded as *the* unit of one descriptive character, for instance »yellow» in ripe peas, »starchy» in maize grains and so on. But when more complicated segregations were found, we conceived the idea of »construction»; for instance of colour and hoariness of stocks. In this case (quoted here as simplified as possible) each of the genes *A, B* and *C* when alone (i. e. without the others as elements in the genotype) shows no obvious reaction; but $A + B$ may cause the production of colour and $A + B + C$ colour and hoariness, $A + B$ as well as $B + C$ giving no observed reaction.

In such cases we spoke — and may perhaps continue to speak —

of a *synthetic* hybridization when A and B or $A + B$ and C were brought together, the character »colour» or »hoariness» being thus »constructed». An *analytical* hybridization was realised when for instance an organism with $A + B + C$ was hybridized with, say, $a + b + c$. Here in the F_2-generation »analysis» of »colour and hoary» was foud — as in Miss SAUNDERS' fine work.

Results like these might have raised hopes as to a possibility of segregating analytically the whole genotype into »factors» — and hence in a remote future we might be able to do some Homunculus-work viz. to construct organisms through the addition or artificial combination of discreet factors, stored perhaps in bottles or small tubes!!

But the nature of the genotypical units hitherto observed is highly problematic. When we regard Mendelian »pairs», *Aa, Bb* and so on, it is in most cases a *normal* reaction (character) that is the »allel» to an *abnormal*. Yellow in ripe pease is normal, the green is an expression for imperfect ripeness as can easily be proven experimentally e. g. by etherization. »No starch» in maize is evidently an abnormality and so in the many cases upon which BATESON — as it seemed with full reason — founded his for a time highly useful and suggestive but now abandoned hypothesis of »Presence and absence»: the »normal» almost always positive and dominant, the »abnormal» being (in a morphological spirit) expressed as a »loss».

Now the notions »normal» and »abnormal» in their valuing sense are not adequate for Genetic analysis, hence classifications according to such valuation are without interest. The question for us is this: what is the nature of the difference between A and a, B and b and so on? There is at present scarcely any doubt about the theory, that »Mendelian factors» are in some way bound in or to the chromosomes. The morphological view regards them as formed particles (say »morphs», ad modum »allelomorphs») of the chromosomes, an old Weismannian idea — mutatis mutandis. From a physiological standpoint we may prefer to regard local conditions (say »chemisms») in or on the chromosomes as responsible for those units — avoiding the hairsplitting remark that »chemisms» are ultimately in some way »particulate» — as all things, even energy, now seem to be.

If comparing an original (and in so far »normal») organism, for instance a wild purple *Lathyrus* or a wild grey mouse, with the undoubtedly derived cultivated organisms, for instance a white sweet pea and a yellow mouse, we might discover that there is one single genotypical point of difference between them, this difference may probably

consist in an alteration of the »chemism» at a special point of a chromosome. Now such alterations may be more or less different, and where several such differences exist in a certain locus of a chromosome we have the so-called »*multiple allelomorphs*». This rather cumbersome expression ought to be replaced — the »morph» eliminated. »Allelogene» seems a more neutral word. Perhaps the best expressions are »multiple allelos» and »multiple allelism», or — to be purely Greek — »*polyallelism*». At any rate multiple allelos are (for the chromosome theory) different states (chemisms) in the same locus of a chromosome. If we consider BAUR's beautiful case of three »allelos» in regard to chlorophyll-modifications, these corresponding »factors» may have analogous signs, e. g. $\mathfrak{A}$, *A*, *a* or the like. The rich material from the American *Drosophila*-researches of MORGAN's school has supplied many cases of multiple allelisms — most or all of them being different »abnormities» compared with the characters of the normal wild fly.

NILSSON-EHLE's famous experiences with cereals establishing the existence of »*equivalent factors*» or factors acting in almost the same way as to the phenotype in question, formed one of the most considerable extensions of Mendelism. Duly understood this discovery removes the idea of unit-characters, but perhaps the most important side of NILSSON-EHLE's principle of equivalent factors is the conquest for factorial analysis of the originally alleged »non-Mendelian» inheritance of many so-called »quantitative characters» in plants and animals — LANG of Zürich was a prominent initiator of these ideas. NILSSON-EHLE's work has also had the greatest influence on the discussion of the problem of Selection — the latest publications of our former antagonist CASTLE best illustrates that fact.

However, this matter shall not be discussed now. Here we wish to emphasize that equivalent or analogous factors in NILSSON-EHLE's sense may be regarded as the same or a rather similar state (chemism) in different chromosomes. This often so-called »polymerism» or »homomerism» (perhaps better »polygenism») must not be confused with multiple allelism (polyallelism) — *different states in the same locus* of one chromosome; polygenism on the contrary being the *same state localized in different chromosomes!*

But however far we may proceed in analysing the genotypes into separable genes or factors, it must always be borne in mind, that the characters of the organisms — their phenotypical features — are the reaction of the genotype in toto. The Mendelian units as such, taken per se are powerless.

To my mind the main question in regard to these units is this: Are the experimentally demonstrated units anything more than expressions for local deviations from the original (»normal») constitutional state in the chromosome?

Is the whole of Mendelism perhaps nothing but an establishment of very many chromosomical irregularities, disturbances or diseases of enormously practical and theoretical importance but without deeper value for an understanding of the »normal» constitution of natural biotypes? The Problem of Species, Evolution, does not seem to be approached seriously through Mendelism nor through the related modern experiences in mutations. Here again the word »normal» was used! It is a dangerous and somewhat illegitimate expression in Experimental Biology. Carnivorous animals, gnats, protozoa and bacteria etc. are »normal» beings, hence in Nature it is »normal» that several individuals are devoured, attacked by malaria or tuberculosis! Degeneration and mutations may be as »normal» as other results of combinations, separations, non-disjunctions etc. in the processes of gametogenesis and fertilization. »Nature is beautiful, but not correct» as a Danish saying goes. »Degeneration» or »Evolution» may be used respectively as terms for a given genetic process — depending on whether our more or less subjective valuation emphasizes a »bad» or »good» tendency!

Chromosomes are doubtless vehicles for »Mendelian inheritance», but *Cytoplasm* has its importance too. I cannot here enter into this problem from which in the near future we shall certainly have important news. Gametogenesis with chromosome-reductions, accompanied by reformations and, as it were, partial rejuvenescence of cell-structures, must in some way act as if especially organized for *obliterating* the individual's personally »acquired characters», which as a rule totally disappear in sexual reproduction — quite contrary to the popular traditional Hippocratic-Lamarckian views. Cytoplasm is perhaps more prone to »memory»; JOLLOS's experiments with Infusoria for instance seem to suggest such a case.

Continuity in inheritance, the cardinal idea of ARISTOTLE, is — as applied to Mendelian heredity — represented by the continuity of chromosomes in the forthcoming generations — but greatly complicated by disjunctions and recombinations of chromosome-pairs. This hereditary continuity is, in so far dissolved into a kind of regular periodic discontinuities: Mendelian heredity always operating with discreet genotypical elements. Hence differences are here always discontinuous

as chemical constitutional differences. Phenotypes however may show discontinuous as well as all degrees of continuous variation!

The genotypical constitution as belonging to every cell penetrates the whole individual with the more or less rare complications, where we may meet »vegetative» segregations or mutations. But these processes have nothing to do with the Weismannian conception of a regular disintegration of the active germplasm during ontogenesis already mentioned. The same holds good in the several cases in which only cells of the germcycle (»Keimbahn») have the full equipment of chromosomes and other granular structures, as for instance in Ascaries (BOVERI) and some beetles (HEGNER).

The Weismannian form of distinction between »Germplasm» and »Soma», viz. absolute independence does not exist in reality. The non-inheritance of acquired characters is not a consequence of this assumed independence or difference, but only a striking expression of the fact, that the external conditions may easily mould phenotypes in a more or less adaptive manner, but can hardly or rarely induce changes in the genotype. The Weismannian distinction »*Keimplasma—Soma*» which from the point of view of Genetics is totally obsolete has in its purely morphological nature nothing to do with our views; his categories are incommensurable with the distinction *Genotype—Phenotype*. In concluding these somewhat aphoristic remarks I have only to say that my terms »*Gene*», »genotypical» and so on have absolutely nothing to do with DE VRIES' expression »*Pangenes*» (1889) and their assumed behaviour as units. May I add that the Galtonian antithesis »*Nature-Nurture*» is not equivalent to our notions »Genotype-Phenotype», the phenotype being the *reaction* of the genotype (»nature») with the ambient conditions (»nurture»).