ON THE SPECIES CONCEPTION IN RELATION TO TAXONOMY AND GENETICS

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A phylogenetic system resulting from comparative morphological studies claims to be the expression of evolution. The character of any phylogenetic classification based on morphological studies exclusively is a speculative one. The fragmental facts procured by fossil relicts from earlier geological periods are also morphological and allow only the conclusion that evolution took place, but in which way changes evolved has not been stated by immediate observation.

Nobody escapes from the idea that all living beings existing yet, have originated from those in the past. It is undeniable that the vegetation which covered the earth in former periods has been changed. This historical process seen as a continuity possible by the power of reproduction of the organisms is called evolution. The idea of evolution which as a consequence of Darwinian views penetrated into taxomony is cause of the fact that species, families, ordines, phyla were considered to be more than categoric divisions; they should represent relationships or lines of descent. Taxomony got a fundamental frame: phylogeny!

The idea that higher organised (more complicated) beings should have risen from the lower organised (simple, or seemingly simple) ones is so plausible to human mind that a theory sufficiently adapted to this idea would be accepted natural and evident. In phylogeny the principle of evolution from lower to higher organisation is brought to expression. Plants and animals surrounding us, as seen from a phylogenetic point of view, form the youngest shoots of a branching system but about the very branching our knowledge is the least. Not of a single genus, not even of a single species we know the direct descent from a genus or a species in a former period. On the other hand we believe in seeing main lines of development in phylogeny, but as soon as we try to reconstruct details we are driven from facts into hypothesis. Phylogeny therefore is and can not be more than a speculative science. Aiming to fix the results of evolution, relationship by means of descent can be made plausible or perhaps even probable, but nothing of this kind can be proved.

In all theories of evolution a common tendency can be observed. They all intend to explain the origin of species. Lamarckism, Darwinism, Mutation- and Hybridizationtheories are as many trials to reveal the origin of the taxonomic units, the species. The two first mentioned theories are built upon philosophical bases, the latter two arose in a period in which genetics began to foot on solid ground.

It is evident that one's species conception must be influenced by one's point of view: the taxonomic and the genetic conception do not fully agree, the first being constructive, intending to delimite groups of organisms under considerable personal appreciation of morphological characters, the second being analytic, based on experimentally proved hereditary individual characters. The taxonomist takes a species as a morphological unit, the geneticist as a population of individuals of different constitution. With regard to the species conception the taxonomist and the geneticist are on similar terms as the author and the critic.

Till the time of LAMARCK the species conception was inviolable. It was the conviction that the species was a created unit (LINNAEUS) and that its constancy was beyond doubt. LAMARCK was the first who got insight in its variability and took from it the possibility of evolution. He meant that an individual could vary in a manner profitable for this individual. Essential to this theory is: reaction of the individual to the environment by direct adaptation; changes of the milieu could alter the type with hereditary effect. Darwinism propagated an allround variation. Only the best utilised individuals survived as a result of competition and nature itself selected the fittests. The most frequent form of a Linnean species was the type. As long as the type remained the fittest it kept the majority and constancy of the species to a certain extent was the result. Varieties were incipient species which by selection could raise to the rank of species. The title of the famous work "Origin of species by means of natural selection" shows that DARWIN understood evolution as the offspring of new species.

Characteristic to modern theories is that the points of comparison are changed under influence of genetics. Not the characters are inherited but the genes manifesting the characters during the individual development. The phenotypical appearance became of secundary importance. Though the moment of evolution still lies in the origin of species, the basis of all evolutionary consideration was the genotypical constitution, the hereditary units, transmitted from parents to children, forming a set completely present in the nucleus (intracellular pangenesis).

DE VRIES assumes the possibility of changes in the gene sets causing mutations. Not only latent genes may become active, or genes may get lost, but also genes may be formed de novo, in the latter case giving rise to progressive mutations. This kind of mutation implies evolution but various hypothetical considerations are necessary to emphasize the evolutionary moment (premutation periods) and as progressive mutations are very rare the direct significance for evolution diminishes considerably.

LOTSY's theory of evolution by means of hybridization tries to explain the origin of species by recombination of genes made possible by the power of sexual reproduction. To LOTSY's opinion the fusion of gametes of different constitution is the primary cause of all diversity among organisms. The surviving forms in a hybrid population are not the fittest ones selected by nature but those which are eventually best adapted to the existing circumstances. Isolation is the cause of differentiation of new species. Not selection of existing forms but succession determines the changes of flora and fauna in successive periods. No complicate hypothesis underlies this theory and the only assumption induced by the theory itself is polyphyletic origin of species.

One may doubt whether hybridizing was the only way evolution took, it is a matter of fact that crossing gives rise to considerable diversity. A single cross between two species e.g. *Tragopogon pratensis. T. porrifolius* shows more diversity in its progeny than any other phenomenon. The greatest advance in experimental evolution are the bi- and trigeneric hybrids (*Aegilotriticum*, TSCHERMACK; *Raphanobrassica*, KARPECHENKO).

The importance of hybridization as a cause of the origin of species cannot be better evidenced than by BAUR's latest publication (1932, p. 289). From experimental researches on species of *Antirrhinum* (sect. *Antirrhinastrum*) BAUR formerly explained the origin of new forms by mutation. On account of his investigations concerning forms occurring in Spain, however, he now declares (l. c.) that it is possible to explain the total abundance of diverse forms from the crossing of a few orginally present forms. He declares that he is able to reproduce all types of *Antirrhinum* that now can be found in Spain, Italy and North Africa by means of material consisting of only one form from each of the groups *Antirrhinum latifolium, A. majus, A. Barrelieri, A. ramosissimum, A. glutinosum, A. molle* and *A. Siculum*!

The phenomenon discovered by MENDEL that the hereditary units do not lose their individuality and being transmitted to a following generation are separated individually, could be used to a certain extent for phenotypic analysis (dominance and recessiveness). Mendelism got a powerfull support by MORGAN's interpretation of the mechanism of Mendelian heredity. The crossing-over theory created the possibility to study the localisation of genes in the chromosomes. Chromosome numbers and chromosome morphology induced taxonomy on a cytological basis.

Hundreds of evidences from experimental work showed that the species could not be regarded as a unit based on genetic identity. It is evident that the morphological species conception had to undergo the criticism from genetic standpoint. The attempts to change or to enlarge the species conception with genetic, cytological and ecological elements have lead to better insight in the problem and brought the investigators on different lines to a field of mutual research; yet the needs of phylogeny could not be satisfied by mendelian or cytological data.

The best starting point for comparison of taxonomic and genetic views is the Linnean species. Undoubtedly the delimitation of species and minor groups with the use of morphological criteria can be carried to extremes which easily leads to naming single specimens in the herbaria. Units minor to the species have little taxonomical value. There characters are more influenced by environment.

Varieties must possess hereditary characters which distinguish them from the species, modifications are due to environmental effects. Inspection alone is an untrustworthy criterion *).

As a rule, however, the herbarium specialist disposes of a few specimens only and the geneticist will object that in those cases only a few of the combinations of characters possible in the species are represented. Nevertheless it is quite possible that the species of the taxonomist coincide with the genetic delimitation, as all phenotypical manifestation is due to the genetic constitution. Genetical researches are useful to expose the artificial nature of morphological classification. It is perfectly true that modern taxonomists do not regard the Linnean species as a model of a taxonomic unit. Not all species described by LINNAEUS are moreover collective ones. The collective species of LINNAEUS

*) STIEFELHAGEN (1910) p. 468, calls *Scrophularia Neesii* WIRTG. a modification of *S. elata* GILIB. Experimental investigation has proved that *S. Neesii* is a distinct form which even can be regarded as a good species (GOETHART and GODDIJN, unpublished).

are taxonomically as well as genetically groups of polymorphic organisms. The taxonomist however clings to his species conception. All minor differences revealed by genotypic analysis do not disturb his own categories. To him species are representations of ideal forms to which he ascribes specific morphological characters of real value. Be it that his species are abstractions, he will always regard them as natural groups of organisms. The individuals of a distinct group may not be alike and differ in many characters, they are all the same different from individuals of other groups. The different groups are separated definitely by morphological, ecological or even physiological divergences and a relative constancy of each group for longer periods is the justification of delimitation in categoric units such as species are. Delimitation of species taxonomically occurs undependent of any theory of evolution!

The last consequence of Mendelian segregation with regard to the species conception was drawn by LOTSY (1916). To his opinion the smallest taxonomic unit which could bear the name of species should be a group of individuals of identical constitution unable to produce more than one kind of gametes, in other words: species should be pure homozygous constitutions. Though the idea is perfectly logical, changing the meaning of the taxonomical species violates the historical development of the conception too much and moreover does not fit practical purposes. Such groups of homozygous individuals may be apt for genetic research, in taxonomy they are rather useless. This fact has been often repeated, but LOTSY's idea must not be judged from this single suggestion. Later on he never again used the term species in this sense. Totally pure homozygous constitutions, if they occur in nature, can never be proved. Even in pure lines no certainty can be obtained that the constitution as a whole is homozygous. The intention of LOTSY's terminology was to replace the term species in taxonomical sense by another one expressing its evolutionary value. The Linnean species he named L i n n e o n, consisting of minor definite groups called J o r d a n o n s (microspecies) and th term "species" than should be preserved for the smallest genetically possible units. "Species" in the publications of LOTSY must be understood as Linneons.

For cases of intercrossing Linneons with fertile crossing products, mixing up with the parent Linneons to a bigger crossing association, he proposed the term s y n g a m e o n. The syngameon embraces those polymorphic groups in which two or more Linneons (species) intermingle and the Linneons are linked by transitional fertile hybrid individuals. This series of terms thus is testifying the evolutionary thought of the hybridization theory. They represent the species conception of a geneticist who spent a great deal of his life on taxonomy. LOTSY's terminology was meant to preserve systematic categories and in the possibilities of crossing he tried to find limits for natural groups. His definition of a "species" was rejected, but the terms Linneon and syngameon have found approbation of many authors. All the same the taxonomist will not be satisfied by this categories as the syngameons represent the most difficult things to deal with in taxonomy. When the taxonomist is able to find out the parent species in a hybrid population he does not care much for the syngameon. The greatest difficulties arise in highly polymorphic - groups designated as syngameons in which the constituent species, possibly more than two, can not be easily recognized. A great trouble are those in which the transitional forms could have risen from crossings other than the likely supposed parent species; the extreme diverse forms of a syngameon need not be identical with the species which caused the origin of the syngameon. It is quite possible that within a syngameon the parent species are lost and new species are not differentiated. Determining syngameons remains a trouble as they always will require experimental investigation to prove the hybrid constitution of the constituents. In complex syngameons the possibility that other than the supposed parents have shared in the constitution of certain forms, is not excluded. Such populations will be regarded as species, or divided into subspecies, or simply taken as hybrid populations. Several hybrid populations are described by LOTSY, COCKAYNE, ALLAN, HERIBERT NILSSON, and I have only to mention the syngameons *Nothofagus Cliffortioides* × *N. fusca* (LOTSY 1925) of the New Zealand forests and *Euphorbia Bothae* P. h. and *Euphorbia anticaffra* P. h. of the Fishriver district in the Cape Province of South Africa, to point out their significance for the vegetation and the rôle they play in evolution (LOTSY and GODDIJN 1928). The *Nothofagus* forests consist for a considerable part of hybrid populations. *Euphorbia Bothae* and *Euphorbia anticaffra* are covering vast regions of the Fishriver valley and in localities of square miles even dominate the aspect of the vegetation. These two hybrid populations are linked and though they are recognized as syngameons by inspection, nothing more can be said with certainty about their presumable origin than that *Euphorbia coerulescens, E. tetragona* and *E. triangularis* have something to do with them. Only experiment can bring further insight. The diversity in *Euphorbia Bothae* is so considerable that f.i. at Botha Hill no two specimens growing close together are alike. So it takes no wonder that several forms were described as distinct species (*E. Ledienii, E. Franckiana* a. o.); such species could be augmented ad libitum (LOTSY and GODDIJN 1928).

Less complicated syngameons as the hybrid populations of Cotyledon species are more or less localized, but nevertheless they are linked by the possession of a mutual parent. They also occur in the Eastern Cape Province e.g. *Cotyledon coruscans* × *C. teretifolia,* is connected on one side with *C. teretifolia* × *C. Beckeri* and *C. teretifolia* × *C. gracilis,* on the other side with *C. coruscans* × *C. Beckeri* and presumable also with *C. coruscans* × *C. gracilis.* Also other hybrids of Cotyledon were recognized such as *C. paniculata* × *C. Wallichii* which seemed to be an isolated hybrid population. The Cotyledons of South Africa thus demonstrate a genus in a period of evolution, separated syngameons being on their way of developing new species.

Another example of a remarkable hybrid population may be mentioned here. At Menaggio on the Lago di Como (and also at the Lago Maggiore) hybrid populations of *Primula acaulis* × *Primula officinalis* occur, showing a great diversity. Out of this population a new Linneon embracing a group of intermediate forms will probably develop. At the outskirts of the syngameon stand forms, nearing the parent species, which certainly will be regarded as members of the parent linneons. Among the segregation products occur a very few forms resembling closely *Primula elatior,* and it is not improbable that *P. elatior* should have risen from a crossing between *P. acaulis* × *P. officinalis.*

In different localities in Switzerland the hybrids *P. acaulis* × *P. elatior* (e. g. at Flüelen) and *P. elatior* × *P. officinalis* (on the Rigi at Felsenthor) occur. Where the possibility of crossing between the three species is present, a complex syngameon is formed. In such a syngameon the combination of parent characters in the separate individuals can hardly be recognized by inspection. Exploration may give the conviction that different Linneons are fused, but experimental statement must confirm the field work.

The only way to solve taxonomical difficulties with polymorphic species goes along lines of experimental research. *Scrophularia Neesii* WIRTG. and *S. Ehrharti* STEV. are closely allied species; they intercross and produce partly fertile, partly sterile progeniture. The products of crossing and backcrossing (only partly with one of the parents) are rather undistinguishable from the parent species. The parent species themselves in their extreme forms are distinct enough, but as no definite criterion separates them they have often been counfounded. All their characters being transgredient, it is plausible that some taxonomists took them together into one and the same Linneon (*S. aquatica* L.; *S. alata* GILIB., in which probably even more species are included). Nevertheless *S. Ehrharti* and *S. Neesii* are separated physiologically, behaving themselves as distinct species when crossed with each other and their reaction being different when hybridized with a same foreign species *).

DANSER (1929) also tried to delimit the possibilities of crossing but he did not touch the taxonomic categories, intending to point out the phylogenetic origin of populations (convivium). All individuals which are hold together by possibilities of hybridization are called a comparium; it does not matter whether the products of hybridization are fertile or sterile. Such groups will as a rule not coincide with taxonomic categories, but are certainly of value for phylogenetic purposes. An association of individuals which are connected by possibilities of exchanging genes, i.e. which can be intermingled, DANSER calls commiscuum (Vermischungsgenossenschaft). These groups may coincide with, or approach to a species; they are polymorphous like Linneons. Convivia, however, are populations, groups of individuals, differentiated within the commiscuum, isolated by geographical influences. Here also isolation is introduced to explain the cause of differentiation. The concepts of LOTSY and DANSER are biological ones. DANSER attempts to find delimitations in connection to plantgeography.

A convivium must be more or less distinguished from the other parts of the convivium, forming a group of close resemblance and hold together by circumstances limiting their intercrossing. A convivium may coincide with a species in certain cases, but not necessarily does. Under particular conditions subspecies or varieties may form convivia. At the best a convivium can be compared with HAAGEDOORN's species as DANSER himself discusses, a population tending to reduce its potential polymorphy. (The potential variability is given by the number of genes in respect to which a group of individuals is not homogenous. The qualitative stability of genes accepted, the potential variability in a population of limited crossing possibilities reduces automatically, according

*) An account on experimental work with Scrophularia by Dr GOETHART and the present writer is in preparation.

to HAAGEDOORN (1921). DU RIETZ (1930) accepts this even as a law). DANSER (1929) has given different examples to illustrate his concept.

It is evident that DANSER, in more refined a way than LOTSY, thought in the same line, trying to delimit groups the origin of which could be understood. The mere morphological species conception is insufficient for such a purpose. No grouping on particular line will clear up phylogenetic relationship between species and certainly not a morphological system of classification.

We have no certainty that the natural taxonomical groups are branches of a natural system. They are groups based on external resemblances, separated by gaps of discontinuity. To ascribe resemblance between groups to phylogenetic origin is a mere hypothesis (conf. LOTSY 1925).

Another refined attempt of grouping individuals was made by TURESSON (1929). He saw the species delimitation as an ecological problem. Ecological experiments have shown that species of a wide distribution, being divided over different localities (habitats) split off races of different hereditary characters. When different ecological types, definite morphologically distinguished races, are tied to different habitats by edaphic factors, belong to the same Linneon, those races are not to be regarded as a kind of species. In essential points there is no fundamental difference between TURESSON's and DANSER's concept, although the term convivium has a wider sense than ecotype. One could say the ecotype is a convivium caused by ecological influence. Four groups are discerned by TURESSON: c o e n o s p e c i e s, a population in which species group themselves on account of vitality and sterility limits, but all of common origin so far as indicated by morphological, cytological or experimental facts; a g a m o s p e c i e s, apomictic population under the same conditions; e c o s p e c i e s, an amphimict population with vital and fertile descendants but more or less sterile when crossed with constituents of any other population; the e c o t y p e is the response (genotypical) of an ecospecies to a certain habitat. GREGOR (1931) e.g. in his study on experimental delimitation of species gives a case in which the system of TURESSON has been applied. *Phleum pratense-alpinum* form a coenospecies. In *P. pratense* two groups are cytologically discerned which do not intercross, a hexaploid and a diploid one. In *P. alpinum* likewise two groups exist, a tetraploid and a diploid. These groups are considered as ecospecies. Some ecotypes of *P. pratense* could be distinguished, four of diploid and three of hexaploid constitution.

In an extensive study on the fundamental units of biological taxonomy DU RIETZ (1930) redefined the terms form, species, subspecies and variety, grouping them morphologically, laying much stress on the effect of geographic isolation and the automatic reduction of polymorphy. DU RIETZ accepts JOHANNSEN's definition of a biotype, a population consisting of individuals with identical constitution (Elemente der exakten Erblichkeitslehre). The variety is a population consisting of individuals of one or more biotypes forming a more or less distinct local facies of a species. The subspecies is a population of several biotypes forming a more or less distinct regional facies of a species, and the species he calls the smallest possible natural populations permanently separated from each other by distinct discontinuity in a series of biotypes. The importance of hybridization and isolation comes to light when he defines the species as: a population consisting either of one strictly asexual and vital biotype, or of a group of practically undistinguishable, strictly asexual and vital biotypes, or of many sexually propagating biotypes forming a syngameon separated from all others by more or less complete sexual isolation or by comparatively small transitional populations.

This concept agrees with the views of LOTSY, DANSER, HAGEDOORN, HERIBERT NILSSON and many others. Criticism of DU RIETZ's views says that the apparently disregards the selective effect of environmental conditions on a genotype complex (conf. GREGOR a. o.) Anyhow DU RIETZ does not deny the possibility that mutation may play a rôle in the process, but he is convinced that the role of isolation in nature is sufficient to explain the process of differentiation. Differentiation by means of automatic reduction of polymorphy is enough to explain what we see in nature. The rôle of selection seems to him overestimated and herewith many other authors agree (conf. DU RIETZ 1930, p. 399).

One other species concept may be mentioned here, more particularly in connection with cytogenetics.

Some points may be stated as evidence from cytological work firstly concerning the basis of heredity. From cytogenetic standpoint it is not a mere assumption that the species have practically a constant number of chromosomes (genoom). A second fact of importance is the probably linear arrangement of the genes, the primary hereditary units, in the chromosomes. The chromosomes occur in pairs, in which the genes occupy identical loci. The chromosome sets consisting of n pairs, the hapliod generations have n chromosomes, and this number, though not always, being constant, proved to be in many cases a characteristic feature of related species.

Multiple series of chromosome numbers with a basic number apparently have taxonomic value. The work of CLAUSEN on Viola and of HURST, TACKHOLM, BLACKBURN and HARRISON on Rosa have become classic in this respect. CLAUSEN found that a definite series of chromosome numbers occurring in Viola species coincides with taxonomical groups; he divided the section Melanium in subgroups based on the numbers of chromosomes. Not always it will be possible to classify a genus according to this principle. Such divisions can be made when a basic number in the somatic cells is stated and degrees of polyploidy may be used to separate the different sections. The study of chromosome morphology, however, widened the prospects for systematic applications and even proved to be of more value than the numbers solely. This has been the merit of NAVASHIN and his school (LEWITSKY, DELAUNAY, TAYLOR, HOLLINGHEAD, MANN, LESLY, AVERY, ROSENBERG etc.). According to these investigators the chromosomes could be used not only for classification, but also for revealing phylogeny of species, by studying chromosome morphology (size, form, satellites, constrictions a.s.o.). This kind of study combined with phenotypical appearance of the species considerably raised the value of cytological investigation and has produced even systems of classification. A single case may be mentioned to illustrate the bearing (stretching) of such investigations. HOLLINGHEAD and BABCOCK (1930) published an interesting study on chromosomes and phylogeny in the genus Crepis. No less than 70 species were cytologically studied by different investigators. Now in Crepis apparently the similarity of chromosomes points to a common origin. The American species can be arranged in a polyploid series, some of the European species too, the others probably arose from interspecific hybridization. These species were divided into sections, one of which, Paleya with the basic number 10, could be considered as the most primitive subgenus from which the other subgenera could be derived: Barkhausia with the numbers 8, 10 and 16, Catonia with 12, and the heterogeneous Eucrepis. Paleya could be supposed to contain or to have contained the progenitors of all other subgenera. In Eucrepis the connecting forms have disappeared with Paleya. Now it is evident from the discussion that different assumptions as to the chromosome changes are made, but there remains the fact that morphological similar species have similar chromosomes and that the phylogenetic system projected by the authors for the genus Crepis undoubtedly proves the great value of this studies for taxonomy, showing the connection between chromosome number and chromosome morphology on one side with phylogenetic relationship on the other.

Acknowledging that Mendelian heredity, except in the cases of cytoplasmatic inheritance, during more than 30 years established by numerous experiments as the basis of all heredity, consolidated by MORGAN's school with cytological data, it is plausible that cytology must be drawn into consideration at the delimitation of species. A step to a cytogenetic species definition was made by BABCOCK (1930).

BABCOCK states from existing evidence that the hypothesis of DARWIN and LOTSY are at least in part correct and that the present species must have risen through differentiation aided by isolation within preexisting species. Interspecific hybridization is rather common in various genera (Hieracium, Rosa) and numerous hybrids exist in nature by apomixis.

Tetraploïds are often self-fertile, triploids have low fertility but, as NAVASHIN proved for *Crepis capillaris,* may sometimes serve as starting points for a series of polyploids. From experimental interspecific crossing it is known that all degrees from fertile to sterile can occur, but that there is a general tendency towards sterility.

BABCOCK accepts hybridization as a modus of origin of species in nature; however, the primary processes in species origin are to him the gene mutations and the chromosome transformations. According to MORGAN and MULLER gene mutations can be experimentally obtained and can arise de novo.

BABCOCK thinks that gene mutation creates the possibility of interspecific differentiation (polymorphic species), but he admits that chemical changes of genes, loss or addition is insufficient to account for differences in chromosome morphology and chromosome number among species of many genera.

Three modes of cytogenetic variation are important: 1. gene variations (mutations), 2. chromosomal variation (addition, rearrangement, translocation, transformation, delation), 3. polyploidy.

As to the species conception BABCOCK constructs seven basic ideas, as follows:

1. Common structural characteristics which unite certain individual organisms into one group, the species. Cytogenetically: the common genetic basis is represented by a specific chromosome complex.

2. Certain characteristic features which distinguish such groups

from each another. Cytogenetically: mostly represented by the chromosome garniture (genom).

3. Relative stability combined with more or less variability. Cytogenetically: made possible by chromosome distribution from cell to cell, inherited variations arising from occasional changes in genes and chromosomes.

4. Common descent of all individuals of the group from one or more preexisting species. Cytogenetically: explained by the mechanism of heredity and genetic variation.

5. Free intercrossing and high (but not necessarily complete) interfertility among the individuals of the group. Cytogenetically: in accordance with the homology of genes in the chromosomes of the individuals.

6. Absence of free intercrossing and usually low fertility if not complete sterility in hybrids between different species (although highly fertile and constant new forms may sometimes arise in this way). Cytogenetically (with a few exceptions): the logical result of accumulation of genic and chromosomal differences between diverging groups of individuals within the species.

7. The facial occurrence of subspecific groups, often occupying different geographic areas which differ more from one another in structure or interfertility or both than do the individuals composing each subgroup. Cytogenetically: this must be regarded as the result of genetic variability within the species, the influence of changes in the environment isolation and of natural selection.

I have quoted BABCOCK (though other geneticists contributed to species conception, such as HALDANE, FISHER a.o.) because in his concept can be seen a trial to conciliate the taxonomic and the genetic ideas, and the points resumed may lead to a cytogenic species definition. Certainly both lines must be followed; morphological and genetic taxonomy are meeting, the mutual interest ends in phylogeny.

The solution of evolution problems is always sought in the origin of species. Suppose we know the origin of species: let it be hybridization or mutation, or both. From species arise again species; neither mutation nor hybridization has shown something else. Hybridization goes the farthest in experimental evolution by bi- and trigeneric hybrids, from which possibly a new genus might arise. Nothing of higher taxonomic rank is formed. It is evident that only a part of a given genetic constitution can be analised. Evolution is not solved with the origin of species; the evolution problem is a phylogenetic one.

As to the bearing of the species conception to phylogeny nothing can be said that is not hypothetic. PLATE (1932) is right when he says that genetics is unable to all apply for phylogenetic needs. The geneticist is interested in the study of the present world, he is working with species of the present time, the last result of evolution; the phylogenist goes back to endless times embracing wider groups than species are. Phylogeny becomes impossible without accepting Lamarckian views of adaptation to some extent. Genes are not characters and only possible in a gene complex interacting in a genetic constitution. Characters and organs cannot be handled a like genes. How should we know that genes manifesting characters, organs, individuals were in constant static condition for geological periods? Perhaps time will come that we are able to understand better static and dynamic processes in evolution and may look upon the problem of acquired characters as a genetically plausible phenomenon.

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