CONSIDERATIONS ON THE EVOLUTIONARY THEORY

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It is not my intention to present a detailed
history of the development of the theory of Evolution as
generally formulated today, but only to present some
steps which have had special importance in the past or
which require now reconsideration. Some points of view
accepted as essential in the past, owing to facts then
considered evident, were not rarely included in the
general theory though sometimes having resulted from
bias, exagerating their importance while having actually
become without much importance, thus representing a
necessary ballast or dead weight.

Some mythological concepts about origin of life
or more specifically of mankind, existed probably in many
parts of the world and have been accepted by still
rather primitive indigenous people. During the middle
ages, ideas of Aristotelean philosophy or the biblical
concepts were accepted as representing the "truth". In ̦
the middle of the 18th century modern or scientific
reasoning started in the biological sciences, and one
can, from then on, distinguish two phases of development
of scientific study of evolution. In the first phase
the questions of evolution as a process predominated,
while after it's acceptance had spread more or less
widely, the discussion passed to a second phase, the
study of the evolutionary mechanism. Not infrequently
both phases are called summarily "darwinism". Actually,

when DARWIN started his work and published the results,
the hypothesis of evolution as a process had been
accepted by many scientists, and DARWIN's contribution
dealt mainly with the evolutionary mechanism, though
greatly contributing to strengthen support of the
evolutionary hypothesis in general.

In preparing the present paper the question arose
which bibliographical references should be given out of
the vast number of publications existing today treating
evolution and connected subjects. Such citations might
help the reader, but would undoubtedly increase the size
of this paper to such an extent that the treatment of
some especially important points might become obscured.
Thus by only mentioning the names of the more important
authors in the field, an experienced geneticist could
easily find the necessary bibliographical reference,
while for others the reading of specialized papers might
be rather confusing.

The question of explaining the diversity of
living organisms became important around the middle of
the 18th century, in view of the rather great number of
taxa which had been accumulated in consequence of the
expansion of world shipping and the exploration of
colonial areas by the great European powers. Special
methods of an archivistic nature were required to bring
order into it and this was the great merit of LINNAEUS
to have provided an orderly arrangement, establishing
both firm concepts such as that of the species and basic
nomenclatural rules for clear identification. Both, the
species concept and the basic nomenclatural rules then
established, are valid still today. LINNAEUS recognized
that his system was an artificial one, but he soon came
to the conclusion that there exists an underlying
Natural System. The origin of the existing diversity
did not represent any difficulty to him, since he
accepted firmly the biblical interpretation and simply
concluded: there are many species as had been
determined by the Creator. The paleontologist CUVIER

was especially interested in the observations that fossil
species were always found only in a restricted series
of geological strata and then substituted frequently by
rather similar species. This extinction and the
following new formation of species he also interpreted
as acts of the Creator, but he added some new elements.
He formulated the hypothesis that at different geological
period the Creator became dissatisfied with his creations,
provided a diluvium destroying all elements of flora and
fauna, resuming then, the process of creation.
Geologists did not find proof that such general
catastrophic changes had ever occured. On the contrary,
they found that some fossil taxa, specially species,
disapeared, i.e., became extinct and then were
substituted by new ones at some specific geological
period, while in others such events had taken place at
quite different geological moments. Furthermore,
scientists may have come to reject CUVIER's hypothesis
as fundamentally illogical and thus as inacceptable: it
attributes to the Supreme Creator the human quality of
using the simple method of "trial and error" repeatedly.
In fact CUVIER's hypothesis was not accepted as a valid
explanation, not only because not having been proved,
but by leading to too many contradictions. Thus an
alternative explanation was required.

        The general situation in the second half of the
18th and the first of the 19th centuries may easily
have caused discussions of another nature. It was
evident that the political system of the absolute
monarchy had resulted in social misery and economic
bankruptcy, requiring drastic changes. Some thought
the only solution to consist in one, very drastic and
revolutionary change, while others preferred a sequence
of smaller changes without a general upheaval, i.e. the
process of slow, but continuous evolution towards a
preestablished end. The French followed the first
mentioned way, that of the great French revolution,
while in England and elsewhere the process of slow and
gradual change was adopted. Similar thoughts may have

played considerable role in scientific thinking.
CUVIER's hypothesis of repeated diluvia correspondend
rather to that of revolutionary change, while Auguste
de St. Hilaire, LAMARCK and others accepted an
evolutionary hypothesis. They postulated that taxa
(species) were step by step transformed into new ones.
No proof of this is available owing to the fact that
such evolutionary transformations require evidently
geological periods, much longer than life of one
scientist or even that of several successive generations
of scientists. But several series of fossils have been
found in subsequent geological periods, showing stages
in the development of some characters which can be
interpreted as successive phases in an evolutionary
chain, for instance the formation of the foot of horses,
the trunk in elephants or others linking apes to *Homo
sapiens*. In such series of fossils the species following
each other are most probably not directly connected, but
represent only steps in a general phylogenetic sequence.
The species at one level need not to have been the
actual ancestor of the fossil species of the following
level. Studies of comparative morphology have also
resulted in establishing what might be considered as
successive steps in an evolutionary sequence even with
regards to characters of higher taxa. The sequence
from Pteridophytes over Pteridosperms and Gymnosperms
to Angiosperms shows an almost continuous change. From
the still autotrophic haploid prothallum of ferns it
passes to the female gametophyte formed within the
female tissue of the diploid sporophyte. The male
gametophyte may form in the Gingkoales a tube or
haustorium producing still two mobile spermatocytes,
until finally in the Gymnosperms such haustorium, now
called pollen tube, transports the two male nuclei to
complete fertilization of the oosphere. The decisive
element allowing the evolutionary concept to be
generally accepted consists in the complete absence of
any evidence to the contrary. The evolutionary concept
was consequently slowly accepted, mainly during the
first half of last century and is still considered
valid up today.

As long as the biblical concept had been
acceptable, there was no problem regarding the origin of
life and the dogmatic statement: "omne vivum ex vivo"
remained valid. Some concepts regarding the origin of
living organisms had been formulated and formed part of
aristotelean philosophy, regarded as fundamental since
the Greek classics. During the last century, PASTEUR
and others accumulated experimental evidence disproving
the speculation that life could have originated by some
combination of dead matter, thus at least apparently
disproving the occurrence of "spontaneous generation".
Such experiments, though without positive results, did
not prove anything, since it was not shown that the
experimental conditions had been the right ones. On the
other hand, if the evolutionary hypothesis is a correct
one, life must have started somehow, somewhere and at
some time. Rather recently scientific results have
indicated how and when life could have had its origin.
Geological and petrographic evidence indicate that in
the course of some 5 billion years the earth suffered
important changes. In about the first half of this long
period, free oxygen was either absent or extremely rare
in the atmosphere which contained larger amounts of
nitrogen, carbon dioxide and ammonia vapor, which form
under some conditions, water soluble complex molecules
dissolved in ocean waters or rivers. Such chemical
combinations similar to proteins have in fact been
produced experimentally. As soon however, as some
photochemically active combinations similar or identical
to chlorophyll were formed, the situation changed and
free oxygen appeared in the athmosphere. This in turn
would have caused the immediate destruction through
oxidation of many or most "organic compounds" - so
called by tradition, though not of organic origin -
unless they were already protected by special membranes.
This might be considered as a possible origin of living
organisms. Again it must be stated that there is no
proof of this assumption, but it shows that such an
origin might have taken place.

So far I have tried to summarize why the
continuous process of evolution has become generally
acceptable. It was never proved, but there are no
contradicting facts known and it is not only logical,
but also in agreement with all facts known, in nature or
from experiments.

Now I will pass to discuss the probable nature of
the evolutionary mechanism which allows the permanent
continuation of the evolutionary process. The concept
of its nature which up to now widely was accepted,
could be rightly called "darwinism" and shall be
submitted to a critical analysis. However, before
entering into this subject, LAMARCK's ideas about the
role of the supposed inheritance of acquired characters
must be mentioned rapidly, mainly for historical reasons.

LAMARCK, a French biologist with a vast taxonomic
knowledge believed that some characteristics in animals,
including man, could not only be improved by intense
training, but also become to be heritable in this
improved form. However, even in human society there are
probably more cases showing the contrary, i.e., of
inefficient children of outstanding people not only in
some special fields but also after special training.
Experiments were carried out by several biologists, and
they did not show any signs of an existence of
heritability of acquired characters. Thus LAMARCK's
hypothesis was rejected and finally abandoned.

DARWIN's way of thinking was probably predisposed
by the general spirit in England at the time of the
beginning of intense industrialization where both
extreme competition between new industrialists and at
the same time very strong exploitation of the labour
forces were considered essential for progress.
Furthermore, he knew that in animal breeding, sudden
heritable changes of characteristics did occur, at that
time called "sports", now mutations. Finally,
travelling as medical officer over the Atlantic Ocean

and landing on the shore of Rio de Janeiro, he became
aware of the richness of both flora and fauna of the
tropics, so much contrasting with the comparably rather
poor vegetation in England. Combining all these
thoughts and observations, he concluded that also in
nature there must exist an extreme struggle for survival
and that any hereditary changes which increase the
chances for survival would be selected rapidly through
competition with their own predecessors. Thus the
hypothesis of darwinistic "Natural Selection" was
established. DARWIN delayed publication of his
hypothesis until much later after having accumulated
a great number of observations which he could interpret
with his hypothesis. This was rather rapidly accepted
by an increasing number of scientists and gained general
acceptance as a satisfactory explanation as a mechanism of
the evolutionary process, based exclusively on mechanical
elements and free of any vitalistic or biblical concepts.
Evidently, as was the case regarding the hypothesis of
evolution as a process, no experimental proof could be
furnished: evolutionary progress is so slow, requiring
geological time scales,but not some based on the duration
of human life. With the intention, however, to provide
"proof" for the darwinian hypothesis, his followers
accumulated a very great volume of observations which
were interpreted according to the requirements of the
darwinian theory. In fact, the latter became a dogma:
either observed structures or functions could be
interpreted according to darwinism or they were simply
disregarded. With this form of procedure a tremendous
bias was introduced. Such bias has led to quite
erroneous interpretations of characters to be of value for
increasing survival capacity. An almost dogmatic
belief in the ever presence of darwinian natural
selection has caused even a teleological form of
thinking. Every character of an organism has to serve
somehow for one purpose specially: to improve survival
capacity. Unfortunately this principle was not rarely
used, interpreting only the functions of characters of
living organism without proving that such an increase
in survival capacity had been achieved. One of many

examples only shall be mentioned in some detail. In the
higher plants (Angiosperms) the function of the floral
organs consists in allowing pollination. The structure
of the floral organs has however also been used largely
to establish order in a "Natural System", i.e., one which
corresponds to the changes which had occurred during
evolution. Consequently, in applying the darwinian
dogma, each special feature of floral organs must
represent some improvement in the pollinating process
and through it to an increase in survival capacity.
Among the families of Angiosperms, the orchid family is
often cited as showing the most astonishing "adaptations"
between flower and pollinator, sometimes to a group of
insects, sometimes to one specie or even to one sex of a
specie only. The often complicated floral structures in
orchids became a very special and often cited object of
darwinistic interpretation. For instance it was
observed that they attracted by their scent, coloration
and form the males of wild bees which, confusing the
flower with a female of their species, tried to copulate
with the flower and during these movements transferred
the pollinia from the anther to the receptive stigma of
the same flower, a procedure called "pseudocopula". The
behaviour of the insect males was clearly observed, but
not that fruit setting followed always after pseudocopula.
Other interpretations in flower ecology were rather
anthropomorphic. For instance longitudinal dark stripes
on floral leaf organs pointing somewhere into the flower
were interpreted as serving as indicating direction as
if the pollinator had learned some traffic signs, while
ignoring transverse stripes indicating to human
impediments of movement. It seems very doubtful that
bumblebees will settle one on top of the other, but a
design on an Ophrys-flower, appearing to the human eye
as an image of such an insect, has been interpreted as
informing the insect that some special food or drink
might be obtained, an impression analogous to that of a
human person on the outside board of a bar or
restaurant. However most important is the fact that
generally no test is made to prove the efficiency of the

adaptative relations between flower and pollinator.
Having studied tropical orchids for several decades, I
undertook numerous collecting trips in many parts of the
American tropics. Since orchids are long lived
perennials, they can be easily found and collected. Most,
if not all, species of this family flower each at a
specific time of the year. They were often found
without flowers, but when cultivated under near natural
conditions they showed that they produce flowers normally
each year. Since the ovary is generally in a bud stage
at flowering time, developing only further after a
successfull pollination, reaching maturity after
considerable time, i.e. after several weeks or mostly
many months, one might thus expect that fruits in some
stage of development should be found on the mother plant
on its natural habitat at any time of the year and in
abundance. On the other hand fruits were only very
rarely found on the natural habitat. This then indicates
very clearly that the adaptations between orchid flower
and pollinator are very inefficient under natural
conditions and thus do not in any way increase survival
capacity. Only ocasionally a few species were found
with all their flowers developed into fruits. A detailed
study of such species under cultivation and constant
observation showed that they had special, often minor
structural changes responsible for automatic
selfpollination besides all characters typical for each
species or genus. Leaving aside the few exceptional
cases of automatic selfing, the majority of orchids show
the relative inefficiency of the adaptations between
orchid flower and pollinator which rather represent a
disadvantage and not an increase of survival capacity.
Since however orchids have evolved and still exist,there
must be some characters reestablishing efficient survival
capacity and thus acting as what may be called a
"compensating adaptation". Orchid fruits when mature
can contain, varying from taxon to taxon, from several
tens of thousands to some hundreds of thousand seeds.
Since it would be sufficient for the preservation of a
specie that one seed per parent plant would give a

mature specimen, the rate of survival under natural
conditions must be an exceedingly low one and a large
number of potential descendants are eliminated by some
form of random elimination, independent from its
individual survival capacity. This should not b·
interpreted as indicating that there is no natural
selection of the darwinian sort, but that this is only
part of the whole selective process and probably a rather
minor part of it when compared with random selection.
This conclusion will lead forcibly to reconsider many
interpretations given to the evolution of characters
which formerly have been considered as the result of
darwinian selection. It will in no way affect the
general acceptance of the evolutionary process as such,
but only concepts regarding the evolutionary mechanism.

The science of genetics which has rapidly
developed during this century, had furnished many
contributions to the understanding of the evolutionary
process and its mechanism, and thus, after changing the
exclusive if not dogmatic basic concepts of darwinism it
becomes necessary to enter into a discussion of pertinent
cytogenetic questions.

When MENDEL discovered the rules of transmission
and persistence of hereditary basic units, this was at
first rejected, but CORRENS and others reestablished the
mendelian rules. DE VRIES formulated his mutation
hypothesis by erroneously interpreting results, but the
existence of hereditary changes was later generally
accepted. JOHANNSEN established a clear distinction
between the carrier of the genetic information, carried
out from mother to daughter cells and from generation to
generation which he called the "gen" and on the other side the
actual manifestation of this information the "phenotype".
The fact that spermatocytes or pollen tubes transfer
only and with regularity nuclei into the oocyte rarely
also some small part of cytoplasm, made it evident that
only nuclei are responsable for genetic transmission.
Cytologists proved then that mainly the chromosomes of

the various components of the nucleus were responsible
for that transmission through the complex mechanical
basis of mendelian segregations. Also the chromosomes
have a complex structure. Sometimes living cells show
that chromosomes contain a spirally enrolled thread or
threads, the chromosomes, though it was long under
discussion whether these remained intact during the
resting stages of the nucleus. After treatment of
living chromosomes (i.e. without fixation of any sort),
these spirals could become visible without staining, but
considering their thickness they leave a central space
which must be filled by some material, the "matrix"
forming the main body of each chromosome. In treating
living cells during nuclear division by various
chemicals (ammonia, nitrates of Ca, Na, Li, etc) I
observed (BRIEGER unpublished) that the chromosomes start
to swell, chromonemas becoming visible within a colorless
and uniform matrix which thus differs clearly from the
surrounding granular cytoplasm. The swelling-matrices
touch and fuse finally while the chromonema unroll almost
completely. Thus the final aspect differs only in so
far from a nucleus in the resting stage that it has no
outer membrane nor a definite form. The functions of the
matrix are unknown, but it certainly has no part in
preserving a genetic information.

        The chromonemas are rather long and thin threads,
though of varying thickness nodules of varying sizes
being connected by a thin thread. The pecularity   of
both mitosis and meiosis also indicate clearly that
there exist some orderly sequence in the arrangement of
gen loci, linked with each other fixed rates when
located in the same chromonema. Attempts were made to
determine the size of a gen locus. The total length of
the chromonema can be measured with some accuracy. The
total number of gen loci has been estimated for some
species to be of the order of several thousands.

A chromonema is composed of two chemically
different parts: the DNA-chain, formed by longitudinal
unions of only four kind of nucleotides and its protein
cover. Since the DNA chain was discovered it was
postulated that its parts should contain permanently the
genetic messages in some coded form which then will be
activated and transmitted somehow and somewhere to be
executed and transformed into phenotypic manifestations.
It was assumed further that the four nitrogen basis in
various numbers of all combinations and permutations
would furnish the basis for the transmission of genetic
informations of the numerous gen loci and the allels of
a specie. However since the same nucleotides are
present in all species, it seems to me very doubtful
if they would be sufficient to build up so many
different types of genetic messages. Before the
discovery of the DNA-chain, it was accepted that
proteins were the basis of what was first called a
"mendelian factor" and later a gen. It would seem to me
more logical to return to such an interpretation,
considering that proteins would be a more adequate choice
in view of their chemical complexity and diversity as
the material basis of a gen locus and its various allels.
The DNA-chain would then have the function to be the
first step in a sequence transferring the information
to other parts of the cell or other cells able to produce
phenotypic manifestations. It really does not matter
very much whether we accept the former assumption of the
DNA-chain or that of its protein cooper as representing
the conserver of the coded genetic information.

The conservation of the whole genotypic information
in some parts of the chromonemas seems to be the last
conclusion which can be applied in a general way to all
organisms with normal mitosis and meiosis. Regarding if
connection with the final phenotypic manifestations,
there are some indications refering to special cases,
but which represent and at present merely some form of
pieces of a preliminary working hypothesis to be further
tested. It must be remembered that the genotype as a

whole represents a kind of master-plan which not only
determines the different phases of ontogenetic
development, but also the functioning in the adult.
Though in some cases phenotypic manifestations seem to
be controled by one or few gens only, in fact a number
of modifier gens are also involved and in addition the
manifestation depends on from influences from the outside.
We do not yet know anything about when and how the
genetic message is modified under the influence of
external factors. It is equally unknown how certain
parts of the genetic message are activated in some cells
in time and space within an organism, though the whole
message is present in chromosomes of all cells of that
organism. Some authors have attributed activation to
protein molecules of the DNA-cover, while others admit
that hormones might be responsible. But what might
activate these "activaters" at the right time and place
only in some but not all cells? It is assumed that
messenger-RNA, diffusing from the chromonema into the
cell transports somehow the message to other parts of
the cell such as the ribosome which then are able to
decode the message and provides its phenotypic
realization. But when no such special inclusions are
known to exist, what part does the decoding? How can
an individual cell   "know" what size, shape or
function it has to assume? SPERMANN, HARRISON and their
groups have established that a certain group of
undifferentiated cells in embryos of Amphibians induce
cells more on the outside to form normal eyes or legs
anywhere in the embryo, in places to which they have
been transplanted experimentally. How they activate in
the induced cells the respective gen loci is completely
unknown. In the process of regeneration of lost parts
or the recovery of cuttings in plants, somehow the course
of ontogenetic development or only parts of it are
reinitiated in accordance to the genotypic master-plan.
Again it is not known what determines which part is
reinitiated. Evidently biologists have not yet
discovered efficient methods of experimentation and
observation to enable them to uncover the pathway or

possibly the pathways linking the conserved genotypic
master-plan with the phenotypic manifestations. It seems
now indicated to review some other items which have been
incorporated into the current evolutionary theory.

        Up to the point of considering the chron some as
the carrier of genetic messages, science is still on a
firm basis, but linking the genetic message to its
phenotypic manifestation we have at present only
incomplete and rather vague working hypothesis.

        When T.H. MORGAN started his work in genetics he
used as laboratory animal the fruit fly, previously
found by CASTLE to be easily cultivated and studied in
milk bottles. BRIDGES, then only a laboratory servant,
though later a very distinguished geneticist, was told
to collect fruit flies in orchards, fruit shops and
elsewhere, to cultivate them and see what variations he
could find differing from the common or "wild type".
He carried this work very efficiently out, isolating
individuals with aberrant characters, tested them in
crosses and kept only those which gave clear mendelian
or near-mendelian segregations in such test crosses.
All other aberrant forms were simply discarded. This
method seemed to allow to reach general conclusions
regarding the nature of heredity, but involved a very
strong bias regarding the general nature of mutations:
namely that they were generally recessive and reduced
survival capacity. Genetic research with mice, rats,
rabbits, poultry or with maize, peas, sweet peas,
snap-dragon, etc., confirmed that initially mutant genes
were generally recessive, but not always subviable.

        NILSON-EHLE and CAST, studying the heredity of
quantitative differences, concluded on the other hand
that such differences were controlled by rather numerous
gen loci the allels of each locus when heterozygous
being intermediate between the homozygotes in their
phenotypic manifestation and had no effect on survival
quality in any combination. The analysis of the mendelian

proportions of monofactorial segregations of this nature
is mostly difficult since the distinction of the three
monofactorial genotypes are not very sharp owing to
graduations of their phenotypic expression.

        Other   cases must be considered where the
phenotypic manifestation of the heterozygotes go beyond
the range of variation of either homozygote, the original
one and the mutated one. A heterotic monofactorial
effect has been studied in two cases, the colour of the
feathers in poultry and the flower colour in some
cultivated form of *Silene armeria*. These cases have
been interpreted attributing the phenotypic effect to
two extremely closely linked gens in the mutant, one a
recessive inhibitor and the other a dominant intensifier
which however is hypostatic with regards to the inhibitor
when homozygous. The recessive inhibitor being inactive
in heterozygotes, the dominant intensifier manifests its
phenotypic effect plainly. In the case of *Silene* which
I used during many years in classwork for students, many
thousands of F2 plants were analyzed without any case of
a linkage break which might have proven the basic
hypothesis. In maize or man as in several other species
heterosis is caused through heterotic interaction of many
gen loci. The use of hybrids with a maximum of strongly
heterotic pairs of allels, resulted with inbreeding
through autogamy or brother-sister matings in a desastrous
loss of vigour caused by the appearance of homozygosity
even only in some gen loci.

        As last case the possibility shall be mentionned
of a mutant having a dominant effect. It seems that in
such cases the mutant reduces survival capacity
considerably. Such cases have been rarely observed and
studied, for instance by R.A. FISHER in poultry.
Apparently the extremely strong phenotypical effect of a
dominant mutant causes a generalized unbalance in the
genotype.

From the foregoing considerations one must conclude that there exists no general rule for mutant allels to be always for even mostly recessive and to reduce generally survival capacity. It should also be remembered that no gen locus will cause specific phenotypic manifestations which are not affected by the presence of allels of other gen loci. These may act simply as modifier only, i.e. their phenotypic manifestation consists exclusively in modifying the effects of allels of other gen loci and which can easily reestablish lost survival capacity. Geneticists and breeders have used these possibilities by changing the modifier effects by carefully planning artificial selection.

The term "wild type allel" was used to identify allels present with very high frequency in natural population or sometimes in old and well established races of cultivated plants. It must however be remembered that the "wild type allels" must in turn have appeared at some time during evolution or breeding and represented once mutations. Thus the question arises how a mutation with its initial extremely low frequency might become as frequent as present day "wild type gens".

In the preceding paragraphs criticisms are brought forward again a certain monopoly attributed to the purely darwinian concept as explaining the evolutionary process. Thus it becomes necessary to reconsider in some detail the factors which are involved during this process. Its basis consists evidently in the substitution of the allels present at a given gen locus by a new mutant allel which causes a new way of phenotypic manifestation. This manifestation is always quite unpredictable and one can expect that initially it causes in the genotype as a whole a certain unbalance. Its frequency is necessarely at first extremely low, but then increases successively during many generations, substituting the original allel and possibly eliminating it completely. When analysing this change in organisms living today, it has become the custom to designate the allels present as "wild type

allels" in contrast to the infrequent new mutant allels.
The present "wild type allel" on its part must have
originated at some previous time as a mutant and has
substituted the preceding original allel. This part of
the evolutionary process, the substitution of preceding
allels is however so slow that it cannot be observed
by direct observation. But such changes have been caused
through artificial selection carried out by geneticists
and breeders. An analogous result was evidently reached
by natural selection which consisted both in random and
in darwinian selection as explained already above, the
first a consequence of the highly excessive offspring of
each plant, well beyond the minimum necessary to
substitute its progenitors and the second a result of
differences in survival capacity. In order to visualize
how the evolutionary process might have progressed, it
became necessary to work out mathematical models of
population genetics, establishing certain indispensable
premisses which shall now briefly be discussed.

Mutation Pressure

        The mutation rate is generally very low and if we
accept a value of u = 0,001 as an upper maximum this
would evidently represent an unusually high one. It
would appear at first in the heterozygous state and the
formation of a homozygote might depend on the occurence
of a second mutant allel for which the expected frequency
would be still smaller, being equal to $u^2 = 1^{-6}$ within
the same progeny and without intercrossing. If we assume
however that such a mutation would occur in n individuals
with homozygotes being formed through intercrossing, the
total frequency of the mutant might rise appreciably
though hardly reaching a value of more than 1%. Even
this very slow increase might be considered sufficient
since we discuss events in geological time which are
generally counted in millions of years.

Number and Interaction of Allels Per Gen Locus

Next we must consider the number of allels per
gen locus. Experimental evidence indicates that most
frequently there are only two forms of phenotypic
manifestations per gen locus. This may either mean that
only two types of mutations were possible, each
determining one of two phenotypes, or that there might
exist any number of mutant allels but only two forms of
manifestations.

In accordance with the nature of interallel
interaction there may exist three genotypes with the
mendelian proportions of (1:2:1) or two with the
proportion (3:1). This means that the number of
phenotypes would be reduced to two, one of which with
three times the frequency of the other.

Non-allelic Interactions

If the allels at each gen locus should not
interact with each other, the maximum of phenotypic types
with n gen loci would be either 3^r or 2^r. This number
would be further reduced in those cases where allels of
different loci should interact in such a way that various
genotypes would manifest identical phenotypes. As an
example cases of such interactions will be mentioned
starting from a bifactorial proportion of (9+3+3+1). In
case of a cumulative effect of the number of dominant
allels, the proportion might be changed to (9+6+1). In
case of complementary interaction requiring at least one
dominant allel in each of the two loci, the result
would be a segregation of (9+7) or when both loci have
to be homozygous recessive (15+1). The 9 possible
genotypes of a bifactorial segregation with dominant
allels in both loci would be reduced to 4 phenotypes and
then further to a minimum of 2 phenotypes by interallel

interactions. Considering the generally very high total
number of gen locci, such reductions would still preserve
a great deal of diversity in populations of a specie,
but phenotypes based on several genotypes would have a
frequency equal to the sum total of the individual
genotypes including heterozygotes and thus be
considerably larger than that of any genotype individually.

   Random Selection

        Random selection is an automatic consequence (a)
of the fact that on the average each individual of a
population produces more offspring than would be
necessary to maintain population size; (b) of the
existance within a population of numerous phenotypes
with different frequencies and (c) in consequence of
differences in survival capacity. In the latter case
the following should be explained: if a mutant allel
causes a rather weak manifestation and this only when
homozygous then it may cause a certain unbalance of the
genotype resulting in lower survival capacity. If the
penetrance should be considerable, the mutant allel may
manifest its phenotypic effect already in the heterozygote
and thus act as a dominant allel. In this case the
unbalance of the genotype will also be much stronger.
In fact rather few such dominant mutants have been
observed in experimental work. Finally if a mutant has
very little penetrance, it can be considered as a
neutral allel and will hardly affect survival capacity.
Generally the action of individual mutants of this type
cannot be analyzed individually and they are thus mainly
considered as modifiers of the action of other mutants
with a stronger penetrance. In fact selection of such
modifiers may affect and reforce the penetrance of other
mutant allels, and remove subviability. This last step
will however not be anymore the result of random selection
which, by definition, is without direction, depending
exclusively on the frequencies of phenotypic

manifestations of mutants. It may result in the
preservation of some of them within the population and
the accidental elimination of others in the next
following generation. Diversity in the population will
be reduced and homogeneity accordingly increased.

Ecoselection is a term which I propose exclusively
to indicate selection caused by interaction between
phenotypes, resp.   their genotypes present in a
population, and the ecological conditions under which the
population lives. One can distinguish three different
situations:

a) Some phenotypes may neither be advantageous
nor disvantageous under the existing conditions and thus
may be considered "neutral", not suffering darwinian
selection, except at most in the form of a modifier
shift, stabilizing the phenotypes and compensating any
eventually existing subviability. Since such
manifestations must have resulted under random selection
reference to them was already made in the preceding
paragraph. Thus they do not form any type of adaptation.

b) Other phenotypes may be advantageous under
existing ecological conditions and their frequencies
might be increased under darwinian selection.
Consequently they might be considered as adaptations.

c) Finally some phenotypic manifestations might
be unfavorable under the existing conditions of the
environment and consequently will suffer under darwinian
selection and may eventually eliminate from the population.

The phenotypic manifestations and their genotypical
basis have been established under random selection and
are completely independent from each other. Ecoselection
may affect them individually. The term "adaptation" does
not refer to some situation resulting from selection,
but something which existed incidentally after random
selection. This situation has been called by some

authors "preadaptation".

DARWIN established the principle of the "survival of the fittest", though originally he used the terms "of the strongest". His followers tried consequently to interpret as many characters as possible attributing to them special adaptative value in increasing favorably their frequencies. This has led many to use even a certain form of teleological thinking, presenting their interpretation of facts, but not proving that all characteristics thus interpreted in fact improved survival capacity. In an earlier part of this paper I tried to show that a proof of any darwinian interpretation is absolutely necessary and that for instance the adaptations of orchid flowers to specific pollinators are generally inefficient. Survival capacity is only restored through another, but completely independent adaptation guarantying an excessive number of seeds in each of the few fruits.

We can now draw the following conclusions:

I. All the items so far mentioned change somehow the frequencies of genotypes, respectively of the phenotypes they cause, within a natural population in favor of the more frequent ones.

II. The different factors enter sucessively into action in the following order: pressure of recurrent mutation rate, interaction between allels at the same or at different gen loci, random selection and finally ecoselection, but they remaining then in action continuously.

III. As a result the population will have characteristics which are either neutral, i.e. are not-adaptive with regard to the ecological conditions present, or can be considered as adaptations to these conditions, besides an evolutionary potential gen reserve, mainly formed by modifiers with rather weak phenotypic effects.

IV. The term ecoselection is used in a different
way from the originally, proposed by DARWIN. Adaptations
are not produced under the principle of a "selection of
the fittest" but are present as a result of random
selection.

V. Ecoselection does not alter the frequencies of
neutral characteristics present, it may increase the
frequency and expression of adaptive characters, but it
will reduce the frequencies of unfavorable characters or
even eliminate them, unless they have been compensated
by other and independent characteristics.

The items which now will be discussed hardly
affect the frequencies of the different allels in the
genotype but can influence strongly their homozygosity
and heterozygosity.

The Mode of Sexual Reproduction

The mode of sexual reproduction is another
important element in establishing models of population
genetics. It has been also an object of special interest
to taxonomist who used the characters of reproductor
largely in distinguishing taxa at different levels.
Also DARWIN and his followers saw their importance and
studied the modalities of sexual reproduction specially
from their point of view. Somehow the opinion was
accepted that there are two alternative forms of sexual
reproduction: allogamy and autogamy. On the other side
I came to the conclusion that they are not mutually
exclusive alternatives, but that there are quite
frequently mixed forms of reproduction in various degrees
of mixture between two alternative extremes. The question
can be easily solved by a simple experimental arrangement,
using plant species with hermaphroditic flowers, the
male and female organs maturing at the same time.

        One plant of a specie carrying a genetic marker
is to be planted in the middle of a number of other
plants of the same species but carrying another marker.
The F1 sometimes the F2 from seeds of the central plant
'will then show whether it was self-pollinated or
outcrossed and the percentages of both can be determined.

        There are also other indication of the occurrence
of mixed pollinations. *Nicotina tabaccum* has the typical
flower of a species adapted to insect pollinization: the
flowers are attractive bright pink and produce nectar
abundantly. Nevertheless even an isolated plant, well
protected from pollinators, will produce well filled
capsules from all flowers. Though it has the
"adaptations" of an insect pollinated species, its
anthers are arranged tightly around the stigma and shed
their pollen abundantly at the time when the stigma is
receptive.

        Another interesting case was observed at the John
Innes Institution when I started work there. The flowers
of the sweet pea are not automatically selfpollinated.
The anthers form a close cycle with the pollen sacs
slightly above the stigma in the middle. The corresponding
petal is doubled up along its midnerve, with its sides
tightly pressed together and only a small opening at the
tip. Rubbing this petal between ones fingers, the
flowers were selfed. If on the other hand, a common bee
settles on the petal its weight forces the petal to move
somewhat downwards and this movement causes pollen grains
out of the pollen sacs and on to the stigma, also causing
autogamy. However if a representative of a larger and
heavier species visits a flower, its heavier weight
forces the petal much further down exposing the stigma.
If the large insect has visited other flowers previously
and collected pollen placing it in a kind of basket on
his legs, then it may happen that some foreign pollen
grains fall out of the basket on to the stigma, causing
a certain degree of allogamy, in the present case
considered as contamination. Thus the species seems to

be adapted to one pollinator species promoting autogamy,
but when visited by another pollinator produces mixed
seeds.

        Next maize shall be mentioned, a wind pollinated
species with separate male and female flowers on the
same individual. Maize is also called as being
"panmitic" since here the pollen is not carried by a
pollinator from plant to plant, but first the pollen of
many plants is mixed in the air by wind and lighter air
currents, before it slowly falls downwards owing to its
weight. The male flowers are formed at the top of the
plant in the tassel, while the female flowers of the ear
stand about in the middle of the plant in the axil of a
leaf sheath, protected by a number of additional sheaths
and only the stigmas, the silk, hanging out of this cover.
Even if pollen grains could fall vertically down, they
will generally not reach the silks which are protected
by the leaf blade above, leaves being arranged in two
rows, to the right or left of the main stalk, on either
side each leaf above the next one lower down. Thus it
seems that the whole arrangement is an adaptation to
allogamy. However if one observes plants growing in an
isolated place, far from other maize plants one will
find that their ears contain generally some kernels which
thus must have come from autogamy. Thus in panmitic
maize, allogamy present dominates but does not exclude
occasional autogamy.

        Though possibly or rather probably some mixture
between allogamy and autogamy are common in hermaphroditic
species, in bisexual ones of animals or plants crossing
between closely related specimens such as brother-sister-
-matings may take effectively the place of autogamy as a
rather close approach. From a general point of view we
can consider exclusive allogamy or exclusive autogamy as
extremes of a wide range of mixed sexual reproduction.
This is important since these two extremes will
necessarily cause quite different results. Under allogamy
heterozygotes are present as the result of combination

at the beginning of every new generation, while under
autogamy the number of heterozygotes will be automatically
halfed in each generation so that ultimately no
heterozygotes will be present, except some resulting
from new mutations.

      This will however affect the general situation
considerably, in view of the fact mentioned above that
many phenotypical manifestations are of a polymeric
nature and thus require the combination of several
mutated gens in different chromosomes. In exclusively
autogamous species intercrossing between individuals not
taking place, the population will consist of several
homozygous genotypes and many combinations of mutants at
different gen loci will not be present. This would
certainly restrict the possibilities of evolutionary
changes and might justify the opinion of many authors
that autogamy is a "blind evolutionary alley". But even
if autogamy should be in some species absolute rare
intercrossing may still act as a compensating factor.
In fact, there is no question that strictly autogamous
species have evolved and exist. This is another
indication that absolute autogamy can only exist when at
least ocasionally substituted by some intercrossing
within a population.

   Hybrid Vigor

      It was just discussed that some degree of autogamy
will reduce the frequency of heterozygotes in a population,
thus it will be indicated to discuss special situation
in which the genetic basis causes an increase of vigor
and thus affects favorably the frequency of heterozygotes.
The necessary genetic basis may result from two special
situations: cumulative interaction of allels at different
gen loci or a heterotic interaction of two allels at the
same gen locus. The first may be responsible when hybrid
vigor becames manifest in crosses between unrelated

populations within a specie or in interspecific hybrids,
as a result of the accumulation of allels which when
incidentally combined had a cumulative effect. In the
second case however one has to assume that within   one
population mutant allels with a heterotic interaction had
become accumulated. Some authors discussed in the past
whether the last mentioned genetic formula could be
accepted and they postulated instead that the two allels
in question should be considered as belonging to two
different gen loci which however were absolutely linked.
Even in the very few cases where a heterotic effect was
caused by one pair of allels at one gen locus, no support
for this assumption came forward. In one of such cases
that of *Silene armeria* where the monofactorial heterozygote
has deeper coloured flower than either parent line. I
used heterozygotes as material for student class work
during many years. F2 segregations were always in
agreement with the ratio 1:2:1 and no break of the
supposed linkage was ever observed in several hundreds
of F2 generations.

By definition homozygotes will have mostly less
vigor than heterozygotes both in the case of cumulative
effects of allels in different or heterotic effects of
allels at one locus. In the case of cumulative
interaction, the situation might be explained using a
bifactorial model: AAbb x AaBb. Such a heterozygous
hybrid would in fact give homozygotes of the formula
AA BB, consequently as vigorous as the double heterozygote
AaBb, but with a frequency of only 1/16. Since there is
no reason to admit that only two loci might be
participating in causing hybrid vigor, the frequency of
such multiple homozygotes will be much smaller and might
become practically insignificant. In the case of
heterotic allels reduction of vigor caused by homozygosis
will be unavoidable by definition.

Next question shall be briefly discussed how gene
causing hybrid vigor might become established during the
evolutionary process. Since the formation of mutants

with various phenotypic effects is a matter of chance,
there is no reason to reject the possibility of two
mutations occurring independently, which however when
incidentally combined might result in a cumulative
effect such as hybrid vigor. The situation is on the
other hand different regarding heterotic pairs of allels.
These might easily be preserved in predominatly or
exclusively allogamic populations where heterozygotes
are formed automatically in each generation. In
predominantly or exclusively autogamous population the
situation will be quite different. In these the number
of heterozygotes is automatically halfed in each
generation through autogamy and thus heterotic pairs of
allels could only be preserved when they cause such an
extremely strong vigor able to compensate the loss
through segregation. This then would explain why heterosis
is absent whenever autogamy predominates. Hybrid vigor
is not to be considered as an "adaptation promoting
allogamy", but as its consequence.

     Self- and Cross-Incompatilility

          Self- and cross-incompatibility are both
interpreted as possible adaptations promoting somehow
either allo or autogamy. In fact however it seems to
me rather more correct to consider them not as causes
favoring one or other form of sexual reproduction but as
effects of the latter.

          Gens for self-incompatibility are not too rare in
species of hermaphoditic plants. If they should arise
by mutation in a predominantly autogamous species they
will have little chance of being preserved under such
conditions while nothing will hinder their presence in
predominantly allogamous populations. Such a self-
incompatibility might either be caused sporophytically
or by an interaction between the female organs of the
mostly diploid sporophyte and the generally haploid

male gametophyte. The latter is the cause of the S-allels
analyzed in details by EAST and his coworkers A.
MANGELSDORF and BRIEGER. These S factors represent a
mixture of both self- and cross-incompatible genetic
elements. In a cross between two plants carrying one
identical allel for instance S_1S_2 x S_1S_3 the S_1 pollen
tubes do not function normally but have their growth
stopped in the style. If both allels should be
different S_1S_2 x S_3S_4 all pollen tube grow normally and
in crosses S_1S_2 x S_1S_2 or after selfing all tubes stop
and no seeds are formed at all. Since the S-allels form
a rather long series or multiple allels in natural
populations, the eventual cross-sterility of some
combinations do not affect seriously the frequency of
individual allels in predominantly allogamous populations,
though in autogamous populations such allels arising
by mutation hardly will have a chance of preservation.
It must also not rare in species of hermaphroditic plants,
but their action is frequently quite incomplete. Thus in
Nicotiana sanderae, the S-allels are quite inactive in
buds and thus in flowers when just opening as also in
the last flowers formed at the end of the flowering
period of a panicle.

       The three factors just discussed, mode of sexual
reproduction, hybrid vigor and self- or cross-
incompatibility hardly affect individual gen frequencies,
but will alter the frequencies of phenotypic character
manifestations and thus indirectly that of their
genotypes whether heterozigous or homozygous.

    Evolution and the Distinction of Taxa

       Evolution and distinction of taxa represents the
last subject to be discussed. Taxonomy or Systematics
whatever designation one may prefer, is the oldest
branch of biological sciences which was started in the
middle of the 18th century, well before any evolutionary

idea had been expressed. Linnaeus's first task consisted
to bring order into the vast material to be studied.
He noticed very clearly that the enormous diversity of
organisms is not a continuous one. This led him to
distinguish very sharply two categories which he
designated as those of the genus and of the species
giving already more weight to the latter.. The origin of
such categories did not represent any problem to him
since he was firmly convinced that the biblical thesis
was correct and that there existed only those species as
created by the supreme intelligence. With the acceptance
of the evolutionary concept new problems arose which are
partly not yet very clearly resolved. Some authors up
to recent times try still to consider the species as a
real and quite distinct entity. Thus DOBSZHANSKY and
others postulated that species must always be separated
by "barriers of reprodutive isolation" and tried to
explain their origin in the following way: a specie,
though initially occupying one continuous area, came
somehow to be distributed over two quite different and
separate areas. When this possibility had to be rejected,
it was assumed that it became adapted to two different
ecological niches within one geographical area. In
both cases it was then assumed that intercrossing between
such separated populations became impossible and that in
consequence each accumulated different mutants which
when later crossed caused mutual incompatibility between
components of the two separated populations. Thus a
barrier was established and the two populations could
now be considered as two separate- species. This
speculative construction can be discarded, since the
basic assumption of the obligatory barrier between species
cannot be accepted. In many groups of animals there
exists in fact a complete incompatibility between species
as usually classified, while in few other groups of
animals and in most higher plants not only interspecific
but also intergeneric hybrids occur in nature and also
result from experimental hybridizations. The result of
such crosses is to some extent unpredictable and may
vary from complete failure to obtain even hybrid embryos

to the other extreme of obtaining Fl-plants with full
fertility. J.CLAUSEN and others have attempted to use
such degrees as a measure of taxonomic affinity between
species or genera procedure which dependes again
essentially on purely subjective reasoning. Similar
gradual differences exist between normally developing
mutants over subviable ones to lethals.

The conclusion that reproductive barriers are not
obligatory for separating species but they do not exist
rarely. This forces one to ask inversity: how can such
barriers be formed during evolution? The S-allels
already mentioned above cause both self and cross-
incompatibility within populations. Heterostyly in some
plants such as *Primula* also causes a certain form of
cross-sterility. There are two groups of plants present,
some with a long style and thus with the stigma in a
high position while the anthers are inserted below in the
corolla tube, and others with a short style and thus the
stigmas in a low position with anthers inserted higher
up in the floral tube. There is a strong degree of
sterility between plants within each group and full
fetility in crosses between plants of different groups.
In analogy one might postulate that mutants might occur
in a heterozygote state Cc without having any effect in
an adult organism, but mutually unable to form zygotes
when combined in reproduction. Thus all individuals of
the constitution C- as those of constitution c- are
interfertile while no zygotes result from combinations
of C- x c-. The population will then be split into two
intersterile parts and each part might accumulate
independently different mutations.

In view of the absence of any objective criterium
to delimitate the range of variation both within and
between species or genera, the taxonomist has to evaluate
subjectively the degrees of similarity or disimilarity
between taxa. This method depends very much upon
personal bias and consequently opinions of different
authors not rarely disagree with each other though it is

also understandable that different authors decided to
use different characters in evaluation diversity. At
first the differences in external and internal
structural elements and their functioning were chosen
to estimate degrees of differences and this method still
represents the basis of taxonomy. Later on chemical
differences were included. With the growing amount of
genetical knowledge, the possibility offered itself to
form opinions over the nature of the genetic basis of
characters and judge thus at least apparently the
progressive increaser of differences in genetical
constitution. Differences in the pairing of chromosomes
during meiosis were accepted as a criterium though this
required the possibility to obtain at least interspecific
hybrids. Rather recently it became possible to go even
further isolating individual chromosome combining some
obtained from different species and then verifying how
intense this pairing was by determining at what
temperatures 50% of the chromosomes unpaired. SIBLEY
and AHLQUIST used this method in a study of a very large
number of Australian birds. In part they confirmed the
decisions of other taxonomists, while in other cases
they suggested new forms of classification. The authors
assumed arbitrarily that a lowering of temperature
determining losses pairing of chromosomes would indicate
degrees of lower evolutionary affinity. However it is
not known that other factors but evolutionary affinity
do in fact control of the pairing intensity. It should
be remembered that a mutant allel has been found in
maize which inhibits pairing during meiosis even in the
presence of ten pairs of homologous chromosomes.

        One can only conclude that there does not exist
an absolutely sure method to judge or measure evolutionary
offinity and that it depends on personal subjective
judgement to which method each biologist might be inclined
to give more weight in case of contradictory results.

        On the other side it is quite evident that
variation of characters of any sort is not continuous

one, but that there exist differences of various
dimensions between the so-called taxa. The decision made
by LINNAEUS more than 200 years ago was a correct one
from a practical point of view. His decision to establish
the ranks of species and genus was fully justified and
remains valid with the inderstanding that these ranks
are conventional and thus arbitrary units. Nevertheless
some informations regarding evolution can be obtained
from taxonomic data. I have studied during several
decads the taxonomy of orchids which represent the
family of higher plants with the maximum degree of
diversification as measured on the basis of the number
of species and genera described. The great majority of
the 20 to 25.000 species is composed of clearly defined
units with a minimum os internal variability while a
comparatively small number is so polymorphous that they
had to be divided into subspecies or even lower ranks.
An identical situation exists regarding about a dozen
genera of a total of more than 800. They contain more
than 100 to over 500 species and can clearly be divided
into a number of subgenera though some of them can very
well be considered as separate genera. It cannot be
decided whether such polymorphous groups are for some
unknown reasons subject to a greater tendency for
diversification or species, resp.   genera still in
active evolution while others have passed such a phase,
unable to further diversify and determined to eventual
extinction.

     The impossibility to define in an objective way
what should be considered as "a specie" leaves only the
conclusion that "the species" is not a fixed unit of any
sort. It is in fact a stage within the evolutionary
process which progresses slowly and might appear static
for a limited period of geological time. What taxonomists
call "a specie" corresponds to a segment of geological
times corresponding in a way to an isolated snapshot
taken out of a lost, but originally long cinematographic
film. Speciation requires first the accumulation of a
large number of mutants and this will be rather slow

both under recurrent mutation rate and random selection.
Considering that geological times are measured in
millions of years, one may estimate that in one million
years there may exist the following numbers of generations:
about 35 million generation in the fruit fly one million
in annual plants, some fifty thousands in longer living
organisms like man or many trees which may start
reproduction after some 20 years. Such high numbers of
generations should be sufficient for even slow evolutionary
progress. SIMPSON has given more detailed informations
about this matter, specially accentuating that evolutionary
progress has taken place at quite different rates in
different taxonomic groups.

        In spite of the impossibility to define the species
concept-like those of a genus or still higher taxonomic
levels-in a definite and objective form, it still
represents a usefull and indispensable tool in all branches
of biological science, necessary the material with one
works.

CONCLUDING REMARKS AND ABSTRACT

        Initially a clear distinction is made between the
evolutionary process and its mechanisms.

        It is perhaps not surprising that from time to
time renewed attention is being given to what CUVIER more
than two hundred years ago considered as evolutionary
revolutions and which is now revised in the modern form
of "punctualism". The gaps in fossil records and between
the highest ranks of living animals are certainly
impressive and still are unexplained. Consequently they
remain open for more or less speculative interpretations.
The main trouble consists in the lack of secure data
from past geological periods regarding "missing links"

which might or might not have existed. Thus only
subjective interpretations are possible to explain those
gaps. In plants such problems seem not to exist and the
evolutionary sequence if not from Algae, at least from
Pteriodophytes to the highest Angiosperms seems to be
reasonably continuous, partly through fossils, partly
based on studies of comparative morphology of still
living intermediaries. Regarding speciation or what
R. GOLDSCHMID called "microevolution", a hypothesis with
respect to its evolutionary mechamism has been exposed
in the present paper. The time rate of evolution has
been studied by SIMPSON in detail and found extremely
variable in terms of geological times, and the rate of
evolutionary radiation is equally different in different
groups, both including gaps or missing links. Such gaps
may be considered larger in animal taxonomy. Without
evidence regarding the existance or non-existance of
such larger missing links, there seems to me no reason
to assume some special evolutionary mechanisms for higher
ranks or for what GOLDSCHMID has called "macro-" and
"mega-evolution".

There are no doubts regarding the evolutionary
process as such. It started some 2.5 billion years ago
when under special external conditions complex water
soluble chemical compounds, similar to protein and other
"organic" substances were formed. The process then
continued permanently forming first simple one-celled
organisms up to that of complex multicellular ones.

The evolutionary mechanism requires still a
careful and critical analysis. Two main theories have
been proposed. LAMARCK postulated that there sould
exist and inheritance of acquired characters but this
was rejected owing to proof to the contrary. DARWIN'S
started from the observation that orgamisms suffer
ocasionally, though rarely heritable changes, at his
time called sports, now mutations, the nature of their
phenotypic manifestations being unpredictable and in no
relation to any directed progress nor automatically

advantageous under ecological conditions. Furthermore
DARWIN postulated that these mutants became subjected to
natural selection. Which led to competition between
them resulting in the "survival of the fittest". Among
the great number of such darwinian interpretations, the
relation between orchid flowers and their pollinators
are cited. The rarity of fruit formation in the majority
of tropical orchids show clearly that these adaptations
are highly inefficient and instead of increasing survival
capacity reduce it to such an extent that only another
and unrelated character, the extremely high number of
seeds per capsule reestablishes survival capacity. Thus
the essence of the darwinian concept must be rejected.
Actually this has already been the case though in an
indirect form. It became evident that adaptations in
general require the accumulation of numerous mutant gens
before they really are of value increasing survival
capacity. Their accumulation must thus precede any
increase at survival capacity it is a consequence, i.e.
their must have been an accumulation preceeding phase of
"preadaptation".

The increase of mutant allels is first caused by
recurrent mutation rate. Interactions between allels
both at the same and at different gen locus reduces the
number of different phenotypic manifestations, increasing
thus the frequencies of those manifestation which are
based jointly on different genotypes. The differences
in frequency on the one hand on the other hand the
excessively large number of descendants of organisms in
general far in excess over the necessary for preservation,
will cause a strong elimination or random selection.

The more frequent phenotypes have a better chance
for being maintainer than the less frequent one. Some
of them cannot be interpreted as "adaptations" but are
rather of a neutral character. Others might be considered
as adaptations favorable under existing ecological
conditions; finally some might be in disaccord with the
existing conditions and thus will have their frequency

further reduced up to complete elimination by what might
be called "ecoselection", which I also have called in the
text "darwinian selection". These different factors will
enter into play in the order they have been mentioned,
but will continue to act jointly, during a considerable
time. The evolutionary process might come to an end when
all probable mutations have been incorporated or
eliminated from a population. However there remains always
the possibility of a mutation occuring with an entirely
new phenotypic effect and which thus may start a new
evolutionary cycle.

Besides the factors already mentioned some others
may affect mainly the relative frequencies of homo- and
heterozygotes. The mode of sexual reproduction is
probably the most important and probably the most
frequently discussed one. It is pointed out that
autogamy favoring homozygotes and allogamy favoring
heterozygotes represent only the extremes of a wide
range of combinations of mixed reproduction. Self-
incompatibility and heterotic gen interactions also favor
heterozygosity.

As a conclusion of the paper the delimination of
taxa, specially of species is discussed. It is shown
that there is no method yet available to define the
species concept objectively. It remains however evident
that variation is discontinuous and that such
discontinuities can be used to distinguish taxa at
different and subsequent levels. In general the lowest
level receives the classification of "species" which
thus represents a conventional and practicaly
indispensable unit. The great majority of these species
is well defined by their relative uniformity within and
the descontinuity between these units. Only a small
percentage offers difficulties owing to polymorphism.
The same can be said regarding the next higher level,
that of the genus.