

LINNÉ's VIEWS ON THE HIERARCHY OF THE TAXONOMIC GROUPS

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It is the fate of most historic personalities that in the course of time their work sinks almost completely into oblivion, and that the few lingering reminiscences of their achievements are transmitted to later generations in the form of second- or third-hand quotations, usually mixed with more or less anecdotic episodes from their life. It must be admitted that LINNÉ occupies in this respect a comparatively favourable position, for most educated people will remember that they heard in their school days of at least three things which are credited to him, in the first place that he produced a classification of the plant kingdom which is based on the number of stamens and carpels, the so-called sexual system, in the second place that he was the first who consistently applied the binomial nomenclature, i.e. the custom to designate an organism by a combination of two names, viz. a generic and a specific one, and thirdly that he was the originator of the pronouncement "Species to numeramus quot diversae formae in principio sunt creatae" (We count so many species as in the beginning different forms were created). Other achievements of LINNÉ may have been of greater importance, but it are these three things for which he is most generally remembered.

The pronouncement quoted above, which means that the groups of individuals which form the species are descended from ancestors that owed their origin to an act of creation, derives its historic importance from the part it played in the debates on the theory of evolution. As it implies that the species are constant, it became the watchword of the antagonists. It is, however, rather strange that this pronouncement has so often been quoted, for it is found in LINNÉ's earlier works only, and was in the later ones replaced by another statement that flatly denies the constancy of the species.

The later pronouncement meant in the preceding paragraph is, like the first, found at different places in his work. It was first brought forward in the 1761 edition of his "Systema Plantarum", then in the dissertation "Fundamentum Fructificationis, prop. J. M. GRÅBERG, 1762" (reprinted in *Amoenitates Academicae VI*), and thirdly in the Supplement to the sixth edition of his "Genera Plantarum", which

dates from 1764. That I quote the doctor's thesis of his pupil GRÄBERG as a work of LINNÉ finds its justification in the circumstance that LINNÉ himself adopted the same attitude. He did this with regard to all the dissertations of his pupils, and when we see that his contemporaries too credited these works to him, it seems permitted to assume that they may indeed be regarded as his spiritual property. It can hardly be doubted that the redaction of these dissertations is by the hand of the promovendi; this appears from the striking differences in style and from the use of words that are not found in the works that LINNÉ edited under his own name. The explanation probably is that the subjects were first discussed at some length by LINNÉ, and that the latter asked the promovendi to write down the gist of the discussion. Howsoever this may be, it can not be denied that the dissertations published in the "Amoenitates Academicæ" are of the utmost importance for our knowledge of LINNÉ's views. LINNÉ's own works are written in the aphoristic style that was so much in vogue in the seventeenth and eighteenth century, and because of its exaggerated conciseness it is often difficult and not always possible to see what he really meant; in the dissertations, however, we find on the whole a methodical reasoning.

In the formulation used in "Genera Plantarum, ed. 6, 1764" the pronouncement which replaced the earlier one quoted in the introductory paragraph, runs as follows:

"1. CREATOR T. O. in primordio vestitit Vegetabile *Medullare* principiis constitutivis diversi *Corticalis*, unde tot difformia individua, quot *Ordines Naturales* prognata". (In the beginning the thrice exalted Creator covered the medullary substance of the plant with the principles of which the various kinds of cortex consist, and in this way as many individuals were formed as there are now Natural Orders)

"2. *Classicas* has (1) plantas Omnipotens miscuit inter se, unde tot *Genera* ordinum, quot inde plantæ". (The vegetable prototypes of paragraph 1 were mixed with each other by the Almighty, and there are now so many Genera in the Orders as in this way new plants were formed)

"3. *Genericas* has (2) miscuit Natura, unde tot *Species* congeneres, quot hodie existunt". (The generic prototypes of § 2 were mixed with each other by Nature, and in this way in every Genus so many Species were formed as at present exist)

"4. *Species* has (3) miscuit Casus, unde totidem, quot passim occurrunt *Varietates*". (The Species whose origin was explained in paragraph 3, were mixed with each other by Chance, and in this way the Varieties arose that here and there are met with).

The form in which these pronouncements are made, strikes us as strange and old-fashioned, and to some extent it was so already in LINNÉ's own time. We might perhaps be inclined to ascribe it to the influence of the atmosphere in which he grew up, that of the vicarage of Stenbrohult, but it seems more probable that it is, at least partly, due to the influence of that other remarkable Swede, the mystic SWEDENBORG. It is true that SWEDENBORG's name is nowhere mentioned

in the works of LINNÉ, but they must nevertheless have known each other rather intimately, for it is reported that SWEDENBORG was elected a member of the Swedish Academy on LINNÉ's recommendation. Although these two exceptional men were in many respects utterly different, there is an unmistakable similarity in the way in which they expressed their views. At any rate we must see LINNÉ against a background filled by men who were used to debate on theological questions and who even when the debate concerned questions of a different nature clothed their arguments in a theological form, and we should be careful not to place him in the circle of the physicists and physically orientated philosophers of that time: that was a world with which he had no contact, and for which he felt no interest. SACHS, the deserved plant physiologist of the preceding century, was in his "Geschichte der Botanik" certainly not always fair in his opinion of LINNÉ, could in fact not be fair to him because he was unable to cross the void that separated his own spiritual atmosphere from that of LINNÉ, but in one respect he certainly was right: LINNÉ was in most respects a man of the past; he was the dominating figure in an epoch of the history of plant taxonomy that was drawing towards its close. It is his great merit that he sifted the material that had been accumulated by his predecessors, and that he arranged it in an easily surveyable system, but the house that was built by him, although solid enough, was old-fashioned, and it was for this reason soon deserted; the foundations of the building in which plant taxonomy would find a more suitable home, were laid a quarter of a century after LINNÉ's death by ROBERT BROWN and AUGUSTE PYRAME DE CANDOLLE. These men expressed their views in a modern way, and were easily understood. That afterwards on the whole so little attention was paid to LINNÉ's ideas, even in those cases where they certainly would have deserved a better lot, is doubtless for a large part due to the antiquated form in which they were presented. The nineteenth century with its entirely different orientation was not interested in this kind of literature; in the biologists of that time it even will have awakened a feeling of aversion.

To this mental aversion it is probably due that LINNÉ's views on the nature and origin of orders, genera, species and varieties received no attention in the immediately following period. A quarter of a century later WILLDENOW, who was a very able and many-sided botanist and who, moreover, as the editor of a new and considerably enlarged edition of LINNÉ's "Species Plantarum" must have been well acquainted with the latter's works, makes no mention whatever of these views in his nevertheless fairly detailed handbook of botany. At a much later epoch, when the contention between the evolutionists and their antagonists had been decided in favour of the first, and it became a kind of sport to hunt for prae-Darwinists, these pronouncements of LINNÉ were once more unearthed in order to prove that LINNÉ too had been one of them. This, however, was doubtless a mistake. The theory of evolution assumes that all organisms, living and extinct, are descended either from a single prototype or else from a

small number of prototypes, but in LINNÉ's scheme first a rather considerable number, viz. fifty to sixty (v. infra) prototypes are created, and then out of the latter, which might be called the prototypes of the first order, by a manipulation of the Almighty himself fifty to sixty times as many prototypes of the second order are formed, i.e. 2500–3600, and as they are the vegetable prototypes only, this number has presumably to be doubled in order to account in LINNÉ's way for the diversity in the whole world of the living. This would mean therefore that LINNÉ must have assumed the existence of 5000–7200 prototypes, and this, I am afraid, is a number that no self-respecting evolutionist will swallow. We should, moreover, not overlook that LINNÉ's prototypes differ only in characters of minor importance from their descendants, and this too is hardly reconcilable with the intention of the theory of evolution. However, LINNÉ may nevertheless on account of this pronouncement be heralded as a precursor of modern views, for the picture it gives in the paragraphs 3 and 4 of the origin of species and varieties agrees in broad lines with the views developed in the first quarter of this century by LOTSY in his hybridization theory.

It is perhaps not superfluous to point out that nobody at present doubts the possibility that new species may arise as a result of hybridization; the only point on which there can be diversity of opinion, is, whether this is, as LOTSY assumed and as LINNÉ too had postulated, the only way in which they arise, or that there are more ways. However, we should realize that the origin of new species is a problem whose solution is but of subordinate importance to the theory of evolution. MACDOUGAL, the editor of DE VRIES' "Species and Varieties, their Origin by Mutation", printed on the fly-leaf of that work three statements, respectively from LAMARCK, DARWIN and DE VRIES, which illustrate the advance of our insight in the problem of the origin of species, but which reveal at the same time of how little avail this advance has been for the solution of the main problem of evolution, the origin of the more widely different groups. LAMARCK says "The origin of species is a natural phenomenon", DARWIN "The origin of species is an object of inquiry", and DE VRIES "The origin of species is an object of experimental investigation". DE VRIES expresses here the modern standpoint, but seen from this standpoint it is clear that there gapes a large gulf between this problem and that of the origin of the larger groups, where, as their representatives can not be crossed, experimental investigation is for the moment out of question.

I have said above that LINNÉ revealed himself in the paragraphs 3 and 4 of his pronouncement as a precursor of LOTSY's hybridization theory. To this end I had to assume, of course, that LINNÉ used the term "miscuit" (i.e. "mixed") in the sense of "crossed". Serious doubts at to the correctness of this interpretation are hardly admissible, for what other kind of mixing could he have observed in "nature" (paragraph 3), and what other kind of mixing could be "accidental" (paragraph 4)? In our time we might perhaps think of chimaeras, but this kind of compound organisms were in the eighteenth century

unknown. The ample exposition of the theory in the dissertation "Fundamentum Fructificationis" moreover removes all our doubts, for here actual examples of hybridization are adduced in support. At first view it may nevertheless seem strange that LINNÉ should have thought here of ordinary crossing, for if two species are crossed, we are inclined to expect that the progeny will be more or less intermediary between the parents, not that the hybrids will all reveal themselves as varieties of the species to which the mother plant belonged.

LINNÉ's idea becomes comprehensible when we realize what he knew or thought he knew with regard to hybrids. That was very little indeed. It is true that hybrids are mentioned at various places in his works, but these so-called hybrids were, with a single exception, always aberrant specimens that were found either in the wild or in gardens, and for whose hybrid nature no sufficient grounds were adduced. How unfounded these assumptions sometimes were, is well illustrated by the fact that he once described a hybrid between a *Veronica* and a *Verbena*! Even the exception to which I referred above, a cross between two *Tragopogon* species, is not wholly beyond doubt. The only hybrids with which he was well acquainted, were those between the horse and the donkey, and as these hybrids always show a greater resemblance to the mother than to the father, he assumed that this was a general character of the hybrids, i.e. that they were all metroclinous. This is doubtless an error, although recent studies have shown that the phenomenon is not so exceptional as it was thought to be in the days that hybridization experiments were almost exclusively carried out with organisms that differed in varietal characters only. At any rate, in order to understand LINNÉ's scheme we will have to follow him in assuming that hybrids are always metroclinous. In the following paragraphs, in which his scheme is subjected to a detailed analysis, we will place ourselves on this standpoint.

In paragraph 1 of his pronouncement LINNÉ goes out from the supposition that the plant body consists of two main parts, the medulla and the cortex. The medulla is the essential part, for here finally the seeds are produced, and the latter are according to the traditional interpretation the main organs of reproduction. This essential part must be more or less the same in all plants, for it is because of this part that they and all their descendants are plants. The cortex, on the other hand, is, notwithstanding the fact that it produces the male organs, regarded as less essential. This too is part of the traditional interpretation, but it finds support in the supposed metroclinous character of the hybrids. Because of its less essential character the cortex is more variable in composition. In fact, LINNÉ assumes that there are fifty to sixty different kinds of cortex. This estimate is based on the number of orders that he recognized in his Natural System, for these orders were supposed to be derived from distinct prototypes. These fifty to sixty prototypes of the orders owed their origin to an act of creation. The medulla was to this end covered with a mantle of cortex that was composed of a mixture of different "principles", and of these mixtures fifty to sixty different ones must have been available.

What LINNÉ exactly meant with these "principles", is unknown to me. It may have been either the Aristotelean elements or the somewhat more numerous ones of BOYLE, but LINNÉ gives no further indication; in fact, it is even a riddle why he mentioned these "principles" at all. The point, at any rate, is that we have a medulla that we will indicate, because of its noble nature, with *A*, and round this medulla a mantle of cortex, which may be of various composition, and which, as it consists of more common clay, we will indicate with a small letter. The fifty to sixty prototypes of the orders therefore may be designated with the letter combinations *Aa*, *Ab*, *Ac*, etc. For simplicity's sake we will confine ourselves to the orders *Aa*, *Ab* and *Ap*.

In paragraph 2 of LINNÉ's pronouncement these prototypes of the orders are mixed with each other. This means that the egg cells¹ of the prototype *Aa* are fertilized with male cells derived from the other prototypes, and so on. Under normal circumstances a cross between representatives of different orders is impossible, and for this reason LINNÉ invokes here the aid of the Almighty.

The egg cells are supposed to be derived from the medulla, an erroneous view that was founded on LINNÉ's interpretation of the flower, or, to be more precise, on one of his interpretations of the flower, for in his works two interpretations are found side by side. According to the first, which LINNÉ based on views that via CESALPINO were borrowed from ARISTOTLE and THEOPHRASTUS, the calyx is derived from the "outer" cortex, the corolla from the phloem, the stamens from the xylem and the carpels from the medulla, whereas according to the second interpretation sepals, petals, stamens and carpels are homologous parts, which evidently would be impossible if they really were derived from different layers of the growing point. The incompatibility of these two interpretations apparently escaped LINNÉ's attention, but this is a point that falls outside the scope of this paper. It is sufficient to realize that at this occasion LINNÉ based his conclusion on the first interpretation, according to which the carpels, and therefore the egg cells too, are derived from the medulla.

The male cells are produced by the stamens, and the latter, as stated above, are supposed to be derived from the xylem, which is a part of the cortex mantle by which the medulla was surrounded. They may therefore be indicated as *a*, *b*, and *p*. Now, however, we are confronted with a difficulty, for if the egg cells of all the prototypes created in paragraph 1 were *A*, the combinations would produce no new forms. In order to save the situation LINNÉ therefore must have introduced an auxiliary hypothesis. He must have assumed that the medulla of these fifty to sixty prototypes before going over to the production of egg cells must have undergone a change, which, of course, would have been due to influences exercised by the fifty to sixty different kinds of cortex. In this way the medulla of the prototype *Aa* would have been changed into *A_a*, that of the prototype *Ab* into

¹ The terms "egg cells" and "male cells" are used here and in the following paragraphs for the sake of convenience. LINNÉ himself spoke of "ovules" and "pollen".

A_b , that of Ap into A_p . LINNÉ does not mention this auxiliary hypothesis, but in the aphoristic writings of that time such omissions are not uncommon. Moreover he will not have felt the need for it, for as the fruits are in the various orders of a different kind, he will have found it self-evident that their medulla too must have been, to some extent, different, and these differences could have had no other cause than the different composition of the cortex.

The egg cell A_a of the prototype Aa gives with the male cells a , b and p the combinations A_aa , A_ab and A_ap , the egg cell A_b of the prototype Ab gives with the same male cells the combinations A_ba , A_bb and A_bp , whereas the egg cells A_p of the prototype Ap gives with these male cells the combinations A_pa , A_pb and A_pp . Therefore, in the order Aa in this way the prototypes of the genera A_aa , A_ab and A_ap are formed, in the order Ab the prototypes of the genera A_ba , A_bb and A_bp , in the order Ap the prototypes of the genera A_pa , A_pb and A_pp , i.e. in each of the fifty to sixty orders in this way fifty to sixty genera are produced, altogether therefore 50^2 to 60^2 , i.e. 2500–3600. When this result had been reached, the direct influence of the Almighty was no longer required, and the origin of the species and varieties is in the paragraphs 3 and 4 of LINNÉ's pronouncement left to nature and chance. The mixing of prototypes of genera belonging to different orders therefore is excluded, for this does not happen under natural circumstances. However, even if it had been possible, it would not have led to the origin of a greater number of new forms, for the cortex consists, as we have seen, in all orders of the same fifty to sixty different kinds.

In the different genera too the medulla will undergo an influence of the cortex. In the genus A_aa it will become in this way A_{aa} , in the genus A_ab it will be changed in A_{ab} , in A_ap in A_{ap} ; in the genus A_ba it will become A_{ba} , in the genus A_bb A_{bb} , in A_bp A_{bp} ; in the genus A_pa it will be changed in A_{pa} , in A_pb in A_{pb} and in A_pp in A_{pp} . It are therefore egg cells of this constitution that combine with the male cells a , b and p . In this way we obtain in the genus A_aa the species $A_{aa}a$, $A_{aa}b$ and $A_{aa}p$; in the genus A_ab the species $A_{ab}a$, $A_{ab}b$ and $A_{ab}p$; in the genus A_ap the species $A_{ap}a$, $A_{ap}b$ and $A_{ap}p$; in the genus A_ba the species $A_{ba}a$, $A_{ba}b$ and $A_{ba}p$; in the genus A_pa the species $A_{pa}a$, $A_{pa}b$ and $A_{pa}p$, etc. As in each genus therefore fifty to sixty species are formed, the total number of species will lie between 50^3 and 60^3 , i.e. between 125000 and 216000.

In the same way we might obtain in each of these 125000 to 216000 species fifty to sixty varieties, but this is not to be expected, for accidents, of course, can not be expected to happen so regularly as what is decided by the clockwork of nature, which is set in action by the Almighty himself.

As it may after all seem somewhat strange that LINNÉ postulated but one kind of medulla, whereas he saw no objection against the assumption of fifty to sixty different kinds of cortex, it is perhaps not superfluous to enter somewhat deeper into this problem than we have done above. At first view it might look simpler to assume the existence of fifty to

sixty different kinds of medulla in order to explain the differences in the constitution of the egg cells of the fifty to sixty prototypes of the orders. However, there are two reasons for LINNÉ's choice. The first reason has already been mentioned; it is found in the important place assigned by LINNÉ to the medulla in the life of the plant. To us the medulla is a comparatively unimportant part, which often disappears at an early stage, but to LINNÉ it was what it had been to ARISTOTLE and CESALPINO, viz. the part that harboured the vegetable principle of life. The latter is, of course, the same in all plants, and it was therefore reasonable to suppose that the part by which it is carried was, at least in the beginning, also everywhere the same. The main reason for the assumption that the medulla must be regarded as the seat of the vegetable principle of life, is apparently found in its central position; the central part is evidently the noblest one. That LINNÉ was accessible to this kind of, in our eyes futile, speculations, can not be doubted. However, the second reason will probably have carried more weight with him. If we assume that there are already in the beginning fifty to sixty different kinds of medulla, the origin of the 2500—3600 different genera by the exchange of the fifty to sixty different kinds of cortex is, of course, easily understandable, but in order to explain the origin of the 125000—216000 species, we would have to assume that the medulla in the prototypes of the genera undergoes a change, and this change could, of course, only be due to an influence exercised by the cortex, and if the latter exercised such an influence in the prototypes of the genera, there is no reason to assume that it could not have exercised a similar influence in the prototypes of the orders. This means that LINNÉ's supposition of an originally uniform medulla is indeed preferable.

In the preceding paragraphs I have tried to demonstrate that LINNÉ's theory, if we accept his premise that the egg cells are derived from the medulla and the male cells from the cortex, is indeed acceptable. However, his other suppositions too require a critical consideration.

The main difficulty lies in the contrast between the first two paragraphs of his pronouncement and the third and fourth ones. In the first two paragraphs the result is achieved by the action of a super-natural power, in the two subsequent ones it is due to crosses that are carried out by nature or that are left to chance. In the first paragraph we are faced by an act of creation, and in the second with a process that although of a similar nature as the crosses meant in the next two paragraphs requires the intervention of the Almighty. The crosses between genera belonging to the same natural order are in paragraph 3 left to nature. LINNÉ apparently was of opinion that these crosses offered no difficulty, which shows once more how small the knowledge of the phenomena of heredity in LINNÉ's time still was. Intergeneric hybrids are in reality very rare, and where they are met with, the difference between the genera is often so small that it might be better to unite the genera. The intergeneric hybrids mentioned by LINNÉ himself are, moreover, without exception products of faulty observation

and uncritical inference. In contradistinction to these intergeneric crosses the interspecific ones of paragraph 4 are supposed to be accidental. However, what LINNÉ really meant with the terms "natura" and "casus" is not clear, but that there must have been in his opinion a fundamental difference between the two, need not be doubted. I suppose that he assumed that the intergeneric crosses were carried out according to an immutable plan, and that "nature" therefore must be seen here as a kind of demiurge who executes God's intentions. In that case the difference between the crosses meant in paragraph 2 and the intergeneric ones of paragraph 3 would be that the first were carried out immediately after the prototypes of the orders had been created, whereas the latter are still in progress. This would explain why not all genera exhibit the total number of possible species (50-60). The comparative rarity of the varieties and their irregular occurrence will probably have induced LINNÉ to regard them as "accidental".

If we wish to criticize the theory, we may point out that it is applicable to plants only. It is true that in one of the medical dissertations reprinted in the "Amoenitates Academicæ" a similar scheme is developed for the animal kingdom. The central nervous system is here regarded as the seat of the principle of life, and the ovaries are directly derived from the latter, but as the remaining tissues are also derived from the central organ, be it in a more indirect way, this scheme is not directly comparable with the one we are discussing here.

If we confine ourselves to the kingdom of plants, we may state that by no means all plants are provided with medulla and cortex, which means that the theory is applicable to a part of the plant kingdom only. The fact that not all hybrids are metroclinous means that it is built on an unfounded generalisation, and the circumstance that in the paragraphs 1 and 2, where natural means are insufficient to produce the desired effect, the aid of a super-natural power is invoked moreover, places the theory outside the domain of natural science.

This seems to be a crushing verdict, and the reader may wonder why I did not place these critical remarks in front. It looks at first view as if the whole further discussion might in that case have been suppressed. However, when we consider the matter at somewhat closer range, we will realize that our critical remarks merely concern LINNÉ's explanation, not the factual contents of his pronouncement. The explanation probably was to him, as I will try to expound hereafter, but a matter of secondary importance. What he really wanted to say, is that there are in his opinion among the taxonomic groups of the plant kingdom four different ranks, viz. the natural order, the genus, the species and the variety, and that the differences between these ranks are of fundamental importance. The latter is expressed in a parabolic way by referring the origin of the orders to an act of creation, that of the genera to a direct intervention of God, that of the species to the regulating activity of nature, and that of the varieties to indeterminate influences. LINNÉ's earlier pronouncement, according to which the species owed their origin to an act of creation, is to be understood in the same way. In reality LINNÉ was not

interested in the creation, but what he wished to express in this rather unusual way is that the species are constant, an opinion which he modified to some extent, as we have seen, in later years.

The idea that the differences between the taxonomic groups of various ranks are of fundamental importance, must have been born in his mind when he occupied himself with the elaboration of his natural system. A natural order or, as we now say, a family like the *Umbelliferae* is more sharply defined than the genera of which it consists, and the various genera are more easily distinguishable than the species. The idea therefore rests on experience. However, that was in those days not considered sufficient; the observed facts required an explanation, and in the production of such explanations LINNÉ never was at a loss. In order to show of what kind these explanations were, I will cite here an example derived from another field of his activity. The explanation I have in mind, is at first sight perhaps not so strange as the set of suppositions by which he tried to make the hierarchy of the taxonomic groups acceptable, but if we enter somewhat deeper into the question, it will prove to be at least as unsatisfactory. It relates to his sexual system.

LINNÉ was at first fully convinced that his sexual system was a natural one. It had to be regarded as such because it was based on parts that are of primary importance for the survival of the plants, viz. the stamens and the carpels; by varying the number, the relative size and the degree of mutual dependance of these fundamental parts God had produced the various groups that were recognized by LINNÉ. This explanation apparently pays no attention to the fact that stamens and carpels are not the only parts of the plant that are of importance for its survival, i.e. that there are other parts on which the classification might have been based with as good right. In fact, some of his predecessors, who stood on the same standpoint, had based their classifications on the fruit and the seed, and theoretically it ought to have been possible to base it on the root, as the latter according to the then accepted view was the only organ that serves for the intake of food, which, of course, is a function as important as reproduction. It is almost unthinkable that LINNÉ would have overlooked these objections, and one gets the impression that he merely added the explanation because he did not want to break with an old custom. He may have reflected that without such an explanation his system might have been unacceptable to his contemporaries, and in this he was probably not far wrong. There is, in my opinion, good reason to regard LINNÉ'S entirely superfluous and largely meaningless explanation of his sexual system as a concession to the spirit of the time, and if this view is accepted here, there is every reason to accept it in all similar cases. With regard to the explanation that LINNÉ supplemented to his views on the hierarchy of the taxonomic groups, it is, however, fair to admit that it is from a purely formalistic standpoint a marvellous achievement. With regard to his explanation of the sexual system this can certainly not be said.

The problem for which LINNÉ in his pronouncement on the

taxonomic groups of different rank had offered a solution, may be formulated as follows: are the differences between the groups of different rank of fundamental importance or should we assume that one rank gradually merges into another. We may put the question also in this form: is it possible to decide whether a group of individuals which show a certain degree of similarity, form a variety, a species, a genus or a natural order (I use here LINNÉ's terminology; at present we recognize a much larger number of ranks, but this is here of no importance). These are the questions for which LINNÉ had sought an answer, and the conclusion at which he had arrived, was that the differences between the various ranks were of a qualitative nature. This conclusion is in itself not so very important, for it is for a good deal intuistic, but it proves that LINNÉ had recognized the presence of this problem, and that certainly is worth noting.

Even now the problem formulated in the preceding paragraph is often overlooked. This finds its explanation in the circumstance that most biologists have not been able to free themselves from the ideas of the orthodox evolutionism. To the earlier evolutionists this problem indeed did not exist. LAMARCK as well as DARWIN assumed a nearly unlimited fluctuating variability, and this, of course, excludes the possibility that differences of fundamental importance might be present between the various organisms or groups of organisms. The differences they observed in nature, were to them of an accidental character only, for they owed their origin to the disappearance of groups of variates, the notorious "missing links". Subsequent investigations, which were based on DE VRIES' theory of the gene, have demonstrated the untenableness of this view. The splitting observed in the progeny of hybrids has taught us that the differences are by no means gradual, but that they are sharply defined, and rest on the presence or absence of definite genes. The discontinuity between the various groups of organisms can therefore no longer be doubted, but it remains an open question whether the differences between the groups of higher rank are of the same kind as those between the varieties, though, of course, more numerous, or that they are partly of an entirely different nature. This is a problem on which the study of the phenomena of heredity so far can throw no light, and for the present it is left therefore to the taxonomists to look for a solution. Most of the latter will doubtless be inclined to regard the various taxonomic ranks as qualitatively different, because this seems to be a sound base for the construction of a natural system, but they are just as LINNÉ unable to prove the correctness of this view. It is the standpoint of CUVIER in his controversy with ST. HILAIRE, which GOETHE in his old age witnessed and in which he displayed such a lively interest; the controversy is still actual, but it is now confined to the groups of higher rank.

The genes that are responsible for differences of a varietal character apparently return in widely different groups, and in this respect they remind us of the constituents of the various kinds of cortex postulated by LINNÉ. We have seen that the latter assumed that the different

kinds of cortex returned in different groups. The characters of the groups of higher rank, e.g. the presence of a pollen tube in the *Phanerogamae*, and the particular structure of the gynaeceum and of the ovule in the *Angiospermae*, are on the other hand confined to single groups, and can therefore hardly be ascribed to the presence of exchangeable genes. It seems reasonable to assume that they rest either on non-exchangeable genes or else on hereditary structures of an entirely different kind. LINNÉ might have given them a place in the medulla.

LINNÉ recognized the existence of a hierarchy among the taxonomic groups and he saw that this presented a problem. He also offered a solution of this problem, viz. that the differences between the various ranks are of a fundamental nature, but he was unable to advance arguments for this view. Such arguments are at the present moment too almost entirely wanting. That the taxonomists are on the whole inclined to accept the presence of fundamental differences between the various taxonomic ranks, rests on their conviction that without this assumption the construction of a natural system would lose a good deal of its significance.