CHAPTER XI

The classification and distribution
of the Araceae

We have now seen many interesting new, and at times unexpected facts, like the early branching out of the head of a family into the heads of sub-families, tribes, and sub-tribes; and we have seen the same thing in a genus, even down to the leaders of sub-species in such things as *Rubus* or *Hieracium*. All goes to show that in all this evolution there must be one chief governing law, regulating the formation of new subdivisions, which, it seems to the writer, must be the law of dichotomy by divergent mutation.

The point must also be stressed that all these things have necessarily followed from the law of age and area, discovered by induction from the accumulated facts of dispersal. The rest has been deduction, which, as the writer grew in confidence that he was getting upon the right track, was replaced by simple prediction, which has proved successful, without a failure, in hundreds of cases. The great bulk of the present book is made up of the results of such deductions and predictions, whose verification has involved much work, while the time in recent years has been largely occupied in marshalling the results, and writing them up into a connected whole, this work, however, being often interrupted by the incidence of new deductions, which had to be fitted in with the old. All this work has given the writer great confidence in the general correctness, not only of the original law of age and area, but also of the next stage, the laws of ASA, of the third stage, the law of divergent. dichotomous mutation, and of later deductions from all of these.

It may interest some people to know what the writer has gradually found to be the best way in which to make these

predictions, which are really deductions made by the use of the sub-conscious mind. Instead of sitting down to think out deductions, which are apt to refuse to come at demand, he is accustomed to soak his mind, if one may employ such a phrase, in some subject such as size and space, and leave it. After a greater or less time, up to three or four months, deductions begin automatically to come up, most often at times of waking in the night. For about 45 seconds to about three minutes, but *not more,* one grasps the deduction and the steps that led to it, which sometimes involved more than one premise. He therefore carries slip notebooks, with pencil, in his pockets, has one beside the bedlight, and one on every table used, to write down the deduction before it fades, which it soon does, beyond the possibility of recall. The notes are attached to sheets of paper, and the often arduous labour of verification is put in hand later. The period during which deductions come up may last as long as three months, and the greatest number ever noticed in a night was five. I have long ceased to keep count, but estimate that I have made about a thousand in the last ten years, and not one of these, to my continual surprise, has failed of verification upon examination of the facts, themselves often collected by those who have been my opponents. In general, therefore, it would seem that the premises, such as the laws of ASA and of dichotomous· divergent mutation, must be correct. Naturally, the subject being biological, the correctness of the deductions does not necessarily go into every detail or every instance, so that it is not difficult to find objections in single cases. But it has gone, throughout, by decided majority vote, as is shown for example in many of the tables. After the verification has been done, the necessary account of the result has been put together, and the files have been sorted under heads, until at last the material for a book was complete.

Such work has given the writer great confidence in the general correctness of his theories, and however undesired the new viewpoints may be, it will be realised that distribution, hitherto made largely a field for profitless but fascinating speculation, shows itself as capable of inductive and deductive treatment as any other branch of biology, and that its neglect has left large arrears to be made up. It must at the start be realised that distribution and evolution go together of

necessity, so that the writer has had to study the latter as much as the former, and it is the final result of this study that is here presented, with much evidence in its favour, which if required could be greatly extended, as the laws which are here brought out seem to be of universal applicability.

The branching out of the head of a family into lines that now represent its classification, and at the earliest opportunities, was discovered by this method, and when once seen, was evidently a necessary consequence of divergent mutation. It is in turn a great support to that theory, which with its continual dichotomy produces the hollow curve. The characters of A, the first genus, and B, its first offspring, will be handed down to two different lines of descendants, most often different sub-families or tribes. One or other of the two chief divergent characters of A and B usually shows in all their descendants, and the importance of these characters is simply due to their *age in the family*; in other families the same pair of characters may only mark the distinction between two small groups, even perhaps only genera or species. The same kind of divergent mutation goes on at every subsequent mutation, continually dividing the family into more and more, and smaller and smaller groups, down to small sub-species. Mutations at the top of a family seem to be larger, *on the whole*, and those above family rank larger again, but the interesting problem is one for the geneticists.

The original parent A starts with an outfit of characters of all kinds. Some, but not all, change to divergent characters at the first mutation, which let us suppose produces another genus B. The next genus again will probably be C, a direct descendant of A, rather than B1, but it may be the latter, the probability in favour of C resting upon the fact that A will get a long start while B is becoming established and ready to mutate. It is not at all unlikely that several of the early genera of a family may be direct descendants of the actual leader. On the whole, the early genera will tend to go into sub-families or tribes different from the one that contains the head, but not necessarily so; it depends upon the particular characters that may be chosen for the mutation.

Classification as we know it is a more or less mechanical result of divergent mutation, for it is only upon such divergences of structure that we can at present base a classification. The first divergences to appear will be those that show

between the earliest genera of a family, which are now, of course, the largest genera in that family. These divergences will thus tend to be inherited by the largest number of descendants, so that their features will mark the first divisions of the key, those separating sub-families or tribes. Later divergences will separate smaller divisions, and so on downwards. There is little or no evidence that the first divergences are necessarily " larger ", for it depends largely upon the family; the same divergence may mark a sub-family in one case, and only a genus in another. But the first divergences are the oldest, and therefore mark a larger part of the family than do the later ones. When one fully grasps the meaning of this dichotomous divergent mutation, it is evident that it automatically brings changes into our notions of classification. This of course can only be founded, if it is to be practical, upon the divergences that exist. So long as we believed these to be gradually acquired *upwards* (towards larger and larger divergences), so long could our classification be regarded as more or less closely approaching natural. But now that we see the evolution of the plants beginning with the wider and larger divergences, and working *downwards* towards smaller and smaller, it is evident that our classification requires much revision to make it " natural ", for a large divergence may easily carry the child into another tribe, and in fact, in the early stages of a family, most often does so. Divergence is no longer due to continually increasing selective destruction of intermediate or transition forms, but is impressed upon the child at its birth. If development is downwards, the smallest genera are the youngest, and it is the oldest and largest, like *Senecio, Ranunculus,* or *Carex,* that contain the relics !

Of the 61 leading families given on p. 173, one may see that at the first mutation the first and second genera became the heads of the first two tribes in 47 cases, and not so in only 14, though even then they sometimes headed different sub-tribes. But before a family has grown to any serious size, it has already produced the heads of most of its sub-divisions, even to the second degree, and they are all evidently closely related to one another, and to the leader of all, of which the first follower certainly, and later ones with rapidly decreasing probability, are direct descendants (*cf.* also *Evol.,* App. III, p. 199).

In our list of these 61 families, it will be noticed that the larger a family is, the greater number of tribes does it seem to show, a fact which at first seems contradictory to our theories. But in a large family, as compared with a small, the leading genera are older, and will consequently have more descendants, and as all or most of these will agree in showing some of the characters of their leader, we shall thus get a group marked out by its possession of these characters, and *so numerous* that we shall give it tribal rank. This of course will not show nearly so well in smaller families, so that it is only when their leading genera show some well marked divergence, especially when that divergence has been elsewhere admitted as of tribal rank, that we shall recognise them as the heads of tribes. Thus in *Sarraceniaceae*, in spite of the very great divergences shown by *Heliamphora* as against *Sarracenia* and *Darlingtonia*, such as raceme (solitary fls.), perianth simple (double), G 3-locular (5-loc.), we do not give it any rank beyond a section of the family, numbered, not named. But if it had a number of descendants more or less closely akin to itself, it would doubtless head a sub-family or tribe. In the *Basellaceae* with five genera, the well marked sectional characters, which mark tribes in larger families like *Urticaceae* or *Chenopodiaceae*, simply divide it into two sections. It is simply because of these tendencies that the proportion of leading genera that head sub-families or tribes is greater in a large family than in a small, and that they are not so clearly marked off from one another.

For the last eighty years we have been so much in the habit of expecting any structural feature to have been developed from something that was closely like itself, allowing of transition stages towards itself, that a serious change in the viewpoint is not easily assimilated. Divergence of variation, so constantly shown, was always one of DARWIN's principal difficulties (*cf.* GUPPY in *AA*, pp. 103-5, especially the latter part about DARWIN). This divergence is not only frequent, but general, in fact one of the general laws by which evolution is working itself out in nature. It is very strikingly shown in the way in which the taxonomic divisions of families, tribes, genera, &c, are made by divergence at the earliest possible opportunities that offer themselves after the birth of the species (or genus if one prefer, for they are the same thing at the start) that was the first head of any family. Thus

for example some species of *Senecio* was probably the first head of the *Compositae*. Had all descendants been closely like their predecessors in every respect, it would not have been possible to group them by the endless variety in structure which they actually show. The supporter of selection is compelled to use that popular refuge, the destruction of the intermediate or transitional types that might have filled the gaps, whether structural or geographical. But, as we have seen, there is little evidence for this, especially now that fossils have been shown somewhat incapable of bearing the weight that has been placed upon them, and when one thinks of the almost fabulous destruction that would be required, and which has left practically no trace, fossil or other.

Destruction of intermediate or transitional forms. To go over the whole question of destruction, and of relicdom, which is involved with it, upon both of which we have written so much, would take up too much space, but it is so important in the present connection that a brief review may be permitted. In accepting divergent mutation, with development in the order family-genus-species, as opposed to the Darwinian conception, the writer took up a new position. But he felt strongly confirmed in his opinions when he found that he could make so many deductions, every one of which proved correct when tested upon the facts. It was one of these that showed, as we have seen in recent chapters, that the heads of the sub-families and other groups into which a family was divided would be the nearest possible of relatives, and not widely separated, as Darwinism, or the result of structural investigation, would make them.

Chap. XIV in *Evol.*, p. 164, gives a general discussion of the pre-war situation (the writer has since been cut off from most literature and correspondence). The weakness of the selection theory, and the many assumptions upon which it rests, and of which a list of 33 is given, were pointed out. The writer realised the illogicality of the theory in his early days in CEYLON, and from 1902 onwards continually attacked it when his work provided an opportunity. One line of attack, based upon the study of endemism, is described in *Evol.* pp. 27-32. The main point that is insisted upon is that in the flowering plants of the present day the local species or genera, and the small genera, are nearly always

young beginners, and not relics. There *are* many relics, especially within range of the ice of the glacial period, but they are few and far between compared to the great numbers of local species and small genera. The figures of genera on p. 185, *AA*, give 4853 of one species and 1632 of two, out of a grand total of 12,571 genera. Even *Ranunculus*, admittedly a very old genus, with over 300 species, has only about 25 of very large range, most of its species being much more local (*cf.* map of NEW ZEALAND on p. 65), while in smaller genera, except in water plants, whose range is usually larger with fewer species, the species of large range are still less common.

Most of *Ranunculus* consists of species of medium or small area, the last being relics upon the older conceptions, though their percentage is greater in a large genus like *Ranunculus* than in a smaller one, which latter comes nearer to one's conception of a relic. If the two genera are reasonably closely related, so as to be not unlike in their reaction to external conditions, the difference is that in the smaller there are few, if any, of the species of very large area of dispersal that occur in such a large genus as *Ranunculus*, even though rarely. This occurrence of age size and area, all connected by the laws of ASA, is fatal to the idea of *general* relicdom for species of small area. And this is further emphasised by the fact that the so-called relics do not occupy broken areas, as one might surely expect, nor are any fossils to be found (and especially so in *Araceae*), except in a few very rare cases, like *Cercidiphyllum*. Real relics are simply rare exceptions to the general rules. Many difficulties, to which no reply has been given, are pointed out for the theory of relicdom in a list of queries upon p. 90 of *AA*, and we may also refer to *AA*, pp. 58-9, 86, 88, 93, 165, 186, 199, 216, 229-34, and *Evol.*, pp. 17, 26, 30-1, 61, 79, 93, 113, 128, 132-3, 160, 173.

The matter is also discussed in a general way under the head of structural discontinuity on p. 90 above, and another general review is in Chap. XXII, *AA*, p. 228, especially from the foot of p. 231, where the arithmetical and other difficulties that we have brought into the question with the laws of ASA are considered. There is also a chapter by DE VRIES on p. 222, that is well worth consideration, especially p. 226, where he says " the conclusion obviously is, that

specific characters have evolved without any relation to their possible significance in the struggle for life. The facts are contrary to the main principle of the selection theory of DARWIN. Moreover, intermediate steps between the endemic species and their parents, in the midst of which they are ordinarily still living, are wanting, and therefore must be assumed never to have existed. Endemic species must have appeared at once, by means of one or a few distinct steps, which embrace their whole differentiation from the parent type... their origin is in full accord with the principles of the mutation theory... one of the best proofs of its applicability to evolution in general. "

DE VRIES also points out that mutation is really a support to the main evolutionary theory of DARWIN. The writer's contentions are largely aimed at getting rid of the illogical appendages, to which, on account of their popular appeal (and without which the theory of evolution might not easily have become firmly rooted), the name of " Darwinism " was given, and which, illogical though they were, have been so much invoked in an attempt to justify the breaking out of the great war.

In the theory of divergent mutation, the writer has gone beyond this standpoint, but that is the result of separate scientific discoveries, and he is also largely concerned with getting the immortal theory of evolution properly established upon a completely scientific basis. If one destroy any previously accepted belief, one should try to find something to put in its place, and for this he has adopted the theory of dichotomous divergent mutation, working downwards towards the species, not upwards as selection demands, and with survival of the parent. For this the evidence is very strong, and continually becoming stronger, and he ventures to hope that it will be found a satisfactory substitute. The way in which the " man in the street " regarded the theory of natural selection has been very well put by Mrs. ARBER (Evol., p. 6), and perhaps that individual may regard the substitution with less disfavour when he realises how well it too agrees with the ordinary observation of everyday life.

In AA, chap. XIV, p. 137, Mrs. REID considers the matter from a palaeobotanical standpoint, pointing out that fossil botanists are looking more for the exceptional cases, while

the writer is seeking the general laws that underlie them,
hence the differences between the two. There are two
lines of attack upon a biological question, and so many laws
are interacting in any case, and many of them probably
unknown to us as yet, that it is difficult or impossible to
obtain a direct and unequivocal answer. Facts are collected
from below upwards, in the endeavour to ascertain by
induction some law that has governed their appearance.
In this way the laws of ASA were discovered, but it must be
clearly understood that the finding of an exceptional case
does not necessarily disprove the law, any more than the
ascent of a balloon disproves the law of gravity. The best
proof of a law that is usually available is its use to make
predictions that can be verified, under which head may be
placed the bulk of the new facts that are brought forward
in this book, such as those given on pp. 24, 26, 40, 51, 52, 69,
70-73, 81, and so on. But there are so many exceptions that
it is always easy to bring up objections to any laws proposed.
But here the exceptions are always much less numerous
than the cases that go as the law directs, and some of the
most troublesome exceptions, such as those which fossils
were supposed to provide, have been shown to rest upon
incorrect interpretation.

Mrs. REID goes on to point out the chief and undisputed
facts of plant migration, extinct floras, &c, and on p. 141 she
says, with perfect truth, that the palaeobotanist must stand
for endemics being, in many instances, survivors from races
that once, though now no longer, flourished widely, like
Sequoia which, belonging to a very old family, is now an
undoubted relic; but it is only one among many, where it
makes no difference to the figures. The whole number of
relics forms but a small proportion to that of the local species
and the small genera that are so numerous. The great
majority of the flowering plants, and *especially the smallest
genera*, which are the youngest, as the laws of ASA show,
and as is confirmed by their great preponderance in number
at the tail ends of the hollow curves, closely follow the laws
of ASA in their development and distribution about the
world. Speculation comes in much more when we have
to deal with the older and larger genera, that have undergone
greater vicissitudes in their much greater span of generic
life, and that show much better the broken distribution

which on the whole is conspicuously missing in the young ones.

Local, or endemic, species and genera, so fiercely defended as relics, belong to the older genera and families to a very great extent indeed, and are but few in number in the small or young ones, which is not what one would expect upon any theory of relicdom, and indeed cannot be explained upon such a theory. They occur in large numbers in places where there are many widely distributed species. Cf. the Spanish and Balkan endemics on pp. 70-73.

We have pointed out in places above, that the further back that we go into the past, the more do we reach a field in which so many things may have happened, geological, climatic, and other, that the resources of several sciences have to be called upon, whilst the results must be largely a matter of speculation, which is a somewhat fascinating pursuit. Genera found fossil in the Pliocene are practically all genera of the present day, when they are of course usually large. In connection with fossils, we must also remember that comparatively local ones, as are so many of those of flowering plants, have been regarded as ancestral to existing things. The laws of ASA, however, indicate that a descendant will in general occupy less area than its ancestor, and that usually largely within the area of the latter. Fossils of small area, as so many are, are therefore probably completely extinct offshoots of the evolutionary tree, with no living descendants, while even fossils found very far back in time show little or no sign of intermediate or transition characters between one form and another, though they may show different *mixtures* (*cf.* SCOTT, p. 169) of characters — facts that are very difficult to fit into any theory of selection. Upon our theories, however, there is no special reason to expect the death of any transitions, and perhaps there never were any, or only rarely. " *Success* ", *under natural selection, means the destruction and death of the less bountifully equipped species* (nature red in tooth and claw, to use HUXLEY's phrase), *but under divergent mutation this is not necessarily so at all.* We must realise that the operation of selection, in plants at any rate, and during their first evolution, is individual, and rarely specific, if indeed it is ever so when once the species has become established upon some area other than very small. With the more careful and detailed splitting

and definition of characters that is always going on, this question of intermediates becomes steadily more important, for close *structural* relationship is continually being shown among things that have no geographical connection, even by overriding genera in their own family, and often across barriers so ancient that they must be older than the plants concerned.

Most of the facts of distribution call for a simple mechanical explanation, which has been provided by the laws of ASA. Selection is individual and does not pick out one species as against another, though it is one of the laws of life. Agricultural work also goes to show that its result is to pick out, not the best *types*, but the best *populations*, which are usually composed of a mixture of types. In the case of man, the competition between really valuable qualities is commonly soon replaced by war, which depends more upon the resources available, and can be prepared for in advance.

Relics or supposed relics should receive proper study in each individual case. What caused them to be taken up with such energy in a collective way was partly DARWIN's surrender to the incisive criticism of FLEEMING JENKIN (*Evol.* pp. 5, 13, 25, and especially 165), which compelled him to stipulate for origin of species on large areas. Examples of actual areas upon which species occur are given in *AA*, pp. 150-168, *Evol.*, pp. 24-32, 34, 50, 62, and above, pp. 66, 107, and map of *Ranunculus* at p. 65. This stipulation of course implied that plants occupying small areas had once occupied larger, and therefore must be relics. But we have shown that one of the laws of evolution is divergent mutation. This in turn means that parent and child will most often, perhaps almost always, be mutually sterile, so that even a solitary divergent child may grow into a new species, without fear of parental crossing. Both will simply follow the laws of ASA, and any competition will be as chance may direct.

One great difficulty for the theory of relicdom, which we pointed out many years ago (*AA*, *cf.* index), is the fact that in all families there are very few large genera at the top, but a crowd of small at the bottom, and those especially " ones ". To this the only answer made, but one often repeated with different illustrations, is that the curves thus made are " accidental ". If they were really so, it is very remarkable indeed that the same accident should happen in
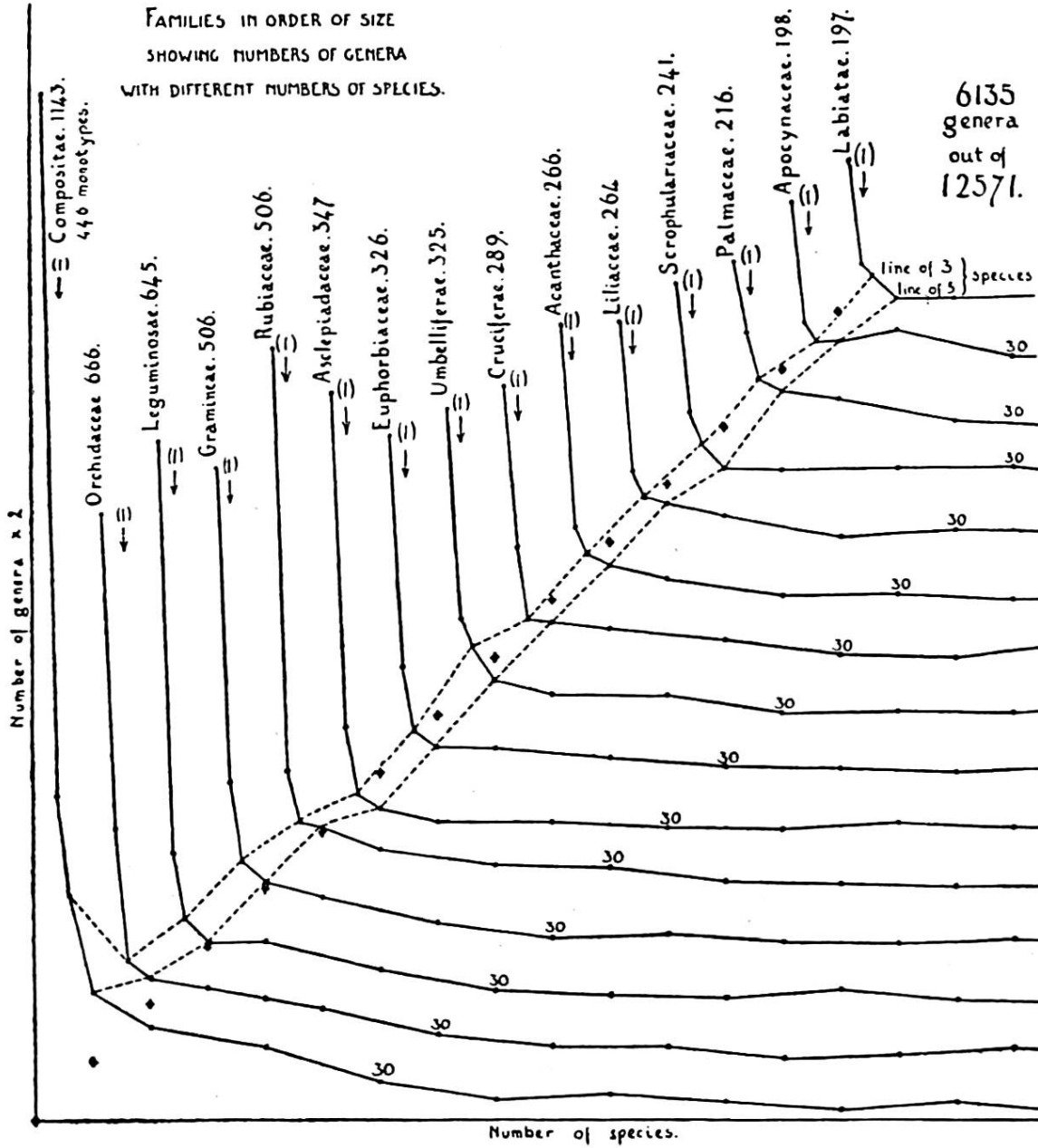
hundreds of cases, and suggests markedly inferior workman-
ship in some part of the mechanism.   It is evident that there
is some law at work, which is evidently the law that differences
occur more frequently the smaller that they are, or the younger
in the case of plants.   At the top of a family we find the
oldest genera with divergences that on the whole (as usual)
mark their descendants right down through the family,
thus dividing it into tribes, while later divergences split
these into subtribes, and so on downwards. *Divergence is
the mark of evolution.*

Another difficulty lies in the query, why should there be
so many genera just at the point of disappearance, followed
by little more than one third as many, on the average, that
are two points from disappearance, and so on upwards, in a
hollow curve giving a logarithmic straight line?

The universal hollow curve was a great blow to any
theory that small genera or local species were of relic nature;
hence the fierceness of the defence of relicdom as general,
and not as only occasional, which seems to be the real state
of affairs.   The further discovery made by YULE, that for
a family of reasonable size the hollow curve plotted as a
straight line by logarithms, showed that the growth of genera
followed the formula 1-2-4-8-&c, and did not involve the
death of the parent that was stipulated for under Darwinism,
but never proved. See pp. 260, 262 (log. curve).

There is no reason to suppose that competition is between
entire species rather than individuals.   Once the species has
established itself as a few individuals at some distance apart,
the competition continually diminishes *for the species, but
not for the individual.*   This is a principle that should be
of some importance in the work of organising the world,
once the general minimum necessities of food, clothing, and
housing have been attended to.

Proof of the theories brought forward here, and in the
two preceding books, involves the destruction of the older
theory known by the name of Darwinism, but should help
still further to establish DARWIN's immortal work upon
evolution, which will be freed of some of its encumbrances.
The writer has been able, especially by the aid of the sub-
conscious mind, to make a great many predictions founded
upon the work described in the first two volumes of this
trilogy.   He has thus been able, finding them all to be

HOLLOW CURVES FOR 15 LARGEST FAMILIES

Monospecific Genera at this end of curve

Number of Genera (or species)

Compositae

Endemics of all Islands ×½

Flora of Ceylon.

Amphipodous Crustacea ×5

Flora of Cambridgeshire.

Hymenomycetineae ×5.

Lizards ×5.

Flora of Italy

Birds of British India.

Endemics of Brazil.

Simarubaceae. ×10.

Tertiary Fossil Plants.

Mrs Reid

Endemics of New Caledonia. ×2.

British Echinoderms. ×5.

Endemic Compositae of Galapagos by area ×5.

One I

Two Is.

(5)

(3) (7)

Line of Genera of 5 spp.

1037/₋

446/₋

Chrysomelidae

215/₁

573/₋

39/₁

210/₁

23/₁

35/₁

341/₁

230/₁

334/₋

17/₁

357/₋

73/₁

MIXED CURVES

The large dots represent the Origins

Number of species (or size of area)

MIXED CURVES : PLANTS, ANIMALS

Logarithmic curves for flowering plants, for Rubiaceae,
and for Chrysomelid beetles (by courtesy of the Editor of *Nature*)

verified by the facts, to write the present connected account of the whole subject. As it all seems to hang together well, it thus makes a strong argument in favour of the line that he has adopted and followed.
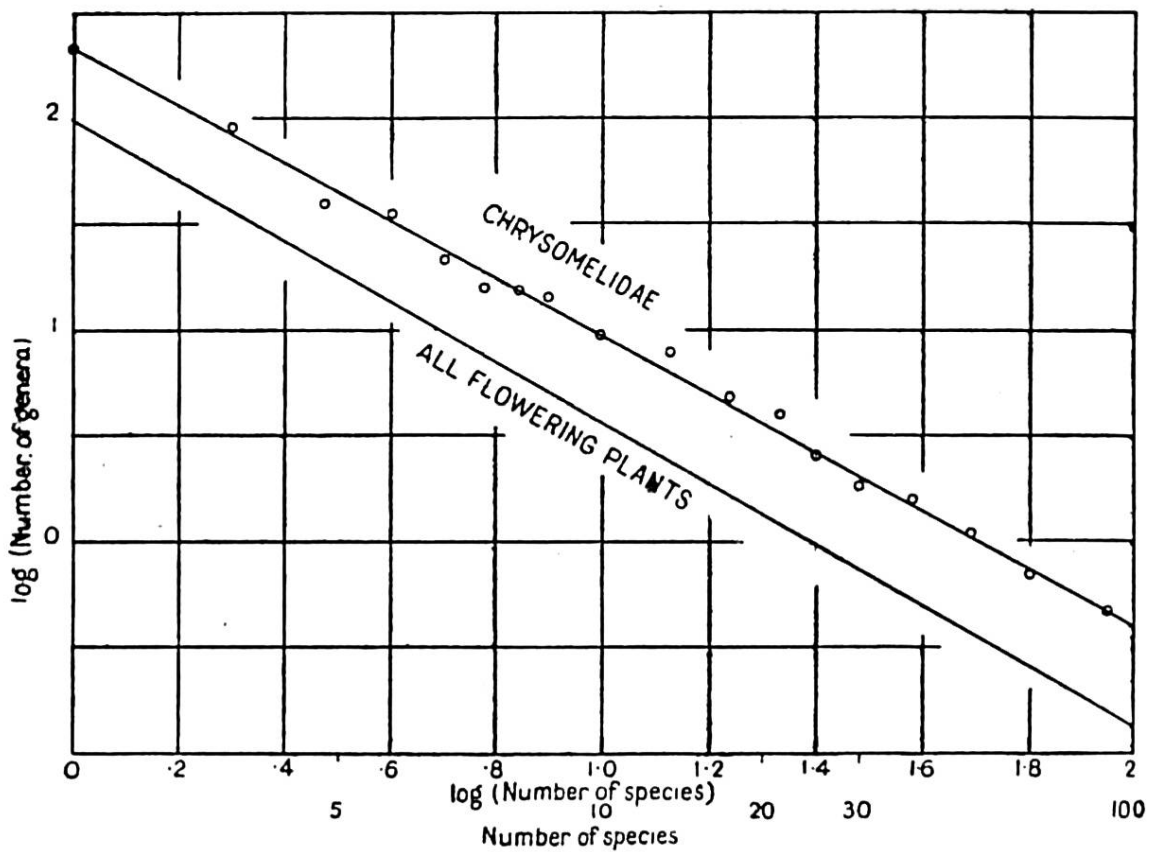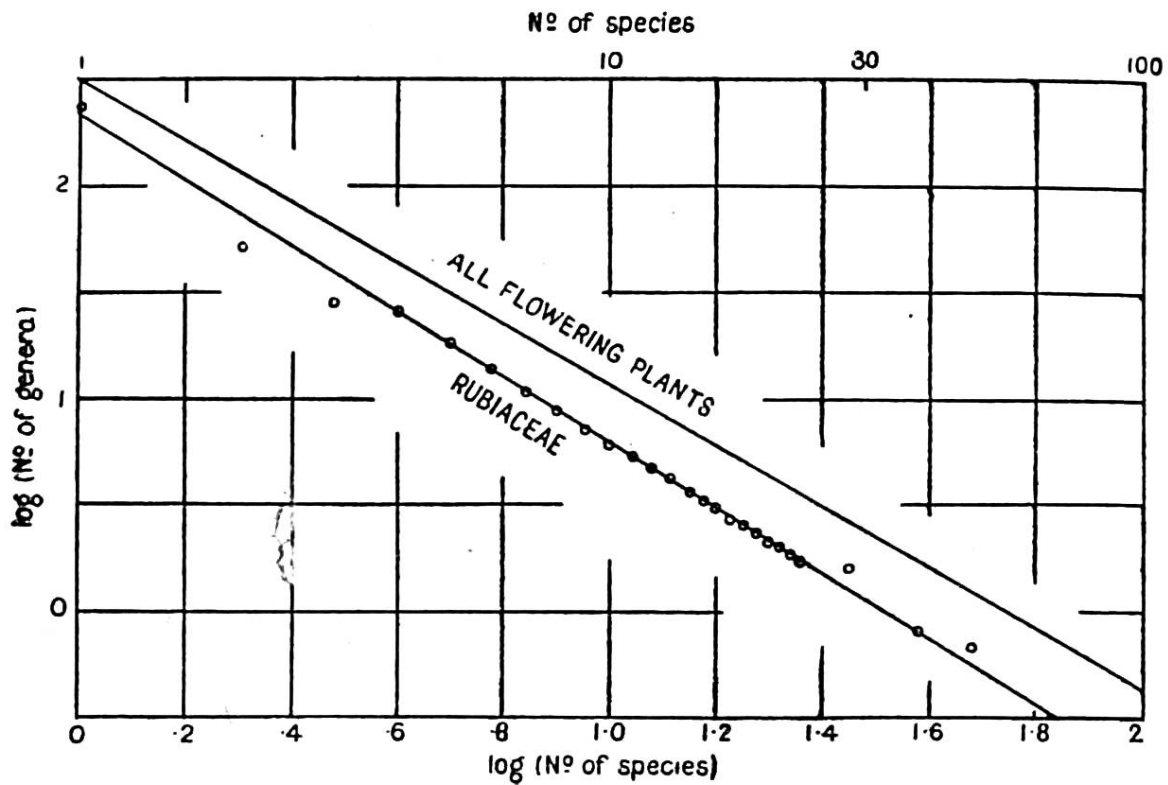
It is thus becoming clear that the great variety that we see in species was not caused by the continual acquirement of new characters in a casual way by selection, but that the new characters were due to genetic acquirement from above, according to laws as yet unknown. In fact we need some kind of extension of the laws of Mendelism to cover their application to the question of specific heredity as different from, and more comprehensive than, individual. Such a law might ordain, so to express it, that "this simple leaf must become compound at this mutation, because of (some unknown but definite) reason", and the leaf becomes compound. In this connection, the physiological principle of compensation is probably of great importance; "what is lost on the swings is made up on the roundabouts", or the reverse.

The outstanding fact, proved almost automatically by the differences that we have seen to exist between the earliest (largest) genera of a family, which show among them all the breaking up of that family into its principal sub-families, tribes, sub-tribes, large genera, sub-genera, &c, is that *divergence is the first feature to show itself in the evolution of a family*. This may be seen right down to the families of two genera only, as is clearly shown in the table on p. 199 of *Evol.* The same thing occurs within the genus, and we have seen that the species, and even the sub-species (as in *Rubus* or *Hieracium*) divide upon the same principles, the most widely distributed (the oldest) belonging in most cases to different groups (cf. pp. 182-3, and many later). What seems most to matter is the period at which any mutation appeared in a family, for the same mutation may appear at an early stage, marking the division into tribes, or only at a late one, marking division into species. But on the whole one may say, that divergence becomes less as one comes downwards towards the species, and that the divergence is of the same kind throughout, but involving more characters as one goes upward from genus to class. But there is no difference between even one class and another that could not be crossed by a single mutation. It is not improbable

that the comparative "width" of the differences may be due to the fact that the possible variety of difference is small at the beginning of life, but tends continually to increase as time goes on, for each new variation may bring other possibilities in its train. As soon as the changes had produced a real leaf for the first time, the road to all kinds of variations in form and structure and arrangement of leaves would begin to open, and so on.

The writer has observed several things that make him think that perhaps it may be possible that very small mutations may be frequent at any time or place, such mutations as will make differences too small to be noticed among the differences obviously due to fluctuating variation, but the evidence is at present so vague that he does not propose to give it, but simply to note the fact.

At the period when a new family is being divided up into sub-families, tribes, and other divisions, all its genera will obviously be quite young and very small, and at that time they will all, with equal obviousness, be as closely related as is possible. It necessarily follows that our classifications, which must, being based upon divergences, put these early genera (*at any rate when they grow large and important*) into separate groups, do not exactly follow the genetic lines of evolution. Consequently they are not " natural " in the sense in which we have hitherto used that word, taking for granted that beings that are closely allied *structurally* will necessarily also be closely allied genetically. This idea is nearest to exact truth, probably, in such things as the small sub-sub-species of *Rubus* or *Hieracium*, departing from it more and more as one goes upward to sub-genus, genus, and family, class. If a classification is to be useful in practice, therefore, it cannot be otherwise than artificial in great part, though such a family as the *Cruciferae*, clearly marked off by its tetradynamous stamens, showed clearly enough in the very artificial system designed by LINNAEUS for practical purposes, and would probably be equally well defined in any other artificial system. From the point of view of the average enquirer, therefore, it would seem better frankly to acknowledge the artificiality of our present system, and to design a simpler one for general use.

The species or genus A gives rise to B, and so on, and the question at once comes up why A should be the head of a

family and not also B in turn. It is fairly clear that B and its descendants must follow the same rules as A, which is only one generation further back. At each upward step, the divergences tend to become more clearly defined, though not necessarily larger. They are *older*. The result is that at each step upwards it tends to become more difficult to connect to the next step again, so that at last we come to B and A. We can connect B to A as its parent, but the next step above that is often very difficult to take, and so we agree to regard the group headed by A as a family. At each step upwards, too, the number of genera of sufficient size to be the parent of the one that we are considering becomes smaller. All this seems to indicate that " larger " mutations, whatever that may mean, are commoner the higher we go. But whether a mutation from Di- to Mono-cot, though rare, is really a larger one than usual, we do not know.

The first mutation in Compositae from *Senecio* probably gave rise to *Hieracium*, thus beginning the two great sub-families *Tubuli-* and *Liguli-florae*. But as yet we have no means of knowing for a certainty that all the former are descended from *Senecio*, or the latter from *Hieracium*, for the divergence between them may have been repeated, or reversed, at some other mutation. This is rendered almost a certainty by the much greater number of the *Tubuliflorae*. *Hieracium*, again, is not only the child of *Senecio*, but is the parent of the heads of the two great sub-tribes of *Liguliflorae*, the *Crepidinae* and the *Leontodontinae*, whether directly or not. The same kind of behaviour is seen all through a family, right down to the heads of the sub-genera, and even down to the heads of the sub-species of such things as *Hieracium* or *Rubus*.

This new outlook, after all, agrees better with what has been so clearly made out in the physical sciences during the last twenty years, and as biology has necessarily a physical basis, it need be no surprise to see mathematical order appear in it also. If once established, and the evidence in its favour is already very great, it may have important bearings upon the various sciences of life, such as ethics, politics, or government. Since our early publications of 43 years ago, we have now spent our leisure for 26 years, and the entire time of about 17, in collecting and marshalling facts, in making deductions by the methods described above on p. 249, and in writing them up in a trilogy of books, and in

many papers. But there remains a great assemblage of papers upon statistical facts, and other work, which should be very useful to other workers upon these lines, and which we hope to be able to put in order. The possibilities of new directions of research that are opened out by an acceptance of the views that are here being put forward are very considerable. We venture to hope that in future geographical distribution will be less despised and rejected than in the past. To it, we think, may be applied a quotation from TYNDALL, *The Glaciers of the Alps*, p. 13 of the Everyman edition. Describing a glacier, he says " At first the ice presented an appearance of utter confusion, but we soon reached a position where the mechanical conditions of the glacier revealed themselves, and where we might learn, had we not known it before, that confusion is merely the unknown intermixture of laws, and becomes order and beauty when we rise to their comprehension ".

Having now given, as usual, the necessary connection to the series of predictions and deductions from the facts, of which we are constructing this book, let us go on with the illustration of our theme by individual families, taking the *Araceae*, which incidentally shows that Monocots behave like Dicots. It has been worked up (in *PR*) by a firstrate taxonomist, my friend the late Prof. ENGLER. It shows the usual splitting at the top of the family into the heads of the subdivisions of the family, and also shows some other interesting features that require consideration. Of its eight sub-families, the last two are very small, and the last of all, *Pistioideae*, contains only the pantropical water-plant *Pistia Stratiotes*.

Any family, other than very small, is usually found to be composed of a few larger genera at the top, well separated by structural characters and by numbers of species, and a larger number of " ones " at the bottom, while between there are genera increasing in size, with less and less overlap in size the higher that one goes. As we have seen, it is the larger genera that give their characters to the sub-groups, each of these beginning with one of them. Thus any grouping necessarily tends to be more or less of a hollow curve, and it is this fact which has specially impressed many of my opponents who wish to prove that these curves are " accidental ", but neglect to explain why each of them is usually headed by

one of the large genera at the top. The more that we break up the family into smaller units of classification, the less do the divisions, though each usually begins with a large genus and ends with increasing numbers of small, approach to the true logarithmic curve, which shows well in the larger families (fig. on p. 262). Their curves are often so irregular that one is surprised when one finds them adding up to a normal curve. This irregularity fits very well with that of their geographical distribution, which is very well shown indeed in the *Araceae*. Let us therefore begin with the usual list.

The Araceae, in order of world size (PR)

1.	Anthurium	489	Heading *Pothoideae*		Trop. Am., W. I.
2.	Philodendron	222	Heading *Philodendroid.*		Trop. Am., W. I.
3.	Arisaema	101	Heading *Aroideae*		Warm As. Afr. E.N.
4.	Amorphophallus	90	Heading *Lasioideae*		Palaeotrop. [2] Am. [1]
5.	Homalomena	81	2nd	Philod.	Malaya, trop. Am. [3]
6.	Schismatoglottis	75	3rd	Philod.	Malaya
7.	Alocasia	63	Heading *Colocasioid.*		Indomal., Phils.
8.	Raphidophora	61	Heading *Monsteroideae*		Indomal., Phils.
9.	Pothos	48	2nd	Pothoid.	Indomal., Madag. [4]
10.	Aglaonema	41	4th	Philod.	E. Indomal., Phils.
11.	Cryptocoryne	38	2nd	Aroid.	Indomal., Phils.
12.	Xanthosoma	38	2nd	Colocas.	Trop. Am., W. I.
13.	Dieffenbachia	27	5th	Philod.	Trop. Am., W. I.
14.	Monstera	27	2nd	Monster.	Trop. Am., W. I.
15.	Spathiphyllum	27	3rd	Monster.	Trop. Am., W. I.[5]
16.	Typhonium	23	3rd	Aroid	Indomal., Austr. 5.
17.	Stylochiton	22	4th	Aroid.	Trop. and S. Afr.
18.	Stenospermation	21	4th	Monster.	Warm S. and C. Am·
19.	Scindapsus	20	5th	Monster.	Indomal.
20.	Caladium	16	3rd	Colocas.	Trop. S. Am., W. I.
21.	Epipremnum	16	6th	Monster.	E. Indomal., Phils.
22.	Culcasia	15	3rd	Pothoid.	Trop. Afr.

[1] Nearly all in MONSOONIA; 4 in ATL. N. AM., MEX.

[2] 34 spp. in AFRICA, mainly west and central.

[3] Six spp. (sub-genus *Curmeria*, probably an independent genus) COLOMBIA and VENEZUELA to COSTA RICA.

[4] One in MADAG.; none east of PHILS. [5] One in CELEBES and PHILS.

Cyrtosperma (11-12 spp.) has two in AMAZONAS and GUIANA, one in AFR. *Sauromatum* (4 spp.) is found from E. AS. to AFR. (*cf. PR*).

23. Syngonium	15	4th	Colocas.	W. I., warm Am.
24. Anubias	12	6th	Philod.	W. Afr.
25. Arum	12	5th	Aroid.	Medit., Europe
26. Urospatha	12	2nd	Lasioid.	Trop. Am.
27. Biarum	11 or 12	6th	Aroid.	Medit.

and 2/11, 2/10, 1/9, 4/8, 1/7, 4/6, 2/5, 7/4, 3/3, 16/2, and 40/1, of which one (*Calla*, 1 N. Temp.) is head of *Calloideae*, and another (*Pistia*, 1 pantrop., a common water plant) head of *Pistioideae*, which contains no other genus or species, while *Calloideae* has three other monotypic genera, one, *Symplocarpus*, the skunk-cabbage, showing the same distribution in E. ASIA and E. N. AM. that is seen in some *Berberidaceae*, in *Epigaea*, &c (p. 89).

Very little study is enough to show that the distribution of the genera is peculiar. Some are confined to AMERICA, some to the old world, and there is little, if any, overlap. Being at a region where the PACIFIC is 6000 miles wide, the gap is serious. In each six of which the list is composed, there are some from both sides. The family was evidently started by the largest genus, *Anthurium*, with its first offspring, *Philodendron*, in S. AMERICA, the two, as is most usual, being in separate sub-families. But it is hard to imagine *Anthurium* crossing the Pacific in time to give rise on the other side to so large a genus as *Arisaema*, especially when one remembers how this is so largely north temperate. *Pothos*, which is tropical, and smaller, and was united with *Anthurium* by LINNAEUS, might be a part of that genus, on the other hand, but there are others between. One must also explain how the great gap, now filled with salt water, came, at so early a period, to be covered with the dense tropical forest in which these genera grow.

AFRICA evidently received its *Araceae* from the Asiatic side, and not in an eastward direction from the American, as is the more usual course. The size of the genera alone shows the course, or order, of formation. The two largest American genera have 489/222, Asiatic 75/63, African 22/15, and the average sizes of all the local genera are 28, 12, 5. If the family was monophyletic, the dispersal was evidently east to west, an unusual direction that at once raises the question as to whether it is not really polyphyletic, from two sources at any rate, even if *Pothos* belongs to the American line. Let us therefore begin with tables of the sub-families :

Pothoideae arranged by size and dispersal

1. Anthurium	489	Heading *Anthurieae*	Trop. Am., W. I.
2. Pothos	48	Heading *Pothoeae*	Indomal., Madagascar (1 sp.)
3. Culcasia	15	Heading *Culcasieae*	Trop. Afr.
4. Heteropsis	6	Heading *Heteropsideae*	Trop. S. Am.
5. Anadendrum	6	2nd Pothoeae	Indomalaya
6. Acorus	2	Heading *Acoreae*	N. temp., E. As.
7. Gonatopus	2	Heading *Zamioculcasieae*	E. Africa

and 2, 1, 1, 1, all Old World.

The dispersal of 1 and 4 should especially be noted; also of 3. The parent of *Culcasia* must have belonged to another tribe, or more probably sub-family.

Philodendroideae

1. Philodendron	222	Heading *Philodendreae-Philodendrinae*	Trop. Am., W. I.
2. Homalomena	81	Heading *Philodendreae-Homalomeninae*	Malaya; trop. Am.[1]
3. Schismatoglottis	75	Heading *Philodendreae-Schism'glottidinae*	Malaya
4. Aglaonema	41	Heading *Aglaonemateae*	E. Indomalaya
5. Dieffenbachia	27	Heading *Dieffenbachieae*	Trop. Am., W. I.
6. Anubias	12	Heading *Anubiadeae*	W. trop. Afr.
7. Zantedeschia	8	Heading *Zantedeschieae*	Southern Africa
8. Piptospatha	8	2nd Schismatogl.	Mal. Penin., Borneo
9. Peltandra	2	Heading *Peltandreae*	Atl. N. Am.
10. Microcasia	2	3rd Schismatogl.	Borneo

and 1, 1 *(Schism.)* BORNEO; 1 *(Homalom.)* NEW GUINEA; 1 *(Philod.)* AMAZON valley; 1 S. NIGERIA *(Anubiadeae)*; 1 MALAYA *(Aglaonemateae)*; and 1 ZANZIBAR, MADAGASCAR (Heading *Typhonodoreae*).

The dispersal of each genus of this remarkable list should be noted, for example the contrasts between 1 and 2; 3, 4, and 5, 6; all previous genera and 9; and even the contrasts in the last few.

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[1] Includes six species of *Curmeria* in the lower ANDES from COLOMBIA and VENEZUELA to COSTA RICA, treated as a sub-genus by ENGLER, but more probably an independent genus with considerable resemblance; it is widely separated geographically, and there is no evidence of any fossils or transitions.

Lasioideae

1. Amorphophallus	92	Heading	*Amorphophalleae*	Palaeotrop.
2. Urospatha	12	Heading	*Lasieae*	C. Am., Brazil
3. Cyrtosperma	11-12	2nd	Lasieae	Trop. As.[1]
4. Dracontium	10	3rd	Lasieae	Trop. Am.
5. Cercestis	9	Heading	*Nephthytideae*	W. Afr.
6. Anchomanes	4	2nd	Amorphoph.	Trop. Afr.
7. Nephthydis	4	2nd	Nephthytid.	W. Afr.
8. Pseudodracontium	3	3rd	Amorphoph.	Siam, Indo-China
9. Echidnium	2	4th	Lasieae	Hylaea, Guiana
10. Montrichardia	2	Heading	*Montrichardieae*	Trop. Am., W. I.

and 1 BENGAL, 1. W. AFR., 1 HIMAL. ASSAM, in *Amorphoph*. 2 trop.
AS., 2 S. IND., 1 MAL. PEN. BORNEO, 1 S. BRAZ., in *Lasieae*, 1 W. AFR.
in *Nephthytideae* (a definitely West African group).

Colocasioideae

1. Alocasia	63	Heading	*Colocasieae-Alocasiinae*	E. As.
2. Xanthosoma	38	Heading	*Colocasieae-Caladiinae*	Trop. Am., W. I.
3. Caladium	16	2nd	*Colocasieae-Caladiinae*	Trop. Am., W. I.
4. Syngonium	14	Heading	*Syngonieae*	W. I., warm Am.
5. Steudnera	8	Heading	*Colocasieae-Steudnerin.*	Himal., SE. As.
6. Colocasia	7	Heading	*Colocasieae-Colocasiin.*	Trop As. Medit.
7. Schizocasia	4	2nd	Colocas.-Alocasiin.	E. Indomal.

and 2,2 trop. AS., AFR. *(Steudn.)*; 1,1, COLOMBIA, 1 MATTO GR.
(Calad.); 2 COSTA RICA, COLOMBIA, *(Syngon.)*; and *Hapaline*,
heading *Colocas-Hapalininae*, 2 BURMA, MAL. PEN.; and *Ariopsis*,
heading *Ariopsideae*, 1 ASSAM to *Travancore*. Here we have two
very small leaders, due to the need for splitting involved in the
structural divergence.

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[1] 2 spp. S. AM., 1 W. AFR. The whole list is geographically wrong.

Monsteroideae

1. Raphidophora	61	Heading *Monsterieae*	E. Indomal., Cey.
2. Monstera	27	2nd Monsterieae	Rio to Mex., W. I.
3. Spathiphyllum	27	Heading *Spathiphylleae*	Mex., trop. Am., 1 sp. in Celebes Phils.
4. Scindapsus	21	3rd Monsterieae	E. Indomalaya
5. Stenospermation	21	4th Monsterieae	Andes, Peru-Costarica
6. Epipremnum	16	5th Monsterieae	E. Indomalaya

The rest are, in *Monsterieae*, 11 trop. AM., 2 W. AFR., 1 VENEZUELA, 1 AMAZONAS, 1 MAL. PEN.; and in *Spathiphylleae*, 2 NEW GUINEA. Note the species of *Spathiphyllum* in CELEBES and PHILS.; also the contrasts of generic localities.

Aroideae

1. Arisaema	101	Heading *Areae Arisaematinae*	Warm Old World Atlantic N. Am.
2. Cryptocoryne	38	Heading *Areae Cryptocorynin.*	Indomal., Phils.
3. Typhonium	23	Heading *Areae Arinae*	Indomal., Austr.
4. Stylochiton	20	Heading *Stylochitoneae*	Trop. and S. Afr.
5. Arum	12	2nd Areae Arinae	Medit., Europe
6. Biarum	11 or 12	3rd Areae Arinae	Medit.

Here again are geographical puzzles, shelved by placing them in separate groups; let us take the detailed classification.

Tribe	Nº in *PR*. Genera		
1. Stylochitoneae	76. Stylochiton	20	Sudan to S. Afr.
2. Asterostigmateae	85. Spathicarpa	6	S. Braz., Parag. A.
	79. Taccarum	4	trop. S. Am.
	80. Asterostigma	5	Braz. Parag. Boliv.
	82. Spathantheum	2	Bolivia
	83. Gorgonidium	1	Malay Archipel.
and 1, 1, 1, 1, Cuba, Andes, Goyaz, and S. Braz. Uruguay			
3. Protareae	86. Protarum	1	Seychelles
4. Callopsideae	87. Callopsis	1	E. and S. Afr.

Tribe	Nº in *PR*. Genera	
5. Zomicarpeae	90. Zomicarpa	3 Bahia (Brazil)
and 1, 1, 1, Colombia, Hylaea, Bolivia		
	89. Xenophya	1 New Guinea
6. Areae Arinae	97. Typhonium	22 Indomalaya
	96. Theriophonum	5 Ceylon to Ganges
	98. Sauromatum	4 E. As. to Afr.
	93. Arum	12 Medit. Eur. C. As.
	100. Biarum 11 or	12 Medit.
and 4, 2, 1, Medit. to C. As.		
Arisarinae	101. Arisarum	3 Medit.
Arisaematinae	102. Arisaema	101 Monsoonia, temp. E. As., Atl. N. Am.
Pinelliinae	103. Pinellia	6 E. Asia
Ambrosiniinae	104. Ambrosinia	1 Medit.
Cryptocorynin.	106. Cryptocoryne	38 Indomalaya
	105. Lagenandra	5 Ceylon, S. India

Finally, there are two very small sub-families, *Calloideae* with four monotypic genera, *Calla*, widely spread over N. temperate regions, and found fossil, with three others in E. ASIA and N. AMERICA; and *Pistioideae*, composed only of the pantropical water-plant *Pistia Stratiotes*.

In the *Aroideae*, in spite of all the splitting, no harmony can be made between the structural and the geographical classification, so that destruction of the most inconceivable extent and selective efficiency has to be called in, while at the same time not only have the intermediates to be killed out, but the surviving genera themselves have to undergo vast destruction to reduce them to their present small size in so many cases. Look, for example, at any of the structural groups that have not been reduced to a solitary genus, and note that even then there are geographical discontinuities. For example, in the *Asterostigmateae*, we find only such small genera as 6, 4, 4, 2, and 5/1, one of these five being the very discontinuous *Gorgonidium* in NEW GUINEA, the rest of the group being American. Or again, look at the *Zomicarpeae*, with only a 3 and 4/1 (again a very great number of ones for so small a parent), three of them and those widely separated in SOUTH AMERICA, the fourth in NEW GUINEA. Again fabulous destruction is required, fully efficient, and in very small, probably young, genera. And finally the tribe *Areae* in this sub-family is split into six sub-tribes, but even then structure and geography are not well harmonised, though

the pressing problems are less immediately obvious. Even in groups reduced to one genus only, we shall see that there may be disharmony within the genus. Most of the largest group, *Arinae*, are in the Me·literranean-Central-Asiatic region, yet the leader *Typhonium* and a follower *Theriophonum* are Indomalayan, and are not followed by any ones there.

Looking in a general way at all these lists, one cannot but be struck by the peculiar disconnected distribution that they show, with many genera in AMERICA and ASIA, though with little or no real overlap, while there are fewer in AFRICA, or in the Mediterranean region. In five of the eight sub-families, the first genus is divided from the second by the widest part of the PACIFIC. In *Pothoideae* and *Philodendroideae* the leader is American, in *Lasioideae, Colocasioideae,* and *Monsteroideae* it is Asiatic, while in *Aroideae* there is discontinuity between ATLANTIC NORTH AMERICA and the other regions where they are found. If the grouping be genetic, the crossing of the PACIFIC must have been in both directions, and by smaller and smaller genera, for the pairs are 489/48, 232/81, 92/12, 63/38, and 61/27.

These phenomena evidently suggest that the family arose from at least two separate heads, or is polyphyletic. It is very difficult to conceive of *Anthurium* crossing the PACIFIC in such early days, when, as it is mainly composed of species of forest undergrowth, it must almost certainly have needed the shade of forest for the whole 6000 miles journey, and finally reaching MALAYA in time to give rise to so large a progeny, in which it does not itself appear. And the difficulty is much increased when we remember that similar relationships occur in five sub-families, with continually younger and smaller genera, to say nothing of other pairs that seem to have behaved like this also. And if one take up the idea that the family is polyphyletic, one has to explain why the same sub-family characters appear independently on both sides of the Pacific in so many cases, though it is true that the cases of a few species in *Cyrtosperma* and in *Spathiphyllum* on both sides seem to point to the possibility that even a genus may be repeated. And it is also true that the sub-family characters are largely vegetative, not involving serious floral differences, so that it is possible that similar

conditions might produce similar results here too, and we may leave it at that.

It would lead too far to go into further detail, but we have said enough to draw attention to the hopeless geographical-taxonomical incongruity, which cannot be explained upon the old idea of destruction of intermediates. The destruction must be so efficient as to leave no traces, though there is nothing even to suggest that there was ever any selection. The confusion is only increased by the splitting. To get an idea of it in another way, we have only to look at the genera that are confined to Africa :

The purely African genera of Araceae

Genus	Spp.	Sub-family	Tribe	Dispersal in Africa
1. Stylochiton	20	Aroideae	Stylochitoneae	Trop. Afr., Na.
2. Culcasia	15	Pothoideae	Culcasieae	Trop. Afr.
3. Anubias	12	Philodendr.	Anubiadeae	West Afr.
4. Cercestis	9	Lasioideae	Nephthytideae	West a. C. Afr.
5. Zantedeschia	8	Philodendr.	Zantedeschieae	Southern Afr.
6. Anchomanes	4	Lasioideae	Amorphophalleae	Trop. Afr.
7. Nephthytis	4	Lasioideae	Nephthytideae	West Afr.
8. Gonatopus	2	Pothoideae	Zamioculcasieae	East trop. Afr.
9. Afroraphido-phora	2	Monsteroid	Monstereae	West Afr.
10. Zamioculcas	1	Pothoideae	Zamioculcasieae	E. Afr. Bourb.
11. Amauriella	1	Philodendr.	Anubiadeae	Nigeria
12. Typhonodorum	1	Philodendr.	Typhonodoreae	Zanz., Mad. &c
13. Pseudohydrosme	1	Lasioideae	Amorphophalleae	West Afr.
14. Rhektophyllum	1	Lasioideae	Nephthytideae	West Afr.
15. Protarum	1	Aroideae	Protareae	Seychelles
16. Callopsis	1	Aroideae	Callopsideae	East a. S. Afr.

Thus, confined to AFRICA, we have members of

Sub-family	Tribes	
Pothoideae	Culcasieae (all)	Zamioculcasieae (all)
Philodendroideae	Anubiadeae (all)	Zantedeschieae (all)
	Typhonodoreae (all)	
Lasioideae	Amorphophalleae	Nephthytideae (all)
Monsteroideae	Monstereae	
Aroideae	Stylochitoneae (all)	Protareae (all)
	Callopsideae (all)	

But there must have been, or more probably, must be, in AFRICA some parental genera for all these, especially as all the nine that are marked (all) are purely African tribes, which would mean " large " mutations from some other groups. Only the *Amorphophalleae* and the *Monstereae* have any members outside of AFRICA. AFRICA is, as we have seen, evidently the westernmost limit of the dispersal of the « *Araceae* », and we have seen that the rule in such cases is, that the original leading genera of the family should be well represented—exactly that which is not the case here. Comparing this list with that of the leaders of the *Araceae* given on p. 267, the only outside genera represented at all are

3. Arisaema   101 spp.   Aroideae - Areae - Arisaematinae
4. Amorphophallus   92   Lasioideae - Amorphophalleae
9. Pothos   48   Pothoideae - Pothoeae

Only *Amorphophalleae*, be it noted, was represented in the list of African tribes given just above. *Arisaema* has only a couple of montane species in ABYSSINIA, *Pothos* a solitary one in Madagascar. *Amorphophallus* is the only possible outside parent for the African *Araceae*, *unless there has been vast selective destruction*, for which we have seen that little or no evidence can be produced. *Amorphophallus* is the fourth genus of *Araceae*, and the only leader really represented in AFRICA, with 34 species there, against about 82 actual local species, mostly in small areas. The tribe *Nephthytideae*, and the couple of small *Amorphophalleae*, are normal enough descendants in its own sub-family. *Aroideae*, on the other hand, though *Arisaema* is actually their head, only have a couple of species of this genus in the mountains of ABYSSINIA, which could hardly be parents to the three *purely African* tribes (one in the SEYCHELLES only) actually shown as the only representatives of *Aroideae* other than the two *Arisaemas*. In the same way, the one stray *Pothos* in MADAGASCAR could not be the parent of the two tribes of *Pothoideae* actually found in AFRICA.

The more we look into the geographical distribution, and compare it with the taxonomy, the more hopeless does the incongruity seem to become. And the old refuge resort of wholesale destruction of transitions or intermediates no

longer offers any security now that we have shown that divergence is the marked feature of evolution, and that selection hardly comes into the matter at all.

The production of one form—be it species or genus, or of tribal or family rank—from another, was long supposed to be a gradual process, the most lately born individuals being better adapted to the local conditions than their predecessors, and replacing them by virtue of that superiority. Now that we have seen that it is not gradual, but sudden, there are various modifications that have to be made in our ways of regarding the process of evolution. In the first place, its immediate direction is reversed. In place of the former idea that small varieties were the first to be formed, and that these gradually separated by the destruction of the inferior transitional forms, into species, and later into genera, &c, we now have to regard it as moving the other way.

With the gradual formation of structures in the upward direction, by which they gradually increased in complication and in efficiency, adaptation and structure went hand in hand, but now that quite important structural changes may come about at a single mutation, the two things become independent. The essential feature of evolution is now the divergent mutation by which it goes on, producing a new species or other form at one operation. Adaptation thus takes an entirely different place; anything that has not inherited sufficient adaptation to survive and to reproduce will simply die out as a result of the continual competition that is always going on; and that will be the end of it. Success will be determined by inheritance of enough adaptation, but there is no longer any necessity for actual improvement in adaptation, though any improvement that may appear as the result of any mutation, and that does not cost too much in material or otherwise, will probably be retained as a matter of course, so that slowly the general standard of efficiency may be raised, though we do not know that an improvement will survive the next mutation.

When evolution is going on under the definite influence of some outward conditions, as for example in the *Podostemaceae* it is always going on (**129,136**) under the influence of the maximum possible plagiotropism, the mutations that take place under that influence will tend to show its effects, as the *Podostemaceae* show it in increasing dorsiventrality. There

is evidently far more to work upon, and also greater variety possible, in the vegetative organs than in the floral, and the effect shows more there. As the parents will differ a little, the offspring will also tend to differ.

Under my working hypothesis (p. 96), at certain times a mutation of at least specific rank will happen in any line of descent. It will probably be conditioned more or less by some state of stress, perhaps temporarily increased by some more or less local happening. In the *Podostemaceae* we have even suggested that it may be possible that after a certain time such a mutation must take place, while the evidence of ordinary water plants, which live under very uniform conditions, without much strain, and show comparatively few species in a genus of very wide dispersal, seems to indicate that in them the rate of change is usually very slow, corresponding with the small and slow variation in conditions.

If the stress be definitely and always in a particular direction, like that in the *Podostemaceae* that urges them in the direction of dorsiventrality, or like that which in many regions that are or have been growing slowly drier urges the plants in the direction of reduction or protection of the transpiring surfaces, the mutations that subsequently occur may be in directions that give indication of the forces that have been at work. In the *Podostemaceae* one sees increasing dorsiventrality of structure, chiefly in the vegetative organs, and in the *Cactaceae* one sees increasing reduction of transpiring surface and increasing storage of water, the perfection of both of which would be theoretically reached in a spherical body, as full of water as possible, with the least permeable skin possible, a condition almost reached by some *Mammillarias*.

Thus on the whole, the outside conditions are still the determining force in evolution, but instead of working by selection of casual variations in directions that may be favourable, they work rather by actual compulsion of transitions in a favourable direction. If, as is usually the case, they are all working more or less parallel, and with more or less equal force, the mutation will not show any recognisable adaptational effect, but when one of them works much more strongly than the rest, as plagiotropism in the *Podostemaceae* is continually working with its maximum efficiency, there

will tend to be a corresponding effect shown, as that family shows a continually increasing dorsiventrality in its vegetative organs, and even at times in the flowers.

Here one must not forget, as some writers upon adaptation seem to have done, the distinction between climate and weather. In any short period the weather in A or B may vary very much between wet and dry, warm and less or more warm, and so on, but on long periods this averages out, and we may say that the rainfall averages so and so, the heat so and so, and so on, and see clearly the difference of *climate* between A and B. A plant may be adapted to the climate of A, but will need a good deal of adaptability to stand the continual variation that is going on in warmth, moisture, &c. But the same plant would not also be adapted to B, unless the difference between the two were so small that it came well within the range of immediate adaptability.

So long as the averages of the climate of a place remain constant, so long will the adaptation of any plant that is suited to it remain also constant. But the conditions are not absolutely uniform over any region of uniform climate, and selection will pick out some species as best suited to such or such an association of plants, others as best suited to some other association. But if the conditions are definitely changing in new directions, especially in one definite direction (like that of greater dryness), then the whole or most of the plants will tend to show greater or less alteration in the direction of better adaptation to those conditions. The adaptation will not be gradually picked up by selection of those best suited, but at each mutation a definite step will be made, which will sometimes, or perhaps always, show some improvement in the reactions to the local conditions.

But under ordinary mesophytic conditions, more especially, or over any short period, any mutational changes that may take place will be dictated largely by purely internal conditions in the plant itself, and from an adaptational point of view will be quite indifferent, with no effect worth mention upon the life of the species. A good illustration is the specific difference between the two common *Chrysosplenia*. The flower is much the same in both, but one has alternate, the other opposite leaves. Both live together, covering much the same areas in the north palaeotemperate region, but

alternifolium also goes beyond this into NORTH AMERICA, so was probably the parent species of the two.

In the local ecological grouping of the plants of the British or other flora into plant associations of many kinds, the plants found in one association are not plants that have mutated into new species to suit the conditions, or even simply to relieve any strain upon their make-up caused by those conditions. They are those plants that with the least strain can best live in those conditions, being picked out as such by ordinary natural selection. But if those conditions remained constant (which is exactly what they do not do) for long ages, mutations to suit them better might ultimately take place, as xerophytic structure, whether newly formed, or whether an advance upon previous similar structure, tends to appear in most young species (those confined to small areas) in SOUTH AFRICA for example (*cf. Penaeaceae*, p. 301).

Between plants that descend from different parents, the result of a change of climate may be *a general similarity of the vegetative body*, which is evidently the portion most likely to be affected by external climatic conditions. The change is unlikely to appear also in the flowers, that are much more likely to retain their general family features, which there is no reason to change, as they have nothing to do with climatic conditions, while in the vegetative organs no change that does not suit the changing conditions is likely to survive at all. In the cases where many plants, originally descended from the same ancestral genus (or closely related genera) are living together in the same conditions, the same floral structure and the same vegetative structure are likely to appear throughout that group, and to be greater than any vegetative likeness that there may be with some other group.

The taxonomic classification of the Araceae is given in the table which follows, but only sizes (taken from ENGLER's monograph), and not names, of the genera. The general tendency of taxonomy is to " split " continually, making new genera from parts of old, and so on. This makes the difficulties less conspicuous, but does not solve them. ENGLER has here carried it a long way towards its logical conclusion of a group for each genus, but not even all this splitting, into 42 groups for 109 genera, has laid to rest the geographical-structural discrepancy everywhere seen.

Subfam. I. *Pothoideae*

Tribe	Pothoeae	48, 6, 2, 1
	Heteropsideae	6
	Anthurieae	489
	Culcasieae	15
	Zamioculcasieae	2, 1
	Acoreae	2, 1

II. *Monsteroideae*

Monstereae	61, 27, 21, 20, 14, 11, 2, 1, 1, 1
Spathiphylleae	27, 2

III. *Calloideae*

Symplocarpeae	1, 1, 1
Calleae	1

IV. *Lasioideae*

Lasieae	12, 11, 10, 2, 2, 2, 1, 1
Amorphophalleae	92, 4, 3, 1, 1, 1
Nephthytideae	9, 4, 1
Montrichardieae	2

V. *Philodendroideae*

Philodendreae

Subtribe Homalomeninae	81, 1
Schismatoglotidinae	75, 8,
Philodendrinae	232,1,2 1, 1
Anubiadeae	12, 1
Aglaonemateae	41, 1
Dieffenbachieae	27
Zantedeschieae	8
Typhonodoreae	1
Peltandreae	2

VI. *Colocasioideae*

Colocasieae

Subtribe Steudnerinae	8, 2, 2
Hapalininae	2
Caladiinae	38, 16,
Colocasiinae	7, 1, 1, 1
Alocasiinae	63, 4
Syngonieae	14, 2
Ariopsideae	1

Sub-family VII. Aroideae

Tribe	Stylochitoneae	20
	Asterostigmateae	6, 4, 4, 2, 1, 1, 1, 1, 1
	Protareae	1
	Callopsideae	1
	Zomicarpeae	3, 1, 1, 1, 1
	Areae	

	Sub-tribe Arinae	22, 12, 12, 5,
	Arisarinae	3 [4, 4, 2, 1
	Arisaematinae	101
	Pinelliinae	6
	Ambrosiniinae	1
	Cryptocoryninae	38,5

| VIII. Pistioideae | | |
| | Pistieae | 1 |

The way in which the larger genera give the characters to most, if not to all, the groups is well enough shown by the fact that the average size of the leaders of all these groups is 38, and that of their next followers only 5. Direct inheritance of characteristic features of the leading genera does not seem very pronounced when one finds the first three, *Anthurium*, *Philodendron*, and *Arisaema*, each with a tribe to itself (or practically so), while 17 others also have each a tribe.

Most of these groupings, in spite of the enormous splitting, do not even yet show complete congruity between structure and geography, nor do most of them show proper arithmetical arrangement, such as should come by dichotomous growth, and such as commonly shows in most families. A few small groups, like *Zamioculcasieae* with a 2 and a 1 in the same region of EAST AFRICA, or *Anubiadeae* with a 12 and a 1 in WEST AFRICA, are passable, but most show a good deal of incongruity, in spite of the fact that 109 genera have been placed in 31 tribes, and that three of these have been divided into 14 sub-tribes, making 42 divisions in all for the 109. No amount of taxonomic splitting seems to make any difference, for it does not (cannot) follow the lines of divergent mutation upon which all families have been formed.

The genera of the Araceae. Even to leave the family for the genera does not get us out of our difficulties. We have seen that it is improbable that *Homalomena* is really

represented in AMERICA by the half dozen species of *Curme
ria* which are at a higher level, and largely on the further side
of the ANDES, in spite of the structural resemblance, and the
same may be said of the two species of *Cyrtosperma* in SOUTH
AMERICA and the one in AFRICA, and of the one very isolated
Spathiphyllum in CELEBES and the PHILIPPINES. There is
no evidence in favour of relicdom in any of these cases, other
than structural, and we have seen how large are the diver-
gences that may appear in that at a single mutation. If
the writer's suggestion of kaleidoscopic mutation, already
brought forward in several places, and discussed in the next
chapter, be adopted, and it be realised that *all* the characters
of *Araceae* are being handed down to them out of a kind
of Pandora's box carried by their ancestors, the matter is
made more simple, and one may begin to collect evidence
in its favour by inductive methods.

Even within one genus as defined by structural characters
only, one may at times find great geographical discontinuity,
for which there seems no reason whatever. And owing to
its being within the genus rather than the tribe, to imagine
that the intermediates have been completely killed out over
such enormous distances becomes even more difficult to
accept, without definite evidence, which does not seem to be
forthcoming.

We have seen, *e.g.* in *Evol.*, pp. 18, 59-60, 107, that adap-
tation must be generic to account for the wide area reached
by so many genera that are large and therefore old, when the
great bulk of their species are comparatively local, even in
such an old and " successful " genus as *Ranunculus*. Even
there only about 25 species cover very large areas, and one
cannot imagine the other 250-odd to be provided with
adaptational outfit in such a way as to make their areas form
a hollow curve. There is no evidence to show that species
compete seriously as units among themselves, as we have
seen in *Evol.*, Testcase I, p. 90, and pp. 107, 142, 144, 166,
179, &c. The competition described in (28) is quite a differ-
ent affair. It is between those portions of two already
established species which happen to find themselves suiting
the same conditions at one particular place, and resembles
that between two individuals that is always going on. Why
then should there be serious discontinuity among the members
of a genus, unless we can show that a barrier has been inter-

posed in an originally continuous area. But if we look at a monograph of any large genus, we are liable to find special structural discontinuity among species that are in close geographical relations, and great structural resemblance between species far apart (Cf. *Evol.* p. 155).

Let us look at *Cyrtosperma*, divided by ENGLER as follows :

Leaves sagittate
  1. Lasiomorpha (mainly marked by several ovules); 1, West and Central Africa; 2, New Guinea.
  2. Eu-cyrtosperma (2 ovules); 3, Polynesia, New Guinea; 4, Perak; 5, Solomon Is.?; 6, Sumatra, Java, Borneo, New Guinea, Phil. Is.
  3. Uniovulatae (1 ovule); 7, Borneo; 8, Sumatra; 9, New Guinea.
Leaves tripartite, portions pinnatisect
  4. Polytomophyllum (ovules 1-2); 10, French Guiana; 11, N. W. Central S. America (S. Gabriel).

There is a great gap between NEW GUINEA and AFRICA in § 1; and between AMERICA and ASIA between § 4 and the rest. The simplest explanation is to suppose that the characters were independently given to the sub-genera in question. In this small genus, §§ 2-3, or more than half the genus, is centred in the MALAY ARCHIPELAGO, evidently its original home. Finally § 4, of two species only, is separated from the rest by the whole width of the PACIFIC, and the ANDES, so must have arisen independently.

No reason whatever can be brought up for the discontinuity in this genus, upon the old conception that structural closeness involved geographical nearness, or destruction of the intermediate forms, and there also seems little possibility of our solution of an overriding genus (**151,** p. 165). The most probable solution seems to us to be that similar characters were being handed down on both sides of the gaps, and just happened to be combined in a fairly similar way in different places.

Something the same solution is suggested for *Spathiphyllum* which has 26 American species,.and a solitary one in CELEBES and the PHILIPPINES (implying considerable age). The species is not even given a separate group in the classification of the genus, the main characteristic being that the petiole

is vaginate to the knee, not to the middle, and the simplest explanation is again that of polyphyletic origin.

Or suppose we take a large genus like *Arisaema*, it is not possible, with the small detail available, to place the species in exact order by area as one places genera by size, but the most widely dispersed seem to belong to nine of 15 of ENGLER's divisions, which contain 92 species out of the 101 all told, so that it is clear that the earliest or oldest species show the greatest divergences, and tend to head the subdivisions of the genus, just as the oldest genera behave in a family, or the most widely dispersed sub-species of *Hieracium* or *Rubus* in the divisions of the species. Everywhere it is the same result; the earlier mutations of the leaders of families, of sub-families, of tribes, of sub-tribes, of genera, of sub-genera, of species, and of sub-species, in general show the largest divergences, and are the most numerous, or cover the largest areas. This fact, which seems ever to appear more distinctly, practically excludes the action of selection as we usually think of it, and gives little reason to suppose that advantage is seriously concerned in evolution, though of course any real advantage that does not cost too much will probably be retained, even if not inherited by the next generation.

We have called these divergences the largest, but as the same one may appear sometimes at the top, sometimes in the middle or lower, this is evidently not a correct description, and we must wait for further work to elucidate the position more completely. As the earliest in any single case, they will be likely to gain much in importance by having more descendants that are liable to exhibit them, and these descendants increasing more and more rapidly with the passage of time. One can hardly put down evolution to mere chance, when one sees how beautifully and simply all its laws work out in their operation, gradually making things more and more complex, and possibly bringing the greatest good to the greatest number, but with slow action. When we learn what these laws are, and can control them, so as to bring out the results that are desired, great changes may take place.

Most of the larger of the 15 groups of *Arisaema* show one or more species that are isolated from the rest by considerable geographical gaps, *e.g.* § 1 has an endemic species in HONG-KONG, which is usually put down as a relic, but now

that we have seen how little evidence there is for any destruction of the necessary intermediates, is much more simply explained as a probable case of polyphyletic origin. The largest group, § 5, has several widely separated species in SOUTH INDIA and CEYLON. Here probably the expansion of the large section in that direction was normal enough, and as this is a pattern of discontinuity common to many genera in different families, was probably caused by a change of climate which made the intermediate lower hills unsuitable to many things. §§ 7 and 11 show species in Atlantic NORTH AMERICA, again a common type of distribution (*cf. Epigaea* p. 89), with an explanation required that is common to all. The groups that are confined to comparatively small regions are small groups, as usual in such cases, with few species, showing smaller range, whether in structure or in geography. The regions where they are found are usually near the centre of the country occupied by the genus, where its species are most numerous. In this case, it is evidently the country of south east ASIA, from the HIMALAYA to CHINA on the one side, and to INDO-CHINA on the other.

Amorphophallus and other large genera show similar phenomena to those we have just seen, but with the total range gradually contracting as the species become less numerous (law of size and space). There are eleven sections, and as usual the structural arrangement mixes up species that are widely separated geographically, without always an overrider. Thus 17, 18 are in the MALAY islands, 19-23 in tropical AFRICA, 23-9 in SE. ASIA, 30 on the GOLD COAST, and so on.

Wherever one looks, one finds this great difficulty of taxonomic-geographic incongruity. It is manifested in a simple way in the great variety of taxonomic relationships that show everywhere, for example in the flora of BRITAIN, where so great a proportion of the taxonomic groupings are to be found, even in quite small families, and one finds the same kind of thing even in genera and species, and we have also seen that it is due to the early breaking up of the leaders by divergent mutation, and is quite independent of selection, adaptation, or relicdom.

If then we are searching, as we always profess that we are, for genetic relationships, it is clear that they are not to be found simply from the taxonomic facts (mainly of structure),

or the geographical facts, but that these must be combined with genuinely genetic investigations, and the combined results used. We have no right to call upon selective destruction in the light-hearted way in which we at present do, to explain the structural relationship between' species 1 of *Amorphophallus* in FORMOSA, and species 2 in SIERRA LEONE, for example. There is no evidence for such colossal selective destruction. Now that we have shown that structural divergence is a characteristic feature of evolution, the old explanation, it seems to us, is no longer valid. The destruction that has so often occurred was usually not selective destruction at all, but indiscriminate, due to changes of conditions that were so rapid and so complete that they killed off many species together, before these could acclimatise themselves to the new conditions. If one add together all the localities of any one genus, for example those of *Amorphophallus*, one will often find, as we saw in *Rhamnus* (p. 107) all the geographical regions covered whose conditions are suitable, while the gaps are reduced mainly to those which are common to many species that have all been exterminated by the same cause, such as the incoming of the sea, or a great change of conditions. There is no evidence for *selective* destruction upon the scale so often demanded. And now that we know what great changes a single mutation can bring about, there is no longer any need to call it in, nor would it be reliable ·as a proof were it feasible. We no longer require evidence of gradual intermediates between extremes.

It is very clear that, as we indicated in Chap. IV p. 89, we must draw a very distinct line between what we there distinguished as real, and structural, discontinuity. The former is due to the interposition of some serious barrier in what was once a continuous area of distribution. Upon the country occupied by this barrier, be it a stretch of sea, a mountain chain, a desert, or something else, the conditions would ultimately no longer allow any species of the previously present genera to exist, unless perhaps a few survivors, or some local endemics better suited to the local conditions, that might occur, especially at the higher levels. The discontinuity thus affects a considerable number of genera *alike*, as we may see in the great number common to both old and new worlds, in all of which a gap has been made by

the ocean; or again, in the plants on both sides of a mountain chain or a desert, or those common to EAST ASIA and EAST NORTH AMERICA (p. 89).

The ease with which this explanation gets over the difficulty of real discontinuity, which usually involves large genera, has led to its almost universal employment to explain structural discontinuity, which is more commonly shown by small genera, and is shown in almost every conceivable direction, the different directions rarely coinciding. The result has been rather to bring the explanation into disrepute. The structural likenesses between species or genera that are far apart are much more probably due to polyphyletic appearances of the character that is in question as showing structural affinity.

Suggested origin of Araceae. In making suggestions like those that follow, for as yet there is little to go upon, the author fully realises that he is going beyond the present bounds of inductive reasoning and knowledge, into the land of speculation, hitherto the great hunting ground of many would-be students of distribution. But he wishes to show that with the now reduced value and importance of mere structural resemblance, other speculative possibilities are open, that are just as probable as those put forward in the past. The work described in this book opens up new directions in which direct inductive work may be done, and new directions in which such subjects as genetics may be brought into play in the study of the problems of distribution, which in their earlier stages we have now seen to be governed by simple and definite laws.

The two questions that mainly come up in the present connection are (1) if the *Araceae* are polyphyletic, what was, or what were, the American and the Asiatic ancestors, to say nothing of AFRICA for the present; and (2) what determines the productions of the similar results that may frequently be observed under the influence of similar conditions, results which we often call adaptation to those conditions, and have often put down to simple selection of casual alterations in the direction of greater efficiency?

The first question at once splits into two : was the ancestor a member of the *Araceae* at all, or was it (as in any case the original ancestor of *Anthurium* must have been) of some

other family, and if so, what? There is little evidence of direct genetic relationship between the American and the Asiatic *Araceae*, unless perhaps *Anthurium* and *Pothos* are parent and child. But there are larger genera than *Pothos* in ASIA, and to have the relationships that are hinted at by the sizes of genera would require a return journey to AMERICA by younger genera. With no evidence for any transitions or intermediate links, between the American and the Asiatic genera, it is going beyond the bounds of reasonable speculation or probability to drown the supposed transitions in five different cases, in anything up to 12,000 feet of water; whilst to join these genera by way of AFRICA is even more impossible. It would seem not improbable that the American genera on the one side and the Asiatic on the other were independent descendants of some genus or genera that did not belong to the *Araceae*. What genus best fulfils the necessary requirements has then to be found. ENGLER says that *Pothoideae* are evidently the oldest group, and are only distinguished from *Liliaceae* by the fleshy outer integument of the seed. But there is no genus in *Liliaceae* as large as *Anthurium*, nor do they affect similar habit of life. In general, it must be a larger genus than *Anthurium*, at least on the American side, for we have seen that destruction, hitherto so much and so lightheartedly invoked, is a broken reed upon which to lean. In the Monocots themselves, the only genera that seem large enough, and widely dispersed enough in more or less similar conditions to have been in both western and eastern tropics in time to be the ancestral genus in both, are perhaps *Carex* and *Dioscorea*, while in the Dicots there are rather more, especially *Begonia* (800 spp.), *Miconia* (600), and perhaps the most probable of all, *Piper* (750) with its follower *Peperomia* (500). *Miconia* is confined to AMERICA, and does not grow in quite the same conditions, nor does *Carex*. The choice perhaps lies between *Dioscorea*, *Begonia*, and *Piper*, with the probabilities in favour of the last, though it involves a mutation from Dicot to Monocot. It is noteworthy that in the families of these three, there is a distribution of sizes not unlike that which we have seen in *Senecioneae* (p. 177) and in *Siparuneae*, with a great gap below the leader :

Dioscoreaceae : Dioscorea (600), and 20, 5, 4, 3, 5/1
Begoniaceae : Begonia (800), and 10, 3, 2, 1
Piperaceae : Piper (750), Peperomia (500), and 8, 6, 5, 2/2, 2/1.

One can imagine that this gap is possibly filled in reality by members of some other family altogether, and now that we have seen the divergent way in which evolution works, it is not completely improbable that some of the missing genera were *Araceae* in two or more distinct regions. It is at least as probable an explanation as the old one, and shows the way to investigation that might produce interesting and perhaps unexpected results. If *Piperaceae* should prove to be the ancestral family, it will be a step on the way to proving the frequently suspected polyphyletic origin of Monocots.

Similar conditions, similar results. In the *Podostemaceae*, we have seen similar conditions producing similar results, but not identical, in different regions of the world. When once the family started to live always upon naked rock in flowing water, it was committed to very definite and strongly marked conditions. The plants were usually forced to lie down, and the rock prevented the roots from taking their normal downward course, so that the plants came under the maximum possible influence of plagiotropism, from which there was no escape. The most widely dispersed genus, *Podostemon*, shows comparatively little dorsiventrality, but the younger and more localised genera tend to show more and more of it. As this is a family which by its plasticity lends itself rather well to experimental work, it may be worth noting that in the opinion of the writer the best taxonomic work that has been done is, by much, that of TULASNE among the general workers. Most writers have used only herbarium material, where the peculiar mode of life makes it impossible to get proper specimens, even if the rock, to which the *Podostemaceae* cling like limpets when alive, though the free parts fall away when dried, be removed also, and so have much confused species with one another. Even TRIMEN's descriptions of the CEYLON species, though they are to be found in the river a mile from the herbarium, were done from herbarium specimens, and he has drawn a pencil through the leaves in a (correct) drawing by the PERADENIYA draughtsman, W. DE ALWIS, with a note " obviously algae ".

I can remember finding several species mixed up on a sheet
in the great herbarium at KEW. Examination of the living
plants *in situ* is required for really satisfactory results. Just
before leaving RIO we found that seed could be cultivated in
the little mountain streams in the gardens.

The effects of this strenuous urge towards dorsiventrality
working upon the plasticity of the material due to its sub-
aqueous life, have been to produce a general likeness through-
out, though one must point out at once that the changes
that have gone on to produce that likeness are changes in
the vegetative organs; the flowers were much alike to start
with, and have remained so. It is of special interest to note
that there is a marked difference between the *Podostemaceae*
of AMERICA and those of ASIA, so much so that one can usually
say offhand from which continent a specimen comes. Those
of AFRICA again are sometimes different from either. In
AMERICA the dorsiventrality shows chiefly, but not exclusively,
in the production of large leaves, while in ASIA a thallus is
more usually produced, commonly a flattening and virescence
of adventitious roots. Differences between parents result
in differences between offspring, as one would expect. But
the differences show mainly in the vegetative organs, though
there is a marked difference in the flower of *Podostemaceae*
and that of *Tristichaceae*, the former being markedly dorsi-
ventral, though it stands very erect; this dorsiventrality
becomes more and more marked, up to its extreme in the
very local *Farmeria* of CEYLON and S. INDIA (**129**). Illus-
trations of the amazing variety shown by the vegetative
organs in this family will be found in (**148**).

Another example of this production of similar results by
similar causes is shown by those plants which have gradually
become subject during their dispersal to drier and drier
conditions, till at length they have been forced, in their
mutations, to adopt storage of water, and we see a general
resemblance in such things as *Cactaceae*, S. African *Euphor-
bias*, and other xerophytes, including the bulbs of tropo-
phytic bulbous plants, of epiphytic orchids, &c. In these
the ultimate tendency seems to be toward the spherical form,
which is the most economical of all. In other places the
tendency has been, probably owing to some peculiarity of
the parents, to a production of phylloclades or of phyllodes;
in others again to the reduction of the transpiring surface to a

minimum by the production of small leaves, twiggy green shoots, and so on.

A troublesome and at present inexplicable question is why there are so many morphological likenesses, in both vegetative and floral organs, between *Araceae* on both sides of the PACIFIC, for the sub-families, as we have seen, seem almost to make a point of appearing upon both sides, though we have seen what difficulties there are in the way. But if their ancestry was from an overriding genus, *Araceae* or not, there is a very fair chance that the similarity is due to that fact, for after all it only means going one generation farther back.

Whether similar conditions would produce likeness in floral organs that were unlike to start with is rather doubtful, but we have little or nothing to go upon at present, for want of proper inductive investigation. The question really is, whether *Araceae*, and especially their sub-families and tribes, could arise independently on both sides of the PACIFIC. If the ancestor belonged to the same genus, it might quite well be possible for the family itself, but if this were not so, would be more unlikely. The question of the sub-families is a more difficult one, but it is worth notice that non-floral characters are a good deal used in their determination, for example presence or absence of latex, parallel or net veining of the leaves, tuberous, climbing, or other stems, &c. The whole question must evidently be shelved until more inductive work has been done, but has been brought up here in order to show that all possibilities of speculation are not yet used up, but that as reasonable an explanation of the facts is still possible as any previous one, and one which suggests feasible inductive work for its solution.

It is clear how in many cases, and especially in the *Araceae*, polyphyly may supply a solution to various problems that confront us. If characters are always, as we know to be the case with most, handed down in a complete condition from an ancestor, *whether that ancestor showed the characters or not*, it will explain many problems that have hitherto been somewhat puzzling, for example the problem of complexes. We have suggested above that the reason that they appear so much more in large genera, and therefore especially in the leaders of large families, is because only these have existed long enough to allow single characters of other genera of the

family, and perhaps even small groups of characters, to have appeared in them, and to have been transmitted to so many descendants that they became " important ". If in a genus of say 500 species, some 50 were to show some character that was considered to be a marked character of some other genus in the family, there would be disputes as to whether this group of 50 should be kept among the 500, transferred to the other genus that showed it, or even made into a special genus with its own particular combination of characters. It is not difficult to find examples of the head of a family showing one or more of the special characters of other genera in the family, and we may instance *Hibiscus*, the head of the *Malvaceae*, where it occurs.

Conclusions. The *Araceae*, and the same is true of other families when studied in the light of the laws of ASA, of divergent dichotomous mutation, and of other principles that we have indicated, are anomalous in several respects, bringing up various problems difficult of solution. We have seen that they look normal enough at first glance, but really form a rather marked exception to the rule of monophyletic families, and in dealing with them I have in one place departed from my rule of induction-deduction, with definite purpose. To explain the distribution upon the old lines would require fabulous destruction of intermediates, in all directions, at all possible times, and disregarding such obstacles as the Pacific at its widest.

The whole evolution, if monophyletic, seems so incredible that I have suggested that *Araceae* are at least di-phyletic, with one parent for AMERICA, one for ASIA, and perhaps even a third for AFRICA. These might belong to some overriding genus which would give a similar stamp to its offspring at great distances apart, and while if there were a more suitable Liliaceous genus, that would be the most likely, I have suggested *Piper* as a possible suitable candidate for the post, as it is apparently older, is more widely distributed, has many points of resemblance, and occupies somewhat similar localities to *Anthurium*. Mutation to monocot structure is probably simple, and the difficulty is largely to explain why the same mutation occurred at such widely separated places. No reason can be suggested till we know something about the laws of incidence of character, of which

at present we are ignorant. An interesting point turns up with regard to *Piper* itself in the fact that there is a great gap (in sizes) between *Piper* (and its follower *Peperomia*) before one comes to the few and small other followers, just as we saw in *Senecio* and *Siparuna*.

There is a great improbability of immediate genetic connection in the family, though the various groups tend very much to appear on both sides of the PACIFIC, to cross which would probably require that the whole route be covered with heavy tropical forest at a very early period. It is much more probable that there has been much polyphyly involved, and that similar conditions, probably acting on things that were not too distantly related and that were living under similar conditions, have produced similar results.

Polyphyly, which the writer has specially brought forward, is a great help towards the solution of many puzzling problems, such as the instances of character-discontinuity that we put forward in Chap. IV, and which find a good illustration in the *Araceae*. The reversal of the immediate direction of evolution to family-genus-species, for which we have given so many and so conclusive proofs, both in *Evol.* and above, involves an appreciable change of outlook in work upon many problems of biology.

Admittedly the whole problem is one of great complication and difficulty, but if any other theory than that which has so long held the field, and which has been shown to be so improbable and unsatisfactory, can be brought up, it deserves at least a proper trial, with inductive study of its premises. The writer is unfortunately now too old for this work, and has been reduced to bringing up a new theory, which seems at least as probable as the old, and one more easily tested by inductive work.

The new views bring simplification into the whole question, and bring evolution into line with the other sciences that are being placed upon a mathematical basis. To suppose that nature advances simply by a casual method of trial and error does not give her credit for the exactness of method that she is now being shown to possess in the physical sciences. Whether any mutation represents an advance is open to argument, but sometimes it must be so, and there will consequently be improvement in the long run, slow though it may be. At present we do not even know that an

advantage may not again disappear in the same way as it came, by simple mutation.

It is more probable than any other explanation that all the characters shown by all the genera were handed down by ancestors in a dominant *or recessive* condition, and that their appearances were frequently polyphyletic, so that a character might often be shown that was not present at all in the immediate ancestor, and the latter might even have belonged to some other family. Such ideas will no doubt be unwelcome to many, but we have shown the great weakness of the old position and the probabilities in favour of the new, probabilities which seem to be converging with others in other branches of biology. We have also shown the great likelihood that our present system of classification is far from being genetic — as indeed is now being taught at GENEVA and probably other Universities, on other grounds than those that we have here brought up.

It is clear that the species in regard to the areas that they occupy are governed by the rules of ASA, like the genera in their relative sizes. The whole scheme of evolution, as it is being developed here, has followed by deductions from the original discovery of age-area. It is thus assuming a much greater likeness to the growth, development, and dispersal of a single human family. There too, however well we may know the characters, featural, mental, or other, we cannot predict what the offspring of any marriage will be like. Both in animals and in plants, it would seem as if GALTON's law, that about half the characters come from the two parents, a quarter from the grandparents, and so on, seems to be operative, but we must fit this into the law of divergent mutation in some way. It is not in the least clear what determines, at any birth, which if any characters shall be changed; probably there is some law that connects the two, for all the characters seem to exist as potentialities among the ancestors. One is familiar with the popular commentaries upon offspring, which in general only apply for one or two generations back; "he has got his mother's eyes", "she's the living image of her aunt as she was at her age", "he is taking to his grand-uncle's line of work", and so on, remarks for which there is usually much justification. The writer has taken, quite independently, to the lines of his grand-uncle, WILLIAM SWAINSON, F. R. S., an authority

upon geographical distribution a hundred years ago, and a great traveller, whose father was one of the seven original founders of the Linnean Society (*cf.* GAGE'S history) ; a niece is just like her aunt was about the time of our marriage; his grandson is very like himself in feature, of course at the same age. Such remarks are very frequent in taxonomic works; we may refer to such a family as *Annonaceae* in (BH), where many genera are described as having the stamens of some other, the carpels of some other again, and so on.

This work thus opens up new avenues for speculation, and provided that these lead to inductive work, this is to the good as it may lead to progress. The most important feature about the writer's own inductive work seems to be that, as a reviewer of *Evol.* said : the confusing mass of facts making up plant geography begins to make sense... If mutation does proceed in some uniform and regular manner, Nature through evolution is unfolding as some vast stream of change more challenging to the imagination than the random variations of DARWIN.