
A table of contents for *Bibliotheca Sacra* can be found here:

https://biblicalstudies.org.uk/articles_bib-sacra_01.php

ARTICLE III.

THE PRACTICAL DETERMINATION OF SPECIES.[1]

BY THE LATE PROFESSOR LEONARD MARSH, M.D., OF THE UNIVERSITY OF VERMONT.

Is organic nature one same continuous series of homogeneous forms passing gradually into each other without difference of kind, so that all classification must necessarily be arbitrary and artificial? Or do these living organisms exist in groups, each in some of its attributes essentially other than all the others, distinctly limited and circumscribed by the hand of Nature herself? In the latter case a natural classification is possible if we can discover these essential differences and the limits which Nature has assigned to each group. The existence of such differences would seem to be obvious enough: as for instance between forms of the vegetable and those of the animal kingdom. No one could mistake an oak for an elephant, or a turtle for a cabbage; yet where the two organic kingdoms approach nearest to each other we not only cannot practically separate the organisms which belong to the one of these kingdoms from those which belong to the other, but we do not know in what their essential difference consists.

The very terms we use — animal kingdom, vegetable kingdom; the names of things — tree, grass, beech, pine; horse, bird, fish, etc., show that we do instinctively classify, separate, or endeavor to separate, into groups the multiform bodies of the organic world. Yet the groups indicated by such names are perhaps as often artificial as natural. The

[1] This Essay was prepared for, and read before, the Phi Beta Kappa Society of the University of Vermont at one of its monthly meetings in 1860. Although of necessity the paper takes no account of the discussions of the last twenty years, it has yet seemed of sufficient value to justify its publication, even after this delay. The Essay is now edited by Prof. J. E. Goodrich.

terms "artificial" and "natural" as used in natural history are extremely vague. It is obvious enough that "natural" means according to the plan of nature, and that "artificial" implies human origin. But what is according to the plan of nature, and what is artificial, is often mere matter of opinion. Artificial classification may be only in reference to some practical convenience or utility, without aiming at scientific precision. Or if it aims at scientific ends it fails to give definite and complete circumscription to its groups, or associates their elements in reference to comparatively unimportant common characters, while those in which their essential resemblance consists are separated. If for example we divide plants into trees, shrubs, and herbs, which is a convenient arrangement for some purposes, definite natural limitation is impossible; nature has made no limits; so that we cannot tell, except by an arbitrary line, where the trees end and the shrubs begin, or where the shrubs end and the herbs begin. Or we may associate among flowering plants all those which have opposite leaves and all those whose leaves are alternate. The limitation is definite, but the common characters are wholly unimportant. In all attempted natural systems of classification many, not to say most, of their groups fail of being natural through one or the other of these faults, so much so that Nature is said to laugh at our classifications, and that some distinguished naturalists doubt the existence in either of the organic kingdoms of more than a very few primordially and permanently different groups of organisms. Yet all men instinctively believe — until they sophisticate themselves out of that faith — in the existence of very many such naturally limited groups, even where they find it impossible to define them. Most minds refuse to conceive of organic nature as a homogeneous whole, or as chaotic, without plan or method or order, having no predetermined relations of its parts, but only such as accident assigns to them. But the attempt to find the plan, to discover the predetermined relations, may well test the faith in their existence of those who assume that no problem is too hard for their skill.

Such plan and relations of course imply arrangement and something to be arranged. The first thing, therefore, necessary for him who attempts [to find and set forth] this plan of nature is to decompose the organic whole into its constituent elements; to separate the essentially different; to associate the essentially like, so as to determine the number, the limits, and the dimensions of the various groups of organisms which nature intended should be distinct; and then to assign, if he can, as nature has assigned them, the relations of each of these groups to all the rest; to find the purpose, meaning, function of each in the one grand vital organism. And this implies, further, a knowledge of the relations and mutual adaptations of the organic and inorganic.

The essential differences and resemblances in natural organic groups doubtless lie ultimately in those organific powers or potencies of which the organisms were but the inadequate phenomena or embodiments. But as we cannot know these powers directly we are compelled to determine them as well as we can by their phenomenal attributes, which are often but their distorted and ever-changing shadows.

Here, then, is a true difficulty, and one in many cases hitherto insuperable, — that, while we have no doubt of the essential difference of certain groups sufficiently distinct at some points, yet practically we can find no interval between them; and, moreover, we have no *principle* of discrimination under the guidance of which their limits might ultimately be discovered. But fortunately all organic phenomena are not always inconstant, and we may be said to have found a truly natural group if it includes all the organisms which possess in common and *exclusively* certain constant attributes essential or important to the ends of such organisms; provided always that such group is not inconsistent with any other natural groups. It is not, however, necessary for the purposes of science, though it may sometimes be for those of utility, that our definitions should be such as to enable us practically and actually to separate at once from all others, and place over by themselves, *all* the individual members of a natural

group. But it is necessary, at least for purposes of classification, that they should indicate a practical method, and have in them a principle of differentiation, in obedience to which the exact limits of groups, if not at once found, may be constantly approached and be theoretically determinable. A mere subjective limitation, though true, is not to the purpose; as in Schleiden's definition of species[1]: that "all individuals which independently of time and place exhibit identical characters under identical conditions belong to one species." This is probably true if he means all their characters, since things which are equal to one another are equal to the same thing. But what is that which is always and everywhere the same? We do not know the exact limits of either of the two grand groups of organic nature, but if we knew certainly what is probably true, that every animal without exception has a nervous system in some form, and that this organ is absent in all vegetables, we might consider both these groups as practically limited, the one positively and the other negatively, and in a way satisfactory to science, though anatomy and the microscope should not be able to tell us in regard to many individual organisms whether they are animal or vegetable. But if it is said that the difference is in that which is always and everywhere present in one and not in the other, we are not thereby made the wiser. Or if we are told that the two kingdoms may be distinguished by the fact that all the individuals of each are more like each other than they are like those of the other kingdom, the fact may be so, and it would enable us to separate trees from quadrupeds; but when we come near the limits of the groups it would be found a wholly indefinite definition.[2] By this process we

[1] "Zu *einer* Art gehören alle Individuen, die abgesehen von Ort und Zeit, unter völlich gleichen Verhältnissen, auch völlich gleiche Merkmahlen zeigen."—Schleiden, Grundzüge der wissenschaftlichen Botanik, 1850, Vol. ii. p. 516 (as quoted by Alph. DeCandolle, Géographie Botanique, Tome ii. p. 1073).

[2] If we say that all points of resemblance and difference, anatomical and physiological, as well as those of relation to physical nature, are to be compared, it is implied either that these are constant quantities or that we know all the limits of their variability. But the very reason for comparing *all* was that each is unreliable; and before we can know the limits of the variability of each, each must be already known.

could never get beyond opinion; it has no scientific *method*. A truly natural group, then, must associate organisms having exclusively characters — one or more — common, constant, important; must not bring together individuals separated in higher natural groups; and its definition must contain a principle of exact limitation, not subjective merely, but expressed in phenomenal and objective attributes.

If now we look over the so-called natural systems of classification in organic science, we shall find a great many undoubtedly natural groups; but in regard to definitions we shall find them of several different kinds: groups whose definition gives exact limits by which they can be easily separated from all others; groups whose definition has a principle of exact limitation, but in which there is more or less difficulty in finding the limit; those in which the method of limitation can give only probable results; and those where the definition is still altogether conjectural.

Of the well defined and completely limited natural groups the number in both the organic kingdoms is comparatively small. Of the great number of imperfectly limited, ill defined, undefined, and unnatural groups, and indeed of the absence of a true method of grouping, we may be convinced by looking over the vast variety of systems of classification, each claiming to be a "natural system." It is an encouraging fact that a few large groups are recognized as natural in all of them, from whatever point the distribution is attempted; but the great want of true *principles* of limitation is shown by the fact that in the classification of animals, for instance, the divisions above the rank of classes vary from zero to seven or eight; classes vary from four to twenty-eight; orders from about thirty to more than a hundred; in regard to genera there is still wider inequality of numbers. In regard to most of these groups, whether they are natural, what is their number and exact limits, the disagreement, discussion, and contention among naturalists are proof sufficient that the true self-asserting method and system of nature have not yet been discovered.

But of all the groups natural or artificial of the organic world no one, nor all the rest together, have given naturalists so much trouble as *species*. This is the especial opprobrium of natural history. The answers are still to seek to the ever-recurring questions: What is species? What are the attributes of species? Is there any such natural group as species? Are species autochthonous or created? Created at one period or at successive periods? Are species primordial or derived? Have their individuals any genetic relations to each other? And if so, have they descended from single or from multiple aboriginal ancestors? Are species constant or variant? Do they vary within limits or without limit? Are they permanent or temporary? Do they remain distinct and separate, or do they combine with each other and amalgamate? Can species be limited by characters common to them all, or only, like other groups, by what is peculiar to each? What is the definition of species? Naturalists differ as much in practical classification of species as they do in opinion in regard to them. Organisms which one naturalist holds to be all of a single species another divides into ten. Where one makes a whole cluster of species forming a distinct genus, another declares that there is but a single species. Sometimes where the systematists have made three or four genera, each with its attendant species, Nature gives intimation that in her opinion all and each of the organisms so methodically arranged into different species and genera are of one and the same species.

It is manifest from such opinions and such practice not only that this class of groups as a class is not well defined and actually circumscribed in the organic kingdoms, but that naturalists have not yet any reliable scientific principle of limitation for these groups, under the guidance of which they might constantly and confidently approach their limits. It is plain that we have not yet the law of species with its phenomenal limits; that the subject is not yet in the realm of science, but remains in the region of opinions.

The wide practical disagreement of naturalists is perhaps

explainable from the character of the so-called definitions of species. This may appear from an examination — which shall be the briefest possible — of a few of them. Many naturalists, and those not the least distinguished, cut the knot by asserting that there is no limit; that not only species but all other groups pass insensibly into each other; that they are distinguishable at their extremes of variation, but [gradually approach each other as they recede from these extremes, and finally become wholly indistinguishable.] Whence it follows that the whole animal kingdom is only one widely variant group, and the vegetable kingdom another, and indeed that the whole organic world is but a single indivisible group. This class of naturalists assert that their groups are natural, though they do not pretend to give their limits or the method of finding them, the exact limits being of no consequence. But science is not so satisfied, as all naturalists imply by their constant search for these limits. And if there is any such natural group as species, for instance, and all organisms are separated by nature into groups of species — whether separable [practically] or not — how then do these naturalists prove that their groups of species are natural while they disagree so widely in the number of them? for in nature the number must be definite. Plainly they have divided one species into several, or united several into one. The method of no definition, then, is very indefinite.[1]

Of the numerous attempted definitions of species some are wholly subjective, as that of Schleiden already mentioned; to which may be added that of Jordan[2]: "that which is the common ground and identical in all the representatives of the same species, that is the species" — as much as to say, the essential thing, that is the thing. But for practical pur-

[1] A definition of Species for purposes of practical classification, as already said, must distinctly circumscribe the group and separate it obviously from all others; or at least inform as definitely in what constant objective and phenomenal thing or attribute, one or many, its specific difference consists, so that there will be left for us only to determine, if we can, the question of its existence.

[2] "Le fond commun, identique chez tous ceux qui représentent une même forme spécifique, c'est l'espèce." — Jordan, quoted in De Candolle's Géographie Botanique, Tome ii. p. 1073.

poses how is this essential expressed in any species or in all species? How shall we know the representatives of a species? According to Dana[1] "a species is based on a specific amount or condition of concentered force, defined in the act or law of creation." This is the idea of species rather than the definition. But as we cannot yet know directly the ideas in nature we are obliged to look for objective limits of organic groups.

Of definitions based on observation the following is adopted by Endlicher and Unger,[2] and Henfrey[3]: "individuals which are alike in all their constant characters belong to the same species." It follows that those which differ in some of their constant characters are of different species. But varieties of the same species so differ; therefore by the definition they are of different species. It is only by implying that "constant characters" means also aboriginal characters that the definition is true, and then it ceases to be practical.

Another class of definitions, the most numerous and usually reckoned the most orthodox, is based partly upon hypotheses of descent, and partly upon an indefinite number of anonymous resemblances. Linnaeus says[4] that "Species are so many as were originally created diverse forms, which forms, in obedience to the laws of propagation, have produced more, but always like themselves." The expression "diverse forms" is not decisive in regard to single or multiple origin of species, but Linnaeus is otherwise known to have held the doctrine of a single origin. The practical part of this definition, however, lies in the words, "always like themselves." Species, then, is a group of individuals all of which are like

[1] Bibliotheca Sacra, xiv. p. 861.

[2] "Les individus qui concordent dans tous les caractères invariables appartiennent à la même espèce."—Endlicher and Unger, Grundzüge der Botanik, 1843, p. 405, as quoted in De Candolle's Géographie Botanique. Tome ii. p. 1073.

[3] An Elementary Course of Botany: Structural, Physiological, and Systematic, 1857, p. 175.

[4] Species tot numeramus, quot diversae formae in principio sunt creatae .... Species tot sunt, quot diversas formas ab initio produxit Infinitum Ens; quae formae, secundum generationis inditas leges, produxere plures, at sibi semper similes. Ergo species tot sunt, quot diversae formae s. structurae hodienum occurrunt. — Linnaeus, Philosophia Botanica, § 157.

the original pair from which they are all descended; that is, they are like each other. But in what respect and in what degree must they be like each other? This rule would easily distinguish an elephant from a horse, or a pine from a palm; but in certain genera of birds or of orchidaceous plants, where one naturalist would find one species, another might find ten or twenty.

Cuvier admits that species may have a multiple origin, but in other respects his definition hardly differs from that of Linnaeus. He says[1] species should be defined as "the reunion of individuals descended one from another or from common parents and from those which resemble them as much as they resemble each other." The parents in this case, few or many, we may assume resemble each other as much as they resemble their descendants. A species, therefore, is a group of organisms which resemble each other *as much as* they resemble their original ancestors. But as we are not in any instance at all acquainted with the aboriginal ancestors, this "how much" adds nothing to our means of discrimination, and there remains as before a group of individuals which resemble each other. It is true that where organisms are very much alike, and especially when they approach each other gradually and very nearly, though they may be quite divergent at their extreme differences, there is very good reason to suppose they are of the same species. And yet animals and vegetables approach each other by very easy grades until they seem to touch. No one, however, supposes them to be of the same species or genus or class or kingdom. Comparative resemblances, therefore, cannot be relied on, since Cuvier himself, moreover, acknowledges that sometimes individuals of the same species are more unlike each other than others of different species.

The elder De Candolle is more definite. According to

[1] La génération étant le seul moyen de connaître les limites auxquelles les variétés peuvent s'étendre, on doit définir l'espèce, la réunion des individus descendus l'un de l'autre ou de parens communs, et de ceux qui leur ressemblent autant qu'ils se ressemblent entre eux. — Cuvier, Règne Animal. Paris, 1817. Tome i. p. 19.

him[1] "Species is a collection of all the individuals which resemble each other more than they resemble others; which can by sexual union produce fertile offspring; and which reproduce themselves in such wise that it might be supposed by analogy that they have all proceeded originally from the same parents." The younger De Candolle determines what is meant by analogy here by saying in his own definition[2] — very much like his father's — that individuals of the same species must resemble each other as those of analogous structure do which are certainly known to have descended, at least since a considerable number of generations, from the same parents. It ought to be stated that Cuvier introduced his definition by saying[3] that "generation is the only means of ascertaining the limits to which varieties may extend"; and it will be remembered that the "original forms" of Linnaeus reproduce their like "according to the laws of propagation."[4]

We may assume, then, that these four great naturalists — perhaps no other four carry an equal weight of authority — agree in saying, or intended to say, that among organisms of analogous structure individuals of the same species

[1] "En résumant ainsi mes idées, je suis arrivé à une définition de l'espèce qui diffère peu de celle donnée par de Candolle dans la Théorie élémentaire (édit. 1819, p. 193); 'On désigne sous le nom d'espèce la collection de tous les individus qui se ressemblent plus entre eux qu'ils se ressemblent à d'autres; qui peuvent, par une fécondation réciproque, produire des individus fertiles, et qui se reproduisent par la génération, de telle sorte qu'on peut, par analogie, les supposer tous sortis originairement d'un seul individu.'" — Alph. De Candolle, Géographie Botanique, Tome ii. p. 1072.

[2] "Je vais donc admettre les espèces du règne végétal comme elles se présentent à nous à l'époque actuelle, et avec les seules données d'une observation de quelques siècles, savoir comme des collections d'individus qui se ressemblent assez pour 1° avoir en commun des caractères nombreux et important, qui se continuent pendant plusieurs générations, sous l'empire de circonstances variées; 2° s'ils ont des fleurs, se féconder avec facilité les uns les autres et donner des graines presque toujours fertiles; 3° se comporter à l'égard de la température et des autres agents extérieurs d'une manière semblable ou presque semblable; 4° en un mot, se ressembler comme les plantes analogues de structure, que nous savons positivement être sorties d'une souche commune, depuis un nombre considérable de générations." — Alph. De Candolle, Géographie Botanique, Tome ii. p. 1072.

[3] See Cuvier's definition quoted above.

[4] See definition of Linnaeus, quoted above.

must be like and different from each other within the same limits as those of the same lineage, or the lineal descendants of the same ancestors. Certainly we may fairly conclude, in regard to organisms which do not vary more widely than others of an analogous structure known to be of a common lineage, that they also *may be* of common lineage, and if so, then of the same species. For I suppose that few will assert that the offspring of the same parents — hybridism aside — are not of the same species.

But may we safely infer by this analogical argument that organisms so varying within observed limits are of the same species, seeing that some species differ from each other less than some varieties, and seeing that the different definitions are indefinite and unlike in regard to the kind and degree of resemblance in the analogous cases? By one, the individuals of one of the compared groups must resemble each other "as" those of the other resemble each other; by another, "as much as"; by the third, "after such a fashion that" the required inference may be drawn; by the fourth, analogous laws of propagation produce analogous results. *As* implies an exact parallelism — which is not intended — or else it is wholly indefinite. *After such a fashion that* is equally indefinite. *As much as* implies a fixed rank and value for each point of resemblance in order to determine the equality. And *according to the laws of propagation* supposes these laws to be all known. By the known laws of propagation, moreover, it appears that species may and often do pass wholly into permanent varieties. That these permanent varieties or races, when well established, do not revert, except rarely, is the opinion of Hooker, Darwin, and De Candolle, names of the very highest authority on practical questions. The potential variability of the "concentered force" (of Dana) or organific power at the basis of the original species seems to have divided itself among, and sometimes to have exhausted itself in, the varieties and races which are its product. So that the opinion of Dana[1] would be incorrect,

[1] Bibliotheca Sacra, Vol. xiv. p. 861.

that "every individual of a species is but a repetition of the primordial type-idea with all its potential elements, the specific law of force being alike in all." Races cannot produce the primordial type-form; they cannot produce each other; they can only reproduce themselves. It is also the opinion of distinguished naturalists that species vary the more freely the nearer their origin, and that many so-called species are in fact only races of very ancient birth. Indeed, if species are not variable without limits, and if confirmed races when they vary at all do not return towards their source, it follows from the laws of propagation that species were more variable anciently than now, or at least that the more a species *has* varied the less variable it is.

Thus it may have happened in regard to any species that the primordial type-form with its original variability has wholly disappeared in its varieties. Or if it still exists we cannot distinguish it from its varieties. Or if it can be reproduced by synthesis of its varieties we should not know it when it appeared. Or a single variety only may remain, as has been shown by De Candolle, giving rise to the opinion of non-variable species. If, then, in our comparison of the resemblances of individuals of a doubtful group with those of analogous organisms known to be of common lineage we should take as a standard of comparison — which we are liable to do — the offspring of a race or non-variable variety, our conclusions might be very incorrect. That is, in order to distinguish species from varieties by observation of the facts of propagation in analogous cases we must first know which is species and which are varieties. Besides we can never know in any case under observation whether the specific variability has reached its limit. So that this method of comparative average resemblances, aided as it may be by study of the laws of propagation, can yet never go beyond probability; it is essentially incapable of exact results. There is sufficient proof of this in the operation of this method, which is the prevailing one, in practical classification, where often a group which is one species according to one naturalist

may become ten or more in the hands of another. In some instances as many as fifty groups are awaiting decision whether they are one species or fifty. In one case forms which had been separated into four genera with an average of eight to ten species each have been found among the offspring of a single individual, — Nature herself reversing the decision of the naturalists, and reducing thirty or forty species to one. Indeed, some physiologists of highest authority assert that to distinguish between varieties and species is impossible. Here is proof at least that varieties may extend much farther than has been commonly supposed.

It will have been noticed in the four orthodox so-called definitions of species last under review that all the individuals of the group have a certain genetic relation to each other: they are said to be descended from common parents; or they are so much alike that they might be supposed to be of the same lineage; or else they are the descendants of parents, few or many, which resembled them as much as they resembled each other. And since in these definitions individuals of a species resemble each other more than they resemble others, it is implied in all of them that the parents were and are exclusively of the same species as their descendants. Or in other words, that the sexual relations of individuals of the same species are essentially different from those between individuals of different species. Here we come to that roaring lion among species, *hybridism*, a beast that has frightened all naturalists, whether hunting species in Africa or otherwhere.

But though these definitions seem to assert a peculiar sexual relation among individuals of the same group, yet their authors otherwise speak doubtfully in regard to this attribute, and evidently intend or wish to define species without it, avoiding the lion in the way. Even Dana, who in his objective definition of species gives, or seems to give, perpetuated fertile union as an essential character, yet says[1] that "were a case of the contrary demonstrated by well-

[1] Bibliotheca Sacra, Vol. xiv. p. 865.

established facts it would necessarily be admitted." This of course implies that there is after all some other sufficient definition of species, and that this is not essential. We have left, then, resemblances parallel with or equivalent to those among individuals known to be descendants of the same parents, which have been shown to be unreliable.

But there is another class of naturalists who discard the antiquated notion of the necessity of genetic relations and sexual peculiarities in order to the constitution or definition of species. The most distinguished names here — though many others adopt the same opinions — are those of Morton and Agassiz. These naturalists define species as "primordial organic forms." These primordial forms are not at all the same as the "diversae formae" of Linnaeus. For though they differ from each other in many other respects yet not necessarily in sexual peculiarities. Against the determination of species by sexual relations Agassiz[1] brings the remarkable objections: that many organisms are hermaphrodite; that in some species there are many individuals which are never developed sexually; that in others multiplication takes place by budding or other methods independently of sexual combination. The different primordial forms, that is, different species, may associate sexually under the same laws of propagation and with the same results as individuals of the same species. It would seem to follow from this definition that the hunt for species at this late period of the world, or of our geological era, is very idle; since true species could not, or might not, exist after the death of the supernatural primordial forms, the first generation being — at least it might be — an amalgam of two species, and the succeeding generations possibly becoming a complete amalgamation of an uncertain number of species. Accordingly many — who knows how many? — organic forms of the present time are such complete amalgamations of species; as, for instance, fowls, sheep, dogs, horses, men, in the opinion of these natu-

[1] L. Agassiz, Essay on Classification. Contributions to the Natural History of the United States, Vol. i. p. 164.

ralists. And since according to Agassiz no one can hope to distinguish these amalgams or fertile hybrids from unmixed breeds, if there are any unmixed breeds, how can there be, for us, any such natural groups as species? What is the probability of any of the primordial forms remaining to the present time uncontaminated, since we know that varieties of the same species both tame and wild mingle freely, and these primordial species were endowed with the same faculty? Can species under such circumstances be any longer recognized? There is no insuperable difficulty, because this naturalist, having, as he says, "cleared the field of what does not belong therein," viz. the weeds of genetic succession and sexual exclusiveness, and having appeased hybridism by making its offspring legitimate, proceeds[1] "to show what in reality constitutes species, and how they may be distinguished with precision." It is a characteristic of species to belong to a given period in the history of our globe; species do not pass from one geological period to the next, but are created anew at each successive epoch; they also hold definite relations to physical conditions then prevailing, and to animals and plants then existing. In order to determine a species with precision we must know its natural geographical range, and its capability of being acclimated beyond that range. If it inhabits water, is it salt, fresh, deep, shallow, running, or still water? does it prefer sandy, muddy, rocky bottom and shores, limestone banks, or coral reefs? If it is a terrestrial species its locality must be known with equal particularity. We must know its peculiar food; the duration of life of its individuals; their mode of association with one another, whether solitary or gregarious; their period of reproduction; their changes during growth and development; their association with other organisms, whether more or less close and constant, or amounting to parasitism; the size to which they attain; the proportion of their parts to one another; their ornamentation, etc. But as individuals of the same species may disagree widely in all these particulars,

[1] L. Agassiz, Essay on Classification, p. 16.

we must know, moreover, all the variations to which the species is liable. In short, he goes on to say, well-digested descriptions of species ought to assume the character of biographies, and attempt to trace the origin and follow the development of a species during its whole existence, giving a history of all its changes in the course of time, those under domesticity and cultivation as well as its natural variations; to which are to be added its anomalies, diseases, etc. No species can be considered well defined whose whole history is not completed to this extent!

This method would seem, certainly, to promise tolerably precise results, and the promise might perhaps be kept in some cases, provided the competent biographer could be found. But bating the objection that with all its practical details it is really as non-practical as that of Schleiden or any other subjective or metaphysical definition, is the method itself, with the author's exclusion of all regard to genetic succession and other sexual relations, capable in any case of limiting species with certainty? For suppose the competent biographer to have been present at the beginning of the geological epoch, and to have witnessed the creation of a primordial form, represented by any number of individuals, which we will call species A. This species has its geographical range and locality, but not to the entire exclusion of other species. These, though different, must resemble species A in many of their physical and perhaps other relations, seeing that they could inhabit the same localities. They might resemble each other as species now do, so that the most accurate observers confound them. But if the biographer in watching the development, changes, adaptations, and variations of species A was not careful to confine his history to the lineal descendants of the primordial individuals constituting the species, his descriptions would be more or less inaccurate, he would not define with precision.

But suppose farther that the individuals of species B, being tenants in common of the same area with those of A, should form with them a still more intimate union — as the hypoth-

esis warrants — until the two species were completely amalgamated. What should be the next chapter in the biography of species A? Can it still be distinguished with precision? Is the amalgam still species A with the variations to which it is liable? Is it species B? Or is it a new species, C? — in which case species may be derivative and not primordial, and of course are not coincident in duration with the geological period in which they exist. Still other species might enter into this amalgam — an indefinite number. And what would thus happen to species A might equally happen to any other species. Species, then, such as they are in the hypothesis of this class of naturalists, can never be "distinguished with precision," biographically or otherwise. According to these naturalists there was no parsimony of primordial individuals at the beginning of things or at the commencement of our geological epoch. The reconstructed earth was at once sown broadcast in every part, land and water, with both vegetable and animal forms, each species in its appropriate locality and in proportionate numbers. Men were created in nations, their specific characters and geographical limits assigned to each. Organic nature was from the first a completed and mature whole, not an embryo placed under a law of gradual development.

But there is still another class of naturalists, with whom all these things are arranged differently. According to them nature was excessively economical of primordial organisms. Not species, by any means, not even classes, were entitled to a separate aboriginal ancestry. Only a few of the great divisions of the organic kingdoms, perhaps three or four in each, can claim a distinct primeval origin, and these only from single primordial pairs. All we vertebrates, for example, are the descendants of one common father and mother, probably a most venerable pair of fishes, or fishlike progenitors; but perhaps we are of a still more remote and unlike ancestry; possibly we are entitled to say with Job, the worm is our mother. It may be said, then, that of one blood were formed not only all nations, but all vertebrates. This view

increases prodigiously the number of our blood relations, some of which we may be disposed to account "poor cousins"; but we may at least flatter ourselves, if we can have confidence in our own opinions, that we are the head of the family.

These naturalists, however, notwithstanding the apparent simplicity of their system, are troubled with species, and in some respects more than other men, since they have to create them as well as to define them. Their primordial organisms must of course have been endowed with variability so much the greater the smaller their number. It must have been very great, judging by present results of it from so simple beginnings; or it may be without limit, since we do not know how much farther it may extend. The development from these embryonic points is illustrated by that of a tree sending off widely diverging branches in all directions; only there is this difference, that the development is as if the oak, for example, having sent out its branches to a certain distance, should at their next bifurcation produce limbs of elm, maple, beech, each new shoot being different from the others and from the parent oak; these branches in like manner, having proceeded to the requisite length, give birth to clusters of still other kinds, and so on, until the trees of all forests are found proceeding from the germ of the oak. So in the various branches of these primordial organisms, in whatever stage of their development; as, in the region of vertebrates, among fishes, birds, quadrupeds, or in any other region, animal or vegetable, organisms which have a certain degree of resemblance to each other constitute a species. These at the origin of the species will resemble each other very nearly, but as the variability of the species expresses itself it may be developed in a few or many diverging lines differing more and more from each other and from the specific type. Up to a certain point and degree of difference these several lines are varieties of the species in which they originated, but at the next degree of variation they all become different species. And thus a species might suddenly

find itself the parent of ten new species, each different from the others and from itself; and in each instance the thousandth or ten thousandth generation, as the case might be, would be of a different species from its parents. Here, then, is a difficulty of distinguishing species almost equal to that in the amalgam hypothesis. For if species literally pass over into each other, and at the point of transition they of course resemble each other as much as a child resembles its parents, who can say where the one ends and the other begins? Thus all species have originated by successive steps from the one, or very few, primordial species, and proceeding to diverge have produced genera, orders, classes, etc., and hence the difficulty of distinguishing not only species but all other natural groups.

I think it is plain by this time that to express an opinion in regard to species savors much more of rashness than of courage. Are there, then, any groups of organisms — at all entitled to be considered natural groups, and which would include somewhere near the same individuals as most of the definitions which have passed under review — which are capable of exact circumscription and limitation by their phenomenal attributes? It seems to me certain that the notion of genetic relations, of descent one from another, enters instinctively into the conception of species in the minds of all naturalists; not that they are necessarily of single original parents, but if of more, those parents were essentially alike, repetitions of each other. Individuals of the same species are in the language of all men "of the same *kind*"; they are a family group; they are blood relations. For notwithstanding species are described by resemblance and not by descent, it is instinctively taken for granted that the resemblance is the consequence of consanguinity. The truth of this statement is easily tested. Suppose, for instance, a tree known to have proceeded from a chestnut to resemble a beech much more than it does its own species; no man knowing that fact would call it a beech, and all naturalists would shrink from reckoning it as of the species beech.

The elder naturalists, though they believed in direct descent, hesitated to introduce sexual characters into the general definition of species because they supposed that many organisms might be agamous, or non-sexual, and of course they could have no sexual character or relations. But I suppose we may now consider it a universal fact in organic nature that propagation depends primarily upon the combination of two sexual elements. And surely it is of no consequence to the principle whether the two elements are placed in separate organisms as in diclinous plants and unisexual animals, or both in a single organism as in hermaphrodites. This necessity of their combination and its results are the same in both cases. So in metamorphotic animals, — whatever may be the intermediate larval forms, or methods of their multiplication, between the proper parents and the reproduction of the sex-bearing organisms which complete the cycle according to the specific law of propagation, — the essential sexual duality is never lost sight of. In the peculiar organic communities of bees and ants this principle is not affected by the fact that so large a proportion of individuals are undeveloped sexually. I cannot therefore feel the force, or indeed understand the reason, of the objection of Agassiz against the admission of sexual characters into a definition of species, because these characters vary in *some* of their relations, and the processes are not the same in all cycles of reproduction. Everywhere sexual duality, everywhere a combination of sexual elements, is the *primum mobile* of development, and their character more than all other influences combined determines the resulting organism or organisms. Possibly their combinations *in varying proportions* may account in part for varieties in the offspring.

The capability, however, of holding this sexual relation to others is for each individual organism confined within very narrow limits. To the vast majority of other organisms of the opposite sex it can hold no such relation at all. With some it may form a temporary imperfect sexual combination or mixture, which nature always hastens to destroy or to

decompose. But within certain limits the capability of the permanent combination of sexual elements, each with any other of the opposite sex, in the production of unlimitedly fertile offspring, constitutes the normal sexual relations of all the organisms within those limits. This is true of hermaphrodites as of other organisms, for they are most of them naturally cross-fertilized, and all of them are capable of being cross-fertilized.[1] Here, then, we have a method of finding exactly circumscribed, definitely limited groups in every region of organic nature. For all organisms of both kingdoms may be associated into a definite number of such groups, each distinctly separated from all the rest. They are also natural groups, for they will not be found to bring together organisms separated in any higher natural groups, or to associate any differences more important than their resemblances; for in classification of primary groups the sexual relation is the most important of all relations. The sexual elements are the concentration of the essential character and potentialities of the organism; and when these are so much alike as to be capable of permanent combination, there are not likely to be essential differences either of structure or of function. I wish here merely to state the undeniable fact of the existence of such groups, in some one of which every legitimate organism in nature has its natural place, and within which it is straitly confined; or if it wanders into a neighboring enclosure it is forthwith expelled. I do not now assert that these groups are species, but only that they are in the fullest sense *natural* groups.

What is the relation of these groups to species as limited by various definitions? Have we not here as near as possible the identical character in identical circumstances, of Schleiden? the common ground, everywhere the same, of Jordan? the concentered force in its intensest phenomenal forms, of Dana? These groups would include everywhere the diversae formae of Linnaeus with all their descendants, like and unlike. Cuvier's individuals as much like each

[1] Darwin, Origin of Species (ed. 1860), p. 91 seq.

cther as they are like their parents, with the parents as much like each other as they are like their offspring, would find themselves all in the same enclosure. The groups of De Candolle, father and son, of individuals more like each other than they are like others, capable of fertile offspring, so much alike that they might be supposed to have all descended from the same parents, would not exceed these groups. They would include the primordial forms of Morton and Agassiz, and sometimes an uncertain number of them, at least all that are capable of complete amalgamation. This method would also indicate the exact point of transition between the metamorphotic species of Darwin and La Marck, provided lineal descendants of the same parents ever came to differ so much as to constitute such separate natural groups.

But would not these groups often include organisms not coming within the limits of species according to the definitions of the species? It is impossible to answer that question until the true limits of species can be determined practically by their definitions. They would no doubt sometimes admit a wider extent of varieties than is commonly assigned to species; but we have not followed the rule of Cuvier so far as to know all the variations to which species are liable. There might originate within these groups, by natural or artificial selection, permanent varieties or races varying more or less in all their physical relations, — period of maturity, habits, size, and proportion of their parts, and in any other particulars not inconsistent with the common measure and limitation of the whole group. How far varieties may proceed within this limit, or have proceeded, can be ascertained, so far as that is now possible, only by experiment or observation. Undoubtedly this limit would in some cases admit into the same group more than one, and sometimes many, so-called species, as determined by average resemblances and differences; but are there any smaller groups than these, manifestly and certainly not varieties within them, which are natural, and distinctly and practically limitable by a common definition? However that may

be, these larger groups than species, if they are larger, are certainly natural groups. For however in each group the organific power at its base, the idea, may manifest itself freely with easy unconstraint, in variety for the sake of variety, in the adaptation of each particular organism to its outward conditions, or in diverging lines of permanent variation; yet in all the individuals, in all the varieties, in every generation, it returns to its central unity in the essential identity of sexual elements throughout the entire group, which is, as it were, *totus in illis*. This definition is free of all hypothesis in regard to origin of the groups, yet it implies the possibility, the physiological possibility, of either a single or multiple origin, and also the genetic derivation, one from another, of individuals within each group. Varieties, therefore, within these groups cannot extend beyond those possible among individuals of the same lineage, as in fact they have not hitherto. These groups it is plain can never be amalgamated. Can these groups be subdivided? Are there within these *separable* SPECIES, distinguishable from varieties and from each other, which cannot be amalgamated, and so be absorbed and disappear in the larger groups? WHAT THEN IS SPECIES?