ARE SPECIES REALITIES OR CONCEPTS ONLY?[1]

PROFESSOR J. H. POWERS
UNIVERSITY OF NEBRASKA

IN the AMERICAN NATURALIST for April, 1908, there appeared the reprint of some remarkable papers, constituting a symposium by the greater botanists of the country on "Some Aspects of the Species Question."

The attitudes taken toward questions of the nature and reality of species were, on the whole, tentative and questioning. But the opening paper presents, with the utmost lucidity and startling positiveness, a definite conception as to the nature of species: "Species have no actual existence in nature." They are not realities. Individuals alone are real. Species are concepts only, concepts framed by the human mind, and arbitrarily framed withal, for no better reason than its own convenience. Species are compared to spoons, made to fit the human mouth, or the mouth of Linnæus; and until it can be demonstrated—so runs the argument—that this organ has departed appreciably from the typical oral aperture of the great Swede, so long must we continue to fashion our species-spoon-concepts to the exact dimensions of his model.

Now these views, although not wholly new, were a surprise to the writer, both in themselves, in the extremity of their statement and still more in the high authority by which they were supported. Do such things still happen in the botanical world? he queried. Surely no zoologist would for a moment, etc. But hold! "Another Aspect of the Species Question,"[2] by a zoologist this time, and containing the duplicate assertion that many zoologists, "long since reached a satisfactory solution of the species

[1] Studies from the Zoological Laboratory of the University of Nebraska, No. 95.

[2] By Dr. J. A. Allen, NATURALIST for September, 1908.

question by recognition of the fact that species . . . have no real existence, but are merely man-made concepts, purely arbitrary and conventional.''

Is this the dictum of biological science to-day? This by-product of early triumphant Darwinism, with the fear of special creation still upon it, this handy postulate of the sorters of dried birdskins and dried plants, eager to affix tag and title to the whole fauna and flora of a continent—is this the last word of science? Or is it even the limit toward which we are tending? To the writer the reply is unhesitatingly in the negative.

But how refute this proposition of the unreality of species? What reasons have been offered to support it? Unfortunately none. Often suggested in the past, it has been suggested only, and it is now put forward as a self-evident proposition, heavily weighted with authority in lieu of evidence.

Forced by my interest in the subject I have been obliged myself to seek the evidence on both sides of the proposition. I can find but two possible reasons, or rather causes, why species are, or may be, thought unreal. These I wish to state and analyze briefly. Be it fully stated here, however, that I do not for a moment impute these reasons to the minds of scientists whose view of species I am calling into controversy. I find it very difficult to imagine what considerations may have influenced them to the adoption of an hypothesis which seems to me not only fundamentally incorrect but highly injurious to scientific thought and experimentation. I simply assert that, after years of consideration, I can find no other causes for such an assumption and I deem the setting forth of the error in these a useful piece of work.

First of all, then, I think that the unreality of species is frequently assumed, at least by young and careless thinkers, because of what I will term *lapses into uncritical, child consciousness.* This seems a hard saying, and yet nothing is easier than to fall back into methods of thought which we know to be erroneous, but which, just at the times when we feel most certain of ourselves, creep

back upon us because they are the ingrained methods of early uncritical experience.

Now the concept of species is roughly equivalent to the concept of *kind* and this is acquired very early in life.

"What's 'at?" asks the child, pointing perhaps to its first sharply perceived bird, a robin, say, in the grass.

"That's a bird, Johnny,—that's a robin."

"What's bird? What's robin?"

"Why a bird is a thing with feathers and wings and that flies. And a robin is a kind of bird. There is a whole lot of them alike, with red on their breasts like that one, and that makes one kind; that makes them robins."

By such experience, such questions and such replies, rapidly extended, the child soon learns the meaning of the word "kind" as it is applied to living things, and later, he transfers this meaning, only a little sharpened, to the word "species."

But those of us who have formed and retained the habit of reviewing our childhood thinking know that these meanings, these concepts of kinds, never seemed wholly real to us as children, and this *simply because the objects of them were not wholly perceived.* This or that kind of bird, as a group, a totality, a whole, was a great vague somewhat, fading out on all sides where it transcended our actual experience; it was luminous only in the center where actual experience and memory kept is *partially real.* The child ascribes reality to perception, and only semireality to conception. But slowly, in adult life, do we partially free ourselves of the sense of unreality in the objects of our conceptual thought.

In science, however, we certainly should and do learn to test, judge and finally affirm the realities back of our concepts, as well as back of our percepts and simple memory images. We know that unvisited foreign countries are no less real than our own, despite their shadowy vagueness in conception. We ascribe exactly the same reality to the surface of the earth at the south pole that we do to that under our feet, despite that it has never been perceived by man. More still, plurality, multiplicity, per-

plexing as they are to perception and imagination, do not deter us from the ascription of full reality to aggregates. A forest is not less real than a single tree; a swarm of bees is as circumscribed a reality as a single insect. The fauna or flora of an entire continent is surely conceived as a definite objective reality as much as though it were the smallest, the most homogeneous of units. The solar system is a reality as truly as is a single planet; the planet as truly as is the dewdrop; the dewdrop as truly, nay, more truly than is the atom.

I say that in adult life, and especially after thorough scientific training, we correct the naïve error of ascribing reality only to that which is obviously a unit or which has been vividly perceived. But I deem that, beyond question, we are frequently subject to lapses in our thought, lapses into the child consciousness in which the unitary object of perception seems to us the only true reality.

The truth is that if species are denied reality because they are pluralities instead of units, individuals have absolutely no right to a better status. Individuals are pluralities. We may recall President Jordan's humorous refutation of Descartes's celebrated maxim: I think therefore I am. Descartes, said Jordan, had no right to consider himself a unity; he had no right to the singular pronoun. Descartes was an aggregate of cells; these are the active units. He must at least have said: *we* think, therefore, *we* are.

Of course the cells, too, are not really units, but again, are aggregates,—nay, they are aggregates of aggregates of aggregates of aggregates at least; they are, moreover, in all probability, quite as fluctuating aggregates as is a whole species; so that, finally, if unity is to be the test of reality, the atom itself, or the electron, withal, is absolutely the only reality with which science has to deal. But alas for even this bed rock of reality. It, too, is not even good sand. For scientific theorists tell us, and undoubtedly with truth, that atoms and electrons are purely imaginary creations, hypothetical x's only, by means of which we steady our thought while deciphering the se-

quences of phenomena. Thus the quest for reality along the road of unity lands us in a complete reductio ad absurdum, acceptable possibly to some metaphysicians, but utterly repugnant to the clarified common sense of science.

If this line of thought as to the status of the individual and the species seems unprofitable or pressed too far for the taste of many, let us return to a closely related but more practical consideration which is now a live factor in several working lines of biological science. I refer to the fact that individuals are not units in another sense, —the sense, viz: that they have complex life-histories, and must be so thought of and so spoken of if they are to be treated in a scientific as opposed to a popular sense. An individual animal or plant is not a static but a dynamic thing. It does not all exist at any one time, but exists only as a series or succession of stages, bound together by physical continuity and causation, each preceding stage being an indispensable condition of the next. We can not, thus, by any possibility, handle and "sort" individuals. We can not even perceive them by a simple perceptive process. All we handle, sort or recognize is *specimens;* but these, dead or alive, are but fractions of individuals, signs or suggestions of individuals—non-existent at the moment—but which we then proceed to build up in thought by a long process of that same conceptual nature which we use in arriving at our knowledge of species.

If any one is disposed to gainsay this assertion then let him reply to the question: which, or what, is an individual insect? Is it the fertilized egg or some embryonic stage, some younger or older larva?—is it the pupa or the imago? Possibly some one may reply, "the imago; this is at least the adult individual, and the only reality necessarily considered in dealing with species." But this is surely an unscientific position to hold at the present time, even with regard to the insect; while, if we shift our attention to certain other groups of animals, where growth and to some extent morphological change persist throughout life, even the momentary suggestion of a fixed, stable,

"adult individual" leaves us. The common assumption that there is a fixed, or at least a typical adult state is often more assumption than fact, or if partially true, due to the accident of constant average environmental conditions. Thus the writer was astonished to find that an adult salamander, tallying with every character of the "typical individual" would yet, under favorable environment, betake itself to a new period of development and, larva like, issue therefrom so changed in its supposedly fixed characters of adulthood that nothing short of continuous observation could convince one of its individual identity, or rather continuity with the former phases of itself. To know individuals, then, means to know life-histories. To know life-histories means to save and sift our perceptual experience, and to solidify it, little by little, into concepts quite as complex as are those in and through which we know species themselves.

Yet it is admitted that *individuals are real,* despite this fact that they are made up of phase on phase of shifting though correlated characters, only a part of which we can ever perceive. Why then not admit that species are real? Are they not, likewise, groups of interrelated units (units in the practical working sense of this word)? Are not these units—the individuals—bound together by a common genesis as truly as are the cells of the individual's body? Are not these individuals further united by common interactions—sexual and otherwise—between each other, and between themselves and a common environment? Variable criteria all, it is true, but yet assuredly real criteria.

But with these last expressions we have in reality passed to another phase of the subject—from the means by which we know species to the nature of species themselves, and some may well have thought that this transition should have been made immediately. Few, it may be said, are child-like enough to deem species unreal because they are plural and but partially perceived; few are so herbarium-dried, so museum-minded, in thought as to confuse specimens with individuals and deem the latter real

because they may easily be seen and handled. The true reason why some deny reality to species lies, it may be said, not in the nature of our knowledge, but in the nature of the groups of objects which we conveniently designate as species.

Species, it may be stated, are not sharply delimited groups of individuals; species pass into each other and into varieties by insensible gradations. Species are not permanent but transient assemblages of individuals; species change as environment changes. In short, an exaggerated Darwinistic conception of the nature and origin of specific groups may be advanced as a reason for denying them full objective reality. We all know Darwin's conception of species, as accentuated varieties, which were in turn due to accentuation and multiplication of individual differences. Genera, too, he viewed as overgrown species, in which variation and extinction, together with other less obvious causal factors, had led to segregation into minor groups, now, in their turn become species. Species were expanded varieties; genera expanded species.

But certainly this view did not imply, in Darwin's own mind, the non-existence, the unreality of species in nature; though it did imply their derivation by intermediate stages, one from another. If we recall his work definitely we shall remember that he found it necessary to introduce a long and labored analysis to account for the very fact of the sharp segregation between allied species—how it was that characters diverged and genera became broken into compact and contrasting groups rather than remaining a sheer chaos of connected and interlacing forms.

In short, if species are not realities, what aberration of intellect led Darwin to work twenty years collecting facts as to their origin? If species are "concepts only," why did he go to sheep-breeders for light on their nature and genesis instead of to logicians and psychologists? It is these latter who tell us of the nature and origin of concepts. Why did not Darwin entitle his work, "The Non-existence of Species?"

But we have progressed since Darwin, it may be said. I hope we have a little. But have we or have we not progressed toward a conception of animate nature as a chaos of such seething instability that distinctions are essentially arbitrary and boundary lines between groups of forms to be drawn only at the pleasure of the individual with due reference to high authority and venerable tradition? Until I read the luminous article of the leader of the aforesaid symposium I had certainly thought not. I had been led to believe that we were progressing, all in all, in the opposite direction.

I lay claim to no particular knowledge of things botanical. I know there are certain genera of plants where specific and varietal characters are much confused and very possibly undifferentiated. Definite species may in such cases be very possibly undeveloped or degenerate, and therefore non-existent. I remember that in my herbarium days I wondered that botanists would carry their system through, whether or no, and describe species where they themselves plainly doubted their reality. It seemed to me this was following the final advice of the Devil in Faust and building systems of words without meaning. I little dreamed, however, that they would ever go so far as to defend the whole Mephistophelean hypothesis of an essentially arbitrary system of words without objective validity.

Surely some botanists are feeling their way far from this conclusion, when, as for example De Vries, after half a life time of experimentation, formulates a theory of species which is not only that of a real thing in nature, but approaches in definiteness and demarcation to the conception of a chemical compound. Species, for De Vries, are almost chemical compounds. Are chemical compounds—chemical species, so to speak—are they realities, or are they too concepts only?

When I began this paper I had in mind to employ the majority of my space in the presentation and analysis of facts concerning a few species with which I have worked personally and which have been chosen with definite re-

gard to the matter of the light they might throw on the nature and origin, and consequently upon the reality or non-reality, of species. But the brief space remaining will permit scarce a reference to them. Moreover, single instances in biological fields can never prove general laws; they can, at most, illustrate them and prepare our minds for wider proof.

Let us look for a moment, however, at the group of little animals known as Hydra. Linnæus, with his long spoon, swallowed the whole genus at a gulp. He knew there were differences, as his description shows, but he called all by the one binomial, *Hydra polypus*. Soon, however, zoologists became convinced that Linnæus had been eating too fast. There were more species than one. But how many? Even yet unanimity has not been reached. Does this constitute an argument for the unreality or for the conventionality of species? In truth it does not. The genus Hydra has never been fully investigated. Interminable discussions of the undecipherable problems of priority have not been lacking; some good observations, and, much more to the point, some good culture experiments have been made. But year-long, controlled and pedigreed cultures are required, cultures successfully carried through sexual as well as asexual phases. Had these been carried out, as I trust they have been by the writer, the truth of the conclusion would have been amply demonstrated, that we have within the genus Hydra (whether or not we shall ever be able to name them) a number of highly autonomous aggregates of individuals, separated, the one from the other, by a large number of minute but highly constant differences. These groups are such as are commonly and appropriately called species, despite the fact that the ordinary student with the collecting bottle may be unable to distinguish them. To deny them reality or treat their systematic segregation as a matter of convention only is as inappropriate, because as untrue to the facts, as to deny the reality of any and all distinctions in nature.

Certain of these differential characters are of special

interest in connection with the problem of the separateness of species. Thus two of these groups that outwardly resemble each other the most, and whose distinctness is still doubted by some eminent zoologists, prove, under examination with modern technique, to be possessed of extraordinary histological differences. Hardly may we find in the whole mammalian series corresponding types of cells showing so fundamental differences in structure as are shown by the cnidoblasts of the two species which (since we must choose among the questioned specific titles) are probably to be designated as *Hydra fusca* and *H. diœcia*. Species, therefore, which Linnæus could not distinguish and which it may be hardly possible for the ordinary student to distinguish with the care commonly devoted to such subjects, may yet be separated by differences which seldom obtain between much more remotely related types.

Did space permit, I might also illustrate from this genus of organisms the proposition that the integrity, and hence the reality, of species is not destroyed by the fact that certain individuals of one may be transformed into members of another. I will but refer to the fact that it is possible, though extremely difficult, to transform *H. viridis* into another, a white Hydra (*H. fusca*), differing from it in practically every specific character. I do not refer to the mere bleaching of the green species. This, up to a certain point, is easy, but carries with it no significant morphological changes. The transformation which I have effected (in both directions) does carry with it such changes, and once produced they are extremely permanent through an indefinite number of generations and in spite of many environmental changes.

Of course we may, if we will, degrade these types from specific to varietal rank, although they probably deserve the specific distinction. But the point of emphasis is the relative autonomy of the two groups. I have never been able to even find them in the same habitat. Interbreeding is precluded by their sharply separated periods (fall and spring respectively) of sexual development. Species,

then, may be sharply demarcated despite of the possibility of reciprocal derivation.

Lastly I had wished to defend the seeming paradox that species may be not only real, but all but absolutely stable despite of the widest variability. This is not a contradiction in terms. The stability of a species depends upon its refusal to vary in certain given directions, *i. e.*, away from its specific characters, or, secondly, upon the non-transmissibility of such variations when once produced. The variability of a species, however pronounced, may mean only the production of non-specific characters or the production of characters of whatsoever order which are not repeated in the offspring.

Thus it proved to be with the species of salamander—*Amblystoma tigrinum*—upon which I spent several years of almost continuous experimentation. Variability and instability of species, when I began my work, for me were synonyms; when I concluded they had lost almost all relation in meaning. The astounding variation of this species was in the main but a somatic by-play in response to environing forces. However, wholly against first impressions, it turned out that this somatic variation, despite its variety and extent, yet had its marked limitations. How I did strive to make *Amblystoma punctatum* out of *A. tigrinum*—such a little thing, too—just to make a leopard salamander out of a tiger salamander. I did not even try it until my third season's experimenting. It was really too insignificant a task. Had I not observed nature working much greater changes?—and, by imitating her methods in experiment, had I not gained the key to her processes? Had I not passed the bounds of specific and even generic characters? Indeed, certain wise ones had nodded gravely, and suggested no less than family rank for the best of my handiwork. Just to make *A. punctatum* out of *A. tigrinum!* Besides I had the thing three quarters done already, time and again, as a by-product of my other work. At last I concluded, however, to make a few bona fide *A. punctatum* just for the fun, and to plague certain species people. And for season

after season I plagued myself at the rate of sixteen hours per day to accomplish this and other kindred things all relating to the stability or instability of this one species. And how much I accomplished, and yet how absolutely little. I made the characters all right, at least of the adult; unless possibly the special distribution of a few skin pores about the head eluded me. Last of all I even segregated the palatine teeth into groups and dragged them well back toward the throat in true punctatum style. It was easy, given time and the knowledge how. Though alas, while I was corralling this chief character, the whole herd of lesser ones which I had previously rounded up were absolutely certain to escape me.

In short, my seeming success was abject failure. Characters, but never in perfect combination; and then, not a trace of tendency toward transmission. I found no mordant of conditions penetrating enough to bring germ cells into the slightest harmony with my special somatic policies.

Species unreal! It may be that they are in some ghostly sense toward which my imagination has not wandered. It may be that many alleged species are unreal enough. But the majority of those with which I have dealt, although chosen for the very reason of their seeming or possible unreality, so to speak, have yet left upon my mind the impression of *almost* indissoluble entities.

An exaggerated impression it may be. Had I been collecting facts about geographical races, for example, I might have verged toward other conclusions. But such study, if it makes for a seeming fluidity of nature, is confessedly but tentative and superficial, and its facts about species are but a part of the facts. Does Mendelianism, for instance, with its unit characters and its mathematics of heredity, make for the unreality of species? Do modern experimentalists claim to be dealing with species as concepts only?

This leads to a last word. Is it of any importance how we think of species? May we, equally well, think of species as conveniently segregated groups of more or less

similar objects, associated for convenience sake with a single appellation; or as correlated, genetically unified groups, as segregated portions of reality (convenient or inconvenient for our intelligence) which nature somehow sets apart, regardless of whether we know and name them or not? To the writer it seems a matter of the first importance. If organic nature is so fluid that our distinctions are conventional only, if specific names are but handy helps by which we point out this or that sizeable mass of organic territory, then must our whole attitude be altered accordingly. It is no wonder that those who hold this view are satisfied with nothing short of a general knowledge of a whole fauna or flora. But if species are downright realities (as science counts reality), subtle, illusive realities, perhaps, still less than half understood, yet existent, demanding ever more exact definition and deeper explanation, then their knowledge becomes a new and better thing, and the impetus they offer to investigation is wholly changed. Then must we recognize the right to modify Linnæan species whenever they disagree with reality, however much we respect Linnæan authority.

The whole spirit of modern biological research seems to the writer to demand the conception of species as realities,—not all alike, in their reality, of course. Linnæan species, elementary species, physiological species, ontogenetic, phylogenetic species,—these and more may well prove to be essentially unique phases of nature's reality. But does not the thought of the investigator that steadies itself by these conceptions of species as realities fully justify itself by results?

And if there are other reasons for the assertion of the unreality of species, over and above the return to that child-like thought which sees reality only in the obvious units of perception, over and above a carelessly exaggerated idea of variation as obliterating all but conventional distinctions, what, we ask, are they?