

oxide. The tellurium was purified from the members of the other groups of the Mendelejeff's table by precipitating the chloride with sodium thiosulphate, and treating the  $\text{Na}_2\text{S}_4\text{TeO}_6$  formed with an alkali. The Te thus obtained was further separated from members of the same group by fractional sublimation of  $\text{TeO}_2$ . The results obtained for the atomic weight of tellurium agreed very closely with that previously obtained by Brauner, Kothner and others, and the conclusions were drawn that the atomic weight of Te has been accurately determined, that the Te used contains no other element known or unknown, and also that the atomic weight of iodine has been correctly determined. The position of Te in the sixth or eighth group of the table was also discussed.

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#### DISCUSSION AND CORRESPONDENCE.

##### HEREDITY AND SUBSPECIES.

I HAVE read with deep interest, and some surprise, two recent articles in this journal by President David Starr Jordan, entitled, respectively, 'The Loch Leven Trout'<sup>1</sup> and 'Ontogenetic Species and Other Species.'<sup>2</sup> To take them up in sequence, the facts presented in the first article are, in substance, that a trout found in Loch Leven differs from the trout of neighboring lakes and streams in its 'large size, more silvery color, sparsity of spots, the red spots and ocelli characteristic of the brook trout \* \* \* being usually wanting,' while 'the orange edge of the adipose fin, characteristic of the brook trout, is wanting in the Loch Leven trout.' These differences are so marked that the Loch Leven trout (*Salmo levinensis*) 'has been usually considered as a valid species, distinct from the other trout of Great Britain.' President Jordan cites Dr. Day as stating that the Loch Leven trout changes into the ordinary brook trout of England (*Salmo fario*), 'when planted in streams of Gloucester or Guilford, the colors of the Loch Leven trout being seen

on exceptionally well-fed individuals only.' He also quotes Dr. Day as regarding some fifteen or more other commonly recognized species of trout inhabiting the lakes, streams and estuaries of Great Britain and northern Europe as 'all forms of one and the same species'; and adds:

A member of one of these so-called species would be changed to one of the others if it grew up under the same surroundings. These forms are not subspecies, for that implies a divergence which should be hereditary, however slight. They are, if this view is correct, local variations of one species, \* \* \*.

Elsewhere in the same article, President Jordan states the results of transferring Loch Leven trout to the waters of the Yosemite National Park in California, where in the course of ten years they have come to be 'exact representatives in form and color of the common brook trout as seen in the streams of England.' He further says: 'These Loch Leven trout in the Yosemite are typical *Salmo fario*, or brown trout of England.'

While these facts are of extreme interest, and have an important bearing on the evolution and character of local forms, they merely emphasize and confirm conclusions derived from general considerations, and not based on experimental research; or, as in this case, on the incidental results of fish-culture. They point the way, however, to a field of investigation evidently pregnant with interesting results, to which President Jordan has forcibly called attention in his later article on 'Ontogenetic Species and Other Species.' In this paper, however, he takes a position that seems to me quite new, and directly antagonistic to the views held, I think, by the generality of students of geographical variation in birds and mammals, as regards the nature of species and subspecies. He says, for example:

It remains, however, to be determined whether these environmental forms—these species and subspecies produced by the direct influence of heat, cold, humidity and aridity—are 'ontogenetic species' \* \* \* or whether they have a real existence outside the lifetime of the individuals actually composing the group or species. We do not know which of the traits induced by direct action of the environment, if any, are

<sup>1</sup> SCIENCE, N. S., Vol. XXII., No. 570, pp. 714, 715, December 1, 1905.

<sup>2</sup> *Ibid.*, No. 574, pp. 872, 873, December 29, 1905.

actually hereditary and which are not. If we find that the dusky woodpeckers of Vancouver Island retain this shade when reared in Arizona, then humidity would be a real factor in the formation of species. *If such birds, transferred in the egg to a region, should develop in the fashion of the local race of this region, and not like their own parents, then the duskiness is not a true specific or subspecific character.*<sup>3</sup> The real character of the species would be found in the tendency to develop dark plumage in humid surroundings and pale feathers under other conditions. *In such case humidity would be merely a factor modifying individual development but not connected with the origin of species.*<sup>3</sup>

It is hard to believe, on reading the above paragraph and the adjoining context, that the writer has duly weighed the real conditions of the problem; if such be the case, he must have a concept of species and subspecies different from that of most of those who have been most intimately concerned with their consideration. It is also hard to understand the meaning of the term 'hereditary' as used in the above transcript.

Young mammals in the nursing stage have a pelage different in color and texture from that later acquired; young birds have a characteristic nestling plumage different in color and texture from that of the adults, or from that acquired with the first moult. Every experienced mammalogist and ornithologist knows that the local differentiation in color between the subspecific forms of a given group is often (but not always) much more strongly expressed in the first pelage or plumage of the young than in the adults of the same forms. In view of such facts it seemingly goes without saying that local differentiations are transmitted from parent to young, and are hereditary in the usual sense of that term; doubtless, no one questions their continued transmission from generation to generation so long as the environment remains stable. Probably also few would question that were representatives of a strongly marked local form (in the case of birds, either as eggs or mature birds) to be transplanted to a region markedly different climatically from their natural home, they would gradually lose their original character-

istics and become, after a number of generations, more or less modified, in better agreement with the new conditions of life. But it would be apparently rash to expect a very material change in a single generation. There is apparently not the least probability that an egg of a large dusky Vancouver woodpecker taken to Arizona would hatch into a smaller pale form like the race native to Arizona.

The case may be somewhat different with fishes, which have a more lax organization, are lower in vital energy, and are probably much more plastic than birds; yet it would be of interest to know whether the eggs of the Loch Leven trout, when taken to other waters, hatch into trout like those that are native to the waters to which the Loch Leven trout eggs had been transferred. In the waters of the Yosemite it appears to have been eight or ten years after the introduction of the Loch Leven trout before it was discovered that they had become indistinguishable from the English *Salmo fario*. It would further be of interest to know whether, *through actual comparison of specimens*, it has been found that *Salmo levinensis*, in losing its Loch Leven characters in California waters, has not also acquired some slight differences from the true *S. fario*; for the California environment must be greatly different from that of England.

So far as known to me, no similar phenomena have been observed in birds, through their transference from one climatic area to another. During the last twenty-five years it has been a common practise to restock certain localities in southern New England and other parts of the northern tier of states with southern quail, in places where the northern birds had nearly disappeared in consequence of severe winters or other causes. It is even said that pure northern stock is now hard to obtain; and I have heard ornithologists congratulate themselves that they had in their collections a few specimens of the original northern bird, taken before the introduction of quail from the south. It is also commonly believed, by those who are in position to know, that the quail of the northern states are now a mixed race. As, however, the imported birds have been brought from the middle portion of the

<sup>3</sup> Not italicized in the original.

eastern United States, as West Virginia, Missouri and Kansas, it is not probable that they would differ enough from the northern birds to be easily distinguishable from them. The introduction of birds from Florida and the Gulf Coast has not been in favor with sportsmen's clubs, owing to their small size and their probable inability to withstand such a radical change in climatic conditions.

It may be added, as vaguely bearing on this point, that some years ago (in 1889) a prominent New York ornithologist (now deceased) received four skins of quail (two pairs) said to have come from South Dakota, which so greatly differed from the other forms of quail known to him that he looked upon them as a new species, of which he prepared a description for publication under the name *Colinus dakotensis*. As they were small and dark, they were later compared with quail from southern Florida and found to be practically the same, and the supposed new species was suppressed. The conclusion reached was that these peculiar South Dakota quail had been imported from Florida; but whether these birds were part of the original importation or from a later generation was never satisfactorily established, and the case is, therefore, without special significance in this connection. The following, however, has a rather direct bearing upon the matter at issue.

In Cuba there is an indigenous species or subspecies of quail (*Colinus virginianus cubanensis*), nearest to the Florida form (*C. v. floridanus*), but differing from it in a marked degree. According to the late Dr. Gundlach<sup>4</sup> quail were introduced into Cuba about one hundred years ago, from just where is not stated, but most probably from Florida. And there doubtless have been other importations since. In 1892 Mr. Frank M. Chapman, of the American Museum of Natural History (to whom I am indebted for the suggestion of this case), collected specimens of the indigenous form and of a form impossible to distinguish from the Florida bird. These latter were undoubtedly descendants of birds long previously introduced into Cuba from Florida, which

<sup>4</sup>*Journ. für. Orn.*, XXII. Jahrg., Juli, 1874, pp. 300-303.

for many generations had maintained in a large degree the characters of the Florida bird. There also occur in Cuba quail that are intermediate in characters between the true Cuban form and the Florida form, due possibly to interbreeding, but also possibly to the action of environment upon the introduced Florida stock.

There are, however, good reasons for believing that the eggs of small southern forms of widely dispersed species would not, if taken to northern localities, hatch into large birds like those of the new region to which the eggs were transferred. For not only does the size of the bird, in species of wide latitudinal range, decline southward, but also (very naturally) the size of their eggs; and there is, furthermore, as a rule, a reduction in the number forming the set, as made known by me in 1876,<sup>5</sup> mainly on the basis of information furnished by the late Major Charles Bendire, than whom there is no higher authority on the eggs of North American birds. Thus eggs of the cowbird from New England average 23 x 16 mm., and those from Arizona 19 x 14.7 mm. It is perhaps of interest to note that in the American tropics the number of eggs in a set, in most passerine birds, is two or three, while in temperate North America the number is four or five, five occurring here about as frequently as three in the tropics.

A word now as to 'ontogenetic species,' and 'species' and 'subspecies.' President Jordan apparently has, as already said, a different concept for subspecies from that of those who first gave currency to the term. The names of all groups in biology are of course conventional terms, employed by common consent as a convenient means of arbitrarily designating groups of greater or lesser comprehensiveness, and having definite relations to each other in any taxonomic system. In introducing the trinomial system of nomenclature, in 1886, the A. O. U. committee on nomenclature stated<sup>6</sup> that this system, as a matter of

<sup>5</sup>'Geographical Variation in the Number and Size of the Eggs of Birds,' *Bull. Nutt. Orn. Club*, I., 1876, pp. 74, 75.

<sup>6</sup>'A. O. U. Code of Nomenclature and Checklist of North American Birds,' 1886, p. 31.

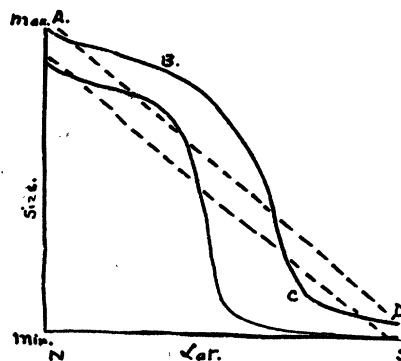
experience, had been found to be "particularly pertinent and applicable to those geographical 'subspecies,' 'races' or 'varieties,' which have become recognizable as such through their modification according to latitude, longitude, elevation, temperature, humidity and other climatic conditions." This has been till now, and in general still is, the sense in which the term subspecies has been employed and understood by the large number of ornithologists and mammalogists who constantly and systematically make use of it in designating geographic forms that, while well-marked, are known, or supposed, to intergrade. If we are to take President Jordan literally, heat, cold, humidity, aridity or other environmental conditions, are merely factors 'modifying individual development but not connected with the origin of species.' It all depends, it is claimed, upon whether or not the characters shown by the forms commonly designated as subspecies are 'actually hereditary.' If their persistent transmission through practically endless generations be not hereditary, there seems necessary also a new definition for heredity, as well as for subspecies. While the action of all such influences is doubtless ontogenetic, and is by many recognized as such, any attempt to distinguish 'ontogenetic species' from other species, or subspecies, tends to confusion of ideas rather than to any useful discriminations. It may be that President Jordan has failed to clearly express, in the paragraph quoted near the beginning of this article, the ideas he intended to convey, for it seems to me—perhaps through some special obtuseness on my part—that there is lack of coherence, if not actual contradiction, between the parts I have italicized and the portion that intervenes.

J. A. ALLEN.

#### THE EVOLUTION OF SPECIES THROUGH CLIMATIC CONDITIONS.

THE very interesting paper by Dr. J. A. Allen (*SCIENCE*, November 24), under the above title, like all really useful discussions of evolution, inevitably suggests further observations. The facts presented are of the highest importance, and for this very reason

we want to be quite sure of them in every case. When two 'subspecies' are joined by intermediates, the transition may be uniform all along the line, or it may not. In the diagram here given, the vertical line indicates difference of size; the horizontal one, of latitude; and the 'curves' are plotted in the usual way.



The two more or less parallel lines indicate the extremes of individual variation. Now if 'the variation in size from the north southward is as gradual and continuous as the transition in climatic conditions' (Dr. Allen, *l. c.*, p. 664), the phenomenon will be expressed by the dotted lines, except that in nature the slope will never be quite so uniform, because the change of climate is not perfectly uniform.

If, however (as is surely true of some of the cases Dr. Allen cites), we have two practically uniform subspecies, each true to its own type within a certain area, but having between them a region in which they completely intergrade, the curve will resemble the solid lines of the diagram. The slight slope of the lines from *A* to *B*, and *C* to *D*, will be explicable as the direct result of environment upon individuals. In such a case, it is clear that the two subspecies, in the regions where they remain true, are in fact isolated from one another, and that it is exactly where they are not isolated, that they fail to conform to any single definable type. Such a condition of affairs might very well be produced if two distinct forms had arisen in isolated places, and their ranges had subsequently overlapped, their evolution not having proceeded far