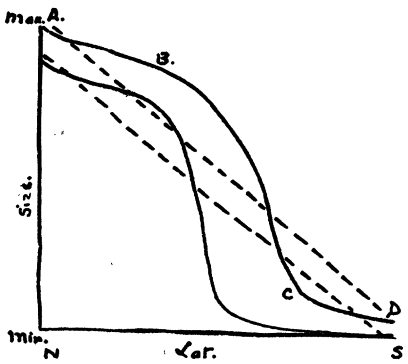
experience, had been found to be "particularly pertinent and applicable to those geographical 'subspecies,' 'races' or 'varieties,' which have become recognizable as such through their modification according to latitude, longitude, elevation, temperature, humidity and other climatic conditions." This 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

enough for them to be incapable of breeding together.[1]

It will be readily apparent that it is extremely difficult to absolutely demonstrate a case like that represented by the dotted lines. Perhaps in no instance is the series of specimens so complete as could be desired, and usually it is not nearly adequate. The case may be complicated by the existence of half a dozen subspecies, occupying small areas, justifying the nomenclature of the 'aspiring young naturalist.'

Last year I published a revision of *Hymenoxys,* a genus of plants (*Bull. Torrey Bot. Club,* September, 1904). I found in that genus a case which seemed to me to exactly agree with those postulated by Dr. Allen, except that the large form was southern, the small one northern. The difference between the extremes was such as to almost constitute a *reductio ad absurdum* of my classification; yet when I had the whole series (borrowed from several large herbaria) spread out on a table I did not know where to draw any hard-and-fast lines. I accordingly called them all subspecies of *Hymenoxys chrysanthemoides,* but a comparison between plates 22 and 23 of my paper will doubtless cause many readers to wonder how I could do it.

Having myself attempted to demonstrate this case, I perhaps ought not to object to any of Dr. Allen's; but all I wish to urge is, that the evidence should be made more complete and the different classes of cases should be distinguished. In the case of the *Hymenoxys,* we do not know what results would come from sowing Texas seed in the vicinity of the City of Mexico, and *vice versa.* Experiment might show that while the characters of the herbarium specimens overlapped, there were in reality several distinct plants, constitutionally and essentially different, and that even most of the apparent intermediates were really hybrids. Experiment might show, on the other hand, that several so-called subspecies were merely phases of one thing, the individuals directly modified by the climate. This has been demonstrated for certain mountain plants, by dividing a single individual and growing the halves at different altitudes. I do not anticipate these results in the case of *Hymenoxys chrysanthemoides;* but it would be very well worth while to see whether they are attainable.

Finally, it is by no means to be assumed that the 'effects' of climate are necessarily direct, and not brought about through the agency of natural selection. This, however, is a large question, not requiring discussion at this moment.

T. D. A. Cockerell.

[1] *Homo sapiens,* who offers a classical example of segregation without physical barriers, is now being subjected to this very process. The result will probably be greater racial uniformity, or rather the breaking down of racial differences, with increased *individual* variability, due to the fact that while the individuals will cross, many of their *characters* have become so far differentiated that they will not do so. The mongrel dogs in the street afford an illustration of this. How all this will affect the development of the higher human attributes, is a question which deserves serious thought. The increase of insanity and crime in civilized countries, though obviously due largely to quite other factors, may be partly explicable as a result of incongruous combinations, produced by 'Mosaic inheritance.' Genius, if produced in good quantity, may become more erratic. On the other hand, no doubt such evils as war, famine and pestilence will be gradually overcome.

ONTOGENETIC SPECIES AND CONVERGENT GENERA.

The recent exchange of views in Science, between Dr. Jordan and Dr. Allen concerning 'environmental species,' gives occasion to notice the widening gap between the formal conception of species entertained by the biologist as units of evolution, and the actual notion of species used by the systematist in working over a group, or the specialist in arranging a collection. For if 'ontogenetic species' do occur, as there seems reason to believe, a marked distinction must be made in theory between them and the *phylogenetic species,* as they might be termed, denoted by the older, or Darwinian, definition which requires the feature of inheritance of the characters marking them. In practise, of course, this feature has usually been taken for granted