An 'ontogenetic species' its traits produced by the direct action of the environment, is the Loch Leven trout (*'Salmo levenensis'*), which I have lately discussed in these columns. Transferred to the brooks of England or to those of California, this supposed species loses its lake-bred characters and becomes the common brook trout.

Perhaps our ornithologists will some day test their species and subspecies by a test of the permanence of this class of characters. No doubt we should drop from the systematic lists all forms which may prove to be purely ontogenetic, all whose traits are not fixed in heredity.

In my recent article, as noticed by Dr. Allen, I have used the word 'barrier' a little too vaguely. For the purposes of this study, I should regard a broad plain as a barrier to a species which inhabits it, even though it were abundant, from one side to the other. A barrier in this sense is anything whatever which checks free interbreeding, even though it offers no actual check to the life or movement of the species. With quiescent animals, the individual moves but a short distance and the traits at one end of an unbroken series may be quite different from those of other individuals at the farther end, as Dr. Allen has very properly suggested. The term 'bionomic barrier,' used by Dr. A. E. Ortmann in a personal letter, seems to me a very apt one as covering the species-producing phases of isolation.

Certain papers of Rev. John T. Gulick on the evolution of species of land-snails and other animals deserve more attention than they have received. In one of these papers, 'Divergent Evolution through Cumulative Segregation' (Smithsonian Report for 1891, p. 273), Mr. Gulick corrects certain erroneous assumptions on the part of Dr. Moritz Wagner. Mr. Gulick says:

> Separate generation is a necessary condition for divergent evolution but not for the transformation of all the survivors of a species in one way.
>
> Separation does not necessarily imply any external barriers or even the occupation of separate districts.
>
> Diversity of natural selection is not necessary to diversity of evolution.
>
> Difference of external conditions is not necessary to diversity of evolution.
>
> Separation and variation—that is, variation not overwhelmed by crossing—is all that is necessary to secure divergence of type in the descendants of one stock, though external conditions remain the same and though the separation is other than geological. The separation I speak of is anything in the species or the environment that divides the species into two or more sections that do not freely intercross, whether the different sections remain in the original home or enter new and dissimilar environments.

All of this is in general accord with my own experience.

DAVID STARR JORDAN.

ORTHOGENETIC VARIATION?

IN a recent paper[1] I reviewed Gadow's hypothesis of 'Orthogenetic Variation,'[2] in the light of his own evidence, and in the light of such observations as could be added. In SCIENCE for November 7, 1905, Dr. Gadow publishes a reply under the title 'Orthogenetic Variation.' It would be superfluous merely to rediscuss the data previously published; in fact, had the matter gone no further than the original paper, elaborate criticism in the first instance might have been unnecessary, since scientific readers could judge the evidence for themselves. But unfortunately, as will be shown below, subsequent use has been made of the idea for presentation to the general public, not expressly as a tentative hypothesis —but *without qualification*.

In the first paragraph of his reply, Gadow says, 'I am anxious that it [orthogenetic variation] should not be misrepresented,' and, in the second paragraph, 'the paper by Mr. Robert E. Coker * * * calls for some remarks on my part by way of protest and correction.' I was glad that after careful reading of his paper, I found no reference to any statement

[1] Gadow's hypothesis of 'Orthogenetic Variation in the shells of Chelonia,' Johns Hopkins University Circular, No. 178, May, 1905.

[2] 'Zoological Results Based on Material from New Britain, New Guinea, Loyalty Islands and elsewhere, collected during the years 1895, 1896 and 1897, by Arthur Willey,' Part III., pp. 207–222, Pl. XXIV., XXV., Camb. Univ. Press, May, 1899.

of mine that had 'misrepresented' his hypothesis, and so needed 'correction.' In fact, I did not suppose I could have misrepresented it, because I had given it merely by quoting his own words at length. Two of the paragraphs quoted by me Gadow quotes in his recent paper, following them with the words "I think I had stated the case fairly. It left no doubt about the definition of at least one kind of orthogenetic variation" (p. 638). But we are indebted to his recent paper for the very concise statement that 'cases of orthogenetic variation are simply ontogenetic stages, passing reminiscences of earlier phylogenetic conditions.' The basis for his assumption that the abnormalities in number and arrangement of the horny shields of turtles are 'orthogentic variations' is a table of percentages of abnormalities, made from 76 specimens (47 new-born, and 29 of various sizes, from three inches to 'large'). This series of percentages of abnormalities, the percentage decreasing with age, is supposed to indicate that turtles 'amend their scutes' and 'grow out of these irregularities by the reduction or squeezing out of certain scutes.'

Gadow states (p. 639) that with the addition of my embryos to his 76 specimens, 'the percentage still decreases with age'; and gives the following revised table, including both sets of turtles:

	Per Cent.
Of 73[3] embryos or newborn, 53 are abnormal..	7[4]
Of 9 specimens from 3 to 8 inches, 3 are abnormal	33
Of 19 specimens from 8 to 24 inches, 5 are abnormal	22[5]
Of 9 specimens from 24 inches to 'large,' 2 are abnormal	24[6]
Of 7 large specimens, only 1 abnormal.......	12[7]

The table requires a comment. The last four groups are based, not on (9 + 19 + 9 + 7) 44 specimens, but on only 29, for 15 turtles were counted twice: the six 8-inch specimens in both second and third groups; the two 24-inch specimens in both third and fourth groups; and the seven 'large' specimens in both fourth and fifth groups. *Twenty-nine specimens divided into four groups, from which a series of per cents. is computed* to be compared with a per cent. based on a comparatively few newborn turtles—and this the *sole basis for an elaborate hypothesis,* given to the scientific world with the supporting (?) evidence and, subsequently, *given to the general public,* without the evidence, in a comprehensive monograph on 'Amphibia and Reptiles.' This I regard as the 'sole basis,' for though his comparison of the abnormalities with supposed phylogenetic stages is interesting and suggestive, and may support an interpretation of the abnormalities as *atavisms,* it does not in the least imply that *the individual recapitulates these stages,* and if the latter assumption has other basis than the table of percentages, what is it?

The writer is now pursuing anatomical and embryological studies, the results of which may have some bearing on the interpretation of the abnormalities in question, and these results will be given out in due time. But the question at present is not—Can Gadow's assumption be disproved? but—Have there been in hand facts to justify its promulgation? Being promulgated, should it be included *without qualification* in a comprehensive monograph intended for the general public, who will not refer to the original paper to find that it is merely a hypothetical assumption from a very small number of facts? The reference is to the 'Cambridge Natural History,' Vol. VIII., 'Amphibia and Reptiles,' by Hans Gadow (London, 1901), where the following unqualified statements occur (the italics are mine):

It is absolutely certain that the number of transverse rows also was originally much greater than it is now. The mode of reduction of the numbers of the neural and costal shields has been studied in *Thalassochelys caretta* (cf. p. 388.) The accompanying illustration (Fig. 68) [This is a reproduction of text figure, from Willey's Zoological Results. R. E. C.] shows *some of the many stages actually observed* in the reduction of the

[3] Presumably typographical error. Gadow's 47 newborn plus my 28 embryos = 75.

[4] Presumably typographical error. 70 intended.

[5] Presumably typographical error. 26 intended.

[6] Presumably typographical error. 22 intended.

[7] Presumably typographical error. 14 intended.

shields. The chief point is that *certain shields are squeezed out, or suppressed by their enlarging neighbors.* The ultimate result is the formation of fewer but larger shields.[8]

Can these words be intended figuratively, the reference being to phylogenetic development, not to 'orthogenetic variation,' with all that that term, as defined by Gadow, implies? If so, the cross reference on a later page is certainly misleading: for in his discussion of the variations of *Thalassochelys caretta,* he says:

The interesting fact in connection with these variations is, moreover, that some of the shields are much smaller than the others, sometimes *mere vestiges in all stages of gradual suppression,* and that the *abnormalities are much more common in babies and small specimens than in adults. The importance of these 'orthogenetic variations' has been discussed on p. 326.*[9]

ROBERT E. COKER.

JOHNS HOPKINS UNIVERSITY,
November 28, 1905.

ON THE GRANTING OF THE M.D. DEGREE.

A SHORT time ago I received a letter from a member of a state board of medical examiners which touches upon a matter of present interest.

The letter, from which I shall quote, was in reply to one giving information respecting courses in this college designed for students who have the study of medicine in view.

After remarking that in his state the medical examiners had decided to give one year's credit to graduates of colleges, provided certain subjects in biology, chemistry and physics had been pursued in the college course, he proceeds as follows:

The fact is that many of the colleges teach these branches better than the average medical school. Any ordinary high school boy can enter the medical department of the university. Yet, they are not willing to give a year's credit to men who take four years beyond their entrance requirement. The confederation of state medical boards is divided on the question. So long as the average medical school admits high school graduates, I shall stand for giving one year's time to men who take a college course. Or, in other words, seven years for the combined medical and college course. Not six years as proposed by Michigan, provided men take both courses at Ann Arbor. The seven years seem to me to be only fair play as an encouragement to the higher education.

What I wish to write you about in particular, is this: The present regulation is not to give the college men any time credit. The plan originates with medical schools in universities where they have also an arts department. They do allow the medical and college course to be completed in six years instead of eight, but it requires men to go to their college department. Now there are several medical schools requiring a straight B.S. or A.B. degree for entrance, such as Johns Hopkins, Harvard, and Rush in 1907. If men going from colleges * * * will all go to schools requiring the A.B. or B.S. entrance requirement, it will do more to help us to bring the medical schools into line than anything I know of at present. It seems to me the professors in these colleges should bring every pressure to bear on their prospective medical students to get them to go to the medical schools only that require degrees for entrance.

Upon the question of requiring either the B.A. or the B.S. degree as a preliminary to a medical course it is not my purpose to speak further than to say that I do not think the time has come in this country to make such requirement, unless upon the completion of such course the degree M.D. is to be given.

President Hadley has this to say on the general subject of requirements for admission to the professional schools of Yale:

However convenient it might be to insist on the possession of a bachelor's degree by all pupils in the schools of law or medicine, I feel that it would be a violation of our duty to these professions to hedge ourselves about by any such artificial limitations. We should make the standard of admission to our law and medical schools higher than it is at present; but we should base it upon qualifications for professional study which we could test by an examination, rather than upon previous residence at an institution entitled to give a bachelor's degree. If a man is really fit to study law or medicine we should encourage him to study law or medicine with us, without making arbitrary restrictions.

No one will be likely to question the wisdom of President Hadley's remarks, provided the

[8] *Loc. cit.*, p. 326.

[9] *Loc. cit.*, p. 388.