

*On the Effect of Magnesium Salts upon the  
Excitability and Conductivity of Nerves.*

S. J. MELTZER and JOHN AUER.

Numerous applications of solutions of magnesium salts to the sciatic, pneumogastric, depressor and sympathetic nerves of rabbits failed to produce any evidence of excitation, but in each case there resulted sooner or later a profound inhibitory effect upon the conductivity of the nerve under observation. Thus, after application to the sciatic nerve, the conduction of motor and sensory impulses was manifestly inhibited: a strong stimulus applied below the 'block' caused strong contractions of the muscles of the thigh but no pain; when applied above the 'block,' stimulation induced pain but failed to cause contraction. Such effects were obtained with hypertonic as well as with isotonic and even with strongly hypotonic solutions. The weaker the solution the longer it took to establish complete interruption of conductivity. Conductivity could be restored by washing the nerve with Ringer's solution.

WILLIAM J. GIES,  
*Secretary.*

DISCUSSION AND CORRESPONDENCE.

THE GEOGRAPHICAL DISTRIBUTION OF STUDENTS.

TO THE EDITOR OF SCIENCE: In Dr. Tombo's interesting article on 'The Geographical Distribution of the Student Body at a Number of American Universities' which appeared in SCIENCE for October 6, 1905, he was careful to state that 'in the case of Harvard University the students of Radcliffe College (undergraduate women) are not included.' May I amend that statement by saying that only men were counted in the Harvard table, the graduate students as well as the undergraduate students of Radcliffe College having been excluded. Had these 407 students been included, several comparative statements in the article would have been affected. Harvard would have led in the North Atlantic division by 47, and would have gone from fourth place to third place in the South Atlantic division. In the line of grand totals Harvard would have led by 361, Columbia being second and Michigan third. Had women been wholly omitted or

separately counted in Dr. Tombo's table, several rearrangements would obviously have been necessary.

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THE MAKING OF LANTERN SLIDES.

TO THE EDITOR OF SCIENCE: Every one knows how troublesome it is, in the making of lantern slides from a variety of objects, to accurately center the images on the negative plates; how very troublesome it is to get a centered lantern slide from an eccentric negative, and how much time is required in cutting paper mats for bounding the field of a lantern slide. For several years I have employed a method which I find obviates these difficulties almost completely.

In making my negatives I take pains to get the desired size of image, but do not take the trouble to center the image upon the spot where the negative plate is expected (?) to be.

I take these negatives and trim them by means of a cutting diamond to the size of the transparent square desired on the lantern slide. I can trim thirty negatives in fifteen minutes.

I then take my lantern plates, lay them film side up on a black ground, lay a trimmed negative centrally on each, and print by a light held above.

The resulting positives are perfectly centered, and the desired field is sharply bounded by a nearly opaque border which is as satisfactory as a carefully cut paper mat.

W. S. FRANKLIN.

SPECIAL ARTICLES.

ORTHOGENETIC VARIATION.

SINCE I am responsible for the term 'orthogenetic variation,' whilst the far greater idea of 'orthogeny' falls to the credit of the late Theodor Eimer, I am anxious that it should not be misrepresented.

The paper by Mr. Robert E. Coker, entitled 'Gadow's Hypothesis of Orthogenetic Variation in *Chelonia*,' Johns Hopkins University Circular No. 178, May, 1905, calls for some remarks on my part by way of protest and

correction. In my paper<sup>1</sup> I concluded as follows, concerning the observed variations in the number and arrangement of the scutes of the loggerhead, *Thalassochelys caretta*:

These variations from the normal type all lie in the direct line of descent, and the more serious the variation, the further back it points. Moreover the changes necessary to turn any given variation into another one less abnormal until ultimately the normal condition is reached, are not erratic, but stand in strict correlation with each other and proceed strictly on definite lines. I, therefore, call this kind of atavistic variation orthogenetic.

Further, I had suggested that the individual, abnormal turtles 'grow out of these irregularities by the reduction or squeezing out of certain of the scutes,' and I concluded as follows:

Of course there is no proof of what I have tried to explain. Comparative anatomy and common sense tell us it is so. But common sense is not evidence in a sceptical court. The only way of proving the correctness of the view explained in this paper would be to take a number of abnormal turtles and to watch, while they are growing up, if and how they mend their irregular shells and become normal.

I think I had stated the case fairly. It left no doubt about the definition of at least one kind of orthogenetic variation. Of course, it can not be expected that a turtlet with, say, twenty-four dorsal scutes can, during its growth, reduce them to the normal number of sixteen. I had said as much. Many a turtlet probably sticks fast during the mending process. Otherwise there could be no abnormal adult specimens, if my surmise is correct. But if they do amend the number of scutes, these cases of orthogenetic variation are simply ontogenetic stages, passing reminiscences of earlier, phylogenetic conditions. We ourselves begin with an embryonic tail of numerous metameres, and their ultimate reduction to five, four, three or two free caudal vertebræ falls under the same category of variation.

Orthogenetic variation implies something

<sup>1</sup> 'Orthogenetic Variation in the Shells of *Chelonia*,' Willey's *Zoological Results*, Pt. III., May, 1899, pp. 207-222, pls. XXIV.-XXV.

progressive, no matter whether it means increase or decrease in numbers. A case of increasing tendency is, for instance, the striation of certain species of the lizard genus *Cnemidophorus*, of which the small *C. deppei* varies in the possession of seven to eleven white stripes, the eleven-striped individuals and the six-striped *C. sexlineatus* representing the two extremes. A study of these lizards has convinced me that in many cases a limited increase in the number of stripes takes place during individual growth. For detail I must be permitted to refer to my paper.<sup>2</sup>

Mr. Coker does not believe that orthogenetic variation (or, as he says, 'determinate') exists in the *individual* turtle. He holds that my assumption can not be proved by percentages. Whilst I rejected the notion that the irregular number of scutes has any serious influence upon the successful life of the turtles, he thinks it is quite conceivable. But 'at least the more extreme cases may be but one of the evidences of a congenital weakness,' so 'that a greater proportion of abnormal than of normal turtles fail in the struggle.' Of course, this may be the case. But how are we to find it out, unless we watch a number of them growing up? The fact remains that there are adults with very abnormal scutes which do very well. Mr. Coker is indignant at my saying that no less than fourteen per cent. of large turtles were wrong, when I had only one such abnormal specimen out of seven large specimens. I grant that this was the wrong way of putting the case. Let me, therefore, mend the sentence as follows:

No less than twenty-two per cent. of turtles of the considerable size of two feet and upwards, are wrong in their scutes, and do, or did very well, for all we know to the contrary.

On the other hand, he found that of 28 embryos of *T. caretta*, of one nest, only 9 had more than the normal number of 16 scutes, with totals of 17, 18, 19 and 20; whilst two had each one costal too few, making a total of

<sup>2</sup> 'Evolution of the Colour-pattern and Orthogenetic Variation in Certain Mexican Species of Lizards with Adaptation to their Surroundings,' *Proc. Roy. Soc. London*, Vol. 72, May, 1903, pp. 109-125, pls. 3-5.

15 costals and neurals. These two specimens are really very interesting, since they fill the gap in my table, between totals of 16 and 14. This reduction is due to the falling out of a pair of costals, as is shown by comparison of *Thalassochelys* with *Chelone*. This shortage of one costal scute in Coker's turtles is, however, not 'precocious,' since 16 is the normal number in this species, but it is rather prophetic so far as Cheloniidae are concerned. Therefore, no need for him to say that it would be difficult to explain this deficiency as precocity. But I am at a loss to understand what bearing his table on page 20 has upon my hypothesis, considering that all his turtles were embryos, most of them still unripe. I trust that he does not impute to me the belief that the turtles should begin to mend their ways before they are born.

The fact remains that, with the addition of Coker's turtles to the 76 specimens enumerated in my table, the percentage of specimens with supernumerary scutes still decreases steadily with age. Of course, such calculations are always precarious unless they are based upon very great numbers, and it is not unlikely that the amount of variation in the members of one brood differs much according to clans, or regions, not to mention the parents about whom we know nothing.

	Per cent.
Of 78 embryos or new born, 53 are abnormal	=7
Of 9 specimens from 3 to 8 inches incl., 3 abnormal	=33
Of 19 specimens from 8 to 24 inches incl., 5 abnormal	=22
Of 9 specimens from 24 inches to "large," 2 abnormal	=24
Of 7 large specimens, only 1 abnormal	=12

Still more surprising are Mr. Coker's conclusions as drawn from the examination of 250 specimens of the diamond-back terrapin, *Malacoclemmys centrata* Latr. Examination of 250 individuals of all ages and sizes in the condition just as they were caught. He found, however, a few cases which indicate that partial fusion, or division, of neighboring scutes has been going on during the creature's life. The occurrence of reduction by fusion, of increase by division during the individual's growth is, therefore, demonstrated. Would it not go a long way in support of my hypothesis if actual observation showed that the total number can be reduced during life by one

single scute? I ought to feel grateful for such a help, willing to waive the experimental proof, and be glad to accept fusion instead of my suggested squeezing out. Lastly, I ought to feel crushed by Mr. Coker's table, which shows, if anything at all, that in the diamond-back the scutes seem to increase instead of decreasing with age. The author himself is confirmed in the 'belief that in *Malacoclemmys* we do not have orthogenetic variation in the sense that there is normally in the individual life history a progressive reduction in the number of scutes.'

No, of course not. The whole investigation has ended in a farce. A beast which frequently goes in for the longitudinal splitting of its true neural scutes is not a fit subject for the study of orthogenetic variation. This is malformation pure and simple; it is as little atavistic, or in line with anything, as are six fingers in man, or five toes in fowls.

I do not profess to know what the diamond-backs are after. Perhaps this crazy splitting may in time establish a new type of chelonian with a completely double series of neural scutes! However, Coker himself did not fail to see that in these creatures 'the tendency is not toward, but away from the type.' He should, therefore, have ruled them out of court. But there surely is also a *beyond the type*, and this seems to have puzzled him sorely, as is shown by his footnote 3 on page 18 of his paper. For instance, if (whether congenital, or due to fusion, division or suppression) a loggerhead or other marine turtle has only five neurals, that would be something beyond the type of Cheloniidae, and in this respect very abnormal, but still orthogenetic, since reduction to five neurals has been realized and has become the normal condition in *Pleurodires* and even in certain species of *Testudo*. Such a case in a marine turtle would be, so to speak, prophetic, and quite reasonable, and a trained morphologist would recognize that such things are 'quite in the line' of tortoises.

My appeal to comparative anatomy and common sense has been of little avail. I venture to make a practical suggestion. The eastern states are the home of many kinds of

chelonians. Ponds, creeks, marshes and rivers are swarming with them, and surely a place could be found which only requires fencing to be turned into a scientific, experimental 'crawl,' where some hundreds of turtles of a suitable kind could be turned loose, labeled, of course, and examined from time to time whether any of them are amending their abnormal shells. For physical reasons such an experiment of possibly far-reaching, fundamental importance, can not be made in Europe.

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September 4, 1905.

#### NOTE ON VECTOR SYMBOLS.

THE question of notation was always of importance, and the introduction of new methods depends a good deal on it. It is, of course, highly desirable to have *one* system of notation used by all the scientific world, but at the same time it is also desirable that the system in use shall be a simple and easy one.

The vector-analysis becomes more important every day in the study of physics. It is, therefore, necessary to have a vector notation as simple as possible. The notation used to-day is far from being uniform, and still the tendency is towards the introduction of German letters for the symbolization of vectors. May be that when printed, the German letters look well, and are well distinguished from the Latin alphabet. The student, no matter of what nationality, can learn to recognize them just as he learns to recognize the Greek alphabet. But the question is the *writing* of the German characters. Those that try to picture a German print-letter on the board when necessary to use the vector symbols in a lecture before a class will know how difficult this is. And to use different signs when written and different signs when printed does not seem reasonable. Why not use as symbols for vector quantities the Latin alphabet? The Latin alphabet is almost universal, and there is no difficulty whatever to write this alphabet. I, therefore, propose—with Professor Karl Heun in Germany—to use the following notation for vectors, a notation as simple as it can be.

All vector quantities are written as follows:  $\bar{a}$ ,  $\bar{b}$ ,  $\bar{d}$ ,  $\dots$ ;  $\bar{A}$ ,  $\bar{B}$ ,  $\bar{G}$ ,  $\dots$ , and their tensors respectively:  $a$ ,  $b$ ,  $d$ ,  $\dots$ ;  $A$ ,  $B$ ,  $G$ ,  $\dots$ . The scalar-product of two vectors  $\bar{a}$  and  $\bar{b}$  is written:  $\bar{a}\bar{b}$ , which, by definition, is  $= ab \cos (a, b)$ . The vector-product of two vectors  $\bar{a}$ ,  $\bar{b}$  is written thus:  $\bar{a}\bar{b}$ , and because it is itself a vector it can be written:  $c = \bar{a}\bar{b}$ . The tensor of this vector:  $c = ab \sin (\bar{a}, \bar{b})$ . The unit vector can be written, for instance,  $\bar{a}_1$ ,  $\bar{b}_1$ ,  $\dots$ , so that  $\bar{a} = a\bar{a}_1$ ,  $\bar{b} = b\bar{b}_1$ ,  $\dots$ .

The advantage and simplicity of this system of notation speak for themselves.

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August 8, 1905.

#### THE OCCURRENCE OF ICHTHYOSAUR-LIKE REMAINS IN THE UPPER CRETACEOUS OF WYOMING.

THROUGH the kindness of Professor S. W. Williston, I have recently received two fragmentary vertebræ found by Mr. W. H. Reed in the Benton Cretaceous near the north end of Medicine Bow Mountains. Although very fragmentary, these vertebræ appear to represent a genus allied to *Ichthyosaurus*. As this reptilian group has not been known in North America in beds as late as the Benton, the discovery is of considerable interest.

The larger fragment consists of the upper three fourths of a deeply biconcave vertebral centrum apparently from the cervical or anterior dorsal region. The centrum is very thin antero-posteriorly, and in this respect somewhat resembles the corresponding centra in *Baptanodon discus*. The sides are considerably damaged, so that it is not possible to make a definite statement concerning the rib articulation, but it seems to have been double, as in *Baptanodon*. A foramen close to the upper end of the diapophyses is larger than any I have seen in this position in the typical *Ichthyosaurs*, and adds somewhat to the difficulty of making a definite determination of the relationships of this form.

When more material is available it will be interesting to learn whether this form really represents a true *Ichthyosaurus* or possibly a more highly specialized form of *Baptanodon*.