of the arthrodira, points out similarities in *dental plates,* but these might ally them as well to chimæroids as to dipnoi—in the *shape of the caudal fin* and its supports, which are scarcely more dipnoan than shark-like (pleuracanth) or ganoidean—in *persistent notochord,* which might be as well shark-like, dipnoan or chimæroid,—in *punctation of dental plates,* which is a character by no means exclusively dipnoan. So that one may, I feel, hardly conclude with Eastman that the lung-fish (*Neoceratodus*) recalls "in its entire organization, save for the absence of dermal trunk-armoring, the principal features of the arthrodires," or that there are present between the modern lung-fish and the ancient arthrodire, "such intimate structural resemblances [that they] can not be explained by parallelism but point plainly to common descent." Eastman is willing to admit, on the other hand, that the evidence is questionable that arthrodires had a vestige of ventral limbs, and that they are unlike dipnoans in possessing a shoulder- and ventral-armoring. But even if we can picture such a *paleozoic descendant* of primitive lung-fishes, can we still imagine one which lacks also pectoral limbs, and opercular bones, and which possessed on the other hand shoulder joints rendering possible a curious dorso-ventral movement of the head?[2] Certain it is that the resurrected doctrine of the kinship of arthrodira and lung-fishes finds little support in the recent studies of Hussakof and others, which have shown that the gap between the arthrodira and the pterichthyids is by no means as wide as we have hitherto taught.

[2] Eastman can answer these objections only by minimizing their value, as when he maintains that the operculum is represented in the rudimentary spine of *Dinichthys,* and that the movable attachment of the rib to the cranium in *Neoceratodus* is comparable to the intermovement of head and trunk in the arthrodira. In his comparison of the gnathals of arthrodira with the splenial of dipnoans, he calls attention to a fleck of cartilage fossilized on the outer (*ectal*) face of a gnathal of *Dinomylostoma* as evidence of its attachment to a meckelian cartilage; but this evidence, even if accepted, would be as readily ganoidean as dipnoan. It may be remarked, however, that the structure in question is too obscure to warrant a definite judgment as to its nature, and the fact that the ectal surface of such a plate is sometimes known to bear tubercles quite like those of the usual head plates does not make the assumption probable that the gnathal plates were placed far from the surface of the head.

Bashford Dean
Columbia University

Evolution Theories: Static, Determinant, Kinetic

In Science for December 7, 1906, Dr. Ortmann presents another of his series of reports upon the kinetic conception of evolutionary processes. It is very gratifying, of course, that my suggestions are receiving so much valuable time and attention, and the more to be regretted that unfortunate methods of study still interfere with the success of so persistent an inquiry.

Would it not be better, for example, to simplify the issues by omitting the discussion of the novelty or antiquity of the ideas, or at least by postponing it until the ideas themselves have been clearly perceived? It will then become evident to Dr. Ortmann that Darwin and many others have entertained kinetic views of evolution, though not bringing them to the point of definite formulation.

In estimating the value of an interpretation which differs from our own it is well to suspend or lay aside temporarily the opinions we have been entertaining, in order to see how the alternative theory accommodates the facts. But instead of making a personal inspection of the kinetic premises, Dr. Ortmann ties himself fast by italics of certitude to his static dogma: "*If the environment remains uniform, perfect uniformity of individuals will result.*" This keeps him far outside of the subject upon which he continues to inform the readers of Science.

Viewed at the long range imposed by this fictitious barrier, many things look quite the same which would be found very different on closer inspection. Thus it appears to Dr. Ortmann that symbasis is the same as amphimixis, whereas the two processes are on distinct lines and work in different directions.

Amphimixis occurs when variations originated under conditions of narrow breeding are brought again into more normal relations of broad-breeding, or into renewed contact with the unrestricted descent of the species at large. Mutative variations are often obliterated by cross-breeding, and replaced by the normal characteristics of the wild type. Amphimixis means that narrow varietal strands can be retracted and reincorporated into the specific network.

Symbasis is the free interbreeding of the normally diverse members of a species, which brings about the coherent evolutionary progress of the whole network of descent. Symbasis keeps the procession moving, while amphimixis rescues the stragglers from the side-paths. Amphimixis corrects abnormal diversity induced by narrow breeding, but does not interfere with normal diversity, nor with evolution, and only appears to do so when the degenerative mutations of narrow-bred organisms are looked upon as genuine examples of evolution.

The static assumption is that the species remains uniform and stationary until acted upon by some agency which is external, or at least intermittent. This was a very natural assumption to make in the early days of evolution because it involved the least possible modification of the earlier theory that each species was the product of a definite creative act. Under the static theory the species could still be held to be ideally uniform. Evolution could be charged to the environment, which was known to be able to influence the development of individual organisms, and could therefore be thought of as influencing whole species. This idea of definitely directed variation has been called *mutation* by Waagen and *orthogenesis* by Eimer. The formation of new species by discontinuous or saltatory variations has also been called *mutation* by de Vries.

All these conceptions are static, like the Darwinian theory of natural selection. They do not permit us to pass beyond the barrier of ideal uniformity and stability, and forbid us to find causes of evolution in anything except environmental influences. Dr. Ortmann's reasoning on the immanence of environmental causes appears to be entirely logical, but it can convince only those who disregard the facts of nature and accept the static assumption as the basis of inference.

The second alternative conception of evolution was that of Naegeli, who believed that evolutionary causes might reside in the protoplasm itself, and who worked out a theory of protoplasmic structure which would provide for systems of changes in definite directions. This excited the 'hereditary mechanism' speculations of Weismann and his successors, which continue to the present day, though it has been usual to invoke environmental causes to change the workings of the 'hereditary mechanisms' and to cause them to yield new forms, which are supposed to be preserved by selective or other isolation.

The kinetic theory differs from its predecessors in recognizing that evolution is neither initiated nor actuated by the environment. Variations appear without environmental causation and are preserved and accumulated in the species by prepotency, instead of by isolation. Isolation and narrow breeding bring the degeneration which amphimixis cures, but inside the normal network of descent individuals are diverse and new variations are prepotent. Symbasis weaves the diversities and the new characters together in endlessly varying proportions, and in this way conducts a constructive, coherent evolution, a gradual advance of the whole network of descent of the species.

Such evolution Dr. Ortmann declares to be incomprehensible, and so it may appear from his static point of view. But the difficulty can be surmounted if he will take the italics out of his *ipse dixit* of uniformity and allow himself to become acquainted with the phenomena of heterism, the contemporaneous non-environmental differences which everywhere exist among the members of species, and even among the simultaneous offspring of the same parents. The normal diversity and free interbreeding by which evolutionary motion can be accomplished are concrete and

well established facts, while the ideal of stable uniformity under changeless conditions remains a pure speculation.

O. F. Cook

Washington,
December 19, 1906

SPECIAL ARTICLES

IONIZATION IN CLOSED VESSELS

In connection with some other work on the ionization in closed vessels it was found necessary to examine the daily variation of this ionization, to find what parts of the day it is most constant and to find the best methods to get as constant an ionization as possible. Soon after the work was started the article of Wood and Campbell on the 'Diurnal Periodicity of the Spontaneous Ionization of Air and other Gases in Closed Vessels' appeared.[1] It was thought that it would be of interest to find the periods of maxima and minima in Baltimore.

Dike[2] has also found a similar periodicity in the amount of radio-active emanation in the atmosphere and his periods agree quite well with the periods as found for the ionization in closed vessels. It would thus seem that the variation of the ionization in closed vessels was due to a variation in the penetrating radiation and that by screening this off by the use of thick lead plates one ought to get a much more constant ionization.

The ionization was measured by means of an iron electroscope 10 x 13 x 20 cm. in size. The charged electrode was bent into the arc of a circle and over this arc the gold leaf fell as the charge leaked off. The electrode was charged across a small air gap and so was air-tight. All parts except the charged electrode were earthed. The position of the gold leaf was read by means of a micrometer microscope, the cross hairs moving in the eyepiece. As the microscope was firmly clamped, the same portion of path traversed by the gold leaf would be always used. The air was enclosed some twenty days before readings were taken. The electroscope was not allowed to become entirely discharged at any time.

An electroscope similar to the above but smaller in size was also used. The readings were made in a tower room on the fifth floor of the physics laboratory. The room was not heated artificially, so that the temperature remained fairly constant. All sunlight was screened off.

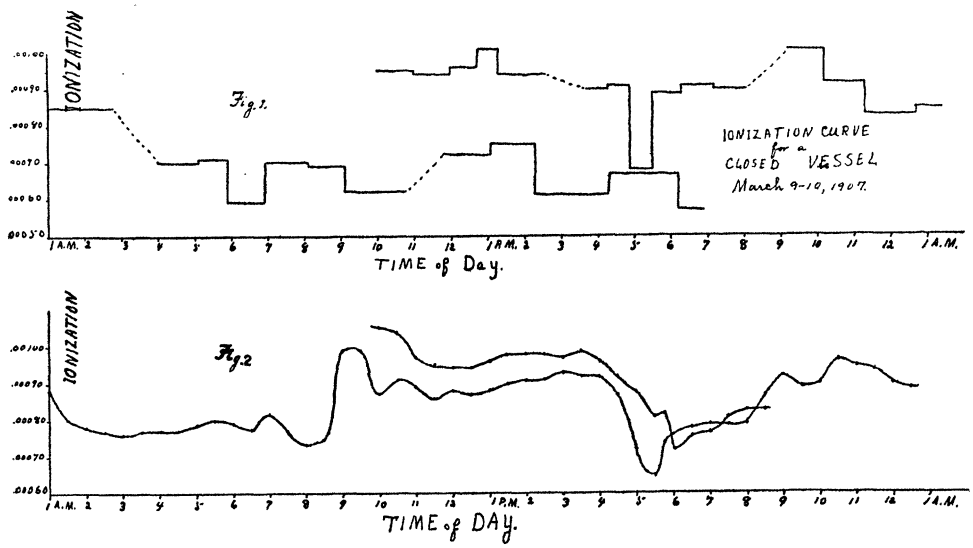

Fig. 1 represents the ionization for March 9–10, 1907. About midnight it began to snow. It will be seen that the value of ioniza-

[1] *Phil. Mag.*, Feb., 1907.

[2] *Terr. Mag.*, Vol. XI., No. 3, p. 128.