tivity was originally derived from Michelson and Morley's experiment, its very *"experimentum crucis,"* its *decisive experiment,* is based on Fresnel-Fizeau's effect. Undoubtedly in the certainty of result this laboratory experiment very much surpasses Michelson and Morley's so-called ether drift experiment. This depends on the velocity of the earth in an hypothetical way; that velocity is a circumstance outside the laboratory, quite beyond our control, not changeable, not reversible. In Fizeau's experiment, on the contrary, the main cause of the effect and all details are well controllable, exactly determinable and they may be changed at will.

Now then, this *"experimentum crucis"* was not even mentioned in recent discussions! Is this experiment, so emphatically pointed out as decisive, now quite worthless? What remains then of the alleged steel logic in Einstein's theory?

My paper[1] shows that the usual interpretation of Fizeau's effect, in the sense meant by Fresnel's dragging coefficient, derives from a subtle error. This I explained more in detail and confirmed in my book "Nouvelles Vues Faraday-Maxwelliennes" with "Supplément. Sur la Propagation de la Lumière" (Gauthier-Villars et Cie, Paris, 1924). My paper "On Kinematics,"[2] treats the same fundamental question in another way. According to my result the true sense of Fizeau's effect is quite different from what we formerly admitted, it being the reserve. However, this in no way lessens the certitude and importance of the experimental result.

Professor Dayton C. Miller claims a result for his repetition of Michelson and Morley's experiment, which at the most is 30 per cent. of the calculated effect. The discussion has shown that that result is questionable on account of several grounds. In this respect I quite agree with Professor Einstein. That is to say, Professor Miller certainly observed an effect of the given magnitude, but the question remains: is it due to the alleged ether drift? There is no uncertainty of this kind in Fizeau's experiment. The recent excellent experiments by Professor P. Zeeman, Amsterdam University, fully confirm the formula for Fizeau's effect established according to my views, without introducing any hypothesis. In fact, Zeeman's experiments, which are universally acknowledged to be correct, confirm my formula to practically 100 per cent.

Zeeman's result is the decisive disproof of Einstein's theory.

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1 *Comptes Rendus,* CLXXV, p. 574 (1922).
2 *Philosophical Magazine,* XLIX, p. 579, March, 1925.

IODINE IN THYROID DEFICIENCY

PERHAPS in addition to Miss Simpson's statements (SCIENCE, February 5, 1926) regarding the use of iodine in thyroid troubles, certain remarks by Boussingault in his "Viajes Científicos a los Andes Ecuatoriales" (Spanish translation by Acosta, I have not the original French text) may be of interest. In one "Memoria" he definitely states (1825) that "till now, iodine is the only specific known for goiter." Elsewhere he continues, regarding certain mineral springs in Colombia, "In the province of Antioquia no other salt is used, save that from these peculiar springs, whose waters I have analyzed and convinced myself that, though the composition of their salts is variable, there is in all an appreciable amount of iodine. Hence the reason that there is no goiter in Antioquia: each inhabitant takes every day a dose of iodine with the salt he consumes." Again: "It is a singular fact that for more than a century these waters have been recognized as a sure specific for goiter."

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SCIENTIFIC BOOKS

The Biology of Fishes. BY HENRY M. KYLE, M.A., D.Sc., Sidgwick & Jackson, Ltd., London.

KYLE'S "Biology of Fishes" is a very complete and useful work, covering almost every phase of fish life and generally in touch with the latest investigations in anatomy and physiology. The author recognizes fully that the fishes constitute an expanding and diverging as well as very ancient type. The reptiles, birds and mammals are, so far as their origins are concerned, all of them divergent groups which have arisen from the fishes, and in some regards no more different from their primitive ancestors than these differ from one another.

One does not willingly criticize so excellent a book, but a few minor points may be noted. Apparently common heredities are not adequately considered as compared with likenesses due to surroundings. The mass of fishes would appear to form a pool from which individual structures and customs can be drawn out for examination without much reference to heredity, although the latter is the basis of rational classification. The distinctions between homologies and analogies are not always clear in the author's statements and structures are often assigned to secondary cause of recent date, when their real origin may be far older and even beyond the reach of investigation. The true sources of most structures must be sought in heredity rather than the immediate response to environment on the part of a living species.

Dr. Kyle has a curious way of personifying the individual fish, by treating as voluntary actions which, so far as we know, are wholly automatic or instinctive. "The trout itself, like other fresh water fishes, has to keep a wary eye on the pike, the otter and other enemies from above. No wonder he is clever and prefers to seek his food by night or when the water is perturbed." This is apparently a vivid way of saying that the trout is alert and timorous, not that each individual thinks the matter over in deciding when to feed.

Again, "Gulls seem to know that fishes can not hear, and keep up a ceaseless chattering and screaming as they fill themselves with the rich booty." As we watch the fish in its daily operations, it is not easy to refrain from reading our own thoughts and our own springs of action into those active but largely instinct-controlled creatures.

The effort to trace a mechanical cause for each special structure sometimes carries our author far into speculation. Thus—"in other species, the feeble development or absence of teeth indicates a disturbance of the growing tissues, and this has resulted in the formation of a long snout, as in the Sail-fish and the Sword-fish. In other cases the disturbance has led to the temporary closure of the mouth (as in the Pipe-fishes and Flat-fishes) and this to a great elongation of the body with increase in number of the vertebrae, loss of the ventral fins and even of the caudal."

The conception of "mutations" or sudden changes in certain characters, leaping outside of or beyond heredity, is accepted by Dr. Kyle, and I think unduly extended. For example, he suggests that the primitive fish was "possibly a mutation from some larval form of lower degree at the beginning."

The earlier and less specialized fishes have an air duct connecting the air bladder with the gullet. Perhaps one third of the known species lose this duct at maturity, such being in general the more recent and more specialized forms, few of them apparently dating before the Tertiary. The loss of this duct, our author ascribes to a "mutation." It is more or less concurrent with numerous other changes, which appear by degrees, the forward movement of the ventral fins, the reduction of the number of their rays, the specialization of the vertebrae, with usually their reduction in number, the presence of fin spines, the rough (ctenoid) edges to the scales, and a variety of other characters, in addition to this "mutation."

He says:

> Mutations do not break through any law of nature, but they make things difficult for the natural selectionist. Organs like the fins and open air bladder duct, which are of proven utility, should not change or disappear. Yet a very large number of the Teleosts have come from just these two mutations.

The fact is that about one third of all fishes have progressively lost certain primitive traits which have been replaced by other characters, and we can only guess as to how it was done. Doubtless our author is right in not ascribing such changes to selection alone, for, as he says, "the elimination of weaklings and the unfit is not a true cause of the formation of a species." It is a negative process, trimming up or keeping in form the mass of the species itself, holding it up, as it were, to its highest efficiency. Those individuals who run the gauntlet of life, whatever its nature, leave descendants endowed with like potentialities.

How, in the lost history of the past, one species gives place to another we can only speculate. It is true, however, that every species, natural or artificial, is modified by some sort of selection, no individual escaping from such influence. In every case we know of, a given species is set off from its relatives, by some sort of isolation, with segregation. Heredity and variation represent inherent tendencies. The waves of life are checked or turned by the external obstacles encountered. No species can cross a barrier without finding new conditions, new climate, new foods, new enemies. In time every barrier surmountable leaves its impress on the group on either side of it. Every student of species, men, animals, plants, languages, has recognized this fact. No sound conception of evolution can leave geography out. Some day, evolutionists will again come back to the dictum of the great master of zoogeography, Moritz Wagner, "Ohne Isolirung keine Arten" (without isolation, no species).

The word mutation is used vaguely by many authors. Sudden changes of the minor order occur frequently and may sometimes give rise to a hereditary series. but this occurs only when separated, by natural or by artificial means, from breeding with the mass. Mutations, as distinguished from mere freaks, are common enough in nature but perhaps in no conceivable case do they mark the origin of a species. These variants are readily cultivated or bred, but they disappear whenever thrown on their own resources. This may be due to interbreeding with the selected, outnumbering and prepotent mass of the species; sometimes from inherent weakness or other cause traceable in the individual case. So far from "mutations" in the de Vriesian sense being the method of the Origin of Species, no actual student of species in the field can accept a theory so divergent from actual facts.

Mutation, in Dr. Kyle's scheme, is not very clearly defined, but the process to which he refers is apparently something not being instantaneous but taking long ages to accomplish and not concerned with any single character of a single species. If not defined in some such fashion, it has no reality. Mutation, as viewed by de Vries, is a sudden change within a species, which gives rise to a new one, closely akin, but with a break in heredity, the new species persisting, and at times replacing the old *within the same environment*. If the new and old are competing, selection may decide.

The conception of the origin of species by mutation, now accepted by many authors, especially botanists, rests on the slightest of foundations. In most groups are found "geminate" or twin species, closely related. Scarcely ever are these twins found in the same region, scarcely ever far apart, but always separated by some barrier, land, water, climate, food, space or enemies. Such separation saves variants, however originated, from being lost through interbreeding with the mass. Forms separated by barriers are subject to new selections; they have new gauntlets to run. They are not products of selection in competition with the old stock, for in no case as far as I know are their characters of survival value, as was supposed by Darwin. Nor is there any adequate evidence for presuming species to appear suddenly, "full-fledged," from germ cells of an old species. The belief in this process is, I think, one of the myths of science.

In Dr. Kyle's work, so complete and suggestive in most regards, I find no important account of the origins and relations of orders and families nor of the origins of species. The lower differences come first in nature and the higher problems are largely beyond our reach, in the realm of speculation. In the history of science guesses, however brilliant, have rarely proved true.

Taxonomy, with Cuvier, is primarily the systematic way of stating the known facts of comparative anatomy. Now that we recognize that comparative anatomy is itself a statement of the trend of evolution, classification has naturally become the expression of evolution. A complete biology of fishes should elucidate this.

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SPECIAL ARTICLES

ON THE EQUILIBRIUM BETWEEN THE ENAMEL OF THE TEETH AND THE SALIVA

IN studying the equilibrium between sea water and calcite[1] it was found that crystals of calcite had to be very clean in order to get an equilibrium by shaking with sea water in a thermostat in any practical length of time. If clean crystals of calcite (or aragonite) were shaken with sea water, an equilibrium was established in a few hours, but if ground-up shell or coral or limestone mud was shaken with sea water, an equilibrium was not even approached in three or four days. In studying the equilibrium of enamel of the teeth and the saliva at different pH values, it was thought that this factor complicated the results. Therefore studies on crystals of fluorapatite [$CaF\ Ca_4\ (PO_4)_3$] were made, using an artificial saliva containing no proteins or capillary active substances that would tend to deposit on the surface of the crystals.[2] At pH between 5.5 and 6.5, the apatite was practically at equilibrium with this artificial saliva containing the same quantity of calcium phosphate as in normal saliva. From this data and also from the fact that impure masses of calcium phosphate deposit on the teeth, it was concluded that at a pH between 6 and 7 the enamel of the teeth should not dissolve in the saliva.

It therefore seemed desirable to find a method of cleaning the surface of the enamel of the teeth so that an equilibrium with the saliva could be established. In this case, theoretically, the enamel should grow with increase in pH and dissolve with decrease in pH.

Since there is a general impression that gritty substances used on the teeth will wear away the enamel, the following observation was made using calcite crystals newly formed and therefore clean on their surfaces and sharp at their angles, to see whether such an abrasive would wear away the teeth. Calcite crystals, which were rhombohedral and of large enough size to feel gritty, did not perceptibly wear the enamel in a total of sixty hours polishing with the dry powder and dry brushes. Only one experiment was performed. During the five years from the age of thirty-five to forty, the teeth were brushed two minutes a day with a dry brush, and during this time hard deposits collected on the teeth, which were removed by the dentist. These deposits seem to collect on rough places. During the next five years, from the age of forty to forty-five, the teeth were polished for two minutes daily with the dry calcite crystals on a dry brush. The roughness gradually disappeared and no hard deposits occurred on exposed surfaces of teeth during this period.

Calcite crystals were formed by nearly filling a one hundred liter jar with water and allowing molecular solutions of sodium carbonate and calcium chloride to run in on opposite sides of the jar at the rate of

[1] Proc. Nat. Acad. Sci., 3: 692, 1917.

[2] *Jour. Dent. Research*, 3: 50, 1921.