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.