

limitation effect in the upper part of the Appleton region, the electron density even in the most heavily ionised part of this region having become insufficient to 'reflect' the very high frequency waves even at grazing incidence. My suggestion was, then, that the very striking coincidence in date between the two phenomena pointed to a similarity in the mechanism of ionisation in the two regions, which must not be overlooked in discussion of the rival claims of ultra-violet light and corpuscles as the effective agents for one region or the other.

The argument is now very notably reinforced by the fact that Wilkins again heard the 10.06-metre signals from Rome, and heard also signals on 10.02 metres from Oslo (the third harmonic of a telegraph station) on May 2, 1932, daily observations throughout the ionospheric winter thus so accurately delimited having failed to give any trace of signals between Nov. 2 and May 1. The signals from Sardinia (9.8 metres) reappeared on May 6. We are without detailed long-wave data for 1931-32, but it should be remarked that 1924-25 and 1931-32 are approximately symmetrically disposed about the somewhat indefinite 1928-29 maximum of the present solar cycle, so that the comparison here made is the next best thing.

It would be unwise to infer forthwith an identical mechanism for the two regions, but clearly both involve agencies of solar origin, and both have curves of annual variation in which a six months' 'summer' gives place to a six months' 'winter', and vice versa, on dates common to the two regions. The dates are somewhat oddly displaced relatively to the astronomical reference points of solstice and equinox, yet they suggest a twelve-monthly variation rather than one related to the two equinoctial maxima of magnetic disturbance.

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<sup>1</sup> *The Observatory*, March 1932, p. 80.

<sup>2</sup> Radio Research Board, Report for the period ended March 31, 1929, p. 34.

### The Tectonics of the Albertine Rift

It has been represented to me that misconception with regard to this matter, so far as my own views are concerned, may arise in consequence of my passing, without comment, some remarks made by Prof. J. W. Gregory in his able review of Prof. Bailey Willis's book, "Living Africa".<sup>1</sup> Gregory quotes my early description of the local rift valleys correctly thus: "fracture valleys running along linear upwarps".<sup>2</sup> That was the view I put forward in 1921, but we have learnt a good deal since then, and Sikes pointed out to me, in 1925, that the faults I invoked to explain the Albertine rift are of a type difficult to accept, because of the immense depths from which they would have to emerge. I agreed; but this in no way weakened my general thesis. Writing to my friend Prof. Arthur Holmes, I afterwards suggested that the rift fractures rose from nearly flat soles, after the manner of the highland thrusts; but he at that time was unable to agree, and I delayed publishing this view until further evidence should be forthcoming; and then, in 1928, Bailey Willis produced his ramp hypothesis to explain the Dead Sea Valley—an explanation more complete but essentially similar to that which I had suggested to Holmes to account for the Albertine depression and the high scarps bordering it. I adopted the ramp explanation, as I stated, in Pretoria in the following year.<sup>3</sup>

That the Albertine Rift did not originate along the crest of a linear upwarp has now been clearly demonstrated by our studies of the riverine history of

Uganda; for it is found that the eastward drainage away from the present escarpment did not commence until the rift valley appeared, and is an inversion consequent upon the uprise of the valley sides; nor did this occur at the inception of the rift. The main reversal indeed was not until late-middle or post-middle Pleistocene times, when renewed movement gave us the present scarp topography.

It is interesting to note that further evidence in support of the compression hypothesis is provided by recent petrological studies. It is brought out by Holmes and Harwood's work on "The Petrology of the Volcanic Fields East and South-East of Ruwenzori, Uganda",<sup>4</sup> and still more strikingly by A. W. Groves in some as yet unpublished work on rocks in the rift valley zone in Uganda, wherein he shows that crushing becomes increasingly pronounced as the rift is approached, and reaches its maximum in mylonisation in the neighbourhood of the faults. I have Dr. Groves's permission to mention this.

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<sup>1</sup> NATURE, 128, 89, July 18, 1931.

<sup>2</sup> *Geog. J.*, 74, p. 133.

<sup>3</sup> Vol. 2, *Compte rendu*, XV. International Geological Congress, South Africa, 1929, pp. 323-353.

<sup>4</sup> NATURE, 128, 977, Dec. 5, 1931.

### An Inherited Abnormality in Rhode Island Red Poultry

A SEMI-FEATHERLESS condition, associated with dwarfism, occurs in several breeds of poultry. It is usually attributed to some food or vitamin deficiency. Dakin and Hamilton described specimens in 1928 and suggested that the condition might be due to sub-thyroidism.<sup>1</sup>

In 1929 certain facts suggested to us that the abnormality might be an inherited character, and breeding experiments were undertaken. Three Rhode Island Red males of this type were reared and at about eleven months old appeared to be practically normal. They were mated to normal hens and produced chicks of both sexes showing every conceivable degree of the abnormality. Some were almost completely naked even at six months old, whilst others seemed almost normal throughout. Both sexes showed the abnormality, but the most extreme cases were nearly all males. All the extremely abnormal specimens died, but the less abnormal of these  $F_1$  individuals survived, and the best of the females were mated to a male from a normal stock. This mating also produced chicks of both sexes showing every degree of abnormality. The worst of the  $F_1$  females which survived were mated back to the parent cock to test whether such a mating would produce living offspring. The percentage hatched was as high as in a normal stock, and again every conceivable degree of the abnormality was produced.

That the condition is not due to infection or other environmental factor is practically certain, for a few birds from an unrelated stock, hatched and reared at the same time under the same conditions, showed no sign of the abnormality. It appears certain, therefore, that the condition is inherited, but the exact mode of its inheritance is not yet known. It is apparently not sex-linked.

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<sup>1</sup> *Proc. Zool. Soc.*, London, 1928.