

water vapour, which of course would be sufficient to account for that large effect.

Sir William Crookes mentions that, according to his experiments, water vapour produces a greater conduction at very low pressures than air; but I decidedly disbelieve the difference to be in any way similar to the numbers given by Mr. Brush, in consequence of a reasoning mentioned at the end of my *Phil. Mag.* paper, and I think the simplest explanation to be afforded by this source of errors vitiating the pressure indications.

It is no mere hypothesis but a certain fact that water vapour is being evolved by heated glass, and probably many other substances. Sir William Crookes gathers quite a number of arguments for it from his own researches, amongst others spectroscopical proofs. The same opinion was put forward by Kundt and Warburg, who were led to it by the very same sort of experiments on conduction of heat as Mr. Brush's, which they made as early as 1875 (*Poggendorff's Annalen*, 156, p. 177). Further investigations on the hygroscopical properties of glass, and on means for partially removing them, were published by Warburg and Ihmori (*Wiedemann's Annalen*, 27, p. 481).

I cannot prove, of course, that there is no new gas evolved, but I maintain that whatever facts Mr. Brush has put forward as an evidence for its existence, can be explained quite simply by the presence of water vapour (perhaps also other condensable vapours), which he seems to have overlooked. I do not think it necessary to go into details, and to analyse more thoroughly the—rather fantastic—speculative part of the paper, where scarcely any statement is not open to serious objections.

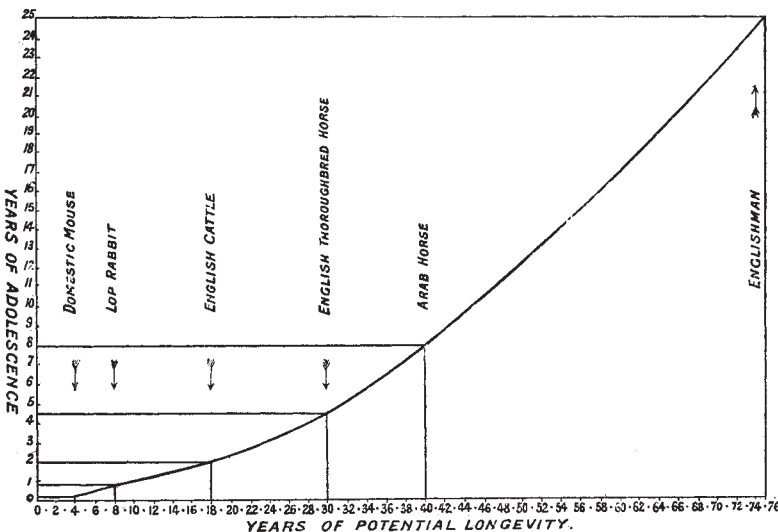
Although thus I differ from Mr. Brush very much in respect to the interpretation of his recent results, I think his elaborate experimental investigations, as reported in his *Phil. Mag.* paper, to be of great value for the theory of these phenomena; certainly it would be very desirable that he might carry on those researches, as he promised there to do.

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The Curve of Life.

THE relationship between the duration of adolescence and potential longevity in different species of mammals has repeatedly been the subject of speculation. M. P. Flourens, in his work



on "Human Longevity," made the ratio between the two periods as 1 to 5; Buffon had previously concluded that it was as 1 to 7. In neither case were the data sufficiently numerous and trustworthy to make these figures generally accepted. In the course of some investigations on the variability of the adolescent period in different breeds of the same species among certain well-known mammals, I have satisfied myself that a relationship exists between the duration of growth and the length of an animal's natural life; although it is evidently not of the kind suspected by the older writers. It may be stated as follows:—The ratio of length of adolescence to length of life in the

shortest lived mammals is proportionally less than it is in longer lived mammals. For example, the period of growth and development of the domestic mouse is, according to my informant, a breeder of these small rodents, about three months. Its natural lifetime is four years. In other words, the mouse may be expected to live about fifteen times its adolescent period as a mature animal. The Arab horse, according to a well-known authority, arrives at maturity in about eight years, its lifetime is about forty years; that is to say, the animal lives four times the length of its adolescence as an adult. Man, on the other hand, who only completes his growth by the union of the sternal epiphysis of the clavicle to its shaft at the age of twenty-five, has, after passing his fiftieth year, or "the middle arch of life," to use Dr. Farr's phrase, only another twenty-five years' expectation of life. His potential longevity accordingly foreshadows a period of maturity not greater than twice the length of his youth.

I have obtained, through the kindness of numerous correspondents interested in breeding and rearing of farm and other domestic animals, the approximate lengths of these two periods in a few well-known mammals; and the accompanying diagram shows the relations between growth and longevity among the same animals plotted as a definite curve. This result was entirely unexpected by me, and it may be interesting to some of your readers.

W. AINSLIE HOLLIS.

Hove.

The Alleged Destruction of Swallows and Martins in Italy.

IN your issue of December 22, 1898, I read the report of a conference held by the Society for the Protection of Birds, at which a paper was read on the decrease of swallows and martins coming to England, giving, as a reason for this decrease, the netting of thousands of these birds on their arrival at the Italian shores, and their subsequent consumption as food.

May I venture to remark that, during a residence of some years in Italy, I have never once seen a swallow, or any member of its family, exposed for sale, and that I have never known, or heard of, an instance of their being netted in the manner described, though I am well acquainted with nearly every part of the Peninsula.

Since reading the above mentioned accusation against Italy, I have asked several Italians whether they knew of such a practice, and am informed that it is simply non-existent, the swallow being, perhaps, the one bird in this country which is regarded with a kind of sentiment by all classes, as the harbinger of spring.

Swallows, moreover, do not arrive on these coasts in a state of exhaustion, and to net them would be no easy feat.

A few isolated cases of the cruel method of capturing them with artificial flies may occur, but not more so than in England.

The Italian may be ruthless in his destruction of other birds, but is certainly not a destroyer of the *Hirundinidae*.

Of the similar charge made against the French, I am not in a position to judge, but I imagine that the cause for the decrease of the *Hirundinidae* in England may lie in quite another direction, and may be attributable to some equivalent decrease of their favourite insects in our islands, or in some atmospheric and climatic change. Italy, I am convinced, is

not responsible in any way for it.

RICHARD BAGOT.

Roma, December 27, 1898.

RADIATION PHENOMENA IN THE MAGNETIC FIELD.

IN the spring of 1897 the scientific world became indebted to Dr. Zeeman for the observation that when a source of light is placed in a strong magnetic field the spectral lines of the light emitted by that source suffer