the subject. When my book was originally planned it was intended that it should be a monograph of the specimens of okapi contained in the national collection, and it thus became entered on our list as "the monograph on okapi."

More, no doubt, might be written about the specimens which I had under examination, and I should have, in some circumstances, been able to add to what the book contains; but the problems which arose in the course of my work could not, in many cases, be satisfactorily solved by the examination of the existing material.

We shall have to wait for new observations made upon fresh or living specimens for a solution of the question as to what are the characteristics of the male and female okapi respectively, what are their geographical variations, and whether there are distinct races or subspecies.

E. RAY LANKESTER.

29 Thurloe Place, South Kensington.

SIR E. RAY LANKESTER is correct in supposing that I was misled by the last paragraph of the preface to his work on the okapi into the belief that there had been or might be an additional volume of text to supplement the illustrations given in the volume under review. From private correspondence which passed between Sir E. Ray Lankester and myself about three years ago I was under the impression that the "text" alluded to was in existence, and perhaps I arrived too hastily at the conclusion that for reasons of economy it had been put aside because of the intervening publication of M. Jules Fraipont's work. The title "Monograph of the Okapi" to which Sir E. Ray Lankester refers as likely to mislead an appraiser of his work was not of my bestowal, but is the official title of this valuable and admirably produced volume. The illustrations are fully described; but I suppose what I missed, and what I hoped might still be forthcoming, were the deductions to be drawn from these illustrations as to the affinities and systematic position of Okapia: in short, a statement of Sir E. Ray Lankester's personal opinions. He is probably quite right to withhold these until something is known of the beast's musculature and intestines.

H. H. JOHNSTON.

The Dynamics of a Golf Ball.

WITH a view to reproduction in the forthcoming Life of the late Prof. Tait, I have just been editing his popular article on long driving, which appeared in the *Badminton Magazine* of March, 1896. On reading Sir J. J. Thomson's lecture, as published in NATURE of December 22, 1910, I was greatly struck with the strong resemblance between golf-ball paths worked out mathematically by Tait and the stream lines of the electrified particles in the ingenious experiment devised by Sir J. J. Thomson. A few of Tait's calculated curves were given in NATURE, vol. xlviii. (June 29, 1893); but better examples will be found in the second paper on the path of a rotating spherical projectile (Trans. R.S.E., vol. xxxix., or Scientific Papers, vol. ii., p. 386) and in the article on long driving already mentioned.

By laborious arithmetical calculations, Tait and his assistant computer worked out a series of possible trajectories with various values for the transverse force due to the underspin, obtaining, among others, the kinked path which Tait had already demonstrated by undercutting a light rubber balloon. It is extremely interesting to see how the several types of curve figured by Tait for the same initial speed of projection, but varying degrees of underspin, are almost accurately reproduced by Sir J. J. Thomson's beautiful method of subjecting a stream of negatively charged particles to a suitable combination of electric and magnetic forces.

C. G. KNOTT.

Edinburgh University, January 2.

On the Simultaneity of Abruptly-beginning Magnetic Storms.

I WAS naturally much interested in Dr. Krogness's communication to NATURE of December 8, 1910 (p. 170), and wish to take this occasion to express my gratefulness to him for making known his criticisms on some of the results of my investigations on magnetic storms, as well as on those of Mr. Faris, where there is opportunity for reply. I am also glad that he has made his statements sufficiently direct, so as to admit of an equally direct answer.

Dr. Krogness first wishes to show that my conclusion, that even the sudden magnetic disturbances do not begin strictly at the same instant, but at measurably different times at various points on the earth, rests on insecure foundation; he would make it appear that it was based on but two cases, viz. the disturbance of May 8, 1902, and that of January 26, 1903. He will find a table (No. VIII.) in No. 2 of my researches (December, 1910, issue of *Terrestrial Magnetism and Atmospheric Electricity*) which summarises the data from thirty-eight abruptly-beginning disturbances between the years 1882 and 1909, thirty-four of which were available to me when the article was prepared which Dr. Krogness reviews (*loc. cit.*, pp. 19–20).

The table gives the date and approximate Greenwich mean civil time for each of these thirty-eight disturbances, next the number of observatories for which time data were available and the approximate portion of a complete circuit of the earth embraced by the contributing observatories. Then the value of x, or the time in minutes required by a disturbance to pass over one-fourth of a great circle, and in the following columns is given the approximate weight to be attached to any particular value of x, as determined from all circumstances involved, and the source from which the data have been obtained. A plus sign attached to x means that the disturbance progressed apparently in an eastwardly direction, as indicated by an increase in the Greenwich mean time of beginning at easterly stations over that at westerly ones. A minus value of x means, of course, the reverse. Nos. 35–38 were since added on the basis of data communicated by Mr. Faris (*loc. cit.*, pp. 213, 214).

Out of thirty-eight values of x, only ten, or about one-fourth, have the negative sign, so that three-fourths of the disturbances of the type here considered show an eastward progression at the times of beginning. In view of the greatly varying circumstances on which the figures are based—different observatories, different instruments, times scaled by different persons, different years, covering a period of two and a half times that of a sun-spot cycle—it is going to be difficult to explain the persistency of the plus sign by any such possible errors as Dr. Krogness points out, which, as a matter of fact, even he will hardly contend would be always in the same direction for *every* observatory, nor even necessarily always the same at the same station.

From this table the following results are derived:—

Weighted mean value of 28 plus values of x	$= +1{\cdot}65$ minutes
,, ,, 10 negative ,,	$= -1{\cdot}80$,,
Weighted mean without regard to sign ...	$= \pm 1{\cdot}69$,,
(Hence velocity of progression for average sudden disturbance, whether to the east or to the west, is	99 km./sec.)
Weighted mean with regard to sign	$= +0{\cdot}74$ minute
(Hence average *algebraic* velocity of eastwardly progression is	225 km./sec.)

We thus get a velocity for the progression of a sudden disturbance on the order of 100 to 200 kilometres per second; hence, if a sudden disturbance passed around the earth completely it would take approximately between seven and three minutes. We are here, then, dealing apparently with a velocity of a greatly subordinate order (1/3000 to 1/1500) to that of electromagnetic waves, which would require but a tenth of a second to pass round the earth, and of kathode rays which would take on the order of a half-second.

Another line of argument set forth in my papers is based on the harmonic analysis of the typical disturbance here under consideration, for which the effect, in general, is an increase in H (horizontal intensity) over the whole earth and a decrease in Z (vertical intensity) in the northern magnetic hemisphere and an increase in Z in the southern. It was found, for example, that the disturbance system of