This, with the angular velocity of the earth inductor, is all we need for determining the absolute measure of the resistance, since we know by calculation the coefficient of mutual induction between the primary and secondary of the transformer.

The method has some advantages. The value of the earth's field need not be constant. Thermo-currents make no difference, as we are using A.C. voltages, and these may be taken very large compared with any possible thermo-effect in the primary. The same coils would be used for determining both the ohm and ampere, so that any error in calculating the coefficients for them would affect both units. Modifications will readily suggest themselves; as, for instance, two sets of such coils, one on each arm of a balance, and the movable coils acting both as secondary and as the movable coil of a Kelvin balance.

Reginald A. Fessenden.

Western University of Pennsylvania, April 3.

Fourier's Series.

I should like to correct a careless error which I made (Nature, December 29, 1898) in describing the limiting form of the family of curves represented by the equation

$$y = 2\left(\sin x - \tfrac{1}{2}\sin 2x \;.\;.\;.\; \pm \frac{1}{n}\sin nx\right) \;.\;.\;. \quad (1)$$

as a zigzag line consisting of alternate inclined and vertical portions. The inclined portions were correctly given, but the vertical portions, which are bisected by the axis of X, extend beyond the points where they meet the inclined portions, their total lengths being expressed by four times the definite integral

$$\int_0^\pi \frac{\sin u}{u} du.$$

If we call this combination of inclined and vertical lines C, and the graph of equation (1) C_n, and if any finite distance d be specified, and we take for n any number greater than $100/d^2$, the distance of every point in C_n from C is less than d, and the distance of every point in C from C_n is also less than d. We may therefore call C the limit (or limiting form) of the sequence of curves of which C_n is the general designation.

But this limiting form of the graphs of the functions expressed by the sum (1) is different from the graph of the function expressed by the limit of that sum. In the latter the vertical portions are wanting, except their middle points.

I think this distinction important; for (with exception of what relates to my unfortunate blunder described above), whatever differences of opinion have been expressed on this subject seem due, for the most part, to the fact that some writers have had in mind the *limit of the graphs*, and others the *graph of the limit* of the sum. A misunderstanding on this point is a natural consequence of the usage which allows us to omit the word *limit* in certain connections, as when we speak of the sum of an infinite series. In terms thus abbreviated, either of the things which I have sought to distinguish may be called the graph of the sum of the infinite series.

J. Willard Gibbs.

New Haven, April 12.

Tasmanian Firesticks.

While preparing for a second edition of the "Aborigines of Tasmania, I received from Mr. Jas. Backhouse Walker, of Hobart, two separate accounts of fire-making by the aborigines, which differ materially from those already known. The accounts come from two very old colonists, Mr. Rayner and Mr. Cotton, and describe fire as being obtained by means of the stick and groove process. Mr. Rayner's account runs thus: "A piece of flat wood was obtained, and a groove was made the full length in the centre. Another piece of wood about a foot in length, with a point like a blunt chisel, was worked with nearly lightning rapidity up and down the groove till it caught in a flame. As soon as the stick caught in a blaze, a piece of burnt fungus, or *punk*, as it is generally termed, was applied, which would keep alight. I cannot say what kind of wood it was. My father has seen them light it. The piece with the groove, he said, was hard, the other soft. The blacks in Australia get fire by the same method. I have seen that done. I think it almost impossible for a white man to do it, for I have seen it tried, and always prove a failure." Cotton's account agrees in the main with Rayner's. We are thus in possession of accounts of three distinct methods of fire production, viz.: (1) flint and tinder; (2) fire drill and socket; (3) stick and groove. At first sight it may appear incredible that a race so low in culture could have known and used these methods; nevertheless such a supposition might occur, for some neighbouring tribes in Australia are known to have at least two methods. As regards the Tasmanians, we may, I think, leave out of consideration the flint process, as both Furneaux and La Billardière seem to have mistaken so-called flint implements for fire flints. We may also eliminate indefinite accounts which simply refer to the process used as one of rubbing two sticks together, although rubbing describes rather the stick and groove method than the drill process. We may also omit the statement about the fire-drill supplied by Bomirck's bushranger as being untrustworthy. We are thus left with the two specimens of fire-drill (in the Pitt-Rivers Museum, Oxford, and in the possession of Sir John Lubbock, respectively) supplied by Dr. Milligan and Protector Robinson, with Melville's description and with A. H. Davies' description. When Melville published his V. D. Almanac in 1833, he gave a short account of the aborigines, but to fire-making he made no reference at all; when he wrote his "Present State of Australia" (mostly an account of Tasmania), printed in London in 1850, he described the drill method of making fire as having been used by the Tasmanians. But, in the meanwhile, Davies, writing in 1845 in the *Tasm. Journ. of Sci.*, says he is "informed" that the Tasmanians raised fire by the drill process. But this statement, on hearsay, was made long after the aborigines had been deported to Flinders Island (1837), and after they had long been familiar with Australian aborigines imported into Tasmania; so that, although his statements may in general be relied on, this one wants confirmatory support, especially as his statement is the first one describing the drill process as being a Tasmanian method. Melville's account appears to me to be taken from Davies. Milligan knew nothing of the aborigines until 1847, when he was put in charge of them at Oyster Cove after their return from Flinders Island, and at a time when it was not likely that, in close proximity to European settlements, they would have continued to produce fire by native methods. Although we are much indebted to Milligan for the vocabularies, on the other hand there is considerable carelessness in his translation of the native sentences, and it is well known locally he was not interested in his charge. Hence his presentation to Barnard Davies of a fire drill as a Tasmanian instrument does not prove the drill to have been Tasmanian. Robinson, in spite of his intimate intercourse with the aborigines, and his voluminous reports on his doings while capturing the wretched remnants, has left us such a comparatively small amount of information concerning them, that I have for a long time past come to the conclusion that he was a very unobservant man, an opinion largely confirmed by his presentation to Barnard Davies of ground Australian stone implements as Tasmanian, but the real origin of which was settled as Australian by Prof. Tylor's paper on the subject read at the Oxford meeting of the British Association. As Robinson was afterwards Protector of Aborigines in Victoria, it is not at all unlikely that he confused his specimens, and called them Tasmanian instead of Australian. On the other hand, we have circumstantial accounts of stick and groove fire-making apparatus by two settlers, well advanced in years, who carry us back to the early part of the century when the natives were still roaming about the country before they were wholly robbed of it, and to a time when they had been little in touch with Australians or Europeans. Either there were two methods of fire-production used by the natives, or the stick and groove was the only one.

H. Ling Roth.

Halifax, England, April 13.

WIRELESS TELEGRAPHY.

Although at the present moment there is not a single commercial line of the so-called wireless telegraphy at work, and probably not a single penny has yet been earned by those exploiting it, the one pound shares of the Company have been quoted at six pounds, and perhaps more. At the same time the shares of many of the Submarine Cable Companies have fallen considerably owing to the popular delusion that wireless telegraphy is going to displace wires. Thus a popular scare—the outcome of ignorance—has appreciated the one property and depreciated the other to the value of about