fact, upon which all are agreed, that if the current in A ceases, the original momentary reverse current in B gives place to a momentary current in B, now in the same direction as that just ceasing in A. It matters not whether the explanation be that the original field continued to act, or whether the loops of force were broken down and continually replaced with immense rapidity by the constant current in A. Whatever the real explanation, the fact remains that on causing the current in A, or bringing the circuit B within the influence of A, there is action of A upon B, and this action influences B so long as the current in A continues. My point is this, that although the current in B is but momentary, the loops of force of A, to which this current is primarily due, do not cease to act, or that their successors do not cease to act, but that so long as the current in A is constant, there are, from the time of the momentary current in B, two equal and opposite forces acting in B. This I claim to be proved, because any fluctuation in the current in A is met with a corresponding fluctuation in the condition of B. That is, so long as the loops of force due to A are around B, they act upon B.

This experiment can be taken a step further. The current in B, as measured by a galvanometer, is proportional to the number of loops of force around the conductor. This can be shown in two ways:

1. By increasing the length of B acted upon.
2. By increasing the density of the loops around B in a given space.

The latter is perhaps the simpler method. Doubling the current in A, thus doubling the strength of field, or doubling the density of loops, causes double momentary current in B. If this be done by decreasing the resistance of A continuously with sufficient rapidity, the current in B will increase in proportion to the decrease of resistance in A, or in proportion to the increase of loops of force due to A. There is no discontinuity in the swing of the galvanometer needle indicating current in B; hence I say that the total current in B is that due to the total number of loops of force around B. In other words, when the looping of lines around B is rapidly continuous, the effect of the first loops is not lost before the effect of the last loops is impressed, and the total effect or the maximum current is the summation effect produced by all the lines so looped.

So many friends have, by pen or word, tried to convince me that I am wrong in what I never said or intended to say, but which I am wrongly stated to have said, that I am obliged to enter somewhat fully into what I have really endeavoured to say. Any friends who will try the experiments mentioned, and send the results of their investigations, will have my warmest thanks. I am open to conviction, but hitherto those who have tried the experiments have come to the same conclusions as myself, while some who have not tried the experiments are ready with other interpretations, and are quite convinced I am wrong in all my conclusions. I ought to state that I have not attempted to find out the rate at which loops must be added to give the summation effect, but have manipulated the apparatus as quickly as I could revolve a contact-piece through 360deg. C. H. W. B.

(To be continued.)

CORRESPONDENCE.

ALTERNATE-CURRENT MOTORS.

SIR,—In your issue of March 6 I find the passage: "Mr. Kapp described the position as it exists. He showed how Ferraris first of all pointed out the right way to get an alternating-current motor that was self-starting, and how Tesla and others had worked in the direction indicated by Ferraris," etc.

I would be very glad to learn how Mr. Kapp succeeded in showing this. I may call his attention to the fact that the date of filing of my American patent anticipates the publication of the results of Prof. Ferraris in Italy by something like six months. The date of filing of my application is, therefore, the first public record of the invention. Considering this fact, it seems to me that it would be desirable that Mr. Kapp should modify his statement.—Yours, etc. NIKOLA TESLA.

New York, 17th March, 1891.

UNDERGROUND CABLES.

SIR,—I notice with some interest a short paragraph in your last issue with regard to the testing and the specifications of cables for electric light use. The point therein mentioned is exceedingly important, and I am glad you have drawn attention to it.

With the underground electric light circuits extending in all directions, it is very necessary for those who have to watch the interests of the public to be able to test and specify with a considerable degree of accuracy the insulation of an underground cable; not as it is, in coming from the makers' hands, but as it will be in a few weeks or months under a steady strain, day and night, of a known pressure.

I think, therefore, that a tentative model test and specification might reasonably be looked for by those who, like myself, do not pretend to know all the details, from electrical engineers or the Institution to which they belong, and I trust the suggestion will be taken up in the proper quarters.

While writing to you upon this question I should like to also add that the subject of how much leakage in parallel systems at low tension, say in amperes or decimals of an ampere, per thousand lamps connected should be regarded as a maximum is also important, and will certainly tend to become more so in large town installations.

If this letter serves to draw replies from those who are competent to advise I shall be very pleased.—I enclose my card, and beg to sign myself A BOROUGH ENGINEER.

CONCENTRIC MAINS.

SIR,—It was very amusing to see, last week, Mr. J. D. F. Andrews let off his surprise at your note on the new concentric house wires, formed of two copper tubes one inside the other. "It should have been brought out many years ago," says Mr. Andrews, "as it is quite eight years since I commenced working at such a system." Now I have no reason to go against Mr. Andrews, or to hold a brief for the unknown inventor of the double copper tube distribution. But I do not see that because Mr. Andrews was working eight years ago on "such a system"—presumably the earlier forms of his present concentric cable—another inventor may not claim novelty for what certainly is a novel and bold idea—the use of bare conductors one inside the other. If there is any possibility of seeing and testing this system, many electrical contractors would like to avail themselves of it, and possibly compare it with that of Mr. Andrews's. The system seems cheap, easy, and practically everlasting. The questions are, is it safe, and will it be allowed by companies and fire offices?—Yours, etc., G. P. O.

SHEER THOUGHT.

SIR,—In a note which you reprint from the *Globe* in your last issue, reference is made to the work of S. Tolver Preston—a work which I read at the time it appeared, and have dipped into it many times since with great interest. I agree with the writer of the note that Mr. Preston is a physicist rather than a mathematician, and have thought it might be instructive to know what mathematicians have done in developing any application of science. From my childhood I have had it dinned into my ears that everything is open to the mathematician, but, as a man of the world, I have been unable to find that they have benefited the human race to any great extent. Was Trevethick, or Newcomen, or Watt, or Stephenson a mathematician? These men made steam engineering. Were Gramme, Brush, Maxim, Edison, Crompton, Mordey, or Esson mathematicians? Yet, with Kapp and Hopkinson (who may perhaps be termed mathematicians), these are the men who have made electrical engineering. The greatest architects and the greatest engineers have not been mathematicians, and if we could sum up the benefits conferred by Trevethick and his successors, by Gramme and his successors, by the architects, engineers, and chemists, it would be found that "sheer thought" stands triumphant, and