LIGHTNING ARRESTERS AND THE PHOTOGRAPHIC STUDY OF SELF-INDUCTION.

A Reply to the Notes of Messrs. Jo. Stanford Brown and Chas. T. Child.

BY E. G. ACHESON.

A theory that cannot withstand the attacks incident to a general discussion, should be abandoned at the earliest moment. It is only by careful, exhaustive study, experimentation and discussion, that any theory can be proven worthy of acceptance. While our thoughts, energies and moneys are being directed to and expended for the adaptation of old, well-known principles, and the improvement of the various devices embodying those principles, it is wise to keep up a constant, restless, untiring search for new principles.

It was with such sentiments in mind that I formerly appeared before you; it is with such sentiments that I now approach this subject.

The reverence that all students of science should, and I am sure, do, feel for the opinions and theories of those who have in past years labored in our chosen field of research, ought not to be of such an all-absorbing character as to debar them from striving to add to and expand those theories; and peradventure, should occasion arise, abandon and throw aside those opinions and theories held sacred by our predecessors.

The discussion on my paper, entitled: "Lightning Arresters and the Photographic Study of Self-Induction," was reopened February 12th, 1889, by Notes from Mr. Jo. Stanford Brown and Mr. Charles T. Child. I regret that my time has been so much occupied during the last few months, that I have been unable to continue my investigations on the subject under discussion. Regrets are, however, useless, and I will, therefore, deal with the question as best I can with the material at hand.

In handling the subject, permit me to first quote each question or point as expressed in the "Notes," and then present such explanations or arguments as I may find possible or convenient.

QUOTATION:—"After careful reading it would seem as if the subject might be approached with advantage from more than one

standpoint, and that even somewhat different conclusions were deducible, simply by rearrangement of the experimental data. We have given, a submarine or subterranean cable, to each end of which, at its junction with the main line, a lightning arrester is connected in the most approved manner, Fig. 1; to determine why, in spite of these precautions, the cable is not infrequently punctured when an "electrical discharge" enters the overhead system. For experimental purposes this has been produced in miniature, as shown in Fig. 2, 2*a*, 2*b*, and 2*c* * * *

REPLY:—Figs. 2*a*, 2*b* and 2*c* have been introduced by the Authors of the "Notes," and while Figs. 2*a* and 2*b* are a close analogy to Fig. 2 and may be used to represent the case as presented in my paper, I make exceptions to Fig. 2*c*, for a condenser as there illustrated, can not be substituted for an elongated condenser as is obtained with a cable (see p. 143.)

QUOTATION:—"C now being made but a fraction of the insulation thickness, and w a short, thick copper wire, it was found that the cable was still frequently punctured at times when a spark appeared at c, and that the length of w appeared to enter the effect equally with the space variation of c."

REPLY:—Part of this quotation contains language similar to that used in my paper, as do various other portions of the "Notes" but as no quotation marks are used, I can not, of a certainty, tell whether the authors made any of these experiments or whether they are simply restating the experiments and results obtained by me. If the latter be the case I beg to correct the results, as stated to have been obtained, in the last quotation. I found the wire w appeared to have as much influence on the results as the space c. The space c was always of a uniform length, hence no space variation of c occurred.

QUOTATION:—"What, then, is the relative efficacy of changing the length of w and the distance of the c points (Figs. 3 and 3*a*), to prevent a spark at B * * *

REPLY:—Owing to the fact that the authors then proceed to describe one set of my experiments and refer to the curves plotted from the results, and then suggest other, and I presume in their opinion, better, methods of determining this question; owing, I say, to these facts I am led to believe that they have not been as careful in reading my paper, or at least have not understood it as fully as might have been.

The question they propound is not the one I had presented to me. Reading from my paper, "A few more experiments also

demonstrated that the wire *w* had apparently as much influence on the results as the space *c*, and consequently it became necessary to analyze the combination more completely to determine the relative values of *c* and *w*." To determine these values I did the simplest thing possible, viz: I first determined the value of *w* without *c*, and next introduced *c* and measured their united values.

Again, the object was not to determine their efficiency as *pre-ventives* of the spark *B*, but rather their values as *producers* of that spark.

QUOTATION:—"That with the connections of Fig. 4 and *w* equal 20 feet, a spark should pass at *B* of over $\frac{1}{8}$ inch in length, might perhaps have been expected from Dr. Lodge's results (London Electrician, XXI, 815), * * * True; Fig. 4 is not an 'alternative path' of necessity, for the *B* points may be moved out of the sparking distance."

REPLY:—The obtaining of this spark $\frac{1}{8}$ of an inch in length, did not surprise me in the least, as I *did* expect it, for I had read of Dr. Lodge's experiments prior to that date. My expectations were, however, excited by his actual experimental results, and not by his theories and formulæ, as set forth in the above reference, and which were constructed to account for his experimental results.

When I remarked in my original paper, that the simple assertion that *B* and *w* were "alternative paths" did not, however, make them such; the peculiar idea contained in the latter part of the last quotation from the "Notes" had not occurred to me, excepting as it applied to the current as originally discharged from the jar *J*; if, however, I properly interpret the meaning in these "Notes," it is there suggested that they (the *B* points) might be separated to such a distance as to prevent any sparking whatever. But unfortunately they would then cease to stand as 1 to 10 compared with *w*, and those are the conditions we are considering, hence I fear it will be necessary to drop this suggestion regardless of its extreme originality.

"QUOTATION:—"W is now supposed 'electrified' by the charge across *A*."

REPLY:—I am not aware that I assumed that to be the case; but rather that *w* became electrified by the energy discharged from the jar *J*.

QUOTATION:—"The 'inverse extra current' of making simply cuts from the charging current; but the 'direct extra current' of breaking, *i. e.*, charging impulse cessation, acts as its continuation, but at a higher potential. (Ganot, 7th edition, 766)."

REPLY:—I do not believe I made any statement contrary to the above. The law is a well known, universally accepted one, and scarcely needs the support of any special authority.

QUOTATION:—"This extra current then is running towards E and would naturally be supposed to equalize on the same path as the current generating it. It might, therefore, be expected to dissipate on reaching earth."

REPLY:—With the kind permission of the authors of the "Notes," I would like to ask them what they mean by the term "equalize," and how they suppose "dissipation" takes place?

While it is well known that a charge of energy in a Leyden jar will become dissipated if left to itself and without any specially prepared discharging circuit, such a process cannot be considered in connection with the phenomena under discussion. The authors cite their authority for the generation of a current in the circuit and its flow or discharge into the jar; now, how they expect it to be dissipated I cannot see from their statement. The very name of this current—the extra current—defines it as existing distinct from the original, hence they cannot expect the original current to neutralize it, for it is the product of that current, and for it to neutralize itself requires it to either short circuit its own extremities or discharge back over the same path in which it was formed or generated.

QUOTATION:—"It would be of interest to some in this connection to know how it was ascertained that the instantaneous time duration of the extra current is shorter than that of the charging current."

REPLY:—A careful re-reading of my paper fails to bring to light any assertion that this was in any manner a demonstrated fact. I do not believe I referred to this in any other way than interrogatively—not being desirous to force my opinions, that were of a purely theoretical nature. The point being now raised however, I will take advantage of this opportunity to express my ideas more freely. It is not to be supposed that the authors of the "Notes" believe otherwise than that the "direct extra current" is less in energy *under ordinary conditions*, than the original current. They have given authority for their belief that it

is of higher electromotive force. Their entire argument is on the basis that it traverses the same circuit as the original current; and hence it cannot meet a greater resistance, but possibly, everything being considered, including the fact that the circuit has already been *electrified* by the original current, the resistance may in reality be less in value than that opposed to the primary current. With these conditions given, are the authors prepared to assume that the extra current is even of the same duration as the original?

QUOTATION.—“On the assumption that the extra current dissipates through B, and not in the earth, it is argued that shortening w would tend to decrease this tendency to puncture the cable.* * * May not this be regarded simply as a safety short circuit for our cable considered as a Leyden jar? And what is this but a working case of a cable effectually protected by a lightning arrester B.”

REPLY:—Once more I ask the authors of the “Notes” how they would expect the energy conveyed or represented in the extra current, to become dissipated in the earth (in this case the jar) in the infinitesimal intervals of time with which we are dealing. It would be interesting to know how they account for the oscillatory discharge of a Leyden jar, or do they disbelieve that oscillations occur? I will venture to express an opinion on this subject. The original current discharged from the jar does its equivalent of work in the circuit, and ceases to exist as current. The work performed appears as heat, light (under some conditions) and a distorting of the lines of energy which were originally within the circuit. The last work mentioned is of such a character that it causes another current, called the extra or self-induced current to be formed. The energy constituting this self-induced current is not localized into one or more places as was that forming the original current when contained on the surfaces of the jar; but it is distributed over or throughout the entire circuit. When such a circuit is “short circuited” surely the energy will be delivered more quickly, than would be the case with the energy in the jar, for there it has to traverse the entire circuit. Where no short circuiting occurs and the connections remain to the jar, the self-induced current will discharge into it.

Returning once more to the subject, I must say that under the conditions of my experiments, I cannot admit that the wire R, as shown in Fig. 2, acts simply as a safety short circuit for the cable

considered as a condenser; but that it is a working case of a cable effectually protected by a lightning arrester I heartily agree; indeed, the major portion of my experiments were performed and my paper prepared to demonstrate and emphasize that fact.

QUOTATION:—"It is not altogether plain how the direct extra current is an effect of Faraday's electro-tonic state, when Faraday says (Researches 1, 16.): 'This state is altogether the effect of induction exerted, and ceases as soon as the inducing force is removed.'"

REPLY:—The theory that the extra current, is due to the peculiar state of the circuit prior to the passage of the inducing current, was formed by me a long time ago, and was set forth in an article published in the *Electrical World*, Feb. 25th, 1888. The authors of the "Notes" are correct in so far as they go, and perhaps they are wholly so, on this point; but it is true that Faraday was never perfectly clear in his opinion as to this electro-tonic state. After having formed the opinion as above expressed in the quotation, he changed his ideas and considered there was no necessity for assuming the existence of this prior state. (Researches Vol. I, 66, 69.) Again, later on he revives the idea. (Researches, Vol. III, 367, 420.)

I do not deny, but rather confess to having attempted to add a little to Faraday's idea on this subject, for while he considered the state due to a peculiar action of the magnetic lines of force emanating from some external source, and that this state in conjunction with motion produced an electric current, I have gone back one step further and assumed a peculiar state prior to the influence of the external magnetic lines of force, and that it is this prior condition of the circuit that is the true cause of an electric current being generated when the circuit is forced to move across external lines of magnetic force. I justify my action in terming this prior state the electro-tonic state, by the fact that Faraday has, so far as I can learn, left it in more or less uncertainty, and also from these words of his. (Researches, Vol. III, 420.) "Again and again the idea of an *electro-tonic* state has been forced on my mind; such a state would coincide and become identified with that which would then constitute the physical lines of magnetic force. Another consideration tends in the same direction. I formerly remarked that the magnetic equivalent to *static* electricity was not known * * *

If an error has been committed in thus venturing to trespass, I can only refer to the opening remarks of this communication, and add that the feelings that have inspired me in this work are there set forth.

As a passing remark I would say that I saw it stated in print, within the past few weeks, that this theory was old and contained nothing new. Evidently some one is wrong.

QUOTATION :—" Figs. 5, 5*a*, 6 and 6*a* illustrate effects of different kinds of work introduced into the circuit."

REPLY :—The authors then give a list of references to support this statement, I presume ; but as I do not possess nor can I have access to any one of the volumes referred to, I will take it for granted they have sustained their statement, and which, by the way, agrees exactly with my own opinions, and which I believe I stated very clearly in my original communication. Without being able to see their references, however, I am at present unable to know whether they have referred to work producing similar results under similar conditions or not ; but I am persuaded to think they have not.

QUOTATION :—" The methods used in the photographic part of the investigation would seem to cast some doubt on the result claimed."

REPLY :—That there is a possibility of the results obtained with the photographic apparatus having a limited value, owing to the uncertainty of uniformity in the speed of the disc and in other portions of the work, cannot be denied.

The results obtained do, nevertheless, correspond closely, not to say exactly, with those of the former portion of the investigations, when studied by the theories advanced. There are so many chances of error in this portion of the work that I would not myself refer to it, had not the authors of the " Notes " reviewed it and suggested various other ways and means for accounting for the peculiarities of the results as obtained.

QUOTATION :—" In the case where the discharges are through 76 feet and 20 inches of the same wire, respectively, hardly any difference appears in the spark. Is it not possible that the illuminating power of the quick, bright spark through the short wire was so much greater than that of the necessarily more extended and consequently fainter spark through the long wire as to make the effect practically the same ? "

REPLY :—The duration of these two sparks I found to be about the same ; but with the shorter wire more work was done in the air space where the spark was produced. The fact that the spark with the short wire is brighter is an evidence of the increased work done at that point. This increase of work was indicated in the photograph by a heavier “ghost,” while the equality of their durations was shown by equal lengthed “ghosts.” The simple fact of a spark being brighter, would not, I think, produce an elongation of its image through a rapidly moving aperture.

QUOTATION :—“In the experiment in which two spark intervals are included in the circuit, as against one formerly, it was found that the spark was materially lengthened. This might be expected considering the practically infinite resistance of the second air space which is introduced.”

REPLY :—I was under the impression, as indeed, I thought all others were who had been closely studying the recent advances in this line, that the question of length of circuit was of more importance in point of time, than resistance, and perhaps more especially so when the resistance was formed by an air space. Again, how can the authors account for the fact of my having obtained so small a time interval with 76 feet of wire in circuit with the air space *A*, when the *B* points were in adjustment, for they will find by referring to my paper that the second air space, which was introduced to replace the wire, was of the same length as space *A*. At this writing I desired to refer to the negatives and examine them, but unfortunately while all the others remain, the one produced with the two spaces in circuit, is missing, and I will not attempt to do what the authors seem to have considered doing, viz : Measure the lengths of the “ghosts” on the prints from the cuts. That they should have found the “ghosts” or trails, on the negatives practically invisible is rather strange, and I believe had I had the pleasure of being present at the time of their examination I might have been of service to them, for I have found not one who experienced difficulty in seeing the “ghosts,” while none could define their limits.

QUOTATION :—“If a wave of current be pictured passing along a linear conductor, it would probably appear something like Fig. 7, its front lengthened out by the self induction effect of the cable, while the tension in the rear part is raised by the direct extra current. This high potential wave crest moves along the

cable until, finding a weak spot in the insulation, it discharges rather than keep up its strength with the impeding current ahead of it."

REPLY :—We have in this quotation and the accompanying figure all of the elements necessary either to substantiate the theory I have been advancing or else prove its utter worthlessness. The difficulty, of course, is in obtaining a correct interpretation.

May I ask how the authors determined that the direct extra current, occupied a shorter interval of time, as shown in the figure, than that of the original current? A fuller explanation of the discharging, through the insulation, of the high potential wave crest of the extra current would be interesting. Is it supposed to break through and rush out, like water through a bung-hole? We are here dealing with a complete electrical current, generated within the wire and having its high and low potentials within the wire. In accordance with the accepted theories, a current is supposed to discharge itself into or through a closed circuit, and this would necessitate both ends of the wave breaking through the insulation, if the insulation be broken at all. Perhaps this is the way in which they assume dissipation occurs. It is evident that the authors of the "Notes" are here confounding self-induced currents with static discharges.

It is probable that the gradual increase of the current as shown in Fig. 7, is more or less a correct representation of the facts.

What causes this cutting away of the earlier portion of the current wave? The authors say correctly, that it is produced by the self-induction effect of the conductor.

It is due to an inverse current having been raised in the path of the charging current. The work of producing or creating this current constitutes the momentary resistance that has been termed *impedance* by Mr. Oliver Heaviside, and it is this resistance that Dr. Lodge assumes bars the path of the discharging current by the way of the wire *w* and causes it to take the path offered by the *B* points. (Fig. 4.)

For the *impedance* of a wire to offer a resistance, or rather, for the *impedance* to exist prior to the expenditure of more or less of the current that it impedes, is exceedingly difficult, in fact impossible, to comprehend; for the existence of the resistance would be wholly unknown, did not the current energy exert itself against it; but the moment the first portion of the current wave

meets and, speaking figuratively, combats with the resisting force, that portion of the current dies, but in dying it overcomes as much of the opposing resistance as is measured by its own energy; in fact they die together. This is followed by the overcoming of yet another portion of the *impedance* by another portion of the current energy, and so on to the complete annihilation of the total *impedance*.

But this process requires the loss of our original current energy; and if this be the case, and it must be so in order that the *impedance* have even an existence, how are we to discharge this energy across the β points, in the manner described by Prof. Lodge? Perhaps it may be said that it is a surplus quantity that was not required for this formation of *impedance* that jumped the space at β ; but this can hardly be so, for any energy remaining after the down-fall of the *impedance*, would have an open road through the low resisting wire.

Out of the dead body of the *impedance*, comes forth the energy expended at the α points.