the aërial wave formed by the vibration of the sonorous body; so the vibrations, more or less rapid or more or less vigorous, of the electric fluid excited by the action of batteries of a greater or smaller number of plates, are propagated in conductors with the same velocity. Every one will see how the hypothesis imagined by us to give a reason for natural phenomena, will serve to suggest certain experimental investigations, the results of which will test their validity or insufficiency."

Melloni then says, that he shall shortly have occasion to publish facts which clearly demonstrate the errors of certain conclusions admitted up to the present time respecting electrostatic induction; and I am aware, from written communications with him, that he considered the results arrived at by Coulomb, Poisson, and others since their time, as not accordant with the truth of nature.* In the meantime he died, and whether his researches are sufficiently perfected for publication or not, I do not know.

The uniformity in the time and appearance of currents of different intensities at the further end of the same wire in the same inductive state, is a very beautiful result. It might at first be supposed to be in opposition to the views I set forth some years ago on induction and conduction, and the statements more recently made with regard to *time*. That, however, does not appear to me to be the case, as a few further observations on Mr. Clark's recent experiments will perhaps show. When the smaller battery is used, much less electricity passes into the wire in a given time, than when the larger one is employed. Suppose that the batteries are so different that the quantities are as 1 to 10; then, though a pulse from each would take the same time for transmission through the wire, still it is evident that the wire would be a tenfold better conductor for the weak current than for the strong one; or in other words, that a wire having only one-tenth of the mass of that used for the greater current should be employed for the smaller one, if the resistance for *equal quantities* of electricity having different intensities is to be rendered equal.

My views connect the retardation of the transmitted current with the momentary induction set up laterally by the insulated and externally coated wire. The induction will be proportionate to the intensity, and therefore its especial effect on the time of retardation proportionately diminished with the less intense current,—a result of action which will aid in rendering the time of retardation of the two currents equal.

The difference of *time* in the former experiments with air wires, and earth or water wires, very clearly depends upon the difference of lateral induction; the air wire presented a retardation scarcely sensible, the earth wire one amounting to nearly two seconds. If the insulating layer of gutta percha could be reduced from 0·1 to 0·01 of an inch in thickness, and mercury could be placed on the outside of that instead of water or earth, I do not doubt that the time would be still more increased. Yet there is every probability that in any one of these varying cases, electric currents of high and of low intensity would appear at the end of the same long wire after equal intervals of time.

Mr Clark's results may be stated thus:—A given quantity of electricity at a high intensity, or a smaller quantity at a proportionally lower intensity, will appear at the further end of the same wire after the lapse of the same period of time. My statement assumed the discharge of the *same* quantity at *different* intensities through the *same wire*, and the quantities in the illustrative experiments were measured by a Leyden jar In the consideration and further development of these results, it must be remembered that it is not the difference either in time, velocity, or transmission of a *continuous* current which constitutes the object in view; for that is the same both for an air wire and a subterraneous wire, but it is the difference in the *first appearance* only of the same current when wires under these different conditions are employed. After the first appearance both wires are alike in power unto the end of the current, and then a difference again appears which is complementary to the first.

There are many variations of these experiments which one would wish to make, if possible, and perhaps by degrees the possibility, or else equivalent experiments in other forms, may occur. If the wire employed were changed from a cylinder to a flat ribbon of equal weight, or to several small wires, all being equally coated with gutta percha and submerged, differences would probably arise in the time of delay with the same current; and I think that the ribbon, presenting more induction surface than the cylinder, would cause more delay; but probably any of these, or of like varieties, would cause the *same* delay for currents of different intensities. Again, one can scarcely doubt that with different conducting substances, as iron and copper, the delay would vary, as is the case in the transmission of sound and light. That the delay for currents of high and low intensity should be the same for the same wire in any one of such cases may still be expected, but it would be very interesting to *know* what would be the fact.

The prosecution of these results and the principles concerned in them, through the various forms they may assume by such like variations of the conductors and also of the currents, offers, as Melloni has observed, most extensive and interesting inquiries; even the power of a current to induce a current in neighboring wires and conductors is involved in the inquiry, and also the phenomena and principles of magneto-electric induction.

* He says, "I deceive myself much, or else the fundamental theorem of electrical induction, as we find it ordinarily announced, ought to be modified so as not to confound two effects completely distinct—the electric state during induction, and after the contact and separation of the inducing body. We know perfectly what occurs in the latter case, but not in the former," &c. Again, "In my last letter I raised doubts with regard to the consequences which have up to the present been deduced from the experiments serving as a base for the fundamental theorem of electro-static induction. These doubts have passed to a state of certitude in my mind, ... and behold me at this time thoroughly convinced that the enunciation of that theorem ought to be essentially modified." (July, 1854.)

On taking Daguerreotypes without a Camera.

BY J. F. MASCHER.*

The accompanying stereoscopic pictures were taken by me, by means of a box (to be described hereafter) that contained neither lenses, reflectors, or in short any refracting or reflecting medium of any kind. I accidentally made the discovery that photographic pictures could be taken in this manner while prosecuting some experiments relative to stereoscopic angles.

It is well known that two pictures taken with two ordinary cameras placed only 2½ inches apart horizontally, will not when placed in the stereoscope show proper or sufficient stereoscopic relief, yet it is well known that the human eyes are only placed 2½ inches apart, yet are enabled to see solid objects in their proper solidity and relief; and to explain the why and wherefore of these facts has challenged the attention of Professor Wheat-

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