

cannot doubt, they are found to be nearly correct, to promulgate as much as possible the facts, in order that so valuable a material may be speedily appreciated by the British agriculturist.

In conclusion I have to add, that these experiments were not originally commenced with that attention to rigid accuracy which is called for in strictly scientific investigations, for they were in fact intended to serve as illustrations to a course of practical lectures on the application of science to agriculture delivered on the spot, and this may form some excuse for the omission of certain data which could easily have been obtained, but which did not appear necessary until it was decided to calculate and exhibit the results in a tabular form.

LXXXI. *On the Chemical and Contact Theories of the Voltaic Battery.* By MICHAEL FARADAY, D.C.L., F.R.S., &c.*

[According to our intention expressed at page 269, we now reprint Dr. Faraday's argument against the *contact* theory of excitement in the voltaic battery, drawn from the consideration of the general and invariable laws of natural forces.—Ed.]

Improbable Nature of the Assumed Contact Force.

2065. I HAVE thus given a certain body of experimental evidence and consequent conclusions, which seem to me fitted to assist in the elucidation of the disputed point, in addition to the statements and arguments of the great men who have already advanced their results and opinions in favour of the chemical theory of excitement in the voltaic pile, and against that of contact. I will conclude by adducing a further argument founded upon the, to me, unphilosophical nature of the force to which the phænomena are, by the contact theory, referred.

2066. It is assumed by the theory (1802.) that where two dissimilar metals (or rather bodies) touch, the dissimilar particles act on each other, and induce opposite states. I do not deny this, but on the contrary think, that in many cases such an effect takes place between contiguous particles; as for instance, preparatory to action in common chemical phænomena, and also preparatory to that act of chemical combination which, in the voltaic circuit, causes the current (1738. 1743.).

2067. But the contact theory assumes that these particles, which have thus by their mutual action acquired opposite electrical states, can discharge these states one to the other, and yet remain in the state they were first in, being *in every point* entirely unchanged by what has previously taken place. It as-

* From the Phil. Trans. for 1840, p. 124.

sumes also that the particles, being by their mutual action rendered plus and minus, can, whilst under this inductive action, discharge to particles of like matter with themselves and so produce a current.

2068. This is in no respect consistent with known actions. If in relation to chemical phenomena we take two substances, as oxygen and hydrogen, we may conceive that two particles, one of each, being placed together and heat applied, they induce contrary states in their opposed surfaces, according, perhaps, to the view of Berzelius (1739.), and that these states becoming more and more exalted end at last in a mutual discharge of the forces, the particles being ultimately found combined, and unable to repeat the effect. Whilst they are under induction and before the final action comes on, they cannot spontaneously lose that state; but by removing the *cause* of the increased inductive effect, namely the heat, the effect itself can be lowered to its first condition. If the acting particles are involved in the constitution of an electrolyte, then they can produce current force (921. 924.) proportionate to the amount of chemical force consumed (868.).

2069. But the contact theory, which is obliged, according to the facts, to admit that the acting particles are not changed (1802. 2067.) (for otherwise it would be the chemical theory), is constrained to admit also, that the force which is able to make two particles assume a certain state in respect to each other, is unable to make them *retain* that state; and so it virtually denies the great principle in natural philosophy, that cause and effect are equal (2071.). If a particle of platinum by contact with a particle of zinc willingly gives of its own electricity to the zinc, because this by its presence tends to make the platinum assume a negative state, why should the particle of platinum take electricity from any other particle of platinum behind it, since that would only tend to destroy the very state which the zinc has just forced it into? Such is not the case in common induction; (and Marianini admits that the effect of contact may take place through air and measurable distances*;) for there a ball rendered negative by induction, will not take electricity from surrounding bodies, however thoroughly we may uninsulate it; and if we force electricity into it, it will, as it were, be spurned back again with a power equivalent to that of the inducing body.

2070. Or if it be supposed rather, that the zinc particle, by its inductive action, tends to make the platinum particle positive, and the latter, being in connection with the earth by other platinum particles, calls upon them for electricity, and so acquires a positive state; why should it discharge that state to

* *Memorie della Società Italiana in Modena*, 1837, xxi. 232, 233, &c.

the zinc, the very substance, which, making the platinum assume that condition, ought of course to be able to sustain it? Or again, if the zinc tends to make the platinum particle positive, why should not electricity go to the platinum *from the zinc*, which is as much in contact with it as its neighbouring platinum particles are? Or if the zinc particle in contact with the platinum tends to become positive, why does not electricity flow to it from the zinc particles behind, as well as from the platinum*? There is no sufficient probable or philosophic cause assigned for the assumed action; or reason given why one or other of the consequent effects above mentioned should not take place: and, as I have again and again said, I do not know of a single fact, or case of contact current, on which, in the absence of such probable cause, the theory can rest.

2071. The contact theory assumes, in fact, that a force which is able to overcome powerful resistance, as for instance that of the conductors, good or bad, through which the current passes, and that again of the electrolytic action where bodies are decomposed by it, can arise out of nothing; that, without any change in the acting matter or the consumption of any generating force, a current can be produced which shall go on for ever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped up in its own course. This would indeed be a *creation of power*, and is like no other force in nature. We have many processes by which the form of the power may be so changed that an apparent *conversion* of one into another takes place. So we can change chemical force into the electric current, or the current into chemical force. The beautiful experiments of Seebeck and Peltier show the convertibility of heat and electricity; and others by Ørsted and myself show the convertibility of electricity and magnetism. But in no cases, not even those of the Gymnotus and Torpedo (1790.), is there a pure creation of force; a production of power without a corresponding exhaustion of something to supply it †.

* I have spoken, for simplicity of expression, as if one metal were active and the other passive in bringing about these induced states, and not, as the theory implies, as if each were mutually subject to the other. But this makes no difference in the force of the argument; whilst an endeavour to state fully the joint changes on both sides, would rather have obscured the objections which arise, and which yet are equally strong in either view.

† (*Note*, March 29, 1840).—I regret that I was not before aware of most important evidence for this philosophical argument, consisting of the opinion of Dr. Roget, given in his *Treatise on Galvanism* in the *Library of Useful Knowledge*, the date of which is January 1829. Dr. Roget is, upon the facts of the science, a supporter of the chemical theory of excitation;

2072. It should ever be remembered that the chemical theory sets out with a power, the existence of which is pre-proved, and then follows its variations, rarely assuming anything which is not supported by some corresponding simple chemical fact. The contact theory sets out with an assumption, to which it adds others as the cases require, until at last the contact force, instead of being the firm unchangeable thing at first supposed by Volta, is as variable as chemical force itself.

2073. Were it otherwise than it is, and were the contact theory true, then, as it appears to me, the equality of cause and effect must be denied (2069.). Then would the perpetual motion also be true; and it would not be at all difficult, upon the first given case of an electric current by contact alone, to produce an electro-magnetic arrangement, which, as to its principle, would go on producing mechanical effects for ever.

Royal Institution, Dec. 26, 1839.

LXXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 157.]

December 8, 1842.—The following papers were read, viz. :—

“Observations on the Blood-corpuscles, particularly with reference to opinions expressed and conclusions drawn in papers ‘On the Corpuscles of the Blood,’ and ‘On Fibre,’ recently published in the Philosophical Transactions.” By T. Wharton Jones, Esq., F.R.S.

The author points out what he considers to be important errors in the series of papers by Dr. Martin Barry, which have lately appeared in the Philosophical Transactions, and are entitled, “*On the Corpuscles of the Blood,*” and “*On Fibre**.” He alleges that Dr. Barry has

but the striking passage I desire now to refer to, is the following, at § 113. of the article Galvanism. Speaking of the voltaic theory of contact, he says, “Were any further reasoning necessary to overthrow it, a forcible argument might be drawn from the following consideration. If there could exist a power having the property ascribed to it by the hypothesis, namely, that of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the other known powers in nature. All the powers and sources of motion, with the operation of which we are acquainted, when producing their peculiar effects, are expended in the same proportion as those effects are produced; and hence arises the impossibility of obtaining by their agency a perpetual effect; or, in other words, a perpetual motion. But the electromotive force ascribed by Volta to the metals when in contact, is a force which, as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be excited with undiminished power, in the production of a never-ceasing effect. Against the truth of such a supposition, the probabilities are all but infinite.”—ROGEE.

* See Phil. Mag., S. 3. vol. xx. pp. 321, 344; vol. xxi. p. 220.—EDIT.