

shot (as imagined by the late Prof. Marreco and Mr. D. P. Morrison), or by a fall of roof, a train of mine waggons that has just passed, or any other accidental circumstance, and subsequently carried past the mouth of the shot-hole at the instant the flame issues from it."

Immediately after the passage quoted above the Commissioners proceed to say:—

"The following facts relating to the part played by dust in coal-mine explosions may, however, now be regarded as conclusively established:—

"1. The occurrence of a blown-out shot in working-places where very highly inflammable coal-dust exists in great abundance, may, even in the total absence of fire-damp, possibly give rise to violent explosions, or may at any rate be followed by the propagation of flame through very considerable areas, and even by the communication of flame to distant parts of the workings where explosive gas-mixtures, or dust-deposits in association with non-explosive gas-mixtures, exist."

"2. The occurrence of a blown-out shot in localities where only small proportions of fire-damp exist in the air in the presence of even comparatively slightly inflammable or actually non-inflammable, but very fine, dry, and porous dusts may give rise to explosions the flame from which may reach to distant localities, where either gas accumulations or deposits of inflammable coal-dust may be inflamed, and may extend the disastrous results to other regions."

This has the appearance of conceding all that is asked, but when read in the light of the first quotation it leaves the matter in considerable doubt. Indeed, it was stated at the inquest on Altofts explosion that the proprietors of that colliery had not gathered from the Commissioners' Report that they were running any risk of an explosion, such as the one that happened; and at the inquest on Elemore explosion, which has been adjourned until the 18th inst., Mr. Lishman, the manager, gave utterance to similar sentiments. Be this as it may, it is obvious that legislative measures ought to be adopted without further delay, with the object of rendering the recurrence of coal-dust explosions impossible for the future. In providing against them it must also be recollected that a local explosion of fire-damp, such as the one which originated Marly explosion, produces exactly the same result as a blown-out shot fired under the most favourable conditions imaginable.

Cardiff, January 5

W. GALLOWAY

The Cambridge Cholera Fungus:

IN your issue of December 23 (p. 171) appears a letter from Dr. E. Klein, in which that gentleman attempts to show that the micro-organisms found by Dr. Graham Brown, Mr. Sherrington, and myself in the substance of the mucous membrane of the small intestine in cases of *Cholera asiatica* are nothing more than "common mould (probably *aspergillus*)," which has grown on and into the tissue during the process of hardening. We were and are, however, perfectly well acquainted with the fact that imperfectly preserved animal tissues are liable to be invaded by various forms of fungi, and took, therefore, precautions which we believe to be ample to prevent such contamination of our material. Moreover, the presence of the micro-organisms in certain parts of the tissues only, their absence in others or on the surface of the specimens, the fact that their presence in the part is accompanied by anatomical changes which could not have taken place during the process of hardening, and, most of all, the characters of the micro-organisms themselves, render such an hypothesis as that brought forward by Dr. Klein absolutely unacceptable.

It is unnecessary for me to answer all the arguments advanced by Dr. Klein in support of his views on this subject. They prove nothing more than that fungi grow on and in animal tissues which are not adequately preserved—a fact which no one will doubt. That, on the other hand, the micro-organisms found by us are of this nature is a matter which neither Dr. Klein nor any other person who is unacquainted with the facts is in a position to decide. Since a short preliminary account only of the work done by Dr. Graham Brown, Mr. Sherrington, and myself, on the pathology of cholera has as yet been published, Dr. Klein has not before him the facts on which alone a decision of any value is possible.

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December 30, 1886

An Error in Maxwell's "Electricity and Magnetism"

THE criticism of Mr. McConnel upon Maxwell's derivation of the inductive action of currents from the principle of energy is perfectly correct. It is inconsistent with the experimental facts appealed to by Mr. McConnel and Mr. Maxwell's own treatment of the field as the seat of electro-kinetic energy.

In the excellent treatise of Messrs. Mascart and Joubert, a similar misleading appeal is made to Helmholtz's proof, and I have little doubt that Maxwell has correctly stated it. I should be inclined to think that the existence of the energy of the field was not distinctly present to Helmholtz's mind.

Maxwell, as is well known, by an ingenious application of Lagrange's equations of motion, proves that, in the case of two currents, this electro-kinetic energy  $T_e$  is given by the equation—

$$T_e = \frac{1}{2}(L_1 i_1^2 + M i_1 i_2 + L_2 i_2^2),$$

where  $M = \int \int \frac{\cos \epsilon}{r} ds ds'$  taken round both circuits, and  $L_1$  and  $L_2$  are similar expressions for the separate circuits.

I believe, though I dare not trespass upon your space to give the reasoning *in extenso*, that this result may be obtained somewhat more simply and without the use of the Lagrange equations, a treatment which has the disadvantage of assuming the electric co-ordinates  $y_1$  and  $y_2$ , the currents being  $j_1$  and  $j_2$ . Then the equation of energy becomes

$$A_1 \dot{i}_1 + A_2 \dot{i}_2 = \frac{dT_e}{dt} + \frac{dT_m}{dt} + R_1 i_1^2 + R_2 i_2^2,$$

where  $T_m$  is material kinetic energy, and  $\frac{dT_m}{dt} = i_1 i_2 \frac{dM}{dt}$

supposing the circuits rigid. Therefore

$$A_1 \dot{i}_1 + A_2 \dot{i}_2 = i_1 \left\{ \frac{d}{dt}(L_1 i_1 + M i_2) + R_1 i_1 \right\} + i_2 \left\{ \frac{d}{dt}(L_2 i_2 + M i_1) + R_2 i_2 \right\},$$

reducing to Mr. McConnel's equation, when the currents are constant.

In the case of two circuits thus moving in connection with their batteries we may infer that  $A_1$  and  $A_2$  must be such functions of  $i_1$  and  $i_2$ , and the coefficients of configuration, that, when the suffixes are interchanged in the expression for  $A_1$ , that for  $A_2$  must result, and *vice versa*. If this be so, then the aforesaid equation necessitates the separate equations—

$$A_1 = - \frac{d}{dt}(L_1 i_1 + M i_2) + R_1 i_1;$$

$$A_2 = - \frac{d}{dt}(L_2 i_2 + M i_1) + R_2 i_2.$$

Or Maxwell's equations are obtained without the use of Lagrange.

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The Manipulation of Glass containing Lead

IN a note on this subject in NATURE (Dec. 16, p. 150), Mr. H. G. Madan has made a suggestion which is likely to be very valuable to those who require to manipulate "combustion-tubing" before the blow-pipe. But, in proposing the employment of oxygen in place of air to produce flames for heating glass containing lead, Mr. Madan introduces a refinement which is unnecessary; for lead-glass may be quite as easily manipulated in flames produced by plain air and gas as soda-glass itself. The pointed flame should be employed for small objects, and the oxidising brush-flame in the case of larger objects. By the oxidising brush-flame, however, I do not mean the brush-flame as ordinarily employed, but one to which the air is supplied liberally through an air-tube without any contraction at its end, and at a steady pressure from a good blower; care being taken, on the other hand, not to introduce such an excess of air as to reduce the temperature of the flame.

In his note, Mr. Madan quotes me as saying, in the "Methods of Glass-blowing," that the reduction of lead-glass may be prevented or remedied by holding the glass a little in front of the visible flame, with the comment that there is hardly enough heat in that region to do all that is required in the manipulation