motion from west to east. Another paper on the same subject is contributed to the *Bulletin* by S. Kostinsky. In this case, the observations discussed were made with the great meridian instrument of the Pulkova Observatory, mounted in the prime vertical. The period obtained was 411 days, and the amplitude 0″·541. In addition to these papers, there is one on the orbits of Bielid meteors, deduced by M. Bredichin from observations made in 1892.

ON A REMARKABLE EARTHQUAKE DISTURBANCE OBSERVED AT STRASSBURG, NICOLAIEW, AND BIRMINGHAM, ON JUNE 3, 1893.

INTRODUCTORY NOTE.

THE Horizontal Pendulum.—The observations described in the subjoined article were made with the horizontal pendulum designed by Prof. Zöllner, and modified by Dr. von Rebeur-Paschwitz. This instrument consists of three thin brass tubes jointed together in the form of an isosceles triangle, the vertical angle of which is about 45°. The two equal sides are prolonged slightly beyond the base, and to the ends are attached two small spherical agate cups, the concavity of the lower one being directed from the centre of gravity of the pendulum, and that of the upper one towards it. When the pendulum is placed in position, these cups rest on two steel-points attached to the stand of the instrument and directed normally to the surfaces of the agate cups. One steel-point is almost exactly above the other, so that the axis of rotation is nearly, but not quite, vertical, its inclination to the vertical being still great compared with the movements of the ground we wish to investigate. The pendulum rests in the vertical plane passing through the axis of rotation, and on the side towards which it inclines. If this is towards the east, and if the axis is slightly tilted in the east and west plane, there will be no deflection of the pendulum ; the only change will be in its sensitiveness. But if the axis is tilted in any other plane, it will no longer incline towards the east, and the pendulum will be deflected from its original position, in order to remain in the same vertical plane with the axis of rotation. It is evident that the smaller the original inclination of the axis to the vertical, the greater will be the deflection for a given tilt of the axis in the north and south plane ; that is, the greater will be the sensitiveness of the pendulum.

From the middle of the nearly vertical tube of the pendulum, there projects outwards a small bar. Passing through an aperture in the frame to which the steel-points are attached, this bar carries a mirror, whose plane is at right angles to that of the pendulum. A ray of light, proceeding from a fixed source, is reflected by the mirror, and registers the movements of the pendulum on a strip of photographic paper wrapped round a revolving drum. The zero-line is traced by a ray of light reflected by a fixed mirror just below the other, and attached to the stand of the instrument.[1]

Observation of Earthquake Pulsations.—Nothing could show better than Dr. von Rebeur-Paschwitz's interesting paper how desirable it would be to have a few well-chosen stations in different parts of the world where these pulsations could be registered. They might then be traced as they spread out from the origin of a great earthquake, and might even be followed, as he suggests, in their course, completely round the world.

In several Italian observatories there are established instruments suitable for this purpose. Horizontal pendulums, with recording apparatus, are now at work at Charkow and Nicolaiew in the south of Russia ; and two others will soon be ready at Strassburg and Merseburg in Germany. A bifilar pendulum[2] at Birmingham, belonging to the British Association, will shortly be furnished with a photographic recorder. Thus Europe is at present fairly well provided for.

A large number of stations in other parts of the world is by no means absolutely necessary. Results of great value would be derived if recording instruments were erected at places near the east and west coasts of North America, in South America, South Africa, India, Australia or New Zealand, and the Sandwich Islands. In Japan Prof. Milne's tromometer[1] leaves little to be desired.

[1] For a fuller account of the horizontal pendulum, see Dr. von Rebeur-Paschwitz's great memoir, "Das Horizontalpendel" (*Nova Acta der kais. Leop. Carol. Deutschen Akademie der Naturforscher*, Bd. lx. 1892, pp. 1-216) ; also Brit. Assoc. Rep., 1893. pp. 303-308.

[2] NATURE, (July 12, 1894), vol. 50, pp. 246-249 ; Brit. Assoc. Rep., 1893, pp. 291-303.

The chief element to be determined is the exact epoch of the beginning, maximum amplitude, and end of the pulsations, or of each group of pulsations. The horizontal pendulum, Dr. von Rebeur-Paschwitz informs me, can be arranged so that its sensitiveness for slow tilts of the ground can be diminished without necessarily lessening its sensitiveness for earthquake shocks. The strip of photographic paper can thus be reduced in width without running any risk of the spot of light leaving the paper during its ordinary daily and other movements. Without increasing the expense, a more rapid movement of the paper could be permitted, and this would enable the determination of the time to be made with greater accuracy. Possibly, also, the construction of the instruments might be simplified if earthquake-pulsations are to be the principal subject of investigation. In the bifilar pendulum, for example, since the amplitude of the oscillations is a point of minor importance, the somewhat elaborate machinery for determining the angular value of the scale divisions might be dispensed with, and also the arrangements for readjusting the spot of light from a distance.

Hardly less important in these investigations is the determination of the exact time of occurrence of the earthquake at or near its centre of disturbance. But on this it is the less necessary to insist, for in so many of the more marked seismic districts there now exist organisations for the study of earthquakes. It may not be out of place, however, to suggest that in all seismic records, and in every part if periodically published, the standard time employed should be clearly stated. It is not universally known, for instance, that, in Japan, Tokio time was replaced on January 1, 1888, by the time of 135° E. long. In accounts from Beluchistan, again, we cannot be certain whether Madras time or railway time is meant, for both are used. The trouble of inserting this important detail is hardly to be compared with the confusion and error that may result from its omission.

C. DAVISON.

IN the last report of the Earth Tremor Committee of the British Association, reference is made to an observation of earth-pulsations by Mr. C. Davison on the evening of June 3, 1893, at Birmingham, which was obtained by the aid of Mr. H. Darwin's bifilar pendulum. I take the following details from the report :—At 5.43 p.m. (G.M.T.) the image was found to be perfectly steady, but at 6.29, when the observer returned to the cellar, it was moving slowly and steadily from side to side of the field of view, thus indicating the passage of a system of earth-waves. At 6.42 the image had come to rest, but at 6.46 the oscillations commenced again, and continued to be visible with varying amplitude until 8.13. After 8.13, though the observer watched for two hours and a half, no further motion was noticed. The period of the waves was found by a number of observations to be between fifteen and twenty seconds, and the range of motion at its maximum one-eighth of a second.

Mr. Davison's observation is especially interesting, because it corresponds exactly with a *very extraordinary disturbance* which was registered by the horizontal pendulums at Strassburg and Nicolaiew. Amongst the considerable number of disturbances common to both these places, that of June 3 is certainly the most prominent during the interval from January 1 to September 4, 1893. In the accompanying illustration (Fig. 1) the two curves, obtained by photography, are shown side by side ; in correspondence with the difference of longitude between the two places, the lower curve was moved 17·5 mm. to the left. The pendulum in both cases was placed in the east-west plane. In the following notes the time is Greenwich Mean Solar Time, and is given in decimal parts of the hour.

(*a*) *Strassburg.*—The disturbance begins suddenly and small at 4·42, the curve having been perfectly sharp and steady before. The range of motion increases to 4 mm. at 4·52 and decreases at 4·69. It then again increases so as to make the curve disappear entirely between 4·77 and 5·05. During the interval the light-point was displaced by 3½ mm. to the north, which corresponds with a deflection of the pendulum towards the south. At 4·82, the person who keeps control over the instrument entered the cellar, to look after it and to determine the time correction, which is done by shutting off the light during

[1] Brit. Assoc. Rep., 1892, pp. 107-109.

a known interval of five minutes. He then locked the cellar, and when he returned at 8·45 he was obliged to make a correction,[1] because the light-point had left the paper. Unfortunately, he forgot to note down its exact place, but from the inspection of the curve it is evident that at 5·61, after a short interval of steadiness between 5·25 and 5·61, the pendulum received a sudden shock, which caused it to oscillate, and at the same time produced a deflection, by which the light-point was probably brought off the lower edge of the paper, from which it was distant 48 mm. at the time of the shock. There can be no doubt that such was the cause of the disappearance of the curve, for the base-line runs on perfectly undisturbed, which is a sign that the instrument continued to be in good working order, as usually. From 8·45 to 9·65 the motion is small; and from 9·65 till 11·16 the curve is nearly perfectly steady.

At 11·16 a new disturbance begins; the range of motion is very small at first, but increases to 5 mm. at 11·45, and to 10 mm. at 11·60; at 12·10 the disturbance, which is much like the first one, comes to an end, and is again followed by a steady part of the curve.

At 12·26 commences the last disturbance, which at 12·47 increases to 6 mm. Between 12·73 and 13·03 no traces of the curve are visible, and during this interval a displacement of 10½ mm. has occurred, which indicates a deflection of the pendulum towards the north. The motion continues to be visible until 14·45; the curve then resumes its nearly steady appearance, which is once again interrupted by small motion at 14·95.

(*b*) *Nicolaiew.*—The following details were communicated to me by Prof. Kortazzi, who informed me that on this day he went down into the cellar one half-hour later than usually, at 6·54, when he found that the light-point had passed from the paper on to the brass rod, which serves to clamp the paper, and was swinging considerably. From this reason the light-point could leave no traces on the paper between 5·95 and 6·62. The disturbance is very large and of long duration. It commences at 4·32 and reaches its first maximum at 4·80, when the range is >60 mm. Strong motion continues until 8·4. From the copy of the disturbance, which Prof. Kortazzi kindly sent me, and which is represented in the above figure, it appears that at about 5·77, or 11m. *before* the light-point was prevented to trace a curve, by passing on to the brass rod, the curve was suddenly interrupted, which shows that the pendulum was performing large oscillations. Between 9·72 and 11·05 the motion is small. At 11·05 a new disturbance begins, which increases suddenly at 11·36, diminishes a little at 12·3, and increases again at 12·47. From 12·7 to 13·22 no traces of the curve are visible. At 13·9 the motion decreases considerably, and after another small increase at 14·87 reaches its end at 15·17.

The figure shows that the motion at Nicolaiew is much more considerable than at Strassburg. Whilst at the latter place the whole disturbance is divided into four distinct parts, which are separated by moments of nearly perfect steadiness, at Nicolaiew the first and second, as well as the third and fourth part, each form a continuous disturbance.

If we denote by V the relative strength of a shock in a direction normal to that of the pendulum, by a the range of motion, measured on the curve, by d and T the distance between the photographic drum and the pendulum mirror, and the period of oscillation of the pendulum, then we have the following relation between the observations in two different places:—

$$\frac{V_1}{V_2} = \frac{a_1}{a_2} \cdot \frac{d_2 T_2}{d_1 T_1}.$$

In the present case

$$d_1 = 1{\cdot}8\text{m.}, \ d_2 = 4{\cdot}6\text{m.}, \ T_1 = 17{\cdot}0\text{s.}, \ T_2 = 10{\cdot}2\text{s.},$$

thus $\frac{d_2 T_2}{d_1 T_1} = \frac{47}{31}$. A shock of the same strength therefore produces at Nicolaiew a disturbance 1½ times as large as at Strassburg.

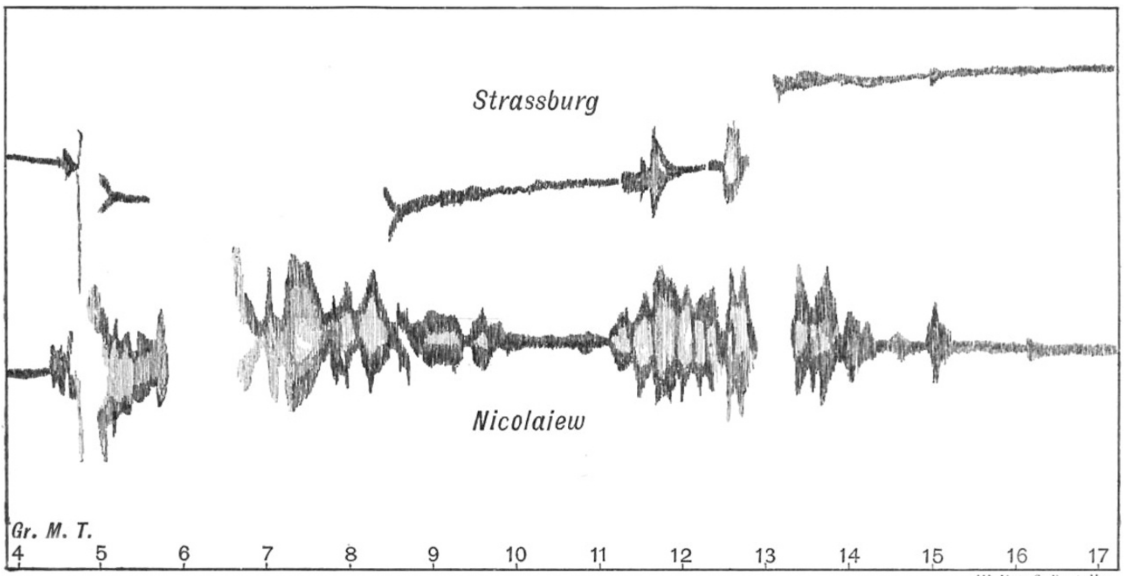

FIG. 1.—Earthquake Disturbance observed at Strassburg and at Nicolaiew on June 3, 1893.

In comparing the two curves, it is evident that the different intensity of motion at the two places is not due to the difference in the values of the instrumental constants. The reason why the motion of the pendulum is so much stronger at Nicolaiew is this, that the soil consists down to a great depth of sand, which is particularly favourable for the development of strong motion. In this respect Nicolaiew resembles the two former stations, Potsdam and Wilhelmshaven. Many facts tend to show that the soil at Strassburg, though often disturbed by small earthquakes of distant origin, never oscillates as much as at the forenamed places. It would not be right, therefore, from the mere look of the curves, to draw the conclusion that the earthquake—if such was the cause of the disturbance—must have originated at a place considerably nearer to Nicolaiew than to Strassburg.

Until now I have not been able to find a record of a phenomenon which might possibly be connected with this disturbance. From its size and duration, one ought to think that it must have been caused by a strong catastrophe, surpassing anything that has been reported during the last year from all parts of the world. But it is strange that the magnetic recording instruments at Potsdam have shown no trace of motion, and that nothing is reported from the delicate seismological instruments which are at work in Italy.

[1] In the original photograph the second part of the curve is much displaced, in the same way as the third part after the interruption. This was altered in the figure to economise space.

The case is remarkable in more than one respect. Displacements of the light-point, which, though the oscillations of the pendulum were much larger generally, were scarcely noticeable during the former observations with this instrument at other places, often occur at Strassburg. I am inclined to think that they are due to a vibratory motion of the ground, which scarcely affects the motion of the pendulum, but may cause a change in its position with regard to the steel pivots. These vibrations appear to be more easily propagated by the soil at Strassburg than at Nicolaiew, for though small displacements occasionally occur at the latter place, they are considerably smaller. This is particularly evident in the present case, where the only displacement worth mentioning is connected with the shock at 5·77. On the other side, the displacement at Strassburg, which produced the long break in the curve, is far the largest that occurred during one and a half years' observation. It is much larger than that which took place when an iron hook was driven into the pillar on the side opposite to the pendulum.

Our figure shows that the displacements of the pendulum were comparatively larger during the first and second than during the third and fourth disturbance. The change during No. III. is about 1 mm. Another fact worth noting is that in the two first cases the pendulum is deflected towards the south, and in the two last towards the north. This seems to indicate, if one considers the special arrangement of the instrument, that the motion arrived from the north in the first and second, and from the south in the third and fourth case. The displacement of the pendulum at Nicolaiew at 5·77 is also directed towards the south, in accordance with the observation at Strassburg.[1] The above conclusion is founded on the supposition that the displacement is produced by a *single* shock, which causes the steel-points connected with the stand of the instrument to slip on the agate cups. In reality, the motion is probably much more complicated, and perhaps one is not justified in supposing the direction of the shock to be opposite to the deflection of the pendulum. The comparison of the observed times, indeed, leads to a different result.

The following table gives a summary of the observations:—

Again the times of disappearance of the curve or of maximum motion are separated by nearly the same interval, viz., at Strassburg III.–I. = 6·83h., IV.–II. = 7·12h.,[1] and at Nicolaiew IV.–II. = 6·9h. The duration at Strassburg of No. I. is 0·83h., and of No. II. 0·94h.; the duration of No. III., if we omit the last part, in which the motion was very small, is 2·84h., of No. IV. 2·69h. At Nicolaiew, during the first half of the disturbance, the strong motion ends 4·08h. after the beginning, and the second part lasts 4·12h. The intensity of I. and II. is evidently larger than that of III. and IV.

We will now see if the direction of motion can be determined by the observations.

I. Though the first trace of motion is 0·10h. earlier at Nicolaiew than at Strassburg, yet it is probable that the corresponding moments are those of the disappearance of the curve at Strassburg and of maximum oscillation at Nicolaiew, or 4·77h. and 4·8h. To judge from the copy, which Prof. Kortazzi sent me, the latter value is only approximate. The difference in time is certainly small, and the direction of the motion remains rather uncertain; the general aspect of the figure, however, makes it more probable that it came from the east.

II. The time of disappearance of the curve at Strassburg, 5·61h., is probably correct within 0·02h. or 0·03h. Mr. Davison's observation shows that the motion, which in this case appears to have commenced suddenly, had not reached Birmingham[2] at 5·72h.; on the other hand, the disappearance of the curve at Nicolaiew took place at 5·8h., or about 12m. later than at Strassburg. If these times were all correct, and if the three moments really corresponded with the same phase, the centre of disturbance ought to be looked for somewhere at the south-west and not too far away from Strassburg. This, however, is a very improbable result. Mr. Davison's instrument could only indicate an east-west tilt, and perhaps the motion had already set in when he left the instrument at 5·72h., but was not perceptible enough in the east-west direction.[3]

The two other observations make it nearly certain that the motion arrived from the west.

III. The case is very much like No. I.: probably the motion arrived at Nicolaiew first, but its direction cannot be determined with certainty.

Disturbance	Strassburg.	Nicolaiew	Birmingham
No. I. (displacement − 3·5mm)	h. 4·42 first trace 4·52 increases 4mm. 4·69 decreases and increases again 4·77 curve disappears 5·05 reappears 5·25 end } curve steady during the interval	h. 4·32 first trace 4·8 first maximum>60mm.	
No. II. (displacement probably > − 48mm.)	5·61 new shock 8·45 } light point corrected 9·65 } motion small	5·8 } curve disappears strong motion 8·4 }	h. 5·72 the image was found to be steady 6·48 strong motion 6·70–6·77 steady again 8·22 end
No. III. (displacement + 1mm.)	11·16 } nearly steady 11·45 first increase 5mm. 11·60 second increase 10 mm. 12·10 end } curve steady 12·26 first small motion }	9·72 } small motion 11·05 } 11·36 sudden increase 12·3 diminishes	
No. IV.? (displacement + 10.3mm.	12·47 increase 6mm. 12·73 curve disappears 13·03 reappears } motion small 14·45 } 14·95 new small increase	12·7 curve disappears 13·22 ,, reappears 13·97 decrease of motion 14·87 new increase 15·17 end	

When looking over these figures, one is inclined to think that the remarkable correspondence between the several phases of the disturbance cannot be due to chance. If we take as the beginning of a disturbance the moment when its first traces are visible, we have the following differences at Strassburg:—II.–I. = 1·19h. and IV.–III. = 1·10h., III.–I. = 6·74h., IV.–II. = 6·65h. At Nicolaiew, where I. and II., III., and IV. appear as a single disturbance each, we have III.–I. = 6·73h.

[1] In the figure the curve is displaced in an opposite direction, but this is the case because the drum stands west of the pendulum at Nicolaiew, and east of it at Strassburg.

[1] The beginning of II., though sudden and sharp, need not necessarily coincide with the movement of greatest motion; in this case the difference IV.–II. would have a smaller value.

[2] The distance between Strassburg and Birmingham is about 800 kilometres.

[3] [Much weight cannot be attached to the absence of observed motion at Birmingham at 5·72h. The image of the wire was adjusted on the cross-wire of the telescope without difficulty, and must have remained practically in contact for a few seconds. A small movement, with a period so long as twenty seconds, might easily at this time have escaped notice.—C. D.]

IV. The curves again disappear at about the same time; but to judge from the time of greatest steadiness before the disturbance commenced at Nicolaiew, it appears to have reached Strassburg first. The last small increase at 14·87h. and 14·95h. is, on the contrary, earlier at Nicolaiew than at Strassburg, but this might be an independent disturbance. After the strongest motion, the light-point resumes its steadiness much sooner at Strassburg than at Nicolaiew.

It is evident that the case is, on the whole, not favourable to an hypothesis which first occurred to me, that all four disturbances might have been caused by four successive waves emanating from a single centre and a single shock, and circulating round the earth. The fact that II. and IV. are more considerable than I. and III. does not appear of much importance, for it is proved by many examples that the intensity of a disturbance is not alone dependent from the distance from the centre; but, if the hypothesis were right, disturbances III. and IV. ought probably to be much smaller. Besides, the velocity of about 100 km. per minute would be a very small value compared to those determined on other occasions.

I reject this hypothesis, but I do not think it improbable that I. and II., III. and IV. may be connected in the way just mentioned, and that both disturbances came from the same part of the world. It is the principal object of this communication to induce persons interested in the subject to study carefully the records of all self-registering instruments. If the disturbance originated at the bottom of the sea, something about it might be found in the ship journals, the tidal records might show a trace, or perhaps the magnetical records at distant places. I have many proofs that the size of a disturbance, traced by the horizontal pendulum, is not always a measure for the importance of the catastrophe which produced it; but in the present case many instances indicate an extraordinary phenomenon, of which an account is likely to appear sooner or later, in case it should have taken place at some remote corner of the earth.

Merseburg, May 18.

P.S.—Some time after having written the above, I received the third volume of the *Seismological Journal of Japan* (1894), in which there is an interesting paper by F. Omori on the eruption of Azuma-san in 1893. From this paper it appears that the volcano was in an active state since May 19, when an explosion took place, which was followed by two other ones on June 4, 4.10 a.m., and on June 7, of which the former is said to have been the strongest. It was accompanied by an earthquake, which was felt at the meteorological station of Fukushima. Supposing the above time to be Standard Time (9h. east of Greenwich), the explosion took place at 7h. 10m. p.m. G.M.T. on June 3, and thus it is seen that it coincides with a part of our great disturbance. I do not, however, believe that this is more than a casual coincidence, for the two other eruptions produced no disturbances. It is also a well-known fact that volcanic eruptions, even when accompanied by earthquakes, are generally not felt to any great distance, unless they bear a very violent character, like the eruption of Krakatoa; but from Mr. Omori's description it appears that the eruption of Azuma-san was nothing very extraordinary. I therefore believe that we must wait to find another explanation for our disturbance.

E. VON REBEUR-PASCHWITZ.

EXPLOSIONS IN MINES.

In a lecture on some modern developments in explosives, given at the Society of Arts on December 17, Prof. Vivian B. Lewes threw out a suggestion as to the cause of explosions in dusty mines free from fire-damp, which explains the anomalies which have presented themselves in several recent explosions.

It was pointed out that until quite recently explosions in mines were always attributed to the accidental ignition of mixtures of air and methane, to which the name of "fire-damp" is given, and undoubtedly this cause is the prime factor in this class of disaster, and the introduction of such precautions as safety-lamps at once brought about a considerable reduction in the number of explosions taking place. Many disasters, however, still continued to occur under apparently mysterious circumstances, the conditions being such that any large proportion of methane in the air of the mine appeared practically impossible, but investigations of such explosions showed that coal-dust in a dry and finely powdered condition had generally been present in the mine at the time of the explosion, and the coked residue of this dust was found afterwards on the surface exposed to the explosive wave, and years of experimental investigation by scientific men of the greatest ability proved the fact that air containing so small a proportion of methane as to be itself perfectly non-explosive, becomes a good explosive again when holding dry and finely divided coal-dust in suspension, and within the last few years explosions having taken place in mines, which have always been celebrated for their freedom from any trace of methane. Further experiments have been made by Mr. H. Hall and Mr. W. Galloway, who have shown that the violent ignition of dust-laden air is possible by a blown-out shot, even if free from any trace of marsh gas, and there is evidence to show that the explosion is developed in throbs or waves.

It is therefore found that the explosions in mines may be brought about, first, by the ignition of a mixture of methane and air, in which the former rises above a certain percentage; secondly, by mixtures of air, coal-dust, and methane, in which the amount of the latter may be excessively small; lastly, by mixtures of coal-dust and air. With regard to these explosions caused by coal-dust and air alone, the Royal Commission on Explosions from Coal-Dust in Mines, in their second report, published this year, say:—

"On a general review of the evidence on this point, we have no hesitation in expressing our opinion that a blown-out shot may, under certain conditions, set up a most dangerous explosion in a mine, even where fire-damp is not present at all, or only in infinitesimal quantities; and while we are prepared to admit that the danger of a coal-dust explosion varies greatly according to the composition of the dust, we are unable to say that any mine is safe in this respect, or that its owners can properly be absolved from taking reasonable precautions against a possible explosion from this cause. But even if we had been able to come to a different conclusion, and to agree with the minority of the witnesses examined, who think that coal-dust alone cannot originate an explosion, we should still have to call attention to the serious danger which results from the action of coal-dust in carrying on and extending an explosion which may originally have been set up by the ignition of fire-damp."

One of the most interesting and instructive explosions which have taken place recently was that which occurred a little more than a year ago at the Camerton Collieries, Somersetshire, in which as far as investigation could go, no trace of combustible gas could be found in the mine at any period prior to the explosion or subsequent to it, and in which everything pointed to the explosion being entirely due to the presence of dry coal-dust in the air.

Of absorbing interest, also, are the experiments made by Mr. Hall at the latter end of 1892 and the early part of 1893, and reported upon by him to the Secretary of State on January 23, 1893, in which he shows by conclusive experiments that dry coal-dust under conditions frequently present in coal mines and in the entire absence of fire-damp, may be inflamed by a blow-out gunpowder shot, and cause a disastrous colliery explosion.

The evidence which can be collected from the investigation in the Camerton disaster, and from Mr. Hall's experiments, point to a cause for such explosions, which has apparently been overlooked, and which Prof. Lewes thought worthy of the gravest attention. Both at the Camerton Colliery and in Mr. Hall's experiments, powder was the blasting agent used, and such powder as is employed for this purpose, gives amongst the products of combustion nearly half the volume of permanent gases in the condition of carbon monoxide, methane, and hydrogen.

In the Camerton explosion, it seems probable that about 1¼ lbs. of such powder were used in the shot which caused the disaster, and this quantity of powder would give, roughly, a little over three feet of inflammable gas, which when mixed with pure air would give over 10 cubic feet of an explosive or, at any rate, rapidly burning mixture, and experiments which have been made upon the effect of fire-damp and dust combined in causing colliery explosions show conclusively that even when the fire-damp is present in such minute quantities as to form a mixture very far removed from the point of explosion, it still makes the mixture of coal-dust and air highly explosive; and from experiments which Prof. Lewes has made, it is clear that traces of