definite shifting of the centre of gravity to one side would have a more marked effect (since the lateral extension of the whole structure was little more than half of that formerly used), but would also show a greater stability, a result all the more to be expected, as the centre of gravity of the system was placed more than a metre below the upper surface.

Experiments, which were begun without loss of time, seemed to bear out this conclusion. Lilienthal appeared to have suddenly gained in power and in the faculty of shaping his motion at will. It seemed to be only a question of time or opportunity that the great step would succeed of describing a complete circle in the air (which always appeared to us to be the key to a definite, if not complete success), when the disastrous accident occurred which has cost the bold experimenter his life.

The following is, as nearly as I can remember it, the report of the mechanic who used to build Lilienthal's wings, and to help him with his experiments.

On Sunday, August 9, Lilienthal had gone out to the village Rhinow, where he used to practise on the bare sand-hills in the neighbourhood. Nobody was with him except his mechanic. The weather was exceptionally favourable, a light wind blowing from the east with a velocity of about 5–6 m. per second.

Lilienthal had selected one of these new two-story surfaces, which, in a considerable number of trials from the artificial cone in Lichterfelde, had shown itself to be especially successful. He took one flight, by way of warming to his work, and then prepared himself for a second, and gave the word to his man to look at his watch and note the duration of the flight. The man saw him soar down until he was nearly above the foot of the hill, then suddenly a gust of wind set in, lifted him up to a height of 30 m. above the ground—according to his man's estimate—and there he stood apparently motionless in the air.

This was a frequent occurrence, and gave no cause for alarm at first; but now the man saw how Lilienthal gradually lowered the fore-edge of his wings more and more, without obtaining the desired effect of getting way forward and downward. The man felt uneasy at this, pocketed his watch, and began to run towards the spot where his master was hanging suspended in mid-air. Suddenly he saw the apparatus heeling over forward still more, and then Lilienthal came down with it with great force head foremost, rolled over once or twice after striking the ground, and remained motionless.

When the man reached the spot, he found the apparatus much shattered, but Mr. Lilienthal apparently uninjured though without consciousness. The local physician was instantly summoned, and at first declared that nothing serious had happened. Lilienthal was brought to the neighbouring inn, and within two hours recovered his senses. He seems to have felt no pain, because he immediately declared he would soon get up and continue practising. However, his arms and legs were lamed. It appears that his spine was fractured.

The man left him to the care of the physician, and took the next train to town to fetch his brother. When the brother came, he found that he had swooned again; and he did not recover his consciousness until death set in, which occurred the same night.

By publishing these lines the editor of NATURE will, I think, fulfil a duty he owes the scientific world, as well as the memory of a man who, throughout his toilsome life, applied his rare energy, courage, and ability to the solving of a problem which has hitherto baffled the ingenuity of all modern engineering.

Lilienthal, who was a successful engineer and manufacturer, has not lived to see his forty-eighth birthday. He leaves a widow and three children.

A. DU BOIS-REYMOND.

Berlin, August 24.

Laboratory Use of Acetylene.

Now that acetylene has come so much into prominence, an instance of its use in a laboratory which possesses no gas supply may be an encouragement to any one similarly situated. Long doomed to the use of spirit-lamps, "benzoline roasters," and the like, the cheap production of acetylene has come as a great boon to us, and is now in regular use for blow-pipe work. The apparatus in use consists of an aspirator holding about fifteen litres, permanently connected with a water supply, and possessing a $\frac{1}{4}$ -inch aperture exit tap (the water flows in from below to minimise absorption); at the top a three-hole rubber cork carries

an upright pipe passing through the table, which serves for filling the aspirator with gas, or using the gas on the table, a second pipe goes to the blow-pipe, and a third carries an open mercury manometer. For filling the jar, the calcium carbide is placed in a four-ounce bottle closed by a cork carrying a small separating funnel from which the water drops; the gas passes to the aspirator through a wide glass tube which acts as a reversed condenser, returning most of the water vapour to the bottle. With the large exit to the aspirator the gas can always be collected under a reduced pressure of several cms. of mercury, which quite provides against any sudden rushes of gas; the operation takes some ten minutes, and requires practically no attention.

In using the gas the water is turned on with all taps closed for a few seconds, to correct any reduced pressure caused by absorption, as shown by the gauge (this is very slight indeed), and then the gas-tap fully opened and the flame regulated entirely by the water entrance. To bring the gas into use takes hardly any longer than with an ordinary gas blow-pipe. A good fusion on platinum foil (*e.g.* $\text{BaSO}_4 + \text{Na}_2\text{CO}_3$) may be effected by using about one litre of the gas. We have used the apparatus for about two months, and I recently discovered that some of my junior workers did not know what acetylene smelt like, which speaks well for it if not for them. I am hoping to introduce the gas on to the benches if the difficulty of the enormous quantity of air required to produce a non-luminous flame can be overcome.

A. E. MUNBY.

The Laboratory, Felsted School.

Coal-dust.—A Question of Priority.

IN the report of a lecture given *in extenso* at page 64, *et seq.*, in the *Colliery Guardian*, for July 10, on "Coal-dust and Explosives," by Mr. H. Richardson Hewitt, of Derby, H.M. Inspector of Mines, the following remarkable statements occur:—

"It was but a few years ago that the Messrs. Atkinson first drew attention to their idea that coal-dust was a dangerous element in mines where blasting operations were carried on . . ."

"After Messrs. Atkinson first drew attention to the subject, Prof. Galloway took it up and made some rough experiments by placing gunpowder cartridges in heaps of coal-dust and firing them in the dark."

Although these statements were obviously uttered in ignorance of the nature of my experiments, they raise a distinct and palpable issue as to priority.

The facts are as follows:—

My first experiments with coal-dust were made on July 3, 1875. I then discovered that a mixture of air and fire-damp, which is not inflammable at ordinary pressure and temperature, on account of the smallness of the proportion of fire-damp present in it, becomes inflammable when coal-dust is added to it, and can be ignited by means of a comparatively small flame.

On December 22, 1875, I gave evidence in the capacity of Assistant Inspector of Mines at the Coroner's inquest on Llan Colliery Explosion (South Wales District), when I attributed that explosion principally to the influence of coal-dust. My evidence was discountenanced by the Chief Inspector of Mines for the district, and was not embodied in the Reports of the Inspectors of Mines, but it was reported *verbatim* in the two local newspapers (*Western Mail* and *South Wales Daily News*) of December 23, 1875.

On March 2, 1876, I read my first paper, entitled "On the Influence of Coal-dust in Colliery Explosions," before the Royal Society. In that paper I announced the coal-dust theory.

In 1878 I published a large number of articles in *Iron*, under the title of "Coal-dust Explosions." In these articles, amongst many other things, I quoted and commented upon what Faraday and Lyell had written about coal-dust upwards of twenty years previously, and I gave complete translations of the papers that had been published in France, having a bearing upon the subject.

Besides contributing a number of other articles and papers on the same subject to various societies and periodicals, I read altogether five papers "On the Influence of Coal-dust in Colliery Explosions" before the Royal Society, viz.: March 2, 1876, already referred to; February 27, 1879; May 30, 1881;

December 29, 1881; May 8, 1884; and one on "A Coal-dust Explosion," February 17, 1887.

During the ten years ending in 1885, I was engaged from time to time in carrying out experiments with coal-dust: first, with apparatus provided by the Glamorgan Coal Company, Limited, and erected at their Llwynypia Colliery; secondly, with apparatus purchased by means of Government grants obtained through the Royal Society; and, thirdly, with apparatus belonging to the Royal Commission on Accidents in Mines.

Before the accounts of my earlier investigations, and the conclusions founded upon them had appeared, the Inspectors of Mines and other mining experts were practically unanimous in attributing the cause of every great colliery explosion to the sudden outburst of a large volume of fire-damp which was supposed to have flooded the workings, become mixed with the air, and, on being ignited in one way or another, produced the various phenomena subsequently observed. This explanation was accepted everywhere as the only one possible; it was recorded in the official reports of the Inspectors of Mines, and they, as well as the experts of that generation, were irretrievably committed to it.

There was not, figuratively speaking, a ripple of dissent from this mode of explanation upon the placid surface of mining opinion at the moment the coal-dust theory was launched upon it.

At first the new theory was ignored; then it was scouted; then it was subjected to scathing criticism; then it was taken up in a tentative manner by some of the younger and bolder men; and, lastly, when it was found to be making serious headway, one of the more adventurous spirits suddenly discovered that it was not new after all, for had not Faraday and Lyell and certain French engineers been its real authors?

Following my lead, first a joint paper, by Messrs. Hall and Clark, was contributed to the North of England Institute, in May 1876, then another by Messrs. Marrecco and Morrison, in 1878, all of whom, with the exception of Mr. Clark, had previously corresponded with me on the subject of explosions; finally, in the year 1879, after the publication of my articles on "Coal-dust Explosions" in *Iron*, and during the next few years afterwards, a very great army of investigators, headed by Government Commissions in England, France, Prussia, Austria and Saxony, and including the Messrs. Atkinson, entered the field.

Some of these investigators contented themselves with criticism pure and simple; others, of whom many had neither aptitude nor training for the work, made experiments with small and imperfect apparatus, and, as a consequence, obtained only negative results; still others were carried away by the side issues; and only a few, such as the Prussian and Austrian Commissions, and Messrs. Hall and Atkinson, H.M. Inspectors of Mines, did really good and substantial work of an enduring kind.

The facts and conclusions recorded in my earlier papers were freely drawn upon: by some they were generously acknowledged; by others they were first denounced and then assimilated; by others they were adopted without acknowledgment; while some of my experiments, and notably my investigations into the nature of the Fire-damp Cap (*Proc. Roy. Soc.*, March 2, 1876), were repeated with some variations and described as if they were original.

A flood of literature was now poured upon the mining world from every side, embodying opinions of the most conflicting and mystifying character, such as—a mixture of coal-dust and air may take fire but it cannot explode; coal-dust can only carry flame from one accumulation of fire-damp to another; a coal-dust flame cannot extend throughout the workings of a mine in the entire absence of fire-damp; a small proportion of fire-damp must always be present in the air when an explosion takes place; some kinds of coal-dust are more inflammable than others—and so on, so that amid the din and hurlyburly of the strife the main question of how to put an end to great explosions was almost lost sight of.

But the scene of each successive explosion when viewed under the new light served gradually to dispel the illusions which had fascinated the majority of the investigators for years; and thus it has come to pass that the new generation of Inspectors of Mines, and those who have been associated with them in investigating the phenomena of explosions, have become con-

vinced, I believe almost to a man, of the soundness of the coal-dust theory; and that the struggle of contending factions, which was at its height ten or twelve years ago, has gradually subsided, leaving us face to face with a work which still remains to be done, namely, to render the occurrence of a great colliery explosion impossible in the future.

Into the consideration of that problem I do not propose to enter on the present occasion, as I have lately done so in considerable detail in the pages of the *Daily Chronicle* of June 24 of the present year.

W. GALLOWAY.

Cardiff, July 17.

THE AUGUST METEOR SHOWER, 1896.

THE moon being absent from the nocturnal sky during the recent return of the Perseids, encouraged the hope that the shower would be somewhat brilliant; but the weather is an element of great importance in such observations, and it was by no means favourable during the late display. In the south of England several nights were partly clear near the important time, and on August 10 the firmament at Bristol was almost free from dark cloud; but the sky was hazy and the stars dim, so that only the brighter meteors were observed.

On August 6, during an hour's watch before 10h. 50m., I counted twelve meteors, of which seven were Perseids, with a radiant at $42^\circ + 56^\circ$. The shower was evidently pretty active, and the meteors fairly bright, but clouds overspread the sky before 11h., and prevented further observation.

On August 7, in an hour's watch preceding 11h., nine meteors were seen, including about six Perseids, but clouds were very prevalent during the whole time, and effectually obliterated the stars at a later period of the night.

On August 10 the weather was fine, but the atmosphere was not transparent enough to be considered favourable for meteoric work. Haze was spread over the sky, and the fainter stars were obscured. Near the horizon nothing could be discerned. I began watching for meteors at about 9h. 50m. and continued until 14h. 15m. During this interval of 4h. 25m. I saw ninety-eight shooting-stars, of which sixty-nine were Perseids, and twenty-nine belonged to the minor, contemporary showers of the period. I registered the apparent paths of a considerable number of the meteors seen, and while engaged in doing this, must have missed many others which appeared while my attention was diverted from the sky. It is probable that fully one hundred and fifty meteors would have been counted by an observer watching the sky uninterruptedly during the period mentioned. Nearly all the Perseids left streaks, but the meteors generally were not very bright. The radiant point was tolerably well defined, but it was certainly not so definitely marked as I have sometimes seen it. I determined it at different times of the night as follows:—

h.	m.	h.	m.				
9	50 to 11	0	...	...	...	43	+ 57
11	0 to 11	30	...	...	...	44	+ 57
11	30 to 13	0	...	...	...	46	+ 57
13	0 to 13	45	...	...	...	45	+ 59
13	45 to 14	15	...	...	...	46	+ 57

The mean of the five positions being at $45^\circ + 57^\circ$, which coincides with the usual place of the radiant on August 10.

On August 11 the heavens were overcast, but on August 12 a beautifully clear sky enabled me to resume observations. I saw fourteen meteors in about an hour and a quarter before 11h. 15m., and of these seven belonged to the Perseid shower. The radiant was at $46^\circ + 57^\circ$, but it was imperfectly defined.

On August 14 the firmament was again clear, and I saw nine meteors in three-quarters of an hour before