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 roarers," 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 ¼-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.* $BaSO_4 + Na_2CO_3$) 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;