admit of being explained by any previous theory. It only remains to add that, if true, the present theory ought to admit of experimental verification. Let well-marked natural varieties of plants growing on the same area be systematically tested with regard to their relative *degrees* of fertility, first within themselves, and next towards one another: let these experiments be made in successive years over a number of natural varieties, by carefully-conducted artificial fertilisation, and by counting the seeds and tabulating the results. In this way experimental evidence would probably be obtained of degrees of sterility between even slight though constant varieties growing on the same areas; and, if so, such evidence would serve as further proof of the present theory. But experiments of this kind, in order to be satisfactory, ought to be conducted by a number of observers in different geographical areas; and my object in publishing so lengthy an abstract of my views in this periodical is that of inducing naturalists in other parts of the world to co-operate with me in carrying out this research. The paper itself, which furnishes fuller particulars as to the way in which such experiments should be carried out, is published in a separate form by the Linnean Society.

THE WOODEND COLLIERY EXPLOSION

QUI s'excuse s'accuse will occur to the minds of many who have followed the details of the disastrous explosion which took place at Woodend or Bedford Colliery on Friday last. We read in the *Times* of the 16th inst.:—"The Four-foot or Crombonke Mine is a very dusty one, and it is considered that at the Woodend pit the dust has increased the extent of the damage." "But to water the mine, as suggested by the Commission, would here be a very difficult operation, because the floor of the mine consists of a species of fire-clay which, as it absorbs the water, causes a lifting of the ground, and so prevents mining operations being conducted." Inasmuch, however, as the floor of perhaps ninety-nine out of every hundred mines consists of the same kind of material, the same argument against watering would hold equally good in most cases, and, if it is allowed to pass, this recommendation of the Commissioners is likely to come to nothing. It has been pointed out more than once in NATURE that the amount of water required to lay the dust is very small—far less than would be necessary to materially affect the floor of a mine in the manner suggested, and it would perhaps be wiser to try the effect in the first place and judge by results rather than to meet the proposition with a simple *non possumus*. We speak thus plainly here, because many of the witnesses who gave evidence before the Commissioners brought forward the very same argument with the same degree of plausibility, and we have reason to believe without having put the matter to a practical test. Many of those who now water regularly, for the express purpose of laying the dust on floors consisting of fire-clay, admit that the water produces no appreciable difference when properly and carefully distributed.

The bursting of the gauze of a safety-lamp, described by one of the survivors, is so contrary to all reason and experience that it cannot be accepted as an explanation of the origin of the explosion. Hundreds, if not thousands, of safety lamps are placed in explosive gas every day when the mines are being tested for the presence of fire-damp, and yet no parallel case has ever been recorded. Under these circumstances we prefer to attribute it to some other still unknown cause. We have yet to learn whether shots were fired in the mine, and if so we have probably not far to look for the explanation.

Up to the present all we know with certainty is that the mine produced very little gas, that it was dry and dusty, and that the explosion was violent but not universal. It would be most interesting, as well as instructive, to ascertain whether any natural local dampness curtailed its extent; but as this is a feature that has not hitherto attracted or received much attention, we are not sanguine that it will be carefully inquired into in the present case. We shall, however, watch the future course of the inquiry, and perhaps again comment upon it for the benefit of our readers.

W. G.

ON THE DIFFERENTIAL EQUATION TO A CURVE OF ANY ORDER

TO Mr. Samuel Roberts (see Reprint of *Educational Times*, vol. x. p. 47) is due the credit of having been the first to show that a direct method of elimination properly conducted leads to the differential equation for a cubic curve: but he has not attempted to obtain the general formula for a curve of any order. By aid of a very simple idea explained in a paper intended to appear in the *Comptes rendus* of the Institute, I find without calculation the general form of this equation. The eft-hand member of it may be conveniently termed the differential *criterion* to the curve. One single matrix will then serve to express the criteria for all curves whose order does not exceed any prescribed number. For instance, suppose we wish to have the criteria for the orders 1, 2, 3, 4:—

Let $m.\mu$ be used in general to denote the coefficient of

$$h^m \text{ in } \left(\frac{1}{1.2}y''h^2 + \frac{1}{1.2.3}y'''h^3 + \frac{1}{1.2.3.4}y''''h^4 + \ldots\right)^m.$$

Write down the matrix—

2·1	3·1	3·2	4·1	4·2	4·3	5·1	5·2	5·3	5·4
3·1	4·1	4·2	5·1	5·2	5·3	6·1	6·2	6·3	6·4
4·1	5·1	5·2	6·1	6·2	6·3	7·1	7·2	7·3	7·4
5·1	6·1	6·2	7·1	7·2	7·3	8·1	8·2	8·3	8·4
6·1	7·1	7·2	8·1	8·2	8·3	9·1	9·2	9·3	9·4
7·1	8·1	8·2	9·1	9·2	9·3	10·1	10·2	10·3	10·4
8·1	9·1	9·2	10·1	10·2	10·3	11·1	11·2	11·3	11·4
9·1	10·1	10·2	11·1	11·2	11·3	12·1	12·2	12·3	12·4
10·1	11·1	11·2	12·1	12·2	12·3	13·1	13·2	13·3	13·4
11·1	12·1	12·2	13·1	13·2	13·3	14·1	14·2	14·3	14·4

The determinant of the entire matrix, which is of the tenth order, is the criterion for a quartic curve. The determinant of the minor of the sixth order, comprised within the first six lines and columns is the criterion for a cubic. The determinant of the third order, comprised within the first three lines and columns (subject to a remark about to be made) will furnish the criterion for a conic, and the apex of the matrix is the criterion for the straight line. By adding on five more lines and columns, according to an obvious law, the matrix may be extended so as to give the criterion for a quintic; then six more lines and columns a sextic, and so on as far as may be required.

The remark to be made concerning the determinant of the third order referred to is that it contains the irrelevant factor 2·1, *i.e.* $\frac{y''}{2}$, so that the criterion for a conic (Monge's) is this determinant divested of such factor. It is *certain* that the next determinant is indecomposable, and is therefore the criterion for a cubic. There is no reason that I know of to suppose that any other determinant except that one which corresponds to the conic, is decomposable into factors. If this is made out, then, observing that the single term which is the criterion for the right line is indecomposable, we have another example of what may be called, in Babbage's words, a miraculous exception to a general law.

A well-known similar case of such miraculous exception I had occasion many years ago to notice in connection with the criteria for determining the number of real and imaginary roots in an algebraical equation. Such criteria may, with one single exception, be expressed