Iron Bort.

WE have been getting occasional pieces of a curious material from the diamond mines, which may prove to have a relation to the mineral described in NATURE of September 7 (J. R. Sutton, "A New Mineral?"), and also may throw some light ultimately upon the origin of the diamond. To outside appearance, in extreme cases, the material has a cindery look; in less extreme cases its diamond affinities are fairly evident. It can be readily disintegrated with a mineralogical file, but it has hard corners which will scratch corundum. The specific gravity is 3·3 to 3·5, *i.e.* slightly lighter than diamond. It is insoluble in acid, is feebly magnetic, and when suspended by a light thread or floated on water (on a cork) shows distinct polarity under the influence of an ordinary large steel horse-shoe magnet. When it is crushed a small bar magnet will readily take up small specks of it. (The mineral previously described in NATURE, by the way, shows no polarity.)

Some months ago I casually examined some pieces of this material, and concluded that they were diamond (bort) with enclosed impurities. Some of the impurity is now proved to be iron, which shows that the statement sometimes made that diamond is not found in association with iron is not quite correct.

Some pieces of this material which had been extracted by the electromagnets at the pulsator were brought in by Mr. Stewart (the manager of the pulsator) a few days ago. They were very unlike the stuff readily recognisable as diamond, but the chain of gradation from these to something more nearly approaching true bort is fairly complete. Whether a diamond buyer would put the same commercial value upon them as he would upon bort is quite another question. Up to the present time I have not come upon any true bort which shows the same magnetic properties. Like true bort, however, this material is a good conductor of electricity.

As a distinctive name for this variety of bort, or iron bort—if bort it may strictly be called—Stewartite would be suitable. J. R. SUTTON.

Kimberley, September 30.

A Starling's Deception.

THREE weeks ago, or, to be quite correct, on September 22, I was considerably startled and surprised, on going into the garden at 9.30 a.m., at hearing what I thought was a wryneck's call in a tree not many yards off. I listened, and in a few minutes the cry came again clear and distinct as one hears it in the spring and early summer. I was astonished, knowing it to be a rare thing to hear the wryneck after the middle of July. I approached the tree (in which two or three starlings were chattering and whistling) and tried to get a sight of the supposed wryneck, but did not, although the call was repeated several times. I put down my failure to the thickness of the foliage and the ivy-grown trunk, somewhere in the midst of which the bird was doubtless in hiding.

Well, the next morning, and on several days following, the unseasonable, but otherwise very pleasant, note continued to be heard, and always from the same tree and, apparently, in association with the starlings, for I noticed that the cry invariably came after one of the starlings had whistled. The whistle, in fact, seemed to be the signal for the wryneck to sing.

It struck me as being altogether very curious, and I determined to find out, if possible, more about it. So one morning (September 27) I resolved to investigate the matter more closely. Standing under the tree, and after a little patient waiting, I got a starling well into view and watched him carefully. Wagging his head from side to side he chattered and cackled for all he was worth; then came the whistle, and immediately afterwards the wryneck's note, in uttering which I quite distinctly saw the quick movement of the beak. And so the mystery was solved! I waited, hoping to see a repetition of the performance, but the bird, I fancy, caught sight of me and flew away. On two or three of the following days I tried to catch him in the act again, but was not successful. In the early days of October the cry was not heard (at any rate by myself), but it fell on my ear once more, and for the last time, on October 6, and from the same tree.

Starlings are great mimics, I believe, and I am wondering if this particular bird has been reared in the immediate vicinity of a wryneck's nest, and so caught the note from the parent wryneck. However this may be, I thought the incident would interest your readers, and perhaps elicit additional facts of a similar nature from some of them.

I may add that in 1901, from August 19 to September 10, a friend and myself heard almost daily what we firmly believed to be a wryneck's cry. It surprised us, certainly, but, other than being very interested in hearing the unseasonable note, we never investigated the matter properly. The question now arises, were we and the neighbours deceived by a starling in 1901 as I was so nearly deceived by one this autumn? BASIL T. ROWSWELL.

"Les Blanches," St. Martin's, Guernsey,
October 18.

Hot Days in 1911.

MR. MACDOWALL'S dot diagram in NATURE of October 12 certainly shows high correlation between the number of hot days in a quinquennium and the difference between this and the number of hot days in the next quinquennium, and Mr. Corless in NATURE of October 19 finds the value of the correlation coefficient to be −0·725; but the conclusion is not that the number in one quinquennium is correlated with the number in the next.

If x_1 is the departure from mean value of the number of hot days in one five-year period, and x_2 that in the next succeeding, then, if these are wholly independent variables, sum $x_1x_2=0$, the minus values neutralising the plus, and the coefficient of correlation between x_1 and x_2-x_1, which is

$$\text{sum}\,x_1(x_2-x_1)/\sqrt{\text{sum}\,x_1^2}\times\sqrt{\text{sum}\,(x_2-x_1)^2},$$

becomes

$$-\text{sum}\,x_1^2/\sqrt{\text{sum}\,x_1^2}\times\sqrt{\text{sum}\,(x_2^2+x_1^2)},$$

or $-1/\sqrt{2}$, since sum $x_2^2=$ sum x_1^2 in a long series.

The value $-1/\sqrt{2}$, or −0·707, is within the limits −0·725±0·059 given, and the conclusion is that the correlation between successive quinquennia is nil.

This conclusion, based on the figures of Mr. Corless, must render ineffectual Mr. MacDowall's endeavours to make long-range forecasts of weather by correlations at five years' distance, and will disappoint any hopes that the new method may have raised in the minds of "official meteorologists." H. E. SOPER.

University College, London, October 23.

MR. MACDOWALL, in dealing with the number of "hot" days in a year (NATURE, October 12, p. 485), compares two series of numbers which are not independent, and uses the comparison in an attempt to make seasonal forecasts. His method does not appear to be statistically legitimate. He obtains a series of numbers $N+n_3$, $N+n_1$, . . . $N+n_{m-3}$, representing the total number of "hot" days for periods of five years, 1, 2, 3, 4, 5; 2, 3, 4, 5, 6, &c., and plots a diagram showing the relation between $N+n_r$ and $n_{r+5}-n_r$, N being the mean of the five-year totals. If the scales of ordinates and abscissæ were the same, and the series of numbers $N+n_3$, &c., represented a random selection, we should expect to find in the diagram a number of dots distributed more or less symmetrically about a line bisecting externally the angle between the axes. This is what Mr. MacDowall obtains in his diagram on p. 485, allowance being made for his difference of scale. The diagram, as it stands, cannot therefore help the forecaster.

We should expect also to find a large correlation coefficient between $N+n$ and $n_{r+5}-n_r$. For a long series of numbers in which there was *no correlation* between $N+n_r$ and $N+n_{+5}$ the value of the coefficient between $N+n_r$, $n_{r+5}-n_r$ would be $-\frac{1}{2}\sqrt{2}$, or −0·71, say. Mr. Corless finds from Mr. MacDowall's figures a value −0·73. Clearly, therefore, this cannot be taken to prove periodicity.

The total number of "hot" days in the nine years preceding 1911 is, according to Mr. MacDowall, 586, compared with an average of 9×77=693, so that unless there