For this purpose I will here employ a simple meteorological method, which seems to have been little used among us hitherto, viz. algebraic addition, step by step, of a series of plus and minus values; the resulting figures being then plotted as a curve.

Suppose e.g. any set of such values, as follows:

$$+3 + 6 - 2 + 8 + 12 - 3 - 9, \&c.$$

By addition we get this series:

$$+9 + 7 + 15 + 27 + 24 + 15, \&c.$$

This latter series is thrown into a curve.

In his *British Rainfall* for 1881, 1891, &c., Mr. Symons has given a series of rainfall values for a number of stations in

A, Boston; B, Oxford; C, Chilgrove; D, Exeter; E, Kendal; F, Bolton.

different parts of England (from 1830 onwards); also the percentage equivalents of these, the average for each station being taken as 100. These latter I have made use of, taking the excess over 100 as a plus value, and the deficiency under 100 as a minus. Thus e.g. 106 would be +6; 94 would be -6.

When this has been done with the values for Boston, in Lincolnshire, and the series treated by the addition-method described, we get the curve marked A in the diagram; and it is to this curve I would especially invite attention. For it is to the eastern parts of our country that Brückner's cycle applies; the west belongs to what he calls *Ausnahmegebiete*, or exceptional regions.

Bearing in mind that these curves rise for plus values and fall

for minus, we note in this Boston curve a general rise from 1838 to 1849 (or 1852), also from 1874 to 1883; while we have general fall from 1852 to 1874, and from 1883 to 1896 (the last point dealt with). In other words, the two former were *wet* periods, the two latter *dry* periods.

The following figures show this:

	Years.	Dry.	Wet.
1838-52	14	4	10
1874-83	9	none	all
1852-74	22	17	5
1883-96	13	9	4

Between the wave crests 1849 and 1883 are 34 years; and between the wave hollows 1838 and 1874 are 36 years. Or, if we like to take as approximate centres of the wet periods (say) 1843 and 1878, we have about the same interval, 35 years.

These fluctuations, standing alone, would clearly be too slender evidence of a cycle. But Brückner claims that his cycle of 35 years has been in evidence in various parts of the globe through these two centuries at least (since 1700).¹ If the eastern part of England, then, may be expected to conform to the law in future, we might reasonably, perhaps, look for the centre of another wet period somewhere in the second decade of next century. And for the near future (without attempting detail) a continuation for some years of the recent *régime* seems not unlikely, dry years preponderating over wet. That is, the curve should go down further, on the whole, for some years yet.

I have given a number of other curves for comparison, viz. B, Oxford; C, Chilgrove, in Sussex (near Chichester); D, Exeter, less reliable perhaps (see *Brit. Rf.*, 1881); E, Kendal; and F, Bolton, in Lancashire. As we go westwards, the curve seems to degrade somewhat (regarded from our standpoint), though the same type may be discerned. In the northern curves, Kendal and Bolton, we find little in common with the Boston curve, though something like a 35 years interval may, in cases, be made out.

It may be well to state that other stations in the east of England yield very similar curves to that for Boston.

A. B. M.

Prehistoric Egypt.

I NOTICE a review in NATURE containing some statements about my own work which are inaccurate.

The term "New Race" is quite correct, as the race was entirely new to us, whatever their age; and as a tentative name which commits us to no theories, it can hardly be said that I "did not understand the facts of the case" in using it. In dating the race to at least 3000 B.C., I was doing all that the facts warranted at the time; and if we all agree now that they are older, it is by mere consensus of guessing, for no absolute proof of earlier age by juxtaposition with other things has even yet been found.

Further, Dr. Verneau's erroneous assumption that the condition of the bones could be produced by exposure to the air alone, is quoted, with the remark that my "sensational discovery therefore falls to the ground." How exposure to the air can possibly break off the ends of bones and scoop out the cellular structure, while the bone remains hard and firm, not even Dr. Verneau can explain. To deny cannibalism in such a case, reminds one of the indignant repudiation of the intention to smoke by a man who already has a pipe in his mouth and a match in his fingers.

No one values more than I do the discoveries of M. de Morgan; but had he dealt more with strict evidence and full details, and given weight to many facts which he has ignored, I venture to think that his work would have needed less revision in future.

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I HAVE read Mr. Petrie's letter, and I still think that M. de Morgan is right, and that Mr. Petrie is wrong. I also think that Dr. Verneau is right, and that his "assumption" is not "erroneous." Mr. Petrie's reference to the revision which he thinks M. de Morgan's work will need is remarkable; for his own will—in my opinion—need much more! however much M. de Morgan's may need!

THE REVIEWER.

¹ We may here recall that Bacon seems to have been aware of such cycle.