The Silurian district of Girvan in Ayrshire is one that has attracted much attention from geologists, and considerable difference of opinion has existed as to the exact correlation of the several members of the formation as there exhibited with the equivalent English deposits. The fossils, though numerous, are often in a rather unsatisfactory condition as regards preservation, and it was most desirable that a careful study of all the known forms should be made by competent palæontologists. The richly stocked cabinets of Mrs. Robert Gray have furnished the larger part of the specimens described, and the completion of this first part of the work was rendered possible by the liberality of Mr. Gray.

The memoir commences with an account of the bibliography of the subject, which appears to be very full and complete, and then proceeds to the description of the lower forms of life. Any one who will take the trouble to compare the lists given by our authors with those previously published cannot but be struck by the large additions which are now made to the Girvan Silurian fauna. A single doubtful furoid and four species of *Feraminifera* are described as occurring in the Girvan rocks, and among the latter is the remarkable *Saccamina carteri*, which is so excessively abundant in some of the Carboniferous limestones. This form has been recognised as identical with the Carboniferous type by Mr. H. B. Brady himself, and its existence in Silurian strata adds another example—one of great interest to geologists—of the wide range in time of some of the lower forms of life.

Among the corals from the Girvan area Messrs. Nicholson and Etheridge enumerate no less than twenty-two forms, some being old and well-known species, but the majority are new to science; indeed several new genera of Actinozoa are established in the present work. The specimens are usually in a bad state of preservation, a difficulty which has been to some extent overcome by the authors by the employment of thin sections. The fact which comes out most strikingly from the study of the Cœlenterate fauna of the Girvan beds is that the nearest analogues of the Silurian fossils of Scotland are to be found not in the English area but in the American. The same fact, it will be remembered, was made very strikingly manifest from Mr. Salter's studies of the fauna of the Silurian limestone of Durness in Sutherland.

Of Trilobites twenty-eight species are now described as occurring in the Girvan district, and among them several forms new to science have been detected.

As the present volume only contains the first part of the results of our author's labours we do not find a full discussion of the bearing of the palæontological evidence on the interesting question of the age of the several Girvan deposits. There can be no doubt, however, that both the Upper and Lower Silurian are there represented, though the exact correlation of the different members of the series can only be successfully attempted when the fossils have been more fully worked out.

The present fasciculus is illustrated by nine very well executed lithographic plates from the pencil of Mr. Charles Berjean. We congratulate the authors on the able manner in which they have executed this first portion of their task, and hope soon to have to record the appearance of other portions of this important monograph.

OUR BOOK SHELF

Natural History Rambles. The Sea-Shore. By Prof. P. M. Duncan, F.R.S. *Lane and Field.* By the Rev. J. G. Wood. *Underground.* By J. E. Taylor, F.L.S. *The Woodlands.* By M. C. Cooke, LL.D. (London: S.P.C.K., 1879).

THESE four handy little volumes are well put together, and seem to us decidedly superior to works of a similar kind with which we used to be familiar in our youth. The evident purpose of the volumes is not to teach their subjects systematically, but to lead those into whose hands they may fall to take an interest in the common objects of nature which may be met with in an occasional walk. For this purpose they seem to us well adapted, and the information they convey on the whole trustworthy. They abound in suitable and well-executed illustrations, and might appropriately be put into the hands of any one, old and young, whose circumstances would give him a chance of using them.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Brorsen's Comet

LAST night, May 3, I observed Brorsen's comet pass nearly centrally over the star DM, + 61°, No. 873. In the principal focus of the telescope were two straight bars, 43" (seconds of arc) wide intersecting in the centre of the field. The bars are at right angles to one another, and were inclined 45° to the hour circle. With this arrangement it was easy, by moving the telescope gently about the polar axis (which is well adjusted), to determine the conjunctions in R.A. and in declination, while angles of position coinciding with the bars, and distances in parts of the breadth of a bar, could be estimated with considerable accuracy. In this manner I made the following observations of the position of the comet with reference to the star:—

Chronometer time.	Angle of position.	Dist.	
h. m. s.	°	"	
10 2 59 ...	260 ...	33	{ Distance measured by beats of chronometer.
10 4 31 ...	270 ...	12	
10 6 20 ...	— ...	—	Star apparently central in comet.
10 7 30 ...	320 ...	10	Star a little right of centre.
10 11 30 ...	0 ...	—	Estimated conjunction in R.A.
10 16 0 ...	45 ...	16	
10 18 15 ...	45 ...	43	
10 20 30 ...	45 ...	65	

Projecting these observations on a chart of ruled squares, it appears that at 10h. 11m. 36s. (corresponding to 10h. 11m. 14s. G.M.T.) the comet followed the star 0°68s. in R.A. and was 12" N. of it, while the nearest approach of the centre of the comet to the star was 7" at about 10h. 7m.

The moon was shining with great brilliancy (being nearly full) and made the comet faint, reducing its apparent diameter to 1½ or 2 minutes. The star is given in the DM as of the 8·8 magnitude, but I think is underrated a little. While the comet was passing over it there was no sensible diminution of its lustre. The DM position of the star for 1855·0 is—

$$\alpha = 6h. 7m. 25s., \delta = + 61^{\circ} 28' 9''.$$

The light of the comet has diminished rapidly since April 4. It is now less bright than a 9th mag. star.

Blackheath

G. L. TUPMAN

It is to be hoped that while the comet remains with us the observations of Prof. Yeung (NATURE, vol. xix. p. 559), and of Mr. Christie (NATURE, vol. xx. p. 5) may be repeated and confirmed by those who possess telescopes of sufficient power.

May I trespass upon your space so far as to ask Mr. Christie to explain a little more clearly what the precise carbon bands are with which the bands in the comet-spectrum are coincident. He speaks of the less-refrangible edge of the brightest comet-band coinciding with the corresponding edge of the green carbon-band at 5200—in the spectrum of an alcohol vacuum-tube—and then proceeds to remark that the bands in the spectrum of alcohol are identical with those in the spectrum of olefiant gas and of carbon-dioxide and carbon-monoxide. If we are to understand that all the gases are inclosed in vacuum-tubes, then it must be remembered that the spectrum they give is the second spectrum of carbon in which the brightest bands have the wave-lengths 5160·5, 5198·4, 4834, and 4505. But it appears from Prof. Young's comparison of the comet spectrum with the blue flame of a Bunsen burner, that the brighter band agrees with that of the first spectrum of carbon whose wave-length is 5165·5 within about the interval of the δ -lines

$$(\delta_1 - \delta_4 = 5183\cdot0 - 5166\cdot7 = 16\cdot3).$$

The spectrum with which Huggins compared the spectrum of Comet II, 1868, and of Coggia's comet in 1874, was obtained by taking the spark in olefiant gas at the ordinary pressure, and is therefore again the first spectrum of carbon.

The following comparison will exhibit the uncertainty which I wish to have explained:—

Position of least refrangible Edges of Bands.				
Brorsen's comet	5600	...	5200	...
Carbon spectrum I.	5634·7	...	5165·3	...
Carbon spectrum II.	5610·5	...	5198·4	...
			4834	

May I suggest the following comparison: to bring up the occulting bar from the blue end of the spectrum till it just covers the brightest comet band, then to introduce into the telescope first the light from a Bunsen burner or blowpipe flame, and then that from a vacuum tube inclosing carbonic oxide.

If the comet spectrum is that of Carbon I., light will be seen in the second case, but none in the first; if it be that of Carbon II., no light will be seen in either case.

Giggleswick, May 5 WILLIAM MARSHALL WATTS

I CAN fully confirm Young's observation that the spectrum of Brorsen's comet is not now the same as that observed by Huggins in 1868, as figured in Roscoe's "Spectrum Analysis," p. 251. On the 28th and 30th I observed the spectrum with a Browning's "miniature spectroscope" on a $\frac{1}{4}$ -inch refractor, and compared it with the carbon spectrum of a low gas flame, and found the three usual bands of the latter to coincide with the three bands of the comet as completely as my instruments would show. It was needful to use a wide slit.

Sunderland, May 6

T. W. BACKHOUSE

Temperature Equilibrium in the Universe in Relation to the Kinetic Theory

I AM inclined to think I shall best answer Mr. W. Muir's letter by not disputing the vague charges of unsoundness he has brought against me, but in endeavouring to make more clear the position for which I contend.

The object of my paper (NATURE, vol. xix. p. 460) was to contest the necessity of supposing that existing physical principles must have been violated in past time. I sought to prove that there was no reality in this necessity, by showing that even from our present imperfect knowledge, an explanation for the existing state of things might be evolved consistent with principles which at present prevail. Perhaps I may do well to add a few words in order to elucidate a point which was not too clearly expressed.

For mere sake of illustration, let us imagine a spherical envelope which permits neither change of volume nor passage of heat, to inclose a space of diameter, say 10^{10} times the distance between the sun and Sirius. First, let all the matter within this space be at the zero of temperature. Second, let all the matter within our envelope be at such a temperature that it is entirely dissociated into discrete molecules. Between these two extremes there is room for any number of mean states in which matter might be more or less aggregated, or discrete, and my point was that the universe might actually be in one of these intermediate states. It should be scarcely necessary to observe that we have limited our space merely for the sake of fixing our ideas. All that we require is gained, if, instead of using the impermeable envelope, we surround our sphere with infinite space filled with matter in a similar condition to that which the sphere inclosed. It is

important to note that the volumes must be taken large enough to form a fair sample of the general state of the universe. Prof. Clerk Maxwell has shown ("Theory of Heat," p. 328) that a demon existing inside a gas might find irregularity where to us giants all appears uniform. With respect to the universe, may we not be in the position of demons?

London, April 29

S. TOLVER PRESTON

Barometric Pressure and Sun-Spots

IN his letter to NATURE, vol. xviii. p. 567, on "Sun-Spots and Weather," Mr. Fred. Chambers has shown that the curve of mean barometric pressure at Bombay throughout the year varies with the inverted sun-spot curve. Taking this fact together with the commonly-received idea that the annual variation of barometric pressure in Central Asia is due to the corresponding annual variation of solar radiation, he thence concludes that "the sun is hottest about the time that the spots are at a maximum, and coldest about the time when they are at a minimum." Now, even if the validity of the logical process by which "secular" is substituted for "annual" in this argument be admitted to hold in a general way, have we any reason to suppose that the atmospheric conditions at Bombay, a marine station on a peninsula, can be adequately taken to represent those which prevail in the centre of the Asiatic continent, or that they approximate to the latter to any greater extent, or even as much as those at St. Petersburg, for example?

On the other hand, if conditions which presumably reach their maximum intensity in the centre of the continent are so distinctly marked at such a distance from it as Bombay, they should at least be visible to some extent at St. Petersburg, which is certainly more continental in position, if not actually nearer the centre of the continent than the former city.

Such being the case, I should be glad to know how Mr. Chambers would account for the following remarkable fact, viz., that the mean annual barometric pressures at St. Petersburg from 1822 to 1871 show a well-defined relation to the sun-spots precisely the reverse of that evinced by the figures for Bombay.

I might, if I had followed Mr. Chambers's example, have concluded, with as good grounds for my opinion, that "the sun is coldest when most spotted," and *vice versa*, but I prefer to wait until more extensive investigations have given us a sounder basis for induction than at present exists. Meanwhile, I place before your readers the figures on which my statement regarding the St. Petersburg pressures is based. The employment of a variety of methods of comparison has invariably given the same results.

In the following table the variations from the mean, expressed in millimetres, are compared with the sun-spots according to the plan recommended by Mr. Meldrum, and which for some purposes is superior to those generally adopted hitherto. For brevity's sake only the final columns are given:—

Mean Cycles			
Max. years in 5th line.		Min. years in 7th line.	
Pressure variation in mm. (1822-71).	Sun-spots (1811-77).	Pressure variation in mm. (1822-71).	Sun-spots (1816-72).
1. -0·52 ...	-33·9 ...	+0·15 ...	+23·3
2. -0·35 ...	-23·4 ...	+0·59 ...	+14·5
3. -0·48 ...	0·0 ...	+0·78 ...	+4·8
4. -0·24 ...	+28·2 ...	+0·63 ...	-5·6
5. +0·44 ...	+43·1 ...	+0·29 ...	-19·0
6. +0·70 ...	+34·2 ...	-0·31 ...	-32·5
7. +0·80 ...	+16·8 ...	-0·83 ...	-37·1
8. +0·62 ...	+0·2 ...	-0·71 ...	-25·4
9. +0·34 ...	-14·2 ...	-0·40 ...	+1·8
10. -0·07 ...	-24·2 ...	-0·14 ...	+30·9
11. -0·88 ...	-26·3 ...	+0·23 ...	+44·8

It will be noticed that the pressure epochs lag behind the sun-spot epochs in the same way as the air-temperature epochs determined by Dr. Köppen.

The figures for the pressure are taken from the Annals of the Central Observatory. The above relation was first brought to my notice by my friend, Mr. S. A. Hill, of Allahabad.

Mr. Chambers's notion that "if the winter rainfall of Northern India is really due to the cold of winter, we should expect it to be greatest when the sun is coldest" is partly