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 5610·5, 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 *b*-lines

$$(b_1 - b_4 = 5183{\cdot}0 - 5166{\cdot}7 = 16{\cdot}3).$$

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 ...	4739·8
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 4¼-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. T. W. BACKHOUSE

Sunderland, May 6

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