the valley of Sto. Amaro. The path ran right up to the edge and seemed to come to an end but for a kind of irregular crack full of loose stones which went zigzagging down to the bottom at an angle of about 70°, and we could see the path down below winding away in the distance towards the main road to Sto. Amaro. We looked over this cliff and told Mr. Wilson firmly that we would *not* go down the side of that wall on horseback. He laughed, and said that the horses would take us down well enough and that he had seen it done, but that it was perhaps a little too much; so we all dismounted, and put the horses' bridles round the backs of the saddles and led them to the top of the crack and whipped them up as they do performing horses in a circus. They looked over with a little apparent uneasiness, but I suspect they had made that precarious descent before, and they soon began to pick their way cautiously down one after the other, and in a few minutes we saw them waiting for us quietly at the bottom. We then scrambled down as best we might, and it was not till we had reached the bottom, using freely all the natural advantages which the *Primates* have over the *Solidunguli* under such circumstances, that we fully appreciated the feat which our horses had performed.

"The next part of the road was a trial; the horses were often up nearly to the girths in stiff clay, but we got through it somehow, and reached Sto. Amaro in time to catch the regular steamer to Bahia."

And here is an uncommonly good anecdote about a parrot:—

"At Sto. Amaro a line of tramways had lately been laid down also under the auspices of our enterprising friend, and we went down to the steamboat wharfs on one of the trucks on a kind of trial trip. The waggon went smoothly and well, but when a new system is started there is always a risk of accidents. As the truck ran quickly down the incline the swarthy young barbarians, attracted by the novelty, crowded round it, and suddenly the agonised cries of a child, followed by low moanings, rang out from under the wheels, and a jerk of the drag pulled the car up and nearly threw us out of our seats. We jumped out and looked nervously under the wheels to see what had happened, but there was no child there. The young barbarians looked at us vaguely and curiously, but not as if anything tragical had occurred, and we were just getting into the car again, feeling a little bewildered, when a great green parrot in a cage close beside us went through no doubt another of his best performances in the shape of a loud mocking laugh. A wave of relief passed over the party, but we were rather late, and the drivers expressed to the parrot their sense of his conduct, I fear strongly, but in terms which, being in Brazilian *patois*, I did not understand."

In another notice we will tell of the *Challenger's* doings between Bahia and Cape Town, and from the Falklands home, and we will also more particularly allude to the general results of the scientific work she has so successfully accomplished.

(*To be continued.*)

ON THE PRESENCE OF OXYGEN IN THE SUN

I HAVE spent the greater part of last winter and the beginning of this in an investigation of the spectra of oxygen. My experiments will be published, I hope, in another place; but there are one or two points of more immediate interest, and, I venture to think, of some importance, which I trust you will allow me to discuss in your columns.

Prof. Draper has lately announced the important discovery that the lines of oxygen are found to be present in the sun. These lines, however, are bright, and not dark, as the Fraunhofer lines. I had found that at a certain temperature, lower than that at which oxygen shows its well-known lines, it gives another spectrum, and it occurred to me, when I heard of Prof. Draper's discovery, that if the temperature of the sun, at some point intermediate between the photosphere and the reversing layer was the same as that at which the spectrum of oxygen changes, the fact that the *known* spectrum of oxygen appears bright would be fully explained. The spectrum of lower temperature, which, for reasons to be given, I shall call the compound line spectrum of oxygen, ought in that case to be found reversed in the solar spectrum, like the remainder of the Fraunhofer lines.

I have consequently devoted all my time during three weeks to the exact measurement of these four lines, and I do not think that the evidence which I am about to give will be considered to fall far short of an absolute proof that the spectrum is really reversed in the sun.

Two difficulties have put themselves into the way of exact measurement. The first is due to the extreme weakness of the spectrum. The light itself is not stronger than that of a non-luminous Bunsen burner; and after that light has passed through four prisms, as in most of my experiments, or through seven, as in some of them, there is not much of a spectrum left to be measured. It is only after having been in the dark for half-an-hour that the eye is able to do the work, and there are a good many days when the eye never obtains sufficient sensitiveness to make any trustworthy measurements. But whenever my eyes were in sufficiently good condition, my measurements agreed so well, that I have no hesitation in saying that they are as accurate as the measurements of the solar lines which will be found by their side. The second and more serious difficulty is due to the fact that the lines in question widen to a great extent with increased pressure and in such a way that the brightest part, and still more, the centre of the band, is displaced towards the red. I have not been able to get the lines perfectly sharp, and the measurement of the centre of the band will give, therefore, too high a value of the wave-length. The following table contains the numbers which I have obtained:—

Oxygen.	Width.	Solar Lines.	
		A.	S.
α 6156·86	±0·3	6156·70	6156·69
β 5435·55	±0·3	5435·44	5435·56
γ 5329·41	±0·6	5329·3	5329·10
δ 4367·62		4367·58	

The first column contains the wave-length of the compound line spectrum of oxygen. The second column contains the number which has to be added or subtracted from the wave-length, in order to get the edge of the lines, as it is their centres which are given in the first column. The third and fourth columns give the wave-lengths of the corresponding solar lines as observed by Angström (A.) or myself (S.). The greatest difference is found in the line γ, but even this difference only amounts to the twentieth part of the distance between the sodium lines, and it would require a spectroscope of very good dispersive power and definition to separate two lines which would be that distance apart from each other. Nevertheless the amount in question is greater than the possible errors of observation, and I believe the difference to be due to the fact mentioned above, that the lines widen unequally. It will be seen from the table that the solar line would fall within the oxygen line, but about one-third of the distance between its most refrangible and least refrangible edge. At a higher pressure the brightest part of the band lies about 5331. None of the other lines widen nearly as much, and δ is always perfectly sharp. Angström gives it as an iron line, but according to Kirchhoff, the solar line is composed of two lines, and separated by a distance of about 0·1.

The average distance between the solar lines in the green, which have not yet been identified, is about 4·4, or more than fourteen times the difference between the centre of the oxygen line and the corresponding solar line. The average distance between the non-identified lines near O a is 4·9, or about twenty-nine times the corresponding difference. In judging, however, of the value of the evidence, I should like the reader to leave the line δ out of account. Although the agreement seems perfect, I have not the same confidence in the correctness of the wave-length as I have with the other lines. The line β is weaker than the others, and the error of observation may be a little larger than with a and γ, which will, I think, be found correct to the decimal place.

Let me point out in a few words the importance of the results obtained. The compound line spectrum of oxygen can only exist under a limited range of physical conditions. It is broken up at a higher temperature into the elementary line spectrum, and at a lower temperature it tumbles together into a continuous spectrum. During its existence its lines may be subject to variations owing to pressure. The spectrum of oxygen is therefore pre-eminently fitted to be at once the pressure gauge and thermometer of the sun. We cannot at the present moment give the exact temperature of the points at which the changes take place; but we can say with certainty why it is that the line spectra of many metalloids are not found reversed in the sun, for the temperature which gives these line spectra is higher than that which gives the compound line spectrum of oxygen, and therefore higher than that of the reversing layer of the sun. Consequently we must look for their band spectra and not for their line spectra. The same may be true for the spectra of some of the heavier elements like gold, silver, and platinum, which have not yet been discovered in the sun. The continuous spectrum of the base of the corona is most likely the continuous spectrum of the cooler oxygen.

As the science of spectroscopy advances we shall be able to determine the physical conditions which exist on the surface of the sun with as great a degree of certainty and a much smaller degree of discomfort than if we were placed there ourselves. I hope that this communication will prove to be a step in that direction. All my experiments were made in the Cavendish Laboratory.

ARTHUR SCHUSTER

St. John's College, Cambridge, November 30

OUR ASTRONOMICAL COLUMN

JUPITER'S SATELLITES.—Amongst the recorded phenomena connected with the motions of the satellites of Jupiter are several notices of observed occultations of one satellite by another, and of small stars by one or other of the satellites. The following cases may be mentioned:—On the night of November 1, 1693, Christoph Arnold, of Sommerfeld, near Leipsic, observed an occultation of the second satellite by the third at 10h. 47m. apparent time. On October 30, 1822, Luthmer, of Hanover, witnessed an occultation of the fourth satellite by the third at 6h. 55m. mean time.

Flaugergues, writing to Baron de Zach, from Viviers, on November 18, 1821, says: "I begin with an observation, very useless, no doubt, but extremely rare, for I have not found a similar one in the collections of astronomical observations which I have examined; *i.e.*, the occultation of a very small star by the third satellite of Jupiter." He proceeds to mention that on August 14, 1821, he repaired to his observatory very early to observe an eclipse of this satellite, and having looked at Jupiter with the telescope, he remarked a very small star near the third satellite. The satellite approached this star, and at 1h. 47m. sidereal time, it appeared to touch it, and at 1h. 56m. 52s. it was not possible to distinguish the star—it had disappeared. The satellite became fainter and disappeared in its turn at 1h. 59m. 10s. sidereal time, on August 13, or 16h. 30m. 8·5s. mean time at Viviers. The sky was perfectly clear, and Flaugergues considered his observations very exact. He adds that he continued to observe for a long time after the immersion of the satellite, hoping to see the star reappear, but he could not again distinguish it; the twilight had much increased, and small stars in the neighbourhood of Jupiter were soon effaced.

There is a similar observation by Mr. G. W. Hough, at Cincinnati Observatory, communicated in a letter to Dr. Brunnow, when Director of the Observatory at Ann Arbor, Michigan, and published in his "Astronomical Notices" Mr. Hough states that on March 28, 1860, he witnessed the end of an expected occultation of a star 9·5 mag., by Jupiter, and the occultation of the same star by the first satellite. When first seen it was distant from the limb of the planet about one diameter of the satellite, or one second of arc, so that the real separation had taken place about six minutes before (or about 8h. 9m. sidereal time), though he was not able to see it. At 10h. 27m. sidereal time the star was occulted by the first satellite and remained invisible eight minutes. Mr. Hough further says that the star is found in the "Redhill Catalogue," an obvious oversight; it would appear to be No. 1630 of Zone + 22° in the *Durchmusterung*, a star of 9·3m. the approximate place of which for 1855 was in R.A. 7h. 8m. 5s., N.P.D. 67° 3'·3.

DONATI'S COMET OF 1858.—This comet which attained so great a celebrity in the autumn of 1858, makes a very close approximation to the orbit of Venus near the descending node, and it may be reasonably inferred that the actual form of its path round the sun may be due to a very near approach of the two bodies at some distant epoch. The discussion of the totality of observations was undertaken some years since by Dr. von Asten, who has published his results in a dissertation entitled "Determinatio orbitæ grandis cometæ anni 1858, e cunctis observationibus." The comet was discovered by Donati on June 2, and was observed until the beginning of March, 1859, at the Cape of Good Hope and at Santiago de Chile; consequently the observations extended over a very wide arc of the orbit, and there have been very few cases where careful discussion could be expected to lead to more reliable results. The period of revolution deduced by Dr. von Asten is 1,880 years, and there is a high probability that this does not differ materially from the true one, applying to the time of the comet's appearance. Prof. Hill, of Washington, also by a complete investigation, obtained a somewhat longer period, but the general character of the orbit remains the same. Employing Dr. von Asten's elements, it will be found that in heliocentric longitude 343°·7, the distance of the comet from the orbit of Venus, is only 0·0047 of the mean distance of the earth from the sun. In 1858 the two bodies came into pretty near proximity, their mutual distance on October 17 being 0·088. It has been mentioned above that the point of closest approach of the orbits of the planet and comet is situated near the descending node; the opposite node falls in the region of the minor planets.

THE OBSERVATORY OF LYONS.—The *Bulletin Hebdomadaire* of the French Scientific Association reports that M. André is actively employed in the establishment of this new astronomical institution and is energetically supported by the Government. M. Raphël Bischoffsheim, the munificent donor of the meridian circle, lately mounted at the Observatory of Paris, has also intimated his intention to present the Lyons Observatory with its fundamental instrument, a meridian-circle of dimensions but slightly inferior to those of the circle, for which the Observatory at Paris is indebted to him. It will also be constructed by Eichens. The Paris meridian-circle is intended to replace the instruments of Gambey, which are now placed in one of the saloons of the institution with other instruments which have seen their day. M. Wolf