of highly trained and experienced geologists than to that of gentlemen, who, whatever their attainments may be, certainly do not in this Report evince much acquaintance with geology. J. G.

Reports of the Mining Surveyors and Registrars for Quarter ending March 31, 1872. Victoria.

THERE is nothing in these Reports calling for special notice. The total quantity of gold got respectively from alluvia (or, as the Reports have it, *alluviums*) and quartz reefs during the quarter were as follows:—Alluvial, 171,851 oz. 10 dwt.; quartz, 164,670 oz. 8 dwt.; total, 336,521 oz. 18 dwt. The quantity of gold, the produce of the colony, exported during the same period was 398,131 oz. 10 dwt.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Aurora Spectrum

IN connection with my letter in last number of NATURE, I have in a diagram approximately placed the aurora lines side by side with the spectrum of hydrogen and of some of the principal air lines (as given in Dr. Watt's index) and with the following results:—

Line No. 1. Close upon, if not identical with, an air-line marked by Huggins N O, and Plücher O.

No. 2.—Not apparently coincident with any prominent air-line. The coincidence with a line of oxygen noted by so careful an observer as Mr. Proctor is puzzling; and if the instrumental power used was sufficient to ensure absolute identity, seems to indicate a second or unusual spectrum of that gas.

No. 3 is not near any principal air-line.

No. 4 is nearly coincident with a faint line of oxygen (confirmatory of Mr. Proctor's observations).

No. 5 corresponds to a rather strong N line.

No. 6 does not coincide with any principal air-line, very faint lines of O and N being the nearest.

No. 7. Upon close examination the positions of this line as respectively fixed by Mr. Proctor and Lord Lindsay are not inconsistent, and the line closely corresponds with a strong line of oxygen situate on the less refrangible side of solar G.

Nos. 1, 4, 6, and 7 fairly correspond in intensity with their representative air-lines. None of the lines are identified with H α, H β, or H γ, and it would appear that the aurora, if a spectra of atmospheric gases, mainly selects oxygen and ignores H α and the stronger N lines. The modification of compound spectra by conditions of temperature and pressure, is however only a partially explored subject, and we have moreover no certain data of conditions in the case of the aurora, which will assist us in bringing it to bear.

I accidentally omitted from the names of some observers of the zodiacal light that of Prof. Piazzi Smyth, whose observations in the south may be said to have conclusively demolished the supposed identity of the light, and the aurora (at least so far as bright lines are concerned) made it extremely improbable that anything beyond a continuous spectrum will ever be seen in the pure zodiacal light, though a further search should be by no means neglected. J. RAND CAPRON

Guildford, Jan. 10, 1873

Polarisation of the Zodiacal Light and of the Aurora

IN the interesting article by Mr. Rand Capron in the last number of NATURE—after collating the various results of the spectroscopic examinations of the aurora and zodiacal light which have appeared at different times in your pages, together with those which have been collected by Dr. Schellen—he terminates his analysis of the general results by remarking that he is "not aware whether the zodiacal light and the aurora have been examined with the polariscope," and suggests that the "light, though faint, might be tested with a Nicol's prism and Savart's bands."

I would refer him to a paper in the March number of the "Monthly Notices of the Astronomical Society" for 1871, in which an observation by Mr. Burton (late assistant to the Earl of Rosse) on the polarisation of the zodiacal light is described.

Mr. Burton was one of the eclipse party stationed at Agosta, in Sicily. He made use of a Savart's polariscope, set so as to give a black centre where the bands were parallel to the plane of polarisation. On looking to the brightest parts of the zodiacal light Mr. Burton believed that he could detect faint traces of polarisation, sufficiently strong to enable him just to recognise that the bands were black centred when their direction coincided with the axis of the cone of light, that is, when the direction passed through the position of the sun.

To make sure that he was not examining the remains of air polarisation given by the slight remaining twilight, he examined the light of other parts of the heavens, but was unable anywhere else to detect any trace of bands. In contradistinction, however, to this must be set an observation of my own, yielding a negative result, though made on the same evening and with a similar instrument, as well as with the same Savart used by Mr. Burton. I was, however, unable to detect any trace of bands either upon the cones of zodiacal brightness or upon the adjacent parts of the sky; but it is very possible that Mr. Burton's eye may be more sensitive to faint lights than my own.

In February last I also met with a negative result in examining a faint trace of the zodiacal light visible in England. I then used a double-image prism as well as a Savart, thinking that its two oppositely polarised fields in juxtaposition might afford a more delicate test for so faint an object.

Capt. Tupman while cruising in the Mediterranean has also, I believe, repeatedly obtained negative results when making use of a Savart on the zodiacal light.

And I understand that Mr. Lockyer, together with the other observers of the Indian Eclipse of December 1871, totally failed to detect any traces of polarisation in the brilliant displays of the zodiacal light which they observed while crossing the Indian Ocean.

I am therefore disposed to conclude that any traces of polarisation must be very slight, if indeed any polarisation at all is to be attributed to the zodiacal light itself and not to the veil of atmospheric impurities lying between us and objects near to the horizon. Certainly we may conclude that there is no such polarisation as is found in the light of the solar corona or—as we might expect—if the zodiacal light were caused by a great cloud of cosmical dust made up of particles smaller in diameter than the wave-length.

Indeed there cannot be as great a percentage of polarisation, or, to speak more exactly, as great a difference between the component radial to the sun's place and the component at right angles, as in the case of a sunbeam dispersed by the dust in our own atmosphere. For if any one will examine the track of a sunbeam passing through a room with a Savart, he will not fail to be struck with the distinctness of the bands. We seem therefore justified in concluding, that if the zodiacal light is composed of cosmical dust, such dust particles must be considerably coarser than those which float in our own atmosphere.

As to the polarisation of the light of the aurora, I examined, both with a double-image prism and Savart, a faint auroral display on November 10, 1871; as also the light of the great aurora of Sunday, Feb. 4, 1872, but in neither instance was able to detect any traces of polarisation.

A. COWPER RANYARD

The Diathermacy of Flame

THERE are some statements in Capt. Ericsson's reply to my letter (NATURE, vol. vii. p. 149) which demand discussion. In the first place he calculates the supply of gas in *his* pipe and applies it to *my* burners. As his pipe did not supply my flames, but his own, which were at least fifteen times larger than mine, the applicability of his figures is rather obscure.

Capt. Ericsson says, "The apparatus contrived by Mr. Williams for determining the diathermancy of flame, as described by himself, is exceedingly faulty, *the temperature it records being that produced by heat derived from several sources. The radiant heat transmitted to the bulb of the thermometer by the flames, acting conjointly with the unknown degree of heat imparted by the surrounding medium*, it will be evident that Mr. Williams' device is worthless as an indicator of radiant intensity." Does Capt. Ericsson really mean that the maximum temperature indicated by a thermometer exposed to several varying sources of heat is not determined by the maximum radiators or convections