Sun-spots and Solar Temperature.

GENTLEMEN,—

There are two points in Prof. Whittaker's letter, published in the January number of the *Observatory*, which I think may be discussed further with profit, if you will allow me space in your columns.

First, with regard to the interpretation of the electric furnace experiments of Dr. King, and other experiments on the radiation of gases enclosed in tubes and subjected to external heating. Prof. Whittaker advances the hypothesis that the gaseous molecules emit their characteristic line-spectra chiefly by reason of the radiation coming from the heated walls of the tube, and not by the action of collisions due to the high velocities acquired by contact with the tube.

This may or may not be true; but certain experimental evidence seems to favour the opposite view. For instance, sodium or iodine may be heated in transparent tubes with a non-luminous flame, and yet be made to emit their spectra with the same facility as when heated in an opaque and strongly radiating tube. Again, the intensity of the D radiation of sodium vapour heated in an opaque tube, and excluding all chemical action, is found by my experiments* to be the same as the intensity of radiations of the same wave-length from the glowing tube, and to fade with the tube when the heat-supply is cut off. The intensity in this case is practically that of a black body at the same temperature, when the sodium vapour is sufficiently dense to completely absorb transmitted light.

All this seems to me to show that there is no essential difference, as Prof. Whittaker appears to hold, between solids and gases, in their mode of transforming energy. Both are able, according to my view, to convert energy of motion into energy of ether vibrations. It may be difficult to conceive how the energy of motion in the molecules is converted by collisions into the energy of electric surgings in the atoms; yet this miracle is without question performed by the molecules in a solid body, when heated by collision. Why not, therefore, by the free molecules of a gas? Also, the reverse action takes place: namely, by the absorption of radiation a solid becomes hot. It is not, therefore, unreasonable to suppose that the gases in heated tubes acquire high velocities by both conduction and absorption of radiation, and that they emit light in consequence of their mutual collisions, and not, as Prof. Whittaker believes, by the direct action of absorbed radiation.

That modes of exciting the line-spectrum radiations more potent than thermal exist cannot be denied, although it has not, to my knowledge, ever been demonstrated that electric or chemical "luminescence" is really a non-thermal effect. It is possible that a proportion of the molecules of a luminescent gas may be in

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violent motion, sufficient to cause radiation, although the mean motion, or temperature, is small. However this may be, the fact that the gases concerned in auroral phenomena are cool in the ordinary sense does not prove that discontinuous radiation is not a function of temperature under certain conditions.

Now as to the second point, namely, the estimate of pressure in the Sun. From a consideration of the depth of the chromosphere, and the force of gravity on the Sun, Prof. Whittaker is led to conclude "that the pressure in the lower parts of the chromosphere must be vastly greater than the pressure of the terrestrial atmosphere at the Earth's surface." I consider this conclusion unwarranted. Consider first the partial pressure exerted by sodium vapour in the Sun. If we pass sunlight through the flame of a spirit-lamp, with salted wick, a great increase in the strength of the solar D lines is observed; that is, the amount of absorption exerted by the sodium in the flame is comparable with that in the Sun. It is not necessary, therefore, to suppose that there is more sodium vapour over every square inch, say, of the Sun's surface, than the infinitesimal quantity in the flame of the lamp. No doubt it is spread out to a total thickness of 500 miles; but what would be the pressure exerted by this minute quantity? Similar reasoning may be applied to each of the other elements; and the sum of all their pressures might conceivably be less than one atmosphere, notwithstanding the tremendous force of gravity on the Sun.

As to the actual evidence of pressure in the chromosphere, the centres of the iron lines at λλ 4222·38, 4227·61, 4233·77, 4236·11, move towards the red under pressure, approximately 0·01 Angstrom units per atmosphere *, but their relative wave-lengths in the Sun, compared with other iron lines much less affected by pressure, agree within 0·001 A. with the relative wave-lengths in the spectrum of terrestrial iron under normal pressure †. Therefore we must either assume that the pressure in the reversing layer is of the order of one atmosphere, or that other hypothetical influences affecting the dielectric constants of the iron vapour in the Sun always exactly counteract the effects of pressure; for the above lines exhibit no instability of position in the solar spectrum.

Similar reasoning may be applied to sun-spots. These "pressure" lines, according to my measures, exhibit no trace of instability, such as we should expect were there great variations of pressure in sun-spots. Their wave-lengths, as a matter of fact, do not differ from those in the spectrum of the surrounding region by an amount exceeding 0·002 A., which is about the limit of accuracy of the measures.

I may mention parenthetically that these same lines, when

* A mean value derived from Humphreys' and Duffield's measures.

† Compare Kayser's 'Standard Wave-lengths of Iron Arc Lines' and Rowland's 'Preliminary Table of Solar Spectrum Wave-lengths.'

measured at the Sun's limb, do show a minute pressure-effect, all of them being shifted in relation to the neighbouring lines about 0·004 A. to the violet, according to our researches. It would be premature, perhaps, to lay too much stress on this apparent shift, until further work has been done; but it certainly looks as if the effective region of absorption for these particular lines was at a higher level when the light comes from the limb, the difference of pressure amounting to a fraction of an atmosphere.

The absence of any large difference of pressure in the reversing layer over spots, compared with the same layer over the surrounding region, is also shown by the relative intensities in the line of a spot spectrum. Humphreys pointed out that under pressure the relative intensities of the lines of an element change, and that these altered intensities show no sort of correspondence with those of sun-spots.

No doubt it is difficult to conceive of a solar atmosphere in which no pressure-changes occur; but are we dealing with a true atmosphere outside the limits of the photosphere?. The difficulty, at any rate, largely disappears if we suppose the solar gases to be in a state of extreme tenuity, as seems on the whole the most probable supposition.

Yours truly,

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