LETTERS TO THE EDITOR.

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Variation of Atmospheric Absorption.

REFERRING to a communication from me in *NATURE* of November 5, 1903 (vol. lxi. p. 5), to the effect that a definitely less amount of heat had been received at the earth's surface in the last years than in preceding ones, I would ask attention to an article published in June in the *Astrophysical Journal*, in which I have further indicated that the diminution of the heat received in 1903 may well have been due not only to an increased absorption in our own atmosphere, but in part to a real change in the solar emission, connected with a diminished transmissibility of the solar envelope.

Still more recently I have made further experiments, as yet unpublished, on changes in the transmissibility of the solar atmosphere. These experiments, made by the study of homogeneous rays from a large solar image formed by a horizontal telescope of 140 feet focus, are independent of changes in the earth's atmosphere, and have indicated that the absorption in the solar envelope has decreased within the last six months.

In agreement with this, independent computations of the total solar radiation through our own atmosphere (and so far less trustworthy than those just mentioned) tend to show that the radiation of the sun has somewhat increased during the same interval. I desire not to be understood as stating that these recent changes have undoubtedly occurred, but I feel that there is increasing probability of the confirmation of this result.

There is no novelty in the suggestion that there may be an increase or diminution of solar heat and light due to various causes, and since my earliest statement of the absorption of the solar atmosphere, in the *Comptes rendus* of the Paris Academy of Sciences for March 22, March 29, and September 6, 1875, the subject has, in fact, engaged my continued attention.

What I wish to remark now is that it is only in comparatively recent years that the gradual perfection of the bolometer and other apparatus is providing specific data which render it likely that such changes are now coming within our means of direct recognition.

In sum, the result of the most recent spectrobolometric observations is an increasing probability that the solar radiation itself varies in a degree appreciable to our present means of daily observation, and a strengthening of the belief I have elsewhere expressed that it probably varied through much larger ranges in the past, and may do so again in the future.

It will be seen that I do not venture yet to assert without restriction conclusions like these, which, so far as they may be shown to be true, are not merely of abstract interest, but which in a utilitarian sense may be said to be of far-reaching concern; yet I think it time to ask more general attention to them.

S. P. LANGLEY.

Smithsonian Astrophysical Observatory, June 20.

Use of Radium in Section Cutting.

EVERYONE who has to cut microtome-sections of material embedded in paraffin-wax is frequently troubled by the electrification of the sections. The electrification causes the sections to adhere to the microtome-knife and to fold on themselves instead of being pushed easily across the blade of the knife so as to form a smooth ribbon. The adhesion to the knife also renders the transference to the microslip difficult, and often leads to the breaking up of sections and consequent loss of continuity in the seriation. Further, even when successfully detached from the knife, the

electrified sections are apt to fly about in such an erratic way that it is often a matter of difficulty to arrange them in an orderly manner on the slip.

These undesirable phenomena may be completely avoided by fixing a 5 mg. tube of radium bromide on the microtome-knife close to where the paraffin ribbon is forming. Apparently the radiations from the radium discharge the electrification of the paraffin sections by ionising the air in their neighbourhood.

HENRY H. DIXON.

Botanical Laboratory, Trinity College, Dublin.

The Blondlot *n*-Rays.

THERE is reason to think that M. Blondlot has rendered very valuable service to science by directing the attention of physicists to the remarkable, if not altogether mysterious, class of phenomena with which he has recently had to deal.

There can be no doubt that the phenomena, strange as they may seem to be, which he and his colleagues have observed, whether appearances or realities, or, should I say, subjective or objective effects, still leave something that remains unexplained. Psycho-physiological phenomena are not the less interesting because they happen not to be physical effects as ordinarily understood, and if they can lead scores of trained physicists astray, they should be regarded as all the more important.

For one, I am unhappy in that I cannot merely not see the effects, but neither have I been fortunate enough to meet with anybody who, on severe cross-examination in the dark, did not satisfy me that the variations in brightness which he had observed were altogether subjective, and the result of imagination or fatigue, for expectation counts for a great deal in these observations, and concentration of attention for still more.

The one thing that seemed conclusive about these rays was that they produced so great an increase in the brightness of a small spark that the effect could be photographed, and M. Blondlot has himself shown us photographs which it would appear show unmistakably this result.

I have followed in his footsteps as closely as I could, but unfortunately have not obtained any difference in the photographic effects which could not be attributed to a spurious cause. In M. Blondlot's experiment there is no proof that the diminished brightness of the spark, when a lead screen is interposed, is not due to the presence of the metallic screen itself, which is so close to the spark that it would damp the oscillations of the spark and affect its photographic effect. I have preferred to put out the source of *n*-rays altogether, and to wait for some time, ten minutes or so, or to place a lead screen at a considerable distance from the spark.

I have used a spark of about $\frac{1}{10}$ th mm. between two brass spheres, each of about 1 cm. radius. The effect on a photographic plate 2 cm. away is that of a luminous band the edges of which are close to the spark, practically straight lines, and at a greater distance curve round, being branches of two hyperbolæ.

A change in the brightness of the spark is accompanied by a broadening of the band, and a change in the intensity of the diffuseness of the plate. The breadth of the band depends upon the exposure, and conversely upon the brightness of the spark. Except when there were errors in the adjustment of the apparatus, the two photographs taken on the same plate indicated the same brightness.

It is interesting to note in connection with this point that M. Jean Becquerel maintains that the alleged change in brightness of a phosphorescent screen is really due to an effect on the retina due to the *n*-rays which are reflected by the luminous body. This explanation, however, will not fit in with M. Blondlot's photographic effects, as these rays are not supposed to produce any direct photographic effects. But M. Becquerel's conclusion confirms my result that the *n*-rays, if there be any such, do not really intensify the brightness of a luminous body, even if this be the property by which they were supposed to have been discovered.

JOHN BUTLER BURKE.

Cavendish Laboratory, June 21.