LETTERS TO THE EDITOR.

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Diurnal Variation of Temperature in the Free Atmosphere.

THE following results, which I have recently obtained by a discussion of temperatures obtained in kite and captive balloon ascents, may be of interest in connection with Prof. Clayton's letter (NATURE, February 4) and Mr. Dines's remark that at a height of 1 km. the daily temperature variation becomes insignificant (NATURE, June 17). The daily variation of temperature at a height of 1 km. over Berlin, deduced from 2232 observations made during the five years 1903–7, is given in degrees C. by

$$T = T_s - (4{\cdot}40 \pm 0{\cdot}08) + (0{\cdot}87 \pm 0{\cdot}13)\sin(nt + \theta_1) + (0{\cdot}14 \pm 0{\cdot}10)\sin(2nt + \theta_2),$$

where T_s is the mean surface temperature, and the probable errors are deduced by the method of least squares. The most probable values for θ_1, θ_2, are 197° and 123° respectively, the time being measured from midnight.

The variation deduced from 962 observations, made during the four years 1903–6, in which the wind at a height of 1 km. was 8 metres per sec. and upwards, is given by

$$T = T_s - (3{\cdot}97 \pm 0{\cdot}15) + (0{\cdot}84 \pm 0{\cdot}23)\sin(nt + \theta_1') + (0{\cdot}35 \pm 0{\cdot}15)\sin(2nt + \theta_2').$$

The most probable values of θ_1, θ_2, are 173° and 102° respectively.

The close agreement in the values for the amplitude of the whole day wave for the two cases proves that there is no large error due to the influence of solar radiation on the instruments, and that the variation is a real variation of the temperature of the atmosphere.

The mean daily range is, then, 1·7° C. (or 3·1° F.), compared with a mean daily range of about 5° C. at Kew, where the temperature variation is given by

$$T = T_s + 2{\cdot}56\sin(nt + 226°) + 0{\cdot}42\sin(2nt + 45°).$$

The maximum temperature at a height of 1 km. appears to occur from two to three hours later than at the surface in the whole day wave, and two to three hours earlier in the semi-diurnal wave.

The variation at a height of 2 km., deduced from all (1132) observations, is given by

$$T = T_s - (9{\cdot}84 \pm 0{\cdot}23) + (0{\cdot}64 \pm 0{\cdot}31)\sin(nt + \theta_1) + (0{\cdot}25 \pm 0{\cdot}23)\sin(2nt + \theta_2),$$

the most probable values for θ_1, θ_2, being 270° and 72° respectively.

The magnitude of the probable errors precludes the results from being regarded as final. More observations are needed. But it appears certain that we do not get, on this side of the Atlantic, the remarkable diminution in amplitude and change of phase in the diurnal component which Prof. Clayton found in the first 1000 m. at Blue Hill. The amplitude of the semi-diurnal component does show an increase at 2 km. over its value at 1 km., but, having regard to the relatively large probable errors, one cannot attach any real significance to the result. At the same time, it is of interest to find that at 1 km. and 2 km. altitude in these latitudes the temperature variation is as great as it is over the ocean near the equator, where the value of the daily range is about 1·5° C.

Cambridge, June 20. E. GOLD.

Temperature of the Upper Atmosphere.

AN explanation of the existence of an isothermal layer may possibly be found in the fact that carbon dioxide condenses and freezes at low temperatures even when the pressure is low. The strata in which CO_2 circulates, falling as small drops and then evaporating, must be comparable in the irregularity of their temperature gradients with the strata near the earth in which water circulates. The temperature of the bottom of the mist of CO_2 must be approximately a function of the pressure, so it is to be expected that the height of the mist will vary from day to day and from place to place. In particular, it appears that the change of temperature gradient should occur in the tropics at a greater altitude and lower temperature than elsewhere. The observations to which Mr. Cave refers (NATURE, June 17) confirm this part of the theory.

F. J. W. WHIPPLE.

Merchant Taylors' School, E.C., June 28.

The Aëronautical Society.

IN reference to Prof. Bryan's remarks on the aims and objects of the Aëronautical Society of Great Britain in NATURE of May 27, I would point out that the general scientific character of the proceedings of a society is not annulled because one or more writers have fallen into error, any more than it would be reasonable to say that Prof. Bryan is not a profound mathematician because, in a Friday evening discourse at the Royal Institution, he fell into inaccuracy in scientific history, and said that the Aëronautical Society of Great Britain was at one time called the Balloon Society, and changed its name to its present title, the fact being that the Balloon Society was quite a separate affair, which had its meetings at the Westminster Aquarium and discussed every subject under the sun. In that remark Prof. Bryan showed he had not closely followed the work and career of the Aëronautical Society of Great Britain.

During my eight years of honorary secretaryship of the society, amongst the readers of papers and those who made communications will be found Dr. W. N. Shaw, F.R.S., Mr. W. H. Dines, F.R.S., Prof. C. V. Boys, F.R.S., the late Prof. G. F. Fitzgerald, F.R.S., Prof. Bryan himself, Sir Hiram Maxim, Mr. Lawrence Rotch, Dr. Hergesell, Mr. F. H. Wenham, Captain R. F. Scott, Lieut. E. H. Shackleton, Mr. Orville Wright, Mr. Charles Harding, Mr. W. F. Reid, &c. These names vouch for the general high standard of the proceedings of the society in recent years.

While making these criticisms on Prof. Bryan's remarks, I sincerely hope he will continue his own epoch-making aëronautical researches, for the sake of aëronautical science and for the honour of the Aëronautical Society, of which he is a member.

Airth, Sunningdale, June 14. ERIC STUART BRUCE.

I HAVE no desire to do injustice to the Aëronautical Society, neither do I expect its proceedings to be free from all errors. But in view of the fact that mathematical formulæ and physical considerations now frequently enter into papers bearing on aëronautics, I consider that the time has come when the society should realise the importance of dealing more efficiently with papers of a theoretical character than was necessary formerly. As I have communicated my views on this point to the society through Mr. Bruce, a detailed reply may be unnecessary.

I do not wish all aëronauts to be profound mathematicians. I consider that papers dealing with practical aëronautics have been the most valuable feature of the society's work. Many of the eminent writers to whom Mr. Bruce refers have dealt with the practical and experimental rather than the theoretical side of the subject. Further, a distinction must be drawn between inaccuracies made in discourses or discussions at meetings and those which are allowed to find their way uncorrected into print. But when papers are published in a scientific society's journal which deal with questions of a theoretical character or contain formulæ, it is not unreasonable to expect that the authors shall correctly state and properly apply such principles of mathematics, physics, and mechanics as are found in ordinary text-books, and I trust that, as the result of this correspondence, the exceptions will be less frequent in the future than they have been in the past.

May I, in answer to very numerous inquiries, state with regret that it has been impossible, as yet, to publish a detailed account of my Royal Institution lectures, and some time will elapse before the work in which I am interested is in a suitable form for publication?

G. H. BRYAN.