

observation of which I have latterly made use—which deductive method I attempted in former times, and upon which others have relied since—will, I am persuaded, continue to be incompetent to deal with this real problem of nature unless man's knowledge of molecular physics receive such unhelped-for accessions as will enable him to trace the history of single molecules. Meanwhile, what I advocate is that we avail ourselves of the mixed method, which introduces data established by observation to supplement the deductive method at the point where the deductive method fails.

G. JOHNSTONE STONEY.

30 Chepstow Crescent, W., March 6.

*Postscript, added March 13.*—NATURE of yesterday's date announces the last supposed spectroscopic detection of water vapour upon Mars by one of Prof. Lowell's assistants. Observations of a like kind had been recorded by Sir Wm. Huggins and Prof. Vogel, and the wave-lengths of three of the lines observed were measured by Vogel, two of which may possibly be water lines recorded by Rowland, but not the third.

On the other hand, Campbell and Keeler in a better climate did not see them. Now, however, they seem to have appeared again. This would be the behaviour of a very variable coloured vapour like  $\text{NO}_2$ ; and what I should desire is that an adequate study be made of the absorption spectra of the several such vapours which are unable to maintain themselves in our atmosphere on account of the presence of water, but are presumably to be found on Mars if water does not exist on Mars, and which if present will account for the orange colour of large tracts upon that planet, and for the variations of its colour at different seasons which are conspicuous.

It is to be regretted that the observers to whom we owe so much—from Schiaparelli to Lowell—have kept in view only one of the competing views as to the state of things on Mars instead of at each step considering them both, especially as the one they have preferred is that which some physicists have felt to be the least probable.

G. JOHNSTONE STONEY.

#### The Isothermal Layer of the Atmosphere.

LIKE Dr. Chree (p. 437) I have had experience of the vagaries of self-recording instruments, but I have generally been able to trace them to some remediable defect in the instrument or to the ignorance or carelessness of those who use them. I fancy that the man who constantly uses a certain instrument, and uses it intelligently and not by mere rule-of-thumb, has a fairly correct notion of the magnitude of the errors to which it is liable. If not, what reliance are we to place on any instrumental observations?

It is quite natural, however, to doubt the observations, and when this investigation first commenced I confess that I did the same. Now that hundreds of ascents have been made with different instruments, in different countries and in widely different circumstances, and all the results obtained are in striking agreement, such a view seems to me to be quite untenable. It is true that different instruments sent up with the same balloon have given widely different temperatures, but the results have been published, not concealed, and the instruments improved. I ascribe these discrepancies, which are the exception, not the rule, to solar insolation, which we avoid in England by making our observations after sunset.

With regard to the general question, the difficulties of registering a true temperature are two:—(1) stagnant and unmixed air which may be at different temperatures in different parts of the same garden; (2) the proximity of bodies of large thermal capacity, which by radiation and convection mask the true air temperature. Kites and balloons when they have left the earth are free from these errors, excepting that No. 1 applies to a balloon which does not burst when swimming at its highest point. Since, however, stagnant air does not matter provided sufficient time is allowed, and in this case time is allowed, I do not see what source of error there can be save solar insolation.

My belief in the accuracy of the thermometric results obtained in England is based on inference from the following facts. If a good trace, together with the constants of

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the instrument, is given to two persons, they, working quite independently of each other, will get practically identical results. If the trace and instrument only be given to two persons, they, each calibrating the instrument for himself, will obtain similar results for temperature within the limits stated, but the agreement for height may differ by a kilometre or more in the higher parts. Hence I believe in the accuracy of the temperatures, but do not claim any great accuracy for the heights.

Now with reference to Dr. Chree's questions.

(1) Each station is held responsible for the accuracy of its own results, and I am not acquainted with the routine pursued at each individual station, but the general practice certainly is to test each instrument in spirit cooled by solid  $\text{CO}_2$  both before and after each ascent.

(2) Answered above.

(3) No. The instruments used on the Continent are expensive, and being heavier require a more expensive balloon, and we have no funds with which to meet the expense, especially when it is remembered that balloons and instruments in England are lost about three times out of ten. We hope that this will be done on the Continent before long.

W. H. DINES.

#### Classification of Secondary X-Radiators.

IN NATURE of February 13 there is a letter by Dr. C. G. Barkla and Mr. C. A. Sadler in which the authors divide the elements—according to the qualities of the secondary X-rays emitted by them—into four groups founded upon the atomic weights, without consideration of any other quality of the element. It may be of interest to mention that practically the same classification was given by me as early as 1896 in the *Naturwissenschaftliche Rundschau* (vol. xi., p. 485), and that this classification was also dealt with in a treatise published by Prof. Voller and myself in the *Annalen der Physik und Chemie* (vol. lxi., p. 88, 1897). To this treatise there is added a table printed directly by the secondary rays of a great number of elements, and this shows not only the great difference between the elements of the different groups, but also the agreement in the behaviour of the various elements of the same group.

B. WALTER.

Hamburg, Physikalisches Staatslaboratorium,

March 2.

#### Gods and Godlings.

LEST some readers should infer from your obituary note on Sir Denzil Ibbetson (March 12, p. 443) that this distinguished anthropologist invented the word "godlings" for the rural deities of India, it is worth noting that "godling" was good English in the sixteenth century, and has never been allowed to drop. The Philological Society's "New English Dictionary" quotes Lambard's "Perambulation of Kent" (1570-6) on raising altars "to this our newe found Godlyng"; and examples from Drummond of Hawthornden, Dryden, and Peter Pindar show the convenience of the word. Coleridge preferred "godkin" for a minor deity with masculine attributes, but sanctioned "goddessling." Charles Colton in 1675 permitted a certain cult of "little Goddinkins": Coventry Patmore regarded "godlet" as the more dignified appellative. Anthropologists have therefore had a fairly ample choice; but it should be added that in some of the above examples, at least, Dr. Murray and his coadjutors suspected a "jocular" intention.

DAVID PATRICK.

Edinburgh, March 14.

#### Tabulated Values of Certain Integrals.

IN NATURE, October 24, 1907 (p. 639), the integrals  $x = \frac{k}{2} \int \cos u^2 du$  and  $y = \frac{k}{2} \int \sin u^2 du$  are given. I shall be grateful if any of your readers can inform me where I can obtain tables of the numerical values of these integrals, or any other tables that will reduce the labour of the numerical calculation of them.

C. E. ADAMS.

9 Telford Terrace, Oriental Bay, Wellington,

New Zealand, January 18.