In his last letter, Sir Henry Howorth expresses his belief that, as a consequence of the recent correspondence, I have been led to change the opinions I previously held as to the cause of an Ice Age. May I assure Sir Henry Howorth that he must have quite mistaken the purport of my letter.

With all due courtesy to Sir Henry Howorth, as well as to any others who may have differed from me, I may say that I have seen no reason to swerve from the belief that the position taken in my book on "The Cause of an Ice Age" is the sound position. My opinions are, therefore, unchanged.

I would like to take this opportunity to thank Dr. A. R. Wallace for his letter on this subject in your columns, and to express the satisfaction with which I found that he had been led to the same conclusion, with respect to Mr. Culverwell's argument, as that at which I had myself arrived by an independent method. I note that Mr. Culverwell dissents; but, even at the risk of being thought very obstinate, I must say that I still believe Dr. Wallace and I are right.

ROBERT S. BALL.

So far as I know, it has not been suggested that a comparatively small elevation of the crust of the earth would cause a glacial state in the elevated part. The roughness of an ordinary terrestrial globe, representing an elevation that might be compared with the thickness of letter-paper, would correspond with an elevation of the crust of the earth that even in the tropics would cause perpetual snow. A few thousand years would be sufficient to cause a sufficient elevation without any catastrophic hypothesis.

THEODORE RYLAND.

The Measurement of High Temperatures.

IN a valuable paper on the "Determination of High Temperatures" (*Wied. Ann.* 1895, No. 10), Messrs. Holborn and Wien give the results of their observations on the changes in the resistance of platinum wire over a range of 0° to 1600° C. The authors come to the conclusion that the relation between temperature and resistance "cannot be accurately represented by the Callendar and Griffiths formula"; although, on the other hand, they admit that by means of that formula Heycock and Neville have determined a number of melting points which are in good agreement ("die sich in guter Uebereinstimmung mit unsern Werthen befinden") with the values found by Holborn and Wien when using a thermo-couple standardised by direct comparison with the air-thermometer.

Platinum thermometers are now so generally used for high temperature measurements that an adverse conclusion of this kind is a matter of importance, especially when associated with the names of accurate observers such as Holborn and Wien.

I trust, therefore, that a brief criticism will not be regarded as out of place.

I would first remark that my delay in commenting on this paper is not due to any want of respect for the authors, but from a sense of the importance of the matter. Before attempting any reply, I wished to ascertain the views of Prof. Callendar, and I accordingly wrote to Montreal calling his attention to the paper, but I have not as yet received his answer. I feel, however, that further delay is undesirable.

Neither Prof. Callendar nor I have at any time claimed that the relation between t and ρt , as given by the empirical formula, has been *rigorously* verified at temperatures exceeding 600° C.

In *NATURE*, November 1895, p. 40, I wrote as follows:—"Results of this kind prove that even if the reduction does not express the temperature accurately in the air-scale, it at all events gives us a *constant* scale in which all high temperatures can be expressed, and it is further evident that this constant scale differs but little (even at these high temperatures) from the true air-scale."

The context shows that the "high temperatures" referred to were those in the neighbourhood of 1100° C.

The above quotation defines my own position with sufficient accuracy, and I will therefore pass on to consider the work of Messrs. Holborn and Wien.

(1). The authors state that the platinum wires (in experiments up to 1200° C.) were placed in an externally glazed porcelain tube, and isolated from each other by means of special porcelain capillaries ("die Drähte wurden durch besondere Porzellan Capillaren von einander isolirt"); and at higher temperatures "in externally glazed tubes of a very infusible substance, and isolated from each other by capillaries of the same material."

From this description it appears evident that the wire was in contact with the porcelain, or the other material, probably throughout the greater portion of its length; otherwise it must have been subjected to some strain. Now, contact with porcelain or any similar substance has been found by us to be absolutely fatal: if by any chance the wire has come into contact with the walls of the surrounding tube at high temperatures, the coil has had to be replaced by a new one. This is probably caused by the action of the silica; but whatever the reason may be, the effect has long been known, and care has been taken to avoid this source of error.

It is true that in the present form of the platinum thermometer the wire comes into contact with mica, and I have little doubt that some similar action (although in a lesser degree) takes place between the platinum and the mica. The method of constructing the framework and coil, however, causes the length of platinum in actual contact with the mica to be but an exceedingly small fraction of the length of the wire (probably less than $\frac{1}{100}$ th), and thus any such effect is diminished. I think it possible that the small changes at high temperatures to which I have called attention in *NATURE*, (November 1895, p. 40), are in some measure due to action at the points of contact.

Unless Holborn and Wien took greater precautions in regard to this matter of contact between case and wire than they indicate in their paper, there is no need to seek for further explanation of the somewhat erratic behaviour of the two platinum wires examined by them; in fact, other experiments by the same observers show how materially the resistance of platinum is affected by exposure to the action of silica or hydrogen.

(2). No adverse conclusion should be drawn from the changes in their Wire No. I., for the authors state that "at the termination of the observations the protecting tube was found to have broken in the oven, and the wire had been exposed to the gases of the oven."

The necessity of complete protection from the furnace gases has from the first been insisted upon by those accustomed to the use of platinum thermometers. It was want of attention to this essential matter which led the B. A. Committee of 1874 to a false conclusion. The behaviour of Holborn and Wien's Wire I. has no significance or value, except in so far as it emphasises the importance of complete protection. It appears doubtful if sufficient precautions were taken in this matter with regard to their series of experiments with the platinum Wire II. The infusible tubes which they used for the air-thermometer bulbs at high temperatures were covered externally with a glaze. The authors say: "As the glaze in this case and also in the porcelain becomes liquid much sooner than the softening point of the material, we made use of our method by which there must always be a smaller pressure inside the vessel than outside. Under these conditions the glaze is pressed into the pores of the tubes, otherwise it would immediately come off."

Now I do not find any mention of a similar precaution when heating the platinum wire in what appear to have been similar tubes; it is possible, therefore, that the changes observed in Wire II. were also in some degree due to contamination by furnace gases.

(3). The authors speak of Wire II. as formed from "pure platinum" ("Aus reinem Platin"). They give the value of its temperature coefficient as—

Before heating	...	...	...	...	003801
After	„	...	...	...	003783

As a general rule, the purer the platinum the higher its coefficient. The samples used by us (as, for example, in the Kew Observatory thermometers) have coefficients which vary from 003860 to 003880. It would hardly appear, therefore, that the wire used by Holborn and Wien merits the term "pure." The purity is, however, not of great consequence, as (see *NATURE*, November 1895, p. 40) we have found that although the coefficient depends on the purity, the deduced temperatures are unaffected provided that the coefficient is not reduced by the impurities to lower than about 0032. The fall in the coefficient (above indicated) is, however, of great significance, and in itself is evidence that the wire had become contaminated during the experiments. Assuming (as was doubtless the case) that the wire had been previously annealed, the above change sufficiently establishes inadequate protection of the wire.

I have carefully investigated the numbers in Table II., where the authors give the results of their observations on this second specimen of "pure platinum." I am unable to draw any con-

clusion from the figures there given, except that this wire, either from its quality or its situation, behaved in a different manner from any one of the many specimens I have examined during the past five years. The value of $\delta R/\delta t$ decreases with increase of temperature in a most phenomenal manner.¹ In cases in which I have observed this phenomenon in a lesser degree it has indicated a breaking down in the insulation, consequent on rise in temperature. I am unable to find any evidence that the insulation was tested at high temperature during these experiments.

(4). Any expression of opinion by Messrs. Holborn and Wien necessarily carries weight; nevertheless, I would venture to suggest that (considering the amount of experimental work previously performed by those who advocate the methods of platinum thermometry) the examination of only two wires, one of which was admittedly exposed to the action of furnace gases, affords insufficient grounds for the adverse conclusions arrived at by the authors.

(5). Heycock and Neville's determinations of the freezing points of copper, gold, and silver are admitted by Holborn and Wien to be in "good agreement" with their own. The nature of this agreement is shown in the following table.

	Heycock and Neville.		Holborn and Wien.	
Copper ...	1080.5	...	...	1082
Gold ...	1061.7	...	...	1072
Silver ...	960.7	...	...	971

It is worthy of notice that copper is the only metal of which Holborn and Wien used large quantities, comparable with the masses experimented on by Heycock and Neville. Also in this case Holborn and Wien determined both melting and freezing points. Their results (using practically the same thermometer throughout) range from 1076° to 1093°, whereas Heycock and Neville's values, when using six distinct platinum coils possessing very dissimilar constants, range from 1079° to 1081°·7, a very different order of agreement.

In the case of gold and silver, Holborn and Wien used small quantities, determining their results by observations of the melting points, and thus the method of experiment adopted renders it probable that the temperatures observed would err on the side of excess. The close agreement in the case of copper, and the higher values found by Holborn and Wien in the experiments on gold and silver, are therefore significant.

Thus, when the conditions are similar (as in the case of copper), we may regard the results obtained by the different observers as practically identical. Such agreement would be impossible if platinum resistance thermometers ordinarily underwent, at high temperatures, changes of the nature of those observed in the wires studied by Holborn and Wien. The differences would then be measurable not by units, but by tens and hundreds! and these discrepancies would be found not only when different methods were used, but also when the same observations were repeated with different platinum thermometers.

If Tables XI., XII., and XIII. of Heycock and Neville's paper² are examined, it will be found that although in each case from 6 to 8 different platinum thermometers were used in which, for example, the value of δ varied from 1.495 to 2.04, the extreme resulting temperatures differ by a smaller quantity than the differences obtained by Holborn and Wien when repeating an observation without change in the conditions, by means of the same thermo-couple.

Finally, I assert that Holborn and Wien have produced no evidence sufficient to support the somewhat sweeping conclusions given by them on p. 394 of their paper. I have shown that in their experiments on only two samples of wire, they have neglected the precautions insisted upon by those who have devoted years of study and experiment to the investigation of the platinum thermometer, and this portion of their work is only useful in so far as it emphasises the validity of the conclusions arrived at by those who preceded them.

I fully appreciate the great value of Holborn and Wien's direct determinations of high temperatures by means of the thermo-couple and the air thermometer, and I admit it is probable that for temperatures exceeding 1400° C. or so, the thermo-couple is the more convenient, and possibly the more

¹ This extraordinary behaviour of Holborn and Wien's Sample II. (for reasons previously given the behaviour of Sample I. is of no significance) is noticeable in the numbers attained by them at low, as well as at high, temperatures.

² Chem. Soc. Trans., 1895, pp. 188-190.

accurate instrument. Below such temperatures, however, I consider that the weight of evidence is in favour of the accuracy of the platinum thermometer. In any case, such evidence is in no way weakened by the experiments of Holborn and Wien.

E. H. GRIFFITHS.

Earth Tremors.

IN Prof. Milne's article in NATURE of December 26, he states that earth tremors are more frequent during the winter than during the summer, that they are frequent with a low barometer, and still more frequent when the locality of observation is crossed by steep barometrical gradients. In the North-West Himalayas, throughout the winter months, slight earth tremors are exceedingly frequent, and occur, so far as can be judged without instrumental records, more frequently by night than by day. This may be in part due to the fact that during the day most people would be moving about in downstairs rooms, while at night the same people would be in upstairs rooms, and both they and their surroundings perfectly quiet; but, whatever may be the day and night relation, there can be no doubt that during the winter months in Simla peculiar little earth tremors are remarkably frequent. My experience has been that these tremors are not so much connected with areas of low barometer as with the commencement of a sudden and large change in atmospheric pressure from a high to a low, a reduction of pressure which need not necessarily be accompanied with steep barometric gradients or high winds at or near the earth's surface. In the case of earthquakes, also, I have noticed subsequent large changes in atmospheric pressure. Thus at about midnight on January 15-16, 1896, a (for these regions) rather severe earthquake occurred, which lasted from 1m. 20s. to 4m. in different localities. On the plains the most severe shocks were felt at midnight, 15th, at Simla, at oh. 30s. a.m. on the 16th, and at Srinagar at 1 a.m. on the 16th. Times for other places in the Punjab were published in the newspapers, but I have omitted to keep them. The above, however, show that the shock was felt at Lahore at midnight, at Simla half an hour later, and at Srinagar an hour later. The barometric records show that for the forty-eight hours from 8 a.m. on the 16th to 8 a.m. on the 18th, pressure changed as follows:—

Srinagar (Kashmir) ...	...	...	...	-0.187
Astor " ...	...	...	...	-0.062
Murree ...	...	...	...	-0.165
Lahore ...	...	...	...	-0.144
Simla ...	...	...	...	-0.140
Quetta ...	...	...	...	+0.022

From the above figures, it appears that a considerable decrease of barometric pressure occurred between the morning of the 16th and the morning of the 18th, and that this fall was central over Srinagar; while the times of occurrence of the earthquake show that the movement of terrestrial disturbance was directed towards this central area of diminishing pressure. It has always appeared that the atmospheric changes which ordinarily occur in tropical and subtropical countries would be a wholly inadequate cause to account for the considerable earthquakes which at times occur; but I have undoubtedly noticed that very slight earth tremors constantly take place when a sudden and large decrease in atmospheric pressure commences after a considerable period of high pressure.

W. L. DALLAS.

Simla, January 29

"Roches moutonnées."

SOME ten years ago, I came across in an old memoir a rational explanation of the term *roches moutonnées*; but I made no note at the time, and have been unable to trace the reference. However, my scepticism was fortified, and I proceeded to search French dictionaries, which made it clear that *moutonné* meant "frizzled like sheep's wool," and not "sheep-like." Yet M. de Lapparent tells us ("Traité de Géologie," 3me ed., p. 281) that these glaciated rocks "produisent une impression analogue à celle d'un troupeau de moutons endormis, d'où le nom de *roches moutonnées*"; and who shall question this precise statement of a French author interpreting his own language? It is the explanation that has been taught to all of us, though I know of only one field-geologist who seriously maintains that *roches moutonnées* might be taken for a flock of sheep. Agassiz states