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 Wein 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°·0 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 paper[2] 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 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.

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, emperatures.

2 *Chem. Soc. Trans.*, 1895, pp. 188–190.

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 0h. 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