altered his opinion since he presented his paper on March 19, 1883, to the Academy of Sciences; but, as I have shown, *he did make the statement he now repudiates;* and even if the result of my calculations, founded upon that statement, do not agree with what he now considers to be necessary, he has no right to attribute the discrepancy to any error of mine.

M. Lœwy then proceeds to show that it *is* possible to attach a weight of three-quarters of a ton to end of cross tube. This, I need hardly say, I never disputed; what I did say was, "The absurdity of hanging this three-quarters of a ton" (*i.e.* weight of mirror, objective, and all their supports and attachments) "on end of cross tube, *and yet calling the instrument one of precision,* is too apparent to need demonstration." If I were asked if it would be possible to hang three-quarters of a ton on each end of the Greenwich transit, I might be able to reply in the affirmative, but if I were asked to guarantee that the instrument would, under its new conditions, be as perfect an instrument of precision as it is in its present state, I would not be inclined to risk my "*reputation*" by any such guarantee, and yet M. Lœwy compares the equatorial *coudé* to a transit instrument in stability.

Lastly, on the question of expense. M. Lœwy is anxious to know where I obtained my information, but as I am content to accept his own figures (see his letter in your issue of May 1), so far as the equatorial *coudé* is concerned, there is no occasion to discuss this point. I take the 12-inch size, as it is the nearest to the only one completed, and most likely to be accurate. A 12-inch equatorial *coudé* is estimated at 44,000 francs, *i.e.* say 1760*l.*

Now, in estimating the relative costs of the two forms, your readers will agree with me that for our purposes the fair comparison is between the equatorial *coudé* and such equatorials as are most generally in use in this country, and it is well known that for 1760*l.* a first-rate 12-inch equatorial and dome can be procured, and *this is as nearly as possible what I said,* and I have to thank M. Lœwy for furnishing me with materials to prove my case with so little trouble.

My paper at the Royal Dublin Society (so far as concerned the equatorial *coudé*) was mainly confined to showing that in consequence of its complication it presented many difficulties in its manufacture, particularly for large sizes, and I considered (as I do still consider) that too much was sacrificed in endeavouring to make it an instrument of precision, and to obtain universality. The very fact of its being difficult to construct renders any success the more creditable, and I gladly take this opportunity of expressing my admiration for the excellence of the optical work of the Messrs. Henri, which appears to have withstood the enormous strain put upon it by the peculiarity of construction of the instrument. I still consider, and in this opinion I am joined by the several astronomers of eminence to whom I have spoken on the subject, that the good results are due to the excellence of the optical work, and have been obtained in spite of, and not by reason of, the peculiar form of the instrument.

And now I would say a few words generally on the comparison which has been instituted between the two forms. The claims of the instrument which I propose are very modest. I simply claim for it that by its peculiar construction I can obtain an instrument of large aperture at about one-fourth the usual cost, and that the observer can be situated in a most comfortable position, and free from all the various inconveniences of ordinary observing. I do not claim that the instrument will be one of precision, or that the images will be better after reflection from the mirrors, or that it will be universal, or that it will do all and everything which the equatorial *coudé* will do at four times the cost. What the equatorial *coudé* claims your readers already know. Like many other matters, this also will probably resolve itself into one of cost. If a director of an observatory has 1760*l.* at his disposal, it is for him to decide whether he will have a 12-inch equatorial *coudé*, which commands the whole visible heavens, or a 24-inch telescope on my plan, sacrificing in this case about 20° near the Pole; or, putting it another way, he may consider the question whether he will spend 1760*l.* on a 12-inch equatorial *coudé* or 500*l.* on one of my form of same aperture.

On this matter I shall have more to say in my second letter, in which also I propose to answer all the various objections M. Lœwy has raised to my form. It may, however, be interesting for him to learn that, with a single unimportant exception, he has not raised an objection which has not already been discussed and provided for in the new instrument; but he has suggested to me another objection to the equatorial *coudé* which I shall also treat of in that letter. HOWARD GRUBB

Rathmines, Dublin, May 19

The Earthquake

MY yacht, the *Glimpse*, lay on the ground in the River Colne at East Donyland, about half a mile above Wivenhoe, and as soon as I was able I joined her in order to study the effects of the late earthquake. I remained in the district about a fortnight, and examined the greater part of the focus of disturbance, over an area of about eight miles long by six broad. I distinguished on the ordnance map by appropriate marks (1) those places where the shock had been so violent that not only nearly all the chimneys had been knocked down but a large proportion of the house walls cracked and some boundary walls thrown down; (2) those where it had been less violent, many of the chimneys having been thrown down, but few or no houses cracked; and (3) those where it had been only sufficiently violent to throw down a few isolated chimneys. This third district extends in some directions much beyond the part examined. District No. 2 may be said to trend from Wivenhoe south-west to somewhat south of Little Wigborough, but sends a small, narrow branch north-west up the Colne valley to Colchester. The main part of District No. 1 is at Peldon, Langenhoe, and Strood Mill, but there are two well-marked outliers, one at Wivenhoe and another at Mortimer in Mersea Island. At and near Wivenhoe the intensity of the shock seems to have been greatest at low levels, and such a supposition would explain the character of that outlier, but no such explanation is applicable to the outlier at Mortimer, since the chief damage there is at a high level, and I was unable to discover any reason for its local character.

A great part of my attention was directed to such facts as indicated the direction in which the disturbance moved. The mate of the *Glimpse* was on deck, and says that the yacht was first, as it were, moved violently forwards to the west, and then even more violently backwards to the east. All the circumstances of the case make this a very good observation. In trying to determine the direction of the shock from the effects, I have taken great care to select such cases as would mark the first shock, and not the recoil. Unless this be done, no true result could be obtained, since very commonly the chimneys at one end of a house have been thrown down by the direct shock and those at the other end in the opposite direction by the recoil. On the whole I was able to observe nineteen cases which I looked upon as satisfactory. Almost all these vary from east to south. Perhaps the shock was rather more from the east at Wivenhoe than at Peldon. The mean of the whole is very nearly true south-east, which may be said to agree with the axis of chief disturbance as laid down by me on the map. The only case which is doubtful is that of the church at East Mersea. The manner in which two portions of the tower have been thrown down seem to indicate a shock from north-west. If this could be relied on with perfect confidence, it would show that the church lay on the south-east side of the vertical line, but I saw nothing else to confirm such a conclusion, and I think it quite as probable that the damage was done by the recoil which over the greater part of the district was from that same north-west direction. If this supposition is correct, the shock came up from below somewhat obliquely from south-east under East Mersea, where scarcely any damage has been done, and was most violent along the stroke of the wave at a distance of about three miles to the north-west. This and the general character of the area of chief disturbance seems to me to point to some very irregular distribution of hard rocks at a considerable distance below the surface. H. C. SORBY

Yacht *Glimpse*, Queenborough, May 25

IN your issue of the 8th inst. (p. 31) Dr. J. E. Taylor draws attention to the fact that sound preceded the Langenhoe earthquake for an appreciable period of time. A similar phenomenon has often been recorded, but as I cannot just now quote another instance, allow me to put forward a personal one.

On the morning of Monday, July 11, 1853, I had just gone to bed when I heard a heavy fast-approaching rumbling sound coming from the direction N.W. ¼ N. I was in St. Jean de Luz, and had stopped at an inn which skirts the high road from Bayonne to Madrid. The noise was coming nearer with the speed of an express train, and knowing that the only heavy coach which plied in those days could not pass at such an hour, I concluded that an earthquake was coming and got up to look at my watch, which I had left on a table at the opposite corner of the room; it was 20m. 8s. past midnight. When the noise seemed to issue from the ground *under* me, the whole house shook; it was then 24m. 8s. past midnight. Although occurring at a time