sions. On balancing this deflection, it was found that, to bring the needle to zero, it was necessary to diminish the slide-resistance by 400 millims. Thus the diminution produced in the resistance by exposure to the light of the paraffin-lamp was $\frac{1}{300}$ part of the whole resistance of the tellurium.

On exposing the selenium bar used in my experiments to the direct rays of the same paraffin-lamp at the distance of 1 metre, the resistance of the selenium was diminished by one fifth of its whole resistance.

From the above experiment we see that at the distance of half a metre (that is, with light of four times the intensity) the change of resistance in the tellurium under the same conditions is only $\frac{1}{1000}$ part of its whole resistance.

On exposing the selenium to a constant source of light at different distances, the change in the resistance of the selenium on exposure for 10 seconds (as measured by the swing of the galvanometer-needle) is almost exactly inversely as the distance, *i. e.* directly as the square root of the illuminating power. This law is true whether the source of light be 1 candle or an Argand lamp whose illuminating power is equal to 16 candles.

Taking the mean of a number of experiments, all of which agreed pretty well together, the deflections at the several distances were :—

	At $\frac{1}{4}$ metre.	At $\frac{1}{2}$ metre.	At 1 metre.	At 2 metres.
With Argand lamp	..	170	83	39
„ candle.....	..	41	18	8
„ candle.....	82	39	18	8

Another series of experiments with a candle and Argand lamp (when the illuminating power of the lamp was equal to 12 candles), both at the distance of 1 metre, gave the following results :—

With the candle the deflection was 19 in 10 seconds.
„ Argand lamp „ 66 „

The ratio of the deflections is very nearly 1 to $3\frac{1}{2}$.

These experiments clearly show that the change in the resistance of the selenium is *directly as the square root of the illuminating power*.

[*Correction.*—In my former paper on this subject (Proc. Roy. Soc. vol. xxiii. no. 163), on page 536, line 15, omit the word "opposing," and line 21, for "which opposes" read "in the same direction as;" also on page 539, line 25, for "which opposes a" read "in the same direction as the"; to the end of line 26 add "but in the opposite direction."]

III. "On the Refraction of Sound by the Atmosphere." By Prof. Osborne Reynolds, Owens College, Manchester. Communicated by Prof. Stokes, Sec. R.S. Received November 22, 1875.

(Abstract.)

This paper may be said to consist of two divisions. The first contains an account of some experiments and observations undertaken with a view

to ascertain how far the refraction of sound caused by the upward variation of temperature may be the cause of the difference in the distances to which sounds of the same intensity may be heard at different times.

Some rockets, capable of rising 1000 feet and then exploding a cartridge containing 12 oz. of powder, having been procured, an effort was made to compare the distance at which the rockets could be heard with that at which a gun, firing $\frac{1}{4}$ lb. of powder and making a louder report than the rockets, could be heard under the same conditions of the atmosphere. In the first instance the rockets and the gun were fired from a spot in Suffolk around which the country is tolerably flat, observers being stationed at different distances. Owing, however, to the effect of the wind and the time required for the observers to proceed to the distant stations, these experiments were not successful in establishing the comparative merits of the gun and the rockets. They were, however, important as showing that on hot calm days in July the reports of the rockets never failed to be distinctly audible at distances of 4 and 5 miles, although the sun at the time was shining with full force on the ground and rendering the air near the surface so heterogeneous that distant objects seen through it appeared to wave about and twinkle.

The next attempt was made during a cruise on the east coast. After three weeks cold and windy weather, the 19th of August was a fine day; and some experiments were made in Lynn Deeps, which revealed a very extraordinary state of the atmosphere as regards the transmission of sound. A party rowed away from the yacht in one of her boats, it having been arranged beforehand that either a rocket or a large pistol was to be fired from the yacht when signalled for; also that when those on the yacht heard those in the boat call they should answer. The boat proceeded to a distance of 5 miles, until those on the yacht had completely lost sight of it; but all the time the calls from the boat were distinctly heard by those on the yacht, although after they had lost sight of the boat they ceased to answer the calls. On the boat also not only were the reports of the pistol and rockets distinctly heard, but every answer from the yacht was heard plainly. The last came after an interval of 35 seconds, which gave the distance $3\frac{1}{2}$ miles. Nor was this all; but guns, and on one occasion the barking of a dog, on the shore 8 miles distant were distinctly heard, as were also the paddles of a steamer 15 miles distant.

The day was perfectly calm, there was no wind, the sky was quite clear, and the sun was shining with great power—conditions which have been described as most favourable to the stoppage of the sound by the heterogeneity of the atmosphere, and which may also be described as most favourable for great upward refraction. On this day, however, it was observed that all the time distant objects *loomed* considerably, *i.e.* appeared lifted. This showed that the air was colder near the surface of the sea than it was above. It is to this circumstance that the extraordinary

distances to which sounds were heard on this day is supposed to be due. The diminution in the temperature of the air being downwards, the sound, instead of being lifted as it usually is, was brought down, and thus intensified at the surface of the water, which, being perfectly smooth, was thus converted into a sort of whispering-gallery.

The report of the pistol and the sounds of the voice were attended with echoes, but not so the reports of the rockets; and it is suggested that these so-called echoes may be found only to attend sounds having a greater intensity in one direction than in another.

The second part of the paper refers to a phenomenon noticed by Arago in his report of the celebrated experiments on the velocity of sound made on the nights of the 21st and 22nd of June, 1822.

It was then found that, although the guns fired at Montlhéry could be distinctly heard at Villejuif (11 miles distant), those fired at Villejuif could not be heard at Montlhéry without great attention, and at times (particularly on the second night) they were not heard at all—although on both nights the wind was blowing from Villejuif to Montlhéry, the speed of the wind, which was very light, being about 1 foot per second. No explanation of this phenomenon was offered by the observers, although it was much commented on. And on the second night the gun at Villejuif, which on the previous night had been pointed upward, was brought down in the hope that this might improve its audibility (this step, however, was found to render matters worse than before).

From this lowering of the gun at Villejuif it seemed as though there was probably some difference in the conditions under which the guns at the two stations were placed, as if that at Villejuif was fired from a level, while that at Montlhéry might be fired over a parapet. An inspection of the district confirmed this view; for Villejuif is on a low flat hill, while Montlhéry is on the top of a steep cone; and not only is it 80 feet above Villejuif, but it is surmounted by the mound of an old castle, which is supported by a vertical wall towards Villejuif and surrounded by a low rampart. Hence it is suggested that in all probability the advantage of the gun at Montlhéry was due to its being fired over this parapet, while that at Villejuif was fired from the level ground.

The fact that the wind blowing from Villejuif did not reverse this advantage, suggested the possibility that at night, when the diminution of temperature is downward, a light wind may not produce the same effect upon sound as when the diminution of temperature is upward, as it generally is during the day.

To ascertain if this is the case, some observations were made on some calm nights in May and June of the present year, from which it was found:—

(1) That when the sky was cloudy and there was no dew, the sound of an electric bell 1 foot above the grass could always be heard further with the wind than against it; but

(2) that when the sky was clear and there was a heavy dew, the sound could invariably be heard as far against a light wind as with it, and in some cases much further. On one occasion, when the temperature at 1 foot above the grass was 38° and at 8 feet 47°, and the speed of the wind was 1 foot per second at 5 feet above the grass, the bell was heard 440 yards against the wind and only 270 with it.

Since, therefore, on the nights of the experiments at Villejuif and Montlhéry it is stated that the sky was clear, that there was dew, and the temperature recorded at the two stations shows the diminution to have been downwards, it is argued that the effect of the wind to render the sound less audible at Villejuif was completely balanced by the downward refraction of temperature.

Another phenomenon recorded by Arago is, that while the reports of the guns at Montlhéry as heard at that station were attended with prolonged echoes, this was not the case with those at Villejuif. It is thought that this difference is sufficiently accounted for by the fact that while Montlhéry is surrounded by high hills with precipitous or wooded sides, which must produce echoes, the country in front of Villejuif is very flat and has not a tree upon it for miles.

In concluding the paper reference is made to the Appendix to the last Report of the American Lighthouse Board, in which Dr. Henry, the Chairman, gives an account of his experiments, extending over thirty years, and the conclusions to which they have led him, both of which are in favour of the apparent stoppage of the sound being due to refraction.

IV. "On the Length of the Spark from a Battery of 600, 1200, 1800, and 2400 rod-Chloride-of-Silver Cells, and some Phenomena attending the Discharge of 5640 Cells." By Warren De La Rue, D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S. Received January 6, 1876.

On the 24th February, 1875 *, we had the honour of communicating to the Society, in conjunction with our friend Mr. Spottiswoode, an account of some experiments to ascertain the cause of stratification in electrical discharges *in vacuo*. These experiments were made with a battery of 1080 cells of powder chloride of silver, which was described; we have now in action 3240 such cells, and have recently completed 2400 rod-chloride-of-silver cells †, making our total force 5640 cells in action. To these will be shortly added another unit of 1080 cells powder chloride, and two other units of 1200 rod chloride, making a total of 9120 cells.

We have more recently made a verbal communication to the Society of

* Proc. Roy. Soc. no. 160, 1875. † *Ibid.* p. 357.