XII. *On the Refraction of Sound by the Atmosphere.*

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In a paper read before the Royal Society, May 1874, I pointed out that the upward diminution of temperature in the atmosphere (known to exist under certain circumstances) must refract and give an upward direction to the rays of sound which would otherwise proceed horizontally; and it was suggested that this might be the cause of the observed difference in the distinctness with which similar sounds are heard on different occasions, particularly the very marked advantage which night has over day in this respect. At the time at which that paper was written no direct experiments or observations had been made to verify the truth of this suggestion, and therefore its probability rested on its reasonableness. Since that time, however, I have carried out a series of observations and experiments which, although far from complete, throw some light on the subject, besides revealing some remarkable facts. I hope to be able to continue the investigation; but since its nature is such as to render the chance of bringing it to any thing like a final conclusion very uncertain, it seems to me that it may be well to publish an account of what has been already done; and this is the object of the present communication.

In order to render the object of the various experiments clear, it may be well to recapitulate here some of the theoretical considerations previously explained. It will be remembered that the idea that the variations of temperature would cause refraction of sound occurred to me while making experiments on the effect of wind upon sound, from which it was shown that when sound proceeds in a direction contrary to that of the wind, it is not, as had been thought, destroyed or stopped by the wind, but that it is lifted, and that at sufficiently high elevations it can be heard to as great distances as in other directions, or as when there is no wind—thus confirming the hypothesis first propounded by Professor Stokes and afterwards by myself, that the effect is owing to the retardation of the velocity of the wind near the earth, which allows the sound moving against the wind to move faster below than above, and thus causes the fronts of the waves to incline upwards, and consequently to move in that direction. Having clearly shown that this was the case, it became apparent that any thing which would cause an upward diminution in the velocity at which sound proceeds would cause a similar effect to that of the wind and lift the sound, and that since the speed of the sound depends on the temperature of the air in which it is moving, an upward diminution in the temperature must cause such an effect. That such a diminution of temperature does very often exist was proved by Mr. GLAISHER'S balloon ascents in 1862, in which he found that when cloudy the mean rate of diminution for the first 300 feet was $0^{\circ}\cdot5$ for each 100 feet, and that when clear it was 1°, and that on some occasions it was greater and on others less than this. A variation of 1° in the temperature of the air alters the velocity of sound nearly 1 foot per second, so that with a clear sky the sound instead of moving horizontally would move upwards on a circle of 110,000 feet radius, and with a cloudy sky on a scale of 220,000 feet radius. This rate of refraction is very small compared with that caused even by a very moderate wind; and consequently in order to verify it by experiment it is necessary to observe sounds at much greater distances. This renders the experiment very difficult to carry out; and to make it worse we have no means of determining what the upward variation of temperature is, which therefore can only be surmised by the behaviour of the sound.

The method of experimenting which first suggested itself was the same as that which I had previously employed for wind—namely, to obtain a means of producing a sound of certain intensity, and proceeding to such a distance that it could no longer be heard at the ground or on the level, and then ascertaining whether the range was extended by attaining a greater elevation or elevating the source of sound.

The difficulty in every item of the experiments was greatly enhanced by the increased distance. For the wind an electric bell had answered very well, the range on the level being always less than a quarter of a mile; but where the range was to be measured in miles, something in the nature of an explosion was the only sound available. A place in which to make the experiments was also difficult to find; for it involved a range of several miles of level and unobstructed country, and thus the time occupied in moving from place to place became a matter of serious inconvenience. The greatest difficulty of all, however, was the effect of the wind; since this was much greater than any thing to be expected from the temperature, it was absolutely necessary that the air should be quite calm, a circumstance which no precaution will insure, and for which, as I know from experience, one may have to wait a long while. These various circumstances rendered the results of the first series of experiments less conclusive than I had hoped they might prove.

Experiments with rockets.

I obtained a quantity of rockets capable of rising to a height of 1000 feet and exploding a charge of 12 ounces of powder. The first experiments with these rockets were made at Debach, a village lying between Ipswich and Framlingham, where the country is tolerably flat and traversed by roads in all directions.

I. On the 14th of July, at about 3 P.M., three rockets and three cartridges were fired from the same spot, observers being stationed at three quarters of a mile and a mile and a half respectively. There was no wind, but the sky was covered with a thick haze, the day being very hot. All six discharges were heard at the nearer station, but only the rockets the distance of a mile and a half, although these were heard very distinctly, even their hiss as they ascended.

II. On the 16th of July, at 3 P.M., the day being very hot with no wind, a single rocket was sent up, an observer being stationed at four miles and a half on the Woodbridge road. The explosion was very distinct, but the hiss was not heard.

III. On the 18th a series of rockets were compared with the discharges of a gun capable of firing $\frac{1}{4}$ lb. of powder, and which made a much louder report than the rockets. The observers drove along the Framlingham road, the times of the discharges having been determined beforehand. This road was chosen because at the commencement of the experiments the wind was blowing almost at right angles to it. The wind was very light when the start was made, but before the first gun was fired it had considerably strengthened and changed in direction so as to blow against the sound. It was to this cause I attribute the fact that the first two guns were not heard at a distance of a mile and a half and two miles respectively. After this the direction of the wind again changed, and the two next guns were heard distinctly, although at greater distances; but, strange to say, the rockets at the same distance were not heard. The wind remained constant in this direction until the end of the experiments, and a rocket was heard at four miles. Owing to the changes in the wind the results of these last experiments have shown nothing as regards the refraction of sound, although they show (what was, indeed, shown by the previous ones) that it is possible on a very hot day when there is little or no wind to hear the discharge of a small cartridge, such as that carried by the rockets, distinctly for a distance of four or five miles, and this when the lower stratum of the atmosphere was so heterogeneous that all distant objects near the ground appeared to waver and twinkle as they do when seen over the top of a furnace.

In the hope of improving the conditions of the experiments, I accepted the invitation of my friend Major HARE, of Docking in West Norfolk, to accompany him in his yacht the 'Feronia' during a cruise on the east coast, taking rockets with me. Here I spent three weeks without having a single calm day.

Experiments in Lynn Deeps.

On the evening of the 18th of August, however, the weather improved; and being then in Lynn Deeps, I made some preliminary experiments so as to get the men into the way of firing the rockets. The yacht was at anchor in what is called the Upper Road, and at 9.50 P.M. I rowed with two men in a direction slightly to leeward of the yacht. The wind was very light: at a distance of two miles they fired a large pistol; the interval between the flash and the report was eleven seconds (which gave us our distance); the report was loud and accompanied with prolonged reverberation; a rocket was also heard distinctly, but was not so loud as the pistol, and was not accompanied with any echoes or reverberation. The hails from the yacht were heard by us in the boat quite distinctly, but our answers were not heard on board the yacht. As there was a light mist it was not thought safe to go further away from the yacht, so we returned and waited in hope of being able to do something the next day. In this we were not disappointed; for on this day we observed what I have no doubt will be thought an extraordinary phenomenon, although not of the kind anticipated.

The morning was perfectly calm, with only a few local breaths, which, measured with the anemometer, never registered more than two miles an hour, and came first from the east and then from the west, but not from the north. Up to 12 o'clock the sky was completely covered with a white cloud, which did not show the least sign of movement. The land from four to eight miles distant was hazy; the thermometer stood at 65° in the cabin with all the lights open. The Upper Road, in which the 'Feronia' was anchored, is two miles below the ends of the stone banks which terminate the Lynn Cut, and five miles from Lynn (see accompanying chart, Plate 34). From this station sounds in Lynn were distinctly heard. Steamers could be heard leaving the dock.

About 12 o'clock the sky cleared, and a slight breeze (four miles) sprang up. We then weighed anchor and proceeded down the Bull-dog Channel. Soon after the sky became perfectly clear, and the breeze died away until the yacht had not steering-way. I then had a boat lowered (with the same two men), and proceeded to row to the Roaring Middle Buoy, while the yacht still continued her course as well as she could down the Bull-dog Channel; she was going north by east in a curve, while we were going northwest. Before leaving the yacht I arranged that on our showing two flags they should send up a rocket, and when one they should fire a pistol, and that whenever they heard us call they should answer. When at about half a mile distant I commenced calling, and the answers came back quite distinct; when a little further some one on the yacht commenced tapping the anchor, and we heard this quite distinctly until we were nearly two miles off them; then the tapping was discontinued, and I commenced calling again. Each time the answer came back quite distinct at the instant it was expected, and afforded a good means of checking our distance, which we also knew from the buoys. At two miles, although the calls were quite distinct, I signalled for a pistol; the report was loud. The sun was very hot to us in the boat—so hot, indeed, that it blistered the skin on my hands and face.

The next time I called, the answer was doubtful; but on my calling again, it came quite distinct in thirty seconds. I then signalled for a pistol, and heard a report which we took to be a pistol, but afterwards found to be a rocket, we being too far off for them to distinguish our signals. I then asked for a rocket, and had one, of which we heard the hiss as well as the report. We now proceeded up to the Roaring Middle Buoy and signalled for rockets and pistols, but could get neither, so we judged that they could not see our signals. Although it seemed hopeless, I called from this point, and to my surprise we all heard the answer faint but quite distinct after an interval of thirty-five seconds. It was now about 3 P.M., so that we had been rowing about two hours and a half. We waited at the buoy and kept calling; but as there were now a number of fishing-boats which answered our calls we could not be certain of an answer. At this time our calls appear to have been heard on the yacht but not answered. When we heard the last call, to be sure of it, the yacht was close by the Sunk Buoy; she was now approaching the Well light-ship, which is six miles from the Roaring Middle Buoy. There was now a very light breeze again, so we set up our sail to get steering-way on, and fell down with the tide. We presently heard a rocket go up and explode, but we could make no impression with our signals: we found on returning that they had completely lost sight of us; nor was this surprising considering that we were in a small boat and the sun was directly behind us. A breeze sprang up, so we returned to the yacht, where on comparing notes we found that we had heard every call as well as report. During the interval in which we had no answers, Major HARE, who had been answering my calls, having completely lost sight of us, had gone below to get some lunch; in the mean time the men on deck had heard our calls, but not having instructions had not answered them.

To sum up the results of our excursion:—We had called and been answered up to three miles and a half, and our calls as well as the reports of the rockets had been heard to more than five miles.

Incidentally I noticed that we could occasionally hear the reports of guns from the shore, which was more than eight miles distant; and once while listening for an answer to one of my calls, I distinctly heard a dog bark, which must have been on shore, as there was no boat between us and it except the yacht. All the time we could distinctly hear the paddles of a steamer, which at the time we were at the Roaring Middle was in the Wisbeach Channel, or nine miles from us and fifteen from the yacht, on which her paddles were also distinctly heard.

It appears to me that the distances at which sounds of such comparative low intensity were heard over the water this day is beyond any thing definitely on record. One hears casually, however, of remarkable instances: once in this district I heard of a clergyman who from the Hunstanton side of the Wash heard a man hammering a boat on the Wisbeach side. When one thinks, however, of the extreme difficulty of identifying a sound with its source at three or four miles distance, it is no matter of surprise that such phenomena should for the most part escape notice. On this day, had we not been purposely on the look out, I do not think any thing we heard would have attracted our attention. I have often heard the rifles of volunteers over tolerably flat country seven miles; and, as I have previously stated, the guns of the naval review at Portsmouth were heard by many persons, including myself, in Suffolk, over a distance of 170 miles*.

With regard to the cause of the exceptional distances over which we heard the sounds on the 19th of August, 1874; as was only natural, my attention was all the while directed to this. For the sake of my experiments, what I had been in hope of was a state of the atmosphere which would cause great upward refraction of the sound, and I was naturally on the *qui-vive* for any indications of such a state. All the morning I had been watching the distant objects to see whether they were lifted or depressed by the refraction of light. They loomed to a remarkable degree, which showed that the upward

* They were also heard by Sir WILLIAM THOMSON, who was on board his yacht about 10 or 15 miles to the west of Portland, and therefore 180 miles from Dover.

variation of temperature was the reverse of what I wanted; and before leaving the yacht I had my doubts of our finding much upward refraction of sound—of our being able to hear the rockets further than the guns. I was in hopes, however, that as the sun came out matters might change, and while in the boat I kept looking out for signs of depression in the distant objects. These, however, never came; they loomed all the time, and very considerably. From the boat we could see the water for five or six miles. The yacht's hull was visible to us all the time. On one occasion we had two buoys and a ship in a line, the nearest buoy being two miles from us; we could see the water between this and the second, and again between this and the ship.

It seems to me, therefore, that although in a manner the reverse of what was expected, our observations this day prove the very great effect which upward refraction has on the distances at which sounds can be heard. The looming of the distant objects showed that the air was colder below than above. This would tend to bring the sound down and intensify it at the surface of the water—in fact convert the sea into a whispering-gallery.

No other explanation appears to hold good. The conditions were exactly those which have been described as favourable to acoustic opacity; the sea was calm, there was no wind, and an August sun was shining with its full power, and, having evaporated the clouds, must have been raising vapour from the sea.

During the experiment I particularly noticed the echoes. Except the first and only pistol, none of the reports were attended with echoes or reverberation. But in most cases, though not in all, after calling I could hear the ring of my voice for ten or eleven seconds; and on one or two occasions when there were boats within half a mile of us, I could distinctly hear the echoes from them. Without attempting to explain the reverberation and echoes which have been observed, I will merely call attention to the fact that in no case have I heard any attending the reports of the rockets, although they seem to have been invariable with the guns and pistols. This fact suggests that these echoes are in some way connected with the direction given to the sound. They are caused by the voice, trumpets, and the siren, all of which give direction to the sound; but I am not aware that they have ever been observed in the case of a sound which has no direction of greatest intensity.

ARAGO'S *Experiments.*

These observations in Lynn Deeps were the last I made in 1874. In the spring of this year my attention was called to a phenomenon recorded by ARAGO, which was noticed during the celebrated experiments on the velocity of sound made by HUMBOLDT, ARAGO, PRONY, GAY-LUSSAC, and others, on the nights of the 21st and 22nd of June, 1822, between Villejuif and Montlhéry. On both these nights the sounds from Montlhéry were heard more distinctly at Villejuif than the sounds from Villejuif at Montlhéry, although the wind was blowing (very lightly) from Villejuif to Montlhéry, the speed of the wind being about one foot per second, or, roughly, three quarters of a mile an hour. This remarkable want of reciprocity was much commented on by the observers, although they appear to have been entirely at a loss to account for it.

On reading M. ARAGO's report*, I noticed that the observations on the barometer showed Montlhéry to be about 80 feet above Villejuif, and it occurred to me that this difference of elevation might afford a clew to the mystery. I had observed in my observations of the effect of wind upon sound that a difference of a few feet in the height of the observer or in the source of sound, especially when near the ground, often made all the difference between hearing distinctly and not hearing at all. It appeared to me probable, therefore, that there might be something advantageous in the situations of the gun at Montlhéry and the observers at Villejuif over the situations of the gun at Villejuif and the observers at Montlhéry. I was confirmed in this impression by a fact mentioned by ARAGO, viz. that on the first night the gun at Villejuif had been pointed upwards at a considerable angle, but that thinking this might have had something to do with its not being heard so well as the other, on the second night it was brought down to the horizontal. The result, however, was that the gun was not heard so well on the second night as it had been on the first. This remark concerning the gun at Villejuif seemed to imply that it was fired from level ground and at no great elevation, whereas at Montlhéry it seemed possible that the gun might have been fired over a parapet. To settle this question I took an opportunity last Easter of walking over the ground from Villejuif to Montlhéry, and by the aid of a map made a section of it.

The two stations are visible from each other; that at Villejuif is on the top of a gently rising hill, whereas that at Montlhéry is on the top of a very steep sugar-loaf hill, terminating in the mound of an old castle, which is supported on the side facing Villejuif by a wall some 20 feet vertical, and then so steep that Villejuif can be seen over the tops of the trees surrounding the castle. Part of the old parapet wall is left, and it is impossible to believe but that any one firing a gun from that spot would place it with its muzzle over the parapet. It seems very probable, therefore, that the gun at Montlhéry was fired over the parapet, which would be the most favourable position for being heard, as the direct sound would be strengthened by that reflected from the wall below it, while the observers, standing somewhat behind the parapet, would not have the advantage of any reflected sound, and would therefore be in a disadvantageous position as compared with the muzzle of the gun. At Villejuif the case would be different; the gun, as fired on level ground, would be at a disadvantage compared with the observers, whose ears would be considerably above it. That this difference was sufficient to affect the results seems to have been proved by the evil effect of lowering the muzzle of the gun †.

* Annales de Chimie, 1822, p. 211.

† From my previous experiments on the effect of wind upon sound, I had been led to the conclusion that under certain circumstances there may be an absence of reciprocity in the passage of sound backwards and forwards between two points. Lord RAYLEIGH, however, pointed out to me that there are strong reasons for believing that this is not the case. To prove the force of these reasons, I made some observations behind a large wheat-stack standing alone on level ground, experience having shown me that a wheat-stack from its

These differences in the conditions of the guns and the observers would seem to afford good reason why the guns from Montlhéry should have been better heard than those at Villejuif, supposing other conditions for the transmission of sound to be equally favourable both ways; but the wind was blowing from Villejuif to Montlhéry, and that this should not have reversed the effect is the most remarkable part of the phenomenon. This is remarkable, however, only on the supposition that the effect of the wind upon sound is invariable. As it seemed to me that there were several good reasons for supposing that this is not the case, I thought it might be worth while trying a few observations. I accordingly made some experiments with my electric bell on some very calm nights in May and June, with the following results:—

When the sky was cloudy and there was no dew, the sound could invariably be heard much further with the wind than against it, even when the wind was not more than one foot per second.

But when the sky was clear and there was a heavy dew, the sound could be heard as far against a light wind as with it, and sometimes much further. On one occasion, when the wind was very light (about 1 foot per second at 6 feet above the ground) and the thermometer showed 39 degrees at 1 foot above the grass and 47 at 8 feet, the sound was heard at 440 yards against the wind, and 270 yards with it.

Now the nights on which ARAGO made his experiments were clear; there was a heavy dew, and the thermometer at Montlhéry showed that at that elevation the temperature was 2° F. greater than at Villejuif; so that after the experiments just described there is nothing surprising in the fact that the wind did not produce much effect on the sound.

A good reason (as I have previously stated) may be given in explanation of these changes in the effects of the wind. The wind tends to lift the sound proceeding against it and to bring down that which is travelling with it. These effects are greatest near the earth and diminish as we proceed upwards (for the simple reason that the retardation of the wind is greater near the surface). The effect of the wind, therefore, will be to intensify the sound proceeding against it at sufficiently high elevations (this was found to be the case in my first experiments) and to weaken the sounds proceeding with it at points at some height above the surface—that is, when the sound which is brought

rough surface is a most effectual barrier to sound—sound produced close to one side of the stack being quite inaudible on the other side. On this occasion, however, I found the most perfect reciprocity; sounds produced close behind the stack could be heard at a distance just as well, and no better, than similar sounds at a distance could be heard behind the stack, provided always that great care was taken to bring the ear behind the stack into exactly the same position as that previously occupied by the source of sound. It appears, however, that a few inches difference in the position of the ear on the source of sound was sufficient to make all the difference as to the audibility of the sound. These experiments therefore, although they confirmed Lord RAYLEIGH and showed my previous idea to have been wrong, suggested another explanation of the phenomenon which had led me to it. They show that the apparent absence of reciprocity was in reality caused by my not having taken sufficient notice of small difference in the position of the ear and the bell, and they suggest that the apparent want of reciprocity in the experiments made at Villejuif and Montlhéry was due in the same way to the small differences in the positions of the guns and the ears of the auditors, as pointed out in the text.

down is destroyed by the roughness of the surface, though over a calm sea, the sound brought down would roll along the surface as in a whispering-gallery. Now when the temperature diminishes upwards, as it does generally during a calm day, the effect of the refraction thus caused will be to increase the effect of the wind on sound moving against it, and to diminish that on the sound moving with it. But when the diminution of temperature is downwards, as it was at Villejuif and Montlhéry, and as it always is near the earth on a clear dewy night, it will directly diminish the effect on sound moving against the wind, and increase it on the sound moving with the wind. That is to say it will prevent the wind lifting the sound in one direction and will aid it in bringing it down in the other. Thus it will prolong the distance to which sound can be heard against the wind, and diminish that at which it can be heard with the wind (when the surface is rough); and when the downward diminution of temperature bears a certain relation to the strength of the wind, it is easy to see that it may neutralize or even reverse its effect.

These facts, all taken together, appear to me to afford a satisfactory explanation of the phenomenon observed by ARAGO. There was, however, one other phenomenon observed during the same experiments on which I will venture a word in explanation.

The reports of the guns at Montlhéry as heard at that station were attended with prolonged echoes, but it was not so with those at Villejuif. This phenomenon was not explained by the experimenters; but I think it admits of a simple explanation. The ground surrounding Villejuif towards Montlhéry is very flat with not a tree upon it for miles, and being all arable would at that time of the year be covered with crops. Around Montlhéry the country is hilly, some of the hills rising 100 feet above Montlhéry itself; their sides are in many places precipitous, and are largely covered with trees. From the flat country around Villejuif there would arise no echoes, but from the hills and trees around Montlhéry it is quite certain that there must arise very considerable echoes; and hence it seems to me that the phenomenon becomes simple enough.

The Report of the American Lighthouse Board.

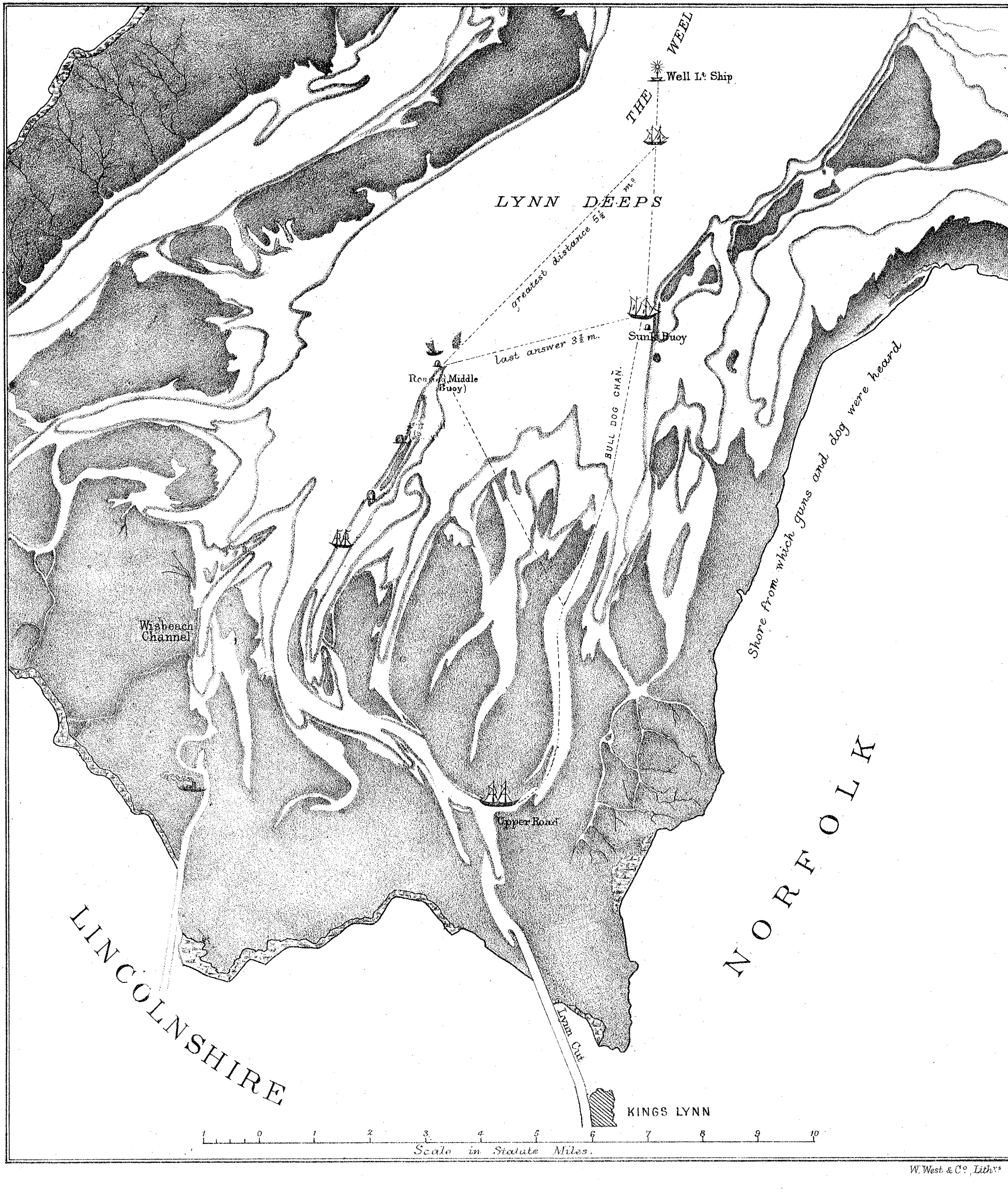
I may remark, in conclusion, that I have just received a copy of the Report of the American Lighthouse Board, kindly sent me by Dr. HENRY, the Chairman of the Board. In an Appendix to this Report, Dr. HENRY has given an account of his experiments on the transmission of sound, undertaken for the Board, and extending over the last thirty years. These experiments have led him to the conclusion that the differences in the distances at which the same sound can be heard at different times are in all cases to be explained by refraction. He has ascribed the cause of the refraction to the wind; and to explain cases in which the refraction did not accord with the direction of the wind, he points out that it is not sufficient to know the direction of the wind at the surface, but that in order to say what would be its effect upon sound, we should know in what direction it is blowing above; for it is not the simple motion of the wind which affects sound, but the difference between its motion above and below. This is very true; and

I have met with instances at night which have led me to apply the same explanation. Many of the phenomena, however, to which Dr. HENRY has applied this explanation are, I feel sure, to be attributed to the effect of the upward variation of temperature. Dr. HENRY does not appear to have been aware of this cause of refraction of sound while making his experiments or drawing up his Report; but in a note at the end he expresses his general agreement with the views stated in my previous paper.

The Heterogeneity of the Atmosphere.

With respect to the stoppage of the sound by the heterogeneity of the atmosphere, Dr. HENRY expressly states that through all his long experience he has never met with a single phenomenon which he can fairly ascribe to this cause; and so far as my experience goes it agrees with that of Dr. HENRY. I am far, however, from thinking that there is no such effect; on the contrary, under circumstances such as those which HUMBOLDT describes as having led him to the idea, it seems to me that it must exist, but that it must at all times be confined to a very small distance above the earth's surface and be over land. That it is the principal cause, or even an important cause of the phenomena under discussion, appears to be more than doubtful; for not only does the necessary effect of refraction appear to be a sufficient cause for these phenomena, and therefore to afford a complete explanation of them, but it is very difficult to conceive the existence of a state of heterogeneity in a calm clear atmosphere at a considerable elevation above the level of the sea.

In the first place such a state of heterogeneity could hardly fail to be observed; for it would necessarily impart a flickering and unsteady appearance to objects seen through it—an effect which may be observed any hot summer's day when looking at objects low down over dry land. Over the sea, however, such an appearance has not been recorded; and although I have often looked for it, I have been entirely unable to detect it. And in the second place, even supposing the air to be in a heterogeneous state at any given instant, such a state could not be maintained many minutes; for different gases, or different portions of the same gas at different temperatures, mix and diffuse very rapidly. It is true that the heterogeneity might be maintained by upward streams of heated air or vapour, and this is doubtless the cause of the heterogeneity of air over dry hot ground; but this heterogeneity, although very apparent near the ground, is never observed at any considerable height. Upward streams of heated air must tend to mix and diffuse rapidly, and the air as it rises is cooled by expansion until it must soon cease to be lighter than the surrounding air. That, as a rule, there are no streams of heated air ascending to any considerable height over land, is definitely proved by the fact that the light smoke from burning weeds never, or very seldom, attains an elevation of any thing like 100 feet. I have often been struck with the way in which such smoke will creep along the ground for the distance of half a mile, and even then not extend to an elevation of more than 20 or 50 feet. Over the sea the cause of such streamlets must be much less potent than over land, and their existence still more unlikely.



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