A NOTICE

OF

RECENT RESEARCHES IN SOUND.

BY

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Reprinted from the American Journal of Science and Arts, for January and February, 1876.

NEW HAVEN:
TUTTLE, MOREHOUSE & TAYLOR, PRINTERS.
1876.

[FROM THE AM. JOURNAL OF SCIENCE AND ARTS, JANUARY AND FEBRUARY, 1876.]

ON RECENT RESEARCHES IN SOUND.

BY WILLIAM B. TAYLOR.

THAT two so eminent physicists as Professor Tyndall in England, and Professor Henry in our own country, should have been for some time past (and almost simultaneously) engaged in investigating the aberrant actions of Sound, is a noteworthy circumstance. That these investigations should also have been undertaken in both cases with especial reference to securing increased efficiency to the national systems of Fog-signaling, is no less notable. In view of the many disastrous marine accidents resulting from fogs on either coast, such inquiries so conducted, have a no slight practical importance; and every thoughtful mind must regard with profound interest a series of researches requiring so much patient labor for the attainment of new and accurate information on the subject, and so high a degree of scientific sagacity and skill for its right interpretation.

As somewhat different explanations have been offered by these two distinguished observers to account for certain abnormal phenomena of sound, a concise statement of the facts and views respectively announced, will interest the general reader. The records of these investigations are, on the one side, the *Philosophical Transactions of the Royal Society* of London, for the year 1874, vol. clxiv, page 183, "On the Atmosphere as a Vehicle of Sound," by John Tyndall, LL.D., F.R.S., a communication read February 12, 1874; and on the other side, the annual *Report of the Light House Board* of the United States for the year 1874: the Appendix to which is an account of the operations of the Board relative to Fog-Signals, by Joseph Henry, Chairman of the Light House Board. In addition to these principal sources of information, reference will be made to an interesting communication read before the Royal Society, April 23, 1874, "On the Refraction of Sound," by Professor Osborne Reynolds, and published in the *Proceedings of the Royal Society* for 1874. The salient points of the observations are selected, and are here arbitrarily designated by bracketed numbers, to facilitate comparisons.

I.

Ten years ago, or in 1865, Professor Henry commenced his investigations on the subject of Sound in connection with fog-signals, at the Light House station near New Haven, Connecticut. Omitting here his careful experiments in regard to the character of the various instruments employed, the principal results then obtained, were the following:

[1.] The reflection of sound was observed to be very imperfect and inexact. A large concave reflector with a smoothly plastered surface of 64 square feet, produced a sensible increase of effect in the sound, within a distance of 500 yards in front of the signal: beyond this distance, the difference became imperceptible. It appeared that "while feeble sounds at small distances are reflected as rays of light are, waves of powerful sound spread laterally, and even when projected from the mouth of a trumpet, at a great distance tend to embrace the whole circle of the horizon." (*L. H. Rep.*, p. 88.) A trumpet, however, which could be heard six miles in front (in the direction of the axis) was heard only three miles in the rear. (p. 92.)

[2.] "For determining the relative power of the instruments, the use of two vessels had been obtained." The instruments at the light-house station were a large bell, a steam-whistle 6 inches in diameter, a double whistle, "improperly called a steam gong," 12 inches in diameter, the cups being 20 and 14 inches deep, producing the harmonic interval of a tone and its fifth, and a Daboll trumpet operated by a hot air engine. The blow-off sound from the "exhaust" of the air engine was also noted. "The penetrating power of the trumpet was nearly double that of the whistle." (*Rep.*, p. 90.) The order of audible range on the first day was found to be 1st, trumpet, 2nd, exhaust, 3rd, bell, the whistle not being sounded. On the second day, 1st, trumpet and "gong," 2nd, whistle, 3rd, exhaust. In the rear the trumpet was heard no farther than the whistle. On the third day, the order was similar, —1st, trumpet, 2nd, whistle, 3rd, exhaust, 4th, bell. (p. 91.) The opportunity was unfavorable to the observation of these sounds when they were moving directly with the wind.

[3.] Simultaneous observations from two vessels sailing in nearly opposite directions, showed that the sound did not extend against the wind so far as in the direction of the wind; and on subsequent days, results obtained from sounds moving nearly against the wind, and at right-angles to it, indicated that an opposing wind, when light, obstructed sound less than when stronger, and that wind at right-angles to the sound, permitted it to be heard farther. (*Rep.*, p. 92.)

[4.] "During this series of investigations an interesting fact was discovered, namely, a sound moving against the wind, inaudible to the ear on the deck of the schooner, was heard by ascending to the mast-head." (p. 92.) These results were obtained in 1865.

[5.] An experiment subsequently made at Washington during a fog, with a small clock-work alarm bell, indicated that the fog did not absorb sound; though want of the opportunity of a comparative observation prevented the result from being entirely satisfactory. (p. 93.)

In 1867, the principal object of investigation was a comparison of different instruments. the character and value of the improvements made in them and especially an examination of a new fog-signal made under the direction of the Board by Mr. Brown, of New York,—the steam siren (p. 194), an instrument which has since played an important part in fog-signaling. Employing 1st a large Daboll trumpet, 17 feet long, (its steel tongue being 10 inches long), and operated by a hot air engine, 2nd, a siren operated by a tubular steam boiler, and 3rd, a steam whistle, 8 inches in diameter,—an elaborate series of experiments was made as to their penetrating power, as to the most efficient pitch or tone, (p. 95), the effect of varying steam pressure from 20 pounds per square inch to 100 pounds per square inch, (p. 97), the material and shape of the trumpets, &c. (p. 98.)

[6.] During this series of experiments in 1867, attention was called by General Poe, of the Light House Board, to the circumstance that the sound of the paddle-wheels of a steamer some four and a half miles distant from the shore could be distinctly heard by bringing the ears near to the surface of the beach. This fact had previously been noticed on the northern lakes. The desirability of experimenting with large hearing trumpets placed near the surface of the water is suggested by Professor Henry. (p. 98.)

[7.] Experiments on the divergence of acoustic beams, while indicating a considerable reduction of sound toward the rear of the trumpet, showed also very strikingly, the increasing tendency of sound to spread on either side of the axis of the trumpet. (p. 98.) This corresponds with the observations [1] on the employment of sound reflectors.

An important suggestion is made, requiring experimental determination, namely, that condensed air would probably give more efficient results to both the fog-whistle and the siren, than steam. "From hypothetical considerations this would appear to be the case, since the intensity of sound depends on the density of the medium in which it is produced; and as the steam is considerably lighter than air, and as the cavities of all these instruments are largely filled with steam, the intensity of sound would on this account seem to be less." (*Rep.*, p. 99.)

In the absence of Professor Henry in England in 1870, experiments were continued by General Duane, one of the Light House District engineers. These will presently be noticed.

[8.] In 1872 Professor Henry observed from a steamer in the harbor of Portland, Maine, that while approaching an island from which a fog-signal was audible,—at the distance of two or three miles, the sound was lost for nearly a mile, and then slightly regained at nearer approach. This was partly in the rear of the signal; and from its position on the farther side of the island from the steamer, with a large house and rising ground interposed, Professor Henry infers that the region of inaudibility was covered by an acoustic shadow, encroached upon at a greater distance by the divergence of the rays of sound, which, bending, reached ultimately the surface of the water. (p. 107.) A similar phenomenon was observed in the same year on approaching Whitehead station near the coast of Maine. The fog-signal was heard from the distance of six miles to about three miles, and then lost until within a quarter of a mile. (p. 107.) Again, at little Gull Island, in a vessel receding from the siren signal in the direction of its trumpet axis, the sound was lost at a distance of two miles, and then regained at a distance of four and a half miles. (p. 111.) These last cases are referred by Professor Henry to a flexure of the rays of sound resulting from differences of wind velocity in the upper and lower strata of air.

[9.] In 1872, it was observed that a fog-signal was heard from one station to another, while a simultaneous signal from the latter was inaudible in the opposite direction. On board a steamer approaching Whitehead station (a mile and a half from the coast of Maine), the signal, a steam-whistle, failed to be heard from the distance of about three miles to about a quarter of a mile from the station; while a smaller whistle on the steamer was distinctly heard by the keeper at the station during that time. The wind was slightly transverse to the direction from the steamer to the station, but approximately in that direction. The steamer after stopping at the station, on passing from it almost directly against a light wind, continued to hear the signal with variable distinctness for about fifteen miles. (p. 108.) In September, 1874, the keeper at Block Island, on the coast of Rhode Island, observed according to instructions, the times when the fog-signal from Point Judith at a distance of seventeen miles was audible, and in comparing the times when the Block Island signal (a powerful steam siren) was heard at Point Judith, it appeared that the two sounds had not been heard simultaneously by the two keepers. (p. 112.)

[10.] In August, 1873, at Cape Elizabeth station in Maine, the phenomenon of ocean-echoes was distinctly noticed on board a steamer as it was passing directly outward from the signal; the sound after each whistle being returned from the unobstructed space beyond. (p. 109.) In September, 1874, at Black Rock Island also, shortly after each blast of the trumpet, a prolonged echo from the open ocean was distinctly heard. The echo was observed not to be loudest at the siren-house, but at a point several hundred yards to one side; the wind being in the direction of the primitive sound, and nearly opposite to the direction of the reflected echo. (p. 112.) This was supposed by Professor Henry to be caused by a reflection of the sound from the crests and slopes of the waves.

[11.] On September 23rd, 1874, three observations were made on board steamers moving in opposite directions about one and a half miles from Sandy Hook, New Jersey. First, before noon with the wind from the west, second, at noon with the wind lulled to a calm, and third, an hour and a half later, with the wind blowing from the east. These observations gave the unexpected result of the sound being heard in each case uniformly farthest from the west, irrespective of the wind. (p. 114.) On the next day, September 24th, the observations were repeated farther out at sea, or six miles from the nearest land. Small balloons, sent off with each observation on the sound, showed that notwithstanding the change of surface wind as before, from morning to afternoon, the upper current of wind was steadily and continuously from the west. (p. 115.) Professor Henry supposes that in the first case "the motion of the air being in the same direction both below and above, but probably more rapid above than below on account of resistance, the upper part of the sound-wave would move more rapidly than the lower, and the wave would be deflected downward, and therefore the sound as usual heard farther with the wind than against it." In the third case with a local sea-breeze in the opposite direction, and the upper current remaining unchanged, "the sound should be heard still farther in the same direction or against the wind at the surface, since in this case the sound-wave being more retarded near the surface, would be tipped over more above, and the sound thus thrown down." (p. 115.) This explanation derived from a communication of Professor Stokes, at the Dublin Meeting of the British Association in 1857, (*Rep. of B. A.*, 1856, p. 22 of Abstracts) would appear to be a very satisfactory solution of the apparent anomaly.

II.

In 1870, General Duane, the engineer in charge of the Light House District embracing the coast of Maine, New Hampshire, and Massachusetts, was assigned by the Light House Board, (as one "who from his established reputation for ingenuity and practical skill in mechanism, was well qualified for the work,") to make experiments and observations on fog-signals. Accordingly during the year 1871, extensive investigations were made by him at Portland, Maine. Passing over his valuable remarks on the qualities of fog-signals, the following are the principal facts observed by him:

[A.] The extremely variable range of sound. The steam fog-whistles on the coast of Maine could frequently be heard at a distance of twenty miles, and as frequently could not be heard two miles, with apparently the same state of the atmosphere. (*L. H. Rep.*, p. 100.)

[B.] The signal was often heard at a great distance in one direction, while scarcely audible at a mile in another direction, and this quite irrespective of the wind. (p. 100.)

[C.] Falling snow was observed not to obstruct sound sensibly, as the steam-whistle on Cape Elizabeth can be "distinctly heard in Portland, a distance of nine miles, during a heavy northeast snow-storm, the wind blowing a gale directly from Portland toward the whistle." (p. 100.)

[D.] The signal station frequently "appears to be surrounded by a belt varying in radius from one to one and a half miles, from which the sound appears to be entirely absent." Receding from the signal, its sound may be audible for the distance of a mile, then lost for the distance of a second mile, and then audible again for a much farther distance. This abnormal phenomenon has been observed at various stations, and at one where the signal is on a bare rock in mid-ocean, twenty miles away from land, and with no surrounding objects to affect the sound. (p. 100.)

No observations have been made to show that this occasional sound-chasm is really a "belt" entirely surrounding the signal; a supposition which appears to be antecedently improbable, and one which would require a large number of radiating observations made simultaneously, to establish it. The curious and exceptional fact, however, is confirmed by the observations of Henry [8] made subsequently.

[E.] Confirmatory of Henry [1], General Duane found that a whistle in the focus of a large parabolic reflector, though giving a notably louder sound in front near the reflector, yet at the distance of a few hundred yards, had its beam of sound so spread that the acoustic shadow behind the mirror vanished, and no perceptible difference appeared. A wooden trumpet or square pyramidal box 20 feet long, in a horizontal position with the whistle in the smaller end, gave, however, more successful results, the increase of sound in the open axis being perceptible at the distance of a mile. (*Rep.*, p. 103.) This corresponds also with Henry's observation [7].

[F.] In repetition and explanation of observation [A] General Duane remarks: "It frequently occurs that a signal which under ordinary circumstances would be audible at the distance of fifteen miles, cannot be heard from a vessel at the distance of a single mile. This is probably due to the reflection mentioned by Humboldt." (p. 104.) This great traveler and scientific observer, in his graphic narrative of exploration in the northern part of South America, published at the beginning of the century, ascribes the diminished audibility during the day, of the noise from the cataracts of the Orinoco, at a place on the Atures, to the unequal heating of the air and the reflection and dispersion of the sound from the surfaces of the striæ of differing density.

[G.] It was further noticed by General Duane that "when the sound is thus impeded in the direction of the sea, it has been observed to be much stronger inland;" tending to confirm his idea that the sound in passing from a warmer to a cooler region of air "undergoes reflection at their surface of contact." (p. 104.)

Professor Henry dissents from this opinion that the extinction of powerful sounds is due to unequal density of the atmosphere. Admitting that "a slight degree of obstruction of sounds may be observed" from such a condition, he thinks it "entirely too minute to produce the results noted." (p. 104.) He believes that the "true and sufficient cause" is the difference between the upper and lower currents of air, which tends to bend the sound rays either upward or downward, as suggested by Professor Stokes in 1857. He adds, "In the comments we have made on the Report of General Duane the intention was not in the least to disparage the value of his results which can scarcely be too highly appreciated." (*Rep.*, p. 106.)

[H.] A difficulty occasionally observed with vessels in a fog, is an apparently false direction of the audible signal; which General Duane regards as "due to the *refraction* of sound in passing through media of different density." (p. 104.)

[I.] While thus adopting "the conclusion that these anomalies in the penetration and direction of sound from fog-signals, are to be attributed mainly to the want of uniformity in the surrounding atmosphere," General Duane was also led from observation and experiments to believe "that snow, rain, fog, and the force and direction of the wind, have much less influence than has generally been supposed." (p. 104.) This is in confirmation of his previous observation [C].

III.

Professor Tyndall commenced his investigations on fog-signals on the 19th of May, 1873, "at the instance of and in conjunction with the elder brethren of the Trinity House," as the scientific adviser of the Corporation.

[1.] On May 20, 1873, observations showed the relative penetrating power of different instruments to be variable. At six miles the fog-horn was inaudible, while an eighteen pound gun with three pound charge was heard for ten miles. On many subsequent occasions the horn was found to be superior to the gun. (*Trans. R. S.*, p. 188.) Occasionally the whistles were superior to the trumpet, though not generally so. (p. 189.) Later experiments in October showed that the pitch of the sound had variable penetration on different days and even at different times on the same day. The siren (an American instrument lent by the United States Lighthouse Board, and put in use October 8, 1873) was generally decidedly triumphant, but not always so. (*Trans.*, pp. 220, 221.)

[2.] The defect of sound in the acoustic shadow of an intervening obstacle (a chalk cliff) was very strikingly manifested. In June the same sharpness of shadow line was observed; and even with the instruments in view, at the distance of a mile, their sound entirely failed near the shadow line at one side. (*Trans.*, p. 190.)

[3.] Although "the wind exerts an acknowledged power over sound," yet, on the 25th of June, "when the range was only six and a half miles, the wind was favorable; on the 26th when the range exceeded nine and a quarter miles, it was opposed to the sound." (p. 194.) On October 11, the sound was observed to be much affected by an adverse wind. It was also noticed on this as well as on subsequent occasions, that "an opposing wind affects the gun-sound far more seriously than that of the siren." With a favoring wind, sounds were heard twice as far as with an adverse wind, even at a point "more deeply immersed in the sound-shadow." (p. 224.)

[4.] July 1, at a distance of five and a quarter miles from a rotating horn it was observed that the sound was sensibly stronger in front than at the rear of the trumpet, the reduction being estimated as seven to ten. (p. 192.)

[5.] July 1. "In a thick haze, the sound reached a distance of twelve and three-quarter miles, while on May 20, in a calm and hazeless atmosphere, the maximum range was only from five to six miles." (p. 193.) And subsequent observations made in London, December 10 and 11, showed that a thick fog offered no sensible obstruction to the passage of sound. (pp. 209, 210.)

[6.] On July 3. at 2.15 P. M. "with a calm clear air and smooth sea," at three miles from the signal station "neither horn nor whistle was heard. The guns were again signaled for; five of them were fired in succession, but not one of them was heard." (pp. 194, 195.) As a hot sun was pouring its beams on the sea, Professor Tyndall supposed that the copious evaporation resulting, would most probably act very irregularly, producing streams or wreaths of vapor, and thus render the air *flocculent* with these invisible cloudlets, whose surfaces would occasion a large amount of repeated reflection and dispersion of the sound waves. As the sun afterward became clouded at 3.15 P. M., the sounds of the signal were heard at three miles, and very faintly at four and a quarter miles; and later at six miles, and seven and three-quarter miles. Toward the close of the day the signals were heard at twelve and three-quarter miles. (pp. 196, 197.)

[7.] On the same day at one o'clock, the echoes from the direction of the open sea were very distinct at the signal station. "The instruments hidden from view, were on the summit of a cliff 235 feet above us, the sea was smooth and clear of ships, the atmosphere was without a cloud, and there was no object in sight which could possibly produce the observed effect. From the perfectly transparent air, the echoes came, at first with a strength apparently but little less than that of the direct sound, and then dying gradually and continuously away." (p. 198.) These remarkable echoes are supposed by Professor Tyndall to be returned from the invisible surfaces of the vaporous striæ, which thus render the air opaque to the sonorous waves. Subsequently, on the 8th of October, the American siren being just received and set up, its loud echoes were observed to be "far more powerful than those of the horn," and to last eleven seconds, while those of the horn had eight seconds duration. (p. 199.) On the 15th of October, the direction of the echoes was found to correspond with the principal axis of the direct or primitive sound; the direction of the return sound changing with the rotation of the horn. (p. 200.)

[8.] On October 8th rain and hail were found not to obstruct sound. While in the morning (after a thunder storm) from Dover and the South Foreland across the English channel "for a time the optical clearness of the atmosphere was extraordinary, the coast of France, the Grisnez lighthouse, and the Monument and Cathedral of Boulogne being clearly visible in positions from which they were generally quite hidden; the atmosphere at the same time was acoustically opaque;" and the horn was feebly heard at six miles. (p. 205.) But in the afternoon a storm arose, and although the rain was falling heavily all the way between the signal station at Foreland and the point of observation on the steamer, "the sound instead of being deadened, rose perceptibly in power. Hail was now added to the rain, and the shower reached a tropical violence." "In the midst of this furious squall both the horn and the siren were distinctly heard," and as the shower lightened, diminishing the local pattering of the deck, they were heard "at a distance of seven and a half miles distinctly louder than they had been heard through the rainless atmosphere at five miles." (p. 206.) On the 23d of October, a similar experience was noticed on land, and contrary to the usual impression, snow was also observed to offer no serious obstacle to sound. (p. 207.)

It must be borne in mind that the investigations by Professor Tyndal were concluded before the publication of the United States Lighthouse Report. And it is noticeable that these two series of original observations thus independently made on the opposite sides of the Atlantic, in the main quite strikingly confirm each other.

Tyndall's notice [1] of the inconstant relative range of different instruments corresponds with Henry (2), though indicating a much more marked variability.

Tyndall's notice [2] of the sound shadow, corresponds generally with Henry [7], and [8], and Duane [E], but assigns a sharper definition to its limit; probably in consequence of the intervention of a larger obstacle (a cliff), and an observation within a shorter distance.

Tyndall [3] confirms Henry [3], and [11].

Tyndall [4] corresponds with Henry [7], and Duane [E].

Tyndall [5] confirms by a series of careful observations, the opinion of Henry [5] and Duane [I],

Tyndall [6] confirms Duane [A], and [F], and in like manner adopts and extends the suggestion of Humboldt as to the cause of acoustic opacity. Professor Tyndall's admirable skill in experimental physics enabled him to illustrate and fortify his hypothesis by exhibiting in a popular lecture an apparatus for producing in an elongated box or tunnel, aerial laminæ of unequal density, through which the sound from a small alarm box failed to excite a sensitive flame. That this mottled condition of the air is therefore a true cause of acoustic obstruction is no longer doubtful. To what extent a similar condition of the atmosphere actually prevails, in view of the law of the diffusion of gases, and how far such usual or unusual inequalities of density in the air are capable of entirely dispersing the powerful sound of a steam trumpet or siren, at the distance of a quarter of a mile, are not so positively determined. With a continuous wind any such condition of aerial "flocculence" might be expected to be very speedily dissipated.

This theory, however, fails entirely to explain the interesting observations of Henry [4], [8], and [9]. It is scarcely credible that a local screen of aerial flocculence could obliterate on the deck of a schooner, a fog-signal audible at the mast-head. Atmospheric refraction on the other hand, completely satisfies the observed condition; an opposing wind blowing at the time. Still less successful is the theory, in dealing with the abnormal phenomenon of simultaneous audibility at long range, with the intermediate "belt" of acoustic opacity, first observed by Duane [D]. And lastly, the assumption of the simultaneous transmission of sound *through* a flocculent air-screen in one direction and its absorption or dissipation by the screen in the opposite direction, (acoustic "non-reversibility,") is obviously inadmissible. Nor is the supposition of acoustic "diffraction" around the defined edge of a vapor cloud, more available.

Professor Tyndall in his recent Preface to the last edition of "Sound" remarks upon this observation of Henry [9]—"a sufficient reason for the observed non-reciprocity is to be found in the recorded fact that the wind was blowing against the shore-signal, and in favor of the ship-signal." (*Preface*, p. xxi.) But he offers no suggestion how this "sufficient reason" is supposed to apply. As it is well known that an ordinary wind cannot increase the range of sound more than two or three per cent (an amount quite inappreciable), this circumstance alone is wholly inadequate to account for the complete suppression of the shore-signal (a ten-inch steam-whistle) from the distance of three miles to *a quarter of a mile*, while the feebler sound of the ship-signal (a six-inch steam whistle) was making itself distinctly heard throughout the three miles. Something more therefore than the direct or convective action of the wind must be invoked to explain the facts.

Tyndall's observation [7] on the aerial or ocean echoes, corresponds with Henry [10] excepting as to the direction of the principal echo. This difference is doubtless due to the special arrangement of the surfaces or points of reflection in the respective cases observed. Professor Tyndall connects this phenomenon with that of acoustic opacity [6]; and here again his fine experimental skill is brought into requisition to demonstrate the reality of artificial "aerial echoes." By so simple a device as the employment of the flat side of a "bat-wing" gas-jet, the sound beam from a reed instrument was shown to be entirely deflected from one sensitive flame, and reflected back toward another.

This view of a relation between the acoustic opacity outward or seaward, and the reinforcement or reflection of sound inward, is in striking accord with Duane [G], who, however, in referring to the "reflection" of sound, does not specifically allude to the ocean "echo." On the refraction theory also, a necessary result is that a deflection of the sound-beam upward in one direction, must be attended with a downward deflection and consequent increase of sound in the opposite direction.

Professor Henry had referred these mystic echoes to the crests and slopes of distant waves; (in conjunction probably with a curvature of the sound-beams, constituting a kind of acoustic "mirage.") To this suggestion, Professor Tyndall opposes the observation that "the echoes have often manifested an astonishing strength, when the sea was of glossy smoothness." (*Sound*, *Pref.*, p. xxiii.)

That this very interesting subject presents features requiring still further and more refined investigation is sufficiently obvious from the single consideration that aerial opacity and echo have not been shown to bear that direct relation which the vapor theory requires. Professor Tyndall has recorded that, on the 17th of October (1873), "It is worth remarking that this was our day of longest echoes, and it was also our day of greatest acoustic transparency, the association suggesting that the duration of the echo is a measure of the atmospheric *depths* from which it comes. On no day, it is to be remembered, was the atmosphere free from invisible acoustic clouds; and on this day when their presence did not prevent the direct sound from reaching to a distance of 15 or 16 nautical miles, they were able to send us echoes of 15 seconds duration." (*Trans.*, p. 202.) If these echoes were not "folded," this would represent an extreme limit of about a mile and a half. Our most powerful sounds cannot afford to waste much of their energy on echoes, if under the inexorable law of increasing attenuation as the square of the distance they are to be audible through a range of 16 miles: less than the 400th of the intensity at one nautical mile, that is heard at the distance of 100 yards from the source; and one 256th of this at the distance of 16 nautical miles, or less than the hundred thousandth of the intensity at 100 yards. And the inference is strong that in such a case, accompanying echoes must be derived from sound beams in a somewhat different direction.

Further observations are needed also to ascertain whether these aerial screens of unequal density and acoustic opacity are capable of returning echoes on opposite sides, as is to be expected if we may accept the analogy of catoptrics: and whether the echoes are as frequently heard from steamers in mid-ocean, or whether they mainly attach themselves to coast lines. As Professor Henry has well stated: "Much farther investigation is required to enable us to fully understand the effects of winds on the obstruction of sound, and to determine the measure of the effect of variations of density in the air due to inequality of heat and moisture." (*L. H. Rep.*, p. 117.)

As the last of the series here selected, Tyndall's observation [8] agrees well with the observation of Duane [I].

IV.

The communication of Professor Reynolds "On the Refraction of Sound by the Atmosphere," is in two parts: the first of which considers "The effect of Wind upon Sound," and the second part "The effect of variations of Temperature." The experiments were all made in "a flat meadow of considerable extent;" and the apparatus employed "consisted of an electrical bell mounted on a case containing a battery. The bell was placed horizontally on the top of the case, so that it could be heard equally well in all directions; and when standing on the ground, the bell was one foot above the surface." An anemometer was also used to determine the velocity of the wind. (*Proceedings of the Royal Society;* republished in the *L. E. D. Phil. Mag.*, for July, 1875, vol. 1, p. 67.)

The experiments were made on four different days, the 6th, 9th, 10th, and 11th of March, 1874; and on the last two days the ground was covered with snow, which furnished an opportunity of comparing the effect of different surfaces on the range of Sound. Additional experiments were made on the 14th of March.

[1.] "On all occasions the effect of wind seems to be rather against distance than against distinctness. Sounds heard to windward [that is *against* the wind] are for the most part heard with their full distinctness; and there is only a comparatively small margin between that point at which the sound is perceptibly diminished, and that at which it ceases to be audible." (*Phil. Mag.*. p. 63.)

[2.] The sound of the alarm-bell was always heard "farther with the wind than at right-angles to its direction; [contrary to the old observation of De La Roche in 1816,—which was obviously an exceptional one;] and when the wind was at all strong, the range with the wind was more than double that at right angles . . . . *With* the wind, over the grass the sound could be heard 140 yards, and over the snow 360 yards, either with the head lifted or on the ground; whereas at right angles to the wind, on all occasions the range was extended by raising either the observer or the bell." (p. 68.)

[3.] When the wind was light the sound beyond the distance of 20 yards, was much less audible at the ground than a few feet above it: and when inaudible in every direction at standing height, the sound could be distinctly recovered by mounting a tree. The same result was obtained by raising the alarm-bell upon a post four feet high; which while materially increasing the audible range of the sound—even in the direction of the slight wind, in all other directions doubled the range. This is explained by Professor Reynolds, by the continual waste and destruction of the sound waves which pass along the rough surface of the ground or grass, causing the waves immediately above to diverge continually downward, to be in like manner absorbed; the effect of which is to gradually weaken the sound more and more, as the waves proceed; so that even "when there is no wind, the distant sounds which pass above us are more intense than those we hear." (p. 68.)

[4.] Whatever therefore tends to gradually bend downward the sound rays will increase their sensible range. Professor Reynolds found by observations with the anemometer that the velocity of the wind increased from the ground upward; (pp. 63, 64) and hence it must give greater rapidity to the upper portion of the sound waves in the direction in which it is blowing and cause their impulses to continually tip downward. "This was observed to be the case on all occasions. In the direction of the wind when it was strong, the sound could be heard as well with the head on the ground as when raised, even when in a hollow with the bell hidden from view by the slope of the ground; and no advantage whatever was gained either by ascending to an elevation, or raising the bell." (p. 68.)

[5.] "Elevation was found to affect the range of sound against the wind in a much more marked manner than at right angles. Over the grass no sound could be heard with the head on the ground at 20 yards from the bell, and at 30 yards it was lost with the head three feet from the ground, and its full intensity was lost when standing erect at 30 yards. At 70 yards when standing erect the sound was lost at long intervals, and was only faintly heard even then; but it became continuous again when the ear was raised nine feet from the ground, and it reached its full intensity at an elevation of 12 feet." (p. 69.) The same results were obtained with snow on the ground, excepting that the sound was heard somewhat lower, being less dissipated or absorbed by the surface contact. At 160 yards the bell was inaudible—even at an elevation of 25 feet, and the sound was supposed to be hopelessly lost; but at a further elevation of 33 feet from the ground, it was again heard; while at five feet lower it was lost. At the proper elevation the sound appeared to be as well heard against the wind as with it, at the same distance. These last two observations very strikingly correspond with and confirm the observations of Henry [3], and [4].

[6.] "The least raising of the bell was followed by a considerable intensifying of the sound;" and while it could be heard only 70 yards when resting on the ground (i. e., one foot high), when set on a post five feet high, it could be heard 160 yards, or more than twice the distance,—the sound-beams evidently rising faster at or near the ground, than they do higher up. (p. 69.) "The intensity of the sound invariably seemed to waver, and as one approached the bell from the windward side, the sound did not intensify uniformly or gradually, but by fits or jerks." This is supposed to be the result of the more or less curved sound rays crossing each other at a small angle and producing an "interference." (p. 70.)

A subsequent experiment was made on the 14th of March, during a strong west wind, its velocity at an elevation of 12 feet being 37 feet per second, at eight feet, 33 feet per second, and at one foot from the ground (there being no snow on the grass) 17 feet per second. While the results as to varying range fully confirmed the previous experiments, the raising of the bell caused the sound to be heard even better against the wind than in the direction of the wind. (p. 71.) This curious circumstance is explained by Professor Reynolds as "due to the fact that the *variation* in the velocity of the air is much greater near the ground, than at a few feet above it;" and "when the bell is raised the rays of sound which proceed horizontally will be much less bent or turned up than those which go down to the ground; and consequently after proceeding some distance these rays will meet or cross, and if the head be at this point they will both fall on the ear together, causing a sound of double intensity. It is this crossing of the rays also which for the most part causes the interference" just mentioned. (p. 71.)

Professor Reynolds concludes that "these experiments establish three things with regard to the transmission of sound: 1. That when there is no wind, sound proceeding over a rough surface is more intense above than below. 2. That as long as the velocity of the wind is greater above than below, sound is lifted up to windward and is not destroyed. 3. That under the same circumstances it is brought down to leeward, and hence its range extended at the surface of the ground. These experiments also show that there is less variation in the velocity of the wind over a smooth surface than over a rough one. It seems to me that these facts fully confirm the hypothesis propounded by Professor Stokes; that they place the action of wind beyond question; and that they afford explanations of many of the anomalous cases that have been observed." (p. 71.)

[7.] In regard to the second part of the communication, treating of the effect of Temperature differences in refracting sound, Professor Reynolds shows that as "every degree of temperature between 32° and 70° adds approximately one foot per second to the velocity of sound," there must necessarily be an upward flexure of the rays, whenever by reason of any considerable increase of temperature in the lower strata of the air, the lower portion of the sound waves is projected in advance of the upper portion. (p. 71.) Atmospheric vapor also, though exercising but little direct influence on the velocity of sound, "nevertheless plays an important part in the phenomena under consideration; for it gives to the air a much greater power of radiating and absorbing heat, and thus renders it much more susceptible of changes in the action of the sun. . . . It is a well-known fact that the temperature of the air diminishes as we proceed upward, and that it also contains less vapor Hence it follows that, as a rule, the waves of sound must travel faster below than they do above, and thus be refracted or turned upward." (p. 72.)

The variation of temperature will be greatest in a quiet atmosphere when the sun is shining. The report of Mr. Glaisher "On eight Balloon Ascents in 1862" showed that "The decline of temperature [upward] near the earth with a partially clear sky is nearly double that with a cloudy sky."* "During the night the variations are less than during the day. This reasoning at once suggested an explanation of the well-known fact that sounds are less intense during the day than at night. This is a matter of common observation, and has been the subject of scientific enquiry." (p. 73.) The opinion must here be hazarded that this familiar phenomenon (as for example, the great distance at which water-falls can be heard at night,) has first received its true and satisfactory explanation from Professor Reynolds.

Assuming that for a few hundred feet upward, the diminution of temperature on a clear summer day is 1° for each hundred feet, a horizontal sound-ray would be bent up in an arc having a radius of about 20 miles. From a cliff 235 feet high, a sound should be audible from 1½ to 2 miles on the sea, and the ray should then begin to rise above the observer's head. This is shown to accord very closely with the observation of Tyndall [6]. Professor Reynolds after quoting the observation at length, remarks: "Here we see that the conditions which actually diminished the range of the sound were precisely those which would cause the greatest lifting of the waves. And it may be noticed that these facts were observed and recorded by Professor Tyndall with his mind altogether unbiased with any thought of establishing this hypothesis. He was looking for an explanation in quite another direction. Had it not been so he would probably have ascended the mast and thus found whether or not the sound was all the time passing

* Mr. Glaisher remarks: "From these results we may conclude that in a cloudy state of the sky, the decline of temperature is nearly uniform up to the clouds; that with a clear sky the greatest change is near the earth, being a decline of 1° in less than 100 feet, gradually decreasing as in the general law indicated in the preceding section, till it requires 300 feet at the height of 5,000 feet, for a change of 1° of temperature." (*Rep. Brit. Assoc.*, 1862, p. 462.)

over his head. On the worst day an ascent of 30 feet should have extended the range nearly one quarter of a mile." (*Phil. Mag.*, p. 76.)

V.

The instructive result, brought into veiw by the foregoing summaries, is that the differences noticed are essentially those of interpretation, and not to any important extent, of observation: an illustration if any were needed, of the high and rare order of imaginative insight requisite to the successful investigation of the more recondite operations of natural law. The differing actions of acoustic reflection and acoustic refraction suggested by the ingenious hypotheses of Humboldt and of Stokes, and espoused respectively by Tyndall and Henry, are probably both operative but their relative importance has yet to be established. It is certain, as already indicated, that some of the phenomena observed lie quite beyond the reach of the acoustic cloud hypothesis.

A particularly interesting case which is claimed with equal confidence for either theory, is the remarkable observation of General Duane, that at Portland, Maine, the steam whistle on Cape Elizabeth, nine miles distant, "can always be distinctly heard" with "the wind blowing a gale directly *toward* the whistle" or against the sound. (*L. H. Rep.*, p. 100.) At Portland Head, about midway between this fog-whistle and the point of observation is another signal,—a Daboll trumpet. While both these signals are better heard with an adverse wind ("a heavy northeast snow storm") than at other times, yet "as the wind increases in force, the sound of the nearer instrument—the trumpet—*diminishes*, but the whistle becomes *more distinct*." (*Rep.* p. 92.) The abnormal influence of the wind in reversing the order of these two signals is not the least surprising feature of the general phenomenon.

Professor Tyndall believes that this curious observation only "proves the snow-laden air from the northeast to be a highly homogeneous medium;" (*Sound*, *Preface*, p. xix,) the intervening air at other times being acoustically less transparent.

Professor Henry supposes "that during the continuance of the storm, while the wind was blowing from the northeast at the surface, there was a current of equal or greater intensity blowing in an opposite direction above, by which the sound was carried in direct opposition to the direction of the surface current;" (*Rep.*, p. 92 ;)—somewhat in the nature of a vertical cyclone. He adds: "The existence of such an upper current is in accordance with the hypothesis of the character of a northeast storm, which sometimes rages for several days at a given point on the coast without being felt more than a few miles in the interior, the air continuously flowing in below and going out above. Indeed in such cases a break in the lower clouds reveals the fact of the existence above of a rapid current in the opposite direction." (p. 92.)

Professor Henry's attention had been directed to this point as early as 1865, by discovering that a signal was audible against the wind at the mast-head of a vessel, after ceasing to be audible on deck: Obs. [4]. "This remarkable fact at first suggested the idea that sound was more readily conveyed by the upper current of air than the lower, and this appeared to be in accordance with the following statement of Captain Keeney, who is commander of one of the light-house vessels, and has been for a long time on the banks of Newfoundland in the occupation of fishing: 'When the fishermen in the morning hear the sound of the surf to the lee-ward, or from a point toward which the wind is blowing, they take this as an indefallible indication that in the course of from one to five hours the wind will change to the opposite direction from which it is blowing at the time.' The same statement was made to me by the intelligent keeper of the fog-signal at Block Island. In these cases it would appear that the wind had already changed direction above, and was thus transmitting the sound in an opposite direotion to that of the wind at the surface of the earth." (*Rep.*, p. 92.) The full significance of this idea however was not apprehended until the hypothesis of Professor Stokes (already alluded to) was taken up and considered. This appeared to furnish a satisfactory explanation of the observed effect of an upper current,—not on the actual range, but on the *direction* of the sound waves.

Professor Tyndall thus comments on the rival hypothesis of Professor Henry: "In the higher regions of the atmosphere he places an ideal wind, blowing in a direction opposed to the real one, which *always* accompanies the latter, and which more than neutralizes its action. In speculating thus he bases himself on the reasoning of Professor Stokes, according to which a sound-wave moving against the wind is tilted upward. The upper and opposing wind is invented for the purpose of tilting again the already lifted sound-wave downward." (*Pref.* to *Sound.*, pp. xix, xx.)

The word "invented" is scarcely the most appropriate term for an hypothesis derived from such patient research and careful induction. While in the case considered, the reversed upper wind of a local circulation is rendered so probable by the circumstances presented, it is proper to remark that this condition is not at all essential to the refraction doctrine. The hypothesis of Professor Stokes by no means assumes that "a sound-wave moving against the wind is tilted upward." (*Rep. Brit. Assoc.*, 1857, pp. 22, 23, of Abstracts.) An opposing wind exercises no sensible influence on either the velocity or the actual range of sound, nor (if *uniform*) on the direction of sound. Ordinarily indeed, a wind (which may be likened to an aerial river) is retarded at the earth precisely as the current of a stream is, over its bed.* When, however, the mouth of the aerial chimney of ascent is low, it may very well happen that the lower current of air (excepting immediately at the surface of the earth) is considerably swifter than the successive layers of the wind above it; and in such a case the effect of the opposing wind will be not to tilt upward the sound-beam, but to tilt it downward. In like manner a "favoring" wind, if more sluggish above, will tilt the sound-beam upward, and thus prove unfavorable to its aubibility. In short, the postulate required for acoustic refraction is simply that there shall be a *difference* of amount between the upper and lower currents of wind. And as this condition is certainly not an unusual one, we have here apparently a true and satisfactory account of the seeming anomalies of sound with reference to the influence of the wind.

* Professor Henry determined by experiment in 1865, when the velocity of the wind was not more than six miles per hour, that the speed of the clouds as indicated by their moving shadows, was several times this rate. (*L. H. Rep.*, p. 93.) And Professor Reynolds in 1874, by observations with the anemometer, ascertained that near the ground the retardation of the wind rapidly increased; so that the lower sound rays move more nearly in the arc of a parabola, than of a circle. (*Phil. Mag.*, pp. 64 and 70.)

But if the natural tendency of a mere diminution of velocity in the upper strata of an adverse wind is thus to bend an advancing sound downward, "a precisely similar effect" as Professor Henry has well remarked, "will be the result but perhaps in a considerably greater degree, in case an upper current is moving in an opposite direction to the lower, when the latter is adverse to the sound." (*Rep.*, p. 107.) In September, 1874, when a signal near Sandy Hook, N. J., was observed to be audible at a greater distance against the afternoon sea-breeze than with it, Professor Henry ascertained by the employment of toy balloons, that the upper current was opposed to the lower one, and in the direction of the maximum sound range: Obs. [11.] He was enabled thus to demonstrate experimentally the reality of the "ideal wind" which had been so confidently accepted before, from other conspiring intimations.

The critical commentary above cited, which postulates for this doctrine of acoustic refraction the super-position of "an ideal wind blowing in a direction opposite to the real one," as a condition "which more than *neutralizes* its action," quite fails to apprehend its true import, and seems to take in view merely a convective effect. No action analogous to "neutralization" is assumed by the doctrine. Generally speaking, there is no solution of continuity between opposing currents; but every gradation of movement in each successive intermediate stratum. And as it is wholly improbable that the sound-beam *which reaches the observer's ear*, ever passes high enough to approach the upper "ideal wind," nothing is neutralized. Obedient to the law of instantaneous resultants, the beam of acoustic impulse presses on ever at right agles to the wave-surface which is conditioned in form by compounded factors.

As wide of the mark is the supposition that the upper and opposing "ideal wind" is "for the purpose of tilting again the already lifted sound-wave, downward." As has just been contended, the one wind is as incapable of depressing the sound-wave, as the other is of lifting it.

The misconception culminates in the objection that "Professor Henry does not explain how the sound-wave *re-crosses* the hostile lower current, nor does he give any definite notion of the conditions under which it can be shown that it will reach the observer.' (*Loc. cit.*, p. xx.) There *is* no "hostile lower current," since as above pointed out, an opposite wind may be just as favorable to the propagation of sound, as a concurrent one.

To give, however, a more definite notion of the conditions under which it can be shown that the sound-wave will reach the observer without crossing currents, the accompanying diagrams are submitted.

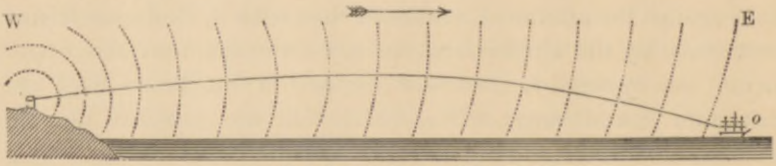

FIG. 1.—Favoring Wind.

Fig. 1 exhibits the more ordinary effect of a *favorable* wind in depressing the beam of sound: *s* being the signal station, and *o* the point of observation; the wind blowing from W to E. As the spheroidal wave-faces become more pressed forward above by the freer wind (assuming it to be retarded at the surface by friction), and as the direction of the acoustic beam is constantly normal to the successive aerial surfaces of impact, it follows that very minute differences of concentricity in the successive waves will, by constant accumulation gradually bend the line of dynamic effect downward, as shown in the sketch on a very exaggerated scale. Of the sound rays below the line represented, some will by reflection from the sea, reach the observer's ear and thus increase the sound.

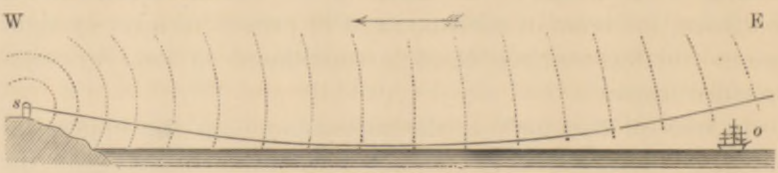

FIG. 2.—Adverse Wind.

Fig. 2 represents the ordinary effect of an *opposing* wind here blowing from E to W. The wave faces being more resisted above by the freer contrary winds (assuming as before a surface retardation), the sound-beams are curved upward, and the lowest ray that can reach the distance of the observer at *o*, is that which touching the surface of the sea is gradually so tilted upward that it passes above the ear of the listener, leaving him practically in an acoustic shadow; very much as an observer on the deck of a vessel when losing the sight of the hull of another vessel ten miles off, by reason of the interposed convexity of the ocean, stands in the *optical* shadow of the earth. In both cases if the conditions favor, the boundary of the shadow may be re-crossed by ascending from the deck to the mast-head, and the sight or the sound-beam thus regained.

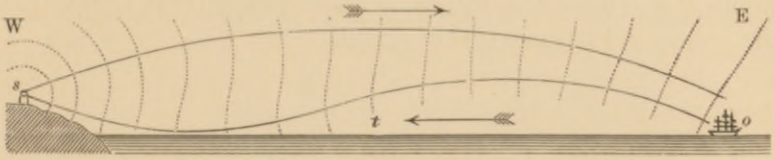

FIG. 3.—Compound Wind.

Fig. 3 represents the disturbing effect of a lower contrary wind with a favoring wind above. In this case the principal result will be a depression of the sound-beam as in fig. 1, but more strongly marked, as the differences of motion as we ascend will be more rapid. Attending this action, however, there will probably be some lagging of the lower stratum of the adverse wind by reason of the surface friction; the tendency of which will be to slightly distort the lower sound radiations, by giving them a reverse or serpentine curvature.

One result of this condition of the *locus* of the *normals* in the lower portion of the deformed wave-fronts, would be to make the sound less audible (or possibly sometimes inaudible) at a point (as at *t*) midway between the two stations. This hypothetical case of compound refraction would appear to offer a plausible explanation, not only of the paradox of a nearer trumpet-sound being diminished in power by the adverse wind which increased the effect of a more distant whistle, but also of the puzzling "belt" of inaudibility previously noticed. Duane [D], and Henry [8].

Numerous other cases might be represented by diagrams, as of a sound being hindered or tilted upward by a concurrent wind of unequal velocity, or downward by an opposing wind of similar character, and of the various permutations of differing currents in oblique directions; to which might be added various resultants of unequal motion producing *lateral* refraction, but this is unnecessary.

Enough has been said, it is hoped, to clear from popular misapprehension, the admirable hypothesis of Professor Stokes, raised by the equally admirable investigations of Professor Henry, to the rank of a "theory;" and to show that it has a real and demonstrated basis, or in other words that it is a *vera causa*. The question of its sufficiency lies entirely within the grasp of mathematical discussion; but a long series of accurate and comprehensive observations will yet be required to discover its full compass of practical result, and to determine its precise limit of capacity in subjugating the "abnormal phenomena" of sound.