DE MOTU SONI

by
Rev. William Derham
Philosophical Transactions, London 1708 – Vol. XXVI

Translated by
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Annotator's note: Unless specifically attributed to either Derham or Welling, all of the annotations and footnotes are mine, are enclosed in brackets, and are printed in blue. Relevant references have been added wherever possible. I have taken small liberties with punctuation and, rarely, changed words (for example, changing *interjacent to intervening*), to avoid excessively awkward constructions. Proper names have occasionally been changed to correspond to modern spelling to facilitate map and web searches for further information.

An abridged translation appeared as "Experiments and observations on the motion of sound," Phil.Trans. Royal Soc. Abridged 5, 380-395, 1809, by Hutton, Shaw, and Pearson. This abridgement is a rather dry condensation of the facts of Derham's paper. Welling attempts to preserve Derham's style and Welling's translation is complete. Welling's translation is handwritten and the original manuscript resides at the NOAA Central Library, Rare Books Division, in Silver Spring, Maryland. A high-quality scanned version is available on the web through the Central Library:

http://docs.lib.noaa.gov/rescue/Rarebook_treasures/QC222D91708.pdf

J. C. Welling was, at the time of this translation, the President of Columbian College (later named George Washington University) in Washington, DC. Welling also participated in several acoustics experiments performed by Joseph Henry as part of the US Lighthouse Board's study of the feasibility of fog horns.]

Experiments and Observations on the *Motion of Sound*, and Other Things Pertaining Thereto, Made by the Reverend Mr. W. Derham, Rector of Upminster Church and Fellow of the Royal Society of London.

1. Disagreement of the most celebrated authors about the progress of sound and the reason of my undertaking.

The most celebrated philosophers have thought it worth while to inquire into the delightful and mysterious reasoning about sound, and especially about its motion and rate of progress; and since the discrepancy among their observations is great, partly that I might remove my own doubts, partly that I might find amusement in my leisure hours I have endeavored, as far as I am able; to unfold and decide the whole matter.

And since my instruments are most suitable, and my opportunities for testing the matter were not to be despised, I suppose myself, in doing so, to be merely performing an appropriate duty, as to pay a debt to the philosophical word, especially to our most famous Royal Society which has deigned to admit me into its number.

The dissent among the most celebrated authors about the velocity of sound [See also, J. M. A. Lenihan, "The velocity of sound in air," Acustica 2(5), 205-212, 1952.] can be seen by a slight glance at the following table, in which is exhibited, (in English feet) the space they ascribe to the progress of sound in a single second of time:

Between the last and the next to the last of these estimates, the disagreement is not considerable, and the estimate of the French authorities is not much greater than these two. But among the rest the disagreement is great; and the reason of the discord manifestly is that it arises either from a defect in the instruments used or from the inadequate distance at which the observations were made, or from the effect of wind. [These observations regarding sources of error in measurement represent one of the most significant contributions of Derham's paper. Derham did not go so far as to assign specific uncertainties to measurements—few investigators of this time did—but his identification of sources of error and his conscious design of experiments to minimize these problems sets his work apart from his predecessors.]

A. The apparatus by which some of these distinguished men made their measurement was not automatic, but was a ball suspended by a cord, which vibrates seconds. [The expression, "vibrates seconds," means that the pendulum period for a full cycle of its motion is two seconds. In other words, the pendulum reached an extreme point in its swing every second—once to the right, then to the left. A pendulum that "vibrates half-seconds" would have a full-cycle period of one second.]

Now to all who are versed in such matters it is obvious that the ball is much less convenient than an automatic instrument, and is not so accurate; since it is necessary that the eye should first be engaged in observing the flash and then should glance towards the ball or pendulum – a process which consumes time and produces confusion. This fact then

Sound speed in feet per second from various investigators

Sir Isaac Newton[a]	968	Princ. Ph. Nat. Math. Lib. 2 prop. 50
Hon. Mr. Roberts[b]	1300	Philosophical Transactions, N. 209
Hon. Mr. Boyle[c]	1200	Essay of Languid Motion, p. 24
Mr. Walker[d]	1338	Philosophical Transactions, N. 247
Mersenne[e]	1474	Balistic Prop. 39
Messrs Flamsteed and Halley	1142	....
Distinguished citizens of Florence[f]	1148	Exp. per Acad. del Cimento, p. 141
... Frenchmen	1172	Du Hamel Hist. Acad. Royale

[a Newton, The Mathematical Principles of Natural Philosophy (Principia), Book II (Of the motion of bodies), Section VIII (Of motion propagated through fluids), Proposition 50. Derham would have had the first edition published in 1687. There were two subsequent editions (1713, 1726). The third edition gives two values for sound speed: 979 ft/s and 1142 ft/s. In the first edition and for the lower value in the third edition, Newton based his calculation on the static compressibility of air and its density; the higher value in the third edition was based on an imaginative and specious correction for the (then unknown) size of air molecules. The higher value was an attempt to reconcile theory with the overwhelming experimental evidence for a speed considerably higher than 968 ft/s. Another century passed before the calculation was done correctly (by Laplace).

b Francis Roberts, "Concerning the distance of the fixed stars," Phil. Trans. 17, 101–103, 1694.

c Robert Boyle, An Essay of the Great Effects of Even Languid and Unheeded Motion, London, 1685, p. 24.

d Walker, "Some experiments and observations concerning sounds," Phil. Trans. 20, 433-438, 1698. Well worth reading to illustrate the nature of the pendulum timing devices but also contains an excellent description of two variations of the echo method for determining sound speed. In the simpler echo method, the observer adjusts his position with respect to a flat wall until the echo from a clap (Walker used two small boards) returned just as the pendulum completed a swing. A refinement consisted of repeated clapping – clapping each time an echo was heard – and dividing the total time by the number of claps to get the single round-trip time. A further refinement of the repeated echo technique was developed later: rather than trying to react instantly to a received echo, if the claps are interspersed evenly with the echoes, the achieved timing precision is much greater. Perception of rhythm is much more accurate than instantaneous reaction to an event.

e M. Mersenne, Cogitata Physico-Mathematica, Paris, 1644, Ballistica, proposition XXXIX, p. 138.

f Essayes of Natural Experiments, Accademi del Cimento, 1634, translated by Richard Waller, FRS, 1667, pp. 140-142.]

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considered in connection with the slowness of our senses, and of our perception or attention, may produce a great error, as is well known to those who have made experiments concerning these matters. This is especially the case if

B. The interval between the sounding body and the observer may have been small. It is manifest, however, that the most of these distinguished men made their experiments at the distance of only a few feet, and made their measurements by the return, or echo of the sound. For some of these observers scarcely extended their measurement beyond six or seven hundred feet, and others not beyond a mile. But I have always observed that an ambiguity arises in a distance so small, though the best apparatus may be used. Because the slightest error in distances so very small is to be considered comparatively great for the pendulum, perhaps has passed through the half of its sweep or arc, from the last vibration when the sound may have been first emitted; but we count that beat as if it had been a full and complete vibration; or perhaps we may anticipate the vibration. And after the sound reaches us, we may perhaps count more or less than is right.

Or, if the distance shall have been long enough, still an error may arise from that very circumstance, if

C. Account is not taken of winds—about which more hereafter. These are the certain, inevitable and perpetual inconveniences which accompany the measurement of the progress of sounds – inconveniences which in small intervals, as I have said, especially if the instruments be bad, are able to produce great errors; and without doubt they have been the greatest cause of the disagreement among authors so eminent. But one can see how very near is the agreement among the distances assigned by the last three observers in our table—a fact which doubtless arises from the circumstance that they were furnished with good automatic instruments, in the use of which the ear is simply occupied with catching the vibrations of the pendulum, while the eye marks the flash or some other indication of the sound. These observations, besides, were made at long distances, in which a petty error will not be of great account. For the observations of the very celebrated Messrs. Flamsteed and Halley were made at a distance of almost three miles, (a few perches [The perch is a surveyor's rod 16 ½ feet in length.] more or less being excepted), from the Royal Observatory on Shooter's Hill, and the sound came to them in 13 ½ seconds. The gentlemen of Florence, who were also celebrated members of the Accademie del Cimento, made their experiments at about the same distances and some experiments at the distance of only a mile. And finally, the very celebrated Messrs Cassini, Picard, and Roemer made their experiments at a distance of 1280 French toises, which is more than one English mile and a half.

That the truth might be brought to light in the midst of the aforesaid disagreements, I have made very many exper-

iments at various distances, to wit, from one mile to twelve miles or more—And for the purpose of measuring the time, I have a very accurate portable instrument which is automatic and furnished with a pendulum vibrating half seconds.

That I might proceed the more safely I proposed to myself the following questions to be discussed:

1. How great is the space which sound traverses in a second or other interval of time.

2. Whether a gun fired towards an observer sends the sound in the same interval of time as when it is fired in the opposite direction.

3. Whether in every state of the atmosphere when the mercury is rising or falling in the barometer sounds traverse the same space in the same interval of time.

4. Whether sounds are propagated with greater velocity by day than by night.

5. Whether a favoring wind accelerates sound and an opposing wind retards it – that is, whether winds affect sound at all, and if so, in what manner.

6. Whether sound is propagated with a greater velocity in a tranquil atmosphere than when the wind is blowing.

7. Whether a strong wind blowing crosswise accelerates or retards the velocity of sound.

8. Whether sounds have the same velocity in summer and winter by day and by night.

9. Whether sounds have the same velocity in snowy and in clear weather.

10. Whether a great and a small sound have the same velocity.

11. Whether at all [angular] elevations, viz: horizontal, ten degrees, 25 degrees, and so on to 90 degrees, the sound of a gunshot strikes the ear of an observer at the same interval of time.

12. Whether sounds of all kinds, i.e., of guns, bells, hammers etc., have the same velocity.

13. Whether charges of powder varying in strength produce a variation in the velocity of sound.

14. Whether on the tops of high mountains and in valleys, that is, whether in the highest and lowest parts of the atmosphere, sounds travel the same space in the same interval of time.

15. Whether an upward and a downward sound have the same velocity, that is, whether it descends from the top of a mountain to its base at the same rate as it ascends from the base to the top.

16. Whether sound is propagated in the beginning with greater velocity and in the end with less, as occurs in many other violent motions.

17. Or whether, on the contrary, it is uniform—that is, whether or not it is propagated half the space in half the time, one fourth of the space in one fourth of the time, *etc.*

18. Whether in all regions northern or southern—in England, France, Italy, Germany, etc.—sounds have the same velocity. [This question seems particularly strange but Derham's discussion in Section 9 gives the background.]

19. Whether sound passes from place to place in a straight line, (i.e., by the shortest path), or according to the [irregular] superficies of the intervening land.

For the settlement of these questions I asked some kind friends of mine, (whose favors I here most gratefully acknowledge) to fire muskets from towers and other high places at a distance of one, two, three and even as far as eight miles, (which I have found to be the greatest distance at which I could hear the sound of a gun in these parts, covered as they are with woods, etc.). These musket shots were of great use to me. But what especially answered my purpose were the cannon (Sakers [Welling's note: The saker is a species of small ordnance.]) which are used at Blackheath in training the raw recruits who are to serve the artillery of our most illustrious queen. I could see the flash and hear the report of these cannons from the tower of my church; sometimes also I made use of a telescope. And hence I have devoted myself with all care and diligence to the observations of these cannons since February, 1704.

After a few observations made in the midst of their discharges, I took measures for making a certain particular experiment through the courtesy of the late Baron Granville then the Governor of London Tower and of other eminent men who on that Tower serve the interests of the royal artillery, (and whose favors I here most gratefully recognize). Two cannon (Sakers) were placed side by side one with its mouth towards me the other with its mouth reversed. These two cannon on the 13th February 1704 were fired every half hour from 6 o'clock P.M. to midnight, while a gentle breeze was blowing directly against the sound. The interval between the flash of each gun (which flash I could see with the naked eye) and the arrival of the sound was always about 120 or 122 half seconds – I have said 120 or 122, since the sound came to me duplicated – that is the first sound came within 120 half seconds (which was a weaker sound) and the second within 122 half seconds (which was a stronger sound) and in the same manner, through the whole time of the observation, the crash of each cannon came in a duplicated form.

This reduplication of sound seems to me an echo, reverberated, as I think, from the watermill at Blackheath, or from the houses situated in its vicinity. [London Tower is just slightly south of west from Derham's church at Upminster; Blackheath is west southwest from Upminster and about 3/4 as far as London. The azimuth angle to Blackheath is more southerly than to London but not by more than 30 degrees.] I have no reason for doubting about this point except the contrary opinion of a learned friend and sagacious philosopher who believes that there is no echo to be heard save that produced by objects reflecting sound near the observer, instead of its being produced by those near the sounding body—or other distant objects.

The next disquisition, therefore, will be,

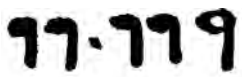
2. Concerning sounds reverberated from a distance, or the distant echo.

Perhaps this disquisition will be considered a digression but since it pertains to the subject of sound I hope that a few observations on this subject will not be ungrateful to ingenious minds.

And in the first place I believe that this [the fact that the

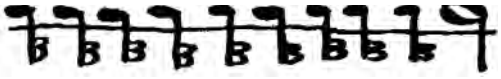
echo came from afar] is not contrary to the laws of the echo. In the second place it is to be remarked that this double sound came directly from Blackheath, for, of the two sounds, the former did not come from that quarter, and then a second sound, (after the manner of an echo), came from somewhere else, *i.e.*, either beyond me, to the right of me, to the left of me or from any different direction – I have often observed the same thing when big guns were fired from ships in the river Thames, (especially if the air was clear and still), at the evening and in the morning where the watch-guns are discharged. After the crash of the gun had struck my ear I have heard it running far away along the river, and booming through many miles from the bank, from the mountains, and from the rocks thickly studded along the Kent county coast. "These all", says my friend, "come from the reverberation of the houses, etc., near you." But to say nothing respecting the weakness of a sound after it has traversed very many miles, and respecting its incapacity if it has come so far, to be rebounded by sound reflecting objects near the observer, rather than by sound reflecting objects situated near the sounding body – to say nothing about these things, I will give one or two examples from which it will plainly appear that an echo made by sound-reflecting objects near the sounding body can be heard through many miles as well as the primary sound—sometimes even clearer than the latter.

I have often observed that the heavy guns fired from ships in the Thames at evening about Deptford and Cuckold's Point, send forth for the most part a crash that is duplicated, triplicated, quadruplicated, or even still more multiplied, and that the later crashes are more sonorous. And when I have gone here and there a hundred and twenty five paces, or even a quarter of a mile, or half a mile in a transverse direction, the sound was still the same. I remember that on the 8th of last March many cannon were fired, somewhere between the aforesaid Deptford and Cuckold's Point, from a ship in the Thames, which I saw from my church. The crash of these was repeated five or six times, in this manner:



Between the flash and the sound I counted 122 half seconds – the wind blowing crosswise. At that time, therefore, the guns were distant from me more than 13 miles. The first two reports were much weaker than the third reports, but the last reports were much the most sonorous of all. And when I had gone across a quarter of a mile towards the right, there was still the same multiplied sound, and when towards the left, it was still the same. And, moreover, in some of my halting places, besides the multiple sound, I clearly heard a weak echo reverberated from my church or the adjacent houses. Indeed, I then made the same observations, as often as the guns were fired.

Another observation of this kind was made on a certain Sunday, about two or three years ago, from the sound of a heavy cannon fired somewhere on the river Thames, this side or beyond the town of Gravesend. The crash of this gun was multiplied at least 8, 9, or 10 times, according to this score:



Very many persons who at that time were on their way to Divine Worship supposed that this multiplied sound was the crash of many guns from a ship engaged in battle but, as I think, it was nothing else than a many-voiced echo from the sound of a single gun as it was fired, or from the sound of another and another reverberated by the many adjacent ships or by the shore. What makes for my opinion is the fact that I was not the only one who heard it (I was walking at the time in my garden)—but also many other persons who were far distant. Mr. Barret likewise an ingenious and learned member of our Royal Society, heard the same repeated sound at his house at a distance of about four miles from Upminster, where I heard it.

From all these considerations it clearly appears that the opinion of my aforesaid friend (so worthy to be respected on many accounts) is false.

3. Concerning Echo or the Reverberation of Sounds in the Air.

To these remarks on echoes I hope it will not be unacceptable to add an illustration from the reverberation of sound by aerial particles, which will confirm what has been said.

When I have heard the crashes of heavy artillery, especially in a still and clear atmosphere, I have often observed that a *murmur* high in the air precedes the report. [Similar observations were made in the second half of the nineteenth century by Joseph Henry. Henry provided much observational evidence to support the idea that winds bent the path of sound (refraction) through that atmosphere—an idea that was foreign to any of the philosophers of Derham's era. But Henry also observed what appeared to be echoes from the air above and these were a source of great curiosity to Henry and others to follow.] And in thin fog I have often heard the sound of cannon murmuring in the air high above my head through many miles, so that this murmur has lasted 15 seconds. This continuous murmur, in my opinion, comes from particles of vapor suspended in the atmosphere which resist the course of the sound waves and reverberate them back to the ears of the observer, after the manner of undefined echoes, which we call a *murmur* in the air. [This explanation presages John Tyndall's explanations for many of the curious phenomena associated with sound traveling through the atmosphere. However, in many cases, Tyndall, a critic of the idea that wind and temperature gradients could bend paths of sound, was subsequently shown to be wrong.]

Where these facts are fully weighed it will be manifest that echoes made at a distance are capable of being heard, and that that aforesaid reduplication of the crash of artillery on Blackheath came without doubt from Blackheath itself, as I have just asserted.

4. Concerning the Sounds of gunshots fired in every angular direction, etc.

To return from this digression concerning the reverberation of sounds, I will proceed to my observations concerning their velocity—observations which I have derived from

my many experiments. And what I have just now suggested respecting the sound of the artillery on Blackheath, I have found in all other cases, viz. that the motion of sound is neither swifter nor slower whether the cannon be fired towards the observer or in the opposite direction.

Likewise, in all positions of a musket, whether horizontal or perpendicular, and at all elevations, ten degrees, twenty degrees, etc., there is no variation in the sound of its discharge. So true in this matter is the observation of those famous gentlemen connected with the Accademi del Cimento in Florence.

The force also of the powder, whether it be strong or weak, and the greater or less the quantity of the charge, though serving to increase or diminish the sound, have no effect in accelerating or retarding its motion.

5. Concerning the velocity of sound in any state of the atmosphere and any time of the year.

Kircher [Athanasius Kircher (1602-1680), a German Jesuit scholar] says, that he always found the velocity of sound to be different at different times, at morning, at midday, at evening and at night. But I, relying on a better chronometer and using a more suitable distance, never have found that the celerity of sound is different at these times, but in all weather, whether the atmosphere be clear and serene, or cloudy and turbid; whether snow is falling or fog, (which both powerfully blunt the audibility of sound) [This generalization is, unfortunately, one of Derham's remarks that influenced thought for about 150 years. Subsequent descriptions by Derham in Section 10 indicate that he was really not unequivocal in this opinion and he may have succumbed to repeating popular lore here.]; whether it thunders or it lightnings, whether heat or cold dries the air; whether it be day or night, summer or winter; whether the mercury is rising or falling in the barometer—in a word I may say that in all changes of atmosphere whatsoever (winds only being excepted) the velocity of sound is neither greater nor less. [The one important factor that Derham missed was the dependence of sound speed on temperature. Given the chronometers available to Derham at the time and the fact that the effects of wind are normally considerably larger than the effects of temperature, it is not at all surprising that Derham did not see the change in sound speed with temperature. It would be almost half a century before Bianconi provided convincing experimental evidence of the temperature dependence of sound speed.] The sound is only more or less clear from this variation of the medium, and perhaps this fact deceives the sagacious Kircher.

Hence it follows that the conclusions drawn by Mr. Walker from his ingenious observations and from those of Dr. Plot (Derham note: Philos. Trans. N. 247.) and of Kircher were erroneous. [The more complete reference is "Some Experiments and Observations concerning Sounds," By Mr. Walker, Late of Brazen-Nose-College, Oxon. Philosophical Transactions (1683-1775), Vol. 20. (1698), pp. 433-438.]

6. Concerning the velocity of a strong and of a weak sound, and of the sound of different sounding bodies.

Though Kircher thinks otherwise I do not doubt that the sounds of all bodies—of muskets, bells, hammers, etc., have the same velocity. In the year 1704, I compared the beatings of a hammer and the crack of a musket at the interval of a mile (the greatest distance at which I could hear the sound of a hammer), and I found that the sound of both reached me in the same time, as also that they traversed 3/4, 1/2 and 1/4 of the same space in 3/4, 1/2 and 1/4 of the same time.

As regards strong and weak sounds I do not doubt that they traverse the same space in the same interval of time. This fact will be in a measure apparent from the following experiments:

January 13, 1704: The master gunner of Tilbury Fort at my request fired two gun shots in succession, and a heavy cannon in which he had well measured a charge of powder. The report of all these reached me at the distance of about three miles in the same time.

The master gunner of England [Translator's note: "The Master Gunner of England" was the title of the Senior Master Gunner in the ordinance service of England at this period.] also on the 11th Sept. 1705, after sunset, as a matter of favor to me fired on Blackheath some muskets, some heavy cannon, (Sakers) and some mortars. I could not hear the muskets on account of the great distance, or because the air was not sufficiently serene. But I heard the sounds of the cannon and of the mortars in the same interval of time, though the crash of the mortar was much more torpid and weak than that of the cannon.

Notwithstanding the fact that I used the greatest care in

Place where the guns were fired	Number of vibrations of pendulum	Distance of Places		Direction of the wind
		by trigonometry miles	by sound miles	
Hornchurch	9	0.9875		crosswise
N. Okendon Ch.	18 ½	2.004	2.0	crosswise
Upminster Mill	22 ½	2.4	2.4	favoring
	23		2.48	snow, crosswise
Lower Warley Ch.	27 ½	3.0	2.97	strongly favoring
Rainham Church	33 ¼	3.58	3.59	crosswise
Alvely Mill	33	3.58	3.57	crosswise
Dagenham Church	35	3.85	3.78	favoring
South Weal Ch.	45	4.59	4.86	crosswise
East Thorndon Ch.	46 ½	5.09	5.03	rather favoring
Barking Church	70 ¼	7.7	7.62	favoring
Blackheath	116	12.5	12.55	crosswise

these experiments, I nevertheless wished afterwards to try them over again at greater distances, but the opportunity was wanting. I leave this matter therefore to be better tested by others.

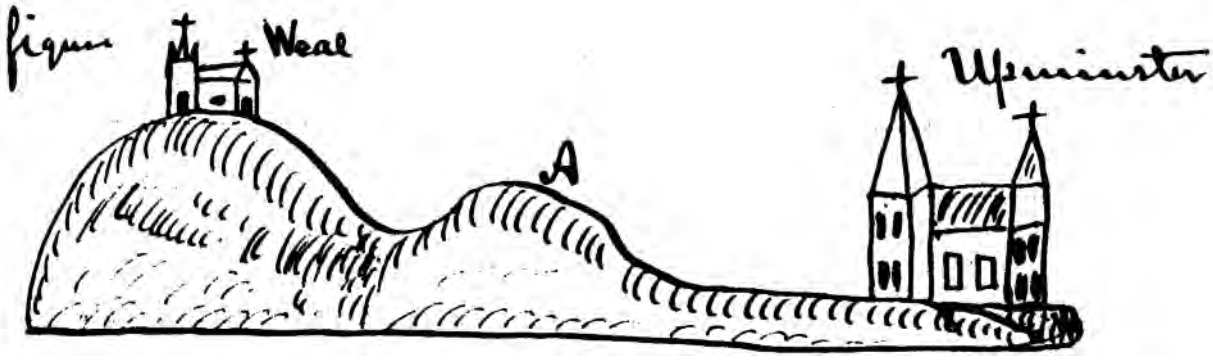
7. Concerning the uniformity of the velocity of sound.

The next observation was concerning the uniformity of the velocity of sound. I have found this to be the same as the illustrious Accademia del Cimento has already defined. That is, sounds traverse half the space in half the interval of time; one fourth the space in one fourth the interval of time and so on. This fact will be plain from the examples in the following table:

The distances of the places marked in the table from Upminster (where I made my observations) I measured with as much accuracy as I could either by the surveyor's rod or by trigonometrical art. [There are two small errors in this table. The actual distance from the Lower Church to the Church at Upminster is 3.10 miles, not 3.0 miles and the distance from the South Weal Church is 4.64 miles.] And from the great consonance between the distances measured in this way and by the velocity of sound, the excellence of my instruments, the truthfulness of my calculations and observations is set in a clear light. For the difference between the distances ascertained by measurement, and taken from sound, either disappears entirely, or is that of only a very few hundredths of parts, unless where there may have been a favoring wind (the case of south Weal Church being excepted, concerning which hereafter). So, too, in the observations made from the churches at Dagenham, Warley, Thorndon, and Barking, the distances marked by sound seemed a little shorter because the wind accelerated the sound. But in working up this column of distances by sound, I have allowed nothing on account of the acceleration of the winds, but have simply divided the number of the vibrations, or half seconds, by 9 1/4 or 9.25 (the number of half seconds in which sound traverses a mile.)

The equible rate of the motion of sound is also manifest from this table, as will appear from a comparison of the vibrations and the distances, or from the column of the distances alone as derives from sound.

But that nothing might be wanting in confirmation of these facts I made a journey to Foulness Sands on our Essex Coast. These Sands, washed and covered by the daily tide of the sea, make a great and smooth plain for many miles. Upon this plain I measured off only six miles, because neither the tide nor my leisure permitted that I should measure a greater distance. At the end of almost each mile I made experiments by the firing of muskets, not without great peril to my life from the influence of the sea and the darkness of the night. From these experiments I found that all my former observa-



tions were most exact and true, to wit, that sound traverses one mile in 9 1/4 half seconds; two miles in 18 1/2 seconds, three miles in 27 3/4 half seconds, and so on.

8. Concerning the upward and downward motion of sounds, or concerning the ascent and descent of sounds. Likewise whether they pass from place to place in a straight line or according to the superficies of the intervening land.

As regards questions 15 and 19, I frankly confess that I have never satisfied myself on these points by any of the experiments which I have hitherto made. In the first place let us treat the progress of sound by the shortest path, under the head of question 19. The reason for doubt about this was the discrepancy between the distance from Weal to Upminster by trigonometrical measurement and by sound, as is exhibited in the preceding table. The trigonometrical measurement was taken in so many ways and with such good angles, that I can have no doubt about it. But since the distance as measured by the motion of sound seems to be greater, and since the superficies of the intervening ground takes on a form like that exhibited in this figure,

I have in consequence, somewhat doubted whether sound may or may not move a little crookedly, that is, whether or not that intervening elevation at "A", by the resistance which it offers to the mass of sound beats them back and retards them.

That I might in some way solve this problem I took care that experiments should be made with the sound of a musket from the top of Langdon hill to the valley below at a distance of 3.79 miles. The distance was well measured trigonometrically, from angles and from a base line sufficiently large; and the experiment was made while a gentle breeze was slightly opposing the sound. Between the flash and the report I counted 35 1/2 half seconds. This number squares so exactly with the distance, and so nearly agrees with other experiments that it cannot be doubted that the sound descended from the top into the valley by a straight line (through the air), and not according to the curved superficies of the intervening ground.

I believe therefore that there was some error in the aforesaid observations at Weal since neither in the last experiment at Langdon, nor in any others, have I observed anything like it.

As regards the upward and downward motion of sound, that is, whether sounds are borne with equal tenor and at the same rate from the top of a mountain to the bottom, I scarcely hope that I shall ever satisfy myself or any body else. For neither in Essex nor in the conterminous parts are any hills found high enough from which one may make sufficient experiments to this end. In fact the highest of all which it has yet been my lot to see, (such as those which they call the Langdon hills), do not much exceed 300 ft. For I measured the highest of these hills both by trigonometry and by a portable barometer, and I found it to be by the former mode 363 ft high by the latter mode, … [ellipsis in Latin manuscript.]

On a former summer, however, when I was making a journey in the western parts of the kingdom, I determined to try an experiment on a certain hill whose altitude I ascertained by measurement a few years previously to be (unless my memory deceives me) about 1875 feet. At a time when the wind was blowing across the path of the sound, but so gently that it would not extinguish a lighted candle, I ordered some muskets to be discharged at the base and at the top of the mountains, and I perceived that the sound reached me from each in almost the same interval of time. If I observed any slight discrepancy at all it seemed to consist in this that the sound may have ascended somewhat more swiftly towards the mountain than it descended from the mountain. But to speak according to the fact, I was scarcely able to measure the time with the accuracy which is due, since unluckily, it had turned out that the chronometer which I used had been somewhat disturbed by a concussion received on the journey.

Hence I leave this experiment to be tried more successfully and certainly by others. And I would that the votaries of higher culture and philosophy among the Italians, (in whom has been implanted a curious felicity of genius) might be willing to try this same experiment on the Alps.

9. Concerning the translation or motion of sound in Italy.

Inasmuch as I have made mention of the Italians, it seems not irrelevant to recite certain observations and experiments made in Italy on my account by that very acute, learned and accomplished friend of mine Dr. Newton, the envoy of her Britannic Majesty at Florence. [This Dr. Newton is Henry Newton, not Isaac Newton. Both Newtons were Fellows of the Royal Society.] The occasion of these was as follows:

The late ingenious and excellent Richard Towneley, Esq., (a name familiar and grateful to our renowned society) had signified to me by a letter written in the year 1704, that "Sounds are rarely heard as far at Rome as in England and in our northern regions." He said particularly that while he was staying at Rome, on an occasion when some cannon of the castle of St. Angelo were fired on account of joyous intelligence, and when he was standing on Mount Trinita, he has observed that the sound was much more languid in that situation than in any other location at the same distance. And after the death of Towneley [The name does not appear in the Latin manuscript. Welling inserts Newton's name here by mistake. The reference is to the late Richard Towneley. Henry Newton is still alive when Derham writes this paper (see the next paragraph)], his brother reported to me, in writing, that in the year 1688, "when on leaving Rome, he repaired, for a season of recreation to Castel Gandolfo (a certain higher location near Lake Albano, about 13 Italian miles from Rome), he had observed that the sound of the heavy cannon booming from the aforesaid Castle of St. Angelo seemed to him reduced in volume and weak. Also at another time when he was passing around the walls of this same castle in a carriage, and when the great guns were bellowing from it, they seemed to send forth a sound which, as there observed, was neither in quality nor volume like that observed elsewhere."

Since these things had been noticed by two men of no common intellect, and since the phenomenon itself seemed entirely new and unusual, the desire entered my mind of enquiring what might be the cause of it. I therefore wrote to

the very distinguished Dr. Newton, whose praises I have already celebrated, and he in the month of October, 1706 was good enough to report to me what he and also what his friends have observed respecting this matter.

He recites that on a journey from Bologna towards Florence he heard at the city of San Michele in Bosco, near Bologna the sound of the firing of Cannon. These cannon were forty miles off, being fired at Mirandula, which a French army then held in siege. And on the following night he heard the same sound while he was resting over night in the Apennines, (twenty miles further off.)

But the observations and experiments which the same very eminent man through his politeness and benevolence caused to be made by others, justly claim for themselves a particular specification as well as my best thanks for these honors which his excellency has conferred upon me. When he received my letter at Florence he disclosed what I wanted to a certain nobleman who was at the same time an eminent philosopher; and he in turn communicated these wishes of mine to the Grand Duke. "The Grand Duke, as he says, in accordance with his singular love of the arts and of learned pursuits, as also in accordance with that gracious indulgence towards the votaries which he has received by inheritance from his ancestors along with his scepter, immediately gave orders that experiments should be made for the sake of fully satisfying me on this point; and he set Joseph Averrani a renowned philosopher of Pisa, and a man skilled in every one of the more liberal arts, over the inspection and direction of these experiments. The memoirs of this most honorable gentleman our most honorable envoy has deigned to write out at length for me. But the gist of the matter amounts to this: After having premised with equal caution and ingenuity very many things which might produce a great difference in the progress of sounds he at length proceeds as follows:

"In the lower fortress of Florence a culverin [a long-barreled cannon] was fired frequently between the first and the third hour of the night. Certain men at the same time were kept at Leghorn and were ordered diligently to observe whether they could hear its report. Of these men, some who had been stationed at the Lanterna and at Mazzoco heard no sound; (perhaps because the roar of the sea obscured the sound); but others who were standing on the bastions of the old fortress (which they call Donjon) and those who had been sent to Mount Rotondo (which is about five miles from Leghorn in the direction of Mount Nero) caught up the sound in their ears. And as often as the gun was fired its report was clearly heard in the same places. Now the distance of this Fortress of Florence from Mount Rotondo in a straight line is reckoned at not less than fifty five miles. And it is worthy of note that the intervening country was studded with many hills, which must needs have somewhat obstructed the path of the sound. To these considerations should be added that on the same evening a west wind was gently blowing which, (since Leghorn is situated to the south southwest with respect to Florence) may justly be supposed to have slightly impeded a freer expansion of the sound. In order however that an open place and one lying level in all directions might be obtained, that tract of the sea was selected which lies between Leghorn and Port Ferraio, the distance of which, according to the calculation of the most skillful sailors, is set down, at sixty miles. The report, however, of artillery not infrequently reaches from Leghorn to this Port, and the places in its vicinage. Nor is there need of favoring winds to promote this passage of sound, in order that it may be surely heard. Indeed any wind whatsoever, whether it be favorable or adverse, is equally an impediment, and renders the sound less audible; it may be because the roar of the sea, agitated by this cause, is more a disadvantage than the current of the air blowing in the same direction is an advantage.

"Hence it is that the sound is heard only when the wind is entirely still or is only murmuring very gently—when the air is serene and the sea tranquil. Nor then, indeed, is it heard indiscriminately from all points, but only from those which are a little more elevated, such as the two bulwarks which are called Stella and Falcona and the place called Mulini. Moreover it is required that the observer should be as attentive as possible, and should not be hindered and troubled by the voice or clamor of persons making a din around him. But then equally by day and by night he may hear the sound provided the atmosphere be clear and still—the only difference being that the sound seems somewhat stronger and clearer in the night time, when no noises occur such as are often wont to disturb the ears by day.

"Moreover, it has been reported to us by most credible witnesses that many years ago when an insurrection was raging at Messina and the city was closely besieged, the sound of the guns startled the ears of the inhabitants of Augusta and of Syracuse [about 80 miles over water].

"Likewise when the French were shaking Genoa with heavy siege guns it is certain that the sound of the cannon reached as far as Mount Nero which overhangs Leghorn [about 90 miles over water].

"From these observations I am inclined to believe that there is no difference in this regard between Italy and the northern regions.

"But as regards the other question—whether a wind blowing directly or adversely accelerates or retards sound—it is not in my power as yet to give a certain reply. For the experiments which I have instituted and by which I had hoped that the truth would be explored, do not suffice for settling the question. In fact during the summer time (when for the most part the winds by day blow from the sea and from the west, while when evening comes on they are generally still), the most suitable occasions for frequently and certainly testing the matter were wanting to me. I hope, however, that towards the close of the year, after weather of another kind shall have supervened, I may obtain more favorable opportunities for instituting and testing experiments of this kind with greater success and frequency, as also with greater accuracy. But for the present it must suffice to report what happened to me on the 10th of August last past, when I was permitted to try the following experiments:

"A culverin of a certain kind [*culverina quaedani*] was placed on the curtain of the lower fortress of Florence, and was so planted there that its mouth pointed towards Artemino, which is the country palace of the Grand Duke of

Tuscany, [Welling renders the Latin, Hetruria, as Esterinæ; Hutton, et al., translate as Tuscany] situated on a rather high hill, and opposite to the west side of the aforesaid fortress, from which also it is distant about 12 miles. I selected a particular day when the west wind was blowing rather briskly, so that the velocity of sound might be resisted by a contrary wind. But this helped the matter only a little, for towards evening the air was entirely still, or at least was agitated with such a slight breeze as would not have put out the flame of a candle. Having left at this spot certain observers skilled in these inquires to whom I had previously given in charge what they should most particularly attend to, I proceeded to the aforesaid palace of Artermino, where the very Honorable Envoy had preferred to be stationed. According to my orders the culverin was frequently fired between the first and third hour of the night, and I plainly counted 49 seconds between its flash and its report. We also fired some cannons at Artermino, and the aforesaid observers, whom I had left in the fortress, counted between the flash and the report of these only 48 seconds. Hence it appeared that the sound was borne from Artermino to Florence only a second more rapidly than it was borne contrariwise.

"I do not so fully confide in this observation of mine that I would venture to refer this very small difference of velocity to the force of a favorable or of an opposing wind. In fact a mistake of the observer himself, as he counted the vibrations of the pendulum, might perchance have given occasion to it. This at least might readily occur. For it must needs often happen that he should not see the flash until after the vibration of the pendulum has begun, and that he should hear the report of the sound before the vibration has been completed; so that in this way he may make his calculation too large by one vibration, whilst in point of fact the interval of time is precisely the same both ways.

"I was hoping however that on the next morning a contrary wind would perchance arise, (for often at this point, at least with the first dawn of day break, a wind is accustomed to blow from the east) which would better serve for the experiments I had begun. I had ordered therefore the culverin to be fired again as soon as day should have dawned, but the wind was propitious neither to my wishes nor my undertaking. In fact it had shifted only a very little towards the north quarter, in so much that the variation of the time and of the velocity of the sound could scarcely be perceived with such a slight change of the wind. I counted, therefore, the usual 49 vibrations of the pendulum, as before. In the mean time I hope thoroughly to try these same experiments as soon as a more suitable weather shall occur, and as more frequent changes of the winds shall afford more convenient opportunities for trying them to better advantage, until at length I shall satisfy myself perfectly.

"As regards the space which sounds traverse in any given time my informants are not yet agreed among themselves, but from certain experiments they conjectured that the matter is as the experiments of the Accademia del Cimento signified." [This is the end of Averrani's contribution.]

So much from this acute and skillful man, whose praises I need not repeat. From his observations together with those which the very honorable and distinguished Envoy [Dr. Henry Newton, that is] has communicated to me it is abun-

dantly clear that sounds can be heard much further in Italy than my before named ingenious friend informed me. For the Excellent Envoy himself has heard the report of heavy guns at the distance of sixty miles. The guns likewise which at his request were fired at Florence and were heard 55 miles. The cannon fired at Leghorn were audible at a distance of 60 miles. Those which were discharged at Messina, as appears from the geographical tables surprised the ears of men who were distant nearly 100 Italian miles. The report of those which were fired in the siege of Genoa traversed (as appears from the maps) more than 90 Italian miles [one Italian mile = 1.0277 English miles].

When all these facts are recalled to the mind and seriously weighed, I can scarcely avoid the belief that sounds are propagated no less widely in all quarters of the south than in the northern parts of the world. Although the examples of a greater progress of sound are not wanting in certain northern quarters of the globe, a Danish nobleman, a servant of our illustrious Prince of Denmark, has told me, in a conversation, that while he was living in Denmark he had clearly heard the report of cannon fired at Carlscroon, a distance, unless my memory deceives me, of 80 English miles. Likewise that very skilful man Dr. Hearn, the physician of the most illustrious King of Sweden has communicated a special memoir to our Royal Society respecting the guns fired at Stockholm when the obsequies [a ceremony to honor the recently departed] of one of the Royal Princes were celebrated in the year 1685, the report of which traversed an interval of 30 Swedish miles, which are equal to about 180 English miles. At that naval battle likewise which was fought between England and Holland in 1672, the sound of the cannon struck the astonished ears of men through an intervening space of more than 200 miles, reaching as it did, across our island even as far as Salisbury and Wales.

What therefore both the Towneley brothers observed is wholly special and peculiar to the aforesaid Castle of St. Angelo or at least to Rome. For it is not permitted to suspect either the perspicacity of their intellects or their conscientious carefulness.

The dimunition of sound which they observed (unless I surmise amiss) must be ascribed either to the situation of this aforesaid castle, or to the intervening houses (rising everywhere and on all sides in that very crowded city) or to the din of the city resounding from all quarters, or to adverse winds, or, in fine, some other like cause, which I leave to be attained by happier conjecture on the part of those whose lot it is to live there, or perhaps these men made their aforesaid observations in that state of the atmosphere in which sounds, although they have the most favorable winds, are nevertheless much more languid than at other times when the winds are entirely adverse. And at one time I had persuaded myself that an atmospheric condition of this kind always obtains at Rome, and not in other parts of Italy, until I fell upon the contrary opinion of Kircher, who says: "Here at Rome, wonderful to relate, when the north wind blows, echo or sound acquires the greatest vigor; when the south wind blows it is weak; when the southeast and east winds blow, it is of a medium character."

But this condition of the air, since it affects sounds so greatly, will not be foreign from our purpose to consider in detail. It is my purpose, therefore, in the next place to treat it more fully, and to set forth the observations which I have made in the premises.

10. Concerning the various weakening and intensity (or audibility) of sound according to the different state of the atmosphere.

I have often observed in summer time, when the air has grown hot, that sounds appeared more languid than usual and were exceedingly weak in their impression on the ear; while in weather of another sort, especially in winter, if it happens to be freezing cold, the same sounds were much more piercing and shrill, and struck the ear more forcibly. Also, when the north or south east wind was blowing, however adversely, I have observed the sounds to be clearer and shriller than if the wind was blowing from contrary quarters, as Kircher also observed at Rome. But this is not uniformly and always the case.

Nor could I form any more certain conclusions from the inspection of a rising or falling barometer as I had too confidently expected to do. For I discovered that sounds were sometimes very clear and shrill, sometimes very faint and languid when the mercury was rising to the top; and on the contrary, sometimes very strong, sometimes very weak, when the mercury was sinking to the bottom.

A like uncertainty obtains with regard to clear and foggy air. In rainy and damp weather I have often observed that sounds are blunted and that after torrential rains they acquire the greatest strength, as Kircher observed at Rome. But the contrary also often happens. For instance, on May 31, 1705, the air on this occasion was more clear and free of vapor than I remember ever before to have seen it. For such was the purity and liquid serenity of the atmosphere that I could clearly and readily perceive exceedingly remote objects. But nevertheless, I was not able to hear the cannon that were fired at that time on the grounds of Blackheath (if I except a single one whose thud reduced to a faint sound, I may have caught in my ears), although I could clearly perceive the flash of them all in the distance. And at the same time the current of the clouds and of the wind was setting in the same direction with the sound. Moreover, the breeze which was then blowing was a very gently one, such as could scarcely ruffle the hair; and, in fine, all things necessary to promote the force and velocity of sound seemed to concur. But on the other hand, when the state of the air and weather has been wholly changed—when everything seemed turbid and the atmosphere full of vapor—I have often heard strong sounds, and not less often have I heard them blunt and weak.

The causes of these variations I leave to be inquired by others, since I confess that it equally exceeds the grasp of my mind to discover them and to assign what may be the proper medium or vehicle of sound – whether the ethereal and more subtle part of the atmosphere, or the vaporous and denser part of the atmosphere, or both combined.

But as regards thick fogs, it is certain that they are dampers of sound in the highest degree. [This statement is

unfortunate. It is certainly not true but it was used through the eighteenth century and the first half of the nineteenth century to support the argument that acoustic fog warning signals would be useless. The degree of certainty in Derham's words is surprising particularly in contrast to the uncertainty throughout this section with regard to other effects. When Derham includes detailed observations, he does not shrink from pointing out inconsistencies. In this section alone, he uses the phrases, "…but this is not uniformly and always the case…", "…but the contrary often happens…" In contrast, he gives no observational evidence regarding propagation in thick fogs, just the unequivocal statement. One wonders whether he is simply repeating "what everyone knows."] For sounds then seem to be for the most part very weak and blunted—a fact which very certainly proceeds from the interposed vapors and thick particles which compose fog. I have likewise observed the same concerning snowy weather. [Some authors assert that Derham believes that falling snow damps sound but the next paragraph makes it clear that Derham is talking about the effects of snow on the ground. Freshly fallen snow is a poor reflector of sound.]

For when fresh snow has fallen on the ground sounds straightway grow dull; but when its surface has been covered with ice, the sounds suddenly become more acute, and I then have heard bells ringing and cannon booming just the same as if there was no snow on the ground. My friend Towneley was telling me not very long ago that he had observed (the like of which I have myself experienced) that when he was riding on horseback ringing not far from him was barely able to reach his ears whenever a house covered with snow lay between him and the sound, so that he on entering the little town, was very much surprised that the bells should so suddenly be stilled while he was passing along the first houses that intervened, and that they should suddenly sound again when he was passing along the next vacant space. Indeed during the whole of his course in this town he observed that the sound of bells reached his ears or not according as building covered with snow were intervening or not.

But concerning these things more than enough has been said. We proceed to other matters of greater moment.

11. Concerning the force of winds or their influence on the velocity of sound.

The illustrious Accademi del Cimento at Florence found from experiments that the velocity of sound was neither retarded by adverse winds nor accelerated by favorable ones, but that, however the winds might blow, sounds always traversed the same space in the same time. Gassendi was of this opinion, and almost all the rest who have philosophised before or since.

Since, however, the contrary of this is plain from mere experience, these authorities must be corrected of error, into which they seem to have fallen for this reason, that their experiments were tried within a too short space. For it is very probable that these philosophers made their observations at a distance of only one or, at the most, of two or three miles. Hence I do not wonder that their observations are faulty; but if they had tested the matter, as I have often done, at ten or twelve thousand paces, using accurate instruments, they would have easily recognized their error.

This common error, I myself, relying on the authority of these men, admitted for a long time, until at length, by more than three years of observation of cannons on the Blackheath grounds, I luckily detected it. When, however, at first I perceived the sounds to come to my ears sometimes quicker, sometimes slower, the suspicions entered my mind that I had committed some error, either because I had less accurately counted the vibrations of the clock, or had badly observed the flash of the cannon, or from want of attention, had fallen into some other such like error. But after the cannons were continuously fired, at my request, every half hour, from six o'clock in the evening till midnight, and after I constantly perceived that the sound reached me, without any perceivable variation, in the space of a hundred and twenty or of a hundred and twenty-two half seconds, however much wind may have been directly adverse; while at other times, when the wind was blowing favorably, either directly or crosswise or obliquely, I found that the sound of the same cannon reached me in the space of 111, 112, 113, 114, 115, 116 or, at the most, of 117 half seconds, then at last I became thoroughly persuaded that there was a certain real difference that produced this variety in the observations.

Nor is it only true that favorable or adverse winds accelerate or retard the velocity of sounds but it is also true that, in accordance with the variety of the degrees with which they blow more strongly or more gently, so much the more or less do they promote or impede this velocity. For greater certain-

Table of the sounds of cannons on the grounds of Blackheath according to the variation of the winds and the force with which they were blowing.

Year, month, and day	Time of Day	# of ½ second vibrations	Direction of the wind[a] and rate of velocity[b]	Direction of the clouds	Height of the barometer	Remarks
1704[c]						
Feb. 11	11 ½ AM	119	E, 2	E	30.22	
Feb. 13	From 6 PM 'til midnight	120 122	NE by E,1	NE by E	29.99	
1705						
Mar. 30	10 AM	119	SW, 7	SW	29.30	
Apr. 2	8 ½ AM	114 ½	S by W, 1			
Apr. 3	10 AM	116 ½	S, 4	lower S, upper W by N	29.80	
Apr. 5	1 PM	111	SW by W, 7	SW by W	29.20	
Apr. 13	8 ½ AM	120	N by E, 2		29.26	
Apr. 24	5 PM	116	SW by W, 0	NW	29.59	
Sept. 11	6 ½ PM	115	W, 2	W by N		saker
	7 PM	115 ½	W by N, 2			mortar
Sept. 29	10 ½ AM	112	SSW, 4	SSW	29.38	
Oct. 6	10 AM	117	ESE, 1&2	SE	29.34	
Nov. 30	12 m	115	SSW, 4	SSW	29.10	
Feb. 15	11 AM	116	S by W, 1	SW	29.60	
1706						
Nov. 29	11 ½ AM	116	SW, 0	SW by W	30.06	
	12 m	118	SW by S, 1			
Feb. 7	12 m	113	SW by W, 4	W	29.83	
Welling's note: [N.B. The distance of the guns from the observer seems to have been about 12 miles. 1142 x 58 sec = 66236 ft ÷ 5280 = 12 ½ miles.]						

[a SW by W would indicate a wind directly from Blackheath to Derham's observation point.
b The wind velocity is indicated by the number given after the direction. See text, Sections 11 and 12.
c Customary usage of the year included January and February following. For example, Feb. 13 is listed above under the heading, 1704. The next entry is Mar. 30 under the heading, 1705, but these two observations are most likely only a few weeks apart rather than a full year apart (and both would be in 1705 by modern accounting).]

ty respecting all these things I will subjoin in the following table certain special observations, which were made after I had before noted that the cannon on the Blackheath grounds ranges from the sound, that is, that they inclined to the quarter of the compass a little beyond S.W. by W.

I have selected these observations from very many others, all of them being cautiously made and each one repeated two or three times or oftener, so that what I have said above respecting their truth is abundantly and indubitably manifest. For, from the experiments made on April 5 and Sept. 29 it is plain that the stronger winds push forward and hasten the velocity of sounds. For on the fifth of April, when the motion of the wind and of the sound was nearly coincident, and when the same wind was a little stronger (as is denoted by the number 7 annexed, just as the number 0 denotes tranquil weather, and as the numbers 1, 2, 3, 4, signify different powers of the wind) then, I say, the sound finished its passage in the space of 111 half seconds. But on the 24th, when the wind was blowing from the same quarter, and the air was still, the sound made the same passage in an interval of 116 half seconds. So likewise on the 7th of February, 1706 when the wind was blowing from the same quarter of the compass and was carrying the sound with it, but now with only half the strength, 113 half seconds elapsed before the sound made its

usual passage. So, in fine, on Sept. 29, 1705, when a stronger and less favorable wind was blowing, the sound completed its progress within 112 half seconds. From which examples, and from others in the table, it plainly appears that stronger winds assist the propagation of sound, but that lighter winds are less effective in promoting its propagation.

The same likewise is plain respecting those winds or currents of air which directly favor or obstruct the progress of sound—that they make its velocity quicker or slower—and where intermediate currents (streams) of air are blown, that they produce in like manner an intermediate progress of sound, as measured by the vibrations of the pendulum.

The greatest difference which I have yet observed in the passage of sound through a space of about 13 miles amounts to about nine or ten half seconds, that is, when strong winds are aiding and only gentle ones are obstructing the sound.

But when only gentle winds, or almost none at all, are either obstructing or aiding the sound, then the difference does not exceed two or three half seconds.

After, in this way, I had perceived what influence the winds have, both for accelerating and retarding the course of sounds, curiosity led me to inquire into the velocity of the winds themselves. And though the inquiry may be foreign to my subject, it will not be wholly ungrateful, as I hope, to curious minds, if I publish in this connection certain observations on this point.

12. Concerning the velocity of winds.

In order to ascertain how large a space winds may traverse in any given time, I have used, in prosecuting my experiments, certain bodies of the somewhat lighter sort, such as thistle down, light feathers, etc., which seemed better to serve my purpose than the instrument which is described for us in the Philosophical Transactions, No. 24; or even that other more available one, recalling the figure of a mill with wings attached, invented, unless I mistake, by our most acute friend, the late Dr. Hook. [Derham often references the Philosophical Transactions by number rather than by volume. The reference here is to Moray and Hook, "Directions for observations and experiments to be made by Masters of ships, Pilots, and other fit persons in their sea-voyages," Phil. Trans. Royal Soc. London, 2, 433-448, 1667.]

From very many experiments which I have made, with the aid of the lighter sort of bodies, when the winds were blowing with different degrees of force, I have found that the most violent wind traverses scarcely 60 miles an hour. For example, on the 11th of August, 1705, the violence of the wind excited such a tempest that it almost overturned the windmill itself near the spot where I made my observations. [The different degrees of the force of the winds, as has just been seen, I have for the most part noted by these numbers: 0, 1, 2, 3, 4, 5, 6, up to 10, 15, or still higher degrees.] Now I have estimated that the force of the above indicated wind answers to about 12 or 14 on this scale. And from very many reiterated experiments I have concluded that that violent wind traversed about 33 feet in a half second, or 45 miles in an hour; hence I gather that the fleetest and most tempestuous winds (that violent wind which raged in the month of November, 1703, not being excepted) do not traverse more than 50 or 60 miles an hour.

After we have measured the velocity of the rapid winds, it is not difficult to conjecture what may be the velocity of less rapid ones; for I have also marked the course of these, and from various experiments I have convinced myself that some of them accomplished 15, some 13, others many less miles per hour; while some are propagated with such a slow motion that they move scarcely a single mile an hour. Moreover, other winds are so sluggish that one may easily outstrip them while making a journey on horseback or on foot. This fact is apparent to our senses, for when we arrest our steps we perceive a soft breeze gently fanning us, but if we advance with it we feel none at all; while if we quicken our pace instead of a breeze accompanying us and blowing in the same direction with our movement, we plainly feel the air resisting us, and blowing full in our faces. Likewise when the atmosphere is entirely quiescent and stagnant, if we chance to be walking or riding on horseback, we then perceive a gentle breeze pressing against us, with such degrees of force, in fact, as correspond to the rates of our own motion. And a breeze of wind or current of air is borne with the same rate of motion or velocity when it presses against us with an equal impetus as we stand still, or linger in our track.

From these observations about the velocity of winds very many things, not without utility, might be noted, but especially might we assign in view of them, one reason why the mercury rises and falls for such a long time before clear weather or rain sets in.

But I will omit these considerations as being foreign to my purpose, and this only will I observe as to sounds, to wit, that while their motion is accelerated by wind it is plain that those parts of the atmosphere by which sounds are impressed or propagated are not the same as those from which winds are blown, but certain other more ethereal and volatile parts, as one may suppose. For the fleetest winds do not pass through more than 60 miles in an hour, but sounds travel more than seven hundred thousand paces, or 778 miles in the same time.

But if it be objected that winds do accelerate or retard sounds it is to be answered that this does not only proceed from the current or tendency of the windy particles alone, but rather from the conjoint and cooperating motion of all the particles of the atmosphere, both the thicker and the ethereal. If the direction of this course of motion favors the waves of sound it is altogether in accordance with probability that the impulse of sounds should be accelerated by this cause, but if the direction is adverse that, the impulse should be retarded.

13. Concerning the velocity of sounds.

After having in this way set forth the operation and effects of wind on the progress of sound and having spoken thus generally respecting the velocity of sounds, it remains at last that I should report the more special observations which I have made on this point. From what has been said, therefore, and from many other things which we have noted before I conclude most decisively that sounds are propagated with

the following degree of velocity, to wit, that they traverse the space of a mile or 5280 English feet in 9 ¼ half seconds. Or which amounts to the same thing, 571 ft in ½ second and 1142 ft in a whole second.

This, however, is the defined space traversed by sounds, if a current of the atmosphere blows across their path, and is their mean progress or velocity.

But if the wind increases the rapidity of the sound, it is possible that it may traverse more than 600 ft in the space of a ½ second; or on the other hand if the wind retard its motion it may not proceed more than 560 ft in the same interval of time.

So, at length, I have brought to an end this memoir of mine, in which I have summarily embraced the principal observations I have made about the progress of sound, and certain other things pertaining thereto.

Practiced and ingenious men will not have much difficulty in applying this exposition to very many uses which are not to be despised. But especially would the aforesaid observations and experiments seem to pertain not a little to

1. The Philosopher, who, even because of them, will be in some respects better equipped for the investigation of the secret nature of sounds and for explaining their very numerous abstruse phenomena;

2. To the Sailor, who hence may learn how far off are the ships which he sees floating in the distance, or lying at anchor; how remote likewise may be the desired land or beach which he sees in the distance—facts which, from shots designedly fired on a given signal, may be easily and certainly known;

3. To the Soldier for the purpose of finding how far an enemy has placed his camp; at what distance an arsenal, a fort, or a besieged city is situated, for the purpose of planting against them siege cannon, and aiming mortars and bomb shells;

4. To the Geographer, for more readily and certainly measuring the distances of places, because any body who is furnished with a small quantity of powder can, in this way, within an hour or two exhibit almost the whole of any region with a table most accurately outlined seeing that gunshots, as I have said, serve to mark distances by their firing, and any mathematical instrument by which angles are measured, either that common instrument which surveyors use, called the Plain Table or a single rule, furnished with graduated scales, will indicate the situations of the various places, which afterwards can be easily delineated. [The Plane Table or Plain Table was normally used in surveying as follows: two or more level tables would be set up at surveyed locations. A sighting device—an alidade—was used on each table to draw direction lines to objects sighted from both locations. From the sighting lines from both tables, and the locations of the tables, a map could be constructed locating any object sighted.] In this way, too one can readily inquire into the correctness and truth of maps, and if they have any errors he can correct them.

In fine, this method of observing sound, would be of great use in measuring distances of inaccessible places, especially of very wide rivers and places of that kind not otherwise easy to be measured. For a specimen of this work, I resolved with the aid of friends to compare the distances of certain among the more celebrated bays and straits, especially of the Strait of Gades, between Tangier and Gibraltar, and the British Channel between Dover in England and Calais in France, where the breadth of the channel according to the measurement of ingenious Frenchmen, is 22.07 English miles. But the lamentable season of war through which we are now passing has interposed an obstacle to these undertakings and to others having for their object the promotion of learning;

5. For the measurement of echoes. Although many learned men have anxiously inquired both anciently and in subsequent times concerning this amusing and pleasant phenomenon of sound, still there is not a good degree of harmony among them respecting many things which relate to it, especially respecting the extent of space necessary for the repetition of one, two, three or more, syllables, or, what amounts to the same thing, respecting the space traversed by an echo in a certain interval of time. Mersenne allows… [ellipsis in Latin manuscript] yards for the repetition of a monosyllabic sound. Blancanus allows 24 yards, to which our very celebrated countryman Dr. Plot gives his assent, but Athanasius Kircher asserts that nothing at all can be defined with certainty respecting it, because, the variation of the winds, the intensification and the relaxation in the force of sound and many other things produce an immense variation.

It is not difficult, however, to offer an explanation of this disagreement among these distinguished men, for it can arise from many causes—certainly from the slowness and from the different disposition of our senses, or from the various audibility of sounds; from the grave or acute sound of the syllables themselves, or from their protracted or prolonged pronunciation, or from any other cause which may protract the interval of time. I can have no doubt for instance, that if any sound reflecting object, should be able to reverberate all the syllables of this verse

vocalis nymphe quae nec reticere loquenti

[Welling's note pertaining to this line of Latin and the next: Quoted from Ovid's Met., iii, lines 357 and 359.]

it would not be able to reverberate all the syllables of the following verse, since its pronunciation is much more prolonged:

corpus adhuc echo, non vox erat, et tamen resume

And still less would it be able to repeat all the harsh and prolonged syllables of the following verse:

Arx tridues rostris sphinx, praester, torrida, seps, strix.

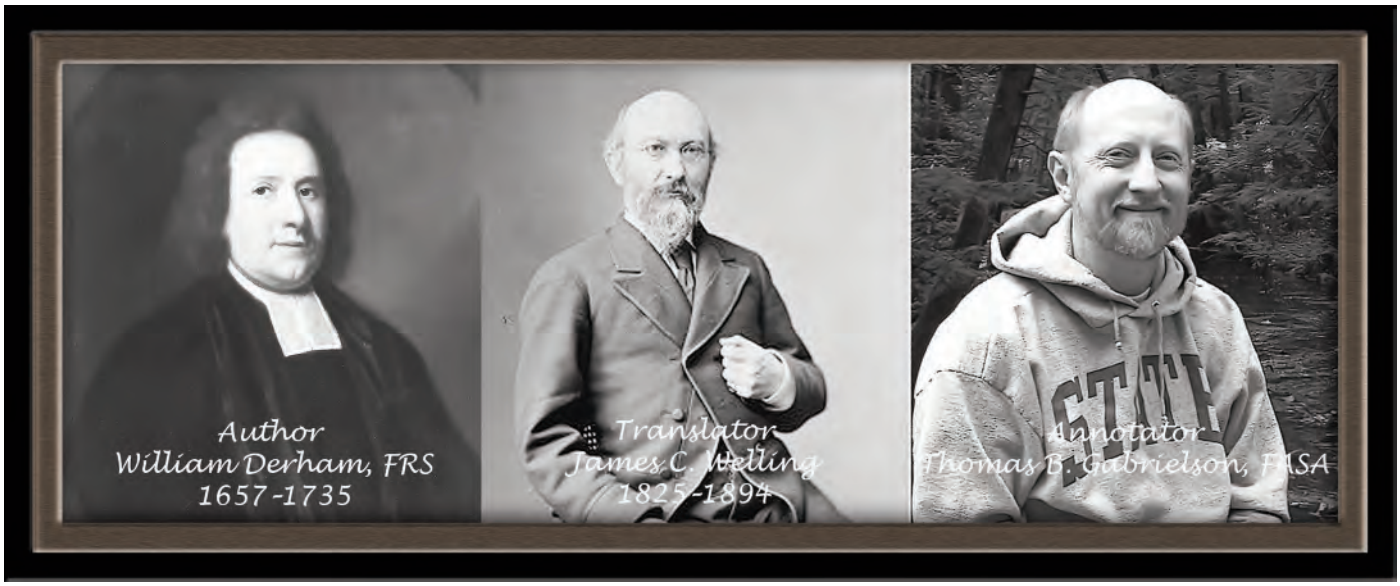
But from the foregoing observations concerning the velocity of sound it may be concluded that echoes, like sounds, traverse certain and determinate spaces in a certain definite time. What I have myself frequently learned from

experience is this: that an echo returns in double the interval of time in which the primary sound reached the sound-reflecting object. For example, if the sound-reflecting object was distant 600 ft., the return of the echo would take place within the same interval of time in which the primary sound would have traversed 1200 ft. if it had not been reverberated.

And this fact has often been of great use to me in measuring the distance of places. For example, when I was standing on the bank of the river Thames opposite the town of Woolwich, the echo of a monosyllabic sound has been reverberated from the opposite houses in six half seconds from which I infer that the width of the River Thames at the at point, from the margin of one bank to the margin of the other is 1712 English feet, or over a quarter of a mile. For, as 9.25 half-seconds are to 5280 feet (a mile), so are six half-seconds to 3423.8 feet – the half of which is 1711.9 feet.

Finally, in this way the height of thunderclouds and the distance of the thunder and lightning itself may be easily ascertained. [The idea of determining the distance to a thunderstorm by counting seconds from the lightning flash to the report of thunder is not original with Derham. This idea was expressed by members of the Accademie del Cimento in Florence prior to Derham's paper. See the English translation by Waller of these writings (reference given in the first Table above).]

Finis.



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