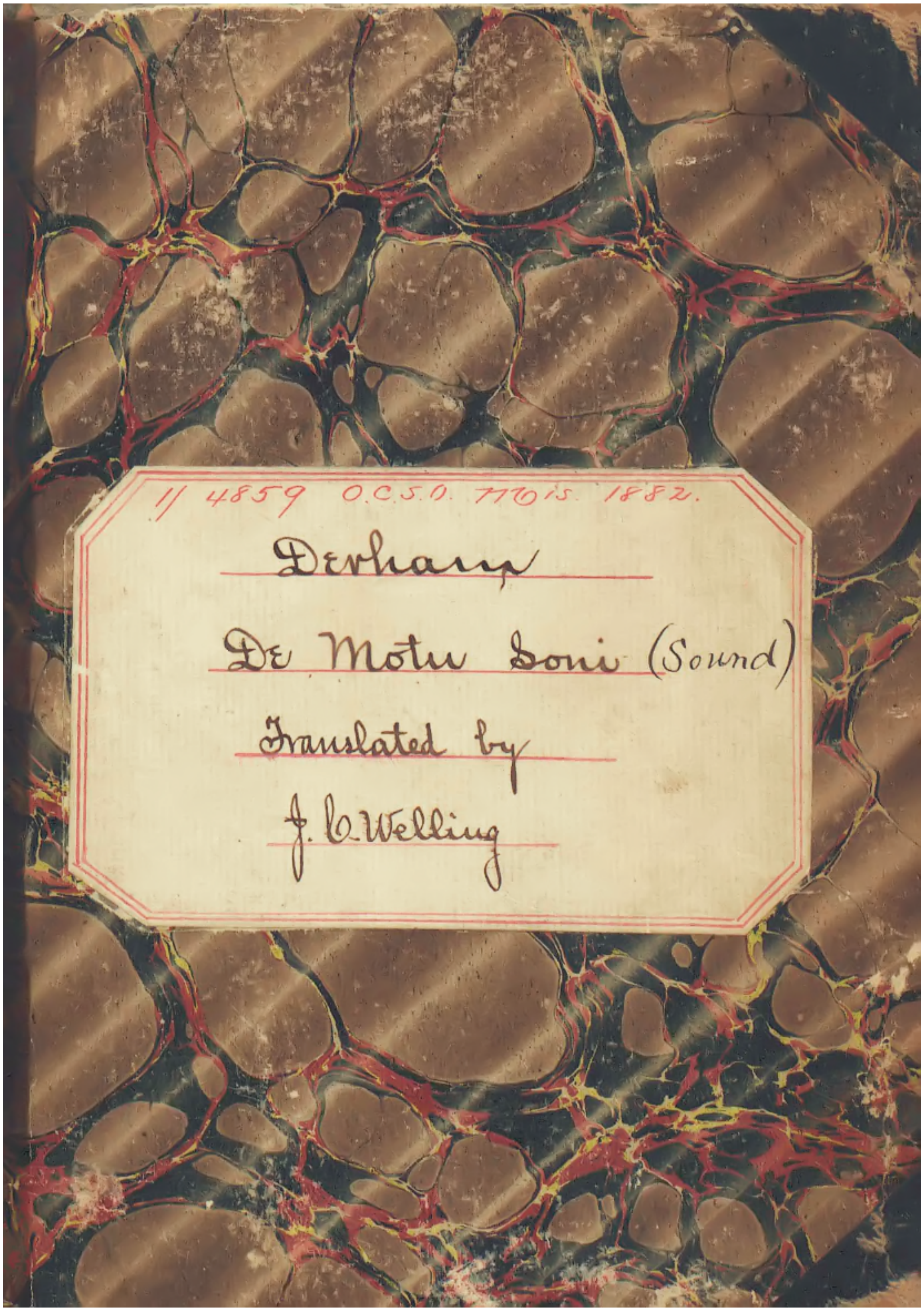

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BACKGROUND AND PERSPECTIVE WILLIAM DERHAM'S *DE MOTU SONI* (ON THE MOTION OF SOUND)

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Author's note: To make this article consistent with the following article and easier to read, I have enclosed footnotes and comments in square brackets, printed them in blue, and placed them in the article where their reference numbers would have appeared.

Introduction

In 1706, the Rector of St. Laurence's Church in Upminster, William Derham, traveled to the eastern coast of England on a mission. [W. Derham, "De Motu Soni," Philosophical Transactions, Vol. 26, (1708 - 1709), pp. 2-35.]

> "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 time of the sea, make a great and smooth plain for many miles. Upon this plain I measured off only six miles, because neither the time 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 night. From these experiments I found that all my former observations were most exact and true, to wit, that sound traverses one mile in 9 ¼ half seconds…"

"Derham's extensive collection of sound speed measurements forced Newton's hand; Newton's prediction was well below the range of Derham's measurements."

Measuring the speed of sound was a hobby for Derham. Though infrequently referenced in modern literature, Derham's measurements over a period of several years overshadowed previous efforts in quantity, quality, and supporting weather observations. He not only confirmed the inaccuracy of Newton's prediction for the speed of sound in air; his measurements also exposed the role of wind in the apparent speed of sound.

Nearly 170 years later, the American scientist Joseph Henry organized a series measurements along another coast, the southern coast of Block Island in Long Island Sound: [J. Henry, Scientific Writings of Joseph Henry, Report of the US Light-house Board for 1875]

> "…on the 19th of August [1875] …General Woodruff and Dr. Welling, starting from the bottom of the cliff below the light-house, went along the beach, one [to the east]…and the other [to the west]. General Woodruff found that the sound of the siren was distinctly heard all the way to the breakwater…Dr. Welling…entirely lost the sound within a quarter of a mile…the wind was in the direction traversed by General Woodruff and contrary to that of Dr. Welling…"

Although it was not obvious at the time, Henry's experiments, inspired by the work of Stokes and Reynolds, were part of a revolution in understanding the role of wind in the propagation of sound. The strange behavior of sound had evoked recurring comment for at least two hundred years. At times, the smoke and flame of nearby gunfire could be seen clearly but the gun's report could not heard; at other times, cannon discharging far from sight could be heard distinctly. Some of these stories inspired Rev. Derham—a Fellow of the Royal Society of London— to conduct several years' worth of experiments; some of the same stories led Henry and Dr. James Welling to the beaches of Block Island.

Joseph Henry is well known, though mostly for his contributions to the study of magnetic fields (the unit of electrical inductance is the henry). In contrast, neither Welling nor Derham appear with any frequency in the literature of acoustics. Welling and Derham were both amateur physicists. Welling was the President of Columbian University—later renamed George Washington University— and an anthropologist by vocation; Derham was Rector of the Upminster parish of the Church of England. Both published on acoustics, Derham in the Philosophical Transactions of the Royal Society, Welling in the Bulletin of the Washington Philosophical Society.

Welling's interest in Henry's experiments on sound led him to Derham's paper; however, Derham's paper was published in Latin. Rather than relying on fragments of translated passages in other papers, Welling translated the entire paper into English. Welling presented his handwritten translation to the U.S. Weather Service in 1883 and the manuscript survives in the Rare Books Collection at the NOAA Central Library in Silver Spring, Maryland. [A high-quality scanned version of the handwritten manuscript is available on line. See http://docs.lib.noaa.gov/rescue/Rarebook_treasures/QC222D91708.pdf]

In the decades preceding Henry's experiments, there were two competing theories regarding the surprising behavior of sound in the atmosphere. Alexander von Humboldt, during his 1799-1800 expedition to the Orinoco River basin, observed that the sounds of cascades on the river were louder and clearer at night even though the tropical forest was noisier at night. Humboldt reasoned that small, turbid irregularities in the atmosphere caused by solar heating of the ground during the day might scatter and attenuate sounds over relatively short distances. The British acoustician, John Tyndall, embraced Humboldt's explanation, named the turbid irregularities *flocculence*, and interpreted many observations as consequences of this flocculence.

In contrast, George Stokes had reasoned in 1857 that the normal increase in wind speed with height above the ground would bend or refract sound waves causing them to be lifted above the ears of observers upwind of the source and bent back down to observers downwind. Two decades later, Osborne Reynolds provided convincing proof of Stokes hypothesis over short ranges. At that time, Joseph Henry was head of the US Lighthouse Board and charged with evaluation of acoustic fog signals to supplement coastal lighthouses. Henry believed that refraction, not flocculence, produced the wide variation in audibility of these acoustic signals.

The experiments of Derham and Henry bookend a collection of adventures in understanding the transmission of sound through the air. Intervening events are chronicled elsewhere [For example, T. Gabrielson, "Refraction of sound in the atmosphere," Acoustics Today, 2006]; here we consider the contributions of the largely forgotten Rector of Upminster.

In 1708, Derham published what was, at the time, the seminal paper on the propagation of sound in the atmosphere. His decision to write this paper in Latin may have resulted in wider readership across the Continent in the 1700's, but, by the 1800's, researchers relied largely on a small collection of translated excerpts in contemporary publications. An abbreviated translation was published in the Abridged Transactions of the Royal Society in 1809 but it was not until James Welling took an interest in Henry's experiments that the complete paper was translated into English.

Derham's paper is important for several reasons. First, he addressed the question of the speed of sound in far greater depth than anyone else had. In 1697, Isaac Newton—a more senior member of the same Royal Society to which Derham belonged—published the first edition of Principia, his treatise on physics. In this work, Newton predicted what the speed of sound should be based on the density and static compressibility of air. Contemporary measurements of the speed of sound had sufficient variation that Newton found values both above and below his prediction; consequently, Newton was unaware how far his prediction was in error.

By 1708, however, more careful measurements suggested that Newton's value was about 20 percent low. By far, the largest set of these measurements was assembled by Derham. Derham and others had reduced the measurement uncertainty sufficiently far that Newton's prediction was no longer tenable. In subsequent editions of *Principia*, Newton wove several creative and ultimately unsupportable arguments as to why his values, if properly corrected, did in fact agree with measurement. [See R. S. Westfall, "Newton and the fudge factor," Science **179**, 751-758, 1973. For example, Westfall writes "…[Newton's] use of the "crassitude" of the air particles to

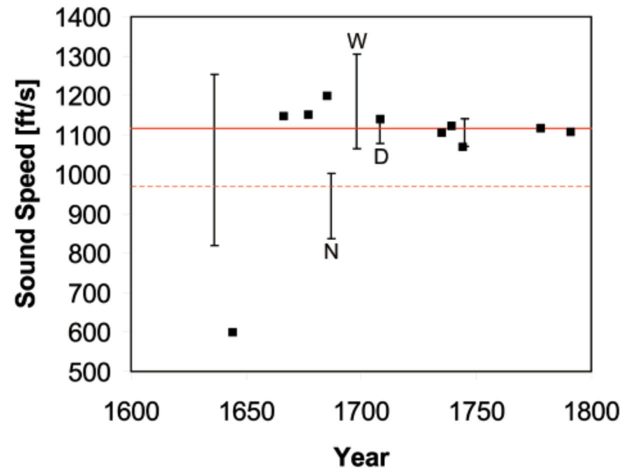

Fig. 1. Early measurements of sound speed. Insofar as is possible, the measurements are referred to 15°C and ranges are shown where there is sufficient information to present them. Three of the ranges shown are: (N) Newton's, (W) Walker's, and (D) Derham's measurements. The solid red line is at the value for the sound speed at 15°C; the dashed red line is Newton's prediction for sound speed. The spread in published values dropped dramatically with Derham's 1708 paper. (Values from Lenihan (1952), Walker (1698), and Derham (1708).)

raise the calculated velocity by more than 10 percent was nothing short of deliberate fraud."]

Newton should have quit while he was ahead. His calculation was flawed but neither he nor anyone else of that era understood the consequences of the miniscule temperature changes experienced by air under normal acoustic compression and expansion. Another century and a half would pass before the consequences of coupled temperature and pressure oscillations during the passage of an acoustic wave were understood clearly. [See B. S. Finn, "Laplace and the speed of sound," Isis **55**(1), 7-19, 1964.]

Derham's extensive collection of sound speed measurements forced Newton's hand; Newton's prediction was well below the range of Derham's measurements. (See Fig. 1.)

However Derham's work was much more than a simple table of sound speed measurements. He investigated the effects of atmospheric conditions on the propagation of sound— both as regards speed and intensity. He established definitively that sound propagation was faster with the wind and slower against the wind.

Naturally, Derham's work was also naïve: he dutifully recorded barometric pressure that has little effect on sound speed or propagation, and did not record temperature that does influence sound speed and propagation. At the beginning of the 18th century, however, these errors are understandable—wind has a far larger effect on sound speed than temperature and it would have been difficult to arrange an experiment to isolate the smaller temperature effect; even Newton failed to emphasize the connection between temperature and sound speed in his theory. [Newton's prediction for sound speed was wrong because he used the static compressibility of air instead of the dynamic ("adiabatic") compressibility. But he still would have known that density of air is a function of temperature and that would have made his isothermal sound speed a function of ambient air temperature. Had Newton suggested explicitly that there should be a dependence of sound speed on ambient temperature, the other members of the Royal Society—including Derham—would have looked for it.]

Measurement of the speed of sound

How was sound speed measured at the turn of the 18th Century? Often by the obvious method of observing both the flash of a gun or cannon and the subsequent report. Timing was accomplished by observing the oscillations of a simple pendulum. The natural decay of the pendulum amplitude limited the usable propagation time.

Derham opens his paper on sound with an analysis of the work of previous investigators. When confronted with the wide range of values for sound speed presented by others, he isolates two critical issues: (1) the difficulty of watching the oscillations of a simple pendulum while at the same time watching for the flash of the gun to start the timing, and (2) the uncertainty inherent in using a short distance from gun to observer for a speed measurement.

Derham understood that using a clockwork pendulum would yield better results than a simple pendulum for two reasons: each half-cycle produces an audible "tick" so the observer can concentrate on watching for the flash of the gun; and the oscillations do not decay so longer measurement times and distances can be used. With his background in clock mechanics, Derham equips himself with a state-of-the-art pendulum clock that he himself tuned. Realizing that the error associated with a short baseline contributed to the wide variation in sound speed values in the past, Derham made the measurements over as long a distance as he could. The location and elevation of his church tower at Upminster allowed a clear line of sight to the artillery training ground at Blackheath 12.5 miles distant and Blackheath provided a ready source of cannon discharge.

But there were other clever methods of sound-speed measurement. Joshua Walker (1698) gives fascinating insight into echo methods:

> "…and standing over against a high Wall I clapt Two small pieces of Boards together, and observed how long it was e're the Echo returned, and I removed my Station till I found the Place whither the Echo return'd in about half a Second. But that I might distinguish the time more nicely, I clapt every Second of Time Ten or Fifteen times together; so that by this Means I could the better discover whether the Distances betwixt the Claps and the Echoes, and the following Claps were Equal. And though it be very difficult to be exact, yet I could come within some few Yards of the Place I sought for, thus: I observed the Two Places where I could but just discover that I was too near, and where I was too far off…"

In Walker's account, clapping pieces of wood together gave a sharp sound with a distinct echo. By adjusting the distance to a wall, the experimenter can find a point of coincidence between the clap-and-echo time and the period of a simple pendulum. Rather than timing the echo over an arbitrary distance, the difference between the echo-return time and the pendulum oscillation time is adjusted to be as close to zero as possible.

Walker makes a further refinement of the echo method. He discovered that it is quite natural to develop an alternating rhythm: clap-echo-clap-echo, with equal intervals between echo and clap. Instead of attempting to match a single clap-echo against a single swing of the pendulum, he establishes this rhythmic clapping and compares it to multiple swings of the pendulum.

This rhythmic clapping is brilliant. Human ability to establish such a rhythm is remarkable—a series of claps can be evenly interspersed between the series of echoes with a remarkable uniformity of interval. [It is, however, all too easy to get bad results. Care must be taken to select a wall without overhangs, steps, or inside corners. All other reflectors must be sufficiently distant to have no influence. And changes in ambient noise can distract sufficiently to upset the rhythm.]

Had this technique been pursued more seriously, it may have been capable of the precision required to determine the temperature dependence of sound speed 50 years before it was actually measured. Matching the periodicity of the clap-echo pattern with the periodicity of the pendulum is an early variety of synchronous averaging. Over a long period of observation, the clapping rate and the pendulum rate could have been matched closely by either increasing or decreasing the distance to the wall. Walker used a simple pendulum—a lead bullet on a wire—and this limited the period of observation as the pendulum's oscillation decayed. [In addition trying to keep the required rhythm while both listening for echoes and watching the pendulum would very likely have created problems. It would have been difficult to avoid unintentional phase locking between the clapping rate and the pendulum unless a second observer was used. If Walker had concentrated only on clapping the boards and a second observer watched the pendulum, this could be avoided.]

Walker suggested the use of an "automatic" clock but apparently did not try it himself. If Walker had Derham's automatic clock and if Walker had chosen to perform the echo experiment on the coldest and hottest days available to them in England, he may have uncovered the temperature dependence of sound speed.

What would have been required to see the temperature dependence of sound speed and could such an experiment have succeeded in 1700? In air, the speed of sound (in m/s) is equal to 20.05 times the square-root of the absolute temperature in kelvin (the temperature in celsius plus 273.1). Consequently, the variation in sound speed with temperature in the vicinity of 15 °C is 0.59 m/s per degree celsius. From winter to summer in England, it would not have been difficult to find temperatures from at least 0 to 25 °C. This temperature range would produce a seasonal sound speed difference of almost 15 m/s—slightly more than 4 percent. Derham claimed a timing resolution of 0.25 seconds. With this timing resolution a total travel time of at least 6.25 seconds would be just sufficient to resolve a 4 percent difference in sound speed. A measurement of ten times that duration would resolve easily the 4 percent variation in sound speed. So the question reduces to this: can we design an experiment to give a one-minute period of coherent observation? Derham's timing measurements from Blackheath to Upminster had a travel time of about 60 seconds but the dependence on wind is so strong that it would have been challenging to uncover the temperature dependence. [Furthermore, refraction—unknown to anyone at that time—would have confounded the interpretation. It would have been natural to select the coldest conditions and compare those results to the hottest conditions but these two extremes would most typically be accompanied by significantly different actual path lengths because of differences in refraction.]

If, however, rhythmic clapping in the echo method could be maintained for 60 seconds, there would be a chance of success. In the echo method, the effects of wind are second order and a short baseline would avoid the effects of refraction. A careful, rhythmic echo measurement may have uncovered the temperature dependence of sound speed using the clocks available in 1700.

Of course, echo methods are not suitable for assessing the effects of wind. A round-trip measurement has a much weaker dependence on wind speed than the one-way measurement. [It is sometimes naively stated that the echo method cancels any effects of wind but that is not true. The round-trip measurement cancels the *first-order* influence of the wind but the measured speed still depends on the square of the ratio of the wind speed to the sound speed. In addition, the error introduced by the wind biases the round-trip measurement—the measured speed is always less than the thermodynamic sound speed regardless of the direction of the wind.]

In retrospect, what is surprising is that the echo methods did not produce particularly good results in the late 1600's and early 1700's. Even Walker who seemed to have an exceptional grasp of the structure of a good measurement did not produce a particularly good value for sound speed. For the eleven measurements he reports, the average (with one standard deviation) is 1305 ± 120 ft/s. By and large, the credible measurements were long-baseline, one-way measurements by flash-and-report timing of guns or cannons. Furthermore, long one-way measurements were necessary to identify the effects of wind.

Like Walker, Derham believed that an "automatic" clock was a far better measurement tool for sound speed measurement than a simple pendulum. Walker describes the typical timing pendulum as a length of cord or wire with a lead bullet crimped onto the end. This is, in fact, the apparatus described by Newton in his measurement of sound speed. Newton's approach was a modification of the simple echo method but not as elegant as Walker's. Newton located himself 208 feet from a reflecting wall and compared the echo transit time to two pendulums. [This is the experiment described in the first edition of *Principia*.]

He noted that the echo returned before one half-cycle of an 8-inch pendulum but after one-half cycle of a 5.5-inch pendulum. From these observations, Newton concluded that the speed of sound must be greater than 920 ft/s but less than 1085 ft/s. These values bracketed his flawed prediction of 968 ft/s so he had little incentive to refine the measurement. [Convincing experimental evidence that the value of 968 ft/s was far too low came after the first edition of *Principia*. Newton did try again later with revised results published in the second and third editions.]

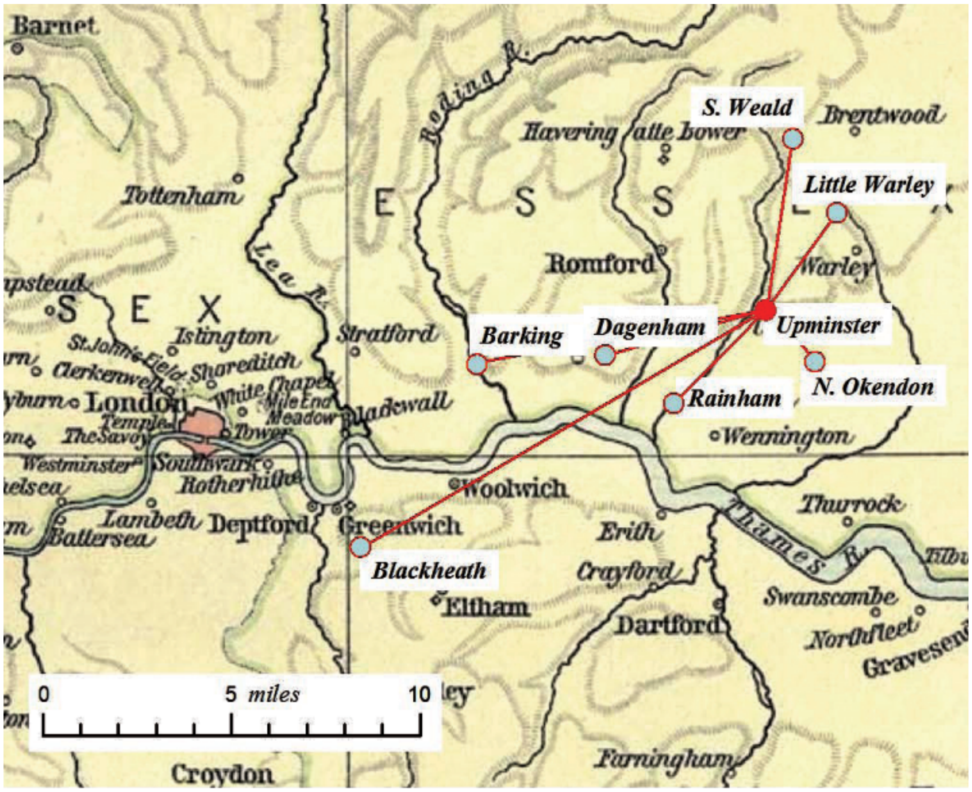

Fig. 2. Derham made an extensive set of measurements over a two-year period between the artillery training range at Blackheath and his church at Upminster. He also made other measurements, often between his church and other churches in Essex–the elevation of the church towers being useful. (Base map, Vicinity of London, reproduced by permission from The Historical Atlas by William R. Shepherd, Henry Holt and Company, NY, 1911.)

A fundamentally different technique was introduced by Joseph Sauveur. Sauveur compared the pitches of organ pipes with their length and concluded that the speed of sound could be determined from this relationship. He was approximately correct but knew nothing of the significance of acoustical end corrections for open-ended pipes; it would be 150 years before these corrections were known well enough for the resonance in an open-ended pipe to be useful for estimating sound speed. [This century-and-a-half period appears more than once. It would be about 150 years before Laplace's proper calculation of sound speed from the physical properties of air was accepted widely and it would be about 150 years before refraction of sound by wind (as opposed to simply increasing or decreasing the apparent sound speed) was understood.]

Analysis of Derham's measurements

Derham compiled what appears to be the largest set of sound speed measurements of his time but he did little quantitative analysis of his own data. Although he was a capable surveyor, none of Derham's many published works shows an aptitude for mathematical analysis. Considering all of his published measurements (without attempting to correct for wind or temperature), the average value is 1138 ± 25 ft/s where one standard deviation is given for the uncertainty. If, instead, we average all of his measurements over his favorite path—Blackheath to Upminster—we obtain virtually the same result: 1135 ± 28 ft/s. Instead of citing 1135 ft/s as the value for sound speed, Derham shows his deference to others and merely claims that his measurements support the value of 9¼ half-seconds per mile (1142 ft/s) obtained by Flamsteed and Halley. There is little significant difference between Derham's value and the Flamsteed/Halley value but Derham reports many measurements with their baseline distances and supporting meteorological observations. (See Fig. 2.). [Derham also cites the speed of sound as 9 1/4 half-second vibrations per mile of travel. This works out to 1142 ft/s. Apparently, there is no extant reference to the Flamsteed/Halley measurements other than Derham's paper so it is possible that Flamsteed and Halley reported the same 9 1/4 half-seconds rather than 1142 ft/s.]

A one-way measurement is influenced by wind. Not all of Derham's contemporaries agreed that wind was important but Derham left the question open and addressed it specifically in his measurements. His table of values from two years of observations from Blackheath to Upminster includes notation regarding wind direction and speed. His wind speed notation was simply a number related to his perception of the wind force. If we use his wind direction to determine the component of his wind-force index that aligns with the direction of the path from Blackheath to Upminster, we can calculate a wind-speed index that relates to the degree to which the wind would aid or hinder sound. Plotting that wind-speed index against the measured travel time, there is a clear relationship between the two. Derham saw this relationship from his numbers and, unless evidence is found to

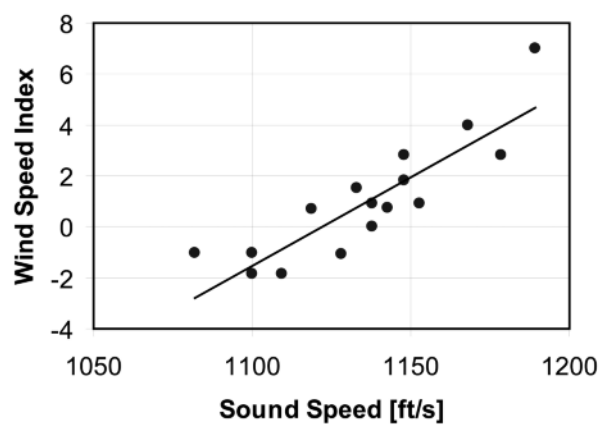

Fig. 3. Derham recorded wind direction and a speed number for each of the measurements he tabulates from Blackheath to Upminster. The vertical scale for this plot is derived from his values for wind direction and speed by projecting his index number onto the direction from Blackheath to Upminster. This gives a quantitative approximation to the aiding or retarding influence of the wind. Derham claims to have established a relationship between wind speed and sound speed and this plot substantiates that claim clearly in spite of the spread in his individual measurements. The black line is a linear-regression fit.

the contrary, his work appears to be the first, clear demonstration of the effect of wind on sound speed. (See Fig. 3.)

This plot reveals another facet of these measurements. Wind aided the propagation in more instances than it hindered the propagation. This may have been a climatological bias—if the prevailing winds were from the west, there may simply have been more opportunities to observe in conditions of wind-aided propagation. However, this could also have been the result of refraction. Once the influence of winds was suspected, experimenters realized that they might be able to reduce the effects of the wind on their one-way measurements by repeating the measurement in the opposite direction. On the surface, this seems a good idea but many who attempted the reciprocal measurement were frustrated. Gerrit Moll (1824) writes of his measurements [G. Moll, A. Van Beek, "An account of experiments on the velocity of sound, made in Holland," Phil. Trans. Roy. Soc. London 114, 424-456, 1824. Zevenboompjes and Kooltjesberg are low hills about 11 miles apart near Amersfoort and Blaricum respectively.]:

> "The first night of our experiments, the 23d, 24th, and 25th of January, 1823, we experienced the same annoyance of which the French philosophers had to complain the first night of theirs. The report of the shots of Zevenboompjes was not heard at all at the station of Kooltjesberg. But at Zevenboompjes all the shots of Kooltjesberg were distinctly heard."

Until the effects of wind-induced refraction were understood, this was a mystery, but sounds are often far weaker upwind of a source than downwind. Derham's data set may have been biased by the difficulty in making an observation with propagation against the wind.

It is unfortunate that Derham did not record temperature but the uncertainty in his measurements would have obscured any relationship. Criticism in hindsight is wasted effort; it is more useful to examine what he did record. In addition to recording the local wind speed and direction, he also recorded (when possible) the direction of movement of clouds. Cloud motion indicates wind direction at altitude that can be different than the direction at the ground; a difference in wind direction with altitude solved an intriguing mystery a century and a half later. Based on observations, Joseph Henry suspected that, near coastal areas, the surface winds might be directed so as to hinder propagation but that an opposite wind direction at higher altitudes could produce good acoustic transmission. Joseph Henry advocated launching small balloons during acoustical measurements to identify conditions that changed significantly with altitude. [Even today, atmospheric acoustic measurements are often made without sufficient supporting meteorological data. Unless the propagation ranges are extremely short, measurements of wind speed vertical gradients and temperature gradients are often necessary (and how many experimenters have neglected to measure humidity in spite of its marked effect on absorption of sound?).]

Where Derham was mistaken

As careful as many parts of Derham's work were, there are several unfortunate statements in his paper. The first relates to the work of the German Jesuit scholar, Athanasius Kircher:

> "Kircher 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); 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 sound is only more or less clear from this variation of the medium, and perhaps this fact deceives the sagacious Kircher.

In considering this statement, it is important to recognize that Derham was disputing more than just Kircher's

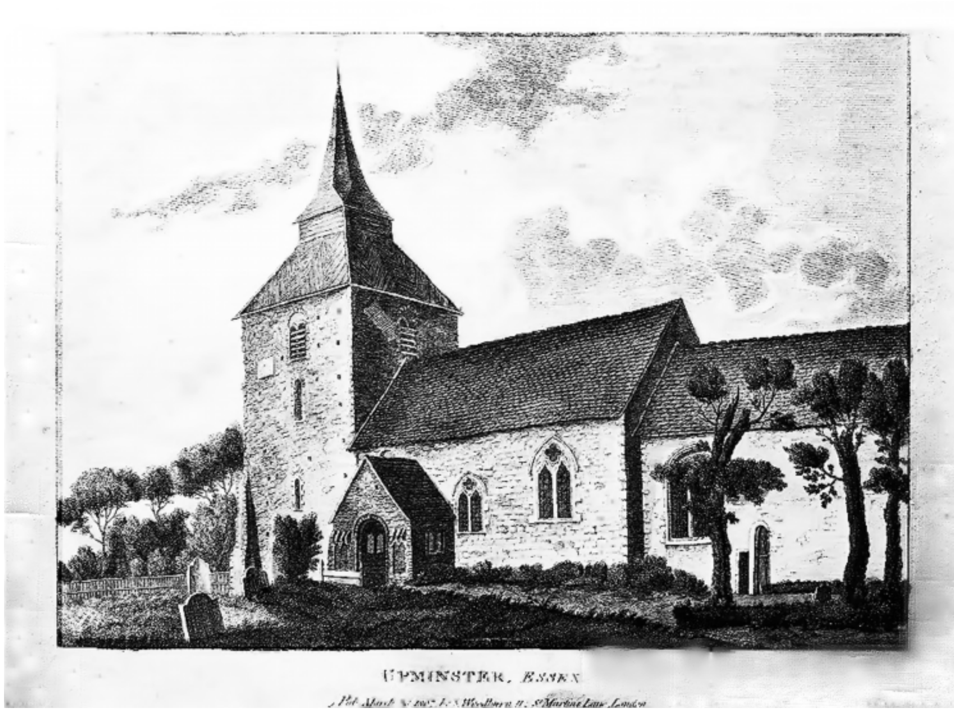

Fig. 4. St. Laurence Church at Upminster. William Derham was Vicar, then Rector of the Upminster parish for much of his adult life. In Derham's time, there was a door and a platform on the south side of the tower from which he made many of his speed-of-sound, astronomical, and meteorological observations. (Image supplied by Fr. Michael Hore, current Rector of the Upminster Parish.)

ideas regarding the speed of sound. In 1700, there were many strange ideas. Some workers thought that wind had no effect on the speed of sound; others thought that any wind, no matter what the direction, retarded sound; still others believed that sound was affected strongly by barometric pressure. Most of these ideas were ill-considered and supported only by observations made in ambiguous circumstances. For example, Kircher did not demonstrate the proper dependence of sound speed on temperature. Derham countered sloppiness with careful, repeated observations but his techniques could not resolve the small temperature dependence of sound speed. Derham said that "…whether heat or cold dries the air,…whether summer or winter,…the velocity of sound is neither greater nor less…" In hindsight, he was clearly wrong. He would have been safe to say that such changes must be smaller than his ability to resolve. He would also have been safe to say that others in writing about apparent variations in sound speed were not making careful enough measurements to support their claims. But he didn't.

Later, Derham writes, "But as regards thick fogs, it is certain that they are dampers of sound in the highest degree." The fact that this statement follows a lengthy discussion of the variability of observations in clear and foggy air is uniformly ignored by those who lifted this quote to support their opposition to acoustic fog signals in the mid 1800's. Derham is wrong but the lack of discussion makes one wonder whether he is simply repeating the opinion of others. In any event, this remark is a mystery: contrary evidence is easy to find. Derham did not describe any personal observations in heavy fog; his sound-speed measurement could not be made if the source could not be seen and this lack of personal observation may excuse his naïve acceptance of current belief. Regarding other questions, Derham constructs measurements to support or refute the question; here, he does not.

Finally, Derham speculates on the connection between wind and sound and the discussion reveals a peculiar (to us today) view:

> "…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 …778 miles in the same time."

Derham assumes that, because the speeds of wind and sound are so much different, that they must be carried by distinctly different parts of the air, one more ethereal than the other. It is quite possible that Derham didn't understand the basis of Newton's expression for the speed of sound since Newton based his calculation on the ordinary properties of air.

Where Derham was correct

Notwithstanding Derham's detractors, he was right more often than he was wrong. His measurements were sound and showed remarkably little variance considering the equipment that was available to him. The care with which he made his measurements served as a model for those to follow:

> "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…"

Derham was the first to establish clearly the dependence of sound speed on wind speed and direction.

He understood the psychology of measurement and tried to avoid the pitfalls. He understood that it was difficult to depend on vision both to watch the timing pendulum and to watch for the flash of a distant gun; he replaced the simple pendulum with an automatic pendulum that he could hear while watching, often through a telescope, for the muzzle flash. He attributed Kircher's faulty conclusions to confusion between clarity of sound and the apparent speed.

He also understood uncertainty in measurement and he realized that the longer the measurement baseline, the less the impact of an uncertainty in timing.

He did not ascribe to the then popular (and incorrect) belief that optical transparency implied acoustical transparency and the optical opacity implied acoustical opacity.

Summary

When considered in the context of the times, Derham's work was exceptional. He first analyzed the weaknesses with other investigators' techniques and then he designed his own measurements to avoid these problems. He was rarely dogmatic; he would often point out inconsistencies in his own observations. [And the few times that he made unsubstantiated statements, he was more often wrong than right. There's a lesson here.] Unfortunately, several of his less-well-considered statements were quoted then re-quoted well into the 1800's – even then, the "sound bite" was more attractive than dogged pursuit of truth.

Derham's work had a substantial impact on research and literature for about 150 years after which it faded into obscurity. The impact was not always positive—quotes often propagated from paper to paper bereft of their context. [Certainly this would "never" happen today.] Derham wrote that freshly fallen snow seemed to deaden sounds dramatically until the snow became compacted. This statement is true but it was twisted by subsequent authors into a statement that *falling* snow hindered the transmission of sound.

More often than not, Derham bases his conclusions (or lack of conclusion) on multiple observations and, if those observations are inconsistent, he says so. Before his brief statement about heavy fog, Derham shares contradictory observations regarding the effects of light rain and fog. One has to wonder, given Derham's openness about contrary observations and his deference to "experts," whether the dogmatic statement regarding heavy fog is anything but an echo of the expert opinion of the day.

In the cultural climate of England at Derham's time, there was no great divide between the pursuit of religion and the pursuit of science. Derham was twice invited (1711 and 1712) to give the prestigious Boyle Lectures—lectures for the purpose of communicating to the public the connection between science and religious thought. Derham's most widely distributed book—*Physico-Theology*—expounded this connection. His earliest book was technical rather than philosophic: *The Automatic Clockmaker* is a summary of the state of the art of timekeeping instruments. This background positioned him well for timing his sound speed measurements.

He published 44 papers for the Proceedings of the Royal Society of London on topics ranging from meteorology to astronomy and entomology. He edited and published the scientific memoirs of Robert Hooke and the naturalist, John Ray. Derham was Rector of Upminster and his access to the church was particularly useful. A doorway and platform were installed in the tower of St. Laurence's Church in Upminster. Derham used this platform as an observation point for many of his sound speed measurements and his astronomical and meteorological investigations. (See Fig. 4.)

William Derham and James Welling, who translated *De Motu Soni*, each stood at crossroads: Derham's paper at the start of the 18th century on the motion of sound marked the beginning of high-quality measurements of the speed of sound; the connection he drew between wind and sound opened new avenues. Welling—another amateur scientist—contributed to one of the many experiments near the end of the 19th century that established refraction as critical to the understanding of sound propagation in the atmosphere.

Prior to Derham's paper, confusion reigned in the realm of meteorological effects on sound. While other investigators believed that wind had some effect, Derham demonstrated an effect and got the magnitude and direction about right with respect to sound speed. But it wasn't until the latter half of the 19th century that refraction or bending of sound by gradients in wind and temperature was demonstrated. Whereas John Tyndall, with his curious idea of flocculence is better known in the community of acousticians, it was left to George Stokes, Osborne Reynolds, and Joseph Henry to elucidate refraction of sound.

A. D. Atkinson provides a fitting conclusion [A. D. Atkinson, "William Derham, F.R.S. (1657-1735)," Annals of Science 8(4), 368-392, 1952.]:

> "If not an outstanding character, Derham yet emerges from his half-forgotten books and letters with surprising clarity…His busy, rather self-important, investigations, now being rowed round the marshes, now climbing the Chapel at Windsor or the Observatory at Greenwich, discussing the wheel-work of the Hampton Court clock with the Fenchurch Street watch-maker, listening from his church-tower for the echoes of gun-fire from half a dozen neighbouring parishes…all these …go to make the portrait of a far from unworthy man. If the …zeal which persuaded him to taste ear-wax and beetles, raise a smile, such trifles add humanity to the picture. If his genius was limited, his curiosity and devotion seem unquestionable, his career an admirable example of a way of life which, now impossible, has often in the past contributed to the progress of thought and literature in England."

Appendix 1: Timeline

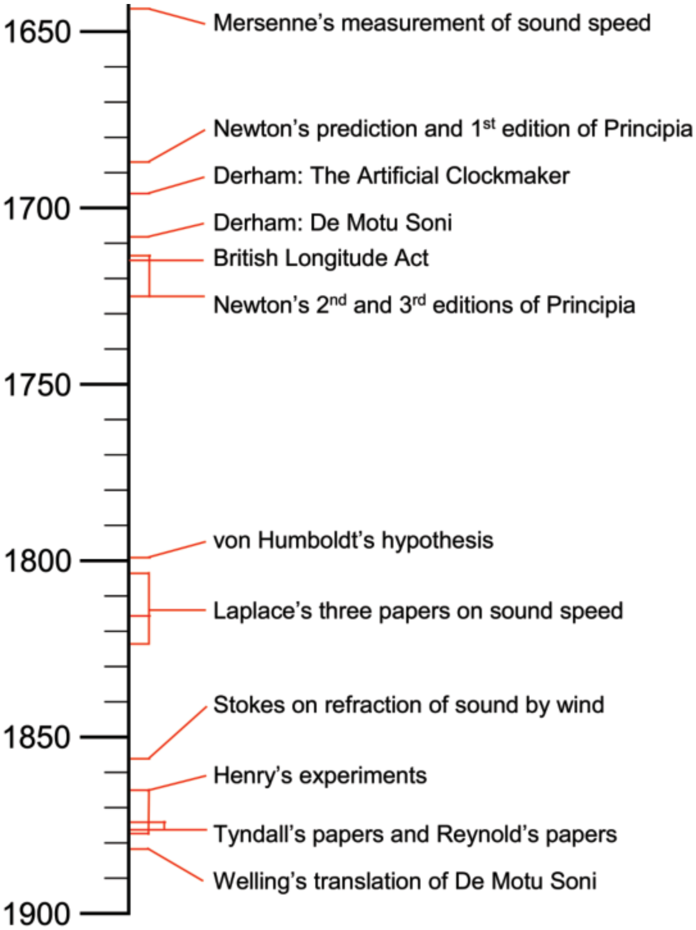

Fig. A1. Time line of significant events with relevance to Derham's paper on sound. Newton's prediction of what we know call the isothermal speed of sound was given in the 1st edition of Principia; it was not until Laplace's third paper that theory for the proper "adiabatic" sound speed was published. Tyndall relied on von Humboldt's hypothesis, which Tyndall called flocculence, in his interpretation of sound transmission; whereas, Henry advocated Stokes' hypothesis of sound refraction. Derham's earlier work on clocks positioned him well for accurate measurement of sound speed but, when the prize for a ship-board chronometer was announced through the Longitude Act of 1714, he and many others shifted their focus to astronomical observations.

Appendix 2: Other publications by Derham

Books and Lectures

The artificial clock-maker: A treatise of watch and clock-work, London, 1696.

Physic-theology: or, a demonstration of the being and attributes of God, from his works of creation, London, 1713. This book went through several editions. Boyle Lectures, 1711 and 1712.

The philosophical experiments of Robert Hooke, London, 1726.

Miscellanea Curiosa, containing a collection of some of the principal phaenomena in nature, London, 1726.

Titles of papers published by Derham in *Philosophical Transactions of the Royal Society of London*

These papers were published in the Philosophical Transactions of the Royal Society of London from 1698 through 1735. I include only the titles (abbreviated in some cases) and the year of the volume in which it was published.

An account of some experiments about the heighth of the mercury in the barometer at top and bottom of the Monument. (1698)

A contrivance to measure the height of the mercury in the barometer. (1698)

Observations of the height of the mercury in the barometer, rains, wind, etc for the Year 1698. (1699)

An account of observations of the weather for the Year 1699. (1700)

Concerning an insect that is commonly called the Death-Watch. (1700)

Concerning observations on the weather for some years past. (1702)

Some observations on the spots of the sun. (1702)

Observations concerning the Late Storm. (1704)

An instrument for seeing the sun, moon, or stars, pass the meridian. Useful for setting watches in all parts of the world with great exactness; to assist in the discovery of longitudes. (1704)

A supplement to the account of the pediculus pulsatorius, or Death-Watch, serving to the more perfect natural history of that insect. (1704)

Experiments about the motion of pendulums in vacuo. (1704)

An account of some magnetical experiments and observations. (1704)

Prospect of the weather, winds, and height of the mercury in the barometer, on the first day of the month; and of the

whole rain in every month in the Year 1703. (1704)
Concerning a glade of light observed in the heavens. (1706)
An account of a pyramidal appearance in the heavens observed near Upminster in Essex. (1706)
Concerning the migration of birds. (1708)
An account of some inundations; monstrous births, appearances in the heavens, and other observables received from Ireland. With observations of the eclipse of the sun, Sept. 3, and of the moon, Sept. 18, 1708. (1708)
The history of the great frost in the last Winter 1703 and 1708/9. (1708)
An account of a child's crying in the womb. (1708)
A short dissertation concerning the child's crying in the womb. (1708)
Experimenta et observationes de soni motu…(in Latin). (1708)
Tables of the barometrical altitudes at Zurich in Switzerland in the Year 1708. Also on the winds, heat and cold occurring in three different parts of Europe. (1708)
Observations concerning the subterraneous trees in Dagenham and other marshes bordering upon the River of Thames in the County of Essex. (1710)
Observations of the eclipse of the moon on Jan. 12, 1711-12. (1710)
Observations upon the spots that have been upon the sun from the Year 1703 to 1711. (1710)
The case of a woman big with child, who recovered from small pox, and was afterwards delivered of a dead child. (1713)
An account of the mischiefs ensuing the swallowing of the stones of bullace and sloes. (1714)
An account of the rain which fell every year at Upminster in Essex, the last eighteen years. (1714)
Extracts from Mr. Gascoigne's and Mr. Crabtries's letters, proving Mr. Gascoigne to have been the inventor of the telescopick sights of mathematical instruments. (1717)
An account of a large quantity of alcalious salt produced by burning rotten wood. (1720)
Observations about wasps and the difference of their sexes. (1724)
Observations on the lumen boreale or streaming on Oct. 8, 1726. (1726)
Observations of the eclipses of Jupiter's satellites from 1700 to the Year 1727. (1727)
The difference in time of the meridians of diverse places computed from observations of the eclipses of Jupiter's satellites. (1729)
A description of some uncommon appearances observed in an aurora borealis. (1729)
Of the meteor called the ignis fatuus from observations made in England. (1729)
Concerning the frost in January, 1730/31. (1731)
An abstract of the meteorological diaries communicated to the Royal Society with remarks upon them. (Five parts from 1731 to 1733)
Observations of the appearances among the fixed stars called nebulous stars. (1733)
Experiments concerning the vibrations of pendulums. (1735)

Appendix 3: Background references

A. D. Atkinson, "William Derham, FRS," Annals of Science **8**(4), 368-392 (Dec. 1952).
J. C. Welling, "Anomalies of sound signals," Bulletin Phil. Soc. Washington **5**, 39-46 (1883) (delivered at the 205th meeting, Nov. 5, 1881).
Bernard S. Finn, "Laplace and the speed of sound," Isis **55**(1), 7-19 (Mar. 1964).
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Essayes of Natural Experiments made in the Academie del Cimento, translated by Richard Waller, London (1634).
Joseph Henry, Scientific Writings, 1886. See the various Reports of the US Light-House Board for 1875.
R. S. Westfall, "Newton and the fudge factor," Science **179**(4075), 751-758 (1973).
Harry Bateman, "The influence of metrological conditions on the propagation of sound," Monthly Weather Report **42**, 258-265 (May 1914). [In Bateman's end-note 1, "A copy of an English translation by Rev. Dr. J. C. Welling is in the library of the U. S. Weather Bureau."]
Walker, "Some experiments and observations concerning sounds," Phil. Trans. **20**, 433-438, (1698).AT

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