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BIOGRAPHICAL MEMOIR WALLACE CLEMENT WARE SABINE

1868-1919

BY

EDWIN H. HALL

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Wallace C. Sabine.

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By Edwin H. Hall [1]

Our colleague, Wallace Clement Ware Sabine, was born in Richwood, Ohio, June 13, 1868. According to family tradition, four racial strains were joined in him, each of his four names representing some family of his ancestors, one Scotch, one Dutch, one English, and one French.

The most remote ancestor of whom we have knowledge was William Sabine, or Sabin, who was living in the town of Rehoboth, near Plymouth, Mass., in 1643. He is supposed to have been of Huguenot stock, though conclusive evidence of this derivation is not found. It seems clear that he was a man of substance and of good standing in his community. His will, written in 1685, probated in Boston and, I think, still to be seen there, mentions by name "sixteen of his twenty children."

From him the line of descent runs through Benjamin, four Nehemiahs in succession, John Fletcher, and Hylas, to Wallace Clement. Early in the nineteenth century the fourth Nehemiah Sabine went from Massachusetts to Ohio, "with a family of eleven children." "He was a pioneer preacher, with a circuit of fifty miles radius. Leather saddle-bags on his horse carried the Bible and his doctrine of eternal rewards and punishments." His son, John Fletcher, "became a Universalist, gentle and generous." "He [John Fletcher] died at the age of eighty-nine, with mind as vigorous and clear as in youth, and with a remarkably retentive memory. His wife was Euphemia Clement, a gentle, industrious, reliable woman. Hylas Sabine was their oldest son." Of Wallace Sabine's maternal grandfather, Jacob Reed Ware, who was of English Quaker stock, it is written, "He was one of the early, ardent abolitionists and lived on the most direct line from southern slavery to freedom in Canada." "Untiring of body, alert of mind, and exceedingly strong of purpose, he lived in perfect health, with such simple habits that at the age of ninety-eight, without disease, he fell asleep." "J. R. Ware married Almira Wallace, a woman of force and uprightness. Anna Ware was their first daughter."

To those who knew Sabine well this brief family history is deeply significant. Gentleness, courtesy, rectitude, untiring energy, fixity of purpose that was like the polarity of a magnet, all these traits we find in him. It is interesting and impressive to see how the individualism and stern conscience that made his ancestors on the one side Quakers in England and, probably, on the other side Protestants in France, found expression in him, under changed intellectual conditions. He was of the very stuff of which martyrs are made; in fact, he died a martyr to his sense of duty, but, with an austerity of morals and a capacity for devotion which none of his conspicuously religious forefathers could have surpassed, he held aloof, silently but absolutely, from all public profession of religious creed, and he took small part in religious observances.

Hylas Sabine, the father of Wallace, was Railway Commissioner of Ohio, and it is said that he was the first commissioner in the country to establish and adequately enforce State supervision of railways through State inspection and compulsory returns. One of his maxims, quoted to me by his son, was substantially this: *Treat everyone with courtesy, and especially yourself.* He had "esthetic sense and ability to design and draw," which qualities both his children inherited.

In mental and physical energy, intensity of feeling, and bent for exact science, Wallace resembled his mother, whose vigorous yet delicate personality charmed everyone who met her.[2]

The blended inheritance from the parents is perhaps shown in the fact that the daughter, now the wife of Prof. Wilbur Siebert, of Columbus, Ohio, once engaged in research on the

[1] In this memoir I shall draw freely, usually without quotation marks, from two papers written by me soon after Professor Sabine's death, one for the Harvard Graduates' Magazine, the other for the records of the Harvard Faculty of Arts and Sciences. In many cases what appears to be a continuous quotation from Sabine's papers is made up of selected passages. E. H. H.

[2] She died in March, 1923, aged 88 years. Most of the genealogical matter contained in this paper was furnished by her, and a considerable part of it is here given in her own telling words.

telephone, under Professor Cross, at the Massachusetts Institute of Technology, and later became highly skilled as a painter of miniature portraits, while the son found his chief distinction in a field of exact science closely related to esthetic art.

As a child Wallace was allowed to develop without forcing, but such was the natural vigor of his mind that he gained the degree of A. B. at Ohio State University at the age of 18. He is said not to have specialized in his college studies, but he had in Prof. T. C. Mendenhall an inspiring teacher of physics, and his early interest in scientific matters is shown by the fact that he attended a meeting of the American Association for the Advancement of Science held in Philadelphia in 1884, when he was 16 years old. On leaving Ohio State University in 1886 he went to Harvard as a graduate student in mathematics and physics, and he received the Harvard A. M. in 1888. From 1887 to 1889 he held a Morgan fellowship, but in the latter year he became an assistant in physics. Rather early in his Harvard residence he was taken by Professor Trowbridge as partner in a photographic study of the oscillating electric discharge, and he showed a remarkable aptitude for work of this kind, requiring high experimental skill, yet he never became a candidate for the Ph. D. Absorption in the work of teaching prevented him for several years from engaging deeply in further work of research. He spent his energy and his talents in building up courses of laboratory work, designing and making apparatus for instruction, and in every way practicing with devotion the profession of a teacher. It is not too much to say that, for the 15 years preceding his taking the duties of a deanship, he was the most effective member of the Harvard department of physics in giving inspiration and guidance to individual students of promise. This was due in part to his comparative youth, though no one of the department was repellently old; in part to his sympathetic willingness to give help and to spend much time in giving help, though others were not lacking in this quality. It was perhaps due mainly to the fact that, while he was no more deeply versed than others in the profundities of physics and mathematics, he had a peculiarly clear vision for the right kind of experimental problem and for the best way of attacking it, and his students instinctively, it may be, perceived this.

For a long time he seemed to be content to remain in comparative obscurity, while directing others into paths of conspicuous achievement. He was made assistant professor of physics in 1895, after six years of teaching, in which he published little or nothing descriptive of research. This was partly because he had a most severe standard for what a research paper should be; it should describe some piece of work so well done that no one would ever have to investigate this particular matter again. To this standard he held true, with the result that his published papers were, to the end, remarkably few and remarkably significant.

One might have expected him, when he found time for research, to take up some problem in light, for that had seemed to be his chief field of interest; but accident, and a sense of duty, turned him to a different quarter. The Fogg Art Museum, on its completion in 1895, proved to have an auditorium [3] that was monumental in its acoustic badness, and President Eliot, not fully realizing the importance of the step he was taking, but acting with his usual sure judgment of men, called upon Sabine to find a remedy, as a practical service to the university. The nature of Sabine's problem and the way he analyzed it can best be shown by the following passages taken from the first [4] of his "Collected papers on acoustics":

> No one can appreciate the condition of architectural acoustics—the science of sound as applied to buildings—who has not with a pressing case in hand sought through the scattered literature for some safe guidance. Responsibility in a large and irretrievable expenditure of money compels a careful consideration, and emphasizes the meagreness and inconsistency of the current suggestions. Thus the most definite and often repeated statements are such as the following, that the dimensions of a room should be in the ratio 2:3:5, or according to

[3] The architect of this building was Richard Morris Hunt, one of the most eminent in America of his generation. The following story is told of a conversation between him and the Rev. Henry Ward Beecher:

Mr. BEECHER. Mr. Hunt, how much do you know about acoustics?

Mr. HUNT. As much as anyone, Mr. Beecher.

Mr. BEECHER. How much is that?

Mr. HUNT. Not a damned thing.

Mr. BEECHER. I think you are right.

[4] Published first in the American Architect and the Engineering Record, 1900.

some writers, 1:1:2, and others, 2:3:4; it is probable that the basis of these suggestions is the ratios of the harmonic intervals in music, but the connection is untraced and remote. Moreover, such advice is rather difficult to apply; should one measure the length to the back or to the front of the galleries, to the back or the front of the stage recess? Few rooms have a flat roof, where should the height be measured? One writer, who had seen the Mormon Temple, recommended that all auditoriums be elliptical. Sanders Theatre is by far the best auditorium in Cambridge and is semicircular in general shape, but with a recess that makes it almost anything; and, on the other hand, the lecture room in the Fogg Art Museum is also semicircular, indeed, was modeled after Sanders Theatre, and it was the worst. But Sanders Theatre is in wood and the Fogg Lecture room is plaster on tile; one seizes on this only to be immediately reminded that Sayles Hall in Providence is largely lined with wood and is bad. . . .

In order that hearing may be good in any auditorium, it is necessary that the sound should be sufficiently loud; that the simultaneous components of a complex sound should maintain their proper relative intensities; and that the successive sounds in rapidly moving articulation, either of speech or music, shonld be clear and distinct, free from each other and from extraneous noises. These three are the necessary, as they are the entirely sufficient, conditions for good hearing. . . . Within the three fields thus defined is comprised without exception the whole of architectural acoustics.

Starting with the simplest conceivable auditorium—a level and open plain, with the ground bare and hard, a single person for an audience—it is clear that the sound spreads in a hemispherical wave diminishing in intensity as it increases in size, proportionally. If, instead of being bare, the ground is occupied by a large audience, the sound diminishes in intensity even more rapidly, being now absorbed. The upper part of the sound-wave escapes unaffected, but the lower edge—the only part that is of service to an audience on a plain—is rapidly lost. The first and most obvious improvement is to raise the speaker above the level of the audience; the second is to raise the seats at the rear; and the third is to place a wall behind the speaker. The result is most attractively illustrated in the Greek theatre. These changes being made, still all the sound rising at any considerable angle is lost through the opening above, and only part of the speaker's efforts serve the audience. When to this auditorium a roof is added the average intensity of sound throughout the room is greatly increased. . . .

In discussing the subject of loudness the direct and reflected sounds have been spoken of as if always reenforcing each other when they come together. A moment's consideration of the nature of sound will show that, as a matter of fact, it is entirely possible for them to oppose each other. The sounding body in its forward motion sends off a wave of condensation, which is immediately followed through the air by a wave of rarefaction produced by the vibrating body as it moves back. These two waves of opposite character taken together constitute a sound wave. The source continuing to vibrate, these waves follow each other in a train. Bearing in mind this alternating character of sound, it is evident that should the sound travelling by different paths—by reflection from different walls—come together again, the paths being equal in length, condensation will arrive at the same time as condensation, and reenforce it, and rarefaction will, similarly, reenforce rarefaction. But should one path be a little shorter than the other, rarefaction by one and condensation by the other may arrive at the same time, and at this point there will be comparative silence. . . . When the note changes in pitch the interference system is entirely altered in character. A single incident will serve to illustrate this point. There is a room in the Jefferson Physical Laboratory, known as the constant-temperature room. . . . While working in this room with a treble *c* . . . organ pipe blown by a steady wind-pressure, it was observed that the pitch of the pipe apparently changed an octave when the observer straightened up in his chair from a position in which he was leaning forward. The explanation is this: The organ pipe did not give a single pure note, but gave a fundamental treble *c* accompanied by several overtones, of which the strongest was in this case the octave above. Each note in the whole complex sound had its own interference system, which, as long as the sound remained constant, remained fixed in position. It so happened that at these two points the region of silence for one note coincided with the region of reenforcement in the other, and *vice versa*.

Sound, being energy, once produced in a confined space, will continue until it is either transmitted by the boundary walls, or is transformed into some other kind of energy, generally heat. This process of decay is called absorption. Thus, in the lecture room of Harvard University, in which, and in behalf of which, this investigation was begun, the rate of absorption was so small that a word spoken in an ordinary tone of voice was audible for five and a half seconds afterwards. During this time even a very deliberate speaker would have uttered the twelve or fifteen succeeding syllables. Thus the successive enunciations blended into a loud sound, through which and above which it was necessary to hear and distinguish the orderly progression of the speech. Across the room this could not be done. . . . This [disturbance] may be regarded, if one so chooses, as a process of multiple reflection from walls, from ceiling and from floor, first from one and then another, losing a little at each reflection until ultimately inaudible. This phenomenon will be called reverberation, including as a special case the echo.

So much from Sabine by way of analysis and illustration of the conditions to be dealt with in architectural acoustics. His final method of experimental attack on the particular difficulty presented by the lecture room in question, that of the Fogg Art Museum, now appears, and, since his work in this room gives us the first known instance of the rational and successful treatment of such a difficulty, the story should be told in his own words.

With an organ pipe as a constant source of sound, and a suitable chronograph for recording, the duration of audibility of a sound after the source had ceased in this room when empty was found to be 5.62 seconds.

All the cushions from the seats in Sanders Theatre were then brought over and stored in the lobby. On bringing into the lecture room a number of cushions having a total length of 8.2 meters, the duration of audibility fell to 5.33 seconds. On bringing in 17 meters the sound in the room after the organ pipe ceased was audible but 4.94 seconds. Evidently, the cushions were strong absorbents and rapidly improving the room, at least to the extent of diminishing the reverberation. The result was interesting and the process was continued. Little by little the cushions were brought into the room, and each time the duration of audibility was measured. When all the seats (436 in number) were covered, the sound was audible for 2.03 seconds. . . . In this lecture room felt was finally placed permanently on particular walls, and the room was rendered not excellent, but entirely serviceable. . . .

Let us note the extreme simplicity of the apparatus and method finally used by Sabine for studying the reverberation of a room. A standard horn or organ pipe was blown by means of a certain air pressure till the room was as full of sound as this source could make it. The action of the horn was then stopped by the push of a button which simultaneously recorded itself on the cylinder of a chronograph; a good observer[5] placed somewhere in the room listened intently but with unassisted ear, till the reverberation became inaudible and at this instant pushed another button and thus made another mark on the chronograph cylinder. The interval of time between the two records on the chronograph—that is, the duration of audible reverberation after the sound supply was cut off—could be measured to about 0.01 second.

Extremely simple in theory, but hard enough in practice; successfully carried out because the man born to do this thing and bound to do it, to break through the armor of difficulties hiding the secret of acoustics, had been found and had found his place. The work must be done "during the most quiet part of the night, between half-past twelve and five."

To secure accuracy, . . . it was necessary to suspend work on the approach of a street car within two blocks, or the passing of a train a mile distant. In Cambridge these interruptions were not serious; in Boston and in New York it was necessary to snatch observations in very brief intervals of quiet. In every case a single determination of the duration of the residual sound was based on the average of a large number of observations.

Three general conclusions which may be rather surprising to most people were derived from these observations, as applying to rooms of fairly regular shape:

1. *The duration of audibility of the residual sound is nearly the same in all parts of an auditorium.*

2. *The duration of audibility is nearly independent of the position of the source (the horn).*

3. *The efficiency of an absorbent in reducing the duration of a residual sound is, under ordinary circumstances, nearly independent of its position.*

All this comes from the fact that a sound wave emitted by the source is reflected back and forth across an ordinary room many times a second and in many directions, so that, when a horn has been sounding for a few seconds and has then stopped suddenly, all parts of the air of the room are about equally full of sound energy.

At first the unit of absorptive capacity employed by Sabine was that of a running meter of the Sanders Theater cushions mentioned in one of the preceding quotations. Thus, the absorbing power of the walls, floor, windows, etc., of a certain bare room was found to be equal to that of 146 running meters of these cushions. But presently it was found that the exposed vertical edge of a cushion counts as much per unit area as the exposed horizontal top.[6] So the square

[5] Mr. Gifford LeClear and Mr. E. D. Densmore assisted in this labor, each making his own observations of the duration of reverberation, for comparison with those of Sabine. This is an example of the latter's power, already mentioned, to enlist the interest of capable young men. On the other hand, he was able to give full recompense to LeClear and Densmore by getting professional work for them at the beginning of their career, now become distinguished, as building engineers. Another who should be mentioned here is Mr. John Connors, head janitor of the Jefferson Physical Laboratory, a man of remarkable resourcefulness and practical good sense. He came from Ireland about the time Sabine came from Ohio. They were nearly of the same age and though, naturally, very unlike in many respects, were close friends. When Sabine died, "John" knew more about his experiments and his plans than did any one of the teaching staff.

[6] This fact was brought out in striking fashion after experiments made with cushions having one edge pushed against the backs of settees had given anomalous results. "It was then recalled that about two years before, at the beginning of an evening's work, the first lot of cushions brought into the room were placed on the floor, side by side, with edges touching, but that after a few observations had been taken the cushions were scattered about the room, and the work was repeated. This was done not at all to uncover the edges, but in the primitive uncertainty as to whether near cushions would draw from each other's supply of sound, as it were, and thus diminish each other's efficiency. No further thought was then given to these discarded observations until recalled by the above-mentioned discrepancy. They were sought out from the notes of that period, and it was found that, as suspected, the absorbing power of the cushions when touching edges was less than when separated. Eight cushions had been used and, therefore, fourteen edges had been touching. A record was found of the length and the breadth of the cushions used and, assuming that the absorbing power was proportional to the area exposed, it was possible to calculate their thickness by comparing the audible duration of the residual sound in the two sets of observations; it was thus calculated to be 7.4 centimeters. On stacking up the same cushions and measuring their total thickness, the average thickness was found to be 7.2 centimeters, in very close agreement with the thickness estimated from their absorption of sound. Therefore, the measurements of the cushions should be, not in running meters of cushion, but in square meters of exposed surface."

meter of cushion replaced the running meter as a convenient unit of reference, though a square meter of open window was taken as the final standard. The general relation of these two units is brought out in the following passage:

For the purposes of the present investigation, it is wholly unnecessary to distinguish between the transformation of the energy of the sound into heat and its transmission into outside space. Both shall be called absorption. The former is the special accomplishment of cushions, the latter of open windows. It is obvious, however, that if both cushions and windows are to be classed as absorbents, the open window, because the more universally accessible and the more permanent, is the better unit. The cushions, on the other hand, are by far the more convenient in practice, for it is possible only on very rare occasions to work accurately with the windows open, not at all in summer on account of night noises—the noise of crickets and other insects—and in the winter only when there is but the slightest wind; and further, but few rooms have sufficient window surface to produce the desired absorption. It is necessary, therefore, to work with cushions, but to express the results in open-window units.

Having no money,[7] in the early years of his investigation at least, for building structures such as he would like to study, Sabine was obliged to depend on chance opportunities to measure the influence of different building materials. Sometimes these opportunities were of a kind not likely to recur, and we should sadly underestimate the merit of his achievements if we failed to note the swiftness and finality of his action in such cases.

This is his story of one; evidently the new rooms described might never be empty again:

Through the kindness of Professor Goodale, an excellent opportunity for securing some fundamentally interesting data was afforded by the new botanical laboratory and greenhouse recently given to the university. These rooms—the office, the laboratory, and the greenhouse—were exclusively finished in hard-pine sheathing, glass, and cement; the three rooms, fortunately, combined the three materials in very different proportions. They and the constant-temperature room in the physical laboratory—the latter being almost wholly of brick and cement—gave the following data:

	Area of hard-pine sheathing	Area of glass	Area of brick and cement	Combined absorbing power
Office	127.0	7	0	8.37
Laboratory	84.8	6	30	5.14
Greenhouse	12.7	80	85	4.64
Constant-temperature room	2.1	0	124	3.08

This table gives for the three components the following coefficients of absorption: Hard-pine sheathing, 0.058; glass, 0.024; brick set in cement, 0.023.

Another case of conditions difficult to obtain is described as follows:

Next in interest to the absorbing power of wall surfaces is that of an audience. During the summer of 1897, at the close of a lecture in the Fogg Art Museum, the duration of the residual sound was determined before and immediately after the audience left. The patience of the audience and the silence preserved left nothing to be desired in this direction, but a slight rain falling on the roof seriously interfered with the observations. Nevertheless, the result, 0.37 per person, is worthy of record. The experiment was tried again in the summer of 1899, on a much more elaborate scale and under the most favorable conditions, in the large lecture room of the Jefferson Physical Laboratory. In order to get as much data and from as independent sources as possible, three chronographs were electrically connected with each other and with the electro-pneumatic valve controlling the air supply of the organ pipe. One chronograph was on the lecture table, and the others were on opposite sides in the rear of the hall. The one on the table was in charge of the writer, who also controlled the key turning on and off

[7] The following letter gives some indication of that severe scrupulosity which was inveterate in Sabine:

HARVARD UNIVERSITY,
Cambridge, November 12th, 1897.

DEAR MR. SABINE: Your explanation of November 3rd about your expenditures in making the investigation which Mr Hooper and I asked you to make is very far from being satisfactory. You have made sufficient progress to be able to prescribe for the Fogg Lecture-room, and you are going to make that prescription. What the Corporation wants is to pay all the costs to this date of that investigation, not of those experiments only which certainly contributed to the result, but of all the experiments made with that object in view which Mr. Hooper and I set before you. Unless you enable the Corporation to do this by rendering an account of your expenditures, you leave the Corporation in the position of having engaged you can work in their interest which not only cost you much time and labor, but also cost you money. It seems to me that on reflection you will perceive that this is not a suitable relation for the Corporation to be left in with one of its assistant professors. You will of course be at liberty to continue the investigation at your discretion, and at your own charge; but up to this time all charges ought to be paid by the Corporation, including the travelling expenses, admission tickets, and the purchase of instruments. These expenses do not require any justification—they are matters of course in such an inquiry.

Very truly yours,

(signed) CHARLES W. ELIOT

Professor W. C. SABINE

the current at the four instruments. The two other chronographs were in charge of other observers, provision being thus made for three independent determinations. After a test had been made of the absorbing power of the whole audience—157 women and 135 men, sufficient to crowd the lecture room—one half, by request, passed out, 63 women and 79 men remaining, and observations were again made. On the following night the lecture was repeated and observations were again taken, there being present 95 women and 73 men. There were thus six independent determinations on three different audiences and by three observers. In the following table the first column of figures gives the total absorbing power of the audience present; the second gives the absorbing power per person; the initials indicate the observer.

	Observer	Total absorbing power	Absorbing power per person
First night, whole audience	W. C. S.	123.0	0.42
Do.	G. LeC.	113.0	.39
First night, half audience	W. C. S.	58.3	.41
Do.	G. LeC.	58.3	.41
Second night, whole audience	W. C. S.	66.2	.40
Do.	E. D. D.	64.6	.39
			[1] .40(3)

[1] This is additional to the absorbing power of the seat and floor area covered by the person. Correction on this account makes the "absolute" absorbing power per person 0.44—that is, 0.44 of the absorbing power of a square meter of open window.
E. H. H.

In view of the difficulties of the experiment the consistency of the determination is gratifying. The average result of the six determinations is probably correct within 2 per cent.

The following passage illustrates the nicety of observation aimed at and attained in these tests:

Under certain circumstances the audience will not be compactly seated, but will be scattered about the room and more or less isolated, for example, in a council room, or in a private music room, and it is evident that under these conditions the individual will expose a greater surface to the room and his absorbing power will be greater. It is a matter of the greatest ease to distinguish between men and women coming into a small room, or even between different men. In fact, early in the investigation, two months' work—over three thousand observations—had to be discarded because of failure to record the kind of clothing worn by the observer. The coefficients given in the following table are averages for three women and for seven men, and were deduced from experiments in the constant-temperature room.

Absorbing power of an audience

Audience per square meter	0.96
Audience per person	.44
Isolated woman	.54
Isolated man	.48

Evidently, the absorbing power of an "isolated woman" as compared with that of an "isolated man" must vary with the fashions in clothing.

Along with this study of the absorbing power of different objects and materials went the determination of a mathematical formula[8]—

$$t = k \div (a + x),$$

in which t is the number of seconds the residual sound lasts—the "duration of audibility,"—k is a constant quantity depending on the size of the room, a is the absorbing power of the bare walls, floor, and ceiling, and x is the increase of absorbing power due to the furniture and audience.

Studying rooms of different shapes and sizes, Sabine found that k is approximately $0.171V$, where V is the volume of a room in cubic meters. Then, knowing the absorbing power of different surfaces and the area of these several surfaces, he could calculate, even in advance of construction, how long the reverberation would last in a given auditorium.

Of course this feature is not the whole of architectural acoustics, but it is one of the most important parts, and it is the only part discussed in the following passage relating to the design of Boston Symphony Hall, the first auditorinm to be affected, in construction, by Sabine's advice. It is greatly to the credit of the distinguished architects of this building, Messrs.

[8] This is the simple, approximate, formula to indicate the nature of the relation discovered. The exact formula is much too complicated for useful reproduction here.

McKim, Mead & White, that they were the first of their profession to appreciate the acoustic studies made by a young and little-known physicist and to change their plans in accordance with his criticisms. The following is a part of his account of the matter:

In a theatre for dramatic performances, where the music is of entirely subordinate importance, it is desirable to reduce the reverberation to the lowest possible value in all ways not inimical to loudness; but in a music hall, concert room, or opera house, this is decidedly not the case. To reduce the reverberation in a hall to a minimum, or to make the conditions such that it is very great, may, in certain cases, present practical difficulties to the architect—theoretically it presents none. To adjust, in original design, the reverberation of a hall to a particular and approved value requires a study of conditions, of materials, and of arrangement, for which it has been the object of the preceding papers to prepare.

It is not at all difficult to show *a priori* that in a hall for orchestral music the reverberation should neither be very great, nor, on the other hand, extremely small. However, in this matter it was not necessary to rely on theoretical considerations. Mr. Gericke, the conductor of the Boston Symphony Orchestra, made the statement that an orchestra, meaning by this a symphony orchestra, is never heard to the best advantage in a theatre, that the sound seems oppressed, and that a certain amount of reverberation is necessary. An examination of all the available plans of the halls cited as more or less satisfactory models, in the preliminary discussion of the plans for the new hall, showed that they were such as to give greater reverberation than the ordinary theatre style of construction. While several plans were thus cursorily examined the real discussion was based on only two buildings—the present [old] Boston Music Hall and the Leipzig Gewandhaus; one was familiar to all and immediately accessible, the other familiar to a number of those in consultation, and its plans in great detail were to be found in *Das neue Gewandhaus in Leipzig, von Paul Gropius und H. Schmieden.* It should, perhaps, be immediately added that neither hall served as a model architecturally, but that both were used rather as definitions and starting points on the acoustical side of the discussion. The old Music Hall was not a desirable model in every respect, even acoustically, and the Leipzig Gewandhaus, having a seating capacity about that of Sanders Theatre, 1,500, was so small as to be debarred from serving directly, for this if for no other reason.

The history of the new hall is about as follows: A number of years ago, when the subject was first agitated, Mr. McKim prepared plans and a model along classical lines of a most attractive auditorium, and afterwards, at Mr. Higginson's instance, visited Europe for the purpose of consulting with musical and scientific authorities in France and Germany. But the Greek theatre as a music hall was an untried experiment, and because untried was regarded as of uncertain merits for the purpose by the conductors consulted by Mr. Higginson and Mr. McKim. It was, therefore, abandoned. Ten years later, when the project was again revived, the conventional rectangular form was adopted, and the intention of the building committee was to follow the general proportions and arrangement of the Leipzig Gewandhaus, so enlarged as to increase its seating capacity about seventy per cent; thus making it a little more than equal to the old hall [of Boston]. At this stage calculation was first applied.

The often-repeated statement that a copy of an auditorium does not necessarily possess the same acoustical qualities is not justified, and invests the subject with an unwarranted mysticism. The fact is that exact copies have rarely been made, and can hardly be expected. The constant changes and improvements in the materials used for interior construction in the line of better fire-proofing—wire lath or the application of the plaster directly to tile walls—have led to the taking of liberties in what were perhaps regarded as nonessentials; this has resulted, as shown by the tables, in a changed absorbing power of the walls. Our increasing demands in regard to heat and ventilation, the restriction on the dimensions enforced by location, the changes in size imposed by the demands for seating capacity, have prevented, in different degrees, copies from being copies, and models from successfully serving as models. So different have been the results under what was thought to be safe guidance—but a guidance imperfectly followed—that the belief has become current that the whole subject is beyond control. Had the new Music Hall been enlarged from the Leipzig Gewandhaus to increase the seating capacity seventy per cent, which, proportions being preserved, would have doubled the volume, and then built, as it is being built, according to the most modern methods of fireproof construction, the result, unfortunately, would have been to confirm the belief. No mistake is more easy to make than that of copying an auditorium—but in different materials or on a different scale—in the expectation that the result will be the same. Every departure must be compensated by some other—a change in material by a change in the size or distribution of the audience, or perhaps by a partly compensating change in the material used in some other part of the hall—a change in size by a change in the proportions or shape. For moderate departures from the model such compensation can be made, and the model will serve well as a guide to a first approximation. When the departure is great the approved auditorium, unless discriminatingly used, is liable to be a treacherous guide. In this case the departure was necessarily great.

The comparison of halls should be based on the duration of the residual sound after the cessation of a source that has produced over the hall some standard average intensity of sound—say one million times the minimum audible intensity, $1{,}000{,}000i'$.

From the known dimensions and materials of the Leipzig Gewandhaus, Sabine found 2.30 seconds as the duration of reverberation of tone C_4, 512 vibrations, therein, and his calculations foretold that the new Symphony Hall, the architects following his suggestions, would reverberate

2.31 seconds with the same tone. The practical results satisfied the director, Mr. Gericke, at once and have now been approved by the audiences of many years.

It is interesting to note, in passing, that Messrs. Wheelwright and Haven, architects in 1902 of a new building for the New England Conservatory of Music, called on Sabine for help in regard to the acoustics of moderate-sized rooms intended for piano practice. Seeing that he had to do here with a question of musical taste, not one of physics merely, he experimented with five different rooms, varying the furnishings in each till a jury of musicians declared themselves satisfied. Then, measuring the duration of reverberation of C_4 in each of the rooms, as approved, he found the shortest time to be 0.95 second and the longest 1.16 seconds, the mean being 1.08 seconds, less than one-half the approved time for a symphony concert hall.

Sabine tells us (p. 199 of his Collected Papers) that investigation of the absorption coefficients of different materials for the single note of violin C, the "C_4" mentioned above, "required every other night from twelve until five for a period of three years,"[9] yet this study, vastly important as it had proved to be, was not enough:

It can be shown readily that the various materials of which the walls of a room are constructed and the materials with which it is filled do not have the same absorbing power for all sounds regardless of pitch. Under such circumstances the previously published work with C_4 512 must be regarded as an illustration, as a part of a much larger problem—the most interesting part, it is true, because near the middle of the scale, but, after all, only a part. Thus a room may have great reverberation for sounds of low pitch and very little for sounds of high pitch, or exactly the reverse; or a room may have comparatively great reverberation for sounds both of high and of low pitch and very little for sounds near the middle of the scale. In other words, it is not putting it too strongly to say that a room may have very different quality in different registers, as different as does a musical instrument; or, if the room is to be used for speaking purposes, it may have different degrees of excellence or defect for a whisper and for the full rounded tones of the voice, different for a woman's voice and for a man's—facts more or less well recognized. Not to leave this as a vague generalization, the following cases may be cited. Recently, in discussing the acoustics of the proposed cathedral of southern California in Los Angeles with Mr. Maginnis, its architect, and the writer, Bishop Conaty touched on this point very clearly. After discussing the general subject with more than the usual insight and experience, possibly in part because Catholic churches and cathedrals have very great reverberation, he added that he found it difficult to avoid pitching his voice to that note which the auditorium most prolongs notwithstanding the fact that he found this the worst pitch on which to speak. This brings out, perhaps more impressively because from practical experience instead of from theoretical considerations, the two truths that auditoriums have very different reverberation for different pitches, and that excessive reverberation is a great hindrance to clearness of enunciation. Another incident may also serve, that of a church near Boston, in regard to which the writer has just been consulted. The present pastor, in describing the nature of its acoustical defects, stated that different speakers had different degrees of difficulty in making themselves heard; that he had no difficulty, having a rather high pitched voice; but that the candidate before him, with a louder but much lower voice, failed of the appointment because unable to make himself heard.

Accordingly, about 1900, Sabine undertook an extension of his investigations to cover nearly "the whole range in pitch of the speaking voice and of the musical scale," from C_1, of 64 vibrations per second, to C_7, of 4,096 vibrations. Carrying meanwhile his full share of work as a conscientious teacher and seizing time and opportunity as best he could for research, he was engaged about five years with this new labor.

The difficulties and devices of the undertaking can best be shown by certain quotations:

In the very nature of the problem, the most important datum is the absorption coefficient of an audience, and the determination of this was the first task undertaken. By means of a lecture on one of the recent developments of physics, wireless telegraphy, an audience was thus drawn together and at the end of the lecture requested to remain for the experiment. In this attempt the effort was made to determine the coefficients for the five octaves from C_2 128 to C_6 2048, including notes E and G in each octave. For several reasons the experiment was not a success. A threatening thunderstorm made the audience a small one, and the sultriness of the atmosphere made open windows necessary, while the attempt to cover so many notes, thirteen in all, prolonged the experiment beyond the endurance of the audience. While this experiment failed, another the following summer was more successful. In the year that had elapsed the necessity of carrying the investigation further than the limits intended became evident, and now the experiment was carried from C_1 64 to C_7 4096, but included only the C notes,

[9] It is not surprising that in 1899 Sabine had an attack of appendicitis that very nearly proved fatal. With his strange, fanatical disregard for pain, he had ignored all warning symptoms, had kept on his feet, and was out of doors, on Cambridge Common I believe, when the abscess broke. As soon as he was out of immediate danger he laughed at this adventure and announced his intention of making a record recovery, which he did. He was two weeks in the hospital, one week at home, and then began the work of the opening college year, "blithely and smilingly with springing step," to use his mother's words.

seven notes in all. Moreover, bearing in mind the experiences of the previous summer, it was recognized that even seven notes would come dangerously near overtaxing the patience of the audience. Inasmuch as the coefficient of absorption for C_4 512 had already been determined six years before, in the investigations mentioned, the coefficient for this note was not redetermined. The experiment was therefore carried out for the lower three and the upper three notes of the seven. The audience, on the night of this experiment, was much larger than that which came the previous summer, the night was a more comfortable one, and it was possible to close the windows during the experiment. The conditions were thus fairly satisfactory. In order to get as much data as possible, and in as short a time, there were nine observers stationed at different points in the room. These observers, whose kindness and skill it is a pleasure to acknowledge, had prepared themselves, by previous practice, for this one experiment.

The next experiment was on the determination of the absorption of sound by wood sheathing. It is not an easy matter to find conditions suitable for this experiment. . . . Quite a little searching in the neighborhood of Boston failed to discover an entirely suitable room. The best one available adjoined a night lunch room. The night lunch was bought out for a couple of nights, and the experiment was tried. The work of both nights was much disturbed. The traffic past the building did not stop until nearly two o'clock, and began again at four. The interest of those passing on foot throughout the night, and the necessity of repeated explanations to the police, greatly interfered with the work.

The main purpose of Sabine's work was, of course, utilitarian, though in a highly refined sense, and we shall presently go on to show how he dealt with the acoustic problems of particular buildings, sometimes before and sometimes after their construction. But we must not overlook an important contribution [10] which he made in 1907 to the theory of the musical scale, or, rather, to the theory of the origin of this scale. Referring to Helmholtz and his Sensations of Tone, he says:

Having given a physical and physiological explanation of the harmony and discord of simultaneous sounds and, therefore, an explanation of the musical scale as used in modern composition, Helmholtz was met by an apparent anachronism. The musical scale, identical with the modern musical scale in all essentials, antedated by its use in single-part melody the invention of chordal composition, or, as Helmholtz expressed it, preceded all experience of musical harmony. In seeking an explanation of this early invention of the musical scale, Helmholtz abandoned his most notable contribution, and relegated his explanation of harmony and discord to the minor service of explaining a fortunate, though of course an important use of an already invented system of musical notes. The explanation of the original invention of the musical scale and its use in single-part music through the classical and the early Christian eras, he sought for in purely aesthetic considerations—in exactly those devices from which he had just succeeded in rescuing the explanation of harmony and discord.

Sabine's explanation of the anachronism that troubled Helmholtz might, perhaps, be surmised by anyone who had read the preceding pages, but it is well to give it in his own words:

In many rooms of ordinary construction the prolongation of audibility amounts to two or three seconds, and it is not exceedingly rare that a sound of moderate initial intensity should continue audible for eight, nine, or even ten seconds after the source has ceased. As a result of this, single-part music produced as successive separate sounds is, nevertheless, heard as overlapping, and at times as greatly overlapping tones. Each note may well be audible with appreciable intensity not merely through the next, but through several succeeding notes. Under such conditions we have every opportunity, even with single-part music, for the production of all the phenomena of harmony and discord which has been discussed by Helmholtz in explanation of the chordal use of the musical scale. In any ordinarily bare and uncarpeted room, one may sing in succession a series of notes and then hear for some time afterward their full chordal effect.

But Sabine goes further and suggests a physical explanation of the differences of musical scale developed by different races:

Housed or unhoused, dwelling in reed huts or in tents, in houses of wood or of stone, in houses and temples high vaulted or low roofed, of heavy furnishing or light, in these conditions we may look for the factors which determine the development of a musical scale in any race, which determine the rapidity of the growth of the scale, its richness, and its considerable use in single-part melody.

We have explained for us by these figures [absorptive powers of various materials] why the musical scale has but slowly developed in the greater part of Asia and of Africa. Almost no traveler has reported a musical scale, even of the most primitive sort, among any of the previously unvisited tribes of Africa. This fact could not be ascribed to racial inaptitude. If melody was, as Helmholtz suggested, but rhythm in time and in pitch, the musical scale should have been developed in Africa if anywhere. These races were given to the most rhythmical dancing, and the rhythmical beating of drums and tomtoms. Rhythm in time they certainly had. Moreover, failure to develop a musical scale could not be ascribed to racial inaptitude to feeling for pitch. Transported to America and brought in contact with the musical scale, the negro became immediately the most musical part of our population. The absence of a highly developed scale in Africa must then be ascribed to environment.

[10] Vice presidential address, Section B, American Association for the Advancement of Science, Chicago, 1907.

Turning to Europe we find the musical scale most rapidly developing among the stone-dwelling people along the shores of the Mediterranean. The development of the scale and its increased use kept pace with the increased size of the dwellings and temples. It showed above all in their religious worship, as their temples and churches reached cathedral size. The reverberation which accompanied the lofty and magnificent architecture increased until even the spoken service became intoned in the Gregorian chant. It is not going beyond the bounds of reason to say that in those churches in Europe, which are housed in magnificent cathedrals, the Catholic, the Lutheran, and Protestant Episcopal, the form of worship is in part determined by their acoustical conditions.

The passages thus far quoted from Sabine's writings say little about the form of auditoriums or the position of absorbing or reflecting surfaces, and they might lead one to think these matters unimportant. This would be a serious mistake. When he came to deal with an auditorium so irregular in shape as a modern theater, with boxes and two or three deep galleries, under the requirement that words spoken on the stage should be easily heard everywhere, the problem was not merely, or perhaps mainly, that of reverberation. It was necessary to consider whether the details of shape and surface, actual or proposed, were such as to kill off the sound by "interference" in certain places or to confuse it by an echo.

This problem was not difficult, though it was laborious, for Sabine, when he was consulted in advance of the construction, as he was in the case of the Little Theater in New York City or the Scollay Square Theater in Boston.

As to the plan of the New Theater in New York, his advice was asked only after construction of the building. The architects had undertaken the task of making a very large auditorium suitable for both opera and drama, and, naturally, had not been entirely successful. Sabine advised certain changes of position of boxes and foyer chairs and recommended lowering the ceiling. The minor changes proposed were made, but instead of lowering the ceiling, the architects suspended beneath it a large oval canopy, 70 by 40 feet, with good effect.

Concerning open-air theaters, Sabine remarks, after observing that the presence of an audience diminishes reverberation:

But in the Greek theatre, occupied or unoccupied, ruined or in its original form, there was very little reverberation. In fact, this was its merit. On the other hand, the very fact that there was little reverberation is significant that there was very slight architectural reenforcement of the voice. One might well be unconvinced by such *a priori* considerations were there not excellent evidence that these theatres were not wholly acceptable acoustically even in their day, and for drama written for and more or less adapted to them. Excellent evidence that there was insufficient consonance [11] is to be found in the megaphone mouthpieces used at times in both the tragic and the comic masks, and in the proposal by Vitruvius to use resonant vases to strengthen the voice.

Apparently the acoustical difficulties presented by churches, though often serious, are not usually so hard to deal with as those found in theaters, perhaps because church audiences are not so insistent as theater audiences on hearing every word that is uttered. The first of the following passage describes one interesting church case:

Among a number of interesting problems in advance of construction, the firm of McKim, Mead & White has brought some interesting problems in correction, of which three will serve admirably as examples because of their unusual directness. The first is that of the Congregational Church in Naugatuck, Connecticut. . . . When built its ceiling was cylindrical, as now, but smooth. Its curvature was such as to focus a voice from the platform upon the audience—not at a point, but along a focal line, for a cylindrical mirror is astigmatic. The difficulty was evident with the speaking, but may be described more effectually with reference to the singing. The position of the choir was behind the preacher and across the main axis of the church. On one line in the audience, crossing the church obliquely from right to left, the soprano voice could be heard coming even more sharply from the ceiling than directly from the singer. The alto starting nearer the axis of the church had for its focus a line crossing the church less obliquely. The phenomena were similar for the tenor and the bass voices, but with focal lines crossing the church obliquely in opposite directions. The difficulty was in a very large measure remedied by coffering the ceiling, . . . both the old and the new ceiling being of plaster.

The hall of the House of Representatives in the Rhode Island State capitol illustrated another type of difficulty. In considering this hall it is necessary to bear in mind that the problem is an essentially different one from that of a church or lecture-room. In these the speaking is from a raised platform and a fixed position. In a legislative assembly the speaking is in the main from the floor, and may be from any part of the floor.

In this legislative hall Sabine diagnosed the trouble as an effect not very different from an echo, due to reflection first from wall to wall and then from ceiling to audience.

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[11] Sabine takes this word from Vitruvius, *De Architectura*, Liber V, Cap. VIII. It means action by which the voice is "supported and strengthened."

The difficulty was remedied in this case by a change in material without change of form, by diminishing the reflecting power of the two side walls. This was done by placing a suitable felt on the plaster walls between the engaged columns, and covering it with a decorated tapestry.

It is interesting to note that this treatment applied to the lower half of the walls would not have been acoustically effective.

The lecture-room of the Metropoltian Museum of Art illustrates the next step in complexity. . . . In this room the reverberation was not merely excessive, but it resolved itself by focusing into a multiple echo, the components of which followed each other with great rapidity but were distinctly separable. The number distinguishable varied in different parts of the hall. Seven were distinguishable at certain parts.

Certain remedial measures were taken on Sabine's advice, and the result was the one predicted—

the reduction of the disturbance to a single and highly localized echo. This echo is audible only in the central seats—two or three seats at a time—and moves about as the speaker moves, but in symmetrically opposite direction. Despite this residual effect, and it should be noted that this residual effect was predicted, the result is highly satisfactory to Dr. Edward Robinson, the director of the museum, and the room is now used with comfort, whereas it had been for a year abandoned.

This is perhaps the most fitting place for reference to Sabine's exceedingly interesting chapter on "Whispering galleries," which is to be found at present only in the volume of his Collected Papers on Acoustics, issued in 1922 by the Harvard University Press. I should like to reproduce here the whole of it, if such a proceeding were consistent with the usual scope of memoirs like the one now in hand, but I can give only two or three short passages:

It is probable that all existing whispering galleries, it is certain that the six more famous ones, are accidents; it is equally certain that all could have been predetermined without difficulty, and like most accidents, could have been improved upon. That these six, the dome of St. Paul's Cathedral in London, Statuary Hall in the Capitol at Washington, the vases in the Salle des Cariatides in the Louvre in Paris, St. John Lateran in Rome, the Ear of Dionysius at Syracuse, and the Cathedral of Girgenti, are famous above others is in a measure due to some incident of place or association.

The ceiling of the Hall of Statues [in Washington], with the exception of a small circular skylight, is a portion of an exact sphere with its center very nearly at head level. . . .

In citing this gallery in an article on "Whispering galleries" in Sturgis's Dictionary of Architecture, the writer made the statement that "The ceiling, painted so that it appears deeply panelled, is smooth. Had the ceiling been panelled the reflection would have been irregular and the effect very much reduced." A year or so after this was written the fire in the Capitol occurred, and in order to preserve the whispering gallery, which had become an object of unfailing interest to visitors to the Capitol, the new ceiling was made "to conform within a fraction of an inch" to the dimensions of the ceiling which it replaced. Notwithstanding this care, the quality of the room which had long made it the best and the best known of whispering galleries was in large measure lost. Since then this occurrence has been frequently cited as another of the mysteries of architectural acoustics and a disproof of the possibilities of predicting such phenomena. As a matter of fact, it was exactly the reverse. Only the part between the panels was reproduced in the original dimensions of the dome. The ceiling was no longer smooth, the staff was panelled in real recess and relief, and the result but confirmed the statement recorded nearly two years before in the Dictionary of Architecture.

Almost any wall-surface is a much more perfect reflector of sound than the most perfect silver mirror is to light. In the former case, the reflection is over 96 per cent, in the latter case rarely over 90.

On the surfaces of the two mirrors scratches to produce equally injurious effects must be comparable in their dimensions to the lengths of the waves reflected. Audible sounds have wave lengths of from half an inch to sixty feet; visible light of from one forty-thousandth to one eighty-thousandth of an inch. Therefore while an optical mirror can be scratched to the complete diffusion of the reflected light by irregularities of microscopical dimensions, an acoustical mirror to be correspondingly scratched must be broken by irregularities of the dimensions of deep coffers, of panels, of engaged columns, or of pilasters.

From this last passage, and from others declaring that a rough plastered surface acts practically just like a smooth one, so far as sound waves are concerned, one might infer that merely painting a solid wall could have no appreciable effect on its acoustic properties; but this would be an error. Sabine had found the absorptive power of a painted brick wall to be only about half that of the same kind of wall unpainted. This is because of the porosity of natural brick, which enables the sound waves to penetrate the material slightly and so lose a little of their energy. Gradually, through Sabine's suggestions and "the skill and great knowledge of ceramic processes" possessed by Mr. Raphael Guastavino, a kind of tile was developed which "has over sixfold the absorbing power of any existing masonry construction and one-third the absorbing power of the best-known felt."

The words just quoted are found in a paper printed in 1914. We may say that, so far as the properties of auditoriums are concerned, Sabine had, in less than 20 years, brought architectural acoustics from the empirical state, in which success with any new structure was a happy accident and failure was a misfortune often made ridiculous by such attempted remedies as the stringing of wires, to the status of a reasoned science and a precise art.

He had done this by force of his own qualities, with but little favor of circumstances and with so little financial assistance or reward that he was probably a poorer man by thousands of dollars than he would have been had he never attempted it. Moreover, he had published his formulas and his devices freely [12] to the world, for anyone to use who could, patenting only, and this with Mr. Guastavino, the kind of tile described above.

But one great practical difficulty in acoustics remained, the nature of which is shown in the following passage quoted from a paper of Sabine's printed in 1915:

> The insulation of sound as an unsolved problem in architectural acoustics was first brought to the writer's attention by the New England Conservatory of Music, immediately after its completion in 1904, and almost simultaneously in connection with a private house which had just been completed in New York. A few years later it was renewed by the Institute of Musical Art in New York. In the construction of all three buildings it had been regarded as particularly important that communication of sound from room to room should be avoided, and methods to that end had been employed which were in every way reasonable. The results showed that in this phase of architectural acoustics also there had not been a sufficiently searching and practical investigation and that there were no experimental data on which an architect could rely.

In some respects the attempts of well-known architects at sound insulation had been weirdly unsuccessful. Thus, concerning the private house mentioned above, we have the following particulars:

> It was practically a double house, one of the most imperative conditions of the building being the exclusion of sounds in the main part of the house from the part to the left of a great partition wall. In the basement of the main dwelling was the servants' dining room. Rapping with the knuckles on the wall of this room produced in the bedroom, two stories up and on the other side of the great partition wall, a sound which, although hardly, as the architect expressed it, magnified, [was] yet of astonishing loudness and clearness.

Sabine's analysis of the sound insulation problem, perhaps the most difficult that he encountered in acoustics, is well shown in the following paragraph, which, though not put in quotation marks, is made up almost entirely of parts to be found in his paper:

The transmission of sound from one room to another involves three steps—the taking up of the vibration from the air by the solid partition, its transmission through the partition, and its communication to the air of the receiving room. In the case of a solid masonry wall, the transmission from surface to surface is almost perfect; but, because of the great mass and rigidity of the wall, it takes up but little of the vibration of the incident sound. In the case of multiple screen walls, the communication from wall to wall, through the intermediate air space or around the edges, is poor compared with the face to face communication of a solid wall. But the vibration of the screen wall exposed to the sound, the initial step in the process of transmission, is greatly enhanced by its light and flexible character. Similarly its counterpart, the screen wall which by its vibration communicates the sound to the receiving room, is light, flexible, and responsive to relatively small forces. This responsiveness of the screens may compensate or more than compensate for the poor communication between them.

In his studies of sound insulation Sabine used layers of felt, then sheet-iron partitions with air spaces between, then such partitions with layers of felt and of air between. With the last combination, made about 6 inches thick in all, sound of 1,000,000 times minimum audibility

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[12] At least one individual thought Sabine was neglecting a golden opportunity. In 1909 a certain young architect, ———, who had dealt successfully, by means of Sabine's formulas and methods, with the acoustic troubles of a certain synagogue and had received a good round sum for his work, visited him, asked him many technical questions, and tried to get a monopoly of his advice for a company ——— proposed to form. Sabine declining this proposition, ——— then undertook to patent, as his own, all of Sabine's formulas and methods. The first intimation to Sabine of this maneuver came in a letter from Mr. Mead, of McKim, Mead & White, saying that ——— had served an injunction on his firm to restrain it from using suggestions made by Sabine for improvement of the acoustic qualities of the lecture room of the Metropolitan Museum of Fine Arts. Fortunately ——— had been a little too enterprising, for though, strangely enough, his claims had been allowed, the patents had not yet been issued. Prompt and vigorous action, in which Mr. Frederick P. Fish, one or two United States Senators, and, I believe, President Taft, had a part, checked Mr. ——— at the Patent Office. In the subsequent legal proceedings testimony of the most positive character was given, showing that Sabine had, previous to 1908, prescribed alterations for improvement of the acoustic properties of seven auditoriums and that in all of these the changes made had been proved successful by years of experience. No evidence was offered by ———, and his application for patents was finally rejected.

on one side of the barrier gave sound of 88 times minimum audibility on the other side. The sound used in this test was that of violin C, 512 vibrations per second.

Sabine makes the interesting observation that, for a thickness of 5 or 6 inches, the combination above described is far superior in absorptive effect to an equal thickness of felt; but felt of 10.4 inches thickness "would entirely extinguish a sound of the intensity of ordinary speech," whereas 10.4 inches of the combination described would not "accomplish this ideal result."

Notable as are these evidences of progress with the problem of sound insulation, it is plain that Sabine regarded his work in this field as only well begun. His chapter on the "Insulation of sound" ends thus:

> At this point the apparatus was improved, the method recast, and the investigation begun anew, thenceforward to deal only with standard forms of construction, and for sounds, not of one pitch only, but for the whole range of the musical scale.

These words, published in 1915, were doubtless written in anticipation of opportunities soon to be afforded him by the liberality of a friend. Colonel Fabyan, an energetic and successful merchant of Chicago, a man of various avocations and enthusiasms, had somehow become acquainted with and deeply interested in Sabine's acoustic investigations. He offered to build, and during the war he did build, at Geneva, Ill., a research laboratory to suit exactly Sabine's purposes. It was planned for the prosecution of work under Sabine's direction, and no expense necessary to the success of this work was to be spared. In fact, this research laboratory was intended by Colonel Fabyan to be also a kind of shrine or temple in celebration of Sabine himself, and to this end there were inscribed on its outer walls four words, names of virtues found in Sabine, and so chosen that the initials of these words were his initials. Sabine smiled appreciatingly and went on with his plans, taking such opportunities as came to him during the stress of war days. Seldom have two so unlike men worked together so happily for a beneficent purpose.

Sabine was not much given to putting his ideas on paper until they were in final shape. So after his death, in 1919, no elaborated scheme of the work he had proposed to do in the new laboratory could be found. Colonel Fabyan, resolved to do all he could to carry out the enterprise in which he and Sabine had engaged with such high hopes, enlisted Dr. Paul E. Sabine, who is a cousin of Wallace, to assume charge of the new laboratory and undertake the work it was intended to accomplish.

In 1900 Sabine married Miss Jane Downs Kelly, originally of New Bedford, Mass. She was a physician of established reputation in Boston before her marriage, and she continued to practice after marriage, especially in connection with the Children's Hospital. The fact that she found time and energy for this professional occupation, while performing with rare competence the duties of a housekeeper and mother of a family, is sufficient evidence of her unusual combination of qualities.

Two daughters,[13] children such as one might hope to see from such parents, were the issue of this marriage—Janet, born in 1903, and Ruth in 1905. In them Sabine's fervently affectionate nature and his fatherly pride rejoiced.

For many years after he became a member of the Harvard Faculty of Arts and Sciences Sabine took no prominent part in its meetings. His colleagues were therefore somewhat surprised when, late in 1905 or early in 1906, he proposed a radical change in the organization of the Lawrence Scientific School, which was under the control of this faculty. Discussion of the affairs of this school was active at this time, because proceeds from the great McKay bequest, intended for the promotion of science teaching at Harvard, were about to come in, and one of the periodical agitations regarding relations between the Lawrence Scientific School and the Massachusetts Institute of Technology had recently occurred.

Sabine's proposition, or the plan growing out of his proposition, was to establish a graduate school of applied science, Harvard College taking over from the scientific school the instruction leading to the degree of bachelor of science. This proposal was approved by the Faculty of

[13] The younger, not thoroughly strong and sorely afflicted by the loss of her father, died suddenly in 1922.

Arts and Sciences, and it was adopted by the governing boards of the university in the spring of 1906. Professor Shaler, who had been dean of the Lawrence Scientific School for many years, died in April of that year, and Professor Sabine was made dean of the new graduate school of applied science.

Sabine took this office at the urgent request of President Eliot, but reluctantly and doubtless with misgivings, being a teacher and student by nature, not an executive, not a manager of men. In one respect he was, perhaps, little fitted for administrative duties. His nature was intense and reserved. Regarding men, and often regarding measures, he had convictions rather than opinions. Dispassionate argument was difficult for him, though he lacked the instinct and temper of the dictator. So the duties of a dean, the real executive head in this case, of an institution in a period of reconstruction, must have been hard for him at times, harder than they would have been for a man of different temperament.

The decision once made, he threw himself into the duties of his new position with characteristic energy, devotion, and elevation of ideals. Anyone reading his brief annual reports for the seven years of his service as dean must be impressed by the vigor of his administration and by his constant endeavors to improve the effectiveness of the departments in his charge, both by changes of plan and by a careful selection of the personnel. The school of forestry was soon put on a satisfactory basis. The Bussey Institution was transformed from a very thinly attended undergraduate school of agriculture to a place for "advanced instruction and research in the scientific problems that relate and contribute to practical agriculture and horticulture"; William Morton Wheeler was called from the Natural History Museum in New York to occupy the newly created chair of economic entomology, and other eminent specialists were enlisted in the warfare against the insect pests with which the trees of Massachusetts had been signally afflicted. Prof. George F. Swain and Prof. Harry E. Clifford were called from the Massachusetts Institute of Technology to chairs of civil engineering and electrical engineering, respectively, in 1909; but at that time "it was impossible to find a suitable appointee for the position in mechanical engineering." In 1911 George C. Whipple came from the practice of his profession in New York City to the professorship of sanitary engineering, and Prof. Eugène Duquesne, from the École des Beaux Arts in Paris, to the chair of architectural design, both of the positions thus filled being new ones at Harvard.

It seems probable that Sabine's methods would in time have built up a strong school of applied science at Harvard, in spite of the great prestige of the Massachusetts Institute of Technology, which had now come under the very able management of President Maclaurin. But President Lowell, of Harvard, who by family tradition was a trustee of the institute and whose main ambition for Harvard did not lie in the direction of applied science, either proposed or assented to the terms of the famous merger which undertook to combine the financial resources, the aims, and the teaching staffs of the two scientific schools. His influence, together with the evident economic advantages of maintaining one school instead of two in one community, prevailed, and so the fusion was, for a time, effected.

Of course this combination involved the extinction of Sabine's office as dean, and he might well have felt that it imperiled some of the aims he had been striving for. Any man, however slight may be his natural ambition for executive power, is likely to become somewhat enamored of it after some years of possession. Moreover, Sabine was strong in the respect and confidence of influential men. I have heard, though I do not profess to speak with authority on this point, that the Harvard Corporation would have rejected the proposed merger if he had opposed it. But he did not oppose, he advocated it. Did he do so with full conviction? I do not profess to know; but my conjecture is that, when once the change had been suggested and he saw that it would involve in some degree a sacrifice of himself, he was no longer able to view the matter with a free mind. Where another might have shown resentment and made opposition, he took inevitably the path of self-effacement.

Sabine was in Germany with his family in the summer 1914. Going to England when the war began, he was witness to the spirit in which the British nation roused to action and was immensely impressed by it. He met leading men of the Government, and, in recognition of his eminence as an authority on acoustics, received the remarkable compliment of being put on a committee to study the physical conditions of the House of Commons.[14]

The years 1914–15 and 1915–16 were for him a period of comparative quiet, spent in teaching and acoustic investigations, doubtless with much attention to plans for the acoustic laboratory to which reference has already been made. But for the war, he would probably have had before him a long career of growing usefulness and fame, and would have lived to a vigorous old age according to the habit of his ancestors. But from that fiery furnace into which other men were drawn by millions he could not hold himself back. He would have felt recreant if he had escaped unscathed.

Going to France in 1916, with the intention of giving a course of lectures at the Sorbonne in the fall, he engaged during the summer in relief work that took him to Switzerland. Overtaxed in strength, he was attacked during the fall by a malady that compelled the postponement of his lectures and nearly ended his life at once. When he was able to be moved, he went from Paris back to Switzerland, this time as a patient; but he gained strength, studying French constantly meanwhile, and in the spring of 1917 gave his lectures, on architectural acoustics, at the Sorbonne.

Such notes of these lectures as are available contain little that had not already been printed in English, but a few paragraphs taken from them by Dr. Theodore Lyman for use in the Collected Papers may well be given here in illustration of what Sabine considered to be, in a word, his contribution to acoustics, *the consideration of "boundary conditions"*:

In no other domain have physicists disregarded the conditions introduced by the surrounding materials, but in acoustics these do not seem to have received the least attention. If measurements are made in the open air, over a lawn, as was done by Lord Rayleigh in certain experiments, is due consideration given to the fact that the surface has an absorbing power for sound of from 40 to 60 per cent? Or, if inside a building, as in Wien's similar experiments, is allowance made for the fact that the walls reflect from 93 to 98 per cent of the sound? We need not be surprised if the results of such experiments differ from one another by a factor of more than a hundred.

It would be no more absurd to carry out photometric measurements in a room where the walls, ceiling, and even the floor and tables consisted of highly polished mirrors, than to make measurements on the intensity, or on the quantitative analysis of sound, under the conditions in which such experiments have almost invariably been executed. It is not astonishing that we have been discouraged by the results, and that we may have despaired of seeing acoustics occupy the position to which it rightly belongs among the exact sciences.

The length of the waves of light is so small compared with the dimensions of a photometer that we do not need to concern ourselves with the phenomena of interference while measuring the intensity of light. In the case of sound, however, it must be quite a different matter.

In order to show this in a definite manner, I have measured the intensity of the sound in all parts of a certain laboratory room. For simplicity, a symmetrical room was selected, and the source, giving a very pure tone, was placed in the center. It was found that, near the source, even at the source itself, the intensity was in reality less than at a distance of five feet from the source. And yet the clever experimenter, Wien, and the no less skillful psychologists, Wundt and Münsterberg, have assumed under similar conditions the law of variation of intensity with the inverse square of the distance. It makes one wonder how they were able to draw any conclusions from their measurements.

The following extract from a letter written to me August 15, 1917, gives Sabine's own comments on these lectures and some indication of the work in which he engaged when they were ended:

The lectures at the Sorbonne then began quietly. There were twenty-five in the audience the first day, and this number rose to fifty—but lecturing at the Sorbonne while the world is being transformed into a totally different institution, and life such as is left of it is sobered for years to come—lecturing at the Sorbonne seems a thing apart. I was also asked to lecture at the École des Beaux Arts, to give a public lecture, and to lecture before the Society of Architects. The latter was so kind as to give me a medal as souvenir of the occasion; they find the time and the heart to do things nicely in France, nicely and kindly even in the midst of death.

The lectures had hardly stopped when I was asked to help in the Information Bureau of the United States Navy here in Paris on the submarine question—a week later by the French Bureau des Inventions on submarine and aeroplane questions; I am also definitely on the staff of the Bureau of Research of the Air Service of the American Expeditionary Force—a long title—and I have just received a request from the British Munitions Inventions Bureau to come over to England for consultation on some problems in acoustics.

[14] He told me gleefully that he thereupon asked to be admitted to the floor of the House during a session, for the purpose of making acoustic observations, and was so admitted, greatly to the damage of that formidable English institution, precedent.

The few remaining weeks will be full ones. In two or three days I shall go to Toulon, the Mediterranean base of the French fleet, for some direct experience of the submarine problem—then to Italy and the Italian front—back to Paris and three days later to England—back to the English front, then to the French front, again back to Paris for a few days to report, and then home. This programme is sufficiently active.

In the air service I can not say that I have become a pilot, but I have become a good passenger, and this, the pilots say, is a very good thing to have along. I can also ride in a Paris taxicab without the slightest anxiety, but the other day I was taken by an American officer in a little Ford machine and my heart was in my mouth all the time.

The fact is that his sincerity, scientific acumen, and energy so won the confidence of those high in authority that he became an unofficial *liaison* agent between the French, the English, and the Italians in matters of great military importance. He told me that he found these allies were not exchanging information concerning airplane practice freely, and I believe he said that he personally brought to pass the first official communication between the English and the Italians on this matter.

September 2, 1917, he wrote to his mother:

This summer has been wonderful. It *is* a pleasure to be wanted by the French, British, and Italian Governments as well as by our own. It is a pleasure to be of service. And along with it have come some wonderfully interesting experiences. I was in the last great Italian offensive on the Isonzo—the Carso—with the shells flying overhead in both directions. In a great bombarding aeroplane I was down over the Adriatic and Trieste and later up over and into the Alps. Tomorrow I go out over the Mediterranean in a dirigible, Tuesday from Genoa in a hydro-aeroplane and Wednesday from Toulon in a submarine. Everything has been opened to me.

I venture the opinion that, when he reached America in the fall of 1917, he knew as much about the varied phases of airplane warfare as any other one man of that time.

Fortunately he did not have to wait long for a hearing in Washington. The authorities there saw the value of the information he had brought home and placed him at once in the innermost circles [15] having to do with airplane production and use. It is a great satisfaction to record that, during all the anxious and impatient months that followed, he spoke with enthusiasm of the devotion and ability of those with whom he was thus brought into the closest relations; and, when some of them were afterward virtually placed on trial for misconduct, he stoutly affirmed their merits, anticipating the verdict which the public has now reached regarding them.

Trying to carry at once his work of teaching in Cambridge and his duties as general adviser, information expert, and adjuster of personal relations in Washington, he was constantly taxed beyond the safety limit of his strength. Weekly, while the college year lasted, he would come to Harvard for two or three days, and weekly he would be summoned back to Washington by telegram. During the whole summer of 1918 he made only one or two visits, and these very brief, to his family.

When the college year 1918–19 opened, he saw in the existence of the Students' Army Training Corps at Harvard conditions which appealed to his imagination with such force that he broke away from his regular Washington engagements. Into the work of teaching, teaching for war, he plunged with a crusading [16] enthusiasm, quite prepared to give his life, if need be, in the effort. I tried, as his wife tried, to make him see that failure to meet a class or to take a train for Washington, when he had, let us say, a temperature of 102°, was not quite so base conduct as deserting the front in the crisis of a battle, but such remonstrances made little im-

[15] The following is an extract from a letter written February 8, 1919, to Professor Sabine's mother by Col. Edward A. Deeds, from the office of the Director of Military Aeronautics in Washington: "During my first conference with him I was so impressed that I immediately made him one of my staff, giving him a desk in an adjoining office. All cablegrams regarding apparatus passed over his desk. He kept our Allies informed of our progress and in turn interpreted their development to us. His judgment was considered so good that within a few months he was made the final authority to select from the samples sent from overseas, of instruments to be put in production."

[16] In addition to his obvious engagements, he had in mind, as I know from a hint he let fall, some desperate enterprise which I took to be an airplane expedition on a great scale, for dropping immense quantities of high explosives in a place where they would he most likely to end the war. In 1922 I told his mother of this surmise of mine, and she appeared almost shocked, saying gently, "I should think he would have wanted to prevent that." A few days later she said to me in the same gentle way, "I would have opened the earth and let the Germans down." Verily, the militant Quaker is a formidable figure.

pression on him; so long as he could stand, he would do his work. It is a pleasure to reflect that his students, if they did not prove themselves altogether worthy of his efforts in their behalf, did at least give him their admiring attention and their love.

Near the beginning of the year 1919 he underwent a surgical operation, too long delayed. About 10 days before his death, which came on January 10, 1919, I heard of his serious illness and called his house by telephone for further particulars. Sabine himself at once answered me in a voice so cheerful and strong that my fears might have been dissipated if I had not remembered that on a previous occasion, after going home from the laboratory unmistakably ill, he had answered my inquiries in the same way. This was, in fact, his method of censoring health bulletins relating to himself.

A day or two later, not venturing to use the telephone again, I called at his door and asked for Mrs. Sabine. Being told that I could not see her, I left my name and was turning away, when I was called from within the house, and Sabine, who had heard me at the door, came halfway down the stairway from the second story to meet me. Finding that he really wished to have a talk with me, I went to his room. He must have known his condition to be one of great danger, but evidently he was not submissive to the ordinary rules of the sick chamber. Most of the time while I was with him he reclined, half sitting, on a couch, apparently taking whatever position was most tolerable, and I knew it would be useless to offer advice. In his care, or lack of care, for his own health he was now, as he had been during all the 30 years of my acquaintance with him, a defier of precept, a law unto himself.

Any time for the past year or two, looking upon his spiritual, still youthful, face, and noting the smiling obstinacy with which he followed a course of toil that must end his life too soon, one might be tempted to think of him as some elfin being that had taken human form in benevolent caprice, but was now planning departure and adventures new. Not that he ever, save in the very ecstasy of pain and weakness, showed any symptom of world weariness. He was full of affection, full of the zest of life, full of plans for future years. He told me that he never enjoyed his work of teaching more than during the fall just past, so trying to most of those who remained in academic life, and he had been looking forward joyfully to the prospect of resuming his work of research, especially that part of it which was to be carried on in the special laboratory built for him by his friend Colonel Fabyan, at Geneva, Ill.

From his ancestry he should have had long life, and he probably counted too much on this inheritance. He had lived through more than one tremendous crisis of illness, and he seemed to feel that he could brave off any attack of disease. But even if he had seen death unmistakable in his path, he would not have halted or turned aside so long as the war lasted. In fact, he had been repeatedly warned that a surgical operation was needed to save his life, and had replied that he could not stop for this while his country was in danger.

With all his high courage and resolution, however, and a clearness of head likely to take him in safety through difficult passes, he was no seeker of danger for its own sake, no sportsman, in the ordinary sense, no player of rough games. Indeed, during his early years at Harvard, the slightness of his figure, the delicacy of his face, the deferential courtesy of his manner, may have raised in the minds of some the question whether he was fitted for the not always easy task of teaching and controlling a large class of possibly boisterous undergraduates. But this solicitude was quite gratuitous. He was the son of a woman who, at 70 years or more, described her own way of crossing the proverbially dangerous streets of Paris thus: "I have no difficulty; I wait till the street is fairly clear and then I walk across, looking neither to the right nor to the left." So Sabine, telling how to deal with an incipient lecture-room disturbance, said: "It is perfectly easy; all you have to do is to survey the audience and look every man in the eye."

I suppose, however, that he was rarely obliged to use even this measure of discipline. For young men were drawn to him; he spoke in a low, though clear, tone, and they kept still in order not to lose his words; they clustered about him after his lectures, partly to hear more and partly, I suspect, for the mere pleasure of being near him. They took his advice about their studies and their life work, and they could not have done better.

We say of such a man, it is a pity he died so young; if he had taken care of himself, had been regular in his meals and in his hours of sleep, he would have had a long as well as a useful life. Yes; but a man must work according to his nature, and Sabine's temperament was not that of the ordinary man, not that of the ordinary scientific investigator. Some of the high things he did could not have been done by a man who must be regular at his meals and regular in his hours of sleep. When we remember how long the plagues he grappled with had baffled the efforts of others, and with what intensity of labor he finally exorcised them, it seems not irreverent or unfitting to recall the words: "This kind goeth not out but by prayer and fasting."

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