THE PITCH OF THE PERCUSSION SOUND.¹

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In this paper I shall not have time to dwell long on the subject of pitch in general, as I wish to confine my observations to two special matters which seem to me of interest.

The pitch of the percussion sound has been regarded as of little consequence until lately, and I do not think that sufficient prominence is given it to-day, except by a few writers. Auenbrugger barely alludes to the pitch of the percussion sound. Neither Laennec nor Piorry mention it. Locher pronounces it chimerical. Skoda considers the variations in pitch as of little practical value, although he admits that "it occasionally happens that the presence of tubercles in the upper part of the lung may be diagnosticated by the different pitch of the percussion sound over the corresponding part of the other lung."

American and English writers lay no special emphasis on the importance of the pitch. French writers generally slight the subject, although Woillez considers it of more importance than the intensity or the quality of the sound. He says: "I place the pitch at the head of the important characteristics of the percussion sound." And again: "If up to the present time its importance has not been understood, it is because it has not been studied apart from its intensity."

Niemeyer expresses himself thus: "Although formerly this distinction (between high and low pitch) was treated in such a neglectful (stiefmutterlich) manner, it is of much use to modern workers in percussion."

Stern says: "To-day men of training are better fitted to distinguish differences of pitch than differences of sonorousness and intensity, and therefore it is evident that many medical investigators of abnormalities of sonorousness and intensity really perceive only the change of pitch."

The four elements of the percussion sound which should be studied with reference to diagnosis are quality, pitch, intensity, and duration. I know that this is an unusual classification, but I am sure that it is the correct one. I have found it in but one of all the writers I have studied on this subject. Some writers recognize only intensity and quality as elements of the sound; others add to these two a third, pitch. Seitz alone considers the duration of the sound of sufficient importance to classify it as a distinct element.

The duration is not to be confounded with the pitch of the sound. The latter depends upon the number of sound-waves which strike the tympanum of the ear in a given time. The former depends on the length of time these waves continue to strike upon the tympanum, and of course can be considered only when the sound is produced by a stroke or blow, and not where the sound is caused by a continued applied force, as in wind and string instruments. The duration of the sound depends in part, to be sure, on the pitch, a high-pitched sound being, *cæteris paribus*, of shorter duration than a low one. Two sounds of the same pitch may, however, differ greatly in duration, as in the case of the long-vibrating peal of the church bell as compared with the short beat of the drum, even though the pitch be alike in both cases.

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The conditions, other than the pitch, which influence the duration of the sound refer in part to the substance of the body percussed, the duration being in direct proportion to the elasticity and homogeneousness of this body. The medium surrounding the body percussed also affects the duration of the sound. Strike a bell suspended in the air gently, and note the duration of the vibration. Now immerse the bell in water and strike it more forcibly, so that the intensity of the two sounds shall be equal, and see how much sooner the vibrations cease. A third factor is the movability of the point or points of support by which the body percussed is held in place. The duration is directly proportional to the movability of this point. So one reason for the long-continued vibration of the church bell is the fact that it hangs suspended in the air, with nothing but its weight to prevent it from swinging with every breeze.

I would urge upon instructors in physical diagnosis the importance of teaching duration as well as pitch, as an essential element of the percussion sound.

Of the three commonly recognized elements of the percussion sound, quality, pitch, and intensity, I admit that in many cases the quality may be the most important and significant, and differences in quality in two sounds may be so marked as to be more easily distinguished than the difference in pitch or intensity. I think it, however, no more than fair to claim that as between the two latter the pitch of the percussion sound is of more aid to us in diagnosis than the intensity.

If, for example, you are comparing the two sides of the chest, it is impossible to percuss them with equal force. Even if it were possible, no one could be sure from which side the louder sound is elicited. The force of the percussion stroke affects the pitch comparatively little. The difference in pitch is something that any one with (I will not say a musical ear, for happily our ears are all with few exceptions musical) an ordinary ear can distinguish. Let any one strike a note on a piano twice in quick succession, the first time somewhat more forcibly than the second, in the hearing of a company of unselected persons, and there will probably be by no means a unanimity of opinion in regard to the intensity of the note. Let him now strike two different notes, and probably the listeners can all distinguish the higher from the lower one. They certainly can if the two notes differ much in pitch. In the percussion sounds of the thorax it is not uncommon to have two sounds for comparison at least a fifth apart.

There are two characteristics always existing in the pitch of a sound, which aid us in our determination. A high sound is, as we have seen, of comparatively short duration. It is also of harder tone. A low sound is of longer duration and of softer tone. This always assists us in determining the pitch of a sound, whether we recognize the aid it affords us or not.

The first point to which I call your attention is the difference in pitch between the percussion sound of the right infraclavicular region and that of the left in the healthy thorax.

Several years ago I was taught by Dr. Guttman, of Berlin, who follows Traube in this regard, that the pitch is higher on the left than on the right side of a healthy chest. All the instruction I had previously received on the subject had been the reverse. I therefore determined to ascertain what was generally taught

by good authorities, and to make some experiments for myself.

To show the teaching of Dr. Guttman, I will quote from his Handbook of Diagnosis: "In the normal thorax differences in the pitch of the percussion sound at different parts may be recognized by a cultivated musical ear; thus, on the right it is usually found to be deeper than on the left, though occasionally the reverse condition is met with."

I will compare this statement with the teaching of Dr. Flint in his work on the Respiratory Organs. Referring to the infraclavicular region, he says: "In the majority of persons the resonance on the left side is somewhat more intense, the vesicular quality is more marked, and the pitch lower than on the right; per contra, the resonance and the vesicular quality are less and the pitch higher on the right side."

I have studied the literature of the subject with some care, and I generally find but little said concerning this point. By inference most of the authorities represent the two sides as alike in pitch; for example, when every region of the thorax is carefully discussed, and such a special point as the difference in pitch between the inner and outer parts of each infraclavicular region is brought out, no allusion whatever is made to a difference between the two sides.

We should not expect this point discussed by so early a writer as Auenbrugger, but I will call your attention to what he has to say bearing upon it: "If a distinct sound, equal on both sides and commensurate with the degree of percussion, is not obtained from the sonorous regions above mentioned (the thorax), a morbid condition of the parts within the chest is indicated." Corvisart, in his Commentaries, says: "It is a circumstance too obvious to be much insisted upon, that the percussion must be strictly similar and equal on both sides. The least sonorous side is certainly diseased." Of the American writers, DaCosta agrees with Flint. The others are silent on the subject. Most of the English writers whom I have consulted say nothing. Dr. R. Douglas Powell, however, affirms that "in the clavicular and infraclavicular regions the note should be even on the two sides as low as the third rib on firm percussion." French writers generally ignore this point, although Mailliot says: "The lungs percussed with equal force on the right and left side give the same result on one side as on the other, from the apices down to the fourth rib." German writers, so far as I have studied them, are also silent here, except Traube, Guttman, and Seitz. The two former generally find the right side of higher pitch than the left, while Seitz finds that "in the axillary region in many cases the sound is relatively deadened (pitch raised) on the right side," thus agreeing rather with Flint.

On which side should we expect, *à priori*, to find the pitch the higher?

The height of the sound depends upon the volume of the air-containing body percussed (the lung), and also upon the tension of the chest wall and parenchyma, being inversely as the volume and directly as the tension. We may represent this by $H = \frac{V}{T}$. There is, I think, no doubt of the correctness of this formula, although Skoda says that the pitch is dependent entirely upon the quantity of air in the lungs, and Woilez thinks that he has demonstrated by the spiroscope that the pitch is not influenced, at least in every case, by the tension.

Other conditions may affect the pitch; as, for example, the condition and thickness of the chest walls, and the size and substance of the organs lying in juxtaposition to the diaphragm.

Flint, with regard to the infraclavicular, and Seitz, with reference to the axillary region, both think that the increase in intensity and the lowering of pitch on the left side may be accounted for, at least in part, by the transmission of sonorousness from the tympanic stomach.

I do not see how Flint can consistently make use of this argument, for two reasons: First, the tympanic sound which he thinks tends to a lowering of the pitch here, he asserts emphatically is always higher pitched than the normal vesicular resonance. Secondly, he teaches in the same work that whenever the intensity of the percussion sound is morbidly increased (here by the tympanic sound, as he says), the pitch is always raised.

All of the conditions, aside from the tension of the chest wall and parenchyma and the volume of the lung, exert upon the pitch a comparatively slight influence, I think.

There is no uniform difference between the tension of the two lungs recognized by authorities and laid down in text-books. There is a difference in the volume, the right lung being the larger. Hence we should expect to find the pitch lower in the right lung in the majority of cases. Should we find the reverse to be true, we should conclude that the tension was greater on the right than on the left side, and, too, that the difference in tension exceeded the difference in volume.

I have been experimenting upon this matter for several years, and have given careful attention to the work. It is needless to say that all the subjects examined were in good health, and that special care was taken with reference to pulmonary disease. All cases of deformed and unsymmetrical thoraces were discarded; also all abnormally large or small ones. I percussed in every case both with and without the pleximeter, and I attended to two points which are often overlooked in comparing the percussion note of the two sides. First, care was taken in every case to percuss on points equally distant from the sternum, for in the regio-infraclavicularis externa the pitch is higher than in the regio-infraclavicularis interna. And secondly, percussion was made at the same period of the act of respiration, always at the close of a full inspiration.

Referring to the formula $H = \frac{V}{T}$, in inspiration, both numerator and denominator are increased, but the increase in tension of parenchyma and chest wall is greater than the increase in volume of the lung, and as a consequence the pitch is higher at the time of full inspiration than at any other period of the act of respiration. At the close of a forced expiration the pitch is slightly lowered, although Stokes says: "A full inspiration makes the sound clearer, full expiration the contrary, but they both raise the tone a little, they render the pitch higher. Why is this? For the simplest reason in the world,—both actions tighten the drum. They strain the walls of the chest and render their vibrations quicker, and therefore the sounds higher." This, by the way, is the only allusion to the subject of pitch that this writer makes in his work on the chest.

I have had opportunity to examine only two hundred subjects; not enough, I know, to determine the matter satisfactorily, but still enough, I think, to be of

interest to you. The persons were mostly men, thirty only being women. They were all between the ages of twenty and forty. I divided them, with reference to the results obtained, into five classes: (a.) Those in whom no difference was found in the pitch of the two infraclavicular regions. (b.) Those in whom the pitch was slightly higher on the right. (c.) Those in whom it was slightly higher on the left. (d.) Those in whom it was markedly higher on the right. (e.) Those in whom it was markedly higher on the left.

I will say here, that whenever I thought there might be a slight difference of pitch, but was in doubt about it, I placed the case in class a.

The result was as follows:—

(a.) No difference	72
(b.) Slightly higher on right	71
(c.) Slightly higher on left	26
(d.) Markedly higher on right	23
(e.) Markedly higher on left	8

At first glance this result might seem to be of little significance, but I do not so consider it. Out of two hundred cases in but seventy-two, or but little more than one third, is the pitch equal on the two sides. In ninety-four cases, nearly one half, the pitch is higher on the right than on the left. In thirty-four cases the reverse is true.

Now what is the practical bearing of this, supposing the work to have been well done, and considering the cases sufficient in number to be of value?

In the first place, a difference in pitch—and a difference in pitch means a difference in intensity and quality as well—at the apex of either lung, unless decidedly marked, does not necessarily indicate disease on the side where the pitch is higher. According to my investigation there is a difference in nearly two thirds of the cases. This, however, is not generally taught.

Secondly, although the right side is much oftener high-pitched than is the left, still out of two hundred cases we have thirty-four, seventeen per cent., in which the pitch is higher on the left. So, although it is much more significant to find the left lung showing dullness on percussion at the apex than the right, yet this by no means is proof of disease, as I think is generally taught by those who make any distinction between the two sides.

While I was conducting these experiments, a friend made the criticism that to have the result satisfactory a resonator should be used, so that the pitch could be accurately determined. I do not agree with him. In that way every variation of pitch could be discovered, to be sure, and probably a slight difference would be noticed in nearly every case, but it would be of no practical value. What I desired to show was what could be discovered by an ordinary ear,—an ear musically uncultivated.

There are so many unsettled questions in medicine that it would seem worse than useless to call attention to any point simply because authorities differ concerning it. If we were to do that we should have our hands full. But there is so much loose writing, especially concerning strictly scientific matters, in even the best medical works, that I think a protest now and then is needed. I desire to call your attention to the pitch of the tympanic sound.

Dr. Flint, in his work on the Respiratory Organs, teaches that the pitch of the tympanic percussion

sound is always higher than that of the normal vesicular resonance, and he affirms that there is no exception to this statement. In his Manual of Percussion he says: "If there are any exceptions they are very few."

Dr. Loomis, in his Physical Diagnosis, says that the pitch of the tympanic sound is lower than that of the normal vesicular resonance.

These statements, unaccompanied by any explanation, are taught as facts by two of our most widely read writers. They are diametrically opposed to each other, and I think I can show that, although the latter writer is, from a scientific stand-point, nearer the truth than the former, they both offer good examples of loose and incorrect writing.

The normal vesicular resonance is a good illustration of a non-tympanic sound. The sound elicited from pulmonary cavities under certain conditions, and also the sound obtained by percussing the intestines or stomach when not too tensely inflated, are good examples of a tympanic sound. The latter is a clear, ringing sound, with a musical tone, the sound-waves striking upon the tympanum of the ear at regular periods. The non-tympanic sound is not a musical sound, the waves striking the tympanum irregularly.

Percuss a freshly removed sheep's lung without inflation, and you obtain a clear musical tympanic sound. Inflate the lung to the degree of tension maintained in life, and the sound becomes less clear, non-musical, and non-tympanic. Here the pitch is raised. Or take a pig's bladder, just barely filled with air, with no stretching of the membrane, and on percussion the sound is clear and tympanic. Now inflate the bladder until the walls are tense, and the percussion sound is non-tympanic, with a higher pitch.

According to Skoda and Niemeyer, and in fact according to authorities generally until quite recently, the relaxation of the chest wall and parenchyma was considered as the chief if not the only necessary condition of tympanicity of the pulmonary percussion sound, and the tension of these parts the sole cause of non-tympanicity. Tension of the membrane often destroys the tympanicity, to be sure, as the two examples just given illustrate, and as is seen in meteorismus of the bowels. In all these examples the pitch is raised. Tension does not, however, necessarily destroy the tympanic sound, as is seen in the case of the drum. With this example before him, how could Skoda say that the stretching of the membrane necessarily destroys the tympanicity of the percussion sound? In many pulmonary cavities the tympanicity and also the pitch are dependent on other factors than the relaxation of the surrounding walls.

I have not space, however, to dwell on theories as to the cause of tympanicity, under all the different conditions with which it is met. I will call your attention to certain pathological conditions in which the percussion sound is tympanic, and consider the question of pitch in each case.

Those conditions which favor retraction of the lung tissue often give rise to a tympanic percussion sound, just as we have seen that the lung when removed from the thorax and allowed to assume its natural uninflated volume gives a clear musical tympanic sound.

There are several pathological conditions under this head, but I will allude to only one, which is sufficient for illustration.

In moderate pleuritic exudations the lung above the level of the fluid shrinks in volume, and when this

retraction reaches a certain point, which is variable in different cases, the percussion sound is tympanitic. In this case the pitch is lowered as the tension is diminished. Here the pitch of the tympanitic sound is lower than that of normal vesicular resonance.

If all cases where the sound is tympanitic could be thus easily disposed of, Dr. Loomis would be justified in his broad statement, for here the two sounds compared are both elicited from lung substance, and the tympanitic one coming from a lung of diminished tension is in consequence of lower pitch.

Without attempting to classify all the conditions which give rise to a tympanitic percussion sound, I will speak of a class of cases which are very different from that just discussed, and which obey different laws with reference to their pitch. I refer to the tympanitic percussion sound of pulmonary cavities.

In the first place, a cavity is either shut in completely by its walls, or it communicates by an opening with the adjoining tissue. If it is completely occluded, the air within cannot be set in vibration so as to produce an audible sound if the walls are stiff and unyielding. If the walls are sufficiently elastic to admit of the production of a distinct sound from the cavity, tympanicity will be present only in case the walls are not too tense.

The tympanitic sound obtained from a completely occluded cavity with elastic walls in a state of relaxation obeys the same law of physics with reference to pitch as the non-tympanitic normal vesicular resonance. The height is directly as the tension and inversely as the volume.

If the cavity communicates by an opening with the adjoining tissue, the sound may be tympanitic with either elastic or unyielding walls. If the walls are stiff and inelastic, the pitch of the percussion sound depends upon the volume of the cavity and the diameter of the opening of communication, being inversely as the volume and directly as the diameter of the opening. If the walls are elastic, the pitch of the sound depends on these two factors in just the same way, and also upon the tension of the walls, being of course in direct ratio with the tension.

There is also a general law with reference to the elasticity of the walls, that the pitch of cavities, *cæteris paribus*, is in inverse proportion to the elasticity of the walls. If a cavity is sufficiently long and narrow, that is, if in form it is a tube rather than a cube, the pitch does not depend upon the volume of the cavity, but upon the length of the air-vibrating column, being in inverse proportion to this length.

It is true that the tympanitic sound of the abdomen is higher pitched than the normal vesicular resonance. This is not because it is tympanitic. You could with more truth say it is in spite of that fact. We have seen that in tympanicity from lung retraction the pitch is lowered. We have also seen that in the case of the tympanitic sound of pulmonary cavities the pitch is sometimes high and sometimes low, according to the elasticity and tension of the walls, the diameter of the opening of communication, the volume of the cavity, or the length of the air-vibrating columns.

Perhaps Dr. Flint arrives at his conclusion by simply comparing the sound of a tympanitic abdomen with the normal vesicular resonance; and Dr. Loomis may arrive at his by thinking with Skoda that the sole cause of tympanicity is relaxation and decrease of tension of the tissues.

A CASE OF CHYLOUS DEPOSIT IN THE ABDOMEN.¹

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THE subject of this sketch has been deaf and dumb from his birth, and his wife became so at the age of four years, the result of some injury. The two children of this union can both speak and hear. Our patient is fifty-five years old. He was born in Westford, Mass., in which place and Lowell he has spent his life. He has lived in Lowell thirty-two years, and for the past thirteen years in Bachelder's Place, which leads out of Walker Street at a point not far from Pawtucket Street,—a locality which is sufficiently healthy. Up to fifteen years ago he had worked in a bedstead factory. Since then, till a year and a half ago, his occupation had consisted in finishing doors, in which a good deal of heavy lifting is required, especially in the handling of the hard woods, but notwithstanding this he has never had hernia. For a year and a half he has been employed in a cash-carrier factory, where his work is much lighter. He knows of nothing peculiar in his family history except that his father died of consumption. His height is five feet and three inches. His weight has varied but little from one hundred and five pounds since he attained to manhood. His general appearance is fairly healthy; his carriage is erect and firm, and his movements are quick and nervous. His figure is not muscular, and, though spare, is well formed except that his abdomen is full for a person of his general build, and, as he says, has always been so. His complexion is light, and his hair has been white ever since the age of seventeen. He has been nearsighted for many years, has always been nervous, and has had only one testicle from birth. His general health has always been good, and he had kept steadily at work until the forenoon of January 15, 1877, when, on account of a sudden attack of pain shooting from the lower part of his back directly forward through his abdomen, and accompanied by vomiting, he was obliged to give up his work, and go to bed. The pain increased so that on the following day Drs. Savory and Fisk were summoned. They found the patient suffering from pain in a tumor, which, from its form, size, and situation led them to think that it was a distended bladder. The catheter was readily passed, but it withdrew only a few drops of limpid urine. Aspiration was then resorted to, which resulted in the extraction of about two quarts of milky fluid, with entire relief to the patient. After the aspiration the tumor collapsed to a level with the surface of the abdomen, and in a day or two the patient was about his work apparently as well as ever. Throughout this attack there was no febrile action, no symptoms pointing to any organ, and no affection of the general system except nervous excitement from pain and fear of the operation, and some slight weakness for a day or two. The evacuations, both of the bladder and rectum, were normal. An experience like that just narrated was repeated on March 21 and October 3, 1877, and July 28, 1878, respectively, except that every recurrence of the tumor was preceded by an exacerbation of pain lasting for about three days, while the quantity of fluid diminished so that by the last aspiration scarcely twenty ounces were withdrawn. I was present on this occasion, and

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