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CONDUCTED BY

LORD KELVIN, G.C.V.O. D.C.L. LL.D. F.R.S. &c.

JOHN JOLY, M.A. D.Sc. F.R.S. F.G.S.

AND

WILLIAM FRANCIS, F.L.S.

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua .... Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

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——"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

*J. B. Pinelli ad Mazonium.*



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## XII. *On our Perception of Sound Direction**.*

*By* Lord Rayleigh, *O.M., Pres. R.S.*†

IT is some thirty years ago since I executed a rather extensive series of experiments in order to ascertain more precisely what are the capabilities of the ears in estimating the direction of sounds ‡. It appeared that when the alternative was between right and left, the discrimination could be made with certainty and without moving the head, even although the sounds were pure tones. Nor was any difficulty introduced by the requirement that the ears should be stopped at the moment when the sounds commenced.

On the other hand, if the question was whether a sound were situated in front or behind the observer, no pronouncement could be made in the case of pure tones. The impossibility of distinguishing front and back carries with it further confusions relating to cases where the sound may be obliquely situated. But with sounds of other character and notably with the speaking voice, front and back could often be distinguished. It is understood, of course, that the head was kept still. A slight rotation, bringing a pure tone (originally situated exactly in front or exactly behind) to the right or the left, gives the information that was previously lacking.

The discrimination between right and left is usually supposed to be explicable by the greater intensity of sensation experienced by the ear which lies nearer to the sound. When the pitch is pretty high, there is no doubt that this explanation is adequate. A whistle of pitch $f^{iv}$, preferably blown from a gas-bag, is much better heard with the nearer than with the further ear. "A hiss is also heard very badly with the averted ear. This observation may be made by first listening with both ears to a steady hiss on the right or left, and then closing one ear. It makes but little difference when the further ear is closed, but a great difference when the nearer ear is closed. A similar observation may be made upon the sound of running water." In a modified form of

* This paper formed the substance of the Sidgwick lecture given at Cambridge on November 10, 1906, and (except the last two or three pages) was written before the delivery of the lecture. I have learned since from Dr. L. More that three years ago at Cincinnati he made experiments which led him to similar conclusions. It is to be hoped that Dr. More will publish an account of his work, the more as it was conducted on lines different from mine.

† Communicated by the Author.

‡ 'Nature,' xiv. p. 32 (1876); Phil. Mag. iii. p. 546 (1877); Phil. Mag. xiii. p. 340 (1882). 'Scientific Papers,' i. pp. 277, 314; ii. p. 98.

the experiment the ear, say the right, nearest to the falling water is stopped with the right hand. The comparatively feeble sound then heard may be much increased if the left hand be so held at a little distance out as to reflect the sound into the left ear. The effect remains conspicuous even when the hand is held out at full arm's length. Of course a reflector larger than the hand is still more effective.

The discrimination between the right and left situations of high sounds is thus easily explained upon the intensity theory; but this theory becomes less and less adequate as the pitch falls. At a frequency of 256 (middle $c=c'$) the difference of intensities at the two ears is far from conspicuous. At 128 it is barely perceptible. But although the difference of intensities is so small, the discrimination of right and left is as easy as before.

There is nothing surprising in the observation that sounds of low pitch are nearly as well heard with the further as with the nearer ear. When the wave-length amounts to several feet, it is not to be expected that the sound (originating at a distance) could be limited to one side of the head. The question is well illustrated by calculations relating to the incidence of plane waves upon a rigid spherical obstacle, and the results may conveniently be repeated here from 'Theory of Sound,' § 328.

| $\frac{2\pi c}{\lambda}$. | $\mu$. | $F+iG$. | $F^2+G^2$. |
|---|---|---|---|
| $\frac{1}{2}$ | 1 | ·522+·139 $i$ | ·294 |
| | −1 | ·159−·484 $i$ | ·260 |
| | 0 | ·430−·217 $i$ | ·232 |
| 1 | 1 | ·668+·238 $i$ | ·503 |
| | −1 | −·440−·303 $i$ | ·285 |
| | 0 | +·322−·365 $i$ | ·237 |
| 2 | 1 | ·797+·234 $i$ | ·690 |
| | −1 | ·250+·506 $i$ | ·318 |
| | 0 | −·154−·577 $i$ | ·356 |

In this table $2\pi c$ is the circumference of the sphere, and $\lambda$ is the wave-length of the sound. The symbol $\mu$ denotes the cosine of the angle at the centre of the sphere between the direction of the sound ($\mu=1$) and the point upon the

sphere at which the intensity is to be reckoned. $F+iG$ denotes the (complex) condensation, and $F^2+G^2$ the intensity. In the present question of a sound situated say to the right of the observer, the intensity at the right ear corresponds to $\mu=+1$, and at the left ear to $\mu=-1$. In the case of the head the circumference ($2\pi c$) may be taken at about 2 feet or a little less, so that $2\pi c/\lambda=\frac{1}{2}$ corresponds about to middle $c$, or frequency 256. It will be seen that the difference of intensities for $\mu=\pm 1$ is only about 10 per cent. of the whole intensity.

For still smaller values of $2\pi c/\lambda$, *i. e.* in the present application for still graver notes, the difference of intensities may be adequately expressed by a very simple formula. It appears that

$$(F^2+G^2)_{\mu=1}-(F^2+G^2)_{\mu=-1}=\frac{3}{4}\left(\frac{2\pi c}{\lambda}\right)^4,$$

while at the same time the total value of $F^2+G^2$ approximates to ·25. A fall in pitch of an octave thus reduces the difference of intensities 16 times. At frequency 128 the difference would be decidedly less than one per cent. of the whole; and from this point onwards it is difficult to see how the difference could play any important part.

So far as I am aware no explanation of the above difficulty, emphasised in 1876, has been arrived at. A few months since I decided to repeat, and if possible to extend, the observations, commencing with frequency 128. Two forks of this pitch were mounted in the open air at a considerable distance apart, and were electrically maintained, one driving the other. In connexion with each was a resonator which could be put out of action by interposing (without contact) the blade of a knife or a piece of card. An observer with eyes closed, placed between the two forks and so turned as to have them upon his right and his left, could tell with certainty which resonator was in action. The ears may be open all the time; or, what is in some respects better, they may be closed while the changes at the resonators are being made, and afterwards opened simultaneously. If one ear be opened, the sound will appear to be on that side; but when the second ear is also opened, the sound assumes its correct position, whether or not this involves a reversal of the earlier judgement. When the sounds were in front and behind, instead of to the right and the left, several observers agreed that a discrimination between front and back could not be made.

In another method of experimenting a single resonator and fork (which need not be electrically maintained) suffice, but

more than one assistant may be required. The observer, either on his feet, or more conveniently seated upon a rotating stool, is turned round until he loses his bearings. There is no difficulty in this*, but some precautions are needed to prevent the bearing being afterwards recovered. The wind may act as a tell-tale. It is often necessary to cover the eyes with the hands as well as to close the eye-lids in order sufficiently to exclude the light. Until all is ready for a judgement, the ears are kept closed by pressure with the thumbs, and it is usually advisable to keep the thumbs in motion and thus to cause miscellaneous noises loud enough to drown any residue of the sound under observation. Pure tones of pitch 128 and 256 yielded by this method result in agreement with those already described.

The turn-table facilitates observation upon the question as to the relative loudness with which a sound is heard according as the source is on the same, or the opposite, side as the ear in use. In my own case I thought I could detect an advantage when the source was on the same side as the open ear, while others could detect no difference. This relates to pitch 128. At 256 the advantage is quite marked.

In considering whether the discrimination between right and left at pitch 128 can really be attributable to the small intensity-difference, it occurred to me that, if so, the judgement might perhaps be disturbed by the introduction of an obstacle, such as a piece of board, near the head of the observer and on the same side as the sound. But it was found that no mistakes could thus be induced, although in each trial the observer did not know whether the board were in position or not. Another circumstance, unfavourable to the intensity theory, may also be mentioned. It was found that the observer on the turn-table could sometimes decide between the right and the left before un-stopping his ears.

The next step in the investigation appeared to be the examination of pure tones of still graver pitch. A globe, such as are sold to demonstrate the combustion of phosphorus in oxygen gas, was sounded with the aid of a hydrogen flame†. Careful observation revealed little or no trace of overtones. The frequency was about 96 vibrations per second, thus by the interval of the Fourth graver than the 128 forks. At the temperature of the observations this would correspond to a wave-length of about 12 feet. If we use this value in the formula already given, we find for the proportional difference

* The process is aided by the illusion of a reverse rotation when the real rotation has stopped.

† Phil. Mag. vii. p. 149 (1879); 'Scientific Papers,' i. p. 407.

of intensities on the two sides of a sphere of 2 feet circumference only about 2 parts per thousand.

Observation in the open air showed that there was no difficulty whatever in deciding whether this low sound was on the right or the left. Several observers agreed that the discrimination was quite as easy as in the case of forks of pitch 128. On the other hand, as was to be expected, the front and back situations could not be discriminated.

At this stage a reconsideration of theoretical possibilities seemed called for. There could be no doubt but that relative intensities at the two ears play an important part in the localization of sound. Thus if a fork of whatever pitch be held close to one ear, it is heard much the louder by that ear, and is at once referred instinctively to that side of the head. It is impossible to doubt that this is a question of relative intensities. On the other hand, as we have seen, there are cases where this explanation breaks down. When a pure tone of low pitch is recognized as being on the right or the left, the only alternative to the intensity theory is to suppose that the judgement is founded upon the difference of *phases* at the two ears. But even if we admit, as for many years * I have been rather reluctant to do, that this difference of phase can be taken into account, we must, I think, limit our explanation upon these lines to the cases of not very high pitch. For what is the difference of phase at the two ears when a sound reaches the observer—say from the right? It is easy to see that the retardation of distance at the left ear is of the order of the semi-circumference of the head, say one foot. At this rate the retardation for middle $c$ ($c'=256$) is nearly one quarter of a period; for $c''$ (512) nearly half a period; for $c'''$ (1024) nearly a whole period, and so on. Now it is certain that a phase-retardation of half a period affords no material for a decision that the source is on the right rather than on the left, seeing that there is no difference between a retardation and an acceleration of half a period. It is even more evident that a retardation of a whole period, or of any number of whole periods, would be of no avail. In the region of somewhat high pitch a judgement dependent upon phase would seem to be hardly possible, especially when we reflect that the phase-differences enter by degrees, rising from zero when a sound is directly in front or behind to a maximum or minimum in the extreme right and left positions.

As to whether there is any difficulty in localizing to the right or left a tone of pitch 512, an early observation,

* Conf. 'Theory of Sound,' § 385.

conducted with the aid of two forks and resonators of this pitch, gave an answer in the negative. The localization was as easy and as distinct at pitch 512 as at pitch 256. But it is quite possible that 512 is not sufficiently near the particular pitch for which the retardation would have the value of precisely half a period.

The calculations for a spherical obstacle already quoted give some information upon this point. From the values of $F+iG$ corresponding to $\mu=\pm 1$ for the case of $2\pi c/\lambda=1$, it appears that there is approximate opposition of phase. A closer examination and comparison of the three cases shows that exact opposition will occur at a somewhat higher value, say $2\pi c/\lambda=1{\cdot}1$. Calculating from this and taking $2\pi c$ at 2 feet, I find at the temperature of the observations a pitch about a minor third *above* 512, as that corresponding to a phase-difference of half a period.

Naturally, in its application to the head, the calculation is not very trustworthy, and I thought it important to make sure by actual experiment that there is no pitch in this region for which the discrimination of right and left is at all uncertain. Tuning-forks not being available, I fell back upon "singing-flames," *i. e.* tubes, usually of metal and about 1 inch in diameter, maintained in vibration by hydrogen flames. In order to eliminate overtones, the tubes were provided near their centres with loosely-fitting rectangular blocks about two diameters long, held in position by the friction of attached springs. In this way and with the precaution of not sounding them more loudly than was necessary, the tones were, it is believed, sufficiently pure. Trials were made in the open air on many occasions, the pitch ranging in all from $d'$ to $g'''$, and there was never the smallest suspicion of a difficulty in discriminating right and left. In the region from $c''$ to $g''$ of special interest, the pitch was varied by half semitones with the aid of sliding prolongations of the tube. During most of the observations the listener was placed upon the rotating stool and was in ignorance of the real position of the source. This precaution is of course desirable; but after a good deal of practice I found that I was able to trust the direct sensual impression. In the case of right or left the impression is always distinct and always correct; but in trying to discriminate front and back there is usually no distinct impression, and when there is it often turns out to be wrong.

We may fairly conclude that in this region of pitch (above $c''=512$) the discrimination of right and left is not made upon the evidence of phase-differences, or at any rate not upon

this evidence alone. And this conclusion leads to no difficulty; for, as has already been explained, the difference of intensities at the two ears gives adequate foundation for a judgement. It would seem that at high pitch, above $c''$, the judgement is based upon intensities; but that at low pitch, at any rate below $c$ (128), phase-differences must be appealed to.

It remained to confirm, if possible, the suggestion that not only are we capable of appreciating the phase-differences with which sounds of equal intensity may reach the two ears, but that such appreciation is the foundation of judgements as to the direction of sounds—in particular of the right and left effects. The obvious method is to conduct to the two ears separately two pure tones, nearly but not quite in unison. During the cycle, or beat, the phase-differences assume all possible values; and the mere recognition of the cycle is evidence of some appreciation of phase-differences. Experiments on these lines are not new. In 1877 Prof. S. P. Thompson* demonstrated "the existence of an interference in the perception of sounds by leading separately to the ears with india-rubber pipes the sounds of two tuning-forks struck in separate apartments, and tuned so as to 'beat' with one another—the 'beats' being very distinctly marked in the resulting sensation, although the two sources had had no opportunity of mingling externally, or of acting jointly, on any portion of the air-columns along which the sound travelled. The experiment succeeded even with vibrations of so little intensity as to be singly inaudible." And in an observation of my own †, where tones supposed to be moderately pure were led to the ears with use of telephones, a nearly identical conclusion was reached. But although the cycle was recognized, in neither case apparently was there any suggestion of a right and left effect.

In repeating the experiment recently I was desirous of avoiding the use of telephones or tubes in contact with the ears, under which artificial conditions an instinctive judgement would perhaps be disturbed. It seemed that it might suffice to lead the sounds through tubes whose open ends were merely in close proximity one to each ear, an arrangement which has the advantage of allowing the relative intensities to be controlled by a slight lateral displacement of the head towards one or other source. Two forks of frequency 128, independently electrically maintained, were placed in different rooms. Associated with each was a suitably tuned resonator

* Phil. Mag. Nov. 1878.

† Phil. Mag. ii. p. 280 (1901); 'Scientific Papers,' iv. p. 553.

from the interior of which a composition (gas) pipe led the sound through a hole in a thick wall to the observer in a third room. With the aid of closed doors and various other precautions of an obvious character, the sounds were fairly well isolated. But each resonator emits a rather loud sound into its neighbourhood, a little of which might eventually reach the other. To eliminate this cause of disturbance more completely, a second resonator * of like pitch was employed in association with each fork, so situated that the phases of vibration in the two resonators were opposed. By a little adjustment it was possible to provide that but little sound radiated externally from the combination, though the internal vibrations might be as vigorous or more vigorous than when one resonator was employed alone. These arrangements were so successful that when one fork singly was in action, the sound was imperceptible from the tube belonging to the other, even though the open end were pressed firmly into the ear, by which the effect is enormously increased. The open ends of the two pipes may thus be regarded as sources of sound of constant intensity.

In the greater number of experiments the observer, leaning over a table for the sake of steadiness, placed his head between the pipes, which were at such a distance apart that one or two inches separated the open ends from the adjacent ears. At the very first trial on July 31, the period of the cycle being 5 seconds, Lady Rayleigh and I at once experienced a distinct right and left effect, the sound appearing to transfer itself alternately from the one side to the other. When the effect was at its best, the sound seemed to lie entirely on the one side or on the other.

The beat may be slowed down until it occupies 40 or even 70 seconds, thus giving opportunity for more leisurely observation. The position of the head should be so chosen that the right and left effects are equally distinct. Under these circumstances it is found that the sound seems to be predominantly on the right or on the left for almost the whole of the cycle, the transitions occupying only small fractions of the whole time. The observations may be made with the ears continuously open, or, as in some of the out-door experiments, the ears may be opened and closed simultaneously at short intervals. It is perhaps better still, keeping the ears open, to close periodically with the thumbs the open ends of the tubes from which the sounds issue. When the tubes are closed, no sound is audible. It should be said that

* Three out of the four resonators consisted of "Winchester" bottles from which the necks had been removed.

although the best results require the position of the head to be carefully chosen, right and left effects are perceived through a considerable range. It is only necessary that the intensities be approximately equal.

These results are quite decisive, if we can assume that the sounds were sufficiently isolated—that nothing appreciable could pass from the open end of a pipe to the wrong ear. It was easy to verify that when one pipe and the opposite ear were closed, next to nothing could be heard; but it may perhaps be argued that the test is not delicate enough. The risk of error from this cause is diminished by approximating the open ends to their respective ears. Many experiments of this kind were made, but without influencing the results. Finally, short lengths of rubber tubing were provided, by means of which the ears could be connected almost air-tight with the pipes. In this case, to avoid being deafened, it was necessary to reduce the sounds by withdrawing the resonators from their respective forks. The right and left effect remained fully marked. Another argument to show that the effects can not be explained by sounds passing round the head will be mentioned presently.

A question of great importance still remains to be considered. Before the laboratory experiments can be accepted as explanatory of the discrimination of right and left when a single sound is given in the open, it is necessary to show that in the former the sensation of right (say) is associated with a phase-difference such that the vibration reaching the right ear *leads*. There was no difficulty in obtaining a decision. While one observer listens as described for right and left effects, a second observes the maxima and minima of the beat as heard by one ear situated symmetrically with respect to the two sources. In the case of sounds of higher pitch to be considered later some precaution is required here; but for the present sounds, corresponding to a wave-length of nearly 9 feet, there is no difficulty. Under good conditions the minimum, represented by a *silence*, is extremely well marked, and can often be signalled to within half a second. This signal, corresponding to *opposition* of phases, gives the required information to the first observer. If a signal for the maximum, representing phase-agreement, is desired, it is best made by halving the intervals between the silences.

The results can be stated without the slightest ambiguity. The *transitions* between right and left effects correspond to agreement and opposition of phase, not usually recognized by the first observer as maxima and minima of sound. When the vibration on the right is the quicker, the

sensation of right follows agreement of phase, and (what is better observed) the sensation of left follows opposition of phase. And similarly when the vibration on the left is the quicker, the left sensation follows agreement, and the right follows opposition of phase. The question which fork is vibrating the quicker is determined in the usual way by observing the effect upon the period of the beat of the addition or removal of a small load of wax. If for example the beat is *slowed* by loading the right fork, we may be sure that that fork was originally the quicker. A large number of comparisons of this kind have been made at various times, and in no case (at this pitch) has the rule been violated. It is not a little remarkable that by merely listening to right and left effects aided by signals giving the moment of phase-opposition, it is possible conversely to pronounce which fork is the quicker, although the difference of frequencies may not exceed ·02 vibration per second.

It should not pass unnoticed that the laboratory experiments cover a wider field than the observations in the open. In the latter case, if the single sound of pitch 128 is in front or behind, there is agreement of phases at the two ears. As the position becomes more and more oblique, the phase-difference increases; but it can never exceed a moderate amount, about one-eighth of a period, which is attained when the position is precisely to the left or right. Phase-differences in the neighbourhood of half a period do not occur. From the laboratory experiments it appears that the right and left effects are not subject to this limitation; but that, for example, a right effect is experienced when the vibration reaching the right ear leads, whether the amount of the lead be small or whether it approaches the half period.

The right and left or, as I shall sometimes say for brevity, the *lateral*, sensations observable in this way are so conspicuous that I was curious to inquire how I had contrived to miss them in the earlier experiments with telephones already alluded to. The apparatus was the same as before. In the neighbourhood of the electro-magnet driving each fork (128) was placed a small coil of insulated wire whose circuit was completed through a telephone. The double wires connected to the telephones were passed through the perforated wall. In order to weaken the higher overtones, thick sheets of copper intervened between the electro-magnets and the coils. But when the telephones were held to the ears, the sounds were perceived to be of a more mixed character than I had expected; and I am forced to the conclusion that I must formerly have overestimated their approximation to the

character of pure tones. Although the cycle could be recognized, a distinct lateral effect was not perceived, and the failure was evidently connected with the composite character of the sounds. By loading the disks of the telephones with penny-pieces (attached at the centres with wax) the higher components could be better eliminated. It was then possible to fix the attention upon the fundamental tone and to recognize its transference from left to right during the cycle. But the effect was by no means so conspicuous as with the tubes, and might perhaps be missed by an unprepared observer *.

The subject now under consideration is illustrated by a curious observation accidentally made in the course of another inquiry. A large tuning-fork of frequency about 100, mounted upon a resonance-box, was under examination with a Quincke tube. This consisted of a piece of lead pipe more than one quarter of a wave-length long, one end of which was inserted into the resonance-box. At a distance of one eighth of a wave-length from the outer end a lateral tube was attached which communicated with one ear by means of an india-rubber prolongation. When the second ear was closed, it appeared to make no difference to the sound whether or not the outer end of the Quincke tube was closed with the thumb. But when the second ear was open, marked changes in the sound accompanied the opening and closing of the Quincke tube. On the view hitherto held, it would appear very paradoxical that a change not affecting the sound heard in either ear separately should be able to manifest itself so conspicuously. It was easily recognized that the alterations observed were of the nature of right and left effects, and that they could be explained by the local *reversal of phase* which accompanied the closing of the Quincke tube.

The conclusion, no longer to be resisted, that when a sound of low pitch reaches the two ears with approximately equal intensities but with a phase-difference of one quarter of a period, we are able so easily to distinguish at which ear the phase is in advance, must have far reaching consequences in the theory of audition. It seems no longer possible to hold that the vibratory character of sound terminates at the outer

* Subsequently by a much heavier loading (53 gms.) the telephone-plates were tuned approximately to pitch 128, as could be verified by tapping them with the finger. To find room for these extra loads, the ear-pieces of the telephones had to be modified. The sounds now heard were very approximately pure tones, and the lateral effects were as distinct as those observed when the sounds were conveyed through tubes. It is easy to understand that considerable complication must attend an accompaniment of octave and higher harmonics, which would transfer themselves from right to left more rapidly than the fundamental tone.

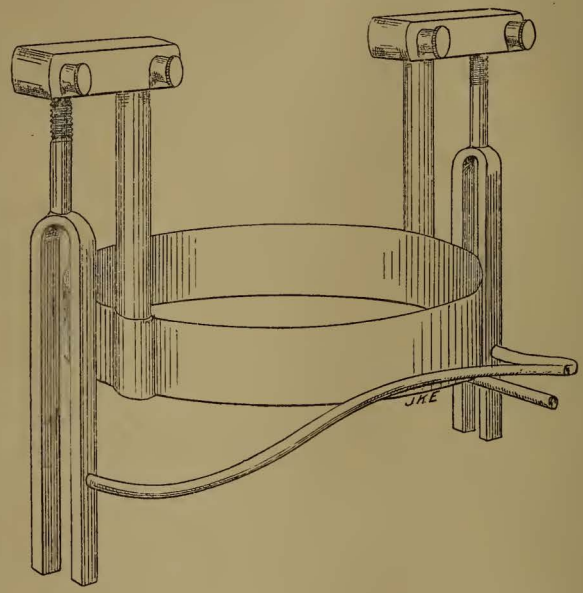
ends of the nerves along which the communication with the brain is established. On the contrary, the processes in the nerve must themselves be vibratory, not of course in the gross mechanical sense, but with preservation of the period and retaining the characteristic of phase—a view advocated by Rutherford, in opposition to Helmholtz, as long ago as 1886. And when we admit that phase-differences at the two ears of tones in unison are easily recognized, we may be inclined to go further and find less difficulty in supposing that phase-relations between a tone and its harmonics, presented to the *same* ear, are also recognizable.

The discrimination of right and left in the case of sounds of frequency 128 and lower, so difficult to understand on the intensity theory, is now satisfactorily attributed to the phase-differences at the two ears. The next observations relate to pure tones of pitch 256. Two large forks of this pitch were used, such as are commonly to be found in collections of acoustical apparatus. Tuned with wax so as to give beats of 3 or 4 seconds period, they may be held (after excitation) by their stalks one to each ear, preferably by an assistant. The sensation of transference from right to left was fully marked, and when the conditions were good, especially in respect of equality of intensities at the two ears, the whole of the sound seemed to come first from one side and then from the other. The method of holding in the fingers is satisfactory as regards the isolation of the sounds. Practically nothing of either fork can be heard by the further ear. But there is a little difficulty in maintaining quite constant the relative positions of forks and ears.

In another arrangement, which has certain advantages, the orks are mounted, stalks upwards, on a sort of crown, in such a fashion that the free ends of the forks are about opposite the ears. A sketch by Mr. Enock is reproduced in the figure (p. 226). If the crown be sufficiently large, it can be adapted to various sized heads with the aid of pads. At the back, attached to the crown, is a forked tube of brass, symmetrically shaped, whose open ends abut upon the faces of the forks. The short limb, forming the stalk, is prolonged by india-rubber tubing and so connected with the ear of the assistant observer. If the forks are vibrating equally, a well-defined silence marks the moment of phase-opposition. To excite and maintain the vibrations, a violin-bow is employed in the usual manner.

Very good observations may be made in this way if the vibrations of the forks are equally and sufficiently maintained. The assistant, listening through the forked tube, is able to give a sharp signal at the moment of phase-opposition. A

transition in respect of right and left effect occurs at this moment, and the sequence rule already stated defining whether the transition is from right to left or from left to right is found to be obeyed.



There are some advantages, of course, in an experimental arrangement allowing the sounds to be uniformly maintained. As in Helmholtz's vowel investigations, the 256 forks can be driven by the 128 interrupter-forks already employed, and in each case the frequency of the driven fork is the exact double of the frequency of the driving fork. The observations may be made in two ways. Either the 256 forks may themselves be brought close to the ears, leading wires being conveyed through the wall; or, what is on the whole preferable, the method employed for the 128 per second tones may be followed. In this case each 256 fork is associated with two resonators, vibrating in opposite phases, with one of which the pipe leading to the observation room is connected. The isolation was good, each sound being inaudible through the tube provided for the other.

Excellent results were obtained in this way. With good adjustment the transitions between right and left were sharply marked and the sequence rule already formulated was obeyed. It is again to be noted that right and left effects are observable in the neighbourhood of phase-opposition, a situation which

does not arise when one sound in the open influences both ears.

Unless the open ends are pretty close to the ears, the sound has more tendency to travel round the head to the wrong ear than was observed at pitch 128. This may raise a question whether, after all, the right and left effects may not be due to small differences of intensities at the two ears varying periodically. The best answer to this objection is to consider what would be the consequence of such invasions. Suppose that at one moment the vibration on the right is in advance by one quarter period, so that a full right sensation is being experienced. The retardation in travelling round the head will at this pitch be about one quarter period, so that the sound starting from the right in advance will on arrival at the left be in approximate phase-agreement with the principal sound there. On the other hand, the sound starting from the left, already a quarter period in arrear, will on arrival at the right be in approximate phase-opposition with the principal sound on the right. The effect of travel round the head is therefore to augment the sound on the left and to diminish the sound on the right. This would evidently tend to cause a sensation of sound on the *left*, and cannot therefore be the explanation of the observed sensation of a sound on the right. The same considerations will apply, if in less degree, to sounds of pitch 128, and to sounds somewhat higher in pitch than 256.

The next sounds to be experimented upon in order of pitch were from forks giving $e'$ of 320 vibrations per second. These could not be driven from the 128 per second interrupters, and were merely held to the ears in the fingers of an assistant. The right and left effect was very marked, but there was a little difficulty at first in fixing the moment of phase-opposition. After a few trials it became sufficiently clear that the rule was the same as at the lower pitch, viz. that the quicker fork asserts itself after the maximum of the beat, corresponding to phase-agreement.

From this point, as the pitch rises, the observations become more difficult, partly no doubt on account of purely experimental complications, but also, I believe, because the effects are themselves less well marked. From two forks of pitch $g'$, electrically driven from the 128 interrupters and provided with resonators in connexion with pipes, fairly distinct right and left effects were obtained, but at first there were discrepancies as to which effect followed phase-opposition. These appear to have been due to faulty observation of the phase of opposition. As might have been anticipated, the moment

of silence representing phase-opposition varies with the position in the room of the ear of the assistant observer. To secure a satisfactory signal at the higher pitches, this position requires to be carefully chosen. In the later experiments a resonator was always employed, whose mouth was symmetrically situated with respect to the open ends of the pipes, which are the proximate sources of sound, connexion with the ear of the assistant observer being through a suitable rubber tube. After this there was no ambiguity, the rule of the lower pitch being uniformly followed. But when the open ends of the pipes were not very close to the ears, perhaps 2 inches distant, the right and left effects seemed to two observers (including myself) not only to be rather obscured, but to be concentrated into the neighbourhood of phase-opposition. A third observer, however, heard the right and left effects more strongly, and with less apparent concentration towards phase-opposition.

On the theory that passage of sound round the head had something to do with these complications, the open ends of the pipes were brought much closer to the ears, but without fitting air-tight, the resonators being re-adjusted so as to diminish the loudness. In this condition of things the two observers experienced the right and left effect more normally and without special concentration in the region of phase-opposition. But the observation is certainly more difficult than at lower pitches, and I believe that the effects are really less pronounced.

Experiments similarly conducted with forks of pitch $c''$ (512) gave results of the same character. When the open ends of the pipes were quite close to the ears, the right and left effect was pretty good and fairly distributed. In this three observers concurred. But a slight withdrawal of the pipes introduced confusion, the extent of which, however, appeared to vary with the observer. In all cases the right and left effects, when sufficiently marked to be observed, obeyed the sequence rule.

At pitch $e''$ (640) the results were not very different. The open ends of the pipes being close to the ears, but not fitted air-tight, only pretty good right and left effects could be observed, and these appeared to be crowded towards phase-opposition. The sequence rule was obeyed.

Finally, trials by the same method were made with forks of pitch $g''$ ($6 \times 128 = 768$). No particular difficulty was encountered in satisfying the necessary experimental conditions, but the results were of a nondescript character. Even when the open ends of the pipes were close to the ears,

I could not satisfy myself that I experienced any right and left effect. Another observer thought he heard a little. It seems clear that at any rate the limit was being approached.

It will be understood that some of these observations were not made without difficulty. Probably an experimenter new to the work would feel misgivings with respect to some even of the easier decisions. But all the more important results have the concurrence of at least three observers *. I regard it as established that up to pitch $g'$ phase-differences are attended with marked lateral effects. They are probably the principal basis on which discriminations of right and left are founded—at any rate below $c'$ (256).

As has already been suggested, it was reasonable to anticipate that phase-difference would cease to avail as an indicator at high pitch. Up to about $e'$ the conditions are favourable. At this pitch the phase-difference at the ears affected by a distant sound increases from zero when the source is in front or behind to a maximum of a quarter period (in one or other direction) when the source is on the right or the left in the line of the ears. This is the phase-difference for which one would expect the lateral sensation to be most intense, so that up to this pitch the lateral sensation would keep step with the true lateralness of the source. At a point somewhat higher in pitch it would seem that complications must enter. The maximum sensation (corresponding to a phase-difference of a quarter period) would occur while the source was still in an oblique position, and the sensation of lateralness would diminish while the true lateralness was still increasing. At a pitch in the neighbourhood of $e''$ (640) the maximum phase-difference would rise to half a period, a phase-difference which could not give rise to lateral sensation at all. Thus, although there might be right and left sensations from sources obliquely situated, these sensations would fail when most needed, that is when the source is really in the line of the ears. In this case a perception of phase-differences would seem to do more harm than good. At a pitch a little higher, ambiguities of a misleading and dangerous kind would necessarily enter. For example, the same sensations might arise from a sound a little on the left and from another fully on the right.

On the whole it appears that the sensation of lateralness due to phase-difference disappears in the region of pitch where there would be danger of its becoming a misleading guide. It is not suggested that there is any precise numerical coincidence. If it were a question of calculating a pitch precisely, it might be necessary to look beyond the size of

* Lady Rayleigh, Mr. Enock, and myself.

modern adult heads to those of our ancestors, perhaps in a very distant past. It is fortunate that when difference of phase fails, difference of intensity comes to our aid. Perhaps it is not to be expected that we should recognize intuitively the very different foundations upon which our judgement rests in the two cases.

A rather difficult question arises as to whether in the laboratory experiments it is possible to distinguish the phases of agreement and of opposition. Not unnaturally perhaps, the apparent movements of the sound from right to left and back are liable to be interpreted as parts of a general movement of revolution, so that, for example, phase-agreement may correspond to the front and phase-opposition to the back position. In a particular case the question is as to the direction of the revolution, whether clockwise or counter-clockwise. With respect to this, my observers frequently disagreed, from which I am disposed to conclude that in these experiments phase-agreement and opposition are not definitely connected with front and back sensations.

At this point there seems to be some discrepancy with the observations of Prof. S. P. Thompson, who found* that "when two simple tones in unison reach the ears in opposite phases, the sensation of the sound is localized at the back of the head." In Prof. Thompson's most striking experiment a microphone is connected in series with a battery and two similar Bell telephones, one of the telephones being provided with a commutator by which the direction of the current through it can be reversed. When the current flows similarly through the telephones, a light tap near the microphone is heard in the ears; but when the current is reversed in one of them a sensation is experienced "only to be described as of some one tapping with a hammer *on the back of the skull from the inside.*" In some (rather inadequate) experiments I have not succeeded in repeating this observation.

The other branch of the subject, which I had hoped to treat in this paper, is the discrimination between the front and back position when a sound is observed in the open; but various obstacles have intervened to cause delay. Among these is the fact that (at 64 years of age) my own hearing has deteriorated. Thirty years ago it was only pure tones, or at any rate musical notes free from accompanying noises, that gave difficulty. Now, as I find to my surprise, I fail to discriminate, even in the case of human speech. It is to be

* Phil. Mag. November 1878, p. 391.

presumed that this failure is connected with obtuseness to sounds of high pitch, such as occur especially in the sibilants. For some years I have been aware that I could no longer hear as before many of the high notes from bird-calls, such as I employ with sensitive flames for imitating optical phenomena. If, as seems the only possible explanation, the discrimination of front and back depends upon an alteration of *quality* due to the external ears, it was to be expected that it would be concerned with the higher elements of the sound. In this matter it would not be surprising if individual differences manifested themselves, apart from deafness. A "paddle-box" formation of the external ear, if not ornamental, may have practical advantages.

My assistant, Mr. Enock, is able to make discriminations between front and back, though I think not so well as I used to be able to do. Experiments of this kind are easily tried on a lawn in the open, the observer closing his eyes and ears, with if necessary a movement of the thumbs over the latter to drown residual external sounds. At the moment of observation the ears are of course opened. In observing sounds from sources not conveniently moveable, such as the ticking of a clock, the rotating stool is useful.

As had been expected, Mr. Enock's judgement was liable to be upset by the operation of little reflecting flaps situated just outside the ears. The arrangement was that of Prof. Thompson's "*pseudophone*" *, whereby the reflectors, whose planes were at an angle of 45° with the line of the ears, could be rotated in a manner unknown to the observer about that line as axis. In my use of it the two reflectors were always adjusted symmetrically. Thus, if the reflectors were so turned as to send into the ears sounds from the front, no mistakes were made, as if the action were co-operative with the natural action of the external ears. On the other hand, if the collars carrying the reflecting flaps were turned through 180° so as to reflect into the ears sounds from *behind*, frequent mistakes ensued. I hope before long to be able to confirm and extend these observations.

In conclusion, I will remark that the facts now established have a possible practical application. In observing fog-signals at sea it is of course of great importance to be able to estimate the bearing. If a sound is of sufficiently long duration (5 or 6 seconds), it is best by turning the body or head to bring it apparently to the right and to the left, and to settle down into the position facing it, where no lateral effect remains. If, as for most fog-signals, the duration be

* Phil. Mag. November 1879.

decidedly less than this, it may be preferable to keep still; but we are then liable to serious errors, should the signal happen to come from nearly in front or nearly behind. A judgement that the signal is to the right or left may usually be trusted, but a judgement that it comes from in front or behind is emphatically to be distrusted. If, for example, the sound seems to come from a position 45° in *front* of full right, we must be prepared for the possibility that it is really situated 45° *behind* full right. A combination of 3 or 4 observers facing different ways offers advantages. A comparison of their judgements, attending only to what they think as to right and left and disregarding impressions as to front and back, should lead to a safe and fairly close estimate of direction.

Terling Place, Witham.

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## XIII. *The Behaviour of Iron under Small Periodic Magnetizing Forces.* *By* J. M. Baldwin, *M.A.*, *B.Sc.**

[Plate VII.]

1. THE object of the present paper, which was undertaken at the suggestion of Professor Lyle, is to investigate the relations which obtain under actual working conditions between the amplitudes and phases of the different harmonics of the magnetic induction produced in iron, and those of the magnetic intensity producing it, for low values of the induction; and from the harmonic expressions obtained for these quantities to deduce the loss of energy in the iron. The work which has been done previously on this subject has been performed under statical conditions, which are seldom realized in practice.

The induction produced in iron in weak magnetic fields has been measured by Baur † using the ballistic method (statical). For the range $H=0{\cdot}38$ to $H=0{\cdot}016$ (with corresponding limits of the induction $B=283$ and $B=3{\cdot}3$) he found that the permeability $\mu$ could be represented closely by an expression of the form

$$\mu = a + bH,$$

where $a$ and $b$ are constants for the specimen. By extrapolation, $\mu$ would tend to a definite limit in exceedingly weak fields. This limit for the specimen examined was 185.

Later, Lord Rayleigh ‡, working with the compensated magnetometer method (statical), showed that, for unannealed

* Communicated by the Physical Society: read October 26, 1906.

† C. Baur, Wied. *Ann.* xi. p. 394 (1880).

‡ Lord Rayleigh, Phil. Mag. [5] vol. xxiii. p. 225 (1887).