	Observed.	Calculated.
$n\phi$	$11^{\circ} 2'$	$10^{\circ} 59'$
n,ϕ	$33 \ 33\frac{1}{4}$	$33 \ 19$
$\phi\phi,$	$22 \ 33\frac{1}{2}$	$22 \ 31$
$c\phi$	$27 \ 42\frac{1}{2}$	$27 \ 47\frac{1}{3}$

Gold.

Measurement of a large though imperfect crystal in the British Museum showed it to be a combination of the cube with the tetrakis-hexahedron $\{410\}$ and the triakis-octahedron $\{811\}$. As the faces were very dull, and but rough measurements could be obtained, I was glad to confirm this observation by the examination of a crystal showing the same combination in Mr. Ludlam's beautiful collection, which he was good enough to lend me. The angles measured on the latter crystal agree fairly well with the calculated angles.

	Measured.	Calculated.
$(410, 811)$	$9^{\circ} 25'$	$9^{\circ} 52'$
$(410, 401)$	$20 \ 18$	$19 \ 44$

I saw recently a very beautiful crystal of the same combination in the collection of the École des Mines, Paris. In the two former the planes $\{811\}$ are deeply striated parallel to their intersection with the faces of the cube.

LXI. *Acoustical Observations.*

By LORD RAYLEIGH, M.A., F.R.S.*

Perception of the Direction of a Source of Sound.

IN a paper with the above title, communicated last year to the Musical Association and afterwards published in abstract in 'Nature,' I brought forward the fact that we are unable to distinguish whether a pure tone (obtained from a tuning-fork and air-resonator) is immediately in front of or immediately behind us—although with other sounds, and notably with the human voice, there is in general no difficulty. In order to make the experiment satisfactorily, it is necessary to provide two similar forks and resonators and to place the observer between them. At a given signal *both* forks are struck, but one of them only is held over its resonator. If this precaution be neglected, the noise attending the excitation of the fork vitiates the experiment. Subsequently to the reading of my paper, it occurred to me that if the ordinary

* Communicated by the Author.

view as to the functions of the two ears be correct, there must be other ambiguous cases besides those already experimented upon. To the right of the observer, and probably nearly in the line of the ears, there must be one direction in which the ratio of the intensity of sound as heard by the right ear to the intensity as heard by the left ear has a maximum value greater than unity. For sounds coming from directions in front of this the ratio of intensities has a less and less value, approaching unity as its limit, when the sound is immediately in front. In like manner, for directions intermediate between the direction of maximum ratio and that immediately behind the observer, the ratio of intensities varies continuously between the same maximum value and unity. Accordingly, for every direction in front there must be a corresponding direction behind for which the ratio of intensities has the same value; and these two directions could not be distinguished in the case of a pure tone. The only directions as to which there would be no ambiguity are the direction of maximum ratio itself, and a corresponding direction of minimum ratio on the other side of the head.

The attitude of my mind with respect to this result was, I confess, one of considerable scepticism. A great number of miscellaneous experiments had been made with forks as well as with other sources of sound; and I thought that, if these ambiguities had existed, indications of them must have been perceived already. It was therefore with some curiosity that I took the first opportunity, last September, of submitting the matter to the test of experiment, the same forks (making 256 vibrations per second) being used as on previous occasions. The decision was soon given. An observer facing north, for example, made mistakes between forks bearing approximately north-east and south-east, though he could distinguish without a moment's hesitation forks bearing east and west. In all such experiments it is necessary that the observer keep his head perfectly still, a very slight motion being sufficient in many cases to give the information that was previously wanting.

A suggestion was made, in the discussion that followed the reading of my paper before the Musical Association, which I thought it proper to examine, though I had not much doubt as to the result. In order to meet the difficulty in the ordinary view as to the functions of the two ears arising out of the fact that a 256-fork seems to be heard nearly as well with the ear turned away as with the ear turned towards it, it was suggested that possibly the discrimination between forks right and left depended on something connected with the commence-

ment of the sound. It might be supposed, for example, that we are able to recognize which ear is first affected. On trial, however, it appeared that the power of discrimination was not weakened, although the observer stopped his ears during the establishment of the sound.

When *one* ear is stopped, mistakes are made between forks right and left; but the direction of other sounds, such as those produced by clapping hands or by the voice, is often told much better than might have been expected.

The Head as an Obstacle to Sound.

The perfection of the shadow thrown by the head depends on the pitch of the sound. I have already mentioned that it appears to make but little difference in the audibility of a pure tone with a frequency of 256, whether the ear used be turned towards or from the source. But the case is very different with sounds of higher pitch, such as that of an ordinary whistle. The one that I employed was blown from a loaded gas-bag, and gave a very steady note of pitch f^{iv} . 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 on the sound of running water.

For the same reason a hiss or whisper, coming from a person whose face is averted, is badly heard. Under these circumstances even ordinary speech is difficult to understand, though the mere intensity of sound does not seem deficient.

Reflection of Sound.

In many cases sound-shadows appear much less perfect than theory would lead us to expect. The anomaly is due in great measure, I believe, to an error of judgment, depending on the enormous range of intensity with which the ear is capable of dealing. The whistle of a locomotive is very loud at a distance of ten yards. At a mile off the intensity must be 30,000 times less; but the sound still appears rather loud, and would probably be audible under favourable circumstances even when enfeebled in the ratio of a million to one. For this reason it is not easy to obtain complete shadows; but another difficulty arises from the fact that there are generally obstacles capable of reflecting a more or less feeble sound into what might otherwise be a nearly complete shadow. An attempt to examine this point led me to a few simple experiments on the reflection of sound, which may be worth recording.

The principal obstacle throwing the shadow was the corner of a large house; and among the sources of sound tried were the human voice, tuning-forks, whistles steadily blown, and a small electric bell, of which the last (which was employed in Professor Reynolds's acoustical experiments) proved to be as convenient as any. The source was placed close to the south side of the house, at a distance of eight or ten yards from the south-west corner, while the observer took up a corresponding position on the west side. With these arrangements the sound-shadow was pretty good, though far from perfect. When, however, a flat reflector, such as a drawing-board of moderate dimensions, was held at the proper angle by an assistant placed at some distance outwards from the corner, the augmentation of sound was immense, and the hearer realized for the first time how very good the shadow really was.

A screen made by stretching a 'Times' over a hoop about $2\frac{1}{2}$ feet in diameter gave apparently as good a reflection as the drawing-board; but when calico was substituted for the paper the reflecting-power was very feeble. By wetting the calico, however, it could be made to reflect very well. These results are in agreement with the striking experiments described by Professor Tyndall.

Audibility of Consonants.

I suppose it must have been noticed before now that the *s* sound is badly returned by an echo. Standing at a distance of about 150 yards from a large wall, I found that there was scarcely any response to even the most powerful hiss. *Sh* was heard a little better; *m*, *k*, *p*, *g* pretty well; *r* very well; *h* badly; *t* badly; *b* seemed half converted into *p* by the echo. The failure of the hiss seems to be the fault of the air rather than of the wall, for a powerful hiss heard directly at a distance of 200 yards had very little *s* left in it.

Interference of Sounds from two unisonant Tuning-forks.

In ordinary experiments on interference the sounds are only approximately in unison, and consequently the silences resulting from antagonism of the vibrations are of only momentary duration. I thought it of interest, therefore, to arrange an experiment in which the sounds should be pure tones, absolutely in unison, and should proceed from sources at a considerable distance apart. With the aid of electromagnetism the solution of the problem was comparatively easy. An intermittent electric current, obtained from a fork interrupter making 128 vibrations per second, excited by means of electromagnets two other forks, whose frequency was 256. These latter

forks were placed at a distance of about ten yards apart, and were provided with suitably tuned resonators by which their sounds were reinforced. The pitch of both forks is necessarily identical, since the vibrations are *forced* by electromagnetic forces of absolutely the same period. The arrangement was successful; and with a battery-power of two Grove cells sounds of fair intensity were obtained. With one ear closed it was possible to define the places of silence with considerable accuracy, a motion of about an inch being sufficient to produce a marked revival of sound. At a point of silence, from which the line joining the forks subtended an angle of about 60° , the apparent striking up of one fork, when the other was stopped, had a very peculiar effect.

Symmetrical Bell.

I do not know whether it has ever been noticed that there ought to be no sound emitted along the axis of a symmetrical bell. It is easy to see that at any point of the axis any effect, whether condensation or rarefaction, which may be produced by one part of the surface of the bell must be neutralized by other parts, and that therefore on the whole there can be no variation of pressure during the vibration. The experiment may be made with a large glass bell (such as those used with air-pumps), set into vibration by friction with the wetted finger carried round the circumference. If the axis of the vibrating bell be turned exactly towards the observer, the sound is feeble as compared with that heard when the position of the bell is altered. The residual sound may be due to want of symmetry, or more probably to reflexion from the ground, which last cause of error it is almost impossible to get rid of.

Octave from Tuning-forks.

When a vibrating fork is held over an air-resonator in tune with itself, the sound emitted is very approximately a pure tone; but when the fork is placed in contact with a sounding-board, the octave may generally be perceived by a practised ear, and is often of remarkable loudness. By means of a resonator tuned to the octave the fact may be made apparent to any one. This result need not surprise us. By the construction of a fork the moving parts are carefully balanced, and the motion is approximately isolated. In the ideal tuning-fork, composed of equal masses moving to and fro in a straight line, the isolation would be complete, and there would be no tendency whatever to communicate motion to surrounding bodies. In an actual fork, however, even if the direction of motion of the masses were as nearly as possible perpendicular to the

stalk, the necessary curvature of the paths would give rise to an unbalanced centrifugal force tending to set the sounding-board in vibration. The force thus arising is indeed of the second order, and might probably be neglected, were it not that the apparatus is especially suited to bring it into prominence.

In order to test the soundness of this view as to the origin of the octave, the following experiment was contrived. A 256 tuning-fork was screwed on to a resonance-box intended for a 512 tuning-fork, and therefore approximately in tune with the octave of the first fork. When a powerful vibration was excited by means of a bow, *the octave sound was predominant*, and but little could be heard of the proper tone of the fork. In order to place the two sounds on a more equal footing, a resonator, consisting of a bottle tuned by pouring water into it to a frequency of 256, was brought near the ends of the vibrating prongs. By adjusting the distance it was easy to arrange matters so that at the beginning of the vibration neither sound had a conspicuous advantage. But, as the amplitude of vibration diminished, the graver tone continually gained on its rival, and was left at last in complete possession of the field. The purity of the remaining sound could be tested at any time by the perfection of the silence obtained by removing the air-resonator. This arrangement may be recommended to any one who wishes to practise his ears in hearing octaves.

From the above experiment (in which, if desired, the ear may be replaced by König's manometric flames), it appears that the octave sound is to be attributed to a motion of the second order, which is rendered important by the peculiar isolation of the motion of the first order. The harmonic sounds heard when suitably tuned resonators are presented to the free ends of the prongs, though also dependent on orders of the motion higher than the first, have a somewhat different origin.

Influence of a Flange on the Correction for the Open End of a Pipe.

In theoretical investigations* as to the amount of the correction to the length of an open pipe due to the inertia of the external air, it has been usual, for the sake of facilitating the calculations, to suppose that the open end is provided with an infinite flange. Even with this simplification no exact solution of the problem has been obtained. It has been proved, however, that, provided the wave-length be sufficient in relation

* Helmholtz, *Crelle*, 1860. Also a memoir by myself "On Resonance," *Phil. Trans.* 1871.

to the diameter of the pipe, the addition which must be supposed to be made to the length is very nearly equal to, though somewhat less than, $\cdot8242 R$, and is certainly greater than $\cdot785 R^*$, R being the radius of the pipe.

It is obvious that the removal of the flange would make a considerable difference, probably reducing the correction below the lower limit above mentioned. In the absence of any theoretical estimate, I thought it desirable to make an experimental determination of the effect of a flange, and ordered some years ago a pair of similar organ-pipes of circular section for the purpose. My idea was to tune the pipes to unison, and then to count the beats when the pitch of one of them was slightly lowered by the addition of a flange; but the experiment lay in abeyance until last winter. Instead of tuning the pipes to unison, I preferred simply to count the beats before and after the addition of the flange, which consisted of a large sheet of stiff millboard perforated with a hole sufficiently large to allow the passage of the pipe. In this way it appeared that the effect of the flange was to reduce the frequency by nearly $1\frac{1}{2}$ out of about 242. If we take the velocity of sound at 1123 feet per second, corresponding to 60° F., the calculated effective length of the pipe is about 28 inches, and the radius is 1 inch. Thus the correction to the length due to the flange is the same fraction of 28 inches that $1\frac{1}{2}$ is of 242, or is equal to about $\cdot2R$. Combining this result with the theoretical estimate above referred to, we may conclude that the whole correction for an open end, when there is no flange, must be about $\cdot6 R$.

Mr. Bosanquet, to whom I communicated the result at which I had arrived, informs me that he has since determined the correction for a flange as $\cdot25 R$.

The Pitch of Organ-pipes.

The whole correction to the length of an organ-pipe, necessary to make it agree with Bernoulli's theory, is considerably greater than any of those spoken of under the preceding heading. According to the rule of Cavaillé-Coll, the addition for an open pipe of circular section amounts to as much as $3\frac{1}{3} R$, whereas for a simple tube open at both ends it should be only about $1\cdot2 R$. This discrepancy is, I believe, often attributed to a peculiar action of the stream of air by which the pipe is excited. Of course it is not to be denied that some disturbance arises from this source, as is proved by the dependence of the pitch on the strength of the wind; but the near agreement between

* See note to a paper "On the approximate Solution of certain Problems relating to the Potential," Math. Soc. Proc. vol. vii. No. 93.

theory and measurements by Wertheim and others, on the pitch of resonators caused to speak by a stream of air, has always seemed to me to prove that a comparatively small part only of the whole discrepancy is to be explained in this way. On the other hand, it is obvious that the "open" end at the base of the pipe is very much contracted, and that the correction thence arising may be several times as great as that applicable to the upper end, where the pipe retains its full section. I was therefore anxious to ascertain what was the proper note of an organ-pipe, regarded as a freely vibrating column of air, and thus to estimate in what proportion the two causes of disturbance contribute to the final result.

There are two methods by which the pitch of a resonator may be determined without the use of a stream of air. The simplest, and in many cases the most accurate, method consists merely in tapping the resonator with the finger or other hammer of suitable hardness, and estimating with the aid of a monochord the pitch of the sound so produced. In attempting, however, thus to determine the pitch of the organ-pipe, I found a difficulty arising from the uncertain character of the sound, and the results were by no means so accordant as I desired. Possibly an observer gifted with a more accurate ear than mine would have been more successful. The other method is one of which I have had a good deal of experience, and which I can generally rely upon to give results of moderate accuracy. It consists in putting the ear into communication with the interior of the resonator, and determining to what note of the scale the resonance is loudest. I have generally found it possible thus to fix the pitch of a resonator to within a quarter of a semitone. In the present case a small hole was made in the side of the pipe near the centre; and over the hole a short piece of tube was cemented, which could be put into communication with the ear by means of a rubber tube. In this way the effective length of the pipe was determined to be 28·7 inches, 4·7 inches more than the actual length. As a check upon this estimate, I closed the upper end of the pipe with a plate of wood and again determined the note of maximum resonance. The effective length of the pipe was now 29·1 inches, so that the correction due to want of openness at the lower end amounted to 5·1 inches. If we add ·6 as a correction for the upper end, we obtain as the corrected length of the pipe in its ordinary condition 29·7 inches. The difference between this and 28·7, obtained directly, is greater, I think, than can be ascribed to errors of experimenting, and is possibly connected with the excessive magnitude of the correction in relation to the wave-length of the sound. The

actual note of the pipe, when blown in the ordinary way by a wind of pressure measured by $2\frac{1}{2}$ inches of water, corresponded to an effective length of 28 inches, so that the blown note was actually *higher* in pitch than the note of maximum resonance. So far, therefore, from the depression of pitch in an organ-pipe below that calculated from the actual length, according to Bernoulli's theory, being principally due to the action of the wind, it would appear that in the absence of a peculiar action of the wind the depression would be even greater than it is. Too much stress, however, must not be laid on a single observation; and all I would maintain is, that by far the larger part of the depression of pitch is due to the insufficient openness of the lower end of the pipe.

May 9, 1877.

LXII. *Notices respecting New Books.*

The Elements of Machine Design: an Introduction to the Principles which determine the Arrangement and Proportion of the Parts of Machines, and a Collection of Rules for Machine Design. By W. CAWTHORNE UNWIN, B. Sc., Assoc. Inst. Civil Engineers, Professor of Hydraulic and Mechanical Engineering at the Royal Indian Civil Engineering College. London: Longmans, Green, and Co., 1877. [Text-Books of Science, small 8vo. Pp. 326.]

THE author takes as a motto for his work an extract from Reuleaux's 'Theoretical Kinematics,' of which the following is part:—"Machine design has been removed by Redtenbacher from its incorrect position as a part of Applied Mechanics, and established on a footing of its own. Its province is to show how the parts of the machine are to be proportioned so as to resist deformation. In order to accomplish this fully, they must be considered both with reference to the external forces acting on the machine, and the corresponding molecular forces within its substance." These words serve as a general description of the author's aim; and accordingly he begins the volume with an account of the theory of the strength of materials, with a view to its application to the questions which form the subject of the volume. These may be briefly enumerated as fastenings, pipes, shafts, bearings, gearing, linkwork, and valves. It is almost needless to say that, though to ensure a due degree of strength is an important element in designing machinery, many other points are brought under notice; and accordingly the work is not so exclusively occupied with the question of strength as the motto might lead the reader to expect. Thus, in the chapter on toothed gearing, our author first considers the cases of shafts driven by rolling contact, and proceeds to show the need of teeth for transmitting force by this means. After briefly mentioning the materials employed, he defines the parts and proportions of teeth, investigates the conditions which deter-