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XXXV. On the present state of experimental acoustics, with suggestions for the arrangement of an acoustic laboratory, and a sketch of research

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when the disk is suspended as a torsion-balance, and its deflection observed, as the currents will not be formed except when the disk is moving.

The effect on the disk might be much increased by placing four other electromagnets above the disk, each opposite one of the lower magnets, as connected with it, so that the lower pole of the upper magnet should be of the opposite name to the upper pole of the lower magnet. In fig. 6 one pair of magnets is shown with the opposite pair, and the wires connecting them. The disk is seen in section, balanced on a needle-point, between the two pairs of magnets. The other four magnets are not shown in the figure.

XXXV. *On the Present State of Experimental Acoustics, with Suggestions for the Arrangement of an Acoustic Laboratory, and a Sketch of Research.* *By* R. H. M. Bosanquet, *St. John's College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE following paper presents an outline of suggested arrangements and work for an acoustic laboratory, which I hope shortly to be able to carry out. I have thought that it may be of interest to the readers of the Philosophical Magazine.

Yours truly,
R. H. M. Bosanquet.

Experimental acoustics are at present in a condition which is perhaps not entirely satisfactory. In the teaching of the subject there is occasionally more demand upon the faith of the learner than is altogether desirable in an experimental science. I think that this arises from the difficulty of access to those experiments which deal with the foundations of the science. The prices charged for complete sets of acoustic apparatus are enough to show that the possession of such apparatus must be confined to few. Adequate sets of such apparatus, used in a sufficient and convincing manner, are exceedingly rare. Under these circumstances, that full experimental knowledge which is desirable in a science of this description does not generally exist.

The ordinary apparatus and arrangements for demonstration appear to err in some points. The effects are not produced in a continuous manner, but by fits and starts, generally by bowing on the sounding body. We do not analyze with ease and certainty a phenomenon which only presents itself to disappear again. These intermittent phenomena are generally produced by an effort, often requiring considerable skill. Under these circumstances there is a tendency to accept the first conclusion that comes to hand, the mind being to some extent satisfied with the production of the difficult phenomenon. Again it has become perhaps too much the practice to refer the phenomena to optical analysis or analogy. In some cases this reference is, no doubt, most convenient; in other cases it is misleading. It is requisite that the analysis of the perceptions of the ear be conducted by reference to the ear itself.

The only form of apparatus in use for the production of simple tones in a continuous manner depends on the application to resonators of tuning-forks driven by electromagnets. These are in many respects ill adapted for demonstration, though no doubt they have furnished most valuable results. There appears to be a want of adaptability about the apparatus; and it is very costly. Professor König is probably better acquainted with the practical use of this apparatus than any one else, except perhaps Helmholtz himself; and König has, in an elaborate analysis of the phenomena *, controverted the entire foundations of the work of Helmholtz. Opinions are by no means at one on the subject, even amongst the highest authorities.

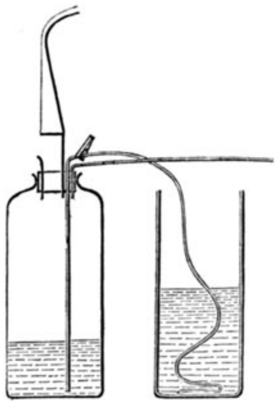
With the object of improving the treatment of this part of the subject, I have introduced resonators which speak in the manner of organ-pipes. These resonators are easily managed with a little simple organ-mechanism; they receive their wind through flexible tubes, and can be placed in any part of the room, at any distance apart, &c.—a matter of great importance in facilitating the analysis of the phenomena. The resonators are fitted with siphons and reservoirs; by a simple contrivance of this kind they can be tuned to any note within range at a moment's notice.

These resonators are made out of bottles, corks, and metal organ-pipes. Their cost is trifling.

The notes should be perfectly pure tones, according to the theory, having regard only to disturbances of the smallest order: but practically, in all tones of this supposed pure character small quantities of harmonics do exist; and I have long maintained that they always must exist in sensible intensity, on account of the transforming power of the air, or, in other

* Pogg. *Ann.* clvii. p. 177; Phil. Mag. [V.] i. pp. 417, 511. See also 'Proceedings of Musical Association,' 1878-79, Spottiswoode "On Beats and Combination-tones."

words, because the aerial disturbances of higher orders are never small enough to be entirely neglected*. The lower harmonics can be accordingly detected in the notes of these resonators by the use of analyzing resonators of proper pitch, whose interiors communicate with the ear. The twelfth can be detected by an experienced ear, in some cases, without the use of analyzing resonators.



Section of speaking resonator, with organ-pipe mouthpiece, siphon with stop, and reservoir for tuning; also flexible tube for putting into the ear when used as an analyzing resonator. This tube can be also used for gas-flame experiments.

With a rough experimental bellows furnishing an unsteady wind, and these resonators, I have been able to repeat some of the less difficult of the experiments of König.

The only statement made by König as to the notes of which the beats consist, is in Phil. Mag. fifth series, i. p. 425, where he says that the two notes of a harmonic interval appear alternately, but that the observation is difficult in the case of the octave. Now in all cases where beats exist, it is possible, by the use of analyzing resonators communicating with the ear, to determine in the manner pointed out by Helmholtz the different notes which vary in intensity, as well as any that do not vary in intensity. But König does not appear to have attempted this analysis at all.

In the cases I have examined I have succeeded in determining the notes which were beating. In this determination, which is sometimes of difficulty, I found it useful not only to employ the analyzing resonators, but to move them about, as

* See abstract of a paper "On the Conditions of the Transformation of Pendulum-Vibrations," Report of British Association, 1876 (Transactions of Sections, p. 45).

well as the primary notes, until the effects were most distinctly obtained. Stationary nodes and loops are formed in the room for all the notes present; and by taking advantage of these the analysis may be much assisted.

Curious results are undoubtedly obtained. For instance, the beats of an octave slightly out of tune are almost entirely on the lower note. If the notes are kept apart in the room, the upper one appears quite steady; while the lower one varies much in intensity, whether we listen to it with the unassisted ear or with a resonator. With the assistance of a resonator the much smaller variation of the upper note can be detected. These two phenomena are not separated by König, but described simply as a beat. According to Helmholtz, the beat of the lower note would be due to the difference-tone, that of the upper note to the octave harmonic of the lower note interfering with the upper one. Now it is easy to convince oneself with the analyzing resonator that the latter interpretation of the beat of the upper note is right; one can hear separately the octave harmonic and the octave note, as well as the beat itself. But the explanation of the beat of the lower note, as due to the interference of the difference-tone, presents this difficulty. If we run up the lower note by means of the siphon, the difference-tone should become audible. But with the arrangements I employed no trace of difference-tone could be perceived, even when the octave was run up to a fifth and a sensitive resonator employed to detect the difference-tone. The notes were placed far apart; and the arrangement was, no doubt, not favourable to the production of a difference-tone: but then how could it be there, so as to form these strong beats of the lower note, if it was not loud enough to be heard separately? I have only stated the above with the object of showing that there is a large ground here, important in a theoretical point of view, which will repay careful working. In fact the repetition and examination of the large collections of experiments in the work of König above cited, with the notes of such resonators as I have described, or with pure notes equally powerful and manageable, is a work essential to the establishment of the foundations of the theory of the subject.

I have come to the conclusion that it is not practicable to carry the work much further with this apparatus without a much steadier wind. For this purpose it is desirable to construct improved bellows. No existing pattern (except that of Cavaillé Coll) delivers a wind of any thing like the steadiness desirable. I shall return to this question later.

Although with an improved bellows hand blowing would be admissible, yet it is always difficult to secure uniformity in hand-blown wind; and where these investigations are to be made on any large scale it will be preferable to employ a small engine, of a kind affording uniform rates of motion, for this as well as for many other acoustic experiments.

This suggestion forms the key to a conception of an acoustic laboratory, which is, I believe, new, and would, it seems to me, constitute a considerable step from an experimental point of view. This is, that, in an acoustic laboratory, power should be employed to produce all the effects required, in a continuous manner where continuity is suitable, and without effort or attention on the part of the investigator when the experiment is once arranged. It is easily seen that determinations of all sorts, which now present almost insuperable difficulties, would become perfectly easy under such conditions; and accurate knowledge would soon take the place of many of our guesses of today, through numberless extensions of the work now possible.

I proceed to mention the principal other subjects which suggest themselves as suitable for research under these conditions.

Aerial Mechanics generally.

So far as our knowledge of the mechanics of fluids, and especially of the air, has progressed beyond its elements of late years, the progress has been mainly in the mathematics of the subject. No doubt the deficiency of experiments arises mainly from the difficult nature of the subject matter; but it is also certainly due to the small extent to which real effort has been made to devise experiments of easy and certain execution, which shall supplement and check the mathematical investigations. No doubt a beginning has been made, and the experiments which are known have been made to tell their tale with admirable perseverance and ingenuity; but it is my opinion that the mathematical structure has, in some respects, been built up too rapidly.

The questions which arise are of great difficulty; and I do not purpose here to enter on any of them. It is sufficient to say that I believe there will be no difficulty in devising a number of experiments by which the simpler cases of aerial motion may be examined in detail. Until the whole circumstances of crucial experiments of this kind have been made out experimentally, I shall continue to feel grave doubts as to the stability of certain portions of the mathematical edifice.

I may allude to a few experiments of importance. Much has been built on a mathematical solution of the following problem:—If a circular disk, closing a circular hole in an infinite plane, execute oscillations at right angles to the plane, what is the motion thereby imparted to the surrounding air? There is a question how far the solutions of this problem, which have been given, conform to the facts. The experimental settlement of this question is quite possible; and it goes to the very foundation of a certain portion of the mathematical equations upon which the modern theory is built.

Again, the equivalence of sound with mechanical energy is at present in the position of a mathematical speculation. The establishment of this equivalence, with a systematic mode of measurement, and the determination of the various laws on which it depends, is a work which alone would constitute an important research. I shall return to this later.

There is another important question in aerial mechanics, which calls for mention among the very first. The whole theory as now developed, neglects intentionally the viscosity of the air. Now the effects of viscosity are more easily dealt with by experiment than by theory. It is pretty certain that the effects of viscosity are not really negligible; for if they were, vortex rings could not be produced in air, nor could they be extinguished when once produced; and in fact, as we all know, they can be produced, and are rapidly extinguished.

The actual composition of the notes produced by the various methods which aim at the production of simple tones is also a matter of primary importance, which no attempt is being made to settle. It is certain that the existing explanation of even such a simple matter as the overtones of organ-pipes is insufficient; for speaking resonators, whose overtones should by theory be inharmonious, give true harmonics, of small intensity, indeed, but with a general effect not very dissimilar to that heretofore supposed to be peculiar to stopped organ-pipes. It is possible that the organ-pipe mouth is responsible for much more than has been supposed.

I have noticed these points only as specimens of aerial mechanics. The oscillating disk first mentioned will be easily constructed, and serve for numerous experiments of this class. On the whole, experimental aerial mechanics must be regarded as, in the future, probably the most important part of experimental acoustics.

Vibration Numbers.

The accurate determination of the vibration numbers or frequencies of notes is at present a matter of great difficulty. Where many such determinations are to be made, the employment of a small engine possessing a uniform rate of speed, controlled by a contrivance to be described later, will place this part of the subject on a new footing. The siren, the revolving stopcock (described later and already constructed), the old toothed-wheel apparatus, and the flashing machine of Lord Rayleigh are examples of instruments which will acquire a new importance for this purpose when driven by a uniform motor.

Revolving Stopcock.

There is a large class of investigations which depend on the regular opening and closing of a channel, for the interruption either of a current of wind or of a current of sound. A turn-table fitted with revolving stopcocks has been constructed for these investigations; and rough results are obtained without difficulty; but it is only by the employment of the uniform motor that accurate results can be expected.

The dissipation of sound in resonators and organ-pipes is a problem which may be attacked by means of the interrupted current of wind. What is the length of a periodic interruption in the wind-supply of a pipe or resonator which just fails to break the continuity of the tone? The answer to this question, and the phenomena we come across in the process of obtaining it, furnish important contributions towards our knowledge of aerial mechanics.

Another problem depending on interrupted wind-supply is, the determination of the velocity of sound in the open air for different musical notes, by a process bearing some slight analogy to Fizeau's determination of the velocity of light by reflection from a distance. The corridor of a cloister supplies an excellent *locale*. The sound is emitted by the apparatus at such intervals that a number of echoes (4, 6, or 8) are heard between two successive sounds. The mode of calculation is obvious.

A problem depending on the interrupted current of sound is, If the ear listen to a sound through the interrupted channel, what are the phenomena presented? They are of some complexity and considerable interest. Professor Mayer has published experiments on the subject.

Reeds.

Notwithstanding the investigations that exist on the behaviour of reeds with respect to columns of air with which they are connected, the subject is still involved in considerable obscurity. There are different kinds of reeds, which possess very different properties; and there is ample room for a thorough experimental investigation. With the appliances of the proposed laboratory these investigations are within reach.

The revolving stopcock can be used to admit wind to the bottom of a pipe or resonator, and so separate out those effects which may be regarded as the results of inexorable motions of reeds from those in which the reeds are influenced by the reaction of the resonator. This arrangement gives rise to a beautiful set of experiments having many bearings. The trouble of maintaining the constant motion of the turn-table is very great; and it is practically impossible to obtain definite results without the uniform motor.

This arrangement, where the stopcock delivers wind into a pipe or resonator having the same vibration-frequency as that of the jet of the stopcock, gives a smooth powerful tone: it is well fitted for the evaluation of a sound of given loudness in terms of mechanical energy. I shall return to this point.

Strings.

The conditions of the flow of sound-energy from strings, through sound-boards, into the surrounding air require investigation. The case of practical interest is that of the violin. By arranging a sort of skeleton so as to represent the principal parts of the instrument, and employing mechanical bowing, it is expected that some light may be thrown on this obscure subject. This question is as yet untouched; but it is probable that the bridge and sound-post transmit a longitudinal vibration, which is communicated to the back at the point where it meets the sound-post at right angles. The effect of "muting," or loading the bridge with a small weight, comes in as a question for explanation.

Orchestral Instruments.

The study of the theory of orchestral instruments is in its infancy. The theory of the fingering of the wood wind—flute, hautboy (oboe), clarionet, bassoon—appears likely to be for the most part tolerably straight forward. The cases where two or more segments of a tube affect each other, though there are open holes between, form a problem which is untouched.

The law that in all lip reed-instruments the note produced is a resonance-note of the tube, was enunciated and proved first by Mr. Blaikley (see 'Proceedings of the Musical Association,' 1877–78, p. 56). On the same occasion I stated that I had obtained and proved the same law experimentally for the hautboy and clarionet (*l. c.* p. 62). We may therefore take as the basis of our work the proposition that, when reeds of movable pitch form notes in combination with a variable resonance, the note produced coincides with a note of resonance. This is not true for reeds of fixed pitch associated with a resonance, as in organ reed-pipes, according to the best practical authorities.

The study of the partial tones of columns of air, such as are enclosed in ordinary brass instruments, is of high practical importance. This study has lately for the first time been put on a sound footing by Mr. Blaikley (*l. c.*). The examination of different forms opens up a considerable field of work.

Changes of Temperature.

The effect of changes of temperature on sounding columns of air, tuning-forks, and other sounding bodies, still requires investigation. It remains unexplained, for instance, why small organ-pipes are more affected by changes of temperature than large ones. The accurate laws of the change remain also to be ascertained.

Velocity of Sound in Tubes.

The laws of the variation of the velocity of musical sounds of different pitch, in tubes of varying diameter, have been formulated*; but the results obtained by different investigators do not agree, and this important element is consequently uncertain to quite a considerable extent. There appears to be no reason why this should not be cleared up by the use of proper appliances.

Quality of Organ-pipes.

The mechanical conditions under which sound of different qualities is produced are not understood in all cases. We know empirically that an organ-pipe of large diameter gives a pure tone. In fact the largest-scaled open organ-pipes have their fundamentals so predominant that analysis by beats fails to detect any harmonics. For investigations as to the lowest limit of audible sounds there is, therefore, no apparatus to be compared with a large-scale 32-foot open diapason as it stands on the organ. The notion that stopped pipes are preferable is a mistake. Whether it be that stopped pipes are not made of sufficiently large scale I do not know; but it is generally easy to demonstrate, by a simple process of analysis by beats, that stopped pipes drop their fundamental about the middle of the 32-foot octave, or at about 25 vibrations per second, whereas with open 32's the fundamental remains approximately unmixed to the very lowest pair of notes. As we diminish the depth of the pipe from back to front, the predominance of the fundamental diminishes; and as we continue, we come to a point where the pipe cannot be made to speak its fundamental at all. Further investigation is required.

* See Pogg. *Ann.* new series, ii. p. 235; also Pogg. *Ann.* cxxxiv. p. 177.

Sympathy and Drawing.

Under certain circumstances two sources of sound react on each other, and affect either the pitch or intensity with which they would speak separately. When two organ-pipes of the same pitch weaken each other's intensity, there is said to be "sympathy." When two of different pitch affect the pitch in which they mutually speak, there is said to be "drawing." Lord Rayleigh brought forward some cases of the latter (see Proc. Musical Assoc. 1878–79, p. 26), in which the pipes spoke the same note, lying above the pitch of either separately: this was with open pipes. I have observed a case where two stopped pipes "drew" together to a note below the pitch of either separately. This mutual influence also occurs with harmonium-reeds. With organ-pipes it presents an interesting problem of atmospheric vibrations. With harmonium-reeds it is practically important in connexion with the construction of tonometers. This subject remains almost unworked.

Loudness of Sound. Mechanical Equivalent of Sound.

The subject of the measurement of the loudness of sound will receive a new foundation in the admission of Fechner's psycho-physical law, with respect to the perception of sound by the ear*. This law is derived from the admission that equal fractions of any existing mechanical intensity produce equal impressions; and it results in the statement that the impression is the logarithm of the mechanical intensity.

Under these circumstances impressions have to be classified as to apparent loudness, in the same manner as stars are classified as to apparent brightness. The criterion of successive stages is that they appear equally distinct from one another when loudness only is considered. The following is a sketch of such a classification.

Audible sounds are divided into ten magnitudes. The first magnitude includes the loudest sounds. The magnitudes down to the fifth include lesser sounds that are still loud; the sixth to the tenth magnitude includes sounds that are not loud, the tenth magnitude containing the softest sounds that can be heard. The distinction between loud and not loud is very definite to my ear; it may not be equally so to others. Of course the following list represents only my own impressions, and it may probably require amendment when considered by others. Each magnitude includes the sounds up to the next on the loud side.

* See Helmholtz, *Phys. Optik*, p. 312. 'Nineteenth Century,' July 1879, p. 166, Galton (where the law is called Weber's law).

The organ is much used in the description, as by reference to it magnitudes can be described in a way that is intelligible to a large number of persons. Stops of average voicing are to be understood, not in the swell-box unless stated. The sound is supposed to be heard in a church or hall of moderate size.

The estimate is to be formed purely as to loudness; and for this purpose it is advisable to compare unmusical noises.

	Sound-magnitudes.	
Loud.	1.	Steam whistle. Cannon close by. Church bells in the chamber.
	2.	Tromba (tuba mirabilis). Sounds of (1) at a little distance. Loud bells at foot of tower.
	3.	Full organ without tromba.
	4.	Trumpet with diapasons. Singing or public speaking at the top of the voice.
	5.	Modern loud diapasons (German). Loud singing or intoning. Ordinary public speaking.
Not loud.	6.	Soft diapasons (old English). Soft singing or intoning. Loudest ordinary conversation.
	7.	4 choir 8-foot stops. Ordinary speech.
	8.	Stopped diapason alone. Soft speech.
	9.	Dulciana. Strong whisper. Tick of watch close to ear.
	10.	Dulciana or salicional (in swell-box closed). Faintest whisper. Tick of watch at arm's length.

Great precision is not attempted; but it is generally easy to say whereabouts in the scale a given sound lies. Precision will come in time.

Several problems then lie before us:—

(1) What is the common ratio of the mechanical intensity in two successive magnitudes?

(2) What is the absolute value of the mechanical intensity corresponding to one definite magnitude?

(3) What is the law of the dependence of the magnitude of sound of given mechanical intensity on variation of pitch.

As to the first two I have made some rough determinations; but the apparatus at my disposal is too imperfect to enable me to quote the results as being of any value. With a better bellows I see no difficulty in the way of answering these two questions*.

* Since the above was written I have made a determination of the ratio by observations of "Tom," Christ Church, Oxford, when the 101 strokes are rung, after 9 P.M. At the foot of the tower, say 30 yards off the source, it was of 2nd magnitude; at the distance of $1\frac{3}{4}$ mile, of 10th magnitude. This gives for the common ratio of the mechanical energy for two consecutive magnitudes, 1 : 3·2 nearly. The experiment with a resonator above referred to, gave 1 : 2·3 for the common ratio, from an estimated difference of two magnitudes, the estimate being of course very uncertain. The

As to (3), I showed some years ago* that, on certain suppositions which cannot be very far from the truth, the energy of notes of different pitch and the same loudness varies as the wave-length. The other conclusions drawn at that time were based on the supposition that mechanical intensity was a true measure of the impression on the sense. The arrangements now described will furnish the means of examining this point in other ways.

(4) When sounds of different pitch excite different parts of the sensorium, it appears probable that Fechner's law applies to each part separately. It is quite certain that a single soft stop sounding the octave below is detected at once, if added to the full organ without such stops; whereas the addition of a similar stop, having the same pitch as any part of the sound actually present, could not be detected by the most experienced ear. This part of the investigation is as yet untouched.

Phonograph and Phonautograph.

These instruments consist of devices for producing marks characteristic of sounds on a moving surface, generally a cylinder which rotates uniformly. The uniform motor will give to the results of these instruments a completeness which they now generally fail to possess. It has hitherto been almost impossible to obtain, for instance, phonographic records of musical sounds, on account of the uncertainty of the speed of rotation; and the exact reproduction of such sounds from the phonograph has presented great difficulties, if, indeed, it has ever been accomplished.

The most interesting applications of the phonograph, however, are to the analysis of speech. The forms corresponding to different vowels have been determined by Messrs. Jenkin and Ewing ('Nature,' xviii. pp. 167, 340, 394, 454). But the point in which the proposed arrangements will be of most value is in the analysis of the inflections of speech, or the rapid variations of pitch which occur continually. This analysis is of the highest importance for phonology, as the inflections are undoubtedly among the principal characteristics of dialects. The employment of the uniform motor in connexion with these recording instruments promises the easy solution of this problem:

observation of "Tom" is by no means final; besides the disturbing influence of buildings, &c., a light breeze got up at the time of the distant observation. But the determination seems worth quoting; indeed it cannot, I think, be very far wrong.

* Phil. Mag. 1872, xliv. p. 381.

Electro-pneumatic Clock Governor.

In all the applications of power useful for acoustic purposes, every thing turns upon the steadiness of the motor and its accurate regulation. I propose to indicate in outline how the pneumatic and electro-pneumatic apparatus in use among organ-builders will furnish convenient means of automatic regulation by the clock.

A good clock will close an electric circuit at every beat of the pendulum for a time which must not be too short. This current will communicate with an apparatus such as is employed in the electric action of the organ, in which air is admitted from a reservoir to a small power-bellows on closing the circuit. In this way a ratchet-wheel will be pushed forward a step every second. This drives a bevel wheel on the same axis. Another bevel wheel opposite, moving freely on the same axis, is turned in the opposite direction by the machine to be regulated. A third bevel wheel, with movable axis at right angles to the first axis, gears in the other two wheels. If the two other wheels move in opposite directions with equal speed, the third simply turns round on its axis. If either of the first two goes quicker or slower than the other, the axis of the third moves with half the differential velocity. If this axis be attached to the governing arrangement of the motor, the whole number of revolutions of the machine performed in any length of time can be constrained to preserve any desired ratio to the movement derived from the clock.

The details would occupy more space than is desirable. I will only say that pneumatic apparatus can be freely used with advantage. The ordinary pneumatic key, connected with its work by flexible tubing, and touched by a stud on the spindle of any part of the machine to be controlled, forms a most valuable resource for automatic regulation.

Pneumatic Motors.

It will not be generally convenient to drive more than one machine at regulated speed from the same motor. For this and other reasons the employment of secondary motors, driven from the bellows, will probably be of advantage. The form I propose to give to these instruments is that of a three-crank shaft and flywheel, with three power-bellows attached to the three cranks. They will also be controlled by the clock governor, with the assistance both of governed supply and pneumatic brake. For the finer regulation of speeds I anticipate that the best results will be thus obtained.

Bellows of Precision.

The only bellows of precision that I know of is that of Cavaillé Coll*: it is expensive; the complete machine costs £80. I will state shortly the principles on which the obtaining of steady wind from the feed depends; it will appear that it is not necessary to increase largely the cost of the ordinary bellows.

The simplest form of supply is one feeder driven by the up-and-down motion of a handle, the stroke overcoming also the weight of the feeder. This arrangement discharges a volume of air into the reservoir with a uniform velocity, which begins and ends suddenly: the weight of the feeder always causes a shock at the beginning and end of the stroke. The area of the feeder is generally equal to the area of the reservoir, so that the velocity imparted by the stroke to the top board of the reservoir is half that of the lift of the end of the feeder. This is the worst form of supply; it is common in very small organs.

In all cases the reservoir must be made with one inverted and one direct rib. This is well understood by organ-builders.

The next best form of feed is that common in English organs. Two equal feeders, each occupying half the area of the reservoir, are moved in opposite directions by up-and-down strokes of a handle. The feeders balance and the shock is materially lessened. The velocity of the upper board is a quarter the lift of the end of the feeder. But the stroke is still generally made with uniform velocity, beginning and ending suddenly.

The next improvement is the application of a pair of cranks, axle, and fly-wheel to the previous combination. In this case there are two discharges for each turn of the fly-wheel, whose velocities follow the pendulum-law. The velocity alternates between zero and its maximum. Great smoothness is here attainable so long as the speed is low; but at high speeds the variation of the velocity of supply will be objectionable.

This inconvenience may be to a great extent obviated by using three feeders instead of two, the three feeders being worked by three cranks on a shaft, set at angles of 120°. The velocity of supply in this case has maxima and minima at intervals of 30°; but these maxima and minima are in the ratio of $2 : \sqrt{3}$, or $1 : 0{\cdot}866$ nearly. This variation is less than that obtained by using four feeders, but it is more frequent. The value, however, is so nearly uniform that it is not thought that any considerable gain would result by increasing the number. If more exact uniformity were desired, six feeders at intervals of 60° would give exceedingly small variations of

* *Comptes Rendus*, 1863, i. p. 339. 'Nature,' xviii. p. 381.

the total velocity of supply, which would be absolutely the same at points 30° apart. The weights of the feeders being borne by the cranks, it is of no consequence that they do not absolutely compensate each other.

The escape-valves open into the feeders in the best modern work; the supply then ceases without noise or shock when the bellows is full.

The principle here applied is that of making the feed of wind itself steady; the plan more commonly adopted has been to employ appliances to overcome the effect of the unsteadiness of the supply.

The arrangement is, I believe, not new; but I do not know of any particular instance where it is in use. It is suitable for the employment of power. Governing arrangements can be applied.

It would be desirable that a lathe should form part of the laboratory fittings. With this assistance the more expensive and novel forms of apparatus might be constructed in the laboratory itself—such as new forms of the siren, oscillating disks, revolving stopcocks, clock governors, &c.

The most suitable engine that I have seen for the purpose is Rider's hot-air engine. This is worked by a certain definite mass of air alternating between a hot and cold cylinder, the two plungers working in cranks at right angles. It is the most silent engine that I have seen; and the smallest size of $\frac{1}{4}$ H. P. can be worked with a gas-burner. Independently of its suitability for laboratory purposes, it is an extremely pretty bit of practical thermodynamics.

So far as I am aware, no laboratory has been fitted with arrangements of the nature of those I have described. The plan seems to me worth trying; and I hope before long to make an effort to carry it out.

The cost of the whole of the new apparatus is hardly likely to amount to that of a set of tuning-forks of König and a "soufflerie de précision" of Cavaillé Coll together. It seems likely to do away with the need of a great part of the expensive apparatus formerly required for these purposes; but I do not suppose that it will be desirable to be altogether without the older apparatus. The experiments of König, for instance, can hardly be said to be repeated unless they are repeated to some extent under the original conditions; and the comparison of different methods may be expected to lead to instructive results. Electromagnetic forks are unquestionably of great importance, and for some purposes cannot be replaced, though for large departments of work we may with advantage find substitutes for them.

If such a laboratory should be fitted up, it would probably be contemplated that instruction should be given, ultimately at least, as well as research undertaken. But the *locale* which would be sufficient for research would not necessarily be suitable for lecturing or other instruction.

XXXVI. *Influence of Atomic Weight.* By THOMAS CARNELLEY, *D.Sc., Assistant Lecturer on Chemistry in the Owens College**.

THE object of the present paper is to point out the influence which the atomic weights of the elements have on the chemical, and especially the physical, properties of both elements and compounds.

As early as 1826, Gmelin (and subsequently Pettinkofer, Dumas, Kremers, Gladstone, Cooke, Low, Odling, Fleay, &c.) directed attention to some curious relations between the atomic weights of certain classes of elements and also between their properties. Many of such relations will at once suggest themselves. Thus, of the elements Cl, Br, and I, bromine stands almost midway between chlorine and iodine, both as regards its atomic weight and its chemical and physical properties.

	Atomic weight.	Specific gravity of the liquid elements.	Melting-point†.	Boiling-point†.	Heat evolved by union with one atom H.
Cl =	35·5	1·33 at 15°	198	240	23783
I =	127	4·00 at 107°	387	473	− 3606
Mean of Cl and I =	81·2	2·66	292	356	10088
Br =	80	2·99 at 15°	248	331	9322

Very similar relations exist between Ca, Sr, and Ba, of which Sr holds a position almost intermediate between the other two; but what has been said with regard to Cl, Br, I, will be quite sufficient to illustrate the kind of relations which were pointed out at the time referred to. It was not, however, till within the last fifteen years that these relations were first traced in a systematic manner; and it is to Newlands, and especially to Mendeljeff, that we owe a new field of research and a new and powerful method of attacking chemical problems. The importance of the work of Newlands and Mendeljeff cannot be easily overrated. The principle proposed independently by each of them will serve in the future, and has done to some extent already, to indicate those directions in

* Communicated by the Author.

† Reckoned from absolute zero −273.