VII. *Outlines of Experiments and Inquiries respecting Sound and Light.* *By* Thomas Young, *M. D. F. R. S.* *In a Letter to* Edward Whitaker Gray, *M. D. Sec. R. S.*

Read January 16, 1800.

DEAR SIR,

It has long been my intention to lay before the Royal Society a few observations on the subject of sound; and I have endeavoured to collect as much information, and to make as many experiments, connected with this inquiry, as circumstances enabled me to do; but, the further I have proceeded, the more widely the prospect of what lay before me has been extended; and, as I find that the investigation, in all its magnitude, will occupy the leisure hours of some years, or perhaps of a life, I am determined, in the mean time, lest any unforeseen circumstances should prevent my continuing the pursuit, to submit to the Society some conclusions which I have already formed from the results of various experiments. Their subjects are, I. The measurement of the quantity of air discharged through an aperture. II. The determination of the direction and velocity of a stream of air proceeding from an orifice. III. Ocular evidence of the nature of sound. IV. The velocity of sound. V. Sonorous cavities. VI. The degree of divergence of sound. VII. The decay of sound. VIII. The harmonic sounds of pipes. IX. The vibrations of different elastic fluids. X. The analogy between light and sound. XI. The coalescence of musical sounds. XII.

The frequency of vibrations constituting a given note. XIII. The vibrations of chords. XIV. The vibrations of rods and plates. XV. The human voice. XVI. The temperament of musical intervals.

I. *Of the Quantity of Air discharged through an Aperture.*

A piece of bladder was tied over the end of the tube of a large glass funnel, and punctured with a hot needle. The funnel was inverted in a vessel of water; and a gage, with a graduated glass tube, was so placed as to measure the pressure occasioned by the different levels of the surfaces of the water. As the air escaped through the puncture, it was supplied by a phial of known dimensions, at equal intervals of time; and, according to the frequency of this supply, the average height of the gage was such as is expressed in the first Table. It appears, that the quantity of air discharged by a given aperture, was nearly in the subduplicate ratio of the pressure; and that the ratio of the expenditures by different apertures, with the same pressure, lay between the ratio of their diameters and that of their areas. The second, third, and fourth Tables show the result of similar experiments, made with some variations in the apparatus. It may be inferred, from comparing the experiments on a tube with those on a simple perforation, that the expenditure is increased, as in water, by the application of a short pipe.

Table I.

A	B	C
.00018	.25	3.9
.00018	.58	11.7
.00018	1.	15.6
.001	.045	7.8
.001	.2	15.6
.001	.7	31.2
.004	.35	46.8

A is the area, in square inches, of an aperture nearly circular. B, the pressure in inches. C, the number of cubic inches discharged in one minute.

All numbers throughout this paper, where the contrary is not expressed, are to be understood of inches, linear, square, or cubic.

Table II.

A	B	C
.07	1.	2000.
.07	2.	2900.

A is the area of the section of a tube about two inches long. B, the pressure. C, the quantity of air discharged in a minute, by estimation.

Table III.

A	B	C	D
.0064	1.15	.2	46.8
.0064	10.	.45	46.8
.0064	13.5	.35	31.2
.0064	13.5	.7	46.8

A is the area of the section of a tube. B, its length. C, the pressure. D, the discharge in a minute.

Table IV.

A	B	C
.003	.28.	46.8

A is the area of an oval aperture, formed by flattening a glass tube at the end: its diameters were .025 and .152. B, the pressure. C, the discharge.

II. *Of the Direction and Velocity of a Stream of Air.*

An apparatus was contrived for measuring, by means of a water-gage communicating with a reservoir of air, the pressure by which a current was forced from the reservoir through a cylindrical tube; and the gage was so sensible, that, a regular blast being supplied from the lungs, it showed the slight variation produced by every pulsation of the heart. The current of air issuing from the tube was directed downwards, upon a white plate, on which a scale of equal parts was engraved, and which was thinly covered with a coloured liquid; the breadth of the surface of the plate laid bare was observed at different distances from the tube, and with different degrees of pressure, care being taken that the liquid should be so shallow as to yield to the slightest impression of air. The results are collected in Tables V. and VI. and are exhibited to the eye in Plate III. Figs. 1—12. In order to measure with greater certainty and precision, the velocity of every part of the current, a second cavity, furnished with a gage, was provided, and pieces perforated with apertures of different sizes were adapted to its orifice: the axis of the current was directed as accurately as possible to the centres of these apertures, and the result of the experiments, with various pressures and distances, are inserted in Tables VII. VIII. and IX. The velocity of a stream being, both according to the commonly received opinion and to the experiments already related, nearly in the subduplicate ratio of the pressure occasioning it, it was inferred, that an equal pressure would be required to stop its progress, and that the velocity of the current, where it struck against the aperture, must be in the subduplicate ratio of the pressure marked by the gage. The ordinates of the curves in Figs. 13—23, were therefore taken reciprocally in the subduplicate ratio of the pressure marked by the second gage to that indicated by the first, at the various distances represented by the abscisses. Each figure represents a different degree of pressure in the first cavity. The curve nearest the axis, is deduced from observations in which the aperture opposed to the tube was not greater than that of the tube itself; and shows what would be the diameter of the current, if the velocities of every one of its particles in the same circular section, including those of the contiguous air, which must have acquired as much motion as the current has lost, were equal among themselves. As the central particles must be supposed to be less impeded in their motion than the superficial ones, of course, the smaller the aperture opposed to the centre of the current, the greater the velocity ought to come out, and the ordinate of the curve the smaller; but, where the aperture was not greater than that of the tube, the difference of the velocities at the same distance was scarcely perceptible. When the aperture was larger than that of the tube, if the distance was very small, of course, the average velocity came out much smaller than that which was inferred from a smaller aperture; but, where the ordinate of the internal curve became nearly equal to this aperture, there was but little difference between the velocities indicated with different apertures. Indeed, in some cases, a larger aperture seemed to indicate a greater velocity: this might have arisen in some degree from the smaller aperture not having been exactly in the centre of the current; but there is greater reason to suppose, that it was occasioned by some resistance derived from the air returning between the sides of the aperture and the current entering it. Where this took place, the external curves, which are so constructed as that their ordinates are reciprocally in the subduplicate ratio of the pressure observed in the second cavity, with apertures equal in semidiameter to their initial ordinate, approach, for a short distance, nearer to the axis than the internal curve: after this, they continue their course very near to this curve. Hence it appears, that no observable part of the motion diverged beyond the limits of the solid which would be formed by the revolution of the internal curve, which is seldom inclined to the axis in an angle so great as ten degrees. A similar conclusion may be made, from observing the flame of a candle subjected to the action of a blowpipe: there is no divergency beyond the narrow limits of the current; the flame, on the contrary, is every where forced by the ambient air towards the current, to supply the place of that which it has carried away by its friction. The lateral communication of motion, very ingeniously and accurately observed in water by Professor Venturi, is exactly similar to the motion here shown to take place in air; and these experiments fully justify him in rejecting the tenacity of water as its cause: no doubt it arises from the relative situation of the particles of the fluid, in the line of the current, to that of the particles in the contiguous strata, which is such as naturally to lead to a communication of motion nearly in a parallel direction; and this may properly be termed friction. The lateral pressure which urges the flame of a candle towards the stream of air from a blowpipe, is probably exactly similar to that pressure which causes the inflection of a current of air near an obstacle. Mark the dimple which a slender stream of air makes on the surface of water; bring a convex body into contact with the side of the stream, and the place of the dimple will immediately show that the current is inflected towards the body; and, if the body be at liberty to move in every direction, it will be urged towards the current, in the same manner as, in VENTURI's experiments, a fluid was forced up a tube inserted into the side of a pipe through which water was flowing. A similar interposition of an obstacle in the course of the wind, is probably often the cause of smoky chimneys. One circumstance was observed in these experiments, which it is extremely difficult to explain, and which yet leads to very important consequences: it may be made distinctly perceptible to the eye, by forcing a current of smoke very gently through a fine tube. When the velocity is as small as possible, the stream proceeds for many inches without any observable dilatation; it then immediately diverges at a considerable angle into a cone, Plate IV. Fig. 24; and, at the point of divergency, there is an audible and even visible vibration. The blowpipe also affords a method of observing this phænomenon: as far as can be judged from the motion of the flame, the current seems to make something like a revolution in the surface of the cone, but this motion is too rapid to be distinctly discerned. When the pressure is increased, the apex of the cone approaches nearer to the orifice of the tube, Figs. 25, 26; but no degree of pressure seems materially to alter its divergency. The distance of the apex from the orifice, is not proportional to the diameter of the current; it rather appears to be the greater the smaller the current, and is much better defined in a small current than in a large one. Its distance in one experiment is expressed in Table x, from observations on the surface of a liquid; in other experiments, its respective distances were sometimes considerably less with the same degrees of pressure. It may be inferred, from the numbers of Tables VII and VIII, that in several instances a greater height of the first gage produced a less height of the second: this arose from the nearer approach of the apex of the cone to the orifice of the tube, the stream losing a greater portion of its velocity by this divergence than it gained by the increase of pressure. At first sight, the form of the current bears some resemblance to the *vena contracta* of a jet of water: but VENTURI has observed, that in water an increase of pressure increases, instead of diminishing, the distance of the contracted section from the orifice. Is it not possible, that the facility with which some spiders are said to project their fine threads to a great distance, may depend upon the small degree of velocity with which they are thrown out, so that, like a minute current, meeting with little interruption from the neighbouring air, they easily continue their course for a considerable time?

Table V.

A	1.	2.	3.	3.8
B	C	C	C	C
1.	.1	.1	.1	
2.	.12	.12	.2	
3.	.17	.25	.3	
4.	.2	.4	.4	
5.	.25	.5		
6.	.30	.52		
7.	.35	.54	.5	
8.	.37	.56		
9.	.39	.58		
10.	.40	.6	.6	.5
15.		.7		
18.	.50			
20.				

The diameter of the tube .07. A is the distance of the liquid from the orifice. B, the pressure. C, the diameter of the surface of the liquid displaced.

Table VI.

A	1.	2.
B	C	C
1.	.1	.1
2.	.13	
3.	.2	.2
4.	.25	.3
6.	.3	.4
7.	.35	.5
10.	.35	.6
15.	.35	.7
20.	.35	.7

Diameter of the tube, .1. A, B, and C, as in Table V.

Table VII.

A	.5	
B	.06	.15
C	D	D
.1	.083	
.2	.16	
.3	.25	.1
.4	.35	
.5	.45	
.6	.53	.2
.7	.6	
.8		.3
1.	.5	
1.2	.4	.4
1.5	.6	
2.	.67	.55
4.	1.3	1.
8.		2.
9.	.3	
14.	.5	

Diameter of the tube .06. A is the distance of the opposite aperture, from the orifice of the tube. B, the diameter of the aperture. C, the pressure, indicated by the first gage. D, the height of the second gage.

Table VIII.

A	.5				1.				2.				4.			
B	.06	.15	.3	.5	.06	.15	.3	.5	.06	.15	.3	5.	.06	.15	.3	.5
C	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D
.1	.05	.05			.03				.017							
.2	.1	.1			.12	.08	.02		.034							
.5	.2	.22			.1	.00			.00							
1.	.32	.36	.1		.17	.1	.1	.05	.04							
2.	.52	.6	.2		.28	.22	.21	.08	.07							
3.	.8	.9	.3		.4	.36	.32	.12	.12		.1	.1				
4.	1.1	1.2	.4		.58	.52	.42	.16	.18		.15	.14				
5.		1.5	.5		.8	.68	.52	.2	.23		.2	.18	.04		.04	.05
6.		1.7	.6		1.	.83	.63	.25	.3		.25	.22	.05		.05	.06
7.		1.9	.7		1.2	1.	.75	.3	.35		.3	.26	.06		.06	.07
8.		2.1	.8		1.5	1.2	.88	.34	.4		.34	.3	.07		.07	.07
9.		2.3	.9		1.7	1.4	1.	.37	.45		.37	.34	.08		.08	.08
10.		2.6	1.		1.9	1.6	1.1	.4	.5		.4	.37	.09		.09	.09

Diameter of the tube .1. A, B, C, and D, as in Table VII.

Table IX.

A	1.15				3.3			4.
B	.15	.3	.5	1.	.06	.15	1.	.06
C	D	D	D	D	D	D	D	D
.5	.1	.1	.1					
1.	.2	.2	.2					
2.	.4	.35	.34	.13	.1	.1	.125	
3.	.6	.5	.5	.2	.15	.15	.18	.1

Diameter of the tube .3.

A, B, C, and D, as in Table VII.

Table X.

A	B
.4	6.
.8	3.
1.2	1.5
1.8	1.
2.	.5
4.	.0

A is the pressure. B, the distance of the apex of the cone from the orifice of a tube .1 in diameter.

III. *Ocular Evidence of the Nature of Sound.*

A tube about the tenth of an inch in diameter, with a lateral orifice half an inch from its end, filed rather deeper than the axis of the tube, Fig. 27, was inserted at the apex of a conical cavity containing about twenty cubic inches of air, and luted perfectly tight: by blowing through the tube, a sound nearly in unison with the tenor C was produced. By gradually increasing the capacity of the cavity as far as several gallons, with the same mouth-piece, the sound, although faint, became more and more grave, till it was no longer a musical note. Even before this period a kind of trembling was distinguishable; and this, as the cavity was still further increased, was changed into a succession of distinct puffs, like the sound produced by an explosion of air from the lips; as slow, in some instances, as 4 or 3 in a second. These were undoubtedly the single vibrations, which, when repeated with sufficient frequency, impress on the auditory nerve the sensation of a continued sound. On forcing a current of smoke through the tube, the vibratory motion of the stream, as it passed out at the lateral orifice, was evident to the eye; although, from various circumstances, the quantity and direction of its motion could not be subjected to exact mensuration. This species of sonorous cavity seems susceptible of but few harmonic sounds. It was observed, that a faint blast produced a much greater frequency of vibrations than that which was appropriate to the cavity: a circumstance similar to this obtains also in large organ pipes; but, several minute observations of this kind, although they might assist in forming a theory of the origin of vibrations, or in confirming such a theory drawn from other sources, yet, as they are not alone sufficient to afford any general conclusions, are omitted at present, for the sake of brevity.

IV. *Of the Velocity of Sound.*

It has been demonstrated, by M. DE LA GRANGE and others, that any impression whatever communicated to one particle of an elastic fluid, will be transmitted through that fluid with an uniform velocity, depending on the constitution of the fluid, without reference to any supposed laws of the continuation of that impression. Their theorem for ascertaining this velocity is the same as NEWTON has deduced from the hypothesis of a particular law of continuation: but it must be confessed, that the result differs somewhat too widely from experiment, to give us full confidence in the perfection of the theory. Corrected by the experiments of various observers, the velocity of any impression transmitted by the common air, may, at an average, be reckoned 1130 feet in a second.

V. *Of sonorous Cavities.*

M. DE LA GRANGE has also demonstrated, that all impressions are reflected by an obstacle terminating an elastic fluid, with the same velocity with which they arrived at that obstacle.

When the walls of a passage, or of an unfurnished room, are smooth and perfectly parallel, any explosion, or a stamping with the foot, communicates an impression to the air, which is reflected from one wall to the other, and from the second again towards the ear, nearly in the same direction with the primitive impulse: this takes place as frequently in a second, as double the breadth of the passage is contained in 1130 feet; and the ear receives a perception of a musical sound, thus determined in its pitch by the breadth of the passage. On making the experiment, the result will be found accurately to agree with this explanation. If the sound is predetermined, and the frequency of vibrations such as that each pulse, when doubly reflected, may coincide with the subsequent pulse proceeding directly from the sounding body, the intensity of the sound will be much increased by the reflection; and also, in a less degree, if the reflected pulse coincides with the next but one, the next but two, or more, of the direct pulses. The appropriate notes of a room may readily be discovered by singing the scale in it; and they will be found to depend on the proportion of its length or breadth to 1130 feet. The sound of the stopped diapason pipes of an organ is produced in a manner somewhat similar to the note from an explosion in a passage; and that of its reed pipes to the resonance of the voice in a room: the length of the pipe in one case determining the sound, in the other, increasing its strength. The frequency of the vibrations does not at all immediately depend on the diameter of the pipe. It must be confessed, that much remains to be done in explaining the precise manner in which the vibration of the air in an organ pipe is generated. M. Daniel Bernoulli has solved several difficult problems relating to the subject; yet some of his assumptions are not only gratuitous, but contrary to matter of fact.

VI. *Of the Divergence of Sound.*

It has been generally asserted, chiefly on the authority of NEWTON, that if any sound be admitted through an aperture into a chamber, it will diverge from that aperture equally in all directions. The chief arguments in favour of this opinion are deduced from considering the phænomena of the pressure of fluids, and the motion of waves excited in a pool of water. But the inference seems to be too hastily drawn: there is a very material difference between impulse and pressure; and, in the case of waves of water, the moving force at each point is the power of gravity, which, acting primarily in a perpendicular direction, is only secondarily converted into a horizontal force, in the direction of the progress of the waves, being at each step disposed to spread equally in every direction: but the impulse transmitted by an elastic fluid, acts primarily in the direction of its progress. It is well known, that if a person calls to another with a speaking trumpet, he points it towards the place where his hearer stands: and I am assured by a very respectable Member of the Royal Society, that the report of a cannon appears many times louder to a person towards whom it is fired, than to one placed in a contrary direction. It must have occurred to every one's observation, that a sound such as that of a mill, or a fall of water, has appeared much louder after turning a corner, when the house or other obstacle no longer intervened; and it has been already remarked by EULER, on this head, that we are not acquainted with any substance perfectly impervious to sound. Indeed, as M. LAMBERT has very truly asserted, the whole theory of the speaking trumpet, supported as it is by practical experience, would fall to the ground, if it were demonstrable that sound spreads equally in every direction. In windy weather it may often be observed, that the sound of a distant bell varies almost instantaneously in its strength, so as to appear at least twice as remote at one time as at another; an observation which has also occurred to another gentleman, who is uncommonly accurate in examining the phænomena of nature. Now, if sound diverged equally in all directions, the variation produced by the wind could never exceed one-tenth of the apparent distance: but, on the supposition of a motion nearly rectilinear, it may easily happen that a slight change in the direction of the wind, may convey the sound, either directly or after reflection, in very different degrees of strength, to the same spot. From the experiments on the motion of a current of air, already related, it would be expected that a sound, admitted at a considerable distance from its origin through an aperture, would proceed, with an almost imperceptible increase of divergence, in the same direction; for, the actual velocity of the particles of air, in the strongest sound, is incomparably less than that of the slowest of the currents in the experiments related, where the beginning of the conical divergence took place at the greatest distance. Dr. MATTHEW YOUNG has objected, not without reason, to M. HUBE, that the existence of a condensation will cause a divergence in sound: but a much greater degree of condensation must have existed in the currents described than in any sound. There is indeed one difference between a stream of air and a sound; that, in sound, the motions of different particles of air are not synchronous: but it is not demonstrable that this circumstance would affect the divergency of the motion, except at the instant of its commencement, and perhaps not even then in a material degree; for, in general, the motion is communicated with a very gradual increase of intensity. The subject, however, deserves a more particular investigation; and, in order to obtain a more solid foundation for the argument, it is proposed, as soon as circumstances permit, to institute a course of experiments for ascertaining, as accurately as possible, the different strength of a sound once projected in a given direction, at different distances from the axis of its motion.

VII. *Of the Decay of Sound.*

Various opinions have been entertained respecting the decay of sound. M. DE LA GRANGE has published a calculation, by which its force is shown to decay nearly in the simple ratio of the distances; and M. DANIEL BERNOULLI's equations for the sounds of conical pipes lead to a similar conclusion. The same inference would follow from a completion of the reasoning of Dr. HELSHAM, Dr. MATTHEW YOUNG, and Professor VENTURI. It has been very elegantly demonstrated by MACLAURIN, and may also be proved in a much more simple manner, that when motion is communicated through a series of elastic bodies increasing in magnitude, if the number of bodies be supposed infinitely great, and their difference infinitely small, the motion of the last will be to that of the first in the subduplicate ratio of their respective magnitudes; and since, in the case of concentric spherical laminæ of air, the bulk increases in the duplicate ratio of the distance, the motion will in this case be directly, and the velocity inversely, as the distance. But, however true this may be of the first impulse, it will appear, by pursuing the calculation a little further, that every one of the elastic bodies, except the last, receives an impulse in a retrograde direction, which ultimately impedes the effect of the succeeding impulse, as much as a similar cause promoted that of the preceding one: and thus, as sound must be conceived to consist of an infinite number of impulses, the motion of the last lamina will be precisely equal to that of the first; and, as far as this mode of reasoning goes, sound must decay in the duplicate ratio of the distance. Hence it appears, that the proposal for adopting the logarithmic curve for the form of the speaking trumpet, was founded on fallacious reasoning. The calculation of M. De la Grange is left for future examination; and it is intended, in the mean time, to attempt to ascertain the decay of sound as nearly as possible by experiment: should the result favour the conclusions from that calculation, it would establish a marked difference between the propagation of sound and of light.

VIII. *Of the harmonic Sounds of Pipes.*

In order to ascertain the velocity with which organ pipes of different lengths require to be supplied with air, according to the various appropriate sounds which they produce, a set of experiments was made, with the same mouth-piece, on pipes of the same bore, and of different lengths, both stopped and open. The general result was, that a similar blast produced as nearly the same sound as the length of the pipes would permit; or at least that the exceptions, though very numerous, lay equally on each side of this conclusion. The particular results are expressed in Table xi. and in Plate IV. Fig. 28. They explain how a note may be made much louder on a wind instrument by a swell, than it can possibly be by a sudden impression of the blast. It is proposed, at a future time, to ascertain by experiment, the actual compression of the air within the pipe under different circumstances: from some very slight trials, it seemed to be nearly in the ratio of the frequency of vibrations of each harmonic.

Table XI.

OPEN.

A	B	C	D	E	F
4.5	4.1	0.7 8.8	8.8	$\overline{\overline{\overline{d}}}$*	1 2
9.4	0.8 2.0 5.0 16.5 19.0	0.3 8.0 18.0 20.0	0.9 8.0 18.0 20.0	$\overline{\overline{f}}$	1 2 3 4 5 6
16.1	0.8 1.2 2.2 3.4 4.0 6.5	0.4 1.0 2.2 4.7 10.0	1.0 2.2 4.7 11.5 13.5 15.0	$\overline{\overline{g}}$*	2 3 4 5 6 7 8
20.5	1.1 4.5	0.6 0.8 1.9 5.7	0.8 1.9 5.7	$\overline{\overline{b}}$	3 4 5 8

STOPPED.

A	B	C	D	E	F
4.5	1.2 5.0	0.3 1.7 9.0	1.8 10.0	$\overline{\overline{d}}$	1 3 5
9.4	1.1 7.0	0.2 0.45 1.6 8.0	0.4 1.6 8.5	$\overline{f}$	1 3 5 7
16.1	0.6 0.9 1.6 2.5 6.0	0.4 0.65 1.1 2.4 4.8 7.0	0.6 1.1 2.4 4.9 9.0	$\overline{\overline{d}}$*	3 5 7 9 11 13
20.5	1.0 1.8 3.2	0.8 1.1 3.8 12.	1.1 3.8 3.8 12.	$\overline{\overline{\overline{c}}}$* o	7 9 11 17 00

A, is the length of the pipe from the lateral orifice to the end. C, the pressure at which the sound began. B, its termination, by lessening the pressure; D, by increasing it. E, the note answering to the first sound of each pipe, according to the German method of notation. F, the number showing the place of each note in the regular series of harmonics. The diameter of the pipe was .35; the air duct of the mouth-piece measured, where smallest, .25 by .035; the lateral orifice .25 by .125. The apparatus was not calculated to apply a pressure of above 22 inches. Where no number stands under C, a sudden blast was required to produce the note.

IX. *Of the Vibrations of different elastic Fluids.*

All the methods of finding the velocity of sound, agree in determining it to be, in fluids of a given elasticity, reciprocally in the subduplicate ratio of the density: hence, in pure hydrogen gas it should be $\sqrt{13} = 3.6$ times as great as in common air; and the pitch of a pipe should be a minor fourteenth higher in this fluid than in the common air. It is therefore probable that the hydrogen gas used in Professor CHLADNI'S late experiments, was not quite pure. It must be observed, that in an accurate experiment of this nature, the pressure causing the blast ought to be carefully ascertained. There can be no doubt but that, in the observations of the French Academicians on the velocity of sound, which appear to have been conducted with all possible attention, the dampness and coldness of the night air must have considerably increased its density: hence, the velocity was found to be only 1109 feet in a second; while DERHAM'S experiments, which have an equal appearance of accuracy, make it amount to 1142. Perhaps the average may, as has been already mentioned, be safely estimated at 1130. It may here be remarked, that the well known elevation of the pitch of wind instruments, in the course of playing, sometimes amounting to half a note, is not, as is commonly supposed, owing to any expansion of the instrument, for this should produce a contrary effect, but to the increased warmth of the air in the tube. Dr. SMITH has made a similar observation, on the pitch of an organ in summer and winter, which he found to differ more than twice as much as the English and French experiments on the velocity of sound. BIANCONI found the velocity of sound, at Bologna, to differ at different times, in the ratio of 152 to 157.

X. *Of the Analogy between Light and Sound.*

Ever since the publication of Sir ISAAC NEWTON's incomparable writings, his doctrines of the emanation of particles of light from lucid substances, and of the formal pre-existence of coloured rays in white light, have been almost universally admitted in this country, and but little opposed in others. LEONARD EULER indeed, in several of his works, has advanced some powerful objections against them, but not sufficiently powerful to justify the dogmatical reprobation with which he treats them; and he has left that system of an ethereal vibration, which after HUYGENS and some others he adopted, equally liable to be attacked on many weak sides. Without pretending to decide positively on the controversy, it is conceived that some considerations may be brought forwards, which may tend to diminish the weight of objections to a theory similar to the HUYGENIAN. There are also one or two difficulties in the NEWTONIAN system, which have been little observed. The first is, the uniform velocity with which light is supposed to be projected from all luminous bodies, in consequence of heat, or otherwise. How happens it that, whether the projecting force is the slightest transmission of electricity, the friction of two pebbles, the lowest degree of visible ignition, the white heat of a wind furnace, or the intense heat of the sun itself, these wonderful corpuscles are always propelled with one uniform velocity? For, if they differed in velocity, that difference ought to produce a different refraction. But a still more insuperable difficulty seems to occur, in the partial reflection from every refracting surface. Why, of the same kind of rays, in every circumstance precisely similar, some should always be reflected, and others transmitted, appears in this system to be wholly inexplicable. That a medium resembling, in many properties, that which has been denominated ether, does really exist, is undeniably proved by the phænomena of electricity; and the arguments against the existence of such an ether throughout the universe, have been pretty sufficiently answered by EULER. The rapid transmission of the electrical shock, shows that the electric medium is possessed of an elasticity as great as is necessary to be supposed for the propagation of light. Whether the electric ether is to be considered as the same with the luminous ether, if such a fluid exists, may perhaps at some future time be discovered by experiment; hitherto I have not been able to observe that the refractive power of a fluid undergoes any change by electricity. The uniformity of the motion of light in the same medium, which is a difficulty in the NEWTONIAN theory, favours the admission of the HUYGENIAN; as all impressions are known to be transmitted through an elastic fluid with the same velocity. It has been already shown, that sound, in all probability, has very little tendency to diverge: in a medium so highly elastic as the luminous ether must be supposed to be, the tendency to diverge may be considered as infinitely small, and the grand objection to the system of vibration will be removed. It is not absolutely certain, that the white line visible in all directions on the edge of a knife, in the experiments of NEWTON and of Mr. JORDAN, was not partly occasioned by the tendency of light to diverge. EULER'S hypothesis, of the transmission of light by an agitation of the particles of the refracting media themselves, is liable to strong objections; according to this supposition, the refraction of the rays of light, on entering the atmosphere from the pure ether which he describes, ought to be a million times greater than it is. For explaining the phænomena of partial and total reflection, refraction, and inflection, nothing more is necessary than to suppose all refracting media to retain, by their attraction, a greater or less quantity of the luminous ether, so as to make its density greater than that which it possesses in a vacuum, without increasing its elasticity; and that light is a propagation of an impulse communicated to this ether by luminous bodies: whether this impulse is produced by a partial emanation of the ether, or by vibrations of the particles of the body, and whether these vibrations are, as EULER supposed, of various and irregular magnitudes, or whether they are uniform, and comparatively large, remains to be hereafter determined. Now, as the direction of an impulse transmitted through a fluid, depends on that of the particles in synchronous motion, to which it is always perpendicular, whatever alters the direction of the pulse, will inflect the ray of light. If a smaller elastic body strike against a larger one, it is well known that the smaller is reflected more or less powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light pass from a rarer to a denser stratum of ether; and frequently an echo when a sound strikes against a cloud. A greater body striking a smaller one, propels it, without losing all its motion: thus, the particles of a denser stratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting substance with equal force; and thus a reflection is always secondarily produced, when the rays of light pass from a denser to a rarer stratum. Let AB, Plate V. Fig. 29, be a ray of light falling on the reflecting surface FG; *cd* the direction of the vibration, pulse, impression, or condensation. When d comes to H, the impression will be, either wholly or partly, reflected with the same velocity as it arrived, and EH will be equal to DH; the angle EIH to DIH or CIF; and the angle of reflection to that of incidence. Let FG, Fig. 30, be a refracting surface. The portion of the pulse IE, which is travelling through the refracting medium, will move with a greater or less velocity in the subduplicate ratio of the densities, and HE will be to KI in that ratio. But HE is, to the radius IH, the sine of the angle of refraction; and KI that of the angle of incidence. This explanation of refraction is nearly the same as that of EULER. The total reflection of a ray of light by a refracting surface, is explicable in the same manner as its simple refraction; HE, Fig. 31, being so much longer than KI, that the ray first becomes parallel to FG, and then, having to return through an equal diversity of media, is reflected in an equal angle. When a ray of light passes near an inflecting body, surrounded, as all bodies are supposed to be, with an atmosphere of ether denser than the ether of the ambient air, the part of the ray nearest the body is retarded, and of course the whole ray inflected towards the body, Fig. 32. The repulsion of inflected rays has been very ably controverted by Mr. JORDAN, the ingenious author of a late publication on the Inflection of Light. It has already been conjectured by EULER, that the colours of light consist in the different frequency of the vibrations of the luminous ether: it does not appear that he has supported this opinion by any argument; but it is strongly confirmed, by the analogy between the colours of a thin plate and the sounds of a series of organ pipes. The phænomena of the colours of thin plates require, in the NEWTONIAN system, a very complicated supposition, of an ether, anticipating by its motion the velocity of the corpuscles of light, and thus producing the fits of transmission and reflection; and even this supposition does not much assist the explanation. It appears, from the accurate analysis of the phænomena which NEWTON has given, and which has by no means been superseded by any later observations, that the same colour recurs whenever the thickness answers to the terms of an arithmetical progression. Now this is precisely similar to the production of the same sound, by means of an uniform blast, from organ-pipes which are different multiples of the same length. Supposing white light to be a continued impulse or stream of luminous ether, it may be conceived to act on the plates as a blast of air does on the organ-pipes, and to produce vibrations regulated in frequency by the length of the lines which are terminated by the two refracting surfaces. It may be objected that, to complete the analogy, there should be tubes, to answer to the organ-pipes: but the tube of an organ-pipe is only necessary to prevent the divergence of the impression, and in light there is little or no tendency to diverge; and indeed, in the case of a resonant passage, the air is not prevented from becoming sonorous by the liberty of lateral motion. It would seem, that the determination of a portion of the track of a ray of light through any homogeneous stratum of ether, is sufficient to establish a length as a basis for colorific vibrations. In inflections, the length of the track of a ray of light through the inflecting atmosphere may determine its vibrations: but, in this case, as it is probable that there is a reflection from every part of the surface of the surrounding atmosphere, contributing to the appearance of the white line in every direction, in the experiments already mentioned, so it is possible that there may be some second reflection at the immediate surface of the body itself, and that, by mutual reflections between these two surfaces, something like the anguiform motion suspected by NEWTON may really take place; and then the analogy to the colours of thin plates will be still stronger. A mixture of vibrations, of all possible frequencies, may easily destroy the peculiar nature of each, and concur in a general effect of white light. The greatest difficulty in this system is, to explain the different degree of refraction of differently coloured light, and the separation of white light in refraction: yet, considering how imperfect the theory of elastic fluids still remains, it cannot be expected that every circumstance should at once be clearly elucidated. It may hereafter be considered how far the excellent experiments of Count RUMFORD, which tend very greatly to weaken the evidence of the modern doctrine of heat, may be more or less favourable to one or the other system of light and colours. It does not appear that any comparative experiments have been made on the inflection of light by substances possessed of different refractive powers; undoubtedly some very interesting conclusions might be expected from the inquiry.

XI. *Of the Coalescence of musical Sounds.*

It is surprising that so great a mathematician as Dr. SMITH could have entertained for a moment, an idea that the vibrations constituting different sounds should be able to cross each other in all directions, without affecting the same individual particles of air by their joint forces: undoubtedly they cross, without disturbing each other's progress; but this can be no otherwise effected than by each particle's partaking of both motions. If this assertion stood in need of any proof, it might be amply furnished by the phænomena of beats, and of the grave harmonics observed by Romieu and Tartini; which M. De la Grange has already considered in the same point of view. In the first place, to simplify the statement, let us suppose, what probably never precisely happens, that the particles of air, in transmitting the pulses, proceed and return with uniform motions; and, in order to represent their position to the eye, let the uniform progress of time be represented by the increase of the absciss, and the distance of the particle from its original position, by the ordinate, Fig. 33—38. Then, by supposing any two or more vibrations in the same direction to be combined, the joint motion will be represented by the sum or difference of the ordinates. When two sounds are of equal strength, and nearly of the same pitch, as in Fig. 36, the joint vibration is alternately very weak and very strong, producing the effect denominated a beat, Plate VI. Fig. 43, B and C; which is slower and more marked, as the sounds approach nearer to each other in frequency of vibrations; and, of these beats there may happen to be several orders, according to the periodical approximations of the numbers expressing the proportions of the vibrations. The strength of the joint sound is double that of the simple sound only at the middle of the beat, but not throughout its duration; and it may be inferred, that the strength of sound in a concert will not be in exact proportion to the number of instruments composing it. Could any method be devised for ascertaining this by experiment, it would assist in the comparison of sound with light. In Plate V. Fig. 33, let P and Q be the middle points of the progress or regress of a particle in two successive compound vibrations; then, CP being $=$ PD, KR $=$ RN, GQ $=$ QH, and MS $=$ SO, twice their distance, 2RS $=$ 2RN $+$

2NM + 2MS = KN + NM + NM + MO = KM + NO, is equal to the sum of the distances of the corresponding parts of the simple vibrations. For instance, if the two sounds be as 80 : 81, the joint vibration will be as 80.5; the arithmetical mean between the periods of the single vibrations. The greater the difference in the pitch of two sounds, the more rapid the beats, till at last, like the distinct puffs of air in the experiments already related, they communicate the idea of a continued sound; and this is the fundamental harmonic described by TARTINI. For instance, in Plate V. Fig. 34—37, the vibrations of sounds related as 1 : 2, 4 : 5, 9 : 10, and 5 : 8, are represented; where the beats, if the sounds be not taken too grave, constitute a distinct sound, which corresponds with the time elapsing between two successive coincidences, or near approaches to coincidence: for, that such a tempered interval still produces a harmonic, appears from Plate V. Fig. 38. But, besides this primary harmonic, a secondary note is sometimes heard, where the intermediate compound vibrations occur at a certain interval, though interruptedly; for instance, in the coalescence of two sounds related to each other as 7 : 8, 5 : 7, or 4 : 5, there is a recurrence of a similar state of the joint motion, nearly at the interval of $\frac{5}{15}$, $\frac{4}{12}$, or $\frac{3}{9}$ of the whole period: hence, in the concord of a major third, the fourth below the key note is heard as distinctly as the double octave, as is seen in some degree in Plate V. Fig. 35; AB being nearly two-thirds of CD. The same sound is sometimes produced by taking the minor sixth below the key note; probably because this sixth, like every other note, is almost always attended by an octave, as a harmonic. If the angles of all the figures resulting from the motion thus assumed be rounded off, they will approach more nearly to a representation of the actual circumstances; but, as the laws by which the motion of the particles of air is regulated, differ according to the different origin and nature of the sound, it is impossible to adapt a demonstration to them all: if, however, the particles be supposed to follow the law of the harmonic curve, derived from uniform circular motion, the compound vibration will be the harmonic instead of the arithmetical mean; and the secondary sound of the interrupted vibrations will be more accurately formed, and more strongly marked, Plate VI. Figs. 41, 42: the demonstration is deducible from the properties of the circle. It is remarkable, that the law by which the motion of the particles is governed, is capable of some singular alterations by a combination of vibrations. By adding to a given sound other similar sounds, related to it in frequency as the series of odd numbers, and in strength inversely in the same ratios, the right lines indicating an uniform motion may be converted very nearly into figures of sines, and the figures of sines into right lines, as in Plate V. Figs. 39, 40.

XII. *Of the Frequency of Vibrations constituting a given Note.*

The number of vibrations performed by a given sound in a second, has been variously ascertained; first, by Sauveur, by a very ingenious inference from the beats of two sounds; and since, by the same observer and several others, by calculation from the weight and tension of a chord. It was thought worth while, as a confirmation, to make an experiment suggested, but coarsely conducted, by Mersennus, on a chord 200 inches in length, stretched so loosely as to have its single vibrations visible; and, by holding a quill nearly in contact with the chord, they were made audible, and were found, in one experiment, to recur 8.3 times in a second. By lightly pressing the chord at one-eighth of its length from the end, and at other shorter aliquot distances, the fundamental note was found to be one-sixth of a tone higher than the respective octave of a tuning-fork marked C: hence, the fork was a comma and a half above the pitch assumed by SAUVEUR, of an imaginary C, consisting of one vibration in a second.

XIII. *Of the Vibrations of Chords.*

By a singular oversight in the demonstration of Dr. BROOK TAYLOR, adopted as it has been by a number of later authors, it is asserted, that if a chord be once inflected into any other form than that of the harmonic curve, it will, since those parts which are without this figure are impelled towards it by an excess of force, and those within it by a deficiency, in a very short time arrive at or very near the form of this precise curve. It would be easy to prove, if this reasoning were allowed, that the form of the curve can be no other than that of the axis, since the tending force is continually impelling the chord towards this line. The case is very similar to that of the NEWTONIAN proposition respecting sound. It may be proved, that every impulse is communicated along a tended chord with an uniform velocity; and this velocity is the same which is inferred from Dr. TAYLOR's theorem; just as that of sound, determined by other methods, coincides with the NEWTONIAN result. But, although several late mathematicians have given admirable solutions of all possible cases of the problem, yet it has still been supposed, that the distinctions were too minute to be actually observed; especially, as it might have been added, since the inflexibility of a wire would dispose it, according to the doctrine of elastic rods, to assume the form of the harmonic curve. The theorem of Euler and De la Grange, in the case where the chord is supposed to be at first at rest, is in effect this: continue the figure each way, alternately on different sides of the axis, and in contrary positions; then, from any point of the curve, take an absciss each way, in the same proportion to the length of the chord as any given portion of time bears to the time of one semivibration, and the half sum of the ordinates will be the distance of that point of the chord from the axis, at the expiration of the time given. If the initial figure of the chord be composed of two right lines, as generally happens in musical instruments and experiments, its successive forms will be such as are represented in Plate VI. Figs. 47, 48: and this result is fully confirmed by experiment. Take one of the lowest strings of a square piano forte, round which a fine silvered wire is wound in a spiral form; contract the light of a window, so that, when the eye is placed in a proper position, the image of the light may appear small, bright, and well defined, on each of the convolutions of the wire. Let the chord be now made to vibrate, and the luminous point will delineate its path, like a burning coal whirled round, and will present to the eye a line of light, which, by the assistance of a microscope, may be very accurately observed. According to the different ways by which the wire is put in motion, the form of this path is no less diversified and amusing, than the multifarious forms of the quiescent lines of vibrating plates, discovered by Professor Chladni; and is indeed in one respect even more interesting, as it appears to be more within the reach of mathematical calculation to determine it; although hitherto, excepting some slight observations of Busse and Chladni, principally on the motion of rods, nothing has been attempted on the subject. For the present purpose, the motion of the chord may be simplified, by tying a long fine thread to any part of it, and fixing this thread in a direction perpendicular to that of the chord, without drawing it so tight as to increase the tension: by these means, the vibrations are confined nearly to one plane, which scarcely ever happens when the chord vibrates at liberty. If the chord be now inflected in the middle, it will be found, by comparison with an object which marked its quiescent position, to make equal excursions on each side of the axis; and the figure which it apparently occupies will be terminated by two lines, the more luminous as they are nearer the ends, Plate VI. Fig. 49. But, if the chord be inflected near one of its extremities, Fig. 50, it will proceed but a very small distance on the opposite side of the axis, and will there form a very bright line, indicating its longer continuance in that place; yet it will return on the former side nearly to the point from whence it was let go, but will be there very faintly visible, on account of its short delay. In the middle of the chord, the excursions on each side the axis are always equal; and, beyond the middle, the same circumstances take place as in the half where it was inflected, but on the opposite side of the axis; and this appearance continues unaltered in its proportions, as long as the chord vibrates at all: fully confirming the non-existence of the harmonic curve, and the accuracy of the construction of Euler and De la Grange. At the same time, as M. Bernoulli has justly observed, since every figure may be infinitely approximated, by considering its ordinates as composed of the ordinates of an infinite number of trochoids of different magnitudes, it may be demonstrated, that all these constituent curves would revert to their initial state, in the same time that a similar chord bent into a trochoidal curve would perform a single vibration; and this is in some respects a convenient and compendious method of considering the problem. But, when a chord vibrates freely, it never remains long in motion, without a very evident departure from the plane of the vibration; and, whether from the original obliquity of the impulse, or from an interference with the reflected vibrations of the air, or from the inequability of its own weight or flexibility, or from the immediate resistance of the particles of air in contact with it, it is thrown into a very evident rotatory motion, more or less simple and uniform according to circumstances. Some specimens of the figures of the orbits of chords are exhibited in Plate VI. Fig. 44. At the middle of the chord, its orbit has always two equal halves, but seldom at any other point. The curves of Fig. 46, are described by combining together various circular motions, supposed to be performed in aliquot parts of the primitive orbit: and some of them approach nearly to the figures actually observed. When the chord is of unequal thickness, or when it is loosely tended and forcibly inflected, the apsides and double points of the orbits have a very evident rotatory motion. The compound rotations seem to demonstrate to the eye the existence of secondary vibrations, and to account for the acute harmonic sounds which generally attend the fundamental sound. There is one fact respecting these secondary notes, which seems intirely to have escaped observation. If a chord be inflected at one-half, one-third, or any other aliquot part of its length, and then suddenly left at liberty, the harmonic note which would be produced by dividing the chord at that point is intirely lost, and is not to be distinguished during any part of the continuance of the sound. This demonstrates, that the secondary notes do not depend upon any interference of the vibrations of the air with each other, nor upon any sympathetic agitation of auditory fibres, nor upon any effect of reflected sound upon the chord, but merely upon its initial figure and motion. If it were supposed that the chord, when inflected into right lines, resolved itself necessarily into a number of secondary vibrations, according to some curves which, when properly combined, would approximate to the figure given, the supposition would indeed in some respects correspond with the phænomenon related; as the coefficients of all the curves supposed to end at the angle of inflection would vanish. But, whether we trace the constituent curves of such a figure through the various stages of their vibrations, or whether we follow the more compendious method of EULER to the same purpose, the figures resulting from this series of vibrations are in fact so simple, that it seems inconceivable how the ear should deduce the complicated idea of a number of heterogeneous vibrations, from a motion of the particles of air which must be extremely regular, and almost uniform; an uniformity which, when proper precautions are taken, is not contradicted by examining the motion of the chord with the assistance of a powerful magnifier. This difficulty occurred very strongly to EULER; and DE LA GRANGE even suspects some fallacy in the experiment, and that a musical ear judges from previous association. But, besides that these sounds are discoverable to an ear destitute of such associations, and, when the sound is produced by two strings in imperfect unison, may be verified by counting the number of their beats, the experiment already related is an undeniable proof that no fallacy of this kind exists. It must be confessed, that nothing fully satisfactory has yet occurred to account for the phænomena; but it is highly probable that the slight increase of tension produced by flexure, which is omitted in the calculations, and the unavoidable inequality of thickness or flexibility of different parts of the same chord, may, by disturbing the isochronism of the subordinate vibrations, cause all that variety of sounds which is so inexplicable without them. For, when the slightest difference is introduced in the periods, there is no difficulty in conceiving how the sounds may be distinguished; and indeed, in some cases, a nice ear will discover a slight imperfection in the tune of harmonic notes: it is also often observed, in tuning an instrument, that some of the single chords produce beating sounds, which undoubtedly arise from their want of perfect uniformity. It may be perceived that any particular harmonic is loudest, when the chord is inflected at about one-third of the corresponding aliquot part from one of the extremities of that part. An observation of Dr. WALLIS seems to have passed unnoticed by later writers on harmonics. If the string of a violin be struck in the middle, or at any other aliquot part, it will give either no sound at all, or a very obscure one. This is true, not of inflection, but of the motion communicated by a bow; and may be explained from the circumstance of the successive impulses, reflected from the fixed points at each end, destroying each other: an explanation nearly analogous to some observations of Dr. MATTHEW YOUNG on the motion of chords. When the bow is applied not exactly at the aliquot point, but very near it, the corresponding harmonic is extremely loud; and the fundamental note, especially in the lowest harmonics, scarcely audible: the chord assumes the appearance, at the aliquot points, of as many lucid lines as correspond to the number of the harmonic, more nearly approaching to each other as the bow approaches more nearly to the point, Plate VI. Fig. 51. According to the various modes of applying the bow, an immense variety of figures of the orbits are produced, Fig. 45, more than enough to account for all the difference of tone in different performers. In observations of this kind, a series of harmonics is frequently heard in drawing the bow across the same part of the chord: these are produced by the bow; they are however not proportionate to the whole length of the bow, but depend on the capability of the portion of the bowstring, intercepted between its end and the chord, of performing its vibrations in times which are aliquot parts of the vibration of the chord: hence it would seem, that the bow takes effect on the chord but at one instant during each fundamental vibration. In these experiments, the bow was strung with the second string of a violin: and, in the preparatory application of resin, the longitudinal sound of CHLADNI was sometimes heard; but it was observed to differ at least a note in different parts of the string.

XIV. *Of the Vibrations of Rods and Plates.*

Some experiments were made, with the assistance of a most excellent practical musician, on the various notes produced by a glass tube, an iron rod, and a wooden ruler; and, in a case where the tube was as much at liberty as possible, all the harmonics corresponding to the numbers from 1 to 13, were distinctly observed; several of them at the same time, and others by means of different blows. This result seems to differ from the calculations of EULER and Count RICCATI, confirmed as they are by the repeated experiments of Professor CHLADNI; it is not therefore brought forward as sufficiently controverting those calculations, but as showing the necessity of a revision of the experiments. Scarcely any note could ever be heard when a rod was loosely held at its extremity; nor when it was held in the middle, and struck one-seventh of the length from one end. The very ingenious method of Professor CHLADNI, of observing the vibrations of plates by strewing fine sand over them, and discovering the quiescent lines by the figures into which it is thrown, has hitherto been little known in this country: his treatise on the phænomena is so complete, that no other experiments of the kind were thought necessary. Glass vessels of various descriptions, whether made to sound by percussion or friction, were found to be almost intirely free from harmonic notes; and this observation coincides with the experiments of CHLADNI.

XV. *Of the human Voice.*

The human voice, which was the object originally proposed to be illustrated by these researches, is of so complicated a nature, and so imperfectly understood, that it can be on this occasion but superficially considered. No person, unless we except M. FERREIN, has published any thing very important on the subject of the formation of the voice, before or since DODART; his reasoning has fully shown the analogy between the voice and the *voix humaine* and regal organ-pipes: but his comparison with the whistle is unfortunate; nor is he more happy in his account of the falsetto. A kind of experimental analysis of the voice may be thus exhibited. By drawing in the breath, and at the same time properly contracting the larynx, a slow vibration of the ligaments of the glottis may be produced, making a distinct clicking sound:

upon increasing the tension, and the velocity of the breath, this clicking is lost, and the sound becomes continuous, but of an extremely grave pitch: it may, by a good ear, be distinguished two octaves below the lowest A of a common bass voice, consisting in that case of about 26 vibrations in a second. The same sound may be raised nearly to the pitch of the common voice; but it is never smooth and clear, except perhaps in some of those persons called ventriloquists. When the pitch is raised still higher, the upper orifice of the larynx, formed by the summits of the arytænoid cartilages and the epiglottis, seems to succeed to the office of the ligaments of the glottis, and to produce a retrograde falsetto, which is capable of a very great degree of acuteness. The same difference probably takes place between the natural voice and the common falsetto: the rimula glottidis being too long to admit of a sufficient degree of tension for very acute sounds, the upper orifice of the larynx supplies its place; hence, taking a note within the compass of either voice, it may be held, with the same expanse of air, two or three times as long in a falsetto as in a natural voice; hence, too, the difficulty of passing smoothly from the one voice to the other. It has been remarked, that the larynx is always elevated when the sound is acute: but this elevation is only necessary in rapid transitions, as in a shake; and then probably because, by the contraction of the capacity of the trachea, an increase of the pressure of the breath can be more rapidly effected this way, than by the action of the abdominal muscles alone. The reflection of the sound thus produced from the various parts of the cavity of the mouth and nostrils, mixing at various intervals with the portions of the vibrations directly proceeding from the larynx, must, according to the temporary form of the parts, variously affect the laws of the motion of the air in each vibration, or, according to EULER's expression, the equation of the curve conceived to correspond with this motion, and thus produce the various characters of the vowels and semi-vowels. The principal sounding board seems to be the bony palate: the nose, except in nasal letters, affords but little resonance; for the nasal passage may be closed, by applying the finger to the soft palate, without much altering the sound of vowels not nasal. A good ear may distinctly observe, especially in a loud bass voice, besides the fundamental note, at least four harmonic sounds, in the order of the natural numbers; and, the more reedy the tone of the voice, the more easily they are heard. Faint as they are, their origin is by no means easy to be explained. This observation is precisely confirmed, in a late dissertation of M. KNECHT, published in the musical newspaper of Leipsic. Perhaps, by a close attention to the harmonics entering into the constitution of various sounds, more may be done in their analysis than could otherwise be expected.

XVI. *Of the Temperament of musical Intervals.*

It would have been extremely convenient for practical musicians, and would have saved many warm controversies among theoretical ones, if three times the ratio of 4 to 5, or four times that of 5 to 6, had been equal to the ratio of 1 to 2. As it happens to be otherwise, it has been much disputed in what intervals the imperfection should be placed. The ARISTOXENIANS and PYTHAGOREANS were in some sense the beginners of the controversy. SAUVEUR has given very comprehensive tables of a great number of systems of temperament; and his own now ranks among the many that are rejected. Dr. SMITH has written a large and obscure volume, which, for every purpose but for the use of an impracticable instrument, leaves the whole subject precisely where it found it. KIRNBERGER, MARPURG, and other German writers, have disputed with great bitterness, almost every one for a particular method of tuning. It is not with any confidence of success, that one more attempt is made, which rests its chief claim to preference, on the similarity of its theory to the actual practice of the best instrument-makers. However we estimate the degree of imperfection of two tempered concords of the same nature, it will appear, that the manner of dividing the temperament between them does not materially alter its aggregate sum; for instance, the imperfection of a comma in a major-third, occasions it to beat very nearly twice as fast as that of half a comma. If indeed the imperfection were great, it might affect an interval so materially as to destroy its character; as, in some methods of temperament, a minor third diminished by two commas approaches more nearly to the ratio 6 : 7, than to 5 : 6; but, with this limitation, the sum of harmony is nearly equal in all systems. Hence, if every one of the twelve major and minor thirds occurred equally often in the compositions which are to be performed on an instrument, it would be of no great consequence, to the sum of the imperfections, among which of the thirds they were divided: and, even in this case, the opinion of the best practical authors is, that the difference of character produced by a difference of proportions in various keys, would be of considerable advantage in the general effect of modulation. But, when it is considered, that upon an average of all the music ever composed, some particular keys occur at least twice as often as others, there seems to be a very strong additional reason for making the harmony the most perfect in those keys which are the most frequently used; since the aggregate sum of all the imperfections which occur in playing, must by this means be diminished in the greatest possible degree, and the diversity of character at the same time preserved. Indeed, in practice, this method, under different modifications, has been almost universal; for, although many have pretended to an equal temperament, yet the methods which they have employed to attain it have been evidently defective. It appears to me, that every purpose may be answered, by making C : E too sharp by a quarter of a comma, which will not offend the nicest ear; E : G*, and A^b : C, equal; F*: A* too sharp by a comma; and the major thirds of all the intermediate keys more or less perfect, as they approach more or less to C in the order of modulation. The fifths are perfect enough in every system. The results of this method are shown in Table XII. In practice, nearly the same effect may be very simply produced, by tuning from C to F, B^b, E^b, G*, C*, F* six perfect fourths; and C, G, D, A, E, B, F*, six equally imperfect fifths, Plate VI. Fig. 52. If the unavoidable imperfections of the fourths be such as to incline them to sharpness, the temperament will approach more nearly to equality, which is preferable to an inaccuracy on the other side. An easy method of comparing different systems of temperament is exhibited in Plate VII. Fig. 53, which may easily be extended to all the systems that have ever been invented.

Table XII.

A	
C	50000
B	53224
B^b	56131
A	59676
G*	63148
G	66822
F*	71041
F	74921
E	79752
E^b	83810
D	89304
C*	94723
C	100000

B	
1 C	+ .0013487
2 G, F	.0019006
3 D, B^b	.0024525
4 A, E^b	.0034641
5 E, A^b	.0044756
6 B, C*	.0049353
7 F*	.0053950

C	
1 A, E	— .0023603
2 D, B	.0029122
3 G, F*	.0034641
4 C, C*	.0044756
5 F, G*	.0049353
6 B^b, E^b	.0053950

D	
1 E^b, G*, C*, F*	— .0000000
2 F, B^b, E, B	.0004597
3 C, G, D, A	.0011562

A, shows the division of a monochord corresponding to each note, in the system proposed. B, the logarithm of the temperament of each of the major thirds. C, of the minor thirds. D, of the fifths; C and D being both negative.

Thus, Sir, I have endeavoured to advance a few steps only, in the investigation of some very obscure but interesting subjects. As far as I know, most of these observations are new; but, if they should be found to have been already made by any other person, their repetition in a connected chain of inference may still be excusable. I am persuaded also, that at least some of the positions maintained are incontrovertibly consistent with truth and nature; but, should further experiments tend to confute any opinions that I have suggested, I shall relinquish them with as much readiness as I have long since abandoned the hypothesis which I once took the liberty of submitting to the Royal Society, on the functions of the crystalline lens.

I am, &c.

Emanuel College, Cambridge,
8th July, 1799.

THOMAS YOUNG.

EXPLANATION OF THE FIGURES.

(See Plates III. IV. V. VI. and VII.)

Plate III.

Figs. 1—6. The section of a stream of air from a tube .07 inch in diameter, as ascertained by measuring the breadth of the impression on the surface of a liquid. The pressure impelling the current, was in Fig. 1, 1 inch. Fig. 2, 2. Fig. 3, 3. Fig. 4, 4. Fig. 5, 7. Fig. 6, 10.

Figs. 7—12. A similar section, where the tube was .1 in diameter, compared with the section as inferred from the experiments with two gages, which is represented by a dotted line. From this comparison it appears, that where the velocity of the current was small, its central parts only displaced the liquid; and that, where it was great, it displaced, on meeting with resistance, a surface somewhat greater than its own section. The pressure was in Fig. 7, 1. Fig. 8, 2. Fig. 9, 3. Fig. 10, 4. Fig. 11, 7. Fig. 12, 10.

Figs. 13—20. A, the half section of a stream of air from a tube .1 in diameter, as inferred from experiments with two water gages. The pressure was in Fig. 13, .1. Fig. 14, .2. Fig. 15, .5. Fig. 16, 1. Fig. 17, 3. Fig. 18, 5. Fig. 19, 7. Fig. 20, 10. The fine lines, marked B, show the result of the observations with an aperture .15 in diameter opposed to the stream; C with .3; and D with .5.

Figs. 21—23. A, the half section of a current from a tube .3 in diameter, with a pressure of .5, of 1, and of 3. B shows the course of a portion next the axis of the current, equal in diameter to those represented by the last figures.

Plate IV.

Fig. 24. The appearance of a stream of smoke forced very gently from a fine tube. Fig. 25 and 26, the same appearance when the pressure is gradually increased.

Fig. 27. See Section III.

Fig. 28. The perpendicular lines over each division of the horizontal line show, by their length and distance from that line, the extent of pressure capable of producing, from the respective pipes, the harmonic notes indicated by the figures placed opposite the beginning of each, according to the scale of 22 inches parallel to them. The larger numbers, opposite the middle of each of these lines, show the number of vibrations of the corresponding sound in a second.

Plate V.

Figs. 29—33. See Section X.

Fig. 34. The combination of two equal sounds constituting the interval of an octave, supposing the progress and regress of the particles of air equable. Figs. 35, 36, 37, a similar representation of a major third, major tone, and minor sixth.

Fig. 38. A fourth, tempered about two commas.

Fig. 39. A vibration of a similar nature, combined with subordinate vibrations of the same kind in the ratios of 3, 5, and 7.

Fig. 40. A vibration represented by a curve of which the ordinates are the sines of circular arcs increasing uniformly, corresponding with the motion of a cycloidal pendulum, combined with similar subordinate vibrations in the ratios of 3, 5, and 7.

Plate VI.

Figs. 41 and 42. Two different positions of a major third, composed of similar vibrations, as represented by figures of sines.

Fig. 43. A contracted representation of a series of vibrations. A, a simple uniform sound. B, the beating of two equal sounds nearly in unison, as derived from rectilinear figures. C, the beats of two equal sounds, derived from figures of sines. D, a musical consonance, making by its frequent beats a fundamental harmonic. E, the imperfect beats of two unequal sounds.

Fig. 44. Various forms of the orbit of a musical chord, when inflected, and when struck.

Fig. 45. Forms of the orbit, when the sound is produced by means of a bow.

Fig. 46. Epitrochoidal curves, formed by combining a simple rotation or vibration with other subordinate rotations or vibrations.

Figs. 47 and 48. The successive forms of a tended chord, when inflected and let go, according to the construction of De la Grange and Euler.

Fig. 49. The appearance of a vibrating chord which had been inflected in the middle, the strongest lines representing the most luminous parts.

Fig. 50. The appearance of a vibrating chord, when inflected at any other point than the middle.

Fig. 51. The appearance of a chord, when put in motion by a bow applied nearly at one third of the length from its end.

Fig. 52. The method of tuning recommended for common use.

Plate VII.

Fig. 53. A comparative view of different systems of temperament. The whole circumference represents an octave. The inner circle L is divided into 30103 parts, corresponding with the logarithmical parts of an octave. The next circle R shows the magnitude of the simplest musical and other ratios. Q is divided into twelve equal parts, representing the semitones of the equal temperament described by ZARLINO, differing but little from the system of ARISTOXENUS, and warmly recommended by MARPURG and other late writers. Y exhibits the system proposed in this paper as the most desirable; and P the practical method nearly approaching to it, which corresponds with the eleventh method in MARPURG's enumeration, except that, by beginning with C instead of B, the practical effect of the temperament is precisely inverted. K is the system of KIRNBERGER and SULZER; which is derived from one perfect third, ten perfect and two equally imperfect fifths. M is the system of mean tones, the *sistema participato* of the old Italian writers, still frequently used in tuning organs, approved also by Dr. SMITH for common use. S shows the result of all the calculations in Dr. SMITH's harmonics, the system proposed for his changeable harpsichord, but neither in that nor any other form capable of practical application.

Fig.1

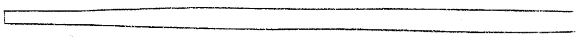


Fig.2

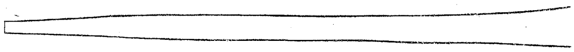


Fig.3

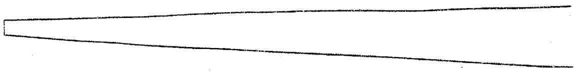


Fig.4

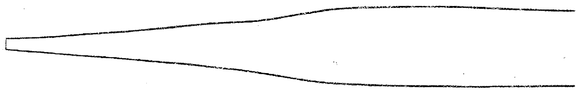


Fig.5

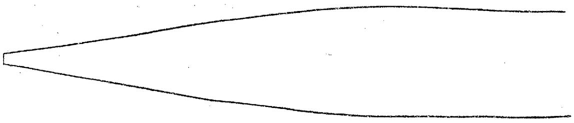


Fig.6

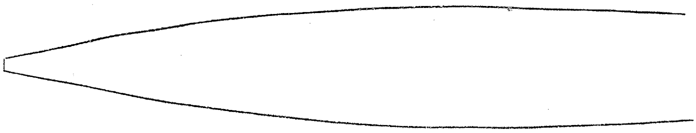


Fig.7

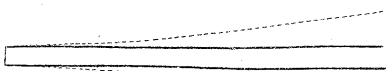


Fig.8

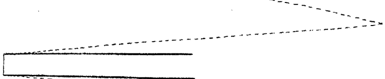


Fig.9

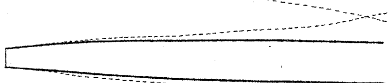


Fig.10

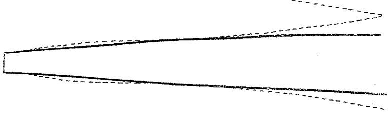


Fig.11

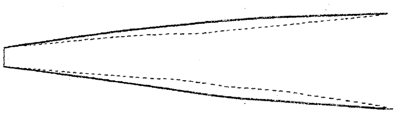


Fig.12

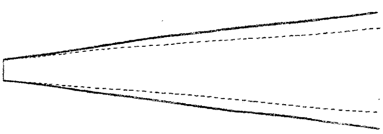
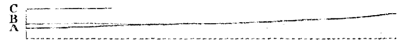


Fig.13



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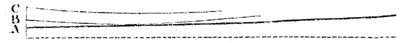


Fig.15

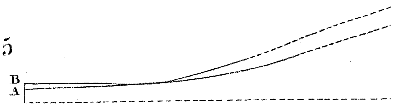


Fig.16

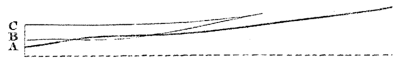


Fig.17

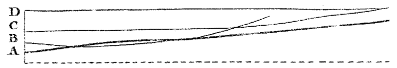


Fig.18

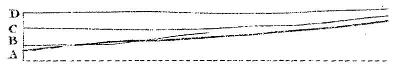


Fig.19

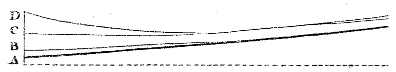


Fig.20

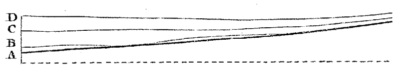


Fig.21

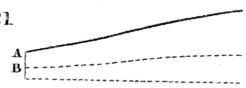
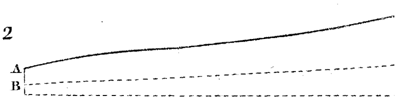


Fig.22



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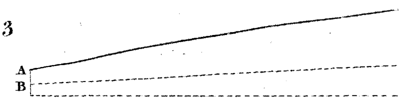

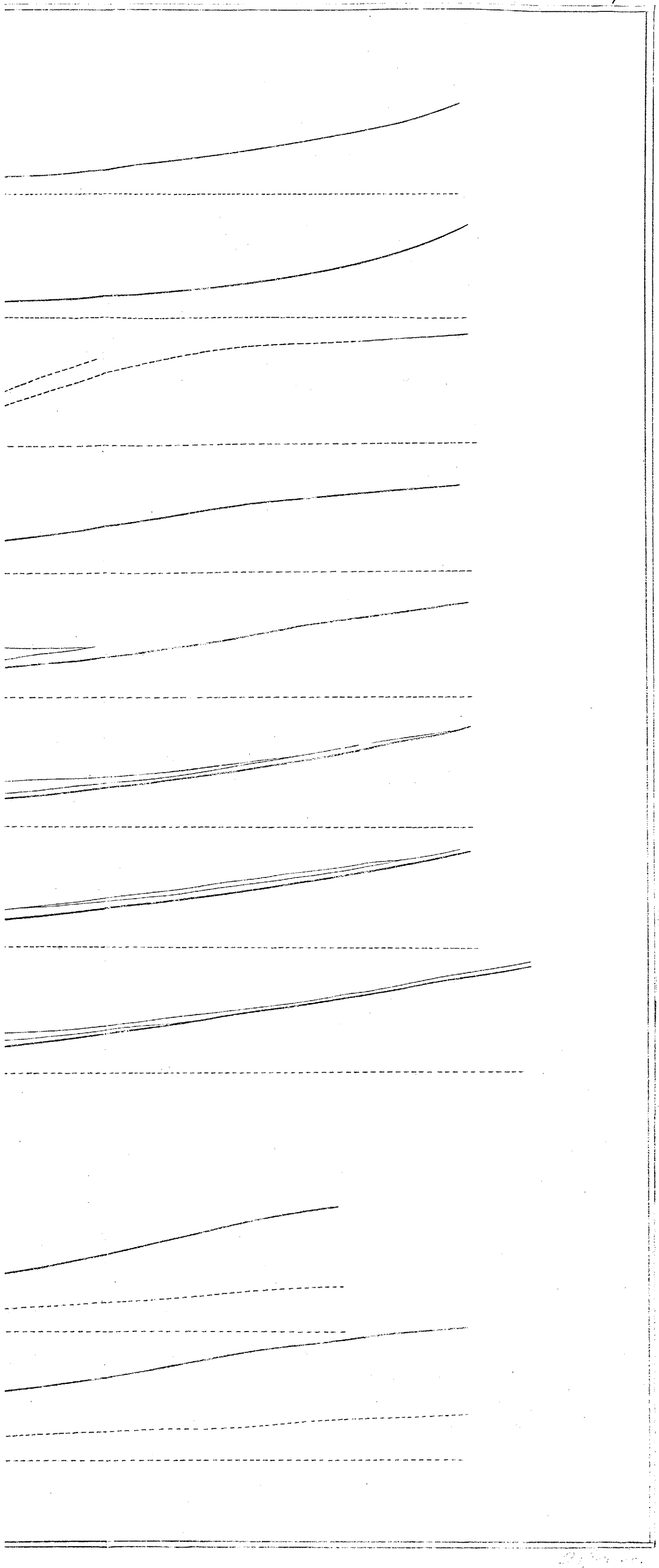

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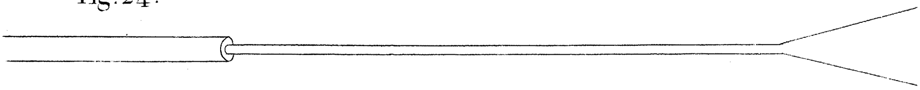
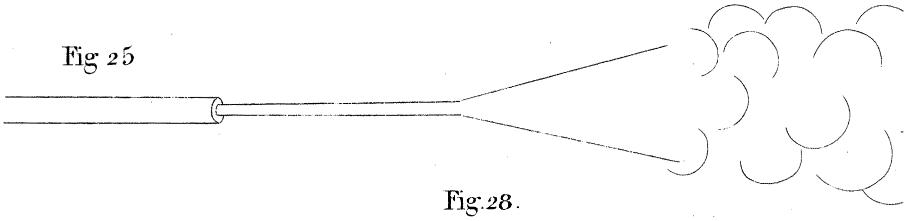
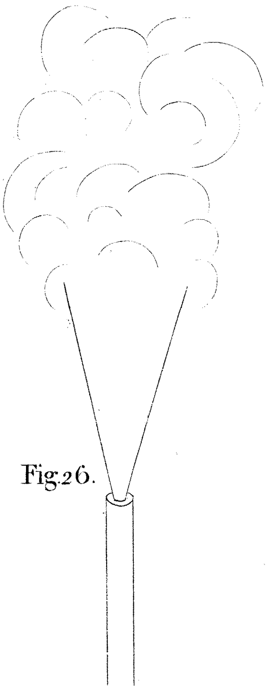


Fig 25





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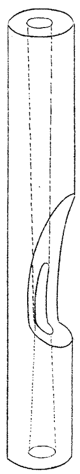
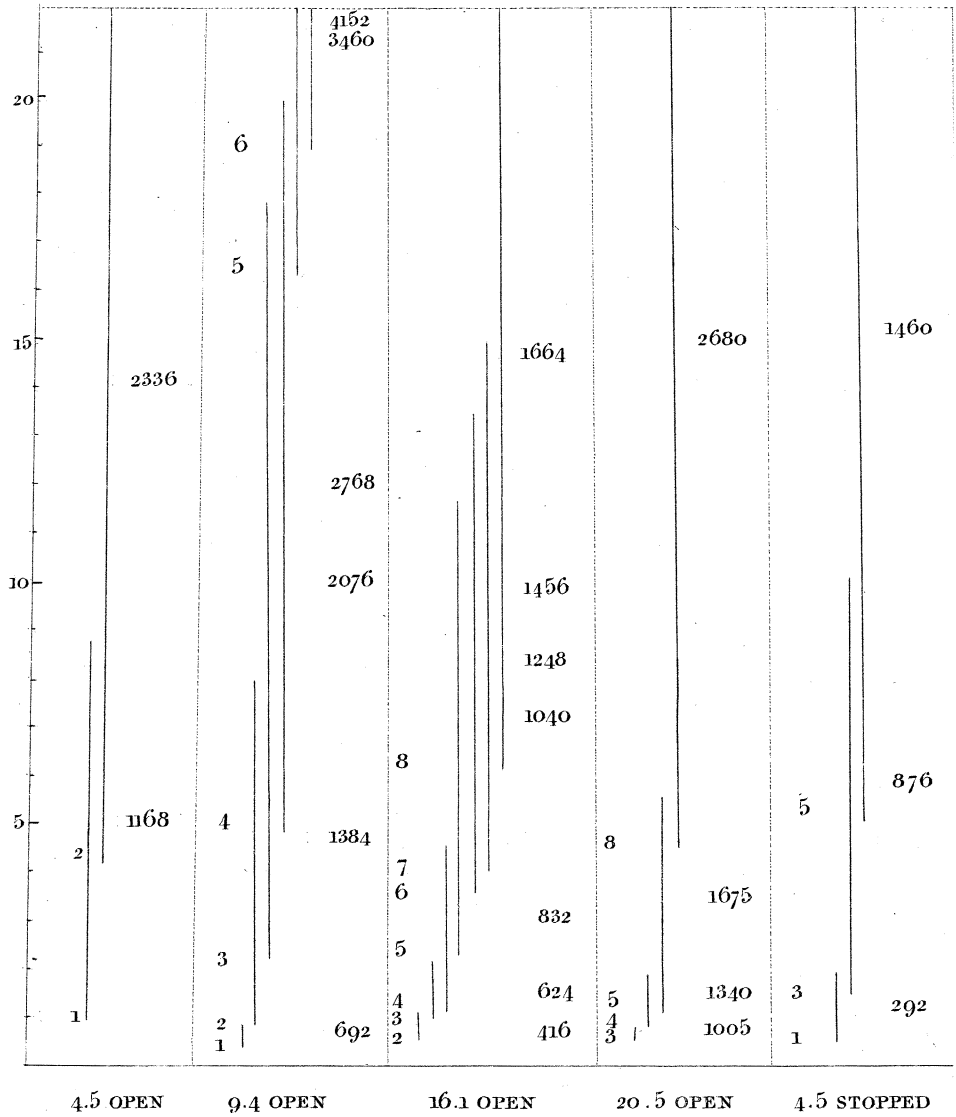
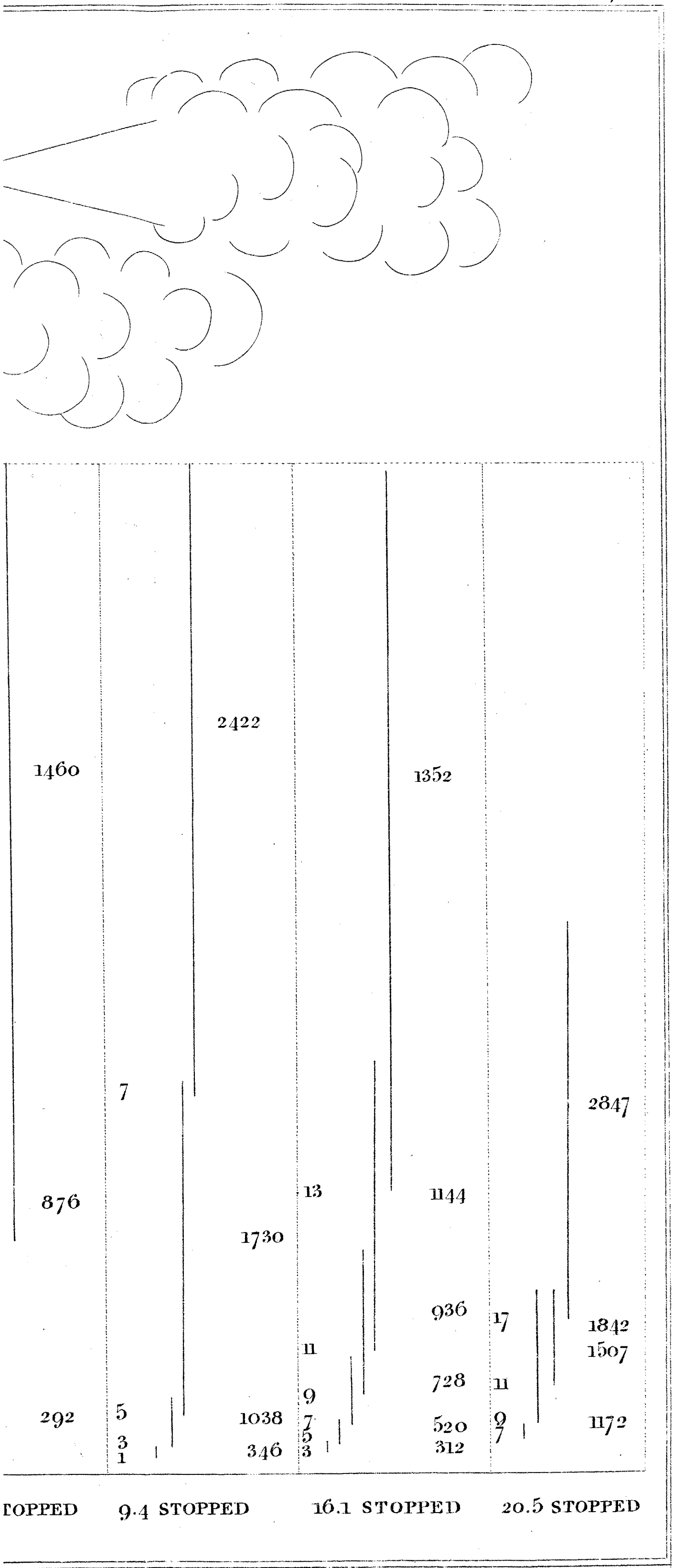


Fig:27.

Fig:28.

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9.4 STOPPED
16.1 STOPPED
20.5 STOPPED

Fig. 29

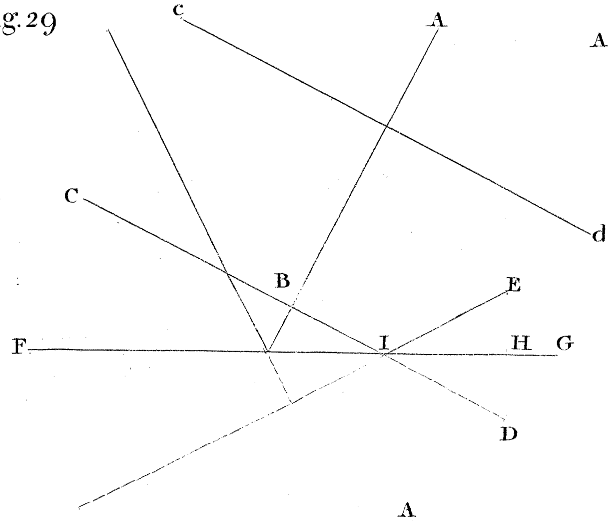


Fig. 30

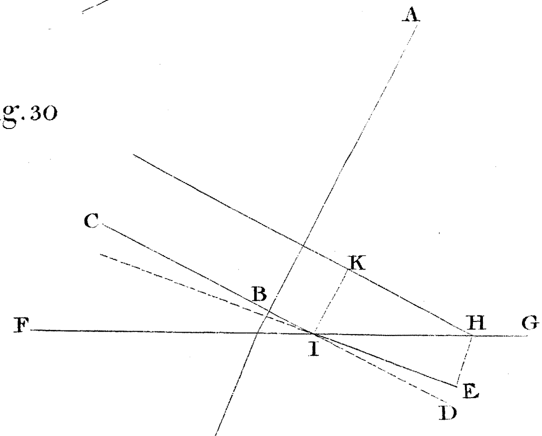


Fig. 31

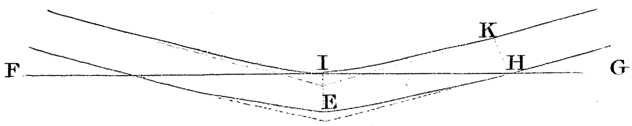


Fig. 32

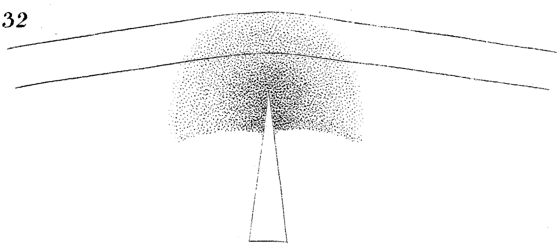
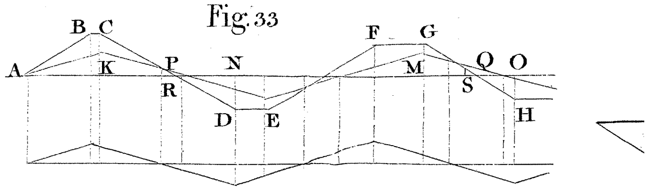


Fig. 33



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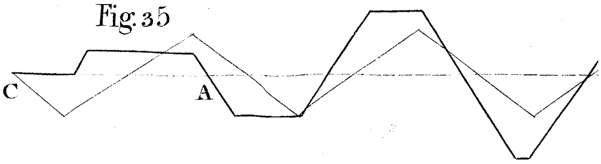

Fig. 36

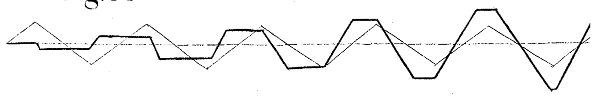

Fig. 37

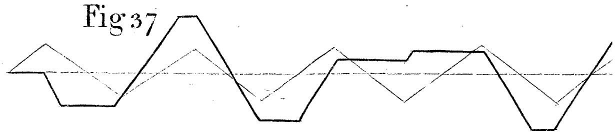

Fig. 38

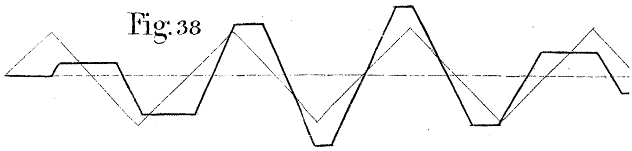

Fig. 39

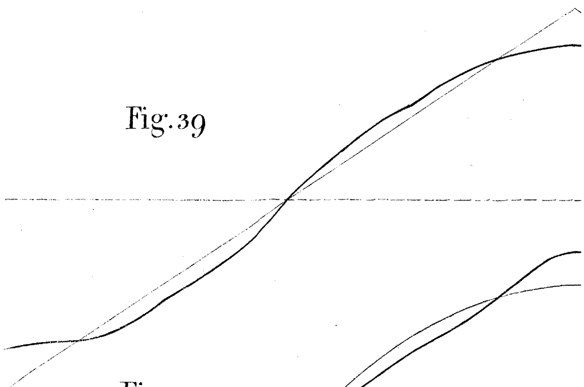

Fig. 40

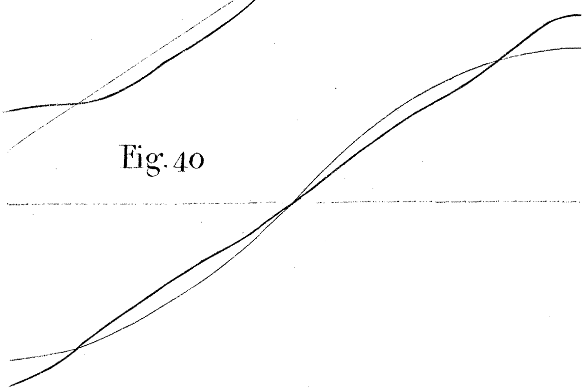

Fig: 34.

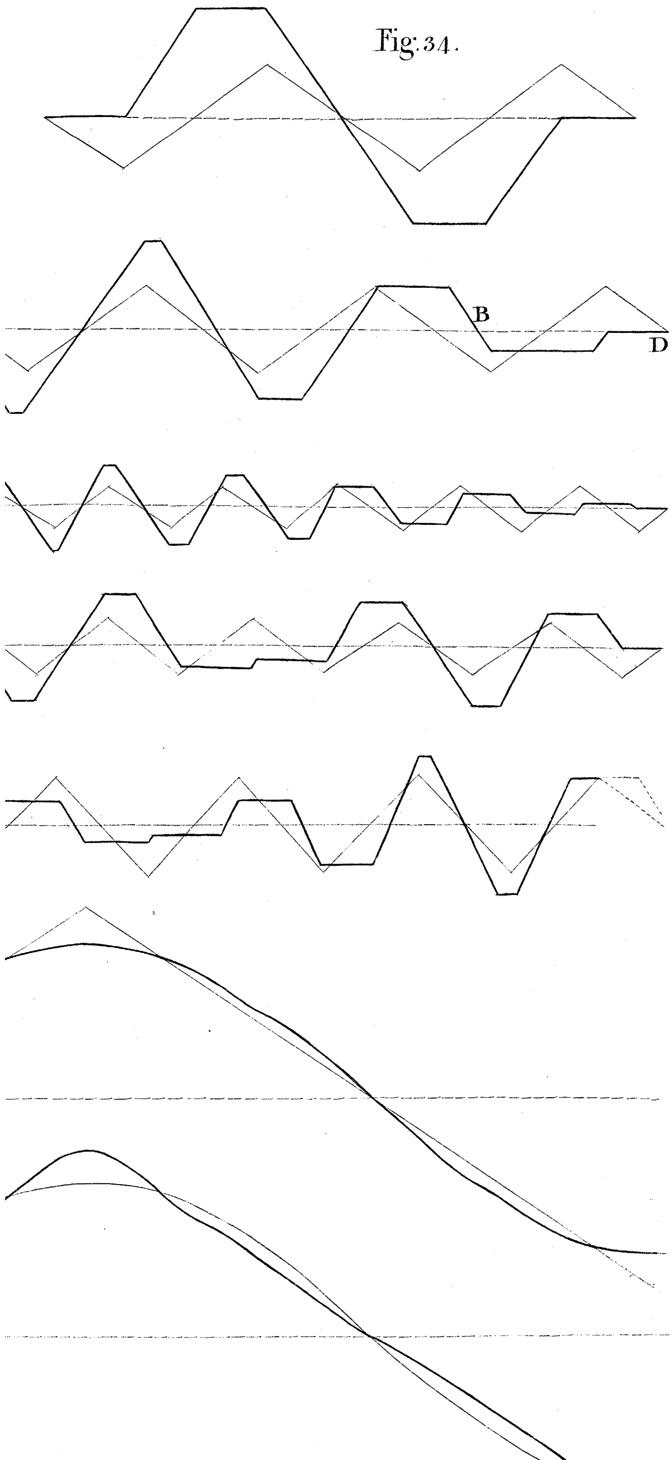

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Fig. 41

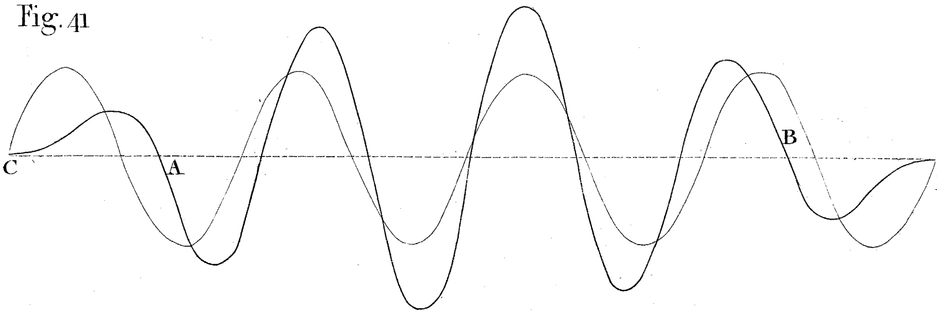


Fig. 42

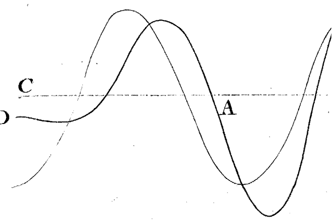


Fig. 43

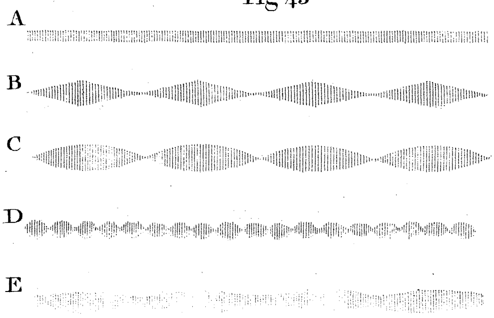


Fig. 44

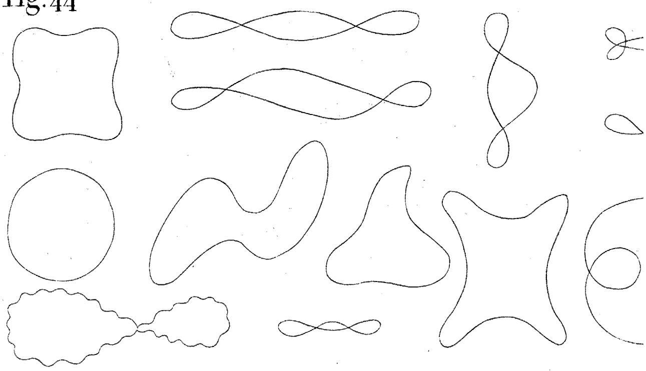


Fig. 47

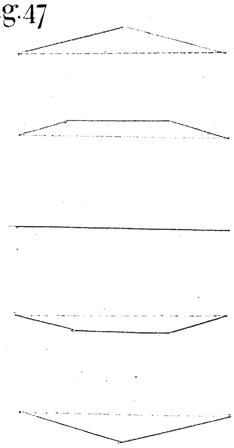


Fig. 48

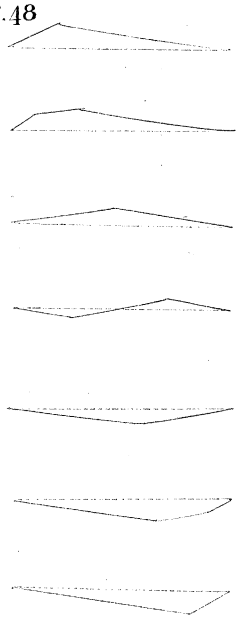


Fig. 45

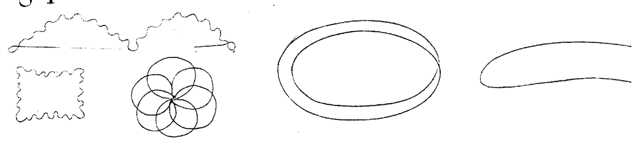


Fig. 46

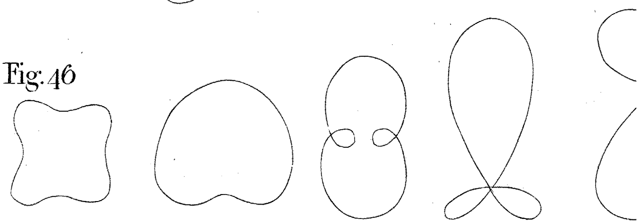


Fig. 49

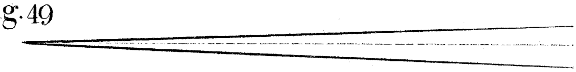


Fig. 50

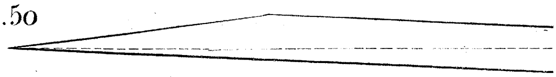


Fig. 51

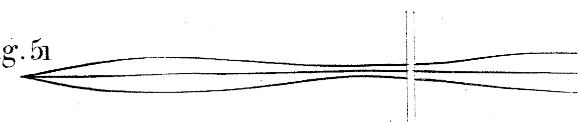
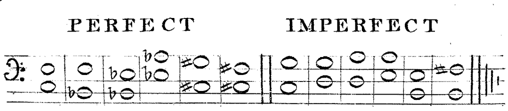


Fig. 52



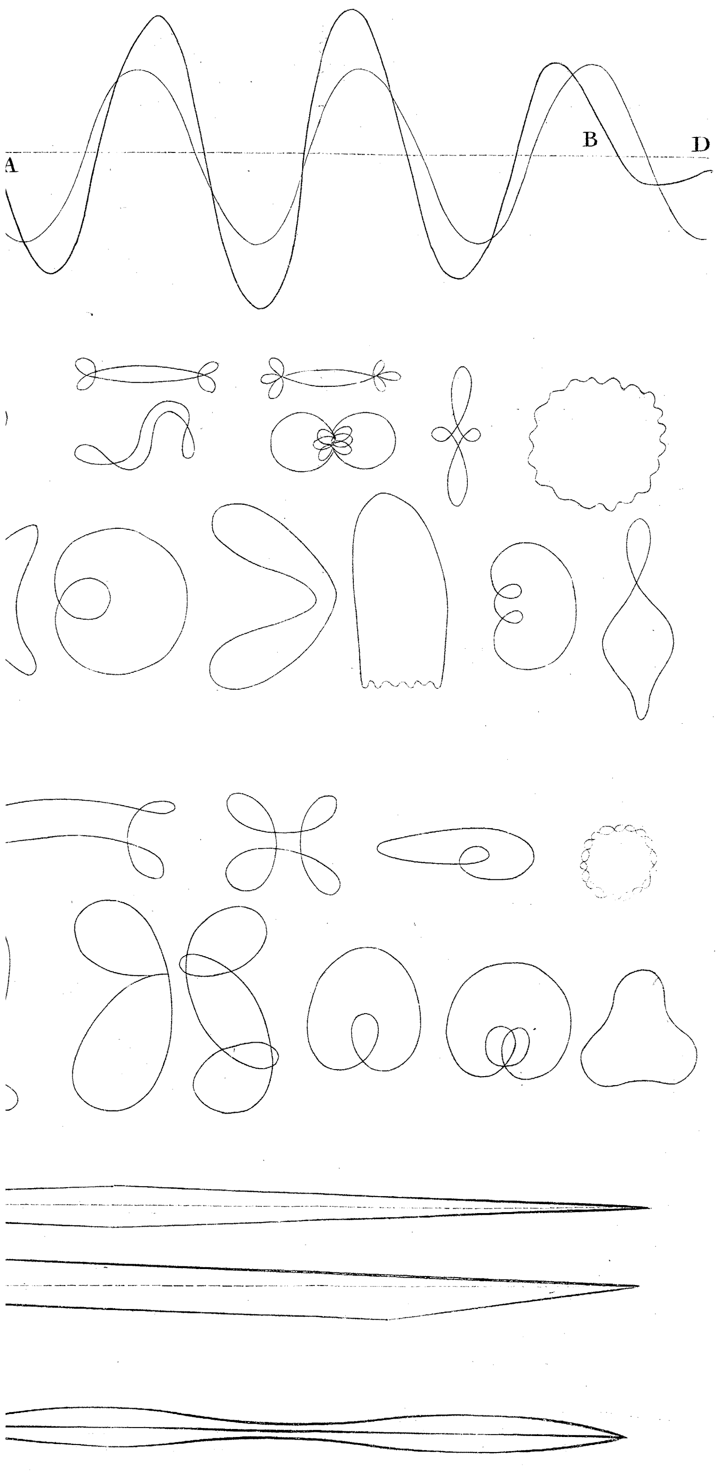


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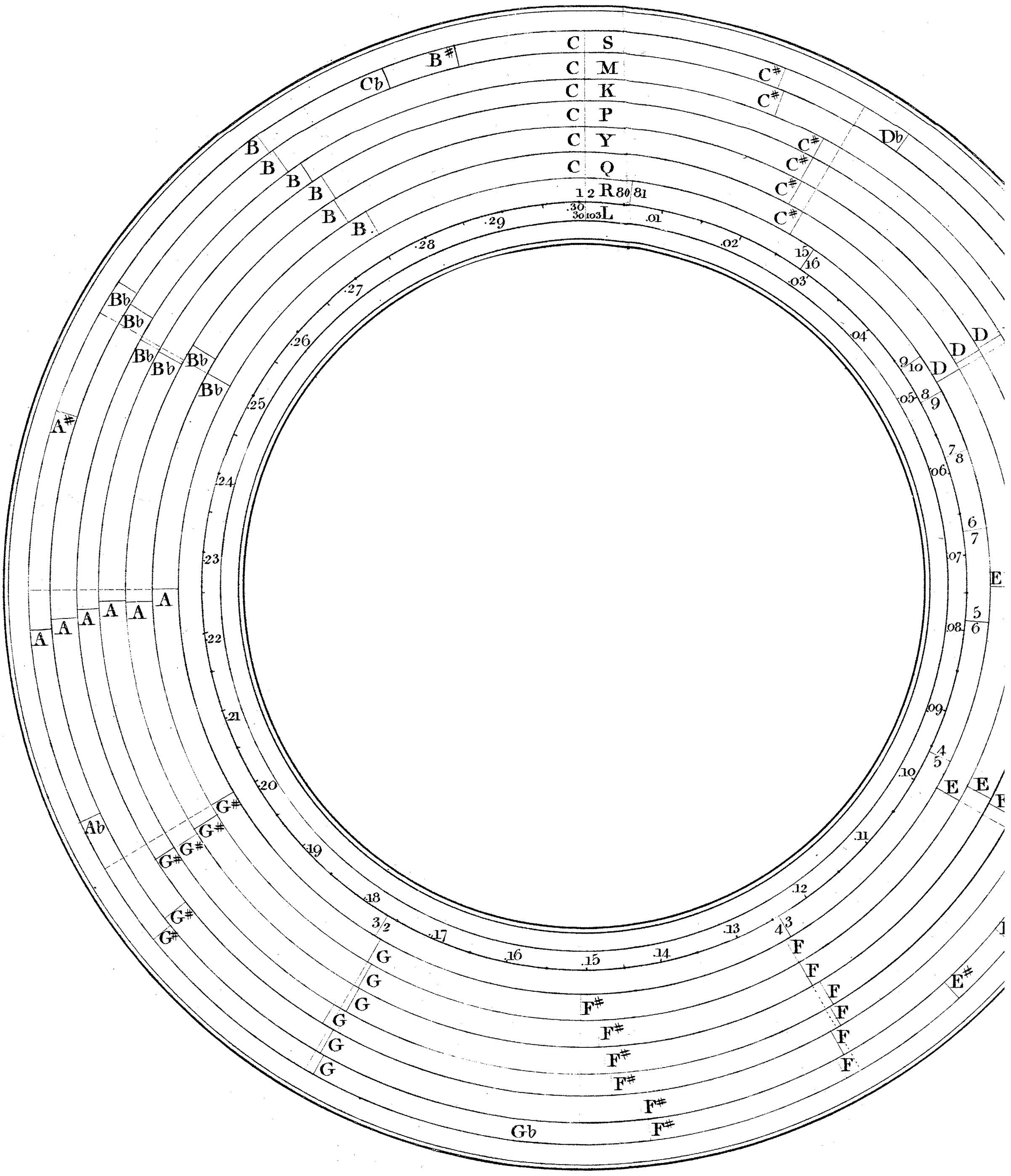
Fig. 53.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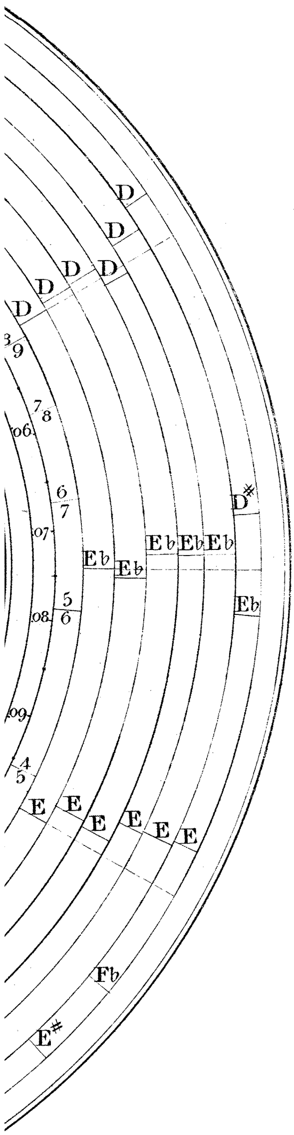

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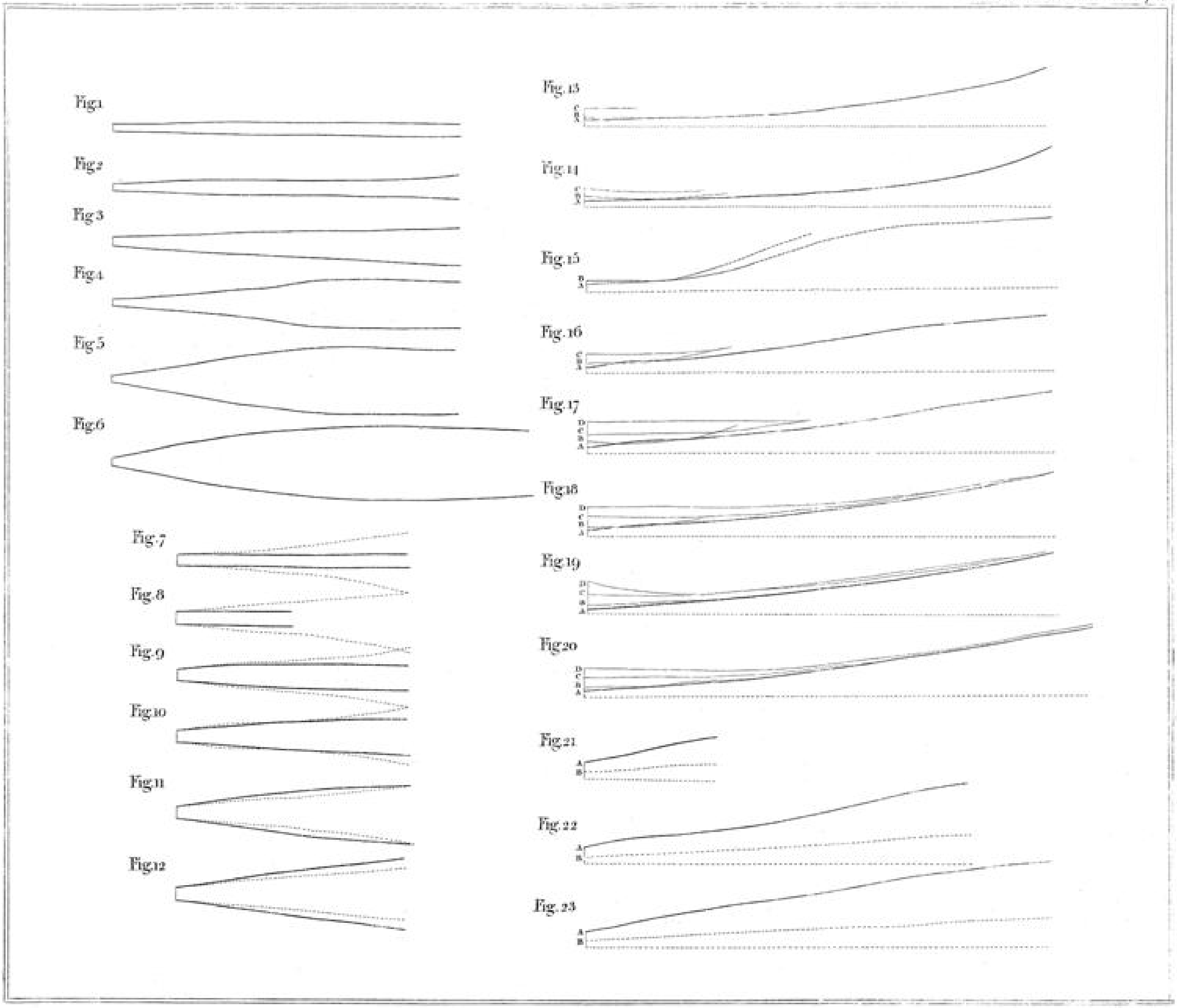
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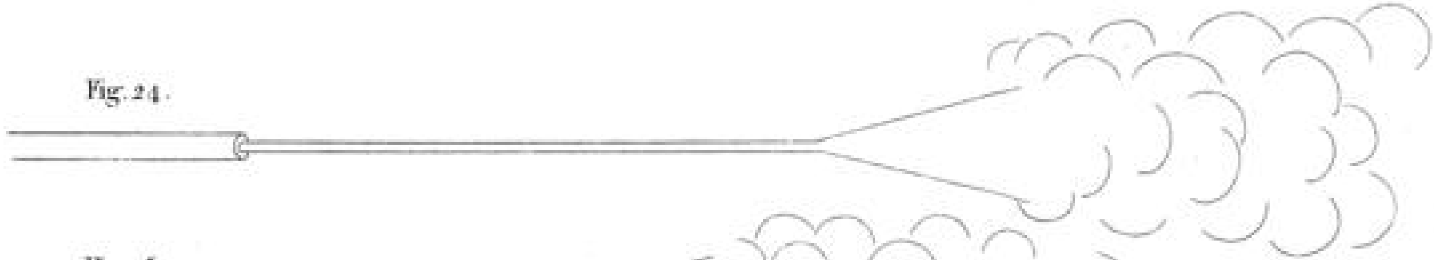


Fig 25

Fig. 26.

Fig. 27.

Fig. 28.

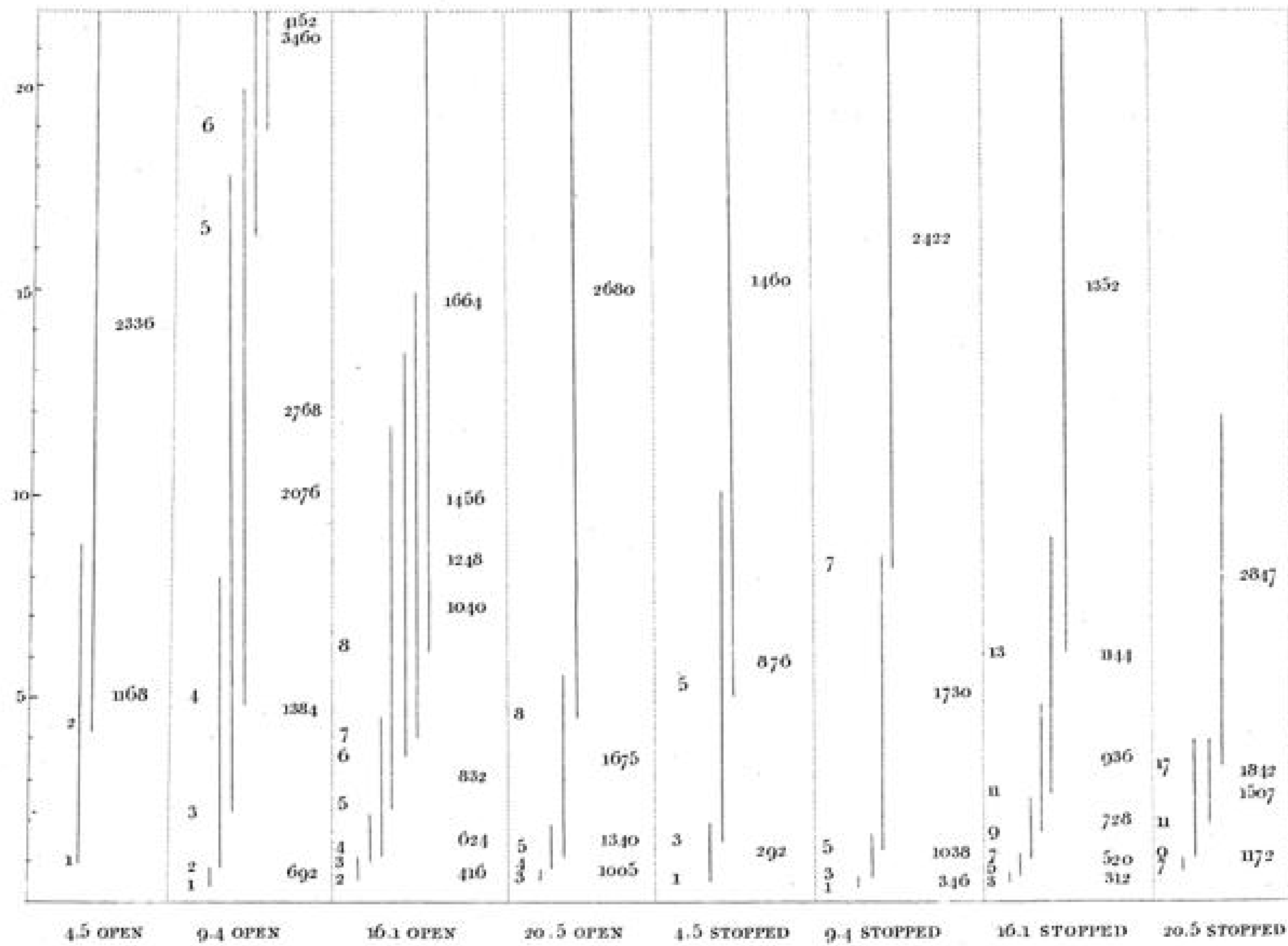


Fig. 29

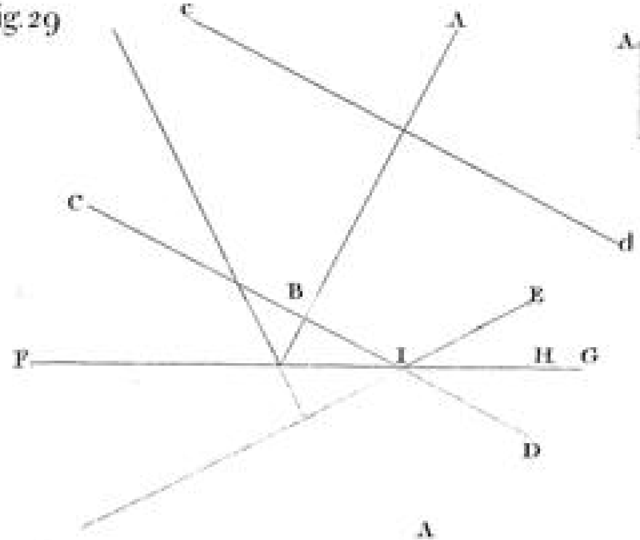


Fig. 30

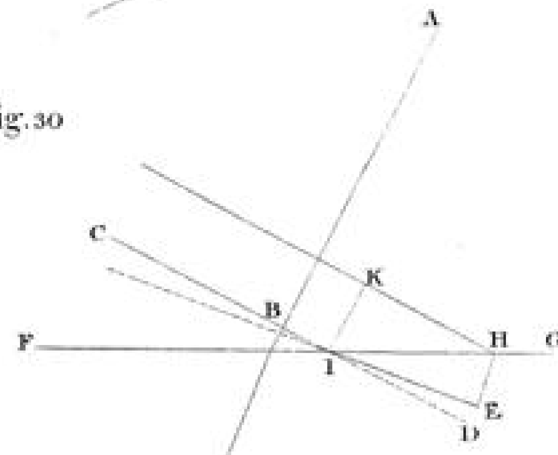


Fig. 31

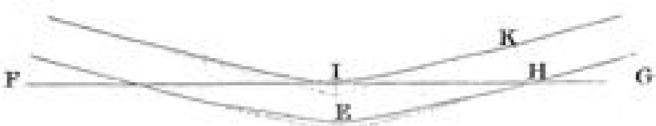


Fig. 32

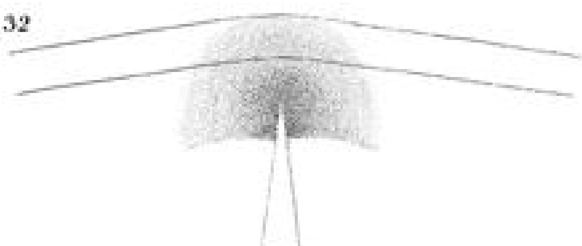


Fig. 33

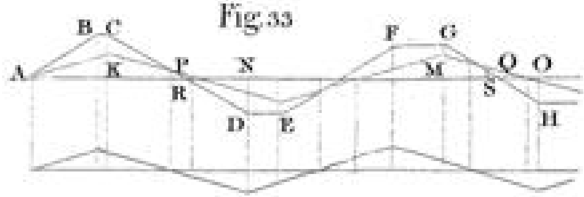


Fig. 34.

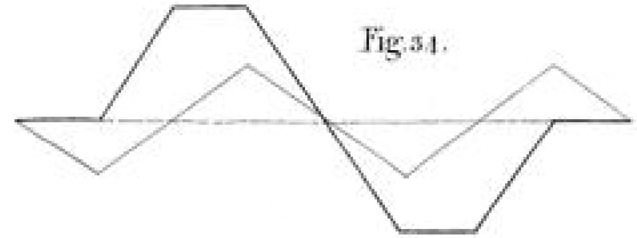
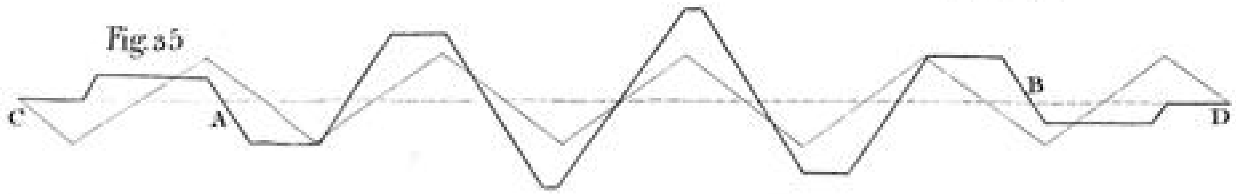


Fig. 35



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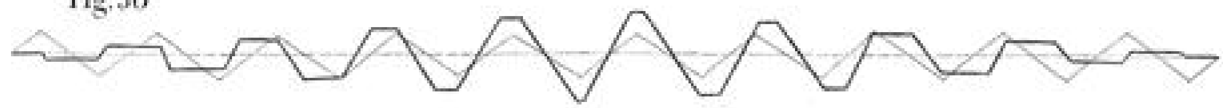


Fig. 37

Fig. 38

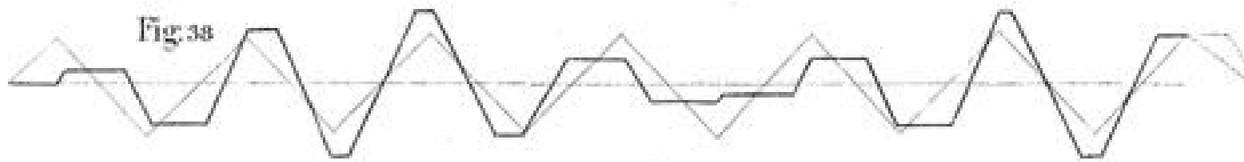


Fig. 39

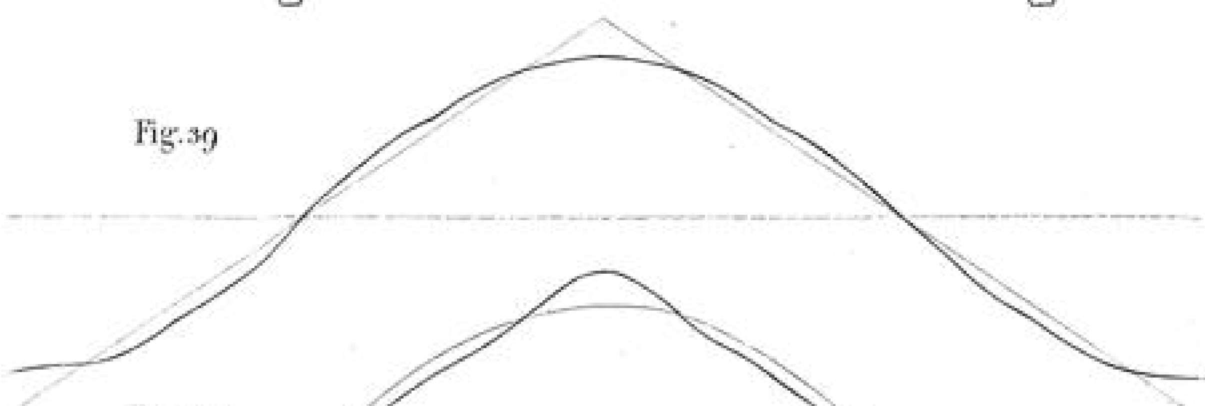
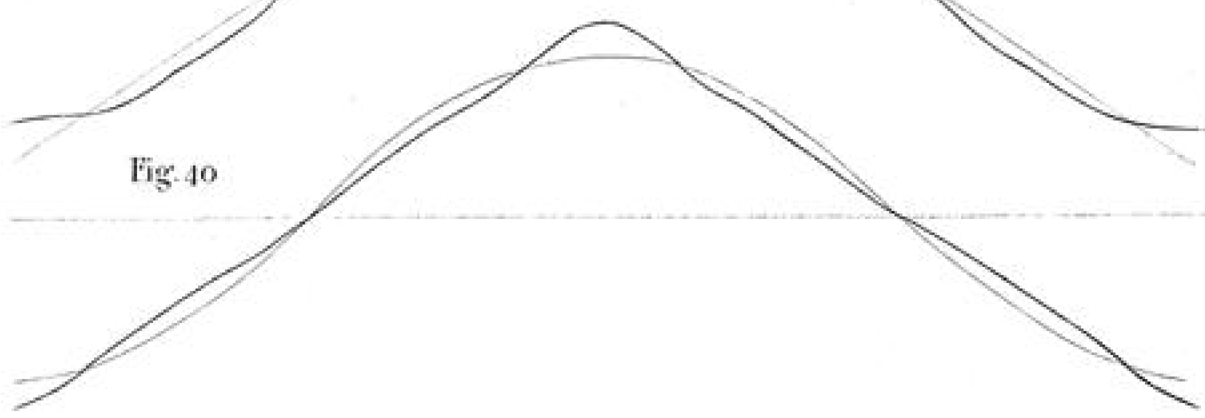


Fig. 40



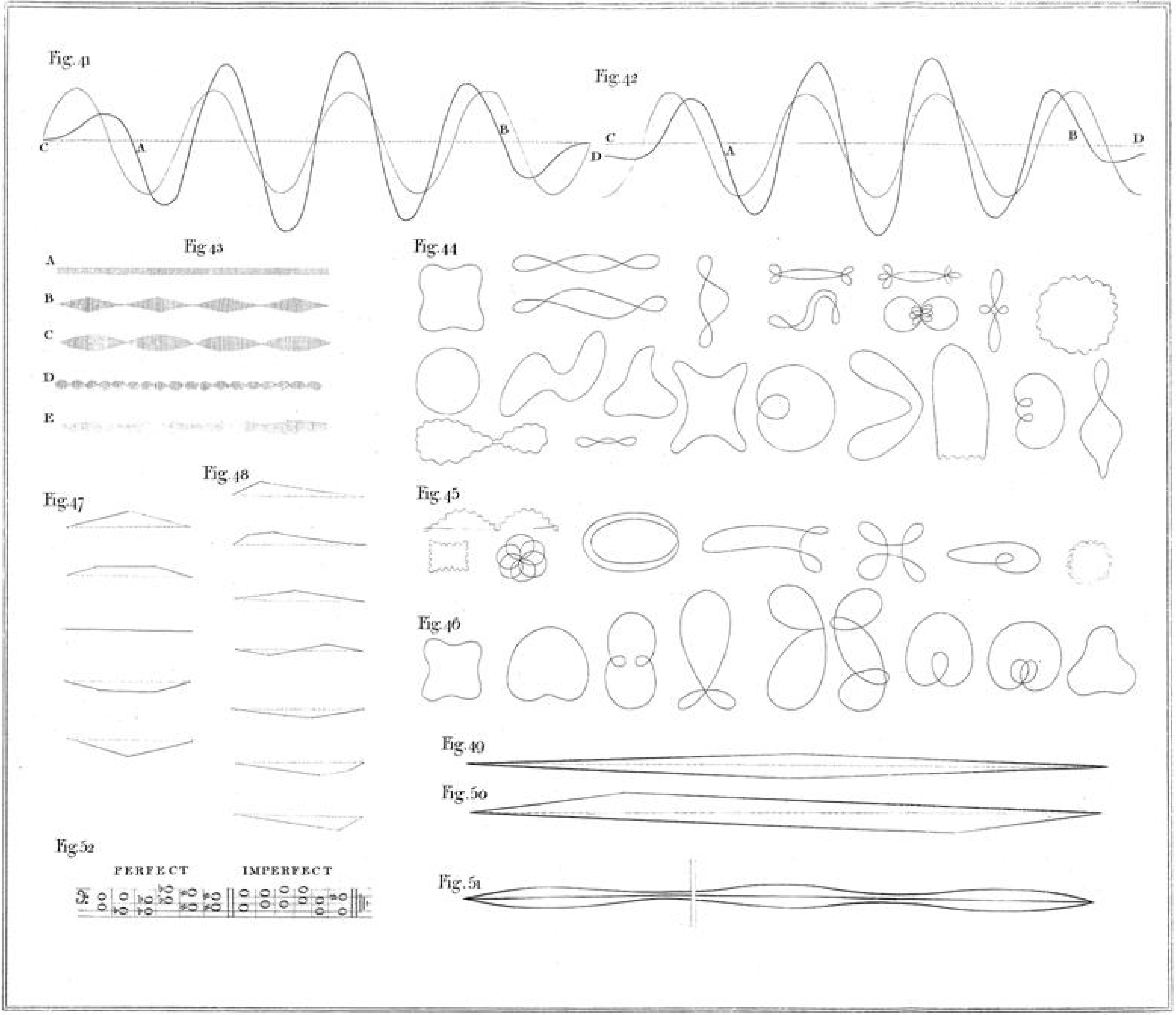


Fig. 41
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Fig. 48
Fig. 49
Fig. 50
Fig. 51
Fig. 52
PERFECT
IMPERFECT

Fig. 53.