II. *The* Bakerian *Lecture. Observations on the Theory of the Motion and Resistance of Fluids; with a Description of the Construction of Experiments, in order to obtain some fundamental Principles. By the Rev.* Samuel Vince, *A. M. F. R. S.*

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However satisfactory the general principles of motion may be, when applied to the action of bodies upon each other, in all those circumstances which are usually included in that branch of natural philosophy called mechanics, yet the application of the same principles in the investigation of the motions of fluids, and their actions upon other bodies, is subject to great uncertainty. That the different kinds of airs are constituted of particles endued with repulsive powers, is manifest from their expansion when the force with which they are compressed is removed. The particles being kept at a distance by their mutual repulsion, it is easy to conceive that they may move very freely amongst each other, and that this motion may take place in all directions, each particle exerting its repulsive power equally on all sides. Thus far we are acquainted with the constitution of these fluids; but with what absolute degree of facility the particles move, and how this may be affected under different degrees of compression, are circumstances of which we are totally ignorant. In respect to those fluids which are denominated liquids, we are still less acquainted with their nature. If we suppose their particles to be in contact, it is extremely difficult to conceive how they can move amongst each other with such extreme facility, and produce effects in directions opposite to the impressed force without any sensible loss of motion. To account for this, the particles are supposed to be perfectly smooth and spherical. If we were to admit this supposition, it would yet remain to be proved how this would solve all the phænomena, for it is by no means self-evident that it would. If the particles be not in contact, they must be kept at a distance by some repulsive power. But it is manifest that these particles attract each other, from the drops of all perfect liquids affecting to form themselves into spheres. We must therefore admit in this case both powers, and that where one power ends the other begins, agreeable to Sir ISAAC NEWTON's * idea of what takes place not only in respect to the constituent particles of bodies, but to the bodies themselves. The incompressibility of liquids (for I know no decisive experiments which have proved them to be compressible) seems most to favour the former supposition, unless we admit, in the latter hypothesis, that the repulsive force is greater than any human power which can be applied. The expansion of water by heat, and the possibility of actually converting it into two permanently elastic fluids, according to some late experiments, seem to prove that a repulsive power exists between the particles; for it is hard to conceive that heat can actually create any such new powers, or that it can of itself produce any such effects. All these uncertainties respecting the constitution of fluids must render the conclusions deduced from any *theory* subject to considerable

* See his Optics, Que. 31.

errors, except *that* which is founded upon such experiments as include in them the consequences of all those principles which are liable to any degree of uncertainty.

A fluid being composed of an indefinite number of corpuscles, we must consider its action, either as the joint action of all the corpuscles, estimated as so many distinct bodies, or we must consider the action of the whole as a mass, or as one body. In the former case, the motion of the particles being subject to no regularity, or at least to none that can be discovered by any experiments, it is impossible from this consideration to compute the effects; for no calculation of effects can be applied when produced by causes which are subject to no law. And in the latter case, the effects of the action of one body upon another differ so much, in many respects, from what would be its action as a solid body, that a computation of its effects can by no means be deduced from the same principles. In mechanics no equilibrium can take place between two bodies of different weights, unless the lighter acts at some mechanical advantage; but in hydrostatics, a very small weight of fluid may, without its acting at any mechanical advantage whatever, be made to balance a weight of any magnitude. In mechanics, bodies act only in the direction of gravity; but the property which fluids have of acting equally in all directions, produces effects of such an extraordinary nature as to surpass the power of investigation. The indefinitely small corpuscles of which a fluid is composed, probably possess the same powers, and would be subject to the same laws of motion, as bodies of finite magnitude, could any two of them act upon each other by contact; but this is a circumstance which certainly never takes place in any of the aerial fluids, and probably not in any liquids. Under the circumstances, therefore, of an indefinite number of bodies acting upon each other by repulsive powers, or by absolute contact under the uncertainty of the friction which may take place, and of what variation of effects may be produced under different degrees of compression, it is no wonder that our theory and experiments should be so often found to disagree.

Sir ISAAC NEWTON seems to have been well aware of all these difficulties, and therefore in his PRINCIPIA he has deduced his laws of resistance, and the principles upon which the times of emptying vessels are founded, entirely from experiment. He was too cautious to trust to theory alone, under all the uncertainties to which he appears to have been sensible it must be subject. He had, in a preceding part of that great work, deduced the general principles of motion, and applied them to the solution of problems which had never before been attempted; but when he came to treat of fluids, he saw it was necessary to establish his principles upon experiments; principles, not indeed mathematically true, like his general principles of motion before delivered, but, under certain limitations, sufficiently accurate for all practical purposes.

The principle to be established in order to determine the time of emptying a vessel through an orifice at the bottom, is the relation between the velocity of the fluid at the orifice and the altitude of the fluid above it. Most writers upon this subject have considered the column of fluid over the orifice as the expelling force, and from thence some have deduced the velocity at the orifice to be that which a body would acquire in falling down the *whole* depth of the fluid; and others that acquired in falling through *half* the depth, without any regard to the magnitude of the orifice; whereas it is manifest from experiment, that the velocity at the orifice, the depth of the fluid being the same, depends upon the proportion which the magnitude of the orifice bears to the magnitude of the bottom of the vessel, supposing, for instance, the vessel to be a cylinder standing on its base; and in all cases the velocity, *cæteris paribus,* will depend upon the ratio between the magnitude of the orifice and that of the surface of the fluid. Conclusions thus contrary to matter of fact show, either that the principle assumed is not true, or that the deductions from it are not applicable to the present case. The most celebrated theories upon this subject are those of D. BERNOUILLI and M. D'ALEMBERT; the *former* deduced his conclusions from the principle of the *conservatio virium vivarum,* or as he calls it, the *equalitas inter descensum actualem ascensumque potentialem,* where by the *descensus actualis* he means the actual descent of the centre of gravity, and by the *ascensus potentialis* he means the ascent of the centre of gravity, if the fluid which flows out could have its motion directed upwards; and the *latter* from the principle of the *equilibrium* of the fluid. This principle of M. D'ALEMBERT leads immediately to that assumed by D. BERNOUILLI, and consequently they both deduce the same fluxional equation, the fluent of which expresses the relation between the velocity of the fluid at the orifice, and the perpendicular altitude of the fluid above it. How far the principles here assumed can be applied in our reasoning upon fluids, can only be determined by comparing the conclusions deduced from them with experiments.

The fluxional equation above mentioned cannot in general be integrated, and therefore the relation between the velocity of the fluid at the orifice and its depth cannot from thence be determined in all cases. If the magnitude of the orifice be indefinitely less than that of the surface of the fluid, the equation gives the velocity of the effluent fluid to be equal to that which a body would acquire by falling *in vacuo* through a space equal to the depth of the fluid. But the velocity here determined is not that at the orifice, but at a small distance from the orifice; for the fluid flowing to the orifice contracts the stream, and the velocity being inversely as the area of the section, the velocity continues to increase as long as the stream, by the expelling force of the fluid, keeps diminishing, and when the stream ceases to be contracted by that force, at that section of the stream called the *vena contracta*, the velocity is that which a body would acquire in falling through a space equal to the depth of the fluid. If, therefore AB *cd* EF (Tab. II. fig. 1.) be the vessel, *cd* the orifice, *c m n d* the form of the stream till it comes to the *vena contracta*, then this investigation supposes AB *c m n d* EF to be the form of the vessel, and *m n* the orifice, the fluid flowing through *c m n d* just as if the vessel were so continued. But as the proposition is to find the velocity of the fluid going out of the vessel, it may perhaps appear an arbitrary assumption to substitute the orifice *m n* instead of *c d*, when no such a quantity as *m n* appears in the investigation. If, however, we grant that the expelling force must act without any diminution until the fluid comes to *m n*, it seems that from the principles here assumed we ought to substitute *m n* instead of *c d*, as otherwise we get the velocity generated by the action of only a part of the force. The conclusion here deduced agrees very well with experiment; but an application of the same principles to another case differs so widely from matter of fact, as to render it very doubtful how far the principles here applied can be admitted. And if we were to grant the application of the principles here assumed, so far as regards the determination of the velocity, yet the time of emptying a vessel can by no means be deduced from it.

In order to determine the time of emptying a vessel, we must know both the area of the orifice $c\,d$, and the velocity at that orifice. Now the theory gives only the velocity at $m\,n$; and as it gives not the ratio of $m\,n$ to $c\,d$, the velocity at the orifice cannot be deduced from thence, and therefore we cannot find the time of emptying. No theory whatever has attempted to investigate the ratio of mn to cd; it is well known that that is only to be determined by an actual mensuration. When the orifice is very small, Sir ISAAC NEWTON found the ratio to be that of 1 to $\sqrt{2}$; when the orifice is larger, the ratio approaches nearer to that of equality. We cannot therefore, even in the most simple case, determine, by theory alone, the time in which a vessel will empty itself.

If A B C D (fig. 2.) be a vessel filled with a fluid, and a pipe $m\,n\,r\,s$ be inserted at the bottom, $m\,n$ being very small in respect to B C, then, according to the theory of D. BERNOUILLI, the fluid ought to flow out of the pipe at $r\,s$ with the same velocity it would out of a vessel A L M D through the orifice $r\,s$. Now in this latter case, the velocity, according to his own principles, varies as the square root of L A, and therefore it varies in the same ratio in the former case; hence if the length $m\,r$ of the pipe bears but a very small proportion to A B, the velocity with which the fluid flows out of the pipe will be very nearly equal to the velocity with which it would flow through an orifice at the bottom equal to rs or mn, the pipe being supposed to be cylindrical. To find how far this conclusion agrees with experiment, I made a cylinder 12 inches deep, and at the bottom I made a small circular orifice, whose area was about the 130th part of the area of the bottom of the cylinder: I also put a cylindrical pipe into the bottom, whose internal diameter was exactly equal to that of the hole, and length 1 inch. Hence, according to the theory, the velocity of the fluid out of the pipe ought to be to the velocity out of the orifice as $\sqrt{13} : \sqrt{12}$, or as 26 : 25 nearly. But by experiment, the quantity of fluid which run through the pipe in 12″ (the vessel being kept full) was to the quantity which run through the orifice in the same time, very nearly in the ratio of 4 to 3, and consequently that ratio expresses the ratio of the velocities; a consequence totally different from that which the theory gives. I then took a vessel of a different base, but the same altitude, and altered the diameter of the orifice and pipe, still keeping them equal, and made the pipe only half an inch long; in this case the velocities, by the theory, ought to have been in the ratio of $\sqrt{12{,}5}$ to $\sqrt{12}$, or as 49 to 48 nearly; whereas by experiment the ratio of the velocities came out the same as before, that is, as 4 to 3 nearly. I then reduced the pipe to the length of a quarter of an inch, and in that case the velocity did not sensibly differ from that through the orifice. Upon examining the stream, in consequence of this great difference in the two cases, when the lengths of the pipes differed by so small a quantity, I found that in the latter case the stream did not fill the pipe, as it did in the former case, but that the fluid was contracted as when it run through the simple orifice. At what length of pipe the stream will cease to fill it, is a circumstance to which no theory has ever been applied, but the determination thereof must be a matter of experiment entirely.

I next inserted pipes of different lengths, and found that when the length of the pipe was equal to the depth of the vessel, the velocity of the effluent fluid by theory was to that by experiment as about 7 to 6; and by increasing the length of the pipe, the ratio approached nearer to that of equality. In long pipes, therefore, the difference between theory and experiment is not greater than what might be expected from the friction of the pipes, and other circumstances which may be supposed to retard the velocity.

If the pipe be conical, increasing downwards, the velocity, by theory, is still the same, and consequently the quantity run out will be in proportion to the magnitude of *r s*. As long as the expelling force can keep the tube full, this appears to be the case; but by increasing the orifice *r s*, the pipe will, at a certain magnitude, cease to be kept full; at what time this happens must depend entirely upon experiment. But if the pipe decrease, having its orifice *r s* equal to that of a cylindrical pipe of the same length, the velocity through the former appears, from the experiment I made, to be greater than through the latter in the ratio of 14 to 11.

If the pipe *m r* (fig. 3.) be inserted horizontally into the side of a vessel, the velocity at the orifice *r s*, by theory, is always in proportion to the square root of the altitude C D, the orifice being still supposed to be very small compared with the bottom of the vessel. By trying the experiment with pipes of different lengths and of the same diameter, beginning with the shortest and increasing them, it appears that the velocity first increases and then decreases; and this is a circumstance which has been before observed. If *r s* be greater than C *m*, the quantity of fluid which flows out in a given time (the vessel being kept full) appears to be increased in proportion to the increase of *r s*, as long as the expelling force is able to keep the pipe full; but at what magnitude of *r s* this effect ceases must be determined by experiment. If *r s* be less than C *m*, the quantity which flows out is greater than if the pipe were cylindrical, and of the same diameter as *r s*.

The velocities of fluids spouting upwards through an orifice or pipe has not been considered by BERNOUILLI; but the following experiments will show the effects in this case. Let A B C D E F (Tab. II. fig. 4.) be a vessel filled with a fluid, *r* an orifice, *x*, *y*, *z*, three pipes each an inch long, having their tops on an horizontal line with the orifice; *x* is cylindrical, of the same diameter as that of the orifice; *y* is conical, increasing upwards, of the same diameter at the bottom as the orifice; *z* decreases upwards, of the same diameter at the top as the orifice. In 12″, the quantities which run out through the orifice and pipes *x*, *y*, *z*, (the vessel being kept full) were found to be in the ratio of 7, 9.4, 11.2 and 10.7. Hence the ratio of the velocities through the orifice and pipe *x* appears to be very nearly in the ratio of 3 to 4, agreeable to what was found to take place for an orifice and short pipe at the bottom. The quantity which run out of the pipe *y* increased by increasing the diameter at the top, in proportion to that area as nearly as could be ascertained, as long as the expelling force could keep it full; and a greater quantity run out of the pipe *z* than through the orifice. All this is agreeable to what was found to take place under similar circumstances when the orifice and pipes were inserted at the bottom. So far therefore as the theory can be applied when the fluid descends perpendicularly, it appears to be applicable also to the case when it spouts upwards.

At the bottom of the vessel A B C D (Tab. II. fig. 5.) having an orifice $r\,s$, I inserted a pipe $a\,x\,y\,z\,w\,v$ conical at the top and cylindrical downwards from it, having the diameter of the cylindrical part equal to that of the orifice, and directly under it. I then stopped the orifice $s\,r$ within, and filled the vessel, and expected, that as there was now no pipe immediately connected with the orifice, the fluid would form the *vena contracta* as if there was no pipe, and that the velocity at the orifice would be the same as through a simple orifice; whereas I found the velocity to be greater, very nearly in the ratio of $\sqrt{2}$ to 1, the length of the pipe being equal to the depth of the cylinder. It appears therefore to flow out with about the same velocity as if the pipe had been continued to the orifice. The fluid therefore must have flowed from the orifice in a cylindrical form, for the pipe was observed to be filled. I see no cause which could prevent the *vena contracta* from being formed. I then stopped the pipe at the bottom $y\,z$, and filled the vessel and pipe, and found the circumstances to be exactly the same.

In order to determine whether there was any pressure of the fluid against the sides of the pipes as it passed through in all their different situations, I pierced some small holes in them at different parts. In the cylindrical pipes, and those in the form of increasing cones, the fluid passed by the holes without being projected out, or without having the least tendency to issue through them; but in the decreasing cones the fluid spouted out at the holes. In the former cases therefore there was no pressure against the sides of the pipes, but in the latter case there was.

In respect to the motion of the fluid through any of the pipes, I found no difference whether I stopped the pipe at the end of the tube which enters into the vessel, in which case the motion began when the tubes were empty, or whether at the other end, in which case they were full at the commencement of the motion. That the fluid should flow into the top of the pipe faster than it would through an orifice, may probably, in part at least, be owing to the adhesion of the fluid to the pipe, and be thus explained. Although the horizontal motion of the fluid towards the orifice accelerates the velocity after it escapes from the vessel by contracting the stream, yet it must diminish the velocity at the orifice; that is, if the same perpendicular motion were to take place without the horizontal motion, the fluid would flow out faster; for as any motion in a fluid is immediately communicated in every direction, the horizontal motion will produce a motion upwards, and in some degree obstruct the descent of the fluid. If therefore this horizontal motion could be taken away, or any how diminished, the fluid would flow out with a greater velocity. Now if a pipe be fixed, the fluid at the bottom of the vessel flowing towards the orifice will, by its adherence to the vessel, continue to adhere to the sides of the pipe as soon as it arrives there, and by this means almost all the horizontal motion will be destroyed, and converted into a perpendicular motion, for the horizontal motion arises principally from the fluid which flows from and very near to the bottom, where the whole motion is very nearly in that direction. This motion therefore being thus nearly destroyed, the fluid will be less interrupted at the orifice, and consequently will flow out with a greater velocity. But why the velocity should also be increased either by increasing the length of the pipe, or making it an increasing cone, under certain limitations, is a circumstance which, I confess, I can give no satisfactory reason for.

The abovementioned experiments were made principally with a view to ascertain how far the theory of the motion of fluids can be applied; and the inquiry has led to several circumstances which, I believe, have not been observed before. That the theory is not applicable in all cases is manifest; but that it brings out conclusions in many instances which agree very well with experiment is undoubtedly true. This tends to show, either that the common principles of motion cannot be applied to fluids, and that the agreement is accidental; or that under certain circumstances and restrictions the application is just. Which of these is the case is not, perhaps, easy for the mind to satisfy itself about. Nothing however which is here said is done with any view to detract from the merit of these celebrated authors. They have manifested uncommon penetration, and carried their inquiries upon the subject to an extent, that nothing further can be hoped for or expected; and if they had done nothing else in science, this alone would have ranked them amongst the very first mathematicians. The fault has been *non artificis sed artis*.

Mr. MACLAURIN, in his Treatise on Fluxions, has given a most admirable illustration of the theory of Sir ISAAC NEWTON. It is there a very principal inquiry to determine the ratio of the force which generates the velocity of the descending surface of the fluid to the force of gravity. Now according to that theory, the pressure on the bottom of the vessel is wholly taken off at the instant of time at which the water begins to flow; and as this conclusion cannot be admitted, we may from hence learn, says the author, that this theory is not to be considered as perfectly exact. It appears therefore to be an important point to determine, what is the pressure of the fluid upon the bottom of a vessel compared with its whole weight at the time the fluid is running out. This may be determined to a great degree of accuracy by experiments constructed in the following manner.

Let A B C D (Tab. III. fig. 6.) be a pair of scales, and O the fulcrum; at the end of the arm C suspend a cylinder E, having an orifice *r s*, immediately under which place a weight *w*, so that the upper surface may be in the *vena contracta*, or at so small a distance below it that gravity can have produced no sensible effect upon the effluent fluid. Stop the orifice *r s*, and fill the cylinder with a fluid, and balance it by a weight W in the other scale. Then open the orifice, and the fluid will run out and strike *w*, and then be caught in the scale D. Now when the orifice is opened and the fluid flows out, the pressure upon the bottom of the cylinder is diminished, part of the fluid now not being supported, notwithstanding which the equilibrium is still continued; which shows that the action of the fluid against *w* is exactly equal to the loss of weight in the cylinder by the motion of the fluid through the orifice. In order therefore to find the diminution of the weight upon the bottom of the cylinder, we have only to find a weight equivalent to the momentum of the fluid against *w*.

Let A B (fig. 7.) be a lever flat on the upper side, suspended by an horizontal axis C D; L a scale hanging from it, which is to be balanced by a weight W; E is the cylinder suspended to something immoveable at M, having its orifice *r s* as far distant from A B as before it was from the weight in the scale; and let the orifice and scale be equidistant from C D. Stop the orifice, and fill the cylinder; and upon opening the orifice, let one person, by means of a cock at *v* upon a pipe which goes into a reservoir *x y z*, keep the fluid in the cylinder exactly at the same altitude, and another put such a weight *w* into the scale L as shall keep A B exactly in the same position; then the weight *w* is equivalent to the momentum of the fluid against A B, together with the momentum of the fluid entering the top of the cylinder through the pipe. To determine what weight is equivalent to this latter momentum, take away the cylinder E and weight *w*, and bring A B up to the pipe, and let the fluid act upon it, and find what weight (*v*) put into the scale will now keep A B horizontal, and this weight (*v*) will be equivalent to the momentum of the fluid flowing into the cylinder; hence $w - v$ is a weight equivalent to the momentum of the fluid issuing out of the cylinder at the *vena contracta*, and consequently equivalent to the diminution of the pressure upon the bottom after the opening of the orifice. In order to keep the fluid accurately at the same altitude, I should propose to have a floating gage *v* (fig. 8.) with a wire standing perpendicularly upon it, and entering a cylinder *w* attached to the side of the vessel, and of a bore just large enough to give it a free motion; then the cock must be opened and adjusted to give it such an aperture as will keep the top of the wire on a level with the top of the cylinder.

Or we may find the diminution of the pressure upon the bottom on opening the orifice in this manner. In fig. 6, take away the scale D and balance the cylinder when filled, and let the end C of the beam be made flat at the point from which the vessel is suspended. Then open the orifice of the vessel, having the same provision as before to keep it filled to the same altitude, and place such a weight at C as shall preserve the equilibrium during the time the fluid is in motion, and this weight is equivalent to w in the former case. This method is the most simple of the two; but the other includes a circumstance of some consequence, that is, that the momentum of the effluent fluid is exactly equivalent to the weight which the vessel loses. Having thus examined all the circumstances which I proposed respecting the emptying of vessels, I proceed next to the consideration of the doctrine of the resistance of bodies moving in fluids.

When a body moves in a fluid, each particle, in theory, is supposed to act upon it undisturbed by the rest, or the fluid is conceived to act as if each particle, after the stroke, were annihilated, in which case the following particles would exert their force uninterruptedly. This supposition is very far from being true in fact, and accordingly we find very little agreement between theory and experiment. To experiments therefore we must have recourse for any thing satisfactory upon this subject. I therefore constructed the machine which is here described, whereby both the absolute quantity of resistance in all cases may be very accurately determined, and the law of its variation under different degrees of velocity.

A B, C D (Tab. IV. fig. 9.) are two cross pieces of wood firmly connected together, with screws at each end, so that it may be fixed upon any plane; E G F is a frame fixed upon A B; $m n$ a small cylindrical well polished iron axis, having the lower end made conical, and an hollow conical piece to receive it, the upper end passing through G in a polished nut of iron just big enough to give it a free motion; on the top of this axis there are fixed four arms *a, b, c, d,* having each a plane *h, g, f, e,* which may be either of pasteboard or tin, and are thus fixed on. A wire has one end made very flat to which the plane is fixed, and the other end is left round and passes under two small staples made of wire, fixed into the arm so tight that you can but just turn it, so that if you fix the plane in any position it will remain there without any hazard of changing it. Two fine silk lines are wound together round the axis, one leaving the axis on one side and the other on the opposite side, and each, passing over a pulley, is connected to a scale; by this means the lines when drawn by weights put into the scales will give the axis a rotatory motion, and will act in opposite directions, and therefore if equal weights be put into the scales they will destroy each other's effects, so far as regard the position of the axis, so that neither the friction at the bottom nor at the nut at the top will be at all affected by whatever additional weights may be thus added. In respect to any additional friction at the pullies by the increase of weight, that may be diminished so as to become insensible, by increasing the radius of the pullies, and making the ends of their axes conical and letting them turn in a conical orifice, so that they may rest just at their points. If we allow the friction at the axis to be one-fifth of the weight added, which is certainly a great allowance for such an axis well polished, and the radius of the pulley be to the radius of that conical part of the axis where it rests as one hundred to one, then the effect of the friction would be only the five hundredth part of the whole weight; and even this might be diminished one hundred times more by using friction wheels; but this is a degree of accuracy which, I think, can never be required. We might also diminish the friction at the nut, if required, by letting the axis on those two sides towards which the lines act rest between two friction wheels. If the arms should be very long, it may be necessary to fix an upright piece upon K, and connect the extremity of the sails to the top thereof by a string or wire. When this machine is applied to find the resistance of water, the axis mn must be produced up above K, and the string applied to that part; the machine must be immersed in a large reservoir of water, leaving the part of the axis to which the string is applied above the surface. Before we proceed to the application, we must investigate a point called the centre of resistance.

Def. If a plane body revolve in a resisting medium about an axis by means of a weight acting therefrom, that point into which if the whole plane were collected it would suffer the same resistance, I call the *centre of resistance.*

Let a be the area of the plane, and $\dot{a}$ the fluxion of the area at any variable distance x from the centre of the axis, and d the distance of the centre of resistance from that of the axis. Now the effect of the resistance of $\dot{a}$ to oppose the weight is, from the property of the lever, as the resistance multiplied into its distance from the axis, or as $x\dot{a}$; but the resistance is supposed to vary as the square of the velocity (which is found by experiment to be true under certain limitations), or as the square (x^2) of its distance from the axis; hence the effect of the resistance of $\dot{a}$ to oppose the weight is as $x^3\dot{a}$; therefore the whole effect is as the fluent of $x^3\dot{a}$. For the same reason the effect of the resistance of the whole plane a at the distance d is as $d^3 a$; hence $d^3 a =$ flu. $x^3 \dot{a}$, consequently $d = \sqrt[3]{\frac{\text{flu. } x^3 \dot{a}}{a}}$.

If the plane be a parallelogram, two of whose sides are parallel to the arms, and m and n the least and greatest distances of the other two sides from the axis, then $d = \sqrt[3]{\frac{n^4 - m^4}{4n - 4m}} = \sqrt[3]{\frac{\overline{n^2 + m^2} \times \overline{n + m}}{4}}$.

Now to find the resistance of the planes striking the fluid perpendicularly, first set them parallel to the horizon, so that they may move edge-ways, or in their own plane, and let two equal weights be put, one into each scale, such as to give the arms an uniform velocity, and then these weights together (w) will be just equivalent to the friction of the axis and the resistance of the arms. Then place the planes perpendicular to the horizon by a plumb-line, and put in two more equal weights, one into each scale, making together W, so as to give the planes the same uniform velocity as before. Then, from what has been already observed, there is no additional friction, and therefore this weight W must be equivalent to the resistance of the planes. But this equivalent weight W acts only at the distance of the radius r of the axis from the centre of motion, whereas the resistance is to be considered as acting at the distance d of the centre of resistance from the centre of motion; hence $d : x :: \mathrm{W} : \frac{x}{d} \times \mathrm{W}$ the weight acting at the distance d, which is equivalent to the resistance acting at the same distance, and consequently it must be equal to the absolute resistance against all the planes. And to find the velocity, let C feet be the circumference described by the centre of resistance, and let the sails make one revolution in t seconds; then the velocity will be $\frac{c}{t}$ feet in a second.

To find the resistance when the fluid strikes the planes at any angle, set them to that angle, and find the resistance in the very same manner as before. But here we must set two of the opposite planes inclined one way and two the other, so that the fluid may strike the two former on their upper sides, and the two latter on their under sides, but both at the same angle. This caution is necessary in order to prevent any alteration in the pressure, and consequently in the friction upon the axis in the direction thereof; for the fluid striking the planes obliquely, part of the force will be employed in resisting the motion, and part will act perpendicular thereto, or in the direction of the axis, and this latter effect will manifestly be destroyed by the above disposition of the planes, because this force will act *upwards* against two of the planes, and *downwards* against the other two, and being equal, they will destroy each other's effects. The planes may be set to any angle thus: Take a small quadrant divided into degrees; let $m\,n$ (Tab. IV. fig. 10) be the outward inclined edge of the plane; suspend a plumb-line A B so as just to touch it at n, and at n apply the centre of the quadrant, and let the radius passing through 90° coincide with A B, and turn the plane till $n\,m$ coincides with that degree at which you would have the plane strike the fluid, and the plane stands right for that angle.

To find the resistance of a solid, we must have two such solids equal to each other, and put on at the opposite ends of two of the arms, for with one only its centrifugal force will increase the friction against the nut, whereas with two opposite to each other this effect will be destroyed. We must also get two thin pieces of lead with the edges feathered off, and of the same weight with the two solids. These must first be put upon the opposite arms, and a weight w found as before. Then the leads are to be taken off, and the solids put on in their place, with that side to go foremost whose resistance is required, and then find W as in the case of the planes; and the absolute resistance will be $\frac{x}{2d} \times W$ upon one of the solids.

By this machine we may find the absolute resistance upon the planes in a direction *perpendicular* to that of their motion. For let the lower end of the axis, instead of resting upon the base of the frame, stand upon one end of an horizontal lever, like that in figure the seventh, and let it be balanced by a weight in a scale hanging at the same distance on the other side of the fulcrum, when the sails have acquired an uniform motion, with the planes horizontal, or when moving edge-ways. Then turn the planes to any angle, and add equal weights to the scales R and T, until the planes have acquired the same uniform velocity as before, and put a weight P into the scale at the other end of the lever, which shall now just balance it, and P will be the absolute resistance of the fluid in a direction perpendicular to the motion of the planes.

The law of resistance, when the velocity varies, may be thus found. Let w, as before, be the sum of the two equal weights which will give the planes an uniform horizontal motion when they move edge-ways. Then set them perpendicular to the horizon, and let W be the sum of the two equal weights, put one into each scale, in order to give the sails the same uniform velocity. Take out these two equal weights, and put in two other equal weights, together equal to Q, such as shall give the planes an uniform velocity double to that before given; then the

Fig. 1.

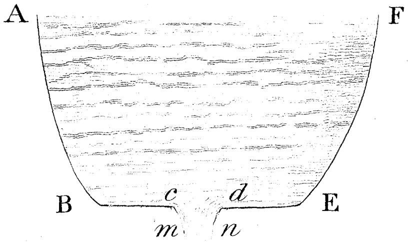


Fig. 2.

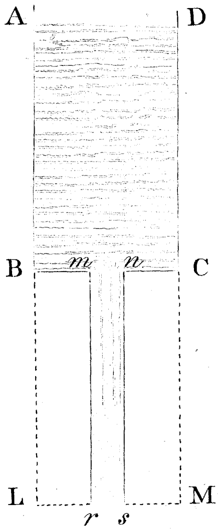


Fig. 3.

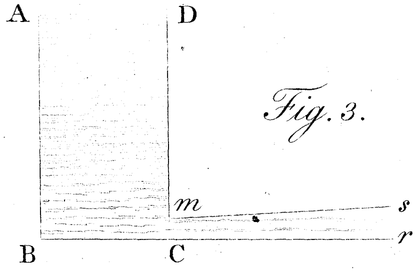


Fig. 4.

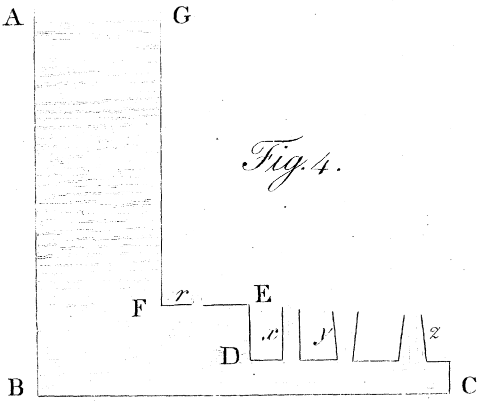
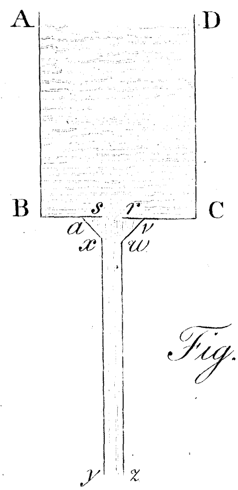


Fig. 5.



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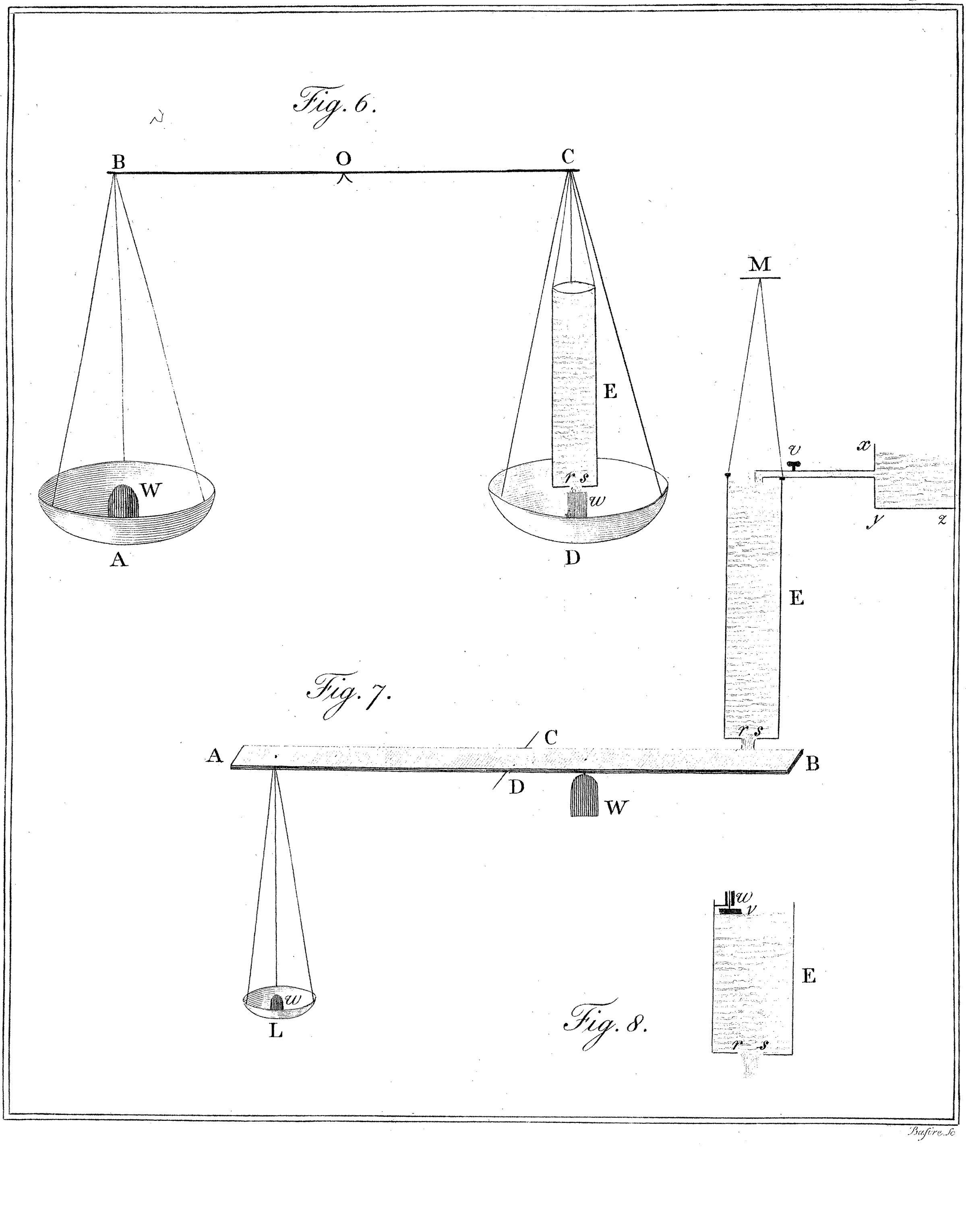

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Fig. 10.

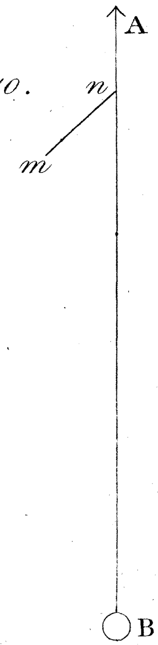
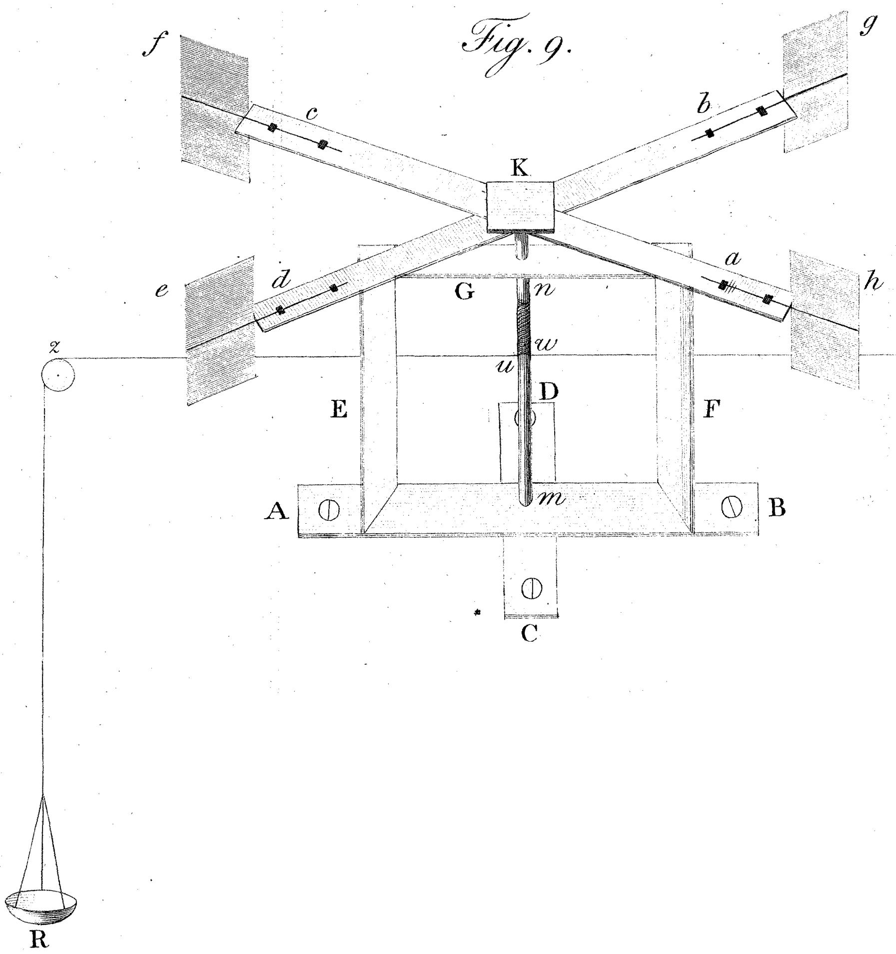


Fig. 9.



resistances with these two velocities of 1 : 2 will be as W : Q. If R be the sum of the two equal weights put into the scales to give an uniform velocity three times as great as that of the first; then with velocities as 1 : 3 the resistances will be as W : R; and so on. This method was proposed by Mr. ROBINS, in order to determine the law of resistance in terms of the velocity. If the planes be set at any angle, we can by this means get, in terms of the velocity, the law of resistance not only in the direction of the motion of the planes, but also in a direction perpendicular to that of their motion. An account of all the experiments which can be made by this machine, some of which I believe have never yet been attempted, I shall lay before the Royal Society at a future opportunity.