CHAPTER IV.

EARLY PAPERS FROM THE PHILOSOPHICAL TRANSACTIONS, AND SIR ROBERT BOYLE; ALSO THE PARIS ACADEMY OF SCIENCES, WITH NOTICES OF COMMUNICATIONS TO THE ACADEMY, 1837-56.

NOTHING later than 1733 appears to have been brought before the Royal Society on Perpetual Motion, and the following papers are chiefly remarkable as being derived from its Transactions:—

Observations of Dr. Papin, Fellow of the Royal Society, on a French Paper concerning a Perpetual Motion.

The paper printed in French, and containing a contrivance for perpetual motion, being set down in such a manner that can hardly be understood but by those that are much acquainted with such descriptions, I have endeavour'd to explain it as follows:—

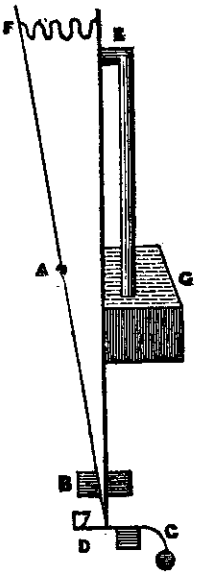
Let D E F be a pair of bellows forty inches long, that may be open'd by removing the part F from E; let them be exactly shut everywhere but at the aperture E; and let a pipe E G, twenty or twenty-two inches long, be soldered to the sayd aperture E, having its other end in a vessel G, full of mercury, and placed near the middle of the bellows.

A is an axis for the bellows to turn upon.

B, a counterpoise fastened to the lower end of the bellows.

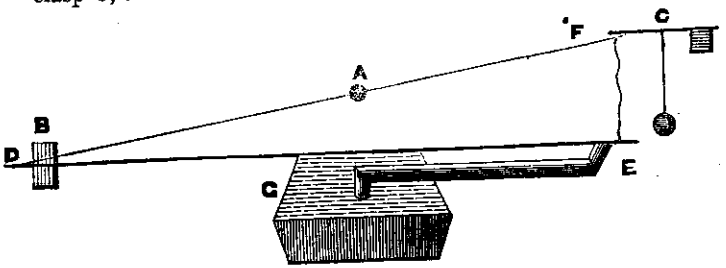
C, a weight with a clasp to keep the bellows upright.

Now, if we suppose the bellows open'd only to ½ or ¼, standing upright, and full of mercury, it is plain that the sayd mercury, being forty inches high, must fall, as in the Torricellian experiment, to the height of about twenty-seven inches, and, consequently, the bellows must open towards F, and leave a vacuity there. This vacuity must be



(*Fig.* 184, *pl.* 5.)

fill'd with the mercury ascending from G through the pipe G E, the sayd pipe being but twenty-two inches long; by this means the bellows must be opened more and more, till the mercury continuing to ascend makes the upper part of the bellows so heavy that the lower part must get loose from the clasp C, and the bellows should turn quite upside down; but



(*Fig.* 185, *pl.* 5.)

the vessel G being set in a convenient place, keeps them horizontal, and the part F engageth there in another clasp C; then the mercury, by its weight, runs out from the bellows into the vessel G through the pipe E G, and the bellows must shut closer and closer untill the part E F comes to be so light that the counterpoise B is able to make the part F get loose from the clasp C; then the bellows come to be upright again; the mercury left in them falls again to the height of twenty-seven inches, and, consequently, all the other effects will follow as we have already seen, and the motion will continue for ever. Thus much for the French author.

Upon this it is to be observed, that the bellows can never be opened by the internal pressure, unless the sayd pressure be stronger then the externall; now, in this case, the weight of the atmosphere doth freely press up the outward part of the bellows, but it cannot come at the inward part but through the pipe G E, which, containing twenty-two perpendicular inches of mercury, doth counterpoise so much of the weight of the atmosphere, so that this being supposed to be twenty-seven inches of mercury, it cannot press the inward part of the bellows but with a weight equivalent to five perpendicular inches of mercury. From this we may conclude,

that the pressure of the atmosphere, being weakened within the bellows more then it can be helpt by the mercury contain'd in the same, as may easily be computed, the sayd bellows standing upright must rather shut then open. Thus, without losing any labour and charges in trying, people may be sure that the thing can never do.*

Some further Remarks on the Instrument proposed by an Anonymous French Author for effecting a Perpetual Motion, an account whereof is given in No. 177 of these Transactions. By Dr. Papin, M.D., R.S.S.

Having seen in the "Journal des Savans" of May 13, and in the "Nouvelles de la Republique des Lettres" of the month of June, that the author of the perpetual motion is not satisfied, but doth endeavour to answer the objection that I propounded against his contrivance in the "Philosophical Transactions" of the month of December, 1685, I find I must explain myself more at large than I did in that paper; but I begg his pardon if I say nothing concerning the new disposition which he says might be given to his engine. My want of time makes me avoid new matters of dispute, and I think it enough for me, if I do but shew that his first description can never succeed.

I am very sorry that this author took so much trouble in trying his bellows with several liquors, as oyl, mercury, water. I thought I had sayd nothing that might make him believe that I did in the least question the truth, which he intended to prove against me by those experiments; and without any tryals I am fully enough convinced, that the mercury in his engine must follow the laws of the æquilibrium of fluid bodies. But the consequence which he draws from that principle seems to me very groundless; for, altho' the lowermost part of the bellows be presst by the weight of forty inches of mercury, it doth not follow that all the parts which are situated higher must bear the same pressure. To the quite contrary, it is plain that the upper part, having no mercury above it, bears none at all; the parts that lye in the middle near the axes of the bellows bear but twenty inches, and so all the

* The Philosophical Transactions. Vol. 15, for the year 1685. 4to. Page 1240. Also, the Phil. Trans. and Collections to the end of the year 1700, abridged, in 3 vols. By John Lowthorp, M.A. and F.R.S. Vol. I, 4to., 1705, p. 592.

rest must bear more or less according as they lye higher or lower. It is evident, therefore, that there are as many parts that bear less then twenty inches as there are that bear more, and the increase of pressure following an arithmetical progression, it is undenyable that all these pressures added together will do no more than one uniform pressure that would be equal to twenty inches everywhere.

Having thus found the quantity of pressure caused by the mercury within the bellows, we must remember that the pressure of the atmosphere within the same bellows is equivalent but to five inches, as I observed in my first paper, *vid.* "Philosophical Transaction," No. 177, page 1241: so that we find that the inward pressure is equivalent but to twenty-five inches of mercury in all. Now, the pressure of the atmosphere upon the outside is everywhere equal to twenty-seven inches; from whence it appears that the pressure without is stronger than the pressure within, and so I had reason to say, that the bellows standing upright, must rather shut than open.

I did not think to have given this computation so at large, but I have been necessitated to do it (as I said in the beginning), since my first paper was not sufficient to make me be understood by the author of the perpetual motion. However, I will be careful to save the time of the reader as much as I can; and although I might observe some other things in his description that will increase the difficulty of opening the bellows, I forbear to speak of them, and I will stick only to that which is most material, and make his perpetual motion to be altogether impossible.

As for the argument the author drawes from comparing his engine to an ordinary siphon, I do beseech him to consider what a difference there is between a siphon that lets the water run down at the bottom, and his engine, that should gather up the heavy liquor into the highest part of the instrument, and I do not question but he will acknowledge the weakness of this argument.*

An answer of Dr. Papin to the Author of the Perpetual Motion.

In the last papers I published in "Philosophical Transaction" No. 184, against this perpetual motion described in

* The Philosophical Transactions. Vol. 16, for the years 1686 and 1687. No 182, June, 1686, p. 138.

No. 177, I intreated the author to permit me to say nothing as to what alterations he might make in his engine, resolving to leave it to others to shew him that upon that principle all he can do signifies nothing. But I find since, in the "Nouvelles de la Republique" for December last, that he still persists to urge some new contrivances, which being added, he conceives his engine must succeed. To this I answer, that I undertook only to shew that his first device would faile, which yet I should scarce have done if I had thought a dispute of this nature could have lasted so long. To come, therefore, to the point where he saith that this engine may well succeed without alteration, because he hath tryed with liquors put into bellows immersed in water; I again say that I grant him the truth of the experiments, but deny the consequences he would draw from them. I have already given the reasons of my dissent, which this gentleman is not pleased to understand. But to end all controversies, he may please to consult Mr. Perrault, De la Hire, or any other at Paris well known to be skilled in hydraulicks, and I doubt not but he will find them of the same opinion with Mr. Boyle, Mr. Hook, and other knowing persons here, who all agree that our author is in this matter under a mistake.*

A Proposition on the Balance, not taken notice of by Mechanical Writers, explained and confirmed by an Experiment. By the Rev. J. T. Desaguliers, LL.D., F.R.S.

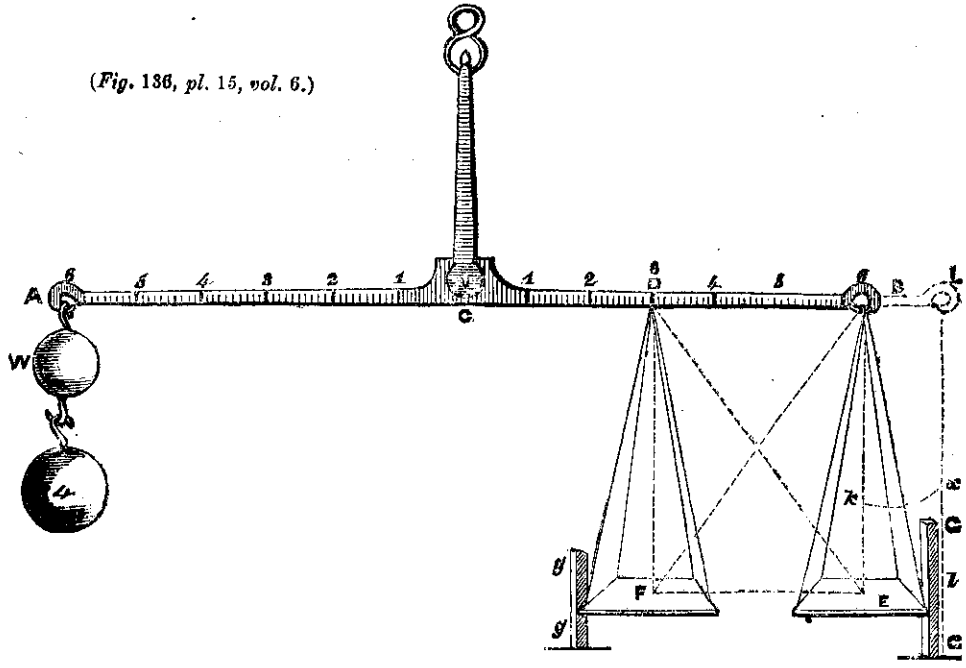
THEOR. 6.—A B is a balance, on which is supposed to hang at one end, B, the scale E, with a man in it, who is counterpoised by the weight W hanging at A, the other end of the balance. I say, that if such a man, with a cane or any rigid straight body, pushes upwards against the beam anywhere between the points C and B (provided he does not push directly against B), he will thereby make himself heavier, or overpoise the weight W, though the stop G G hinders the scale E from being thrust outwards from C towards G G. I say likewise, that if the scale and man should hang from D, the man, by pushing upwards against B, or anywhere between B and D (provided he does not push directly against D), will make himself lighter, or be

* The Philosophical Transactions. Vol. 16, 1686-7. No. 186, January, 1687, p. 267.

overpoised by the weight W, which before did only counterpoise the weight of his body and the scale.

If the common center of gravity of the scale E, and the man supposed to stand in it, be at *k*, and the man, by thrusting against any part of the beam, cause the scale to move outwards so as to carry the said common center of gravity to *k x*, then, instead of B E, L *l* will become the line of direction of the compound weight, whose action will be increased in the ratio of L C to B C. This is what has been explained by several writers of mechanics; but no one, that I know of, has considered the case when the scale is kept from flying out, as here by the post G G, which keeps it in its place, as if the strings of the scale were become inflexible. Now, to explain this case, let us suppose the length B D of half of the brachium B C to be equal to 3 feet, the line B E to 4 feet, the line E D of 5 feet to be the direction in which the man pushes, D F and F E to be respectively equal and parallel to B E and B D, and the whole or absolute force with which the man pushes equal to (or able to rise) 10 stone. Let the oblique force E D (= 10 stone) be resolved into the two E F and E B (or its equal F D) whose directions are at right angles to each other, and whose respective quantities (or intensities) are as 6 and 8, because E F and B E are in that proportion to each other and to E D. Now, since E F is parallel to B D C A, the beam, it does no way affect the beam to move it upwards; and therefore there is only the force represented by F D, or 8 stone, to push the beam upwards at D. For the same reason, and because action and reaction are equal, the scale will be pushed down at E with the force of 8 stone also. Now, since the force at E pulls the beam perpendicularly downwards from the point B, distant from C the whole length of the brachium B C, its action downwards will not be diminished, but may be expressed by $\overline{8 \times B\,C}$; whereas the action upwards against D will be half lost, by reason of the diminished distance from the center, and is only to be expressed by $8 \times \frac{\overline{B\,C}}{2}$; and when the action upwards to raise the beam is substracted from the action downwards to depress it, there will still remain 4 stone to push down the scale; because $8 \times B\,C - 8 \times \frac{\overline{B\,C}}{2} = 4\,B\,C$. Consequently, a weight of 4 stone must be added at the end

(*Fig.* 136, *pl.* 15, *vol.* 6.)



A to restore the æquilibrium. Therefore a man, &c., pushing upwards under the beam between B and D, becomes heavier. Q.E.D.

On the contrary, if the scale should hang at F, from the point D, only 3 feet from the center of motion C, and a post G G hinders the scale from being pushed inwards towards C, then, if a man in this scale F pushes obliquely against B with the oblique force above mentioned, the whole force, for the reasons before given (in resolving the oblique force into two others acting in lines perpendicular to each other) will be reduced to 8 stone, which pushes the beam directly upwards at B, while the same force of 8 stone draws it directly down at D towards F. But as C D is only equal to half of C B, the force at D, compared with that at B, loses half its action, and therefore can only take off the force of 4 stone from the push upwards at B; and consequently the weight W at A will preponderate, unless an additional weight of 4 stone be hanged at B. Therefore, a man, &c., pushing upwards under the beam between B and D, becomes lighter.*

[The paper concludes with some further rules and experiments.]

An Experiment explaining a Mechanical Paradox, that two bodies of equal weight suspended on a certain sort of balance do not lose their æquilibrium by being removed, one farther from, the other nearer to, the center. By the Rev. J. T. Desaguliers, LL.D., F.R.S.

Prop. 7.—If the two weights P W hang at the ends of the balance A B, whose center of motion is C, those weights will act against each other (because their directions are contrary) with forces made up of the quantity of matter in each multiplied by its velocity; that is, by the velocity which the motion of the balance turning about C will give to the body suspended. Now, the velocity of a heavy body is its perpendicular ascent or descent, as will appear by moving the balance into the position *a b*, which shews the velocity of P to be the perpendicular line *e a*, and the velocity of B will be the perpendicular line *b g*; for if the weights P and W are equal, and also the lines *e a* and *b g*, their momenta, made up of *e a* multiplied into

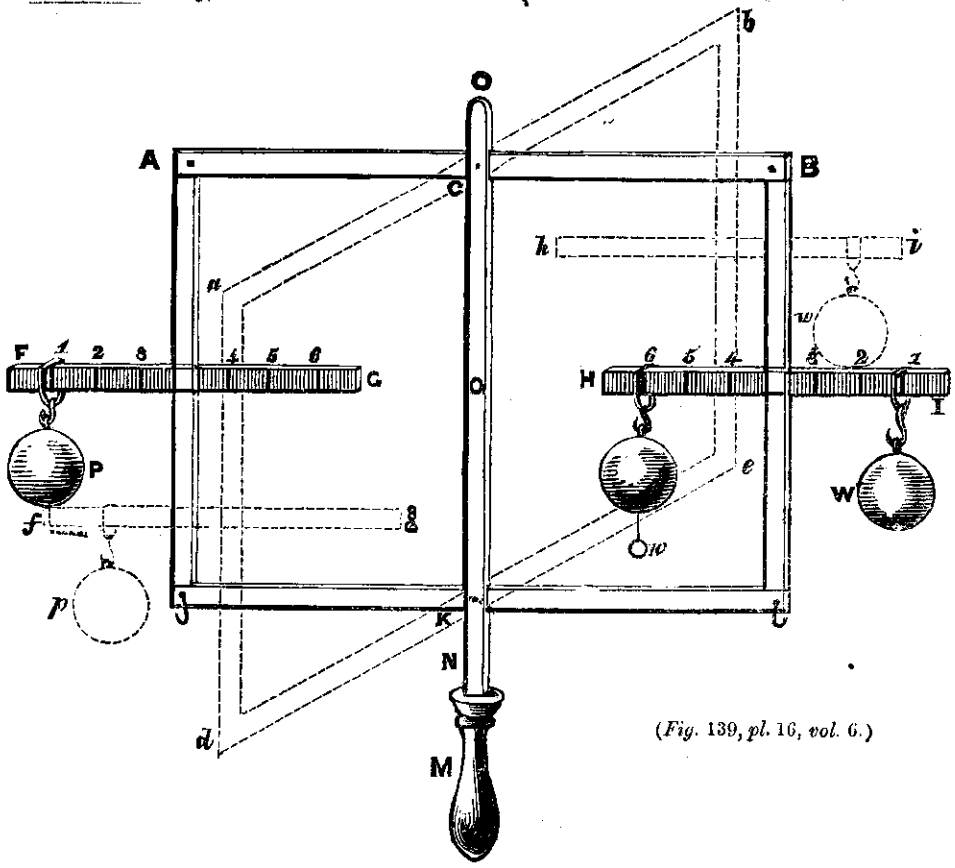
* The Philosophical Transactions, from 1719 to 1733, abridged. 4to. London, 1734. Vol. 6, p. 306.

W, and *b g* multiplied into P, will be equal, as will appear by their destroying one another in making an equilibrium. But if the body W was removed to M, and suspended at the point D, then, its velocity being only *f d*, it would be overbalanced by the body P, because *f d* multiplied into M would produce a less momentum than P multiplied into *b g*.

As the arcs A *a*, B *b*, and D *d*, described by the ends of the balance or points of suspension, are proportionable to their sines *e a*, *g b*, and *d f*, as also the radii or distances C A, C B, and C D; in the case of this common sort of balance, the arcs described by the weights, or their points of suspension, or the distances from the center, may be taken for velocities of the weights hanging at A, B, or D, and therefore the acting force of the weights will be reciprocally as their distances from the center.

SCHOLIUM.—The distances from the center are taken here for the velocities of the bodies, only because they are proportionable to the lines *e a*, *b g*, and *f d*, which are the true velocities; for there are a great many cases wherein the velocities are neither proportionable to the distances from the center of motion of a machine, nor to the arcs described by the weights or their points of suspension. Therefore, it is not a general rule that weights act in proportion to their distances from the center of motion; but a corollary of the general rule that weights act in proportion to their velocities, which is only true in some cases. Therefore, we must not take this case as a principle, which most workmen do, and all those people who make attempts to find the perpetual motion, as I have more amply shewn in the Phil. Trans., No. 369.

But to make this evident even in the balance, we need only take notice of the following experiment:—A C B E K D is a balance in the form of a parallelogram passing through a slit in the upright piece N O standing on the pedestal M, so as to be moveable upon the center pins C and K. To the upright pieces A D and B E of this balance are fixed at right angles the horizontal pieces F G and H I. That the equal weights P W must keep each other in æquilibrio, is evident; but it does not at first appear so plainly, that if W be removed to V, being suspended at 6, yet it shall still keep P in æquilibrio, though the experiment shews it. Nay, if W be successively moved to any of the points 1, 2, 3, E, 4, 5, or 6, the æquilibrium will be continued; or if, W hanging at any



(*Fig.* 139, *pl.* 16, *vol.* 6.)

of those points, P be successively moved to D, or any of the points of suspension on the cross piece F G, P will at any of those places make an æquilibrium with W. Now, when the weights are at P and V, if the least weight that is capable to overcome the friction at the points of suspension C and K be added to V, as *u*, the weight V will overpower, and that as much at V as if it was at W.

From what we have said above, the reason of this experiment will be very plain.

As the lines A C and K D, C B and K E, always continue of the same length in any position of the machine, the pieces A D and B E will always continue parallel to one another, and perpendicular to the horizon. However, the whole machine turns upon the points C and K, as appears by bringing the balance to any other position, as *a b e d;* and therefore, as the weights applied to any part of the pieces F G and H I can only bring down the pieces A D and B E perpendicularly, in the same manner as if they were applied to the hooks D and E, or to X and Y, the centers of gravity of A D and B E, the force of the weights (if their quantity of matter is equal) will be equal, because their velocities will be their perpendicular ascent or descent, which will always be as the equal lines 4 *l* and 4 L, whatever part of the pieces F G and H I the weights are applied to. But if to the weight at V be added the little weight *u*, those two weights will overpower, because in this case the momentum is made up of the sum of V and *u* multiplied by the common velocity 4 L.

Hence follows, that it is not the distance C 6 multiplied into the weight V which makes its momentum, but its perpendicular velocity L 4 multiplied into its mass. Q.E.D.

This is still further evident by taking out the pin at K; for then the weight P will overbalance the other weight at V, because then their perpendicular ascent and descent will not be equal.*

* The Philosophical Transactions, from 1719 to 1733, abridged. 4to. London, 1734. Vol. 6, p. 310. Another paper by Dr Desaguliers, being "Remarks on some attempts made towards a Perpetual Motion," from the Phil. Trans, abridged, 4to., 1734, vol. 6, p. 323, will be found transferred to Chapter II.

An Historical Account of a strangely Self-moving Liquor. Printed first in the Philosophical Transactions, No. 176, *p.* 1188, *anno* 1685, *vol.* 15.

An ingenious teacher of mathematicks, having occasion to make a composition for a new fire-engine, whereof he was to shew his Majesty a trial, mingled divers ingredients in an earthen pot over kindled coals; but could not or did not do it so warily, but that the matter took fire, and began to blaze furiously, which obliged him to stifle the blaze as hastily as he could; and having removed the vessel from the fire, and suffered it to grow cold, when afterwards he came to look upon it, to see if what remained might be of any use to him, he was surprised to find it variously and briskly moved. Wherefore, having set it aside, to be sure that it might be thoroughly cold, he, after some hours, visited it again, and found it move as before; and having cast store of seeds upon it, to see if the liquor would move them also, the bituminous part of it connected them into a kind of thick scum, that covered most of the superfices, but yet left some intervals, in which the liquor appeared, and discovered that it continued its motions. Two days after, the engineer discoursing with me of this fire-work, about which he had advised with me before, told me, among other things, of this odd accident. And when I asked him if the motion continued still, and had been answered affirmatively, though it was then a dark night and ill weather, my diffidence or my curiosity made me engage him to send for the pot as it was, partly to be sure of the matter of fact, and partly to try if the knowledge I had of the ingredients, which he had before told me, would afford any hint of the cause of so odd an effect; a like to which in kind, though not in degree, I had many years before devised, and successfully practised, the way of producing.

The vessel being come, though the hasty transportation of it seemed to have sufficiently disturbed it, there did appear manifest signs of such a motion as the engineer had ascribed to it; and, therefore, he being willing to leave it with me, I caused it to be set aside in a laboratory, where some furnaces kept the air constantly warm, and did there and elsewhere, at different times, look heedfully upon it, now and then displacing or quite taking off some of the thick scum that too much covered the surface of it, and by this means I had the opportunity to take notice of several phænomena, whereof these are the chief:—

1. I observed that the motion of this liquor was not very brisk, but very various, so that having loosened some small portions of the scum, one of them would be carried towards the right hand for instance, and another towards the left, at the same time. 2. Where the liquor first came out from under the scum, it seemed to move the most briskly, flowing almost like a stream whose motion upwards had been checked, and, as it were, reverberated by that incumbent obstacle. 3. Several motions in this liquor were the more easy to be observed, because, though it were dark, yet it was not uniform; consisting in part of oily and bituminous ingredients, which, though they seemed to have but one common superficies with the rest of the liquor, yet, but their colours and power of vigorously reflecting the light, they were easily enough distinguishable from the rest. And I often observed that some of these unctuous portions of the matter, emerging to the surface of the liquor, though perhaps at first one of them would not appear bigger than a pin's head, yet, in moving forwards, it would at the same time diffuse itself circularly, and make, as it were, a great halo, adorned with the colours of the rainbow, and so very vivid as afforded a very pleasant, and at first surprising, spectacle; these phantasms often nimbly succeeding one another, and lasting till they lost themselves against or under the thick scum. 4. The motions of this odd liquor were not only various, but frequently vortical; to be satisfied of which, I sometimes put short bits of straw, or fragments of some such like stuff, upon the discovered part of the surface of the liquor, by which they were carried towards very distant, if not opposite, parts of the vessel at the same time. But to make the vortical motion more evident, I several times detached considerably large pieces of the thick scum from the rest of the body, and had the pleasure to see them move both with a progressive motion in crooked lines, and with a motion about their own middlemost parts. All this while, the liquor, whose parts were thus briskly moved, was actually cold, as to sense. 5. To observe what the presence, or absence, of the free air would do to this liquor, I caused many spoonfuls of it, with some of the scum, to be put into a cylindrical glass, which, though large itself, had a neck belonging to it, that was but about the bigness of one's thumb, that it might be well stopped with a cork. But having, by this means, kept the

free air from having a full and immediate contact with the whole surface of the mixture, as it had when that mixture lay in the wide-mouthed vessel, I could not perceive the liquor to move to and fro, no, not though the orifice of the neck were left open: whereas, having, at the same time, poured some of the liquor into a very shallow and wide-mouthed vessel, called in the shops a clear-caked glass, it moved rather more than less nimbly and variously than in the great earthen pot (which yet was of the same shape), and shewed us many of those vivid and self-dilating circles that have been mentioned in the third number; and these, by the fineness of their colours and the quickness wherewith they succeeded one another, afforded a delightful spectacle as long as I stayed to observe the liquor. 6. Though the motions of the hitherto mentioned liquor did not seem to be always equally brisk, yet they appeared to continue manifest and various in some diversities of weather, as to cold and heat, and when I looked on it by candle-light as well as by daylight; and when, being not well enough to visit it myself, I sent one purposely to look upon it, about ten o'clock at night, he brought me word that it continued to move as formerly, and so it has done for ten days; and how much longer it will continue to do so, time must determine.

POSTSCRIPT.—Some time after the foregoing account had been written, when I came to look upon the liquor (which in the mean time had been several times viewed, and appeared to retain its motions), I found, to my trouble, that somebody's impertinent curiosity and heedlessness had cracked the lower part of the earthen pot; at which overture, the liquor, though not the scum, was run out; which had put a period to our observations, but that, foreseeing that such an accident might happen, I had long before taken out some spoonfuls of the liquor, and kept it close stopped in a vial. By this means I had the opportunity to observe that, when I poured out the liquor into a wide-mouthed vessel, it would move as before, though this were done some weeks after it had been put up. And I remember that long after, having one day received the honour of a visit from a foreign minister, who was an inquisitive person and a man of letters, we chanced, among other things, to talk of the liquor; and though it were scarce to be hoped that it could still retain any of its motive virtue, yet, to gratify his curiosity, and that of some ingenious men

were present, I caused the vial to be brought, and having unstopped it, I poured out the liquor into a convenient-shaped vessel, in which, after we had suffered it to rest awhile, they were delightfully surprised to see it move (though not, in my opinion, quite so briskly as before), yet very manifestly and variously. This encouraged me to think it possible that it might retain some motion, though but languid, seven or eight weeks after; and therefore, on the 25th of July, I looked upon it again, and having caused it to be poured into a china cup, it manifested, at first, a brisk and various motion; but this, after awhile, did so slacken that I began to have some suspicion that the motion it was put into by effusion and the first contact of the air might have given it the greatest part of its agitation. But this being but suspicion, I put the vessel into divers postures in a window, the better to discover the true cause of this phænomenon; but whilst I was busy about this, which ingrossed my attention, a mischance overturned the cup, and, by throwing down the liquor, put an end to my speculation: yet this mischance hindered me but from observing how long the odd agitation of our liquor would have continued, but not from finding that it lasted a great while; for I shewed it the foreign minister about or after the beginning of June, that is, about five months or more after the liquor was first observed to move.*

The following is extracted from a letter of S. Hartlib to Mr. Boyle:—

I had lately received a letter from Mr. Oldenburg, amongst other particulars, in these words:—"In our passing through Mentz we met with a rare artist called Beckor, a young man, who hath found, he saith, the perpetual motion, the possibility whereof hath been hitherto so much disputed by philosophers. He hath almost finished a work, wherein he doth demonstrate his invention, which we have seen, and the design and way whereof the master told me himself, he would within a very few weeks put in print here at Franefort; which being done, I shall send you a copy of it.—July 18, 1658."†

* The Works of the Hon. Robt. Boyle, in 6 vols. 4to., 1772. Vol. 5, page 71.

† *Ibid.* Vol. 6, page 113.

The following eminent authority is often adduced in Encyclopædias; but his paper is little, if at all, known, entitled—"A Demonstration of the Impossibility of Perpetual Motion. By M. de la Hire. Written on the occasion of several schemes having recently appeared:"—

There is not any of those who pretend to have found out perpetual motion, who do not agree that two weights placed in a position to move, following their natural direction in equal time, or in any way reciprocal to their weight, remain in equilibrium. Yet there is no perpetual motion scheme, where one cannot draw a conclusion quite opposed to this principle; for, whatever may be pretended, perpetual motion is nothing more or less than the elevation of one weight to a certain height by the descent of another weight at the same time; and reciprocally the restitution of the first to the place where it was before its movement, by the descent of the one that had been raised, and so on *ad infinitum;* sometimes by means of weights, which, being raised, in their fall agitate other weights; sometimes by means of liquid bodies, which, being raised, can run, and move other parts far separated from the centre of motion; from which no advantage can be derived, and which is entirely contrary to the preceding principle.

Those who occupy themselves with this chimera, find nothing but embarrassment, for generally their machines have so many weights, &c., to move them, that their inventors forget always to be on their guard against the many hinderances that arise,—the height, &c., of the powers employed, their natural direction, &c., all these are sometimes so strangely jumbled together that it requires very hard work to be able rightly to distinguish them. This is one great reason that leads such persons to a false demonstration of perpetual motion; and when they propose their beautiful inventions to those who are versed in science, and who cannot immediately make them see or understand in what way their reasoning is false, they then publish to the world that the very cleverest men have been convinced of the truth of their perpetual motion.*

* Mémoires de l'Académie Royale des Sciences. Paris, 1730, 4to. Vol. 10, page 605. [See Appendix D.]

The Paris Academy of Sciences received numerous communications on Perpetual Motion, but in their "Comptes rendus Hébdomadaries des Séances," in conformity with a regulation of long standing, they only name the authors and the matter of their papers when upon this subject, as—

Vol. 4, 1837.—A Letter from M. PASCAL, on perpetual motion.

Vol. 6, 1838.—M. Jennisson addressed a presumed solution of the problem of perpetual motion, offered by M. FREYBERG.

Vol. 13, 1841.—M. FURIET read a paper on perpetual motion, its object being to prove, by the most popular manner, its impossibility.

Vol. 14, 1841.—A communication received from M. VERGER on perpetual motion.

Vol. 29, 1849.—M. le Ministre de l'Instruction Publique transmits a note and paper addressed to him by M. le Prefet des Hautes-Pyrénées, in which the author, M. DULOT, describes a piece of mechanism by means of which he believes he obtains perpetual motion.

Vol. 38, 1854.—M. JAUFFRET announces himself as a candidate for the prize given for the improvement of navigation; he offers a propeller which he believes realizes perpetual motion.

M. THEOD. SYLVESTRE considers he has discovered perpetual motion.

M. BADANNEL begs the grant of a commission to examine a piece of mechanism he has invented, to give continued rotation and keep itself going.

The Academy, according to a regulation of ancient standing, cannot grant a commission to examine into papers relative to perpetual motion.

M. EUG. ROMEY announces his invention for showing the movements of the earth, acting also as a clock.

Vol. 39, 1854.—M. PARCEINT presents a paper and drawing of a propeller having a universal and continuous movement.

The Academy, as before, decline investigating this matter.

Vol. 40, 1855.—M. AIME LECOQ addresses a paper on perpetual motion to the Ministre du Commerce, &c., who

sends it to the Academy, and they refuse to grant a commission for the examination of communications relative to this subject, which they consider unattainable.

M. HUHN, of Prussia, announces having invented a system of aërial navigation, composed of a series of fanners, shaped like a bird's body, and the whole attached like a railway train. They are propelled by perpetual motion machinery.

As before stated, the Academy refuse accepting this and like offers.

Another note is addressed by M. RIEFFER on perpetual motion.

Vol. 41, 1855.—M. CASTAGNE addresses a letter relative to the squaring of the circle, and also on perpetual motion.

The Academy, as before, decline his offers, and state they consider all commucications on these two subjects null and void.

M. MUYTON writes a letter relative to perpetual motion.

The Academy, as before stated, decline to take the subject into consideration.

Vol. 42, 1856.—M. BLANCHET announces his intention of submitting to the judgment of the Academy a paper in which he considers he has resolved the question of perpetual motion.

The Academy, as in like cases, refuse any examination by a commission.

Vol. 43, 1856.—The Academy have received two notes on perpetual motion—one by M. PETREMENT, and the other by M. GRUSSET—which they refuse, on their former-named regulations.

Vol. 47, 1858.—A paper presented by M. le Ministre de l'Instruction Publique, enclosing two notes—one descriptive of an astronomical apparatus, and the other a piece of mechanism—showing perpetual motion. Both by M. VITELLI, a Neapolitan.

The Academy receive the first of these notes, but the second they reject.

M. MARCHAND sends a note on perpetual motion, which is refused, on the ground of their before-named decision.*

* Comptes Rendus Hébdomadaries des Séances, de l'Académie des Sciences. Paris, 4to.

...ment of the Memoirs of the Royal Academy ... Paris, for 1700, an account is given of—

A false report of the Perpetual **Motion being discovered, and the impossibility of it demonstrated.**

There was in this year a report spread that the perpetual motion was found. It was seen in a place where the difficulty of the thing was not well known, where the invention was not examined as it would have been in an academy, where an air of easiness succeeds sometimes, and the air of confidence almost always. M. Sauveur explained the invention to the academy, who were very much surprised at it. A little while after the noise that this discovery made, the perpetual motion disappeared with its author. On this occasion, M. Parent proved the impossibility of it by this single reason, that all the parts of a machine have a common centre of gravity; that while they turn round an axis or fixed point, whichever it be, this common centre of gravity finds itself necessarily in one situation, where it is lower than in any other, and that presently all must stop. For, since there is a point where the force, which many bodies have to descend, is entirely re-united, as soon as this point cannot descend any more, all these bodies must remain fixed. M. Parent determined in general that there must inevitably be this point of rest for all the machines possible.*

* The Philosophical History and Memoirs of the Royal Academy of Paris; or, an Abridgment of all the Papers relating to Natural Philosophy. Translated and abridged by J. Martyn, F.R.S., and E. Chambers, F.R.S. 5 vols, 8vo. 1742. Vol. 1, p. 203.