

IX. *Experiments upon the Resistance of the Air.* By Richard Lovell Edgworth, Esq. F. R. S. In a Letter to Sir Joseph Banks, Bart. P. R. S.

Read January 16, 1782.

DEAR SIR,

Edgworth's Town, Ireland,  
May 2, 1782.

THE last time I had the pleasure of seeing you, the conversation turned upon the resistance of the air, and upon a singular experiment related by Mr. ROBINS in his Treatise upon Gunnery. I have since repeated his experiment, and tried some others, which I beg you to lay before the Royal Society, if you think them worthy their attention.

I have the honour to be, &c.

MANY experiments have been tried to ascertain the force and velocity of the wind, with a view to the construction and management of different engines, and more particularly for the purposes of navigation. Several machines, which have been employed in these enquiries, are described in the Transactions of the Royal Society, and in the Memoirs of Foreign Academies; but the most accurate which I have seen was invented by the late Sir CHARLES KNOWLES; and from a number of experiments made with it, he had constructed tables, shewing at one view the force of the wind upon each sail of a ship at every degree of velocity from one to ninety miles an hour.

But

But these calculations, and many more of a similar nature, that are to be met with in BELIDOR'S *Architecture Hydraulique*, and other books, are founded upon a supposition that the effect of the wind is directly as the surface upon which it acts. If, for instance, its force be estimated as one upon one square yard, its force upon two square yards should be estimated as two, upon three square yards as three, &c.; but in fact this proportion is not to be depended upon, nor must the resistance of surfaces be estimated merely by their extent; but several other circumstances must be taken into consideration.

No figures can resemble each other more than a parallelogram and a square having the same superficial contents, as they are both bounded by four straight lines meeting at right angles, yet they oppose different degrees of resistance to the air.

If two similar cards, for instance, are placed opposite the wind, one upon its end, and the other on its side, and both inclined to the same angle, the wind will have the greater effect upon the card that is placed end-ways.

To determine the difference of resistance between these two surfaces, and to ascertain the effect of other figures moving through the air, I tried the following experiments. The two first are to be found in Mr. ROBINS'S *Treatise upon Gunnery*; but I thought it proper to repeat them, that they might be more readily compared with others made with the same apparatus, especially as Mr. ROBINS made use of a machine constructed upon a smaller scale than mine, and turning upon friction wheels, which are not proper for machines of this nature, nor indeed for any purpose, where an uniform motion is required.

Having fastened a strong joist of wood from one side of a large room to the other, so as to form a kind of bridge at some distance from the floor, I erected a perpendicular shaft or roller, which turned freely in brass sockets fixed into the floor and bridge upon pivots of hardened steel one-sixteenth of an inch in diameter. On each side of this roller was extended an arm of deal, feather-edged, and supported by stays of the same material, feathered in the same manner, to oppose as little surface as possible to the air when in motion.

Round the upper part of this roller was wound a string of cat-gut, which, passing over pulleys properly disposed, was fastened to a scale that descended into the well of an adjoining stair-case.

The extremity of these arms described a space of more than forty feet in every revolution, the weight descending in the same time only six inches. The time in all the following experiments was the same; and, as each revolution was performed in four seconds, the velocity of the end of the arm on which the surface was fixed, was at the rate of about seven miles an hour.

The first figure that I tried was a parallelogram of tin, nine inches long, and four inches wide. Its longest side was placed parallel to the floor, at the extremity of one of the arms. Its shortest sides were inclined to an angle of forty-five degrees from the perpendicular, and in this situation it was carried round with its surface against the air.

After suffering it to revolve until I was satisfied that its motion was become uniform, I put as much weight into the scale as moved it with a velocity of five turns in twenty seconds. I then changed the situation of the parallelogram, placing its shortest sides parallel to the floor, and inclined to the same angle

angle as before. I now found, that more weight was required to produce the same velocity, though the quantity of surface was the same as in the preceding experiment. The weight necessary to put the machine alone in motion, with the velocity above mentioned, was two pounds and a half. When it carried the parallelogram with one of its shortest sides downwards, it required four pounds and a half additional weight; and when the parallelogram was reversed, another half pound was barely sufficient to give it the same velocity.

The difference, therefore, occasioned by placing the same parallelogram with its longer or shorter sides inclined from the direction of its motion was equal to one-tenth of the greatest resistance.

It has been observed, that in these two experiments the mean velocity of the plane was not the same, as its extremity extended farther from the center of the machine in one than in the other. This is strictly true; but the size of the parallelogram bore so small a proportion to the length of the radius to which it was fastened, that the error arising from this circumstance is scarcely perceptible, and the advantage being in favour of that which required the least weight, I did not think it necessary to bring it into account.

Having formed a general idea of the reason of the difference in these experiments, it occurred to me, that there would be a greater disproportion between the resistance of some other figures, which Mr. ROBINS had not tried; and having put a rhomboid, in the form of a lozenge, nine inches long, and four broad, in the place of the parallelogram, the difference was increased from one-tenth to one-seventh of the weight employed to give them the required velocity.

Pursuing the same reasoning that led me to the last experiment, it occurred to me, that even against figures of exactly the same shape, the resistance of the air, when the dimensions of the figures were enlarged, would not be increased in the same proportion as the size of the planes, but in a much higher ratio; and that, by bending the planes as a sail, the resistance would be still farther increased, though the section of air, that would be intercepted by the planes, must by these means be considerably lessened.

The result far surpassed my expectations. A square of tin, containing sixteen square inches, placed perpendicularly, was resisted as two and a half. A square, containing sixty-four inches, or four times the former quantity, instead of meeting with a resistance as ten or four times the former resistance, required no less than fourteen pounds to give it the same velocity.

Four-tenths (or nearly half as much again) was an increase of resistance that made me suspect some error in the experiment; but having repeated it several times with great care, and having examined all the parts of the machine, I was satisfied that I had made no mistake.

I now placed the parallelogram of nine inches long upon the arms of the machine, with its shortest sides parallel to the horizon, bending it to such an arch that its chord measured eight inches, and inclining it to an angle of forty-five degrees. And though the section of air that it intercepted was by these means diminished one-ninth, yet the resistance was increased from five to five and a half. And when the parallelogram was bent yet farther, and its chord contracted almost to seven inches, the resistance was increased to five and three-quarters.

I mention these numbers in gross to avoid confusion; but in  
the

the table at the end of this paper, the measures and weights are set down exactly.

Dr. HOOK, whose name must be respected by every experimental philosopher, was aware, that although he thought he could demonstrate that flat sails were preferable to such as were curved and hollowed by the wind, yet until proper experiments had been tried, nothing could be positively determined.

He says somewhere in his posthumous works, “ That he  
“ was surpris'd at the obstinacy of seamen, in continuing,  
“ after what appeared the clearest demonstration to the con-  
“ trary, to prefer bellying or bunting sails to such as were  
“ hauled taught; but that he would, at some future time,  
“ add the test of experiment to mathematical investigation.”  
He reasoned upon a supposition, that the air in motion followed the same laws as light; and that it was reflected from surfaces with the angle of reflection equal to the angle of incidence, which is not the case, as it never makes an angle with the plane, but is always reflected in curves. MONS. PARENT, and other mathematicians, have fallen into the same mistake. No demonstration of this sort was more commonly known or received among practical mechanics, than that the best angle for the sails of a wind-mill, at the beginning of their motion, was an angle of forty-five degrees; and that the maximum of an under-shot water-wheel was when it mov'd with one-third of the velocity of the water: but Mr. SMEATON, in an excellent paper in the Philosophical Transactions, has refuted this opinion by the clearest experiments.

I had intended to diversify these experiments, and to extend them to a more interesting subject of enquiry, to determine the best shape of sails, and the angle to which they should be set, to obtain the greatest progressive effect with the least lee-

way; but, as a more complicated apparatus than I could at present procure is necessary for this purpose, I determined to offer you the slight progress I have made, in hopes that some gentleman, more conversant and more interested than myself in these inquiries, may pursue them with success and advantage to the public. I shall only remark, that the general cause of the different resistance of the air upon surfaces of different shapes, is the stagnation of that fluid near the middle of the plane upon which it strikes. The shape and size of the portion thus stagnated, differs from the shape and angle of the plane. The elasticity of the air permits the parts in motion to compress those which are first stopped or retarded by the plane, and forms, as it were, a new surface of a different shape, for the reception of those particles which succeed. With the assistance of a good solar microscope the curves of the air striking against different surfaces may be delineated, and when the general facts are once clearly ascertained, mathematicians will have an ample field for curious and useful speculation.

T A B L E.

	Turns.	Time.	Weight.
Machine alone - - -	5	4	2 8
With a parallelogram of nine inches long and four broad, one of its longest sides, parallel to the horizon and the parallelogram, inclined to an angle of 45°, -	5	4	7 0
Ditto, with one of its shortest sides downwards, - - -	5	4	7 9
With a lozenge nine inches long and four broad, with its longest side parallel to the horizon, - - -	5	4	5 8
Ditto reversed, - - -	5	4	6 0
With a square piece of tin, four inches by four inches, - - -	5	4	5 0
Ditto, eight inches by eight inches,	5	4	16 6
With the former parallelogram, placed with one of its shortest sides downwards, inclined to an angle of 45°, and bent into an arch whose chord was eight inches long, - - -	5	4	8 0
Ditto bent to an arch, the chord of which was seven inches and a quarter,	5	4	8 5

