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,

Edgeworth'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

Having faftened a ftrong joift of wood from one fide of a large room to the other, fo as to form a kind of bridge at fome diftance from the floor, I erected a perpendicular fhaft or roller, which turned freely in brafs fockets fixed into the floor and bridge upon pivots of hardened fteel one-fixteenth of an inch in diameter. On each fide of this roller was extended an arm of deal, feather-edged, and fupported by ftays of the fame material, feathered in the fame manner, to oppofe as little furface as poffible to the air when in motion.

Round the upper part of this roller was wound a ftring of cat-gut, which, paffing over pullies properly difpofed, was faftened to a fcale that defcended into the well of an adjoining ftair-cafe.

The extremity of thefe arms defcribed a fpace of more than forty feet in every revolution, the weight defcending in the fame time only fix inches. The time in all the following experiments was the fame; and, as each revolution was performed in four feconds, the velocity of the end of the arm on which the furface was fixed, was at the rate of about feven miles an hour.

The firft figure that I tried was a parallelogram of tin, nine inches long, and four inches wide. Its longeft fide was placed parallel to the floor, at the extremity of one of the arms. Its fhorteft fides were inclined to an angle of forty-five degrees from the perpendicular, and in this fituation it was carried round with its furface againft the air.

After fuffering it to revolve until I was fatisfied that its motion was become uniform, I put as much weight into the fcale as moved it with a velocity of five turns in twenty feconds. I then changed the fituation of the parallelogram, placing its fhorteft fides parallel to the floor, and inclined to the fame

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 surprised 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. Monf. 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 moved 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