LETTERS TO THE EDITOR

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The Woolwich Aëronautical Experiment

I HAVE read, not without some surprise, the accounts given by the daily papers, stating that the recent experiment at Woolwich had been fruitless. The lessons of the experiment are numerous, although it would have been easy to predict all that happened; but the impressions relating to balloons and ballooning are generally so vague and so incorrect, that I may be justified in trying to summarise the results which were obtained in connection with the siege of Paris, which might otherwise be lost altogether.

As I stated in my article on the "Flying Man" (vol. x. p. 230), the principal object to be considered in the theory of aërial motion is the friction of the moving surface against the air; the friction increasing according to the square of the rate of motion v^2, the force necessary to move the body at a certain rate varies according to v^3. Consequently it is easy to impart a small motion to a balloon; but the difficulty very quickly becomes insuperable except with an almost inexhaustible source of power, such as a powerful steam-engine. *Hand-power cannot be made of any avail;* M. Dupuy de Lome's experiments proved this definitively, and that question must be considered as settled in favour of *steam-power*.

The problem now at issue is to ascertain whether it is possible to construct a *safe* fire-engine balloon, and to use it successfully for travelling to a distance. But I shall give some calculations on the recent experiments with hand power.

If we suppose that the motion of the directing balloon is uniform, the friction consumes all the force which is generated;* consequently, if n = number of men pulling the fan, m = the real motive power generated by each man, k = a coefficient which depends on the nature of the surface of the balloon, l = great axis of balloon, r = radius of equatorial section, v = rate of motion—I suppose that r was the same for Dupuy de Lome as for Bowdler, and that M. Dupuy de Lome's great axis was $2r$, his number of men twelve instead of two, and his rate of motion 9 ft. per second—we shall try to find what ought to be the motion of Bowdler's balloon.

As according to the principles of mechanics

$$v = \sqrt[3]{\frac{n.m.l}{k r^2}}$$

it is easy to find

$$v^1 = \frac{3}{\sqrt[3]{6} \times 2}$$

under these circumstances. But m is not the same, as Dupuy de Lome's men were pulling on a large screw acting without any transmission; Bowdler's apparatus was a small screw 3 ft. in diameter. I suppose Bowdler's utilisation was only half of Dupuy de Lome's; consequently the real equation is

$$\frac{v^1}{v} = \frac{1}{\sqrt[3]{6} \times 4} = \frac{1}{\sqrt[3]{24}}$$

not far from $\frac{1}{3}$. The motion of Bowdler's balloon could not be more than 3 ft. a second.

It was impossible for Major Beaumont to see any difference with the motion of the air being at a distance from the earth. It could be ascertained with very great difficulty even with an aëronautical compass of the best description.

But the fact of the balloon having been put into a state of rotation by the rudder is a demonstration of the fact of a differential motion having been obtained. It is the very pressure resulting from the differential motion which is the only force that rotates the balloon in acting on the rudder. The rudder is pushed as it is in the sea when the ship is acted on by sails or steam, and in the air the action is very easy, as the balloon is almost symmetrical around its vertical axis.

It is true the governing power could be imparted very easily by direct action on an excentric helix adjusted for the purpose, as has been suggested, but not tried, so far as my knowledge goes. I will say the same of the vertical motion, which is very important also for ballooning; but the theory being a little more complex, I shall keep it in reserve for a future communication.

The rotatory power is of importance in making observations in the air, and it is praiseworthy in Major Beaumont and Mr. Bowdler to have directed their attention to that particular point.

The abstract principles of aëronautics have been pretty well ascertained, but the practice is a very difficult thing, and can only be tested by a series of experiments. With such an experienced balloonist as Mr. Coxwell, and the resources of an enlightened Government like that of England, it seems likely that such experiments will be tried more easily than in France. Under the present circumstances, I think it is our duty to assist you so that you may derive benefit from the knowledge we bought so dearly amidst our great national calamities.

W. DE FONVIELLE

* I speak only of the motion in still air.

Fogs, Field-ice, and Icebergs in the Atlantic

THREE unwelcome phenomena have this year, in more than an ordinary degree, vexed the coasts of the United States and the navigation of the Atlantic; I allude to fogs, field-ice, and icebergs. The first have so much interfered with the success of the Nantucket fishermen that but few mackerel have been caught by the seine, the schools cannot be followed, and the boats have frequently remained idle for days. No one who has not met with these fogs can form an idea of their density. With a bright sun shining over head, objects cannot be discerned at the distance of 100 ft. Collisions have been numerous in all the great American ports and rivers. On one occasion hundreds of tons of cargo remained two days in New York before it could be transported across the Hudson to Jersey city, although the distance was frequently under a mile from wharf to wharf.

At sea these fogs have extended almost without a break for 1,600 miles, the wind being from east, through south, to west. When sounding the steam whistle I noticed, what has probably been noted before, that the denser the fog the greater were the reverberations, and that the echo was always heard to windward as plainly as if it were deflected from a cliff in that direction. I presume that this arose from the resistance the waves of sound encountered in travelling against the wind, none being heard to leeward. These fogs are attributed to the great difference which exists in spring and summer between the temperatures of the air and water. Having, however, often remarked that they come when these conditions are not found, I am induced to believe the cause must often be looked for in the atmosphere alone, by the mixture and condensation of the different strata of air there. At times these fogs are in streaks, and the alternations of heat and cold, as they sweep by, are very noticeable. Now, if the sole cause were due to a simple difference of temperature between the air and water, I cannot understand why this should be, unless the sea was composed of similar streaks of hot and cold water, which here is not the case.

In the Atlantic, seamen were astonished to find that early in February field-ice and bergs had reached the parallel of Cape Race, and have since been seen as far south as 42° N. lat., drifting to the north-east in the heated waters of the Gulf Stream. Two steamers and an equal number of sailing vessels are known to have been seriously damaged by colliding with them; and the wonder is that so few accidents have taken place when it is borne in mind that for hundreds of square miles the steam and sailing tracks between America and this country are dotted with them. A few of the bergs have been supposed to be three miles in length, and on two occasions steamers passed through or around ice-fields 100 miles in length. It is also alleged that another was stopped five hours by field-ice so far south as the forty-third parallel.

There is a general belief that the vicinity of ice may be readily detected by the fall in the temperature of the water. Unless it be in very large masses, and the ship close to, this test is not of much value, owing to the natural law which causes a cold surface fluid to sink until equilibrium is restored. A better test is the cold, damp feeling of the air, but this is only noticeable when to leeward of the berg or field, and is practically of no value, as the wind passing over the sea-water at 28° will cause a similar sensation. In some states of the atmosphere the clouds near the horizon assume a peculiar grey tint when the ice-field is of large dimensions.

Unless the weather be very foggy, an iceberg is easily distinguished on the darkest night at a considerable distance by the light reflected from it, and to this cause I attribute the great immunity of ships from accidents. Ordinary islets dropped in the Atlantic would cause an infinity of wrecks, owing to the absence of this useful property. When an iceberg reaches a low latitude it loses much of its beauty; the brilliant white and pris-