GLIDING AND GLIDERS AT ITFORD

Discussion held at the Engineers' Club on January 12th, 1923. Mr. H. B. Molesworth in the Chair.

The discussion was led by Mr. W. O. Manning, who emphasised the following points:

It was difficult to say what form the glider of the future would take.

Speed would depend to a large extent upon the situation in which the glider was to be used.

For ærodynamical efficiency it seemed obvious that the machine should be a monoplane.

Without an engine the problem of the fuselage was simplified, and in a glider might differ very much from that of a power-driven machine.

There were a good many things to be learned from gliders from a power-driven point of view. The shape of the fuselage was one, and in the future we should no doubt have considerable information on such subjects. We needed information on the exact camber of high lift wings, for it was well known that wings which gave a high lift in the wind tunnel did not give the same results in the full size.

Gliding would certainly be of the greatest value in studying body resistance.

Capt. Sayers conveyed the regret of Major Wright at being absent through indisposition, and said there was one practical feature which Major Wright felt to be of the utmost importance. They had had to leave a glider at Itford for nearly three months, and the shed blew down and crashed it. Major Wright wished to emphasise the importance in this country of choosing gliders for experimental purposes so that they should be transportable by road. That point was certainly important in this country at present, but it was hoped that in the near future it would be less so, as there seemed to be very real signs that in the near future a proper protective shed would be erected near Itford.

With regard to his own experiences, Capt. Sayers thought that Mr. Manning had undoubtedly struck one of the points where gliding would teach us a great deal, namely, the matter of body resistance. We had had little experience in England, but Germany had had much, and there was some evidence that the shape of the body was very important indeed. In the case of the Hanover, for instance, the complete machine had less resistance than the wing alone.

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Another point which English experience showed was the fact that the problems of controlling a glider were rather more complicated than with power-driven machines. We were faced with the fact that gliders fitted with controls which one would expect to operate in a power-driven machine had no appreciable effect. It was interesting to discover that the Germans in their early experiments appeared to have struck exactly the same troubles as we did. They therefore did not make fun of our failures in lateral control. They had spoken very highly indeed of the English experiments, and were warning their people that they would have to get a move on.

MAJOR C. C. TURNER wished to ask one question. He had examined the fuselage of the Peyret machine with much interest, and wondered whether the shape of that fuselage was purely for constructional or aerodynamical reasons. It seemed to him that the designer had introduced a form of fuselage which did not seem to be quite accounted for by aerodynamical considerations. The speaker concluded that although he might have betrayed his ignorance in asking such a question, he should like to have light upon it.

MR. MANNING replied that he did not remember what the shape of this particular fuselage was, but that it was probably a mixure of aerodynamical and constructional points.

MR. ANDREWS said that the late Lord Rayleigh in his "Mechanical Principles of Flight" formulated the fundamental principles of gliding in the following paragraph:

"I premise that if we know anything about mechanics, it is certain that a bird without working its wings cannot, either in still air or in a uniform horizontal wind, maintain its level indefinitely. For a short time, such maintenance is possible at the expense of an initial velocity, but this must soon be exhausted. Whenever, therefore, a glider pursues its course for some time, without working its wings, we must conclude:

1. That the course is not horizontal.
2. That the wind is not horizontal.
3. That the wind is not uniform."

Let us investigate the matter from first principles.

I. A plane falling through *still* air under the influence of gravity.

Speed will gradually increase until the Resistance of the air is equal to the Weight of the plane, after which the plane will continue to fall with uniform motion. Generally speaking, the motion is unsteady when tried practically, due to the fact that the c.g. of the body is at the geometrical centre in the body itself. If the c.g. is brought lower the plane will fall steadily. This is really the case of the parachute, and the size of the parachute should be such that the limiting velocity of downward motion is small, say about 15 f.p.s.

The advantage of a low c.g. from the point of view of stability can be readily seen by a few simple model experiments, as, for example, gradually lowering the c.g. of a postcard by suspending a nut attached to the four corners by thread below the postcard. Remember that in vacuo all bodies fall at the same rate, with uniform accelerated motion, irrespective of weight. This independence of weight can be shown experimentally by cutting a paper disc to fit a penny and dropping them together with the paper uppermost.

II. Extension of I due to an initial velocity—say horizontal—given to plane.

Before considering this case it is as well to look at the effect of the c.g. not being at the centre of symmetry. Since reaction due to resistance of air can only act at centre of symmetry, assuming plane to be homogeneous, it is obvious that in this case there can be no equilibrium, and motion will be unstable, its exact form depending on external circumstances.

If, however, the plane is given an initial horizontal velocity, the point of application of reaction due to the air moves forward and acts through the centre of pressure—a term well understood by aeronautical engineers—and equilibrium is attained, and we get the conditions necessary for a downward glide. This is the well-known case of volplaning with the engine shut off in the case of an aeroplane, and is solely due to the influence of gravity and not to the action of wind.

III. Wind effect and gliding proper.

It is important to remember when considering this side of the question that the air forces depend upon the motion of the plane *relative to the air*, and not upon the motion as seen by an observer on the ground. What is really required is an observer moving in the air.

A. Suppose plane falling vertically at a uniform velocity of 16 f.p.s., and that wind is blowing vertically upwards with same velocity. The forces balance and no motion will result.

B. Plane falling in any direction, say α to the horizontal, plane parallel to the ground, wind velocity U; wind blowing in any direction, say α^1 to the horizontal ground line, with velocity U^1.

Resolving horizontally and vertically we have

i. *Vertically downwards*

$$U \sin \alpha - U^1 \sin \alpha^1$$

ii. *Horizontally*

$$U \cos \alpha - U^1 \cos \alpha^1$$

If $U \sin \alpha = U^1 \sin \alpha^1$ the glider maintains a horizontal position, its motion being determined by the relative values of $U \cos \alpha$ and $U^1 \cos \alpha^1$.

If $U^1 \sin \alpha^1 > U \sin \alpha$ the plane will rise above its original position, and therefore flies.

If $U \sin \alpha > U^1 \sin \alpha^1$ the plane loses position and comes to the ground.

C. Another case which is fairly easy to consider is that of a variable wind, which may be due to

(a) Horizontal striated flows.
(b) Fluctuating wind.

Take the case of the fluctuating wind perfectly horizontal. Suppose the wind to rise in intensity from zero to U^1 f.p.s. Then the rate of change of velocity of the wind will be $\frac{dU^1}{dt}$ (since rising from zero to U_1).

By the well-known law of dynamics $P = mf$ this acceleration will produce on the aeroplane a force $m\frac{dU^1}{dt}$, and the glider can fly in a horizontal wind. Unfortunately, this effect cannot continue indefinitely—more than a gale would

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soon be blowing—and a rising wind is always followed by a diminishing wind. In this case $\frac{dU^1}{dt}$ will be negative, and therefore, in order to glide under these conditions, it will be necessary to fly *with* the wind, that is, the pilot must turn completely round. In this way he may arrive over his original starting place at an increased altitude.

We thus arrive at the following general conditions. In order to glide :—

1. Fly against a rising wind.
2. Fly with a falling wind.
3. To rise, take advantage of a steady wind blowing upwards, which means that for successful gliding it is necessary to choose a slope up which the wind usually blows at an angle in excess of the gliding angle of the plane in still air.

Since writing the foregoing, a footnote on p. 251 of Vol. 2 of "Aerial Flight" by F. W. Lanchester leads me to suppose that Rayleigh's conclusions quoted previously, from his "Mechanical Principles of Flight," were originally published in a letter he wrote to "Nature" in 1883 (see XXVII p. 534).

On p. 263 of the same volume Lanchester gives the velocity of the soaring bird as from 30 to 50 f.p.s., and its natural gliding angle as approximately 1 in 5 or 1 in 6. Consequently, the downward velocity of a bird in gliding flight relative to the air is about 7 to 8 f.p.s., and therefore, in order for gliding (or soaring) to take place, it requires an up-current of a velocity equal to or superior to this value.

MAJOR GNOSSPELIUS said that he saw a good deal of the experiments in Germany and afterwards witnessed those at Itford, and emphasised the difference in the conditions of the two countries. The Germans managed to rise to a considerable height—he thought it was 1,000 feet. The official figures were 220 metres.

At first it appeared very strange to see the gliders go off, rise steadily and fly without an engine. When the speaker was in Germany the wind was something like 5 or 6 metres a second, equal to 12 feet a second. That was much lower than was ever the case with satisfactory results at Itford.

A noticeable point was that the gliding angle did not seem to be the whole thing. One machine would start, go out and make a glide into the valley. The next would fly level, and another would go higher. The relative gliding angles of these machines could be measured quite well. This was rather interesting because on looking at the machines one could see no real reason for the difference. The Hannover was certainly the best. It was their last year's machine with additions to the wing shape. The old one had taper wings, and the new one square, which seemed better. The Greif machine looked a very good apparatus, having a circular body, with tapered wings, both made of wood. One would have said, looking at it, that it was a better machine than the Hannover, but although it glided well in a low wind velocity, it was not so good as the Hannover. Major Gnosspelius was interested to find that this machine rather bore out some experiments he had previously made. The original Vampyr had a square body, with the tail on a level with the main plane. The Greif had a nice bird-shaped body, but the tail was in

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a different place, the wings being considerably above it. The Vampyr at first resembled the Greif without a tail. A small tail was afterwards added, and the resistance was thereby increased by about 25 per cent., which spoilt it entirely. That was a very great surprise. The tail was finally lifted, and the performance thereby improved. Major Gnosspelius was of opinion that the resistance in the case of the Greif, Hannover, and Vampyr was increased on account of each having a low tail, though he could not say why this should be so. Last year the Hannover won the competition, followed by the Vampyr, Darmstadt and Greif.

These tests showed the relative efficiency of the various machines, but one would want to see them all tried on the same day. With these German tests it was simply a case of higher wind velocity. If they had 5 metres a second they did something; under that they did not do much.

The winds that were flown in at Itford were extraordinary. The speaker did not think that many pilots of power machines would have cared to go out into that kind of country in such a low wind. He rather thought that the controllability of a power machine might have been very severely tested under those conditions. In Germany the speaker had met Dr. Klemperer, who was very interesting, and probably the most scientific of the German gliding pilots. He believed firmly in controls, and was convinced that some of the energy could be extracted from pulsating air. Mr. Handley-Page had seen Dr. Klemperer gain a height out of the pulsation of the wind. It was rather difficult to understand what this pulsation was, but, at any rate, Klemperer believed that he could get some power out of it, and the speaker felt there was some reason for this statement. Last year Klemperer's machine was the most satisfactory of all. Major Gnosspelius was of opinion that if one could make a machine with a sufficiently fine gliding angle to get enough energy out of this pulsation, they had solved the problem of getting about without much energy. That, in his opinion, was what birds did. They managed to extract this energy, and we should try to do the same. The glider was obviously the thing to try it with, because one could be constructed for a reasonable sum, say £100 to £200, which was much more feasible than £1,000 or so for a power machine.

We should get up a Gliding Club to foster the business, as the idea was certainly worth going for.

Major Gnosspelius concluded with this advice: "Put the tail high."

MAJOR C. C. TURNER said that it might interest those present to know that experiments had been made by Mr. Dynes with a very delicate anemometer for testing the effects of air pulsations. His experiments had proved it to be a fact that aviators really flew better when going into the wind than with the wind behind them.

MR. PATRICK ALEXANDER recalled that he was one of the few who saw Otto Lilienthal's experiments in 1892 and Pilcher's in 1896. Even in those days Hargreaves of Australia had investigated the difference between pulsation and fluctuation. Experiments by physicists and meteorologists had proved that the vibration of the waves of the sea induced waves in the air, which might be vertical as well as horizontal. Research in this direction would no doubt produce some valuable data in the sphere of aeronautics.

Mr. Charles Cooper put the question as to what was likely to be the commercial utility (if any) of gliding. One apparently could not go anywhere at will in a glider at present, and everyone did not wish to remain in the air for nine hours. Could anyone go anywhere in a glider—say from London to St. Albans, or to Manchester, which would be a very delightful thing.

Major Gnosspelius said that the results of the next gliding competition would probably answer Mr. Cooper's question.

Capt. de V. Robertson asked whether observers at Itford saw any effects on soarability due to sunlight.

Mr. Manning said he believed there was no sunlight at that time.

Mr. Houlberg said that the impression left with him at Itford was one of disappointment, and he came away with the conclusion that not much more had been learnt than was already known. The point that impressed him most was the extreme need for controllability of the machine. Of two machines of identical performance, one had much better control surfaces, and that one was the better of the two. Maneyrol's machine was not new, but had been abandoned owing to difficulties of structure and control, but with the latter improved it had put up a good performance. This pointed to the fact that, given sufficient controllability, an apparently inefficient machine could be made to glide under most conditions.

Another thing which the Itford trials had led the speaker to deem desirable was some means of varying the wing surface. Observation of bird flight showed that they made tremendous use of the fact that they had the power to vary their loading. In Maneyrol's case, if he wished to keep in a particular direction, owing to his low loading he would in many cases be drifted back, and either have to reduce his gliding angle or go over the ground again. There was no doubt that variable loading of some kind would enable one to keep better direction.

Mr. J. L. Parker gave some interesting facts in his experience as a pilot. He had not done any gliding, but had made observations on various atmospheric effects when flying. He had almost always found that on a rough day one could get a better climb than on a calm day. With regard to Major Turner's remarks about climbing into the wind or otherwise, the speaker had never noticed any difference.

The Chairman described his observations on the flight of vultures when in India. On a perfectly calm day the birds would rise to about one hundred feet, and without moving their wings again would soar round and round in a circle and go right out of sight up in the air, never moving their wings in the least. The speaker felt that it would be most interesting to learn the secret of the soarability of birds and to what extent this was due to the pulsations of the air. We should then be well on the way to improving the lifting power of an aeroplane. The meeting that evening had been highly interesting, and he proposed a hearty vote of thanks to Mr. Manning for leading the discussion. This was passed unanimously, and the meeting closed.