ing point in this chapter is the author's suggestion that night travelling is an adaptation to the necessity that most birds have of devoting the daytime to the seach for food.

The book is lucidly and carefully written and the author occasionally slackens his rein and reveals his power as a stylist, the description of a "bird-night" at Eddystone being perhaps the finest example. There are several good photographs of various stations, notably those of Fair Isle by Mr. W. Norrie, but the chief illustrations are maps and weather-charts—all conspicuous for clearness and simplicity. We have already referred to the admirable first frontispiece. The book is dedicated to the Duchess of Bedford, herself an ardent ornithologist, who has given the author valued assistance.

THE QUESTION OF THE BIPLANE VERSUS *THE MONOPLANE.*

THE recent order of the War Office suspending the use of monoplane flying machines for military purposes has led to the renewal, in the daily Press, of a discussion of the old riddle, "Which is the better, the monoplane or the biplane?" When Blériot crossed the Channel, the daily papers rang with the praises of the monoplane; now everyone favours the biplane, and there is a danger lest the monoplane may be condemned for faults not necessarily attributable to the mere fact that it is a monoplane.

The military authorities have wisely called in the assistance of the National Physical Laboratory in seeking an explanation of why so many of the recent accidents have occurred with monoplane machines. Even if the work placed in the hands of the Teddington department does not extend beyond overhauling and testing the machines used in the Army, the physicists ought to have sufficient scope for arriving at many important conclusions regarding essential features of aeroplane construction. For the purposes of an inquiry of the type proposed, it appears desirable that the same tests should be applied to biplanes as to monoplanes; but the value of the work will be greatly enhanced if the investigation is conducted on general lines, and not confined to the mere testing of the Army machines. It is easy enough to say that when a stay has broken it should be replaced by a stronger one, and to draw up a report which would suffice to enable any defects in existing machines to be patched up, but it is essential for real progress that the Laboratory authorities should have a free hand to assist in the evolution of a more perfect type of flying machine than either the existing monoplane or biplane.

It must not be forgotten that the terms monoplane and biplane usually imply something more than the mere difference between a "single-decker" and "double-decker" (to quote the German equivalents). The former usually has the propeller in front, the latter behind. Thus an inquiry necessarily turns on at least two points, namely, the relative advantages of the single- and double-decker, and whether the propeller is better placed in front or behind. Further subjects suggested are the gyrostatic effect of the propellers, the relative merits of rotary and oscillating engines, and so forth.

In regard to the first point, it must be remembered that even Lilienthal experimented successfully with the double-decked type; that Chanute, after trying not only "single-" and "double-decked" gliders, but also "multiple-winged machines," finally decided on the glider with two superposed surfaces as the best on which to experiment; that his experiments were continued by the Wrights, and led to their first realisation of artificial flight. One advantage of the two-surfaced arrangement is that, with an equivalent area, the wings can be made of lesser span, and thus the bending moments they have to sustain are proportionately reduced; moreover, these bending moments are much better sustained by the framework, which naturally takes the form of a latticed girder. Of course, from this point of view a triplane would even be better than a biplane, but the gain would be less important.

There would be no difficulty in constructing a "two-decker" with a propeller in front, and, from the point of view of the physicist, the position of the propeller depends largely on whether it is better for the propeller to receive the wash from the planes or for the planes to receive the wash from the propeller. One advantage of the latter plan has not, perhaps, received the attention that it deserves. It must not be forgotten that the action of the propeller sets up a rotation in the "wash" behind it, and, as Sir G. Greenhill has pointed out, so far from being negligible, the amount of this rotation is directly related to the horse-power and rate of revolution of the engine. In fact, the propeller exerts on the air a constant torque, which tends to produce angular momentum, and is equal in amount to the torque of the engine. If, then, the main planes are placed in the wash of the propeller, the rotating air on striking them will produce a difference of pressure on the two sides tending to counteract the corresponding torque on the aeroplane, and the machine will not heel over sideways to the same extent that it would if a single propeller were placed behind. For the purposes of the War Office, the propeller in front is disadvantageous, as it interferes with scouting or shooting from an aeroplane. On the other hand, we have the recommendation of a well-known engineer that the engine should be in front of the aviator, so that the latter shall not be crushed underneath the former in case of an accident.

Apart from these essential differences between monoplane and biplane, great importance attaches to an investigation into the gyrostatic couples caused by both rotary engines and propellers. At present, apart from setting up strains in the framework, which require the latter to be adequately stayed, these cause a mixing-up of the longitudinal and lateral motions of the machine which must necessarily greatly increase the danger of accidents when the machine is being navigated in gusts of wind. It is important that more

attention should be given to the question of balancing, not only of the actual propeller torque by the use of two propellers, but also of the gyrostatic couples due to both the propeller and the rotary engine. Why do not the makers try an engine rotating about a horizontal axis perpendicular to the line of flight, driving a pair of propellers rotating in opposite directions by means of bevelled cogs? The arrangement would be perfectly symmetrical, and the gyrostatic couple of the engine *might* be used to assist in lateral steering.

Another disadvantageous feature of many monoplanes, though not an essential feature of them, is that the wings are usually of considerable breadth, and, of course, are cambered. The result is that when such a machine pitches, effects may occur the nature of which will remain entirely unknown until some experimental knowledge has been obtained regarding air pressures on rotating planes. To assume that these effects are negligible, or even that they may not be the cause of accidents, is, in the circumstances, scarcely justifiable.

It will be interesting to see whether any questions of stability are considered in connection with the present inquiry. The tendency which has existed up to the present time of shelving the problem of inherent stability, and attempting to attribute accidents to other causes, is, after all, very natural. If stability could be ignored altogether, the problem of aviation would be greatly simplified, and much laborious work, both theoretical and experimental, would be saved. Those of us who have spent much time in studying the theory of stability would have been glad to give our attention to other subjects instead, had we believed that a final solution of the problem of flight was possible which should make aviation independent of stability considerations. At the present time, no experimental information exists regarding inherent stability, and a comparison of theory with practice is urgently needed. Under theoretically assumed conditions, stability, both longitudinal and lateral, is greatly affected by variations in the inclination of the flight path to the horizon, and this is a point on which experimental tests would be of particular interest. The fact that so many accidents have occurred the causes of which are unknown shows that aviators have not yet been altogether successful in their attempts to dispense with theories of, and experiments on, stability. The accounts of many accidents are strangely suggestive of what would happen under theoretical conditions if an aeroplane should be flying at an inclination to the horizon consistent with inherent instability.

As regards the monoplane and biplane, these limits are probably very different in existing machines of the two types, but there is no essential difference between the "single-" and "double-decker" in regard to stability. Many monoplanes are of the Antoinette type, and can be made laterally stable by making the tail of sufficient length; many existing biplanes do not possess sufficient auxiliary surfaces for lateral stability, though this defect is probably remedied when the planes are bent up; on the other hand, the auxiliary planes in them are as a rule more favourable to longitudinal stability. These are, however, details of construction which do not depend on whether the machine is a monoplane or biplane. It is probable that most existing aeroplanes satisfy the condition that lateral, like longitudinal, *instability* increases when rising in the air.

It is necessary to repudiate any suggestion that a so-called "theory" of stability (which is really an experimental study of the results of certain assumed hypotheses, the apparatus for which are the methods of mathematical analysis) should be applied to actual aeroplanes without first being subjected to a second experimental test performed with the actual aeroplanes or models of them. But would not even this course be better than continuing to use aeroplanes about the stability of which nothing is known? And admitting that most flights have to be performed in gusty winds, is this any reason for being satisfied with a flying machine which would not fly straight in still air? Some people appear to think so. But is it not probable that the problem of stability presented by an actual aeroplane is more complex and not less complex than that presented by a system of narrow planes moving at small angles through a resisting medium? If this be so, the complexities of the simpler problem may afford some clue to those existing in the more difficult and at present unsolved problem.

It is hoped that no suggestions made in the present article will be regarded as authoritative statements except so far as they may be confirmed by experiments conducted with the aid of mathematical or physical apparatus. If any conclusions are to be drawn from these remarks, they should be to the effect that it is far less important to try to decide whether a biplane is better than a monoplane than to investigate the relative merits of flying machines on a perfectly broad basis. It is, therefore, to be hoped that the staff of the National Physical Laboratory will not only be given a very free hand in the investigations that are placed in their hands, but that they will produce a powerful and thorough report, and—if a small criticism is permissible—give a little more attention to formulating broad general principles, and confine themselves a little less exclusively to the tabulation of minute experimental details than they have done on some previous occasions.

G. H. B.

THE INTERNATIONAL METEOROLOGICAL COMMITTEE.

WEATHER TELEGRAPHY AND MARITIME METEOROLOGY.

MEETINGS were held in London during the week ending on Friday, September 20, of two Commissions constituted by the International Meteorological Committee to deal with questions concerning international weather telegraphy on the one hand, and with those concerning mari-