Messrs. Koefoed and Helland-Hansen, yet no mention of this veteran's name or work is made. Buchanan, Nansen, Bruce, and others have also observed this phenomenon. We doubt also if some of these old observations are less exact than those of more recent date. The Buchanan methods give, for instance, observations of great exactitude, and certainly equal to any of the most recent observations of the younger Scandinavian school of oceanographers.

Part ii. deals with instruments and methods; and here useful discussion could be entered upon, as, for instance, on the question as to whether one is able to obtain results of greater accuracy with the more finely graduated Richter thermometer on the deck of a ship in the polar regions, with discomforts of mist, sleet, snow, wind, and weather. A stronger marking and coarser scale certainly in many cases will give more accurate results than the very finely-graduated scale of the Richter thermometer instruments, as the reviewer knows by extensive work on board ship in all weathers and almost all latitudes. The question of a ridge rising to within about 400 fathoms of the surface is discussed, but so far no absolute proof of this has been obtained, owing to the great difficulty of penetrating the polar pack—some of the heaviest ice in the world—between the north of Spitsbergen and the east of Greenland. The Duke of Orleans has, however, come nearer accomplishing this important investigation than anyone else, for he obtained a more complete line of soundings two degrees further north in the middle longitudes of the Greenland Sea than any previous navigator.

The authors divide the Greenland Sea into three areas:—

(1) East—having high temperatures and salinities, being influenced by the Gulf Stream.

(2) Middle—a deeper region with mixed conditions.

(3) West—a shallower region with low temperatures and salinities, being influenced by the polar current.

Plate lxii. gives a chart indicating the stations of the *Belgica* and those of other expeditions in the Greenland seas and regions adjacent; but again we miss the stations of Leigh Smith, 1870, those of Bruce (S.Y. *Blencathra* of Major Andrew Coats), 1898, and those of the Prince of Monaco, 1898–9. Many interesting problems are raised by the temperature, salinity, and current observations made by the Duke of Orleans and his staff, and not least of them is the theory of the Spitsbergen-Greenland ridge already referred to, but it is impossible in this short review to enter fully into all these questions.

The zoology of the voyage, discussed by Prof. C. Hartlaub, Messrs. D. Damas, E. Koefoed, and M. J. G. Grieg, occupies more than a third of the volume. The plankton work is very exhaustively and thoroughly handled by Messrs. Damas and Koefoed. Several dredgings in depths down to 750 fathoms also secured a number of interesting benthoic forms. The numerous inset maps and sectional diagrams and tables are especially useful, bearing as they do on the distribution of plankton.

The plates by Werner and Winter maintain the high reputation this firm has justly won. M. Édouard Mérite's work is reflected throughout the natural history of the expedition, though much of this excellent artist's colour work only appears in the Duke of Orleans's less technical work, "A Travers la Banquise du Spitsberg au Cap Philippe." Dr. Récamier, too, did much to make the voyage a success. There are useful sketches of the new coast between 77° and 79° N., and some most excellent half-tone blocks, many of which show polar ice well; the frontispiece is especially to be commended as "a thing of beauty." One regrets to see that glazed paper is used instead of pure rag paper, which actually produces richer effects and is infinitely more durable.

Altogether the Duke of Orleans is to be most heartily congratulated, not only for having personally conducted all the above work, but even more so for having placed the observations and material gathered during the voyage into competent hands for examination and description, and for having spared no trouble or expense in bringing out a volume which is second to none as a monumental contribution to the oceanography of the Arctic Ocean. Too often polar expeditions are dispatched by the help of men of means, but these same people have little or no conception of making use to the full extent of the material, obtained with great care, toil, and trouble, on the return of the expedition. The excellent work of many trained men of science who accompany such expeditions is in consequence largely wasted. The Duke of Orleans, however, has realised the full value of this subsequent work, and the thanks of the scientific world is due to him for having seen it through so handsomely to the finish.

WILLIAM S. BRUCE.

AËROPLANE STABILITY.

IN 1896 I had the pleasure of attending a lecture on naval architecture given before the British Association in Liverpool by the late Dr. Francis Elgar, F.R.S. I had learnt the theory of the metacentre in my undergraduate days, but it came to me as a great surprise to learn that this theory had only been evolved after many ships had foundered, owing to want of theoretical knowledge of their conditions of stability.

I was interested in aërial navigation at the time, and although I had not got further than throwing gliders, it was evident from their behaviour that a mathematical theory of stability must necessarily be of even greater importance in connection with aërial navigation than with naval architecture, and I wrote in *Science Progress* to the effect that if the future development of artificial flight were not to be a repetition of the chapter of accidents by which naval architects had gained their theoretical knowledge, there would be abundant work for mathematicians in reducing the conditions of stability to pure calculation.

About the year 1903 I noticed that if a glider or other body is moving in a resisting medium, such as air, in a vertical plane with respect to which it is symmetrical, the small oscillations about steady motion in that plane are determined by a biquadratic equation; and Prof. Love directed my attention to the condition of stability given by Routh. Mr. W. E. Williams was a post-graduate student in my department, and with his collaboration we published a paper on "The Longitudinal Stability of Aërial Gliders" (Proc. Royal Soc., lxxiii.), which was intended to direct attention to the general method, and the importance of further investigation, rather than to furnish a complete solution of the problem.

Mr. Williams shortly afterwards obtained a so-called "Research Fellowship"; but "research" in this case was interpreted as meaning practical work done in a physical laboratory away from Bangor, so the award had the effect of preventing the continuation of original work on this important problem. On the other hand, the necessity of providing, with one assistant, classes in all grades of pure and applied mathematics, and of devoting special attention to the requirements of junior students whose knowledge of the "first four books" and of arithmetic had been neglected at school, left no time for me to carry on the work single-handed. It is only since the comparatively recent abolition of these *infra* university courses that I have been able to give any attention to the subject.

Some criticisms having been raised by the late Captain Ferber, mainly referring to the form in which the conditions of stability were stated, I suggested his developing the work as I had not time to do so. His results were published in the *Revue d'Artillerie,* October and November, 1905, and include a discussion of lateral as well as of longitudinal stability.

At the beginning of last year the work of my department was, for some unknown reason, exceptionally light, and I had in Mr. E. H. Harper an assistant well able and willing to collaborate in a much more exhaustive investigation both of longitudinal and lateral stability. About October I received a formal letter of inquiry from the Government Committee, in an envelope which I at first took for an income-tax application, and in reply stated that what I wanted was a small grant to enable me to devote my whole time to this work. I received a reply that the committee "regretted," &c., but that "very great interest was taken" in the work. The main difficulties of the subject have, however, now been practically cleared up, though a long time must elapse before a detailed written account is ready for publication. Had any prizes been offered in England for which such an investigation would be eligible, the delay might have been avoided or shortened.

Reference must be made also to Mr. Lanchester's remarkable investigations, published in his "Aërodonetics," and to the appearance of a German translation of the preceding volume, "Aërodynamics," shortly after its publication in English.

It is here proposed to give a general idea of the peculiarities of aëroplane stability as deduced from my work, and a comparison with Ferber's and Lanchester's methods; though with regard to the latter it is rather difficult for any critic to be sure of not misjudging the author's intended meaning.

It is necessary that the distinction between equilibrium and stability should be kept in mind. An aëroplane is in *equilibrium* when travelling at a uniform rate in a straight line, or, again, when being steered round a horizontal arc of a circle. A badly balanced aëroplane would not be able to travel in a straight line. The mathematics of aëroplane equilibrium is probably very imperfectly understood by many persons interested in aviation, but it is comparatively simple, while the theory of stability is of necessity much more difficult.

It is necessary for stability that if the aëroplane is not in equilibrium and moving uniformly it shall tend towards a condition of equilibrium. At the same time, it may commence to *oscillate,* describing an undulating path, and if the oscillations increase in amplitude the motion will be unstable. It is necessary for stability that an oscillatory motion shall have a positive modulus of decay or coefficient of subsidence, and the calculation of this is an important feature of the investigation. A slight reference to this question of rolling is given by Chatley on p. 99 of "The Problem of Flight," but he seems to have overlooked the fact that this damping may be, and often is, negative in the case of unstable aëroplanes.

At the present time it is certain that aviators rely on their own exertions for controlling machines that are unstable, or at least deficient in stability, and they even allege that, owing to the danger of sudden gusts of wind, automatic stability is of little importance. Moreover, even in the early experiments of Pilcher, it was found that a glider with too V-shaped wings, or with the centre of gravity too low down, is apt to pitch dangerously in the same way that increasing the metacentric height of a ship while increasing its "statical" stability causes it to pitch dangerously. It thus becomes important to consider what is the effect of a sudden change of wind velocity on an aërodrome. If the aërodrome was previously in equilibrium it will cease to be so, but will tend to assume a motion which will bring it into the new state of equilibrium consistent with the altered circumstances, *provided that this new motion is stable.* Thus an aërodrome of which every steady motion is stable within given limitations will constantly tend to right itself if those limitations are not exceeded. Excessive pitching or rolling results from a short period of oscillation combined with a modulus of decay which is either negative (giving instability) or of insufficient magnitude to produce the necessary damping.

The new work depends very largely on the property that for a system of *narrow aëroplanes inclined at small angles to the line of flight* approximate methods may be used, greatly simplifying the algebra, and enabling the various oscillations to be separated and their moduli of decay to be calculated approximately. Of the six equations of motion as applied to the small oscillations of a symmetrical aërodrome, three determine oscillations in the plane of symmetry, and lead to conditions of *symmetric* or *longitudinal* stability. The other three determine asymmetric or skew symmetric oscillations, leading to conditions of *asymmetric* stability. The three equations in each set are mutually interdependent, but independent of the other three, thus accounting for the fact that Lanchester found it impossible to separate "lateral" and "directional" stability. Failing any better terminology, I have provisionally adopted the term "asymmetric" stability.

Of the two, symmetric stability presents by far the simpler problem. For the systems above mentioned there are two symmetric oscillations, one of long and one of short period. The short-period oscillation consists mainly of an oscillatory motion of the centre of gravity perpendicular to the line of flight (*i.e.* a vertical oscillation if the aërodrome is moving horizontally), combined with a rotatory oscillation about the centre of gravity. To a first approximation it produces no fluctuations in the velocity in the line of flight, and is unaffected by head resistance or fluctuations in the propeller thrust, provided the latter passes through the centre of gravity of the aërodrome, as has been assumed in many of our calculations. The condition of stability depends only on the areas and positions of the aëroplanes relative to the centre of gravity, and is independent of the inclinations or angles of attack of the planes, the oscillations remaining finite when the planes are parallel. This condition of stability is generally satisfied in any arrangement which satisfies the other conditions of stability. It must not be overlooked, though it is very unlikely to give trouble. The corresponding *trajectory* or *curve of oscillation* is independent of the velocity, the actual *time rates* of oscillation and decay being proportional to the velocity.

In the slow oscillations the variations of velocity in the line of flight are a predominating feature. The trajectory is wave-like, the crests of the waves being more pointed than the troughs, and the descending parts steeper than the ascending ones. This is evidently the type of oscillation studied by Mr. Lanchester. One condition of stability is that the front plane (or planes) must be inclined at a greater angle than the rear ones. The second condition depends on the type of machine.

The terms "monoplane" and "biplane," as usually defined, refer to the question of whether a machine has not or has superposed planes. According, however, to a property which I call the *principle of independence of height,* this distinction does not affect stability to any appreciable extent. The important point is whether the weight is sustained partly by the front and partly by the rear planes, as in certain Voisin machines, or is wholly supported by the front planes, the rear ones acting merely as a tail in the neutral position. For a monoplane with neutral tail the condition of stability takes the form given by Lanchester, when the necessary substitutions have been made by making use of the condition of equilibrium. The reason why Lanchester's method leads to a correct result is to be sought in considerations of the peculiar nature of the oscillations, and in especial in the relative smallness of their modulus of decay. For a machine of the Voisin type, with sustaining surfaces arranged tandem, the condition of stability is nearly as simple, and certain modifications are sufficient to cover the case when the propeller thrust does not pass through the centre of gravity provided that this thrust is constant.

A very convenient plan in such cases is to suppose the actual machine replaced by an *equivalent monoplane,* with neutral tail, although if the inclinations of the planes be varied for vertical steering the equivalent monoplane will be changed.

The most remarkable result, however—and Mr. Harper was the first to point this out to me—is the *important effect on stability of the direction of motion in the vertical plane.* Longitudinal stability falls off rapidly when the aëroplane begins to rise, even if other things are constant. A monoplane would, under theoretical conditions, become unstable when ascending at an angle to the horizon of *less* than twice the angle of attack (or inclination of the main plane to the line of flight).

The effect of head resistance is to increase the stability, and a further increase occurs if the thrust of the propeller, instead of being constant, decreases when the velocity increases. By the use of three planes instead of two, an additional increase of stability can be obtained. On the other hand, if the aëroplane be gliding downwards the longitudinal stability is greater than in horizontal flight.

I think the above conclusions indicate a source of danger which may possibly have led to mishaps when aëroplanes have risen too rapidly in the air.

Captain Ferber's investigations, on the other hand, refer mainly to the stability of a single aëroplane as dependent on fluctuations in the position of the centre of pressure consequent on variations of the angle of attack. He assumes Joessels's formula, introducing two arbitrary constants in place of the numerical coefficients. The difficulty I have several times pointed out is that, if a plane is turning over, its rotational motion may affect the position of the centre of pressure, as well as possibly the resultant thrust, and no experimental information is apparently available on this point. For this reason the use of narrow aëroplanes is to be recommended, stability being secured by a tail or by two planes placed one behind the other. Moreover, the theory of narrow aëroplanes gliding at small angles affords the simplest introduction to a general study of aëroplane stability, just as geometrical optics in which aberration is neglected affords an introduction to a general study of lens construction. It is to be remembered that both the symmetrical and asymmetrical oscillations are determined by equations of the fourth degree, each in the form of a determinant of the third order containing the dynamical constants and resistance coefficients, and when this determinant has been expanded, *four* conditions of stability have to be satisfied, one being that Routh's discriminant $BCD - AD^2 - EB^2$ shall be positive. Fortunately, for purposes of approximation, $CD - EB$ may be substituted for the last in many of the systems occurring in aviation. It will thus be seen that stability is a very complicated problem, and that approximate methods are essential.

Asymmetric stability is far more difficult of investigation than symmetric. It is necessary to take account of the separate effects of straight or horizontal aëroplanes, vertical fins, and bent-up or V-shaped planes. The late Captain Ferber's solution is based on the substitution for the actual planes of their projections on three coordinate planes (p. 46 of his paper). Unfortunately, even assuming the sine law of resistance, this substitution does not seem to give even the correct first approximation which is all the author claims. In particular, if the aërodrome is rotated about any axis in its plane of symmetry, couples are set up on the main aëroplane which have an important effect on the stability, but are apparently not included in his scheme. The final result is a biquadratic with one root equal to zero, and Captain Ferber regards an aëroplane as stable when it describes a helix; whereas such an arrangement should really be regarded as lacking in stability. The couples in question are taken account of by Lanchester, who uses what he calls "aërodynamic and aërodromic radii" to represent their effects. For a narrow aëroplane gliding at a small angle, the effect depends on the moment of inertia of the area of the plane about the vertical plane of symmetry. A horizontal tail of negligible lateral dimensions does not affect the asymmetric stability.

To secure stability, recourse must be had to vertical fins, or to bent-up aëroplanes or aërofoils. The effect of vertical fins (neglecting "wash") depends on their areas, and the first and second moments of these about the axes, and in studying them it is necessary to have recourse to the "principle of parallel axes." The sections in "Lanchester" on "fin resolution" practically embody this principle, but are a little difficult to follow; they suggest the path taken by an explorer who had not a compass to guide him to the mathematically direct road in the form of the principle in question. His conditions of stability seem reasonable deductions from the hypotheses he makes, but the conclusions must not be regarded as final. Both the necessary and the sufficient conditions of stability are really far more complicated, and it is highly improbable that the problem could have been carried much further without the elaborate use of analysis which I have found necessary, and the assistance of an independent calculator, which Mr. Harper has kindly provided. The only way of proceeding was to calculate the coefficients in the biquadratic for particular arrangements of fins and planes, starting with the simpler ones, and passing to more complicated ones *when one has become thoroughly familiar with the different terms and their meanings.*

For an aëroplane with one vertical fin only, the conditions of asymmetric stability require that the centre of pressure of the fin should be slightly in front of the centre of gravity of the machine, while at the same time it should be at a height above the centre of gravity large compared with its distance in front. Two of the conditions of stability are difficult to reconcile with the conditions of equilibrium, the difficulty increasing as the velocity increases and the angle of attack diminishes; moreover, they are inconsistent unless a certain relation holds between the radii of gyration of the machine and of the main supporting surface. It is doubtful whether this condition would be consistent with practical requirements.

The failure of practical aviators to obtain automatic stability may be due in no small measure to the number of conditions that have to be satisfied. A vertical fin in front might satisfy one condition of stability, and introduce instability through another condition, while a similar fin at the back might satisfy the second condition and introduce instability through the first. In either case the impression produced would be that the device secured automatic stability, but that such stability was a hindrance rather than a help, the correct interpretation being that the conditions of stability had not been sufficiently investigated. By abolishing the fins the aviator would obtain a machine with *defective stability, i.e.* with one or more roots of the biquadratic vanishing, and would find it easier to maintain his balance by artificial control than in the previous unstable arrangement.

Of arrangements with two fins, the cases have been considered where both fins are at the level of the centre of gravity, where one is above, and where both are above. The conditions of stability assume various forms, but there is one arrangement which appears to possess an exceptionally wide range of stability, and I have made provisional application for a patent in this connection.

A machine such as the Voisin type, with two planes of considerable span at different angles of attack, is more stable than one with a single sustaining system, and the difference is equivalent to a variation in the arrangement of the fins which is easily calculated.

The asymmetric oscillations of an aërodrome do not separate into two kinds, of long and short period, like the symmetric ones. As a general rule the biquadratic has one root determined approximately by the first two terms representing a quick subsidence, one root determined by the last two representing a slow subsidence, and a pair of roots determined by the middle terms representing a damped oscillation.

The inclination of the flight-path to the horizon has a considerable influence on the asymmetric stability. In several instances we found that instability occurs when an aërodrome is descending at an angle to the horizon the tangent of which is double that of the angle of attack of the main planes. Other arrangements become unstable when rising at more than a certain angle; in the best arrangement referred to above the stability is practically independent of the inclination. As the symmetric and asymmetric oscillations of an aëroplane are independent, it is important that it should preserve its asymmetric stability even when it is not in longitudinal equilibrium. The dependence of stability on inclination affords a very simple and likely explanation of certain "vagaries" described on pp. 342, 343 of Lanchester's "Aërodonetics"; account would, however, have to be taken of accelerations of the centre of mass in an exact comparison of theory with observation.

Bent-up or V-shaped wings lead to much more difficult analysis, and it appears that their effect is not exactly equivalent to any combination of vertical fins except in certain cases. A pair of "stabilisers" or small planes, which may be fixed at the extremities of the main aëroplanes at an angle of, say 45°, is equivalent to a single raised vertical fin if the planes of the stabilisers are parallel to the line of flight.

Mr. Harper has worked out the asymmetric stability of the Antoinette type with a single pair of bent-up supporting surfaces. The conditions of stability are satisfiable by furnishing the machine with a tail of suitable length, or by raising the dihedral angle of the V-shaped wings above the centre of gravity.

I should like to direct attention to the importance of eliminating the personal element in experimental tests of aëroplane stability, by the use of models. The possibility of long-distance flights by skilled aviators having been demonstrated, there is not so much point in repeating these verifications as in extending our knowledge in other directions, and finding how far the element of skill can be dispensed with by effecting improvements in aëroplane design.

The stability of dirigibles opens up another field of study, on which we hope to do something during the coming year.

Owing to the attention now given to aëroplane construction, it appeared desirable to give, in the present article, an advance account of investigations which may not be ready for publication *in extenso* for some time to come. G. H. BRYAN.

Added January 27, 1910.—The *Aëronautical Journal* for January, now to hand, includes a short abstract, illustrated by badly executed diagrams, and containing numerous uncorrected printers' errors, in which the symmetric stability of a single-surfaced aërodrome without tail is made to depend on a cubic instead of a biquadratic equation. This result is obtained by the very doubtful method of "assuming V to be constant for a short period," that is, neglecting fluctuations in horizontal velocity. Owing to this assumption the conclusions reached may perhaps represent the conditions that the machine may be stable with reference to the shorter oscillations, but not with respect to the longer ones, and the inference that a machine can be much more stable at moderate velocities than is generally supposed must not be regarded as conclusive.

THE WORK OF THE WOBURN FRUIT FARM.[1]

AMONG the profusion of agricultural and horticultural reports, many of which can at best be said only to possess a very ephemeral interest, it is pleasant to come across something of permanent and abiding value, work carefully executed and followed to its logical conclusion.

Such must be the feeling of every discerning reader as he studies the report by the Duke of Bedford and Mr. Spencer Pickering on the chemical relationships of the copper fungicides. The problem is one of very great economic importance. Modern conditions of fruit-growing tend to foster and distribute from country to country the fungi parasitic on fruit trees. On the other hand, the grower is more and more anxious to keep them down; not only do they adversely affect his yield, but they often spoil the looks of his fruit, a very serious matter in modern markets.

The most popular fungicides are the copper compounds, more particularly Bordeaux mixture, a basic salt prepared by adding lime to a solution of copper sulphate. This mixture has been used by fruit and potato growers for a number of years with great success, and has formed the subject of a vast number of papers. Unfortunately few of them are of much value, since it only rarely happens that a man is found to combine an interest in horticultural problems with exact habits of thought. Until recently nothing was known of the composition of Bordeaux mixture, not even the proportions in which the constituents should be mixed to give the best results. Certain American investigators recommended 4 lb. of copper sulphate in fifty gallons of water, and some of the English writers, borrowing not too intelligently in this as in other matters, recommended the same strength, oblivious of the fact that the American gallon is little more than four-fifths of the English

[1] Eleventh Report of the Woburn Experimental Fruit Farm, by the Duke of Bedford, K.G., F.R.S., and Spencer U. Pickering, F.R.S. (The Amalgamated Press, Ltd., 1910.)