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VORTICISM IN AERONAUTICS.

By W. H. Sayers.

From "The Aeroplane,"
March 21, 28, April 4, 11, 18 and 25, 1923.

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By W. H. Sayers.

Thanks very largely to the very painstaking work of a group of German scientists since the outbreak of war there has been during the past few years a very considerable advance in knowledge on the subject of the airfoil and its behavior.

This advance in knowledge is undoubtedly of the very first importance - it is the beginning of a rational explanation of what had before been a very puzzling set of phenomena, and although the theory of airfoils is admittedly far from complete, it is already in a state sufficiently advanced to make it possible to predict and to calculate certain results which previously could only be attained by direct experiment.

The Practical Value of Theory.

Not only has this new knowledge produced this result, but it has given a means of analyzing experimental results in order to separate - at least to some extent - the influence of particular experimental conditions from the more general results which the experiments are intended to demonstrate.

For instance, when models are tested in a wind-channel there is a disturbing influence caused by the proximity of the channel walls.

* Taken from "The Aeroplane," March 21, 28, April 4, 11, 18 and 25, 1923.

This influence depends on the type of channel used and on the relative size of the model and the channel itself.

In many cases the result of this particular type of interference is unimportant. In some cases, particularly where the model is large and the tunnel small it may be extremely important.

Take, for instance, the effect of aspect ratio on the properties of an airfoil. To discover this by direct test in the wind-channel tests would be made on a series of models of the same chord but of increasing span, generally in the same wind-channel. As the span of the model increases so does the importance of the channel wall effect and tests so made do not show the effect of the change of aspect ratio only - but that of aspect ratio plus the interference effect. And as a result tests of this nature have been of very limited value - and in some cases distinctly misleading.

Aspect Ratio and Interference.

Theory, as now developed, allows the calculation of the result of aspect ratio change and of wind-channel wall interference separately. The calculations agree very closely indeed with the model tests and one may now safely conclude that the effect of a change of aspect ratio on the qualities of any wing section can be calculated accurately from the results of tests on a single model of that section.

The mutual interference of two or more wings can also be calculated by means of the modern theory of airfoils. It is not so far established that the theory is in as complete agreement with experimentally determined facts as it is in regard to aspect ratio.

But it is in very fair agreement and it gives order and sequence to a large number of experimental results. As a result it is now certainly possible to estimate with very fair accuracy from a test on a single monoplane model the qualities of biplanes and multiplanes of any gap/chord ratio stagger, or decalage. It is possible to estimate the effect of a combination of wings of different spans, chords, angles of incidence or even different sections from tests on one monoplane model of any aspect ratio of each section involved.*

Influence on Design and Research.

This work has a twofold value. It greatly reduces the amount of direct experimental work needed in the wind-tunnel for design purposes. The designer of a biplane or a triplane need no longer wait for tests of model biplanes or triplanes - he can go ahead with the results on monoplane tests with a fair degree of confidence - for the deficiencies of the theory in its present state do not in all probability lead to errors as serious as those which may be involved in the translation of model results to full-scale estimates.

And, in the second place, it gives some sort of direction to pure research work. Experimental results begin to fall into some sort of order and the problems to be solved assume relatively definite aspects. This should and will make for much more rapid pro-

* General Biplane Theory. By Max M. Munk. Report No. 151, U.S. Advisory Committee for Aeronautics, Washington, 1922.

gress towards a really thorough understanding of the very complicated phenomena involved in aerodynamics than was in any way possible when experiments were merely shots in the dark, and results were purely records of unconnected observations.

To attempt to give any sort of a complete account of the theory of the airfoil in its present state in this paper is quite impossible - even if the present writer felt competent to undertake the task, which he does not. The most which it is possible to do here is to give a very brief outline of the history of the subject, to state roughly the physical assumptions on which the theory is founded and to draw attention in the most general way to some of the results to which it has already led.

Dangers of the Undertaking.

From many points of view this is a most dangerous undertaking. The subject is a most complex one. The new theory of airfoils has been developed in mathematical form, and the mathematics involved are of a formidable complexity.

Some competent authorities - particularly in this country - raise what are claimed to be serious objections to the theory on mathematical and physical grounds, and these will doubtless hold that the present effort is a case of "rushing in where angels fear to tread."

But as these same authorities admit themselves that the theory does give results of surprising accuracy for which they cannot account, one may feel emboldened to risk their contumely - if thereby an equal but opposite velocity to that with which it strikes the body.

If, on the other hand, either body, or particles, are perfectly inelastic, there will be no bouncing. In the first case the resistance will be twice as great as in the second case.

The Newtonian theory leads to a resistance varying as the square of the speed - which is fairly close to the truth - but even for the perfectly inelastic condition it gives values for resistance very considerably greater than those found by experiment.

The failure of this theory is due not to any defects in the theory itself, but to the assumption that a fluid might be represented as a collection of separated particles.

Modern Hydrodynamics.

The science of hydrodynamics - based on the works of Euler, Lagrange, Stokes and other famous mathematicians - attempted to elucidate the problems of motion in a medium very different from that of the granular fluid of Newton. The fluid of hydrodynamic theory is of absolutely uniform consistency, and if in some respects this "fluid" more truly represents the fluids which exist in nature than Newton's granular fluid there are certainly other respects in which it is an equally artificial hypothesis.

Real fluids are certainly not of absolutely uniform consistency - there is some degree of "granularity" present - but equally they are not composed of the free and disconnected particles assumed in the Newtonian medium. That is to say, that a particle of a free fluid does not bounce off a body without disturbing other fluid particles in its vicinity.

The essential property of a fluid - its "fluidity" - is that it is incapable of sustaining shearing stresses - or in other words that it will bear distortion without developing any resistance to that distortion.

Ideal and Real Fluids.

If this quality is complete the fluid possesses no viscosity. Real fluids are all imperfect fluids - they do possess viscosity and they do resist distortion, and the fluid of hydrodynamics does not accurately represent any real fluid on that account.

Mathematical research into the motions of the perfect non-viscous fluid lead to certain fairly definite results. It was possible by certain ingenious devices to calculate the form of the flow round bodies of almost any shape moving in it, and from the form of the flow to calculate the fluid pressure which would be developed at any point on such a body.

In many cases the form of flow calculated from the theory was very closely similar to that actually observed in real fluids with similar bodies. Experimental measurements of the real pressures developed gave a very close agreement with the calculated ones to within certain limits.

In Fig. 1 the dotted line shows the calculated pressure on the surface of an airship model of the shape sketched below the curve.

The full line shows pressures actually measured. There is a striking similarity between the two curves, except at the tail of the model. Unfortunately this apparently small difference is of the very greatest importance.

All pressures on the model ahead of the maximum diameter where the surface is inclined outwards from the nose have a component directed backwards on the model resisting its motion. All those behind the maximum diameter, where the surface slopes in the opposite direction have a component in the opposite direction.

In the theoretical case all these components parallel to the length of the airship balance out and there is no resistance at all.

As the pressures at the tail in the real fluid are considerably less than those calculated for the ideal fluid, it is obvious that in this case the fore and aft pressure components do not balance out - and there is a definite resistance in the case of the real fluid.

The Breakdown of Theory.

This is a typical case. In general in the ideal fluid of hydrodynamics there is no such thing as resistance. In every real case there is a resistance.

Some part of this resistance can at once be traced to the viscosity of the real fluids. The fluid does not assume its full velocity immediately outside the boundary of the body, but there is a thin layer in which the velocity changes from that of the body to that of the stream. In this layer slipping occurs, accompanied by resistance analogous to frictional resistance.

But the force caused in this way is strictly parallel to the surface of the body and has no component at right angles. It does not therefore alter the pressure at right angles to the body and over and above this skin friction resistance there is still the pressure above mentioned.

By a fundamental theorem of hydrodynamics, in fact as a direct result of the principle of the conservation of energy, changes of pressure of the type here encountered can only be accounted for by changes in the velocity of the fluid stream. A region of high pressure is a region of reduced velocity - and vice versa.

Obviously the difference between the non-viscous fluid of theory and the real fluid is that the velocity of the flow in the real fluid is not that which would occur in the theoretical fluid. But up to the present it has not been possible to calculate the nature of this change which occurs as a result of the imperfect fluidity of real fluids.

When one comes to the case of surfaces exposed otherwise than edge-on to the flow of a fluid, it is a matter of common knowledge that a pressure is developed at right angles to the surface. It is such a pressure which accounts for the lift of an airfoil.

The theoretical form of flow past a flat plate at right angles to the stream in the non-viscous fluid of theory is something like that shown in Fig. 2. The streamlines are symmetrical on both sides, and there is a negative pressure on the back of the plate precisely equal and opposite to the positive pressure on the front. The total force is zero. This is again typical. Accordingly there can be by this theory no such thing as the lift of an airfoil.

A Modified Theory.

As long ago as 1847, Stokes realized that some effort should be made to remedy the utter failure of hydrodynamics to account for the observed facts of fluid resistances. He suggested that flow might be discontinuous, and his suggestion was taken up and investigated by Helmholtz and others.

The theory of discontinuity assumed that the streamlines past a flat plate might take a form such as is shown in Fig. 3, and instead of closing in at the back of the plate might be separated by a relatively large piece of fluid as a whole stationary relative to the plate, though necessarily in some state of turbulence. This theory accounts for the development of pressures such as are known to exist. It was attacked by certain very high authorities on theoretical grounds, but it is certain that a flow very much of the supposed type does actually occur in nature - and is extremely marked in the case of bodies of bad shape - that is to say, of high resistance.

But the theory entirely fails to account for the forces actually measured on inclined planes and on wing sections at ordinary angles of incidence. The lift forces measured experimentally are very much greater than can be accounted for by the theory of discontinuity.

Also the form of flow actually observed in these cases does not resemble that which would follow from the theory.

Thus it is fair to say that - up to this stage at any rate - theory had entirely failed to account for either the resistance or the lift of airfoils. Within certain limits it was possible to account for resistance, both of airfoils and of other bodies, as a direct result of friction due to the viscosity of the real fluid, but the measured total forces on many bodies greatly exceeded anything traceable to that cause.

The credit for having been the first to point a way out of this impasse - so far as the behavior of airfoils is concerned - undoubtedly belongs to Mr. F. W. Lanchester, who in 1908 published the foundations of the vortex theory of airfoils in a remarkably advanced form in the first volume of "Aerial Flight." His work in this direction actually dates from 1894, and in the main was presented to the Physical Society of London in a paper in 1897. This paper was rejected, and in this country at any rate his work has been treated with a totally undeserved contempt by the leading aerodynamical authorities.

In Germany, on the other hand, it has been recognized as of the utmost importance and it is only fair to him to say that the modern theory of airfoils is Lanchester's theory in all essentials, and is so acknowledged in Germany.

It has been very greatly amplified, and an immense amount of extremely brilliant work has been necessary to bring it to its present state. Professors Prandtl, Betz and Munk - to mention only a few - deserve every credit for having rescued Lanchester's work from the oblivion to which the British aerodynamicists were willing to consign it and for having developed it into a useful and useable form, but they have no claim to be in any way originators thereof.

The "Vortex" theory of the airfoil may be approached in sever- ways. Starting from the known fact that there is a difference in pressure on the two sides of an airfoil, the theorem previously mentioned which connects pressure differences with velocity differences is sufficient to show that there is a difference in the velocity of the air on the top and on the bottom of an airfoil. In Fig. 4, if AB represents a portion of an airfoil, it is found that on the lower side there is an increase of pressure acting upwards and on the top side a decrease of pressure, also acting upwards.

The equation of Bernouilli - the statement of the theorem in question - at once leads to the conclusion that the velocity of the air passing the upper surface is greater than that passing the lower surface.

This may be represented as indicated in Fig. 5. Here the direction of the original motion of the airstream into which an airfoil is introduced is from left to right. The airfoil in some manner or another sets up local velocities shown by the small arrows above and below which oppose the stream velocity below, and assist it above, the airfoil itself. From this supposition to that of assuming a circulatory or cyclic flow right round the airfoil, as shown in the right part of the figure, is an obvious step.

The combination of such a circulation round the wing and of a velocity of translation, either of the wing itself through the fluid or of the fluid past the wing, at once gives rise to a force at right angles to the direction of the motion - in other words, to a lift force.

In the ideal fluid of no viscosity such a circulation round an airfoil would not be set up, but if it could be supposed to be set up it would continue to exist, and would produce a lift.

This lift would be proportional to the product of (1) strength of the circulation, (2) the velocity of the undisturbed airstream, and (3) to the span of the airfoil.

Just as there is no reason why such a circulation should be set up, there is no reason why, if it did come into existence, the circulation should be of any particular strength, and therefore there is no limit to the lift which might result. As a matter of fact, if it were supposed that an airfoil is in flight in the ideal fluid, carrying no load, and if one were suddenly to apply a load, then a circulation would be set up sufficient to cause a reaction equal and opposite to the impulse applied to the airfoil. The greater this impulse, the greater the lift.

In the case of an airfoil working in a real fluid - that is, one with viscosity - there are, however, definite reasons why a circulation should be set up, and why the strength of the circulation should be definite under given circumstances.

Fig. 6 shows the theoretical streamlines for a perfect fluid round a body of airfoil section. At the point A it will be noticed that the streamline leaving the trailing edge is sharply kinked, and is even vertical at one place. This means that there is a very high - in fact, an infinite - velocity in this neighborhood.

There is no objection to this in the case of the ideal fluid - in the case of a fluid having any viscosity an infinite velocity is impossible and conditions which tend to produce very high velocities lead actually to the production of vortices.

In order that a steady, stable form of flow may be produced, it is necessary that no sudden kinks such as that at A should occur in the streamlines, but that the streamlines should flow smoothly round the airfoil, those from above and below the surface should unite in nearly parallel directions at or about the trailing edge.

If a circulatory flow such as that of Fig. 7 be superposed on the flow of Fig. 6, the downward velocity of the circulation at the neighborhood of the trailing edge will oppose the upward velocity of the streamlines at A in Fig. 6. If the circulatory flow is of the right strength it will produce a flow such as that of Fig. 8, where the above wing and below wing-streams unite smoothly with no violent changes of velocity.

Steady conditions can then be established, and only then. The strength of the circulation necessary is thus defined as that needed to neutralize the infinite velocity at A.

It can be shown that the strength of circulation necessary for these conditions increases with increased angle of incidence, and thus that the lift will also increase with increasing angle.

The precise mechanism by which a vortex produced by the conditions at A leads to the establishment of a circulation round the whole wing cannot be simply explained. There are, however, well-established theorems of hydrodynamics which prove that the total circulation in any complete fluid system can neither be increased nor diminished.

If a vortex - which involves circulation - is formed at A, that vortex and its circulation will pass off downstream, and in order that this theorem of the conservation of circulation may be satisfied, an equal and opposite circulation must be set up elsewhere in the system, and in fact would be set up round the wing.

In the ideal fluid of theory, a circulatory flow such as that postulated round the wing can have no sudden termination at the end of the wings. It must either form a closed circuit vortex ring such as the well-known smoke rings, or what are known as vortex filaments must extend from the wing-tips to infinity.

Quite independently of the hypothetical qualities of the fluid of theory it can be shown that the circulation round a real wing cannot be considered as cut off short at the wing-tips.

In Fig. 9, the top view shows a wing-producing lift in end elevation. The increased pressure below, and the decreased pressure above, must obviously tend to cause a flow of air round the wing-tips as shown by the curved arrows. The actual effect when this tendency is superimposed on the stream velocity is to cause the streamlines below the wing to be diverted outwards as they cross the wings, while the streamlines on top are diverted inwards. This is shown in Fig. 9, bottom - where dotted lines represent streamlines below and full lines those above the wings. The divergence from the original direction of flow will be zero at the center line and will increase as the tip is approached.

At A it will be seen that a top and a bottom streamline cross each other. The result, whether one is dealing with the ideal fluid of theory, or the real fluid of fact, is that these two streamlines will roll round one another - much as the strands of a rope - and will form a vortex filament. Thus all along the trailing edge of the airfoil a succession of such vortex filaments is formed, those to one side of the center-line being of opposite rotation to those on the other side of the center-line.

Vortex filaments of the same hand are attracted together and tend to wrap round each other - and as a consequence the "sheet" of vortex filaments on each side of the wing finally roll themselves up into a sort of rope, and form a single large vortex filament trailing backwards from the neighborhood of each wing-tip. Fig. 10, taken from Lanchester's "Aerodynamics," indicates the nature of this rolling up into one vortex system.

The flow round the wing-tips of Fig. 9 (top) is obviously a source of a loss of lift. In a similar manner each of the vortex filaments which leaves the trailing edge of the wing may be regarded as a loss of circulation round the wing - that is, each vortex filament carries away some of the circulation which was present round the wing between the center of the span and the point at which the filament leaves the wing.

As the lift is dependent on the strength of circulation the distribution of these vortex filaments along the span governs the distribution of lift along the span.

In the case of any real wing the flow is of the nature indicated in Fig. 9. That is to say, that except at the center there is movement sideways as well as fore and aft and up and down, and it is impossible to deal with the flow and its effects without taking account of these lateral divagations.

If the wing is supposed to be of infinite span, however, no lateral disturbance of flow occurs, for the whole of this disturbance is due to the sudden change of conditions at the wing-tip - which with infinite span is infinitely far away and therefore produces no effect.

It should here be pointed out that in this theory viscosity is only called into account so far as it is necessary to account for the production of circulation and to account for a definite value of the circulation under given conditions. No definite value has to be assigned to viscosity for this purpose - and for the sake of simplicity it is thereafter neglected. Having used viscosity to account for circulation it is assumed to vanish for all other purposes.

In the case of the wing of infinite aspect ratio the streamlines round a symmetrical airfoil are symmetrical fore and aft. Their form is shown in Fig. 12. It will be seen that there is an upward velocity ahead of the wing and a downward one behind the wing both of equal magnitude, and that the stream after passing the airfoil is moving in exactly the original direction. There is a force at right angles to the wing - a lift - and there is no drag or resistance.

It is a little difficult to understand exactly how it happens that the air is disturbed and deflected upwards before it reaches the airfoil, but it may be to some extent explained by reference to Fig. 11, which represents a section of a plate subjected to a vertical impulse in the direction shown by the large arrow. The plate is accelerated downwards. There will be a flow round the edges more or less as shown by the curved arrows. If on this flow a horizontal flow is superposed then the upflow ahead of the wing is explained. This upflow meets the wing, and is deflected downwards by it.

Thereafter it meets the upward flow at the rear edge which neutralizes the downward deflection, leaving the final flow horizontal.

The wing of infinite span does not exist in practice, but it is to be observed that the infinite wing is only a mathematical concept used to investigate what will occur when there is no lateral disturbance of the flow over the wing - that is to say, when all the streamlines in plan-view remain parallel to the original direction of the airstream, or of the line of flight.

As a matter of fact, it is possible to approach very closely indeed in practice to this simplified type of flow of the infinite wing by testing an airfoil in the wind-channel between vertical partitions touching the end of the wing and extending upwards to the limit of the moving airstream.

It is not possible to set such partitions in actual contact with the wing-tips, as the friction between wing and partition will make it impossible accurately to measure the forces on the wing, but it is possible to devise forms of "labyrinth" packing which reduce leakage round the wing-tip to a very small amount, and which still leaves the wing absolutely free, within narrow limits.

A wing under these conditions simulates very closely the conditions which occur in the infinite wing of theory.

Experimental Checks on the Theory.

As long ago as 1914, Dr. A. Betz, of Göttingen, calculated the lift coefficient at various angles of incidence for an infinite wing of what is known as the Joukowski type, and tested a model of such a wing in the wind-tunnel under conditions such as those outlined above. The results of this experiment are shown by the curve of Fig. 13. The dotted line is the calculated lift coefficient, the full line nearly parallel thereto is the experimental lift coefficient. The full line which is roughly horizontal with a turn-up at each end is the experimental drag coefficient. The theoretical drag is everywhere zero.

It is obvious that the real lift is everywhere less than the theoretical, and that while the theoretical lift continually increases as the angle of incidence increases the real lift ceases to increase and falls off at an angle of about 10°. At the same time the difference is not very great until fairly close to the critical angle of 10°.

The real drag is everywhere of appreciable magnitude instead of being zero, but over the range from $-7\frac{1}{2}^\circ$ to $+5^\circ$ it is sensibly constant, and it is in fact of about the value which would be accounted for by the known skin-friction drag of a plane of the same area as the wing.

This agreement between theory and experiment is far closer than any which had before been found. The discrepancies are of the kind which would be expected to result from the effects of viscosity which are regarded as negligible in the theory, and the only really disconcerting feature in the failure of theory to give any indication of the sudden falling off in lift at the critical angle of the real airfoil.

It may here be remarked that up to the present this phenomenon has not been satisfactorily explained. It may be said, however, that it is pretty certainly a viscosity effect, and that as will be shown later this deficiency need not be regarded as a defect of the vortex theory, but as simply a result of the fact that the theory is not yet complete.

Even more important than the generally satisfactory result of comparing calculated with theoretical lift was the result of the calculation of pressure distribution and center of pressure of the same wing and a comparison with the measured pressures. The calculated pressures are everywhere higher than the observed ones - as follows from the fact that the calculated lift is greater than the observed lift, but the general similarity of the calculated and the observed curves of Fig. 14 is striking. Here again observed pressures are in full lines and calculated ones are in dash lines.

The wing of infinite span - or infinite aspect ratio - and its wind-tunnel equivalent are of scientific importance only, as representing a simplified case for calculation, and as a base on which the general theory of the real wing can be constructed.

The results mentioned above indicate that there is a general correspondence between theory and practice. In the light of these results it becomes a reasonable working hypothesis to assume that the lift of an airfoil is in fact to be accounted for on the assumption that there is a circulatory flow round the wing.

This circulation in the real wing is always less than the theoretical circulation, which is what might reasonably be expected from the existence of considerable viscosity in the real fluid as against the negligible viscosity assumed for theoretical purposes.

Even for the wing of infinite aspect ratio therefore it is not possible to calculate what will be the actual circulation, or the actual lift.

But as a matter of fact, the lift can fairly easily be measured, and the real circulation can be calculated from the lift - since the two are directly proportional one to another.

It is to this fact that the theory owes its real value, for as will hereafter be indicated, deductions as to certain important effects of circulation and of the vortices which result therefrom depend on the value of the circulation and on the strength of the vortices only, and so long as a real value can be assigned to the circulation, the fact that this value differs from the theoretical value is unimportant.

Put in another way, the incomplete theory says that at a given incidence a particular airfoil ought to have a certain lift coefficient. In practice, it develops that lift coefficient at a greater angle of incidence. Nevertheless, the real wing, in certain respects, behaves very much as theory indicates it should behave at that lift-coefficient.

The Monoplane of Finite Span.

Passing from the wing of infinite span to the practical case of a wing which has a limited span, it can be shown that a drag is set up which is due to the disturbing effect of the trailing vortices.

For the sake of simplicity, the fact that a wing has chord as well as span, is neglected and it is assumed that a wing may be represented as a lifting line round which there is a circulation of such strength as to provide the required lift. This simplification in fact leads to certain errors, but they are unimportant for wings of ordinary aspect ratio - in fact, for aspect ratios as low as 3, experiment shows that a real wing behaves as an approximation to the lifting line sufficiently close for most practical purposes.

A wing of finite span together with its system of trailing vortices may be represented in its simplest form by the diagram of Fig. 15, where AB is the lifting line with its circulation and the two trailing vortices are indicated by the parallel lines.

This system viewed from in front is represented by Fig. 16, which shows roughly the motion in the vortices. It is obvious that everywhere between the cores of the two vortices there is a downward velocity and everywhere outside an upward one.

There are definite laws governing the distribution of velocity round the core, or center of a vortex filament. The velocity is everywhere inversely proportional to the radius from the center, and to the "strength" of the vortex - or the circulation is proportional to the product of velocity by radius - or more strictly - by circumference. It can be seen that the strength is constant and does not vary with the radius at which it is measured.

In the simplified case here considered it will be seen that the downward velocity anywhere between the two vortices can very simply be calculated.

The strength of the vortex is the strength of the circulation round the wing, which is a measure of the lift. From the distance of any point from the core of a vortex the velocity due to that vortex for any value of lift is calculable, and the total velocity is the sum of the velocities due to both vortices.

This downward velocity extends right up to the hypothetical lifting line, and it modifies the whole flow over the line by tilting the flow of the whole stream so that it has a downward velocity at the lifting line itself.

If now a wing of very small chord is substituted for the lifting line of hypothesis, the result is as shown in Fig. 17. A, top, represents the flow round the infinite wing where there is no down velocity due to vortices, because the vortices are removed to an infinite distance. B represents the finite wing, which is producing the same lift per unit of span as the infinite wing A. The whole airflow system has been tilted through the angle α by the effect of the vortex velocities.

This would reduce the incidence of the wing and therefore the lift, and to restore the incidence and lift to the original values the wing itself must also be tilted through the angle α.

The lift force caused by the circulation is always at right angles to the general stream velocity at the wing. In the case A this force is therefore vertical and has no horizontal drag component. In case B of the finite wing, this force is also tilted back through the angle α, and it has therefore a horizontal component D, which is a drag, and a vertical component L_1 which is the available lift.

In practice this angle of tilt α is always a small angle and in consequence for all practical purposes L_1 may be taken as equal to L.

The drag which results from this tilting of the system is called the "induced drag" to differentiate it from such drag forces as result from viscosity.

Thus although the infinite span wing in a non-viscous fluid has no drag, any wing of finite span has a definite drag which has nothing whatever to do with the viscosity of real fluids - but must be additional to any resistance caused by friction in the fluid itself.

Also it is obvious from the argument as set out that this drag is greater the greater the strength of the circulation round the wings - that is, the greater the lift per unit of span - and is also greater the smaller the distance between the cores of the trailing vortices - that is, the smaller the span.

The system of vortices of Fig. 15, however, does not represent any real case. The assumption of a single vortex running from each wing-tip involves the assumption that all the circulation is carried off at the tip and therefore that the circulation and consequently the lift is uniform along the span.

Actually the lift varies continuously, from the center of the span to the tips, reaching zero at the tips themselves, and in consequence a sheet of vortices springs from the trailing edge over the whole span. This sheet as already described (Fig. 10) rolls up into a pair of main vortices behind the wing - but at the wing itself the velocity of downwash is that caused by the sheet of vortices.

It is not possible to calculate what is the resulting angle of downwash at the wing unless the exact distribution of vortices along the span is known. A good deal of trouble was expended in the effort to find an expression for this distribution of vortices which could be regarded as both possible, probable and convenient for calculation purposes.

The vortex distribution along the span governs the distribution of circulation round the wing along the span, and therefore the distribution of lift along that span.

Finally, it was found that a distribution of the lift such that the lift was everywhere proportional to the ordinates of a semi-ellipse with its major axis equal to the span gave satisfactory results.

The semi-elliptical distribution is not unlike the distribution found on real wings. It gives a downward velocity which is constant over the whole span, so that its effect on a wing of defined span is equivalent to tilting the whole wing through a fixed angle. A variable downwash would involve warping the wing to reproduce similar conditions for varying mean strengths of downwash.

This condition of uniform downwash is also the condition of minimum "induced drag" for any given set of conditions, and it follows from certain well-known laws that a fairly large change of the lift distribution from the assumed elliptical distribution will not very greatly change the resulting "induced drag."

The assumption of elliptical distribution of lift leads to quite simple relations between lift and induced drag.

The "induced drag" is proportional to the square of the lift, is inversely proportional to the square of the stream velocity and also inversely to the square of the span.

This result does not involve the chord of the wing at all - and it shows that the "induced drags" of any two wings which are of the same span, and which carry the same total load at the same speed are exactly equal. In this form the result has little direct practical value, but by a very simple transformation the original relation between "induced drag", lift, speed and span can be transformed into one between induced drag and lift coefficients and aspect ratio, speed disappearing altogether.

It is then found that the coefficient of induced drag is proportional to the square of lift coefficient, and is inversely as the aspect ratio. From this relation it is simple to calculate the change of "induced drag" caused by any change of aspect ratio.

Also as the induced drag is the result of tilting back of the whole airflow and airfoil system, the fact that one can calculate induced drag means that one can calculate the angle of tilt appropriate to any aspect ratio for any given lift coefficient.

It should be quite clear that on this theory a change of aspect ratio not only alters the "induced drag" for each value of lift coefficient but also alters the apparent angle of incidence at which each lift coefficient is developed by any particular wing section.

usual British coefficients.

The curve which touches the extreme left margin of the figure at O is the theoretical induced drag. The curve farther to the right marked by small circles is the measured curve for the real wing. It will be noticed that over a considerable range the two curves are practically parallel, the divergence becoming serious only in the neighborhood of no lift, and when the real wing is at about 12° incidence - approaching the stalling angle.

This process has been repeated for wings of different aspect ratios. A set of curves showing the lift and drag of a series of wings of aspect ratios varying from 1 to 7 are shown in Fig. 19.

By making the assumption that change of aspect ratio will not alter that part of the drag which is caused by viscous friction, and that only the induced drag was altered, the lift and drag figures for an airfoil of aspect ratio 5 was deduced by the theory from each of the separate curves of Fig. 19.

The result is shown on Fig. 20, where it is obvious that the points deduced from all the curves of Fig. 19 - with one or two exceptions in the case of the wings of aspect ratios 1 to 3 - lie very closely indeed on the same curve, which is to all intents and purposes that experimentally found for the aspect ratio 5.

A similar check has been applied to the theory of the change in angle of incidence caused by change of aspect ratio, using the same set of models. When the lift is plotted against angle of incidence seven distinct curves are obtained.

By deducing from each wing from the theory what the angle of incidence of a wing of aspect ratio 5 should be for each measured lift, one single curve corresponding to the observed curve of aspect ratio 5 was obtained.

These two checks on the theory are sufficient to prove that for practical purposes one may assume that the drag of a real wing can be regarded as composed of two parts, one the "induced drag" of theory, the other a drag due to viscous forces.

The induced drag varies with aspect ratio and this variation can be calculated.

The viscous drag does not vary with aspect ratio but is always the same for the same section of wing.

Therefore a test at one aspect ratio is sufficient to determine the behavior of a wing of the same section at any aspect ratio greater than about 3.

The Calculation of the Effect of Change of Aspect Ratio.

The majority of model tests on wings which are available for design purposes are made at one standard aspect ratio - usually 6 in this country. The designer has to estimate the effect of any change from this value which may be necessary in a particular case.

By means of the expression derived from the vortex theory by Prandtl and others, the effect of any such change can be calculated very simply, and therefore it is thought that it may be worth while to give the necessary expressions here.

The "induced" drag coefficient K_{Di} is given by the following expression:-

$$K_{Di} = \frac{2K_L^2}{\pi} \; \frac{1}{n} \qquad (1)$$

Where K_L is the lift coefficient and n is the aspect ratio.

Given the result of tests on a wing of known aspect ratio the general problem is to find the drag of a wing of the same section, but different aspect ratio.

Let n_1 be the aspect ratio of the tested wing,

n_2 be the aspect ratio of the new wing,

K_{D_1} be the drag coefficient of tested wing,

K_{D_2} be the drag coefficient of the new wing.

Then

$$K_{D_2} = K_{D_1} + \frac{2K_L^2}{\pi} \left(\frac{1}{n_2} - \frac{1}{n_1}\right) \qquad (2)$$

This relation is true for the whole drag coefficient because there is no change in the viscous or frictional drag caused by change of aspect ratio.

The change of angle of incidence for a given lift coefficient which results from change of aspect ratio is equally simply calculated. Calling the angle of tilt of the airstream system α

$$\alpha = \frac{2K_L}{\pi} \; \frac{1}{n} \qquad (3)$$

Given the incidence α_1 for any lift coefficient K_L of the wing of aspect ratio n_1 to calculate the angle of incidence α_2 of the wing of aspect ratio n_2 the expression

$$\alpha_2 = \alpha_1 + \frac{2K_L}{\pi} \left(\frac{1}{n_2} - \frac{1}{n_1}\right) \qquad (4)$$

may be used.

α is here expressed in radians, and must be multiplied by 57.3 to give the angle in degrees.

The lift and drag coefficients in the above expressions are the ordinary British absolute units.

For wings which are not rectangular in plan the aspect ratio should be taken as span divided by mean chord, or $S \div A^2$ where A is the area of the wing and S the span. The results will be found to apply with quite good accuracy to wings of tapered plan, provided the taper is not exaggerated. In the case of a wing tapering to a point, each half of which was a triangle, having a mean aspect ratio of 9.9, the induced drag given by the expressions above is too low by about 12 per cent, but for wings of elliptical, trapezoidal and round-tipped types the results obtained by formulas (2) and (4) are of sufficient accuracy for all practical purposes.

The Biplane.

Very similar methods to those outlined above for the determination of the effect of aspect ratio have been applied to a number of other problems. In the case of the monoplane, the vortex system trailing from the wing-tips disturbs the airstream at the wing itself. If a second wing is introduced into the same airstream, disturbances caused by the vortices of the first wing react on the second, and the vortices of the second wing similarly react on the first.

Fig. 21 is identical with Fig. 16, except that a second lifting line, or wing CD, parallel to AB is introduced below AB. Obviously CD is under the influence of a downward current due to the vortices of AB as well as that due to its own system of vortices. In precisely the same way the vortices caused by CD increase the downwash at AB.

In the biplane case there is another effect due to the circulation round the wings. In Fig. 22 the two wings of the biplane A and B are represented diagrammatically as lifting lines together with the circulation round each. It is obvious that the circulation round the lower wing increases the stream velocity above it - that is, round the upper wing - while the circulation round the upper wing decreases the stream velocity at the lower wing.

Fig. 23 illustrates the case of a staggered biplane. Here it will be seen that in addition to the effects already mentioned the circulation round B creates an upward component of velocity at A and so adds to its incidence and therefore, lift, as well as increasing the stream velocity.

The circulation round A on the other hand produces a downward velocity at B and decreases the lift.

If the stagger is reversed it can easily be seen that the upper wing is in a downwash due to the lower wing, and the lower wing works in an upwash due to the upper wing.

It is obvious that with variations in the gap and the stagger the general problem of biplane interference is somewhat complicated, but the general effect is fairly clear.

The primary effect of combining two wings to form a biplane is that due to the downwash caused by the trailing vortices, and the effect of this is to increase the angle of incidence necessary to produce on each wing the same lift as would be produced by each wing alone. This increased incidence leads as already explained to an increase in the induced drag of each wing.

The secondary effects, due to the influence of the circulation round the wing are the increase of stream velocity at the upper wing and the decrease of the velocity at the lower wing causing an increased lift on the top and a decreased lift on the bottom wing.

There is also the tertiary effect of stagger, which is to increase the lift of the forward wing, and to decrease that of the lower wing as a result of the vertical components of velocity due to circulation.

Qualitatively all these effects are known to occur experimentally, but accurate calculation of biplane effects from the theory for practical cases is extremely complex.

By making certain assumptions of a simplifying nature, however, it is possible to arrive at results of a quite useful nature.

The minimum induced drag of a biplane - or of a multiplane for any given span - is obtained when all the wings are of the maximum span, and all carry the same load. It is also necessary that as in the monoplane the distribution of lift should be such as to produce a uniform downwash all along the span. In order that both wings - or all wings in the multiplane - shall carry equal loads there must be a change of incidence of the lower wing relatively to the upper to compensate for the difference of lift caused by the secondary and tertiary interferences already mentioned.

With equal lift on the two wings, the increased drag on the top wing due to interference from the bottom wing is equal to the increased drag on the bottom wing due to the interference of the upper wing.

Under these conditions the biplane becomes the equivalent of a monoplane of greater span, but carrying the same total load, and the induced drag of the biplane can be calculated as that of the monoplane of this equivalent span.

The biplane, under these conditions has a less "induced" drag than has a monoplane of the same span. This sounds at first rather like a contradiction of the known higher efficiency of the monoplane, but if it is remembered that to produce the same lift at the same speed, the area of both monoplane and biplane would have to be approximately equal, and that therefore on the same span the monoplane would have only about one-half the aspect ratio of each wing of the biplane, it will be seen that there is no contradiction.

The assumed condition of equal lift over all wings is not realized - and not realizable - in practice. Neither is the best distribution of lift. Nevertheless it is held that relatively large changes in these conditions make comparatively small changes in the total induced drag.

That being the case the effects of biplane interference can be regarded as equivalent to those caused by variation in the aspect ratio of a monoplane carrying the same load.

That is to say, a biplane of a real aspect ratio n is regarded as replaced by a monoplane of aspect ratio Kn, where K is a constant for each particular biplane arrangement. When the gap is zero, K is 1 - that is, the biplane becomes a monoplane. When the gap becomes very great relatively to the span $K = \sqrt{2}$.

The value of K depends only on the ratio of gap to span, and for practical limits of gap it does not vary very greatly from 1.1. The theoretical values of K for varying gap-span ratios have been calculated and compared to those obtained from model tests. Owing to the fact that a real biplane does not conform to the theoretical best distribution of lift, the real value of K is always less than the theoretical, but the difference is only of the order of 5 per cent for the range used in actual practice.

The problem of biplane corrections has been worked out, and the results tabulated in a form whereby the characteristics of any biplane may be estimated from the test figures of model monoplanes by Dr. Max Munk in one of the reports published by the American Advisory Committee.*

This method is that outlined above of assigning an equivalent aspect ratio to a biplane combination. This gives the corrected drag and the corrected angle of incidence of the complete biplane, but does not give the changed distribution of lift as between upper and lower wings.

Dr. Munk in the same report gives a method of computing the center of pressure of the biplane from the known C.P. of the monoplane.

* General Biplane Theory. American Advisory Committee for Aeronautics, Report No.151.

These results have not received the same degree of accurate confirmation by experiment as have the results of the calculation of aspect ratio, but they are pretty certainly in reasonably close agreement with the facts.

The Effect of Boundaries.

When an airfoil is producing lift in a restricted airstream - as, for instance, when an airplane is flying close to the ground - the air disturbance set up is obviously affected by the boundary.

If A is an airfoil close to the ground (Fig. 24) the circulation flow round it cannot continue past the boundary. All flow quite close to the boundary must be parallel to the ground. That is, at the boundary all vertical velocities are neutralized. That is to say, that in effect the ground produces vertical velocities at every point equal and opposite to those caused by the airfoil.

The same effect would be produced if an inverted airfoil B (Fig. 25) were at work underneath the original one, at a distance equal to twice the ground distance.

This applies generally to all forms of vortex motion near a boundary to the fluid. They behave exactly as though the boundary were a mirror reflecting an image of themselves, and as if that image produced the same air disturbance as a real vortex.

The effects can be calculated in the same way as in the biplane case, the only difference being that all the effects are in the opposite direction. The real wing A works in an upwash caused by the image B. This decreases the angle of incidence needed to produce a given lift, and reduces the drag. Secondary effects similar to those of the biplane case also occur. The image B reduces the velocity at A, and so slightly decreases the lift, but the net effect is to increase the L/D ratio.

A similar effect occurs when a model is tested in the wind-tunnel. Here the floor - or rather as models are usually tested upside down, the roof - of the tunnel produces the same effect as the ground in the case mentioned.

The floor produces another image on the other side. In addition the two side-walls produce further side images. The general total effect is to improve the apparent performance of the model by straightening out the airstream round it - thus models in the wind-tunnel behave as though they had an increased aspect ratio - that is, the drag is too low.

When the "tunnel" is of the type most usual on the Continent - that is, when the models are tested in a free jet of air without material tunnel walls - the surface of the jet also behaves as a mittor, but it is a sort of reversed mirror - the airfoil is not turned upside down, but is reflected as the same way up. In this case the drag is increased.

This correction may be quite important. In a circular tunnel, if the span of the model is one-half the diameter of the tunnel, the decrease of drag is about one-eighth of the induced drag of the model. Fortunately this correction varies very rapidly as the size of the tunnel is increased - so that the majority of uncorrected model tests may be used without serious error.

The general outline of the vortex theory has now been described in so far as relates to its general application to the wings of an airplane. It has also been applied with marked success to the airscrew, which is a special form of airfoil moving in a spiral path, and leaving behind it a set of spiral trailing vortices.

The effect on the performance of each blade airfoil of its own and its neighboring blades system of circulation and vortices is very much that which would occur in a multiplane with a very large number of airfoils each having a gap equal to the advance per revolution divided by the number of blades of the airscrew itself.

In this special case the calculation of the interference effects is distinctly more complex than in the case of the ordinary airfoil, but the theory does a good deal towards clearing up the hitherto somewhat vexed question of the "inflow velocity" of the airscrew, and it is claimed that it can be used for practical design work with satisfactory results.

Despite the admittedly satisfactory results which have been attained by the application of the theory to airfoils and airscrews there remains in this country at any rate a fairly strong reluctance on the part of certain "authorities" on aerodynamics to accept the vortex theory as essentially sound.

Mr. Leonard Bairstow is and has been from the days when Mr. Lanchester advanced the theory in its earliest form, a leader of this "anti-vorticist" movement, and so far as can be gathered he still remains of that persuasion - although he admits that he is unable to explain why the theory works so well if it is in fact unsound.

For all practical purposes an objection to a theory which does not assert that the theory fails to give correct results has comparatively little weight. The vortex theory does not give a complete explanation of the qualities of a real airfoil.

Neglecting viscosity, it obviously does not explain any viscous effects, and one may object to it on the score of incompleteness - if one is prepared to provide a more complete theory.

But a theory is only after all the setting out of a set of phenomena in an orderly and co-related form, and the test of a theory is simply, Does it introduce order into what was otherwise an unrelated statement of experience? If it does, with sufficient accuracy for practical purposes, then for practical purposes the theory is to be regarded as valid until it can be replaced by one which either covers a greater range of experience or covers the same range with greater accuracy.

On these grounds the vortex theory may be regarded as valid until something either better or more complete replaces it.

One of the most insistent objections to the theory is that it gives no indication of the breakdown of flow over an airfoil which occurs at the critical angle - or "burble point" of an airfoil - of the resulting falling off of lift. It may be as well to consider this particular item in a little more detail.

The airflow round an airfoil in the fluid of negligible velocity follows the profile of the section smoothly as in Fig. 26 A.

In the real airfoil at ordinary angles it does not quite do this. Close to the surface of the airfoil there are small eddies forming as it were rollers between the skin of air which sticks to the airfoil (B). These small eddies are the result of viscosity, and are responsible for some part of the resistance of an airfoil in a viscous fluid.

The flow round a stalled airfoil is something like that shown in C of Fig. 26. What is happening is the formation of eddies, really similar to the roller eddies of B, on a greatly enlarged scale. Like the roller eddies these large eddies are the result of viscosity, but in this case they are so large compared to the size of the airfoil that they cause the general flow round that airflow to be greatly changed.

It does not seem exactly fair to blame the incomplete vortex theory for failing to indicate the result of viscosity when the theory expressly neglects viscosity. But it is by no means certain that the vortex theory is incapable of throwing useful light on this phenomenon of burbling.

The formation of eddies at the airfoil necessarily means dissipation of energy - that is to say, resistance. In any finite airfoil, as has already been explained, the production of lift also involves resistance - and the dissipation of energy.

All the energy available for dissipation must be supplied from the stock of kinetic energy in the airstream flowing past the airfoil - and on very general principles one would expect that when the energy dissipated in overcoming viscous forces exceeded a certain amount a considerable modification of the flow round the airfoil would occur.

At zero lift the induced drag is zero, and the total drag is therefore infinitely greater than the induced drag. As the lift increases the ratio of lift to drag improves, and in all good airfoils the real drag reaches a value of twice the induced drag for the corresponding lift. Theoretically this should be the condition for best L/D, and in practice it is so to a fairly close approximation. As the angle increases the ratio of total drag to induced drag becomes first less than two, and then increases again and always eventually exceeds twice the induced drag.

On general principles one would expect that when the real drag again reached twice the "induced" drag another critical point would be reached.

And in fact, examination of a large number of airfoils shows that the curve of real drag crosses the curve of twice the induced drag at some position very close indeed to the stalling angle.

The actual drag at the observed maximum lift coefficient is for many airfoils quite considerably less than twice the induced drag. But the drag is always increasing very rapidly when it reaches the neighborhood of twice the induced drag, and if polar curves of airfoils are plotted over a polar curve of twice the induced drag for the appropriate aspect ratio, the airfoil curve crosses the double-induced drag curve very near to the critical angle.

In Fig. 27 the two extreme curves marked $n = 5$ and $n = 2\frac{1}{2}$

are the curves of induced drag for an airfoil of 5 and of $2\frac{1}{2}$ aspect ratio. The second curve is therefore one of twice the induced drag proper to an aspect ratio of 5. The dotted and the circle-marked curve are characteristics of two entirely different airfoils of aspect ratio 5. It will be seen that these two curves cross the $n = 2\frac{1}{2}$ curve at a lift coefficient not far from the maximum of the respective wings.

This rough rule breaks down entirely for certain airfoils - such as some thin strut section whose initial drag at zero lift is so high compared to the slope of the lift curve that the drag is always more than twice the induced drag. This is not particularly surprising for it is the second point at which the relation holds which constitutes the critical or stalling condition, and as the first - or best L/D condition - is not achieved the second point is never reached. And as a matter of fact, these sections do not appear to have a critical angle of the type found in normal airfoils.

One result of any rule of the type suggested above is that the maximum lift of any given section will fall off as the aspect ratio increases, and that at the same time the position of maximum L/D and of stalling point will approach one another. As a matter of fact, there is fairly good evidence that this does not occur to some extent, certain airfoils tested under "infinite aspect ratio" conditions showing the maximum L/D at values of lift very fairly close to the maximum. For instance, the airfoil whose characteristics were shown in Fig. 13, tested with partitions at its ends, has its maximum L/D ratio at a lift coefficient of about 80 per cent of the maximum. Incidentally, it stalls at a point which indicates that according to the rule above mentioned, it has a real aspect ratio of 14.5 instead of one of infinity.

It is not suggested that this rule connecting stalling with the development of a resistance equal to twice the induced drag can be regarded as one to be relied upon in all cases, but the general run of the evidence is that there is some definite relation between the frictional or viscous resistance and the theoretical induced drag which leads to a breakdown of flow and is the cause of stalling.

Thus, although the vortex theory does not in itself provide an explanation of stalling, it is quite possible that it will lead to the discovery of some such relation, and so throw light upon this rather important phenomenon.

An objection of a somewhat more abstruse nature has also been advanced by certain British aerodynamicists. In the mathematical development of the theory it has been assumed that the vortices which trail behind the wing flow straight back - at right angles to the span. Now as an observed fact, the disturbance behind the wing is not strictly so arranged, and it is held that departure from this rectangular distribution of the vortices, invalidates the whole of the mathematical development of the theory. (A photograph of a wing-tip vortex taken in a small very high-speed wind-tunnel is given in Technical Report No. 83 issued by the American Advisory Committee for Aeronautics.

Under certain conditions of temperature and humidity it was found that the water-vapor in the air condensed as a result of the disturbance in the vortex, and the vortices could thus be seen and photographed.)

This appears to be an academical objection of very little real importance. For what is in question in practice is the gross effect of the disturbance caused by any given wing in producing "induced drag."

There is a definite value for the induced drag caused by any arrangement of trailing vortices, and this actual induced drag can be equated to that which would be produced by a particular rectangular arrangement.

The conventionalized rectangular arrangement of vortices lends itself more readily to mathematical treatment than does a more complicated system such as pretty certainly occurs in practice, but the experimental evidence seems to show pretty conclusively that to within a fair degree of accuracy the real arrangement differs little in its effects from the equivalent rectangular arrangement which has been assumed for the purpose of calculation.

As a matter of fact, there is some very considerable reason for suspecting that the reluctance shown in certain British circles to an acceptance of the vortex theory is of the nature of an attempt at face-saving for, as has already been recorded, when Mr. Lanchester first published the general statement of the theory certain eminent lights of the N.P.L. to all intents and purposes ridiculed his suggestions. It is from the same quarters that the main objections to the theory are still raised.

Figs.1,2,3.

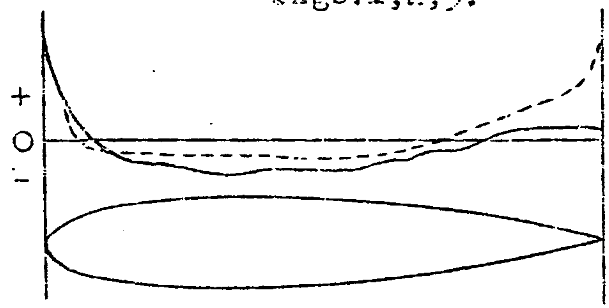

Fig.1.

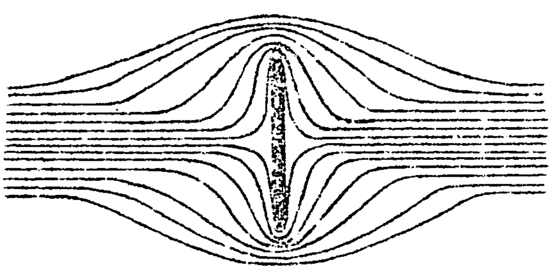

Fig.2.

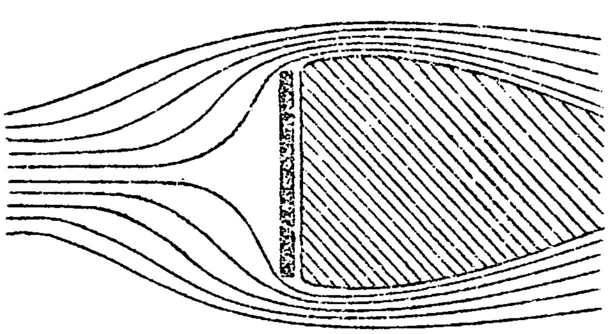

Fig.3.

Figs.4,5,6,7.

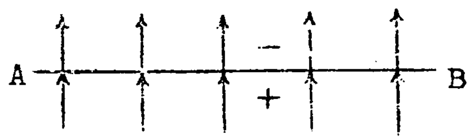

Fig.4.

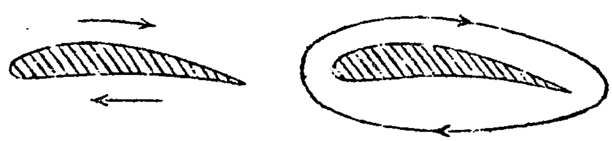

Fig.5.

Fig.6.

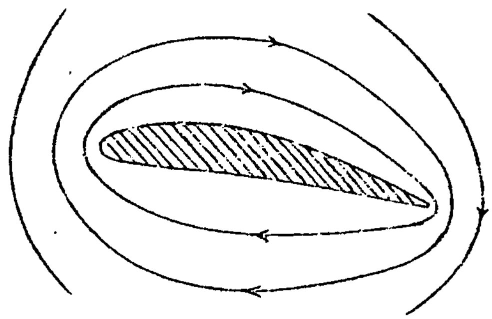

Fig.7.

Figs. 8, 9. 10.

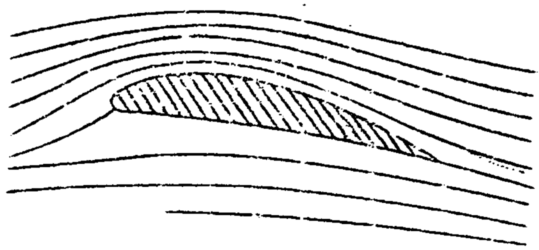

Fig. 8.

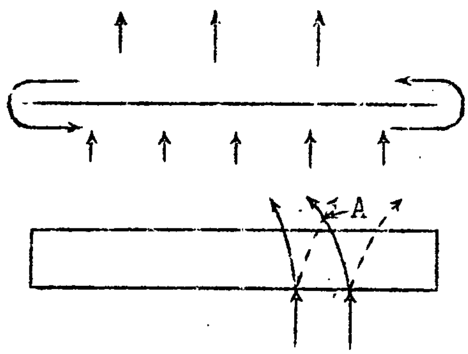

Fig. 9.

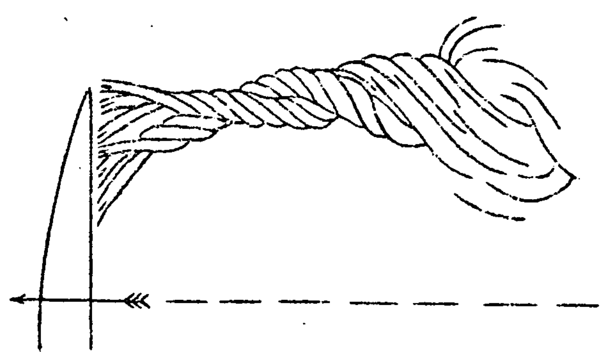

Fig. 10.

Figs. 11, 12, 13.

Fig. 11.

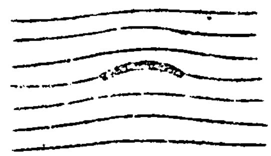

Fig. 12.

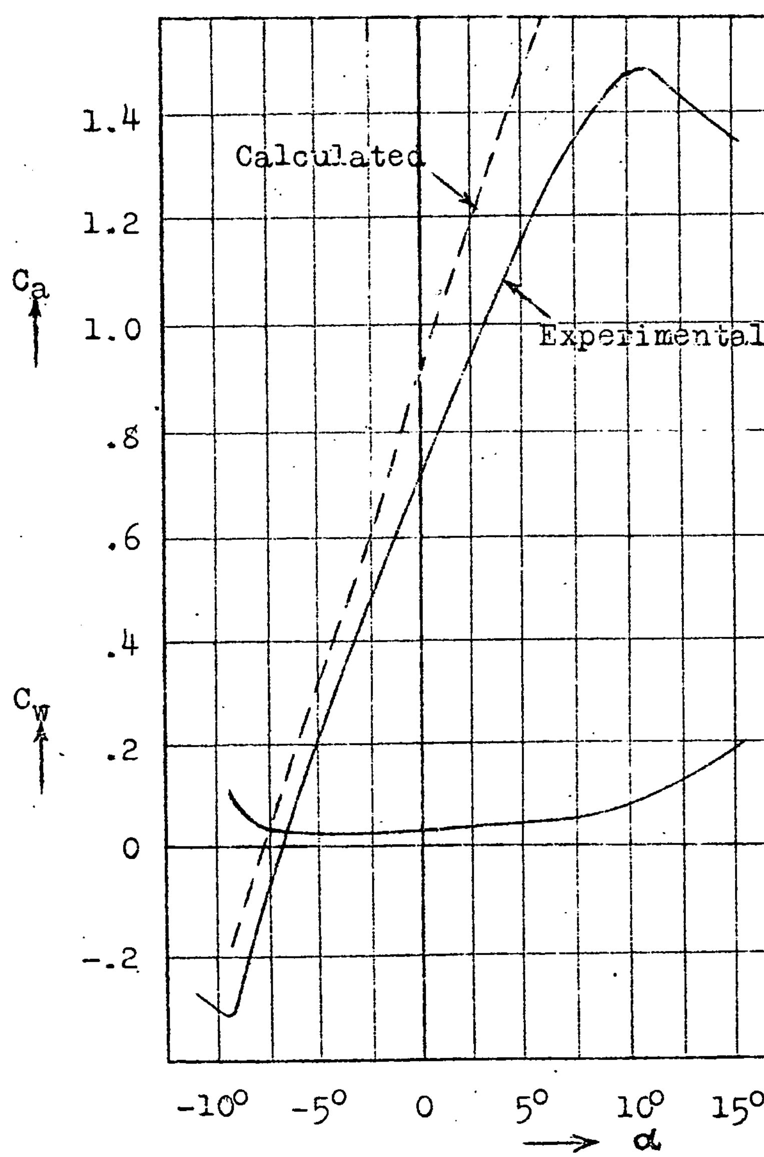

Fig. 13.

Fig.14.

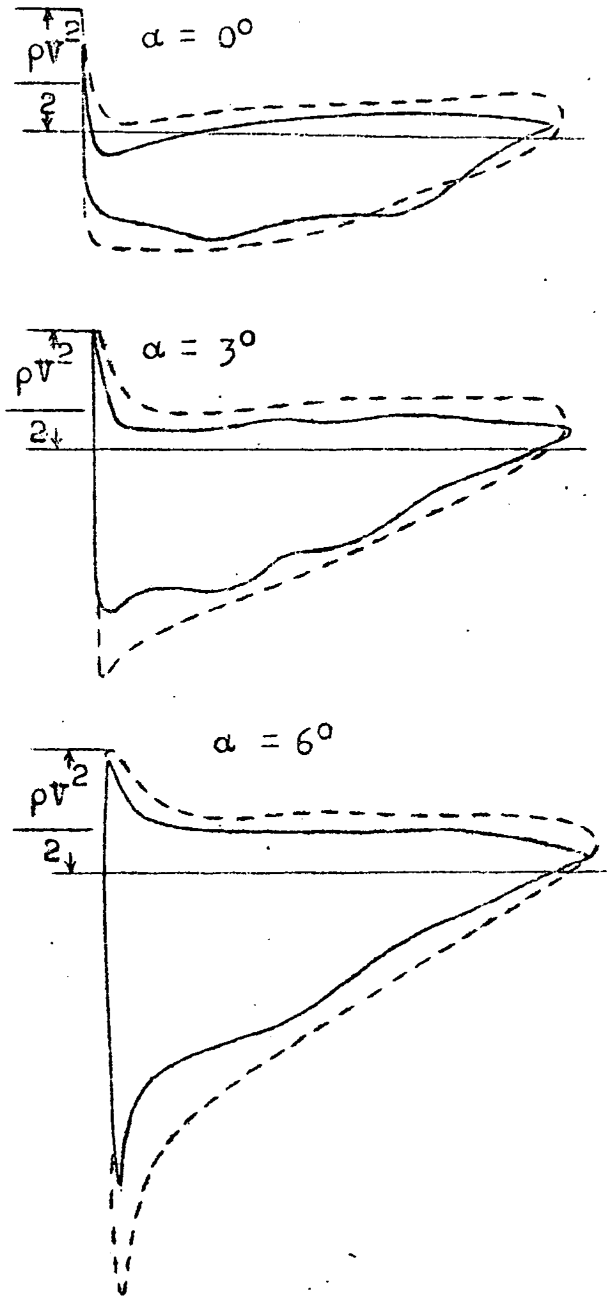

Fig.14.

Figs. 15, 16, 17, 18.

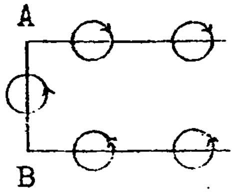

Fig. 15.

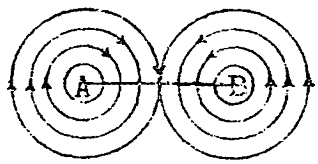

Fig. 16.

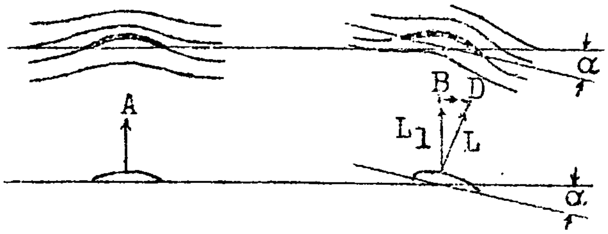

Fig. 17.

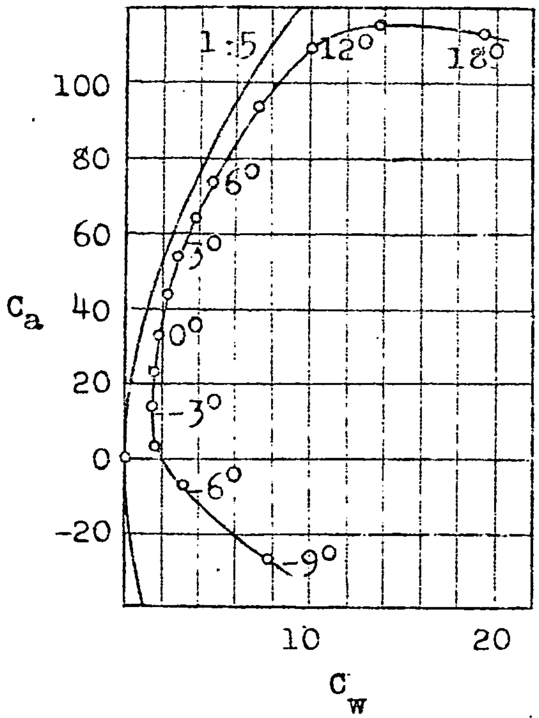

Fig. 18.

Fig.19.

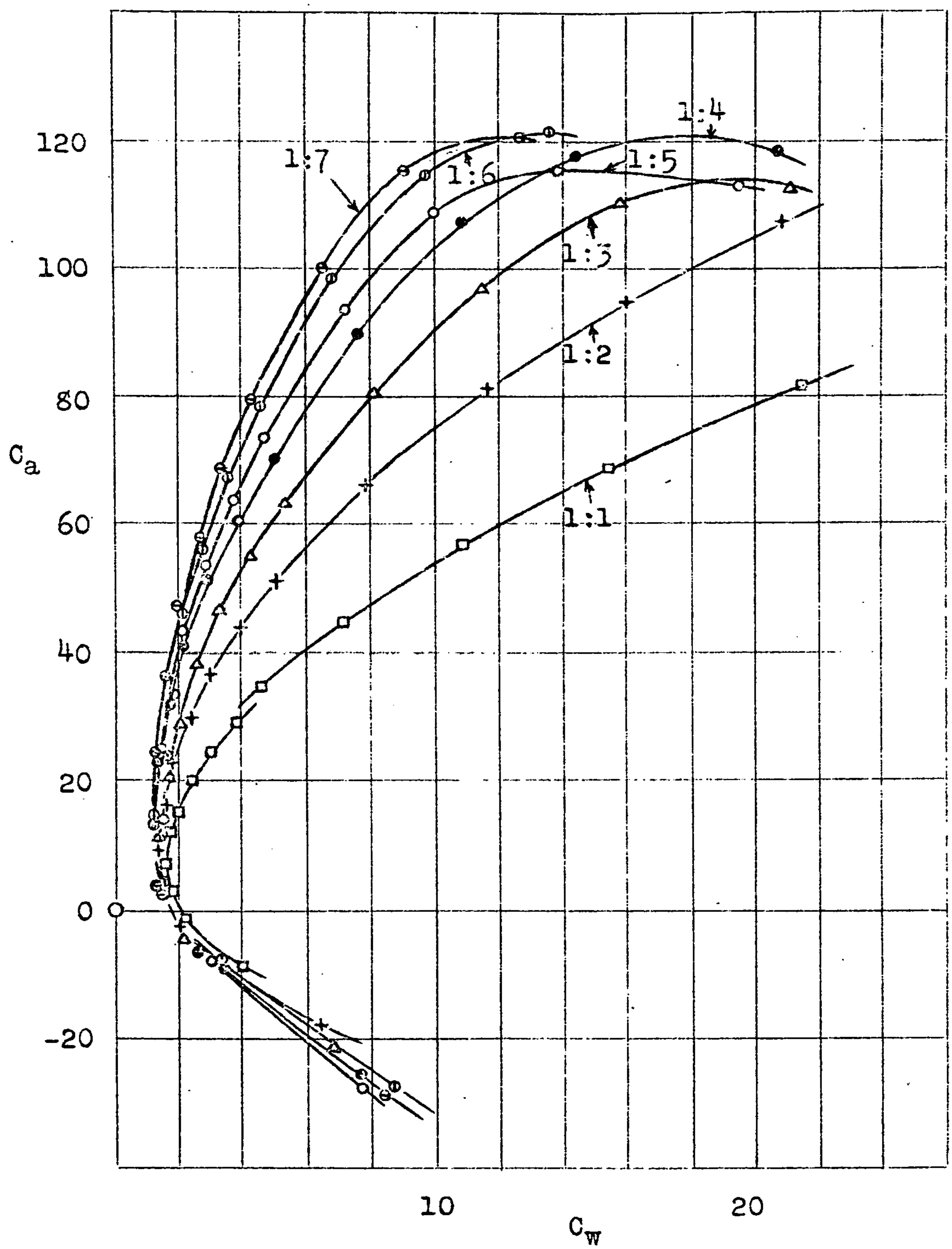

Fig.19.

Fig.20.

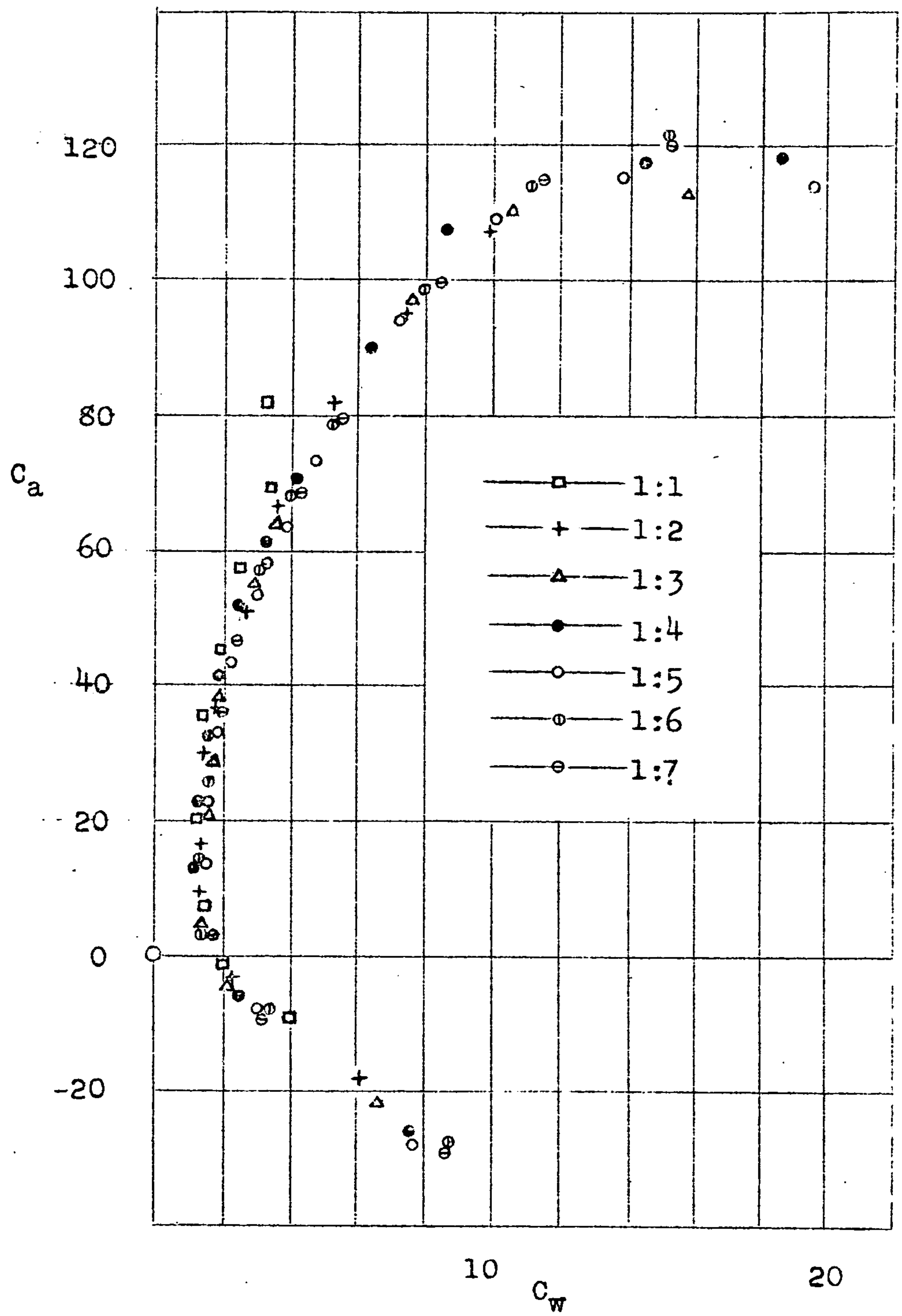

Fig.20.

Figs. 21, 22, 23, 24, 25.

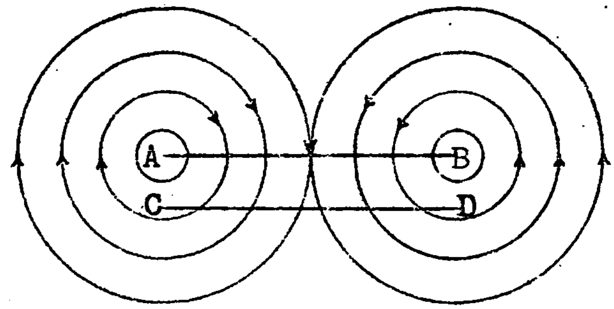

Fig. 21.

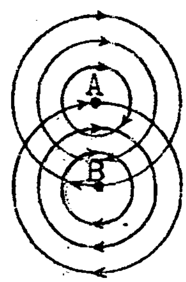

Fig. 22.

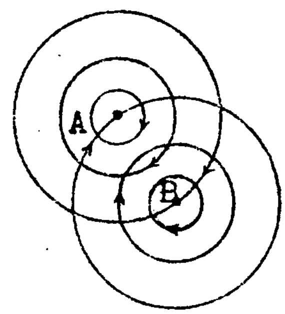

Fig. 23.

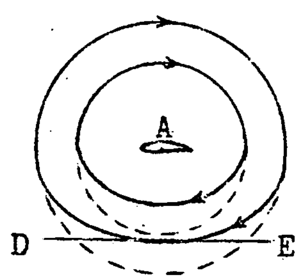

Fig. 24.

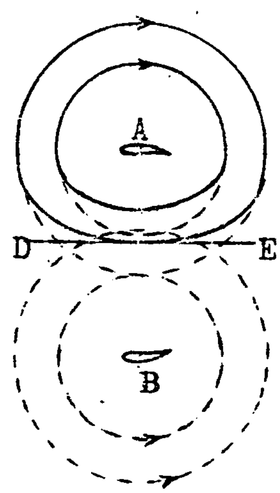

Fig. 25.

Figs. 26, 27.

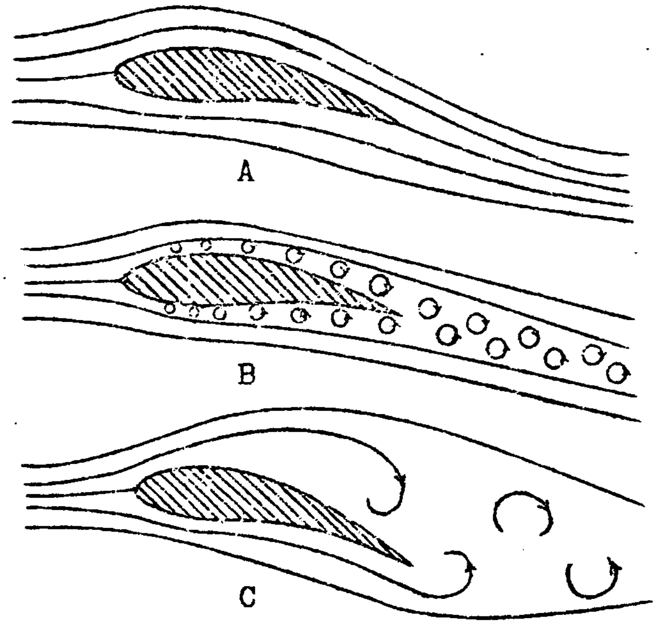

Fig. 26.

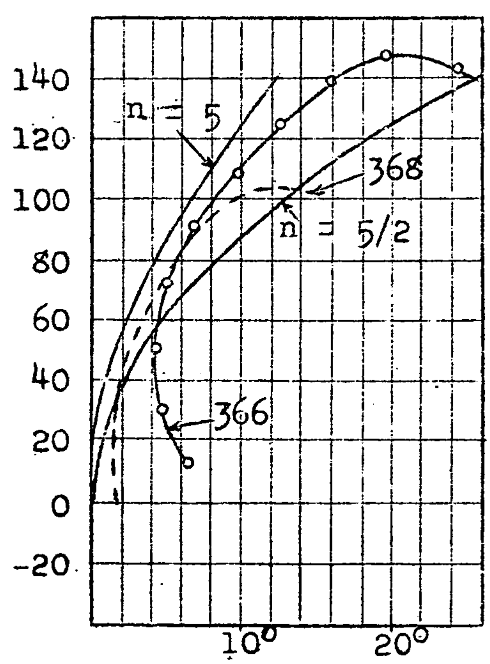

Fig. 27.