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GLIDING

Edited by Alan E. Slater, M.A., F.R.Met.S.

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OFFICIAL ORGAN OF THE BRITISH GLIDING ASSOCIATION

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Cover Photograph—Mr. C. E. Hardwick launched in his Petrel at the Long Mynd. *By A. R. Simmonds*

International Contacts

It is not an easy task to run an international organisation for bringing together those who have a common interest. It must not only get work done by people who are too distant for frequent personal contact, but must first show the necessity for its coming into existence at all.

Quite a lot of information can be exchanged between countries in a haphazard way without an international organization to foster it, and such an organization has therefore to justify itself by making the exchange more comprehensive, more economical of effort, and, especially in matters which concern safety, more rapid.

The first body of this kind in the field of Gliding was the International Commission for the Study of Soaring Flight, or ISTUS. It was formed in 1930 with the participation of eight nations, whose number had increased to 21 by the time it suspended activities in 1939. Its business affairs were looked after by the staff of the German Research Institute for Soaring Flight (D.F.S.), in a building adjacent to Darmstadt aerodrome; and as this Institute was subsidized, the arrangement was most convenient financially as well as in other ways.

At the annual conferences of ISTUS, papers were read which were afterwards published, but although some of these annual volumes contained a great amount of interesting information, it never got circulated to the extent it deserved In this country, for instance, very few people were willing to plough through so much material, nearly all of it in foreign languages, and of these few, still fewer (i e. none) had the time to sift it and write it up, let alone funds to publish the result.

A successor to this body, the International Scientific and Technical Organization for Soaring Flight (OSTIV) was formed in 1948 to carry on the good work. It has been up against the same difficulty of finding volunteers in each country who can spare time to further its work, but these are gradually sorting themselves out and there are now tangible signs of progress.

It was not found possible to publish the papers read at its first congress, held in Switzerland in 1948, but those given in Sweden in 1950 are now in print and form the first OSTIV publication, upon which it is to be congratulated. GLIDING has already published one of the papers, and another appears in this issue, with summaries of the rest. Those who want to possess the whole volume can get it for 5s. plus postage, from Mr. J. P. Honig, Kanaalweg 3, The Hague, Holland.

One contribution to the 1950 Congress will be missed, owing to its having been spoken and not written: that was the valuable talk and film show by Karl Erik Oevgaard, who has since so tragically lost his life, apparently due to oxygen failure, while making a record climb to 55,000 feet in the Bishop standing wave. We hope it will be possible to collect and publish a substantial part of all the valuable information on waves and rotors which he gathered on his many soaring expeditions among mountains.

The next congress of OSTIV is to be held in Spain during this year's International Championships. It is likely to be on a larger scale than the previous one, and we hope that sufficient funds will be made available for the results to appear as Publication No. 2.

B.G.A. News

Annual General Meeting

The Annual General Meeting of the British Gliding Association will be held on Saturday, 8th March, 1952. The time will be announced later. On the following Sunday morning, 9th March, an Instructors' Conference is expected to be held; the place is not yet decided.

International Championships

This event will be held in 1952 in Spain. The following have been provisionally chosen as pilots in the British team: Flt. Lt. R. C. Forbes, Geoffrey H. Stephenson, Lorne Welch, Philip A. Wills, Frank Foster. Five Sky sailplanes have been lent by Slingsby Sailplanes, six cars by the Standard Motor Co., and radio equipment by Messrs. Pye. On another page we publish an appeal by Viscount Kemsley, President of the B.G.A., for donations to meet the expenses of sending a British team.

At the time of going to press, the place of the meeting has not yet been announced. The dates are 30th June to 15th July inclusive.

CORRECTION

In the article "Flying Tcehnique at the Championships," published in the last issue, a correction should be made to the wind data given alongside the tephigram at the top of page 102. Wind direction at Liverpool at 800 mbs. was 264 deg., not 214; so there was no sharp change at that height on the "wave day."

The wind speed at 1,000 mbs. was omitted in our chart, as the figure given in the Daily Aerological Report, 51 knots, was almost certainly in error.

It should have been mentioned that the excellent map of notable competition flights, published on page 100 to illustrate the same article, was drawn by Mr. Peter Rivers, to whom we are grateful for this and other drawings done by him for GLIDING.

Progress Report

A year ago, we shot our line on how we were doing. Our subscribers may be interested if we extrapolate it another twelve months.

Then, we reported that we had kept our optimistic heads above water, and as the year progressed, things went on improving. Our circulation continues to rise, and we now have subscribers (accent, subscribers) in: Argentina, Australia, Austria, Canada, Ceylon, Eire, Egypt, Germany, Holland, India, Israel, Malaya, Malta, M.E.A.F., New Zealand, Nyasaland, Pakistan, Portugal, Singapore, South Africa, Southern Rhodesia, Spain, Sweden, Switzerland, Uganda, and the U.S.A. We also sell the odd copy in England, Wales, Scotland, and Northern Ireland. In addition, we exchange publications with a number of other countries.

However, no sooner did we heave a sigh of relief at having emerged from the waters, when someone turned on the tap of inflation a lot harder, and our costs once more caught up with us. So we are now financially back nearly (but not quite) to where we were, i.e. paying our way through the honorariness of our Editor and contributors, who, bless them, seem to like it. We have the sensation of one running up the 'down' side of a moving stairway, and when we run harder someone neatly speeds up the escalator accordingly. But courage—our legs are still strong, and at the other end, our eyes still sparkle indomitably just above flood-level. And, everyone goes on telling us how good we are, so our hearts are high. We're quite a sight.

Accidents

Clubs are reminded that all accidents or incidents must be reported on the appropriate form, of which supplies can be obtained from the B.G.A. Secretary. The Accident Analysis Committee wishes to thank all those Clubs who have made regular reports throughout the past year, but there are still one or two clubs which it is believed have had an accident which has not yet been reported.

The Airflow Round a Conical Hill

by Dr. Jiri Förchtgott.

(Translated by the author from *Meteorologické Zprávy*, No. 3-4, March 1951)

THE effect of a short mountain ridge, or even of a conical hill, on the streamfield of a stable air current is a form of mechanical disturbance. The opinions of our practical observers—sailplane pilots—on this question have gradually changed in the years since the war. Initially there was the conviction that only a weak vertical component of no practical importance was produced by conical obstacles, the region of up-currents being too small. Later some of the more experienced glider pilots began to look for new possible soaring places in the vicinity of the well-used ridge of Rana when the air above the Rana slope became crowded with school gliders. They were successful in the space above the tops of the isolated hills Oblik and Mily—the smaller space with weaker up-currents became more interesting, and real soaring in it was considered as a sign of high pilot quality.

Thanks to these pilots, prejudices about the smallness of the effect of isolated hills on the air current were overcome. Thanks also to glider pilots, many further prejudices concerning the real nature of some atmospheric phenomena will be overcome.

Experience has shown that there is sufficient space with enough uplift for soaring in front of isolated hills. As in the case of long ridges, it can be supposed that the velocity and depth of the streaming layer are of equal importance, and one must expect to find certain types of flow corresponding to different kinds of airstream. This means that, just as soarable regions are found in the lee of long ridges, they may also be found in the lee of isolated hills.

Single observations made in the lee of conical hills have in the meantime no direct attestation by means of gliders (in our country), but they indicate some surprising facts that are very marked under certain airstream and humidity conditions. The shape, structure and motion of clouds can show clearly the type of flow; and this can also be followed occasionally or systematically by soaring flight.

When the air is stably stratified there are several possible types of flow that can be recognised by the pattern of the flow in the lee of the hill. To classify these regimes precisely a series of systematic observations is needed. At present it is possible to describe, though only schematically, one of these flow patterns, perhaps the most fundamental one, which by its characteristics has attracted the attention of authors in various countries (e.g. R. S. Scorer, London, explains in a similar way the origin of some typical clouds in the lee of Gibraltar; also one must mention observations of cloud caps over conical mountains, volcanoes, etc.).

The experience of glider pilots shows that on the windward side of isolated hills it is possible to reach the same height as in front of a ridge of similar section, the only condition being a greater wind velocity. It seems that the same type of flow as for mountain ridges is produced by isolated hills in winds of higher velocity. For instance, Figure 1 shows the analogy of the vortex type of flow behind a ridge, but the same airstream blowing over a ridge would produce a "higher" type of flow—the "wave type." The relation between the depth of the air-current and the type of flow produced by an isolated hill is not at present known.

Flow over a conical obstacle presents a problem in three dimensions, whereas the flow over a mountain ridge is two-dimensional. Most of the airstream in the layers below the top (e.g. below the level "a") flows round the sides, air in the upper levels only going over the top. One must expect great horizontal deflections of the streamlines in the low levels near the foot of the hill, while in higher levels the vertical deflection gradually predominates. Near to the front foot of the obstacle the streamlines diverge and cause a weakening of the stream, with the result that the vertical component of the wind is very small or even negative in front of the lower half of the

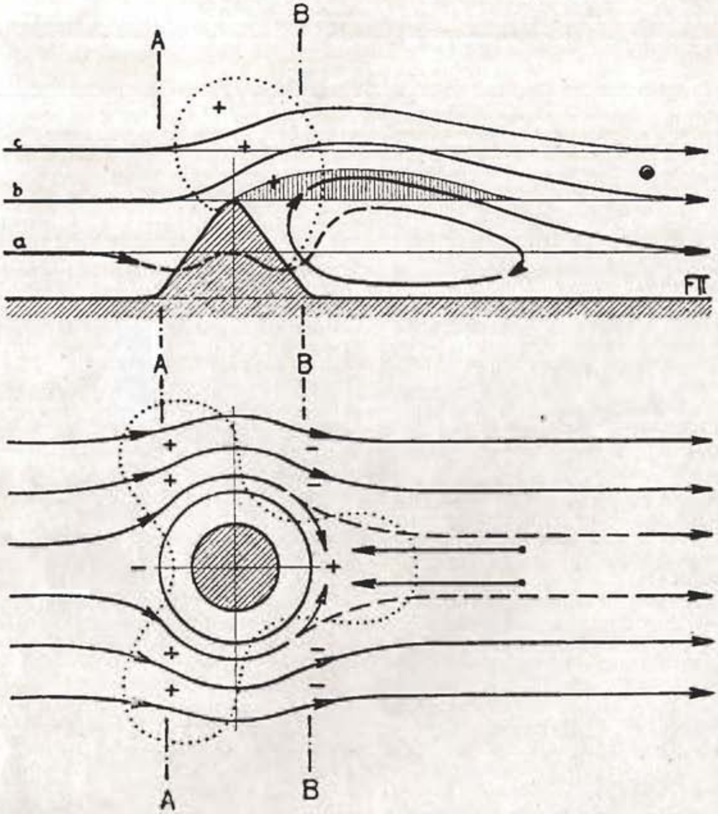


Fig. 1.—Side view and ground plan of the streamlines over a conical hill. The lower diagram shows the flow in the level "a." Up- and down-components are denoted by + and — respectively.

slope—no use for soaring. Only in front of and above the top is there enough lift for soaring. Since the streamlines in level "a" converge on the right and left of the front side of the obstacle, the lift is greater there than immediately in front of it. In Fig. 1, upward wind components are indicated by + and downward ones by—. The cross-section A, in Fig. 3, shows the lift and sink in front of the hill. The sink in front of the hill explains why everyone who flies in a glider below the top of the hill is forced to land. According to experience of soaring in the slope wind in front of a ridge most pilots tend to stay in front of an obstacle; but now, in front of a conical hill, one finds the unsoarable downward component, which is proportional to the intensity of the sideways divergence.

In the lee of the obstacle, the streamlines return to their original distribution. Below the top level, on the right and left sides behind the obstacle is found divergence of the streamlines and a down component. Behind the lee foot there must be expected intense convergence and a corresponding

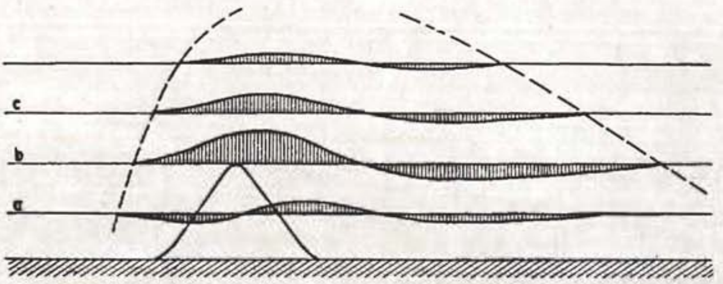


Fig. 2.—Vertical section to one side of the obstacle, showing up- and down-components.

up-component along most of the lee slope. By observations on Milesooka, this fact was sometimes found by cloud patches ascending the lee slope while the windward slope was clear; another time by hawks, soaring from low down on the lee slope, while on the windward slope they were never seen. The ground plan distribution of the down- and up-components in levels below the top is seen in the second diagram of Fig. 1, the dotted line enclosing the region of up-component.

In the lee of the obstacle a flat horizontal vortex, not very turbulent, extends for a considerable distance, and this causes a reversal of the surface wind on the lee side (see Fig. 1). In the side view, we see that the greatest up-component is centred above the lee slope (enclosed by dots in Fig. 1).

The type of flow just described often produces a wave cloud standing near the top of isolated hills and mountains when they rise above the surrounding obstacles. The cloud is elongated down the direction of the wind and is centred to the lee of the top. When the humidity is insufficient in the lower layers the "cloud cap" cannot be expected, but in higher levels cirrus-like fibred clouds are often formed (*Stromschnelle der Luft*). Such waves are the product of deformation of the airflow just described, and appear mostly in mountainous regions where the obstacle dimensions and well-developed types of flow extend to the highest levels.

When soaring in the lee of isolated obstacles one must expect slight or moderate gusts, even in the region of up-component. The basic requirements for this type of flow are suitable wind velocity and stable stratification of the air. Increasing instability probably deforms this type of flow into a more or less continuous row of rotating Cu clouds extending down-wind from the hill for several tens of kilometres.

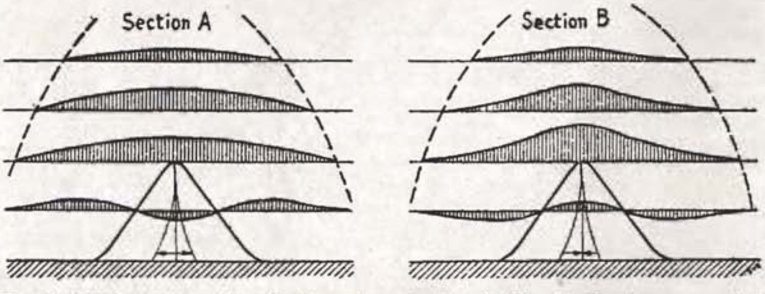


Fig. 3.—Vertical sections through lines A and B of Fig 1, showing up- and down-components of the wind.

Best Flying Speeds

by Nicholas Goodhart

As is well known, best performance in cross-country flying can only be achieved if the sailplane is flown at the optimum speed between thermals. In order to determine best speeds it is first necessary to have a curve of sinking speed against air speed (Fig. 1) for the sailplane in question.

Determination of Racing Speeds (Theory)

Paul MacCready, who did so well in the 1950 International Championships in Sweden, has written a short paper in which he describes a method of determining best speeds, and also proves its soudness by a calculus. We are not concerned with the proof, but he shows that if a tangent is drawn from A (Fig. 1) to the curve of sinking speed versus air speed of the sailplane, then if AO = the achieved rate of climb whilst circling (W_t) + the instantantaneous downdraught (W_d), OC is the best speed to fly.

The method of determining best speeds is, therefore, to draw a series of tangents for different values of AO and read off the values of OC and AB.

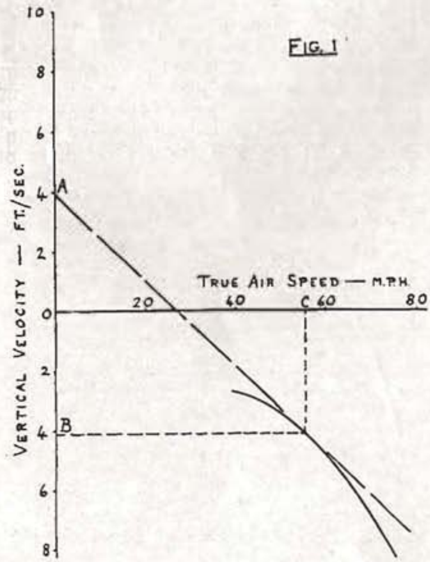


FIG. 1

The following values were obtained for a British Olympia.

AB (ft/sec).	5.5	8.1	11.0	14.1	17.5
OC (m.p.h.)	50.5	56	62	67.5	73.5

Since $AO = W_t + W_d$ and $OB = W_s$, where W_s is the sinking speed of the sailplane relative to the surrounding air, it is clear that $AB = W_t + W_d + W_s$

For convenience we want AB in integer values; the next step is therefore to plot AB against OC (Fig. 2).

From this curve a new set of values of OC can be read off against whole-number values of AB.

For the Olympia:—

AB (ft/sec)	4	6	8	10	12	14	16	18
OC (m.p.h.)	47	51 5	56	60	64	67.5	71	74

A series of tables can now be constructed for selected values of achieved rate of climb (W_t).

For $W_t = 2$ ft/sec.

$W_d + W_s$	A.S.I.
2	47
4	51.5
6	56
8	60
10	64
12	67.5

For $W_t = 4$ ft/sec.

$W_d + W_s$	A.S.I.
2	51.5
4	56
6	60
8	64
10	67.5
12	71

For $W_t = 6$ ft/sec.

$W_d + W_s$	A.S.I.
2	56
4	60
6	64
8	67.5
10	71
12	74

($W_d + W_s$) is the downdraught strength in which the sailplane is flying plus the sinking speed of the sailplane relative to the air, i.e. the variometer reading.

The tables therefore give, for any achieved thermal rate of climb, the speed at which the aircraft should be flying related only to the variometer reading (always providing the variometer is accurate).

All the foregoing assumes that the A.S.I. is reading true air speed and the variometer true vertical velocity. This will not be so at altitude and a correction should be applied.

This correction varies with the type of variometer. For a pellet type it has been suggested that the reading of the variometer should be reduced by about 2% per 1,000 ft. of altitude before the tables are applied.

Practical Application

For a pellet type variometer, it is simplest to arrange the tables so that they can be fitted alongside the variometer. The figures for A.S.I. readings should be so spaced that they come opposite the appropriate variometer readings. The position of the red ball thus gives a direction idnication of the speed at which the sailplane should be flying.

So far it is all straightforward and simple; now we come to the first snag, which is how to determine the achieved rate of climb, and consequently, which table to use. The achieved rate of climb is not the variometer reading while climbing in the thermal, but the mean rate of climb from the moment of circling to the moment of setting course, i.e. including initial and final fumbles. The only way to measure this is to divide the total height gained by the total time spent circling. This is not easy, as entering a thermal is a critical period and operating a stop-watch is an added distraction.

Fortunately the best speeds in the tables are not critical, and errors of 5 m.p.h. either way have extremely little effect on the achieved cross-country speed. The solution to the snag of measuring rate of climb is, therefore, to guess it. This is not as unsatisfactory as it sounds, and can be made reasonably accurate if a few barograph charts are assessed, to see what rates actually are achieved.

Having guessed the achieved rate of climb and selected the right table, the next snag arises. This is how to strike a balance between the A.S.I. reading and the variometer, which meets the table. This is not immediately easy, since variations in speed produce variations in variometer reading. Once again, it is easiest to guess the best speed to fly and then check that the variometer is reading what the table says it should. If it isn't, guess again.

FIG. 2

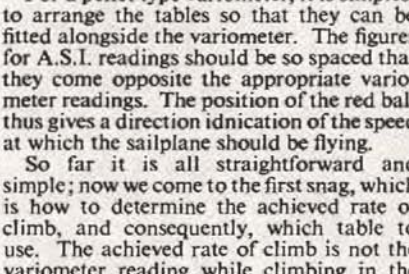
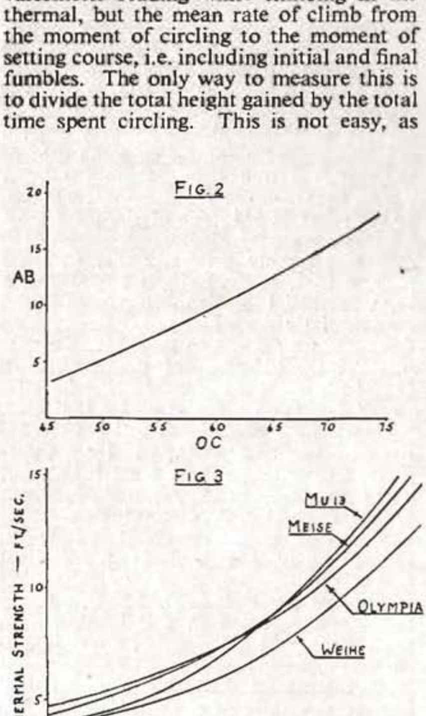


FIG. 3



Achieved Speeds

When best flying speeds have been obtained, a few more sums will produce a preformance chart which shows what the achieved speed through the air should be, for any given thermal strength. In this case, thermal strength means the actual vertical velocity of the air and not the achieved rate of climb of the sailplane.

The calculations have been made for four types of sailplane and the results are shown in Fig. 3. The curves for the Olympia and Weihe should be reasonably accurate, as they are based on performance measurements made in this country; the curve for the Meise has been obtained simply by applying a weight correction to the Olympia curve and assuming that it is aerodynamically similar. The Mu-13 curve has been obtained from a calculated polar based on figures given in the Spring, 1951, issue of GLIDING. In each case sinking speed in a thermal has been assumed to be 1.5 times the minimum sink.

These curves make an interesting study when handicapping systems are being considered. The Weihe handicap of 10% *vis-à-vis* the Olympia is correct when there is no wind and thermal strength is 9 ft/sec., which gives the Olympia an achieved rate of climb of 5 ft/sec. In a 20 m.p.h. following wind the handicap is correct if the thermals are such as to give the Olympia a rate of climb of 3 ft/sec. For the Mu-13 the handicap of 10% is about right at all thermal strengths when the following wind is 10 m.p.h.

The Meise appears to be correctly handicapped when its rate of climb is 10 ft/sec. in no wind. The 20 m.p.h. wind case is similar to the Olympia, i.e. a rate of climb of 3 ft/sec.

In so far as a single handicap can cover all cases, it therefore appears that the handicap used in the National Competitions is a good compromise for British thermals.

Final Note

Paul MacCready says: "The most important factors in distance soaring (aside from an adequate sailplane) are still luck and practical meteorological knowledge."

The Artificial Horizon in the Sailplane

by T. R. Beasley.

THE sailplane pilot has for many years envied the power pilot one instrument in the latter's cockpit, at the same time smiling at the many other dials and gauges he finds unnecessary. That one instrument is the artificial horizon. Until recently it has been considered a luxury to be frowned upon for its great weight and expense; some pilots have even been so conservative as to call it "cheating."

Before the war, one or two experimental German sailplanes were fitted with horizons; most of these were suction-operated, either by means of a venturi tube or sometimes by means of a hand pump. The former is obviously rather unsatisfactory, because it is liable to ice up when most needed, i.e., in cloud when the A.S.I. has already failed due to ice. Very little information is available on the hand-pump installation, but it would obviously be very fatiguing, as a pressure of 3 to 4 inches of mercury must be maintained, and while grappling with the forces in a cu-nimb the operation of the pump could be neglected—again when most needed.

The most satisfactory instrument would therefore be electrically driven, preferably from the sailplane's low voltage D.C. dry batteries used for the turn and bank indicator. Here a difficulty is encountered in that an artificial horizon gyro unit must have three degrees of freedom and must therefore have a larger gyro than a turn-and-slip if the precession force is to operate the linkage to the indicating bar. A D.C. gyro unit would have to be heavy as it could only reasonably be driven at approximately 6,000 r.p.m., unless the size was to be increased to accommodate the extra armature windings necessary for higher speed; commutation may also give trouble. The gyro unit in an artificial horizon must erect itself to the true vertical despite the attitude of the aircraft. In a D.C. instrument this could be crudely carried out by pendulum control or some type of D.C. torque motor or solenoid system. Again, the weight of the assembly increases, so the moment of inertia of the gyro must be increased accordingly.

It is probably primarily due to these reasons that, to the writer's knowledge, a D.C. artificial horizon has not been commercially manufactured. Perhaps, as our demands are not so great as the power pilots', it would be possible to make a small D.C. instrument using gravity erection of a gyro taken from a turn-and-slip indicator.

Commercial electric artificial horizons use high-frequency A.C., usually a three-phase supply taken from a small inverter. The instruments in use on modern British aircraft use a 115 volt, 400 cycle three-phase supply and consume about 15 watts. Unfortunately the cost is prohibitive; excluding batteries, a new installation would cost well over £100. For this reason they are not fitted in sailplanes, so no description will be given of these instruments.

An instrument which has been installed in several sailplanes since the war is the Horn or Askania artificial horizon of German origin; several have found their way into this country, and a brief description and a few notes on installation may be of help to users of these useful instruments.

The instrument is an unusual departure from the usual run of artificial horizons, in that it includes a turn indicator and a ball-type slip indicator. The turn indicator has its gyro axis fore-and-aft, so theoretically a tendency to instability would be noted during any rolling to port; this would probably be negligible in a sailplane. The indicator is not calibrated, but a pointer deflection of about 0.3 inches represents a rate of turn of 180° per minute. Perhaps a brief description of the complete instrument would be of interest to those not yet acquainted with it.

The presentation is similar to the familiar Sperry horizon, a small miniature aeroplane being fixed and the horizon line being the moving part of the indicator system. Thus the pilot sees on his instrument an "artificial horizon" at all times parallel to the true horizon not visible to him. Suppose the aircraft dips its port wing, so will the model aircraft on the gyro instrument. As already mentioned, the turn indicator pointer is at the top of the dial and must not be confused with the pointer on some Sperry instruments which is a *bank* indicator.

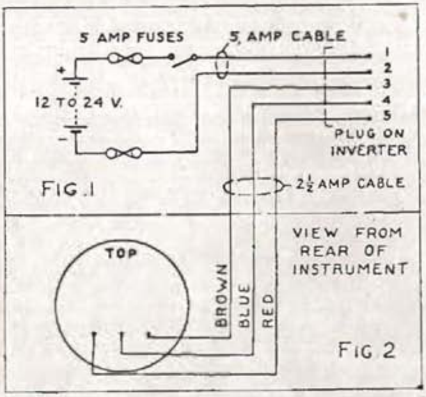
At the bottom of the instrument face is the bubble slip indicator, which consists of a black ball running in a glass tube filled with damping liquid. It might be mentioned here that the back side of the tube is coated with very good luminous green paint, as are all the other indicating pointers and the fixed datum marks.

Surrounding the dial is a large ring; this is the caging ring, worded "Los" and "Fest" ("Free" and "Caged"). On some types of horizon—types numbered 127-235-A/2 and 127-277-B/2,—this ring is of black plastic with suitable finger grips cut around the circumference; on other models—type number 127-249-A/2,—it is of light alloy with scalloped finger grips.

A brief description of the mechanism may interest the technically minded.

Both gyros are A.C. operated, the three-phase supply at 36 volts and 500 cycles per sec. being obtained from a small inverter. As mentioned above, the instrument exists in three types, differing primarily in the caging mechanism and construction. The actual gyro units are very nearly identical.

The heart of the instrument is the gyro unit; this is basically a squirrel-cage induction motor, the rotor and shaft being



integral. A recess at one end carries the actual squirrel cage, which consists of an aluminium ring cast round laminations of high-permeability iron. The stator is wound on a former attached to the top cover of the rotor case and projecting into the recess in the rotor. It may be of interest that the rotor spins at 26,000 r.p.m., so it should be obvious that the instrument must be handled with the greatest care in order to avoid damage to the bearings.

Erection is carried out by means of three-phase torque motors operated through a mercury switch controlling erection motors in both roll and pitch.

The internal wiring is not shown in the installation wiring diagrams given. This wiring must not be interfered with except by a skilled instrument repairer familiar with gyroscopics.

Care should be taken in the installation of the instrument, and the writer recommends the use of anti-vibration mountings on the instrument panel, as a hard landing would quite easily damage the gyro unit. Wiring should be carried out as shown in the circuit diagram (Fig. 1). It is important to connect the three phases correctly; if an original cable is still attached to the instrument it will probably be brown, blue and red. These should be connected to pins 3, 4 and 5 respectively on the inverter. Fig. 2 clarifies this. The connections inside the instrument are to soldering tags, so it is unwise to disconnect the joint very often. It is a good plan to have a fairly long lead, about 3 feet, and connect to a junction box or suitable plug and socket.

A list of Do's and Don'ts may be of some help to owners of these useful instruments—

1.—Wiring should be carried out with decent material and not any old junk in the hangar. Five-amp. P.V.C. cable is quite cheap, very light, and makes a neat installation. Always include a fuse (5 amp.) in the circuit. A panel type tubular fuseholder is ideal; it is wise to include an extra one to carry a spare fuse.

2.—Switch the positive input lead to the inverter if a single pole switch is used; the switch should be on the inverter side of the fuse (see diagram, Fig. 1).

3.—If radio apparatus is to be used, or considered, use screened cable throughout and see that the negative battery lead is earthed, and all the cable screens and metal parts of the sailplane are bonded together.

4.—Never switch the horizon on when uncaged, and never switch off before caging. Never transport the instrument uncaged.

5.—Never leave one phase disconnected.

6.—Never connect to a D.C. source of current or any source but 36-volt 500-cycle 3-phase.

7.—Cage before performing any aerobatics likely to result in toppling the gyro; however, if the gyro is toppled it is not necessary to wait for it to re-erect and stabilise itself. Simply cage and uncage, and the unit will be ready again for immediate use.

8.—It is unwise to switch the instrument on frequently for short periods, as the starting current is quite high. If many starts are carried out, the battery may be found to have given out when the instrument is most needed. This should be remembered when showing the installation to admirers.

9.—The three components—accumulator, inverter and instrument—will all have some effect on the c. of g. position of the aircraft, and this should be remembered when installing the apparatus.

10.—The three types of instrument differ in weight and current consumption, as can be seen below: it is obvious that the best instrument to use would be the first.

Type	*Current (per phase)*	*Weight*
127-277-B/2	0.3-0.4 amp.	2.0 kg.
127-249-A/2	0.3-0.4 amp.	2.1 kg.
127-235-A/2	0.45-0.65 amp.	2.25 kg.

Perhaps a word on batteries may be of help. The best for the job would undoubtedly consist of two Venner 12-volt lightweight accumulators. Unfortunately these are very expensive and probably beyond the reach of most clubs. The instrument will operate on 12 volts, but becomes a little sluggish, so 18 should be considered a minimum. Three motor-cycle batteries of reputable make may be used, and are quite satisfactory, although rather heavy.

The photographs (Figs. 3 and 4) show an installation in the front cockpit of a Kranich. The batteries can be seen under the centre canopy. Instruments are:—

To left of panel: Cosim variometer.

Top left on panel: A.S.I.

Lower left on panel: Mk. XIV altimeter.

Top right on panel: Turn-and-slip.

Lower right on panel: Artificial horizon (type 127-249-A/2).

To right of panel: Compass, type E2A.

In the centre of the panel at the top can be seen a panel light, and below this two switches, one for the T. & S. and the other for the A.H. Immediately below the panel can be seen the oxygen regulator to supply both cockpits.

The second photograph (Fig. 4) shows the centre canopy, under which are the two 12-volt batteries and the two oxygen economisers.

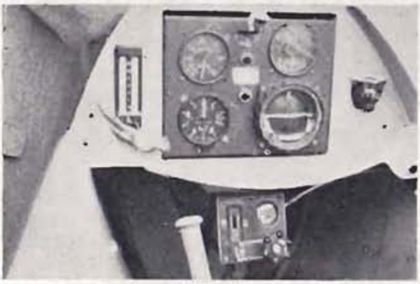


Fig. 3


Fig. 4

Wind Velocities Near a Soaring Slope

by A. H. Yates

The results of the first meteorological and gliding expedition to the Vale of Clwyd are discussed from the points of view of aerodynamics and meteorology, and Dr. Scorer adds a preliminary report on the second expedition.

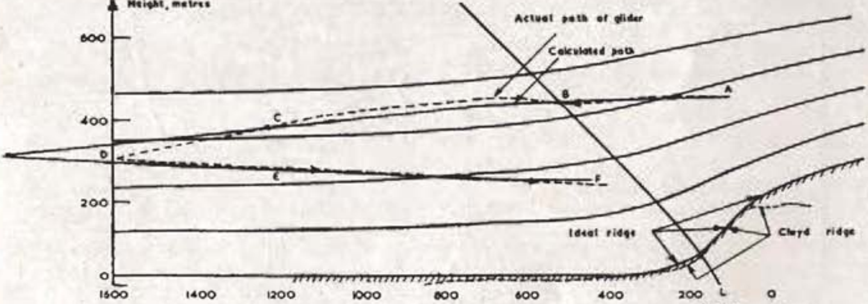
IN a recent issue of GLIDING, Dr. Scorer (Ref. 1.) wrote about observations of the path of a glider which flew upwind from above a soaring slope and which later returned to the slope at a lower level. The real sinking speed of the glider was obtained by theodolite observations from the ground and the apparent sinking speed from readings of the variometer in the glider. The observations confirmed the common experience that the apparent sinking speed was less on the outward journey than on the homeward, but showed that the difference is partly an illusion due to incorrect indication by the variometer. The actual sinking speed (from the theodolite observations) was indeed less on the outward journey than on the return, but the difference was less than that indicated by the variometer.

Dr. Scorer argues that the effect is explained if the wind slows up on approaching the hill. He shows mathematically that the difference in sinking speeds on the outward and homeward journeys is proportional to the wind velocity gradient and confirms that the variometer will record only half of this "differential sink." So far, so good. There are, however, two snags. Firstly, Dr. Scorer doubts whether the wind does slow up on approaching the hill. "The wind speed is greater over the crest," he argues. Secondly, a calculation shows that a very large drop in wind speed up to the hill would be required to account for the differential sinks measured in his experiments.

The first of these doubts can be set at rest. The wind really does slow up as it approaches a soaring ridge and then accelerates across the crest. The actual region in which the slowing occurs can be found only by calculation and the results of such a calculation are now presented.

In Figure 1 are shown the cross-sections of the ridge at Prestatyn (Ref. 1) and of a rather similar ridge for which calculations have been made. This second ideal ridge shape is one of the streamlines in the series formed by what aerodynamicists call "a source in a uniform stream." By the addition of further sources and sinks the

FIG. 1. THE FLOW PATTERN OVER A SOARING SLOPE



flow about the actual Prestatyn slope could be calculated, but the additional complication is scarcely worth the trouble.

In Figure 2 are shown the variations in wind speed at altitudes of 200, 400 and 600 metres above the foot of the ideal hill. Along the 200 metre line the wind speed (U′) falls steadily from its value far upstream (U) to 0.75 U just in front of the hill and then increases rapidly. The *direction* of the flow along the 200-metre line is also changing steadily; it is being given an increasing upward component as the hill is approached. This upward component of the wind speed, which we may call "hill lift," is plotted in Figure 3, and it is seen that this increases right up to the hill face. Curves are also given of the variation in total wind speed and of "hill lift" at 400 metres and 600 metres altitude. At these heights the slowing up of the wind speed is less marked and the "hill lift" is, of course, weaker. The hill lift is a maximum above the "source" at 0 and this agrees well enough with observations of the lift on soaring ridges. Experience shows, however, that hill lift is rarely more than half the value calculated. The discrepancy is probably due to the earth's boundary layer which has been neglected in these calculations.

We find that the wind approaching the hill is slowed up until it has passed the line LL′ in Figure 1 and after that it accelerates. The path of the flight of Ref. 1. is marked (ABCDEF) and is seen to lie largely in the region where the wind speed was falling towards the hill. The actual variations in wind speed and hill lift can be seen from the paths ABCDEF marked on Figures 2 and 3.

Now, if the glider started from A and was flown out from the hill at a constant indicated airspeed, its true rate of sink was the combination of three separate effects—

(a) the sinking speed in still air

less (b) the hill lift (Figure 3)

less (c) the fall in sinking speed due to flying into a region of increasing wind speed (from the formulae in Ref. 1.).

FIG. 2. VARIATION OF TOTAL WIND SPEED ALONG THREE HORIZONTAL LINES AT 100m 200m & 300m

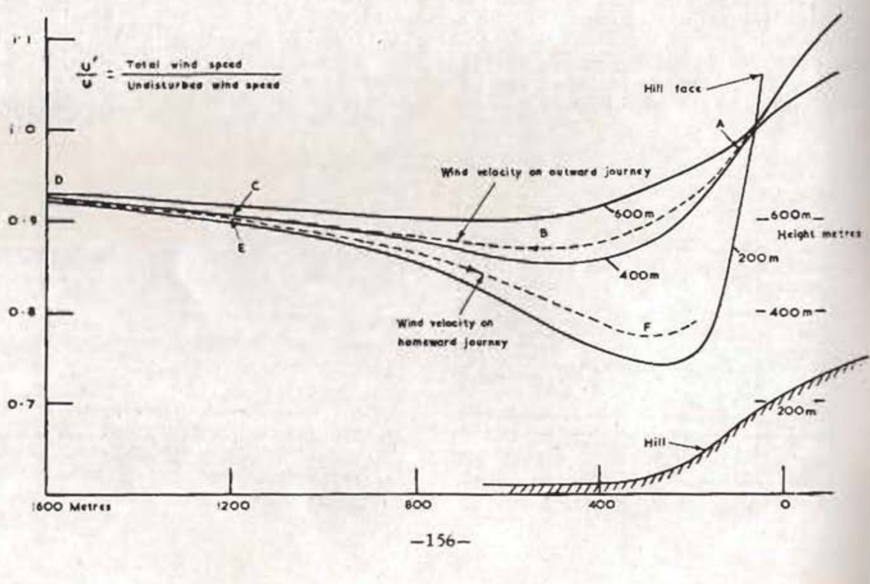
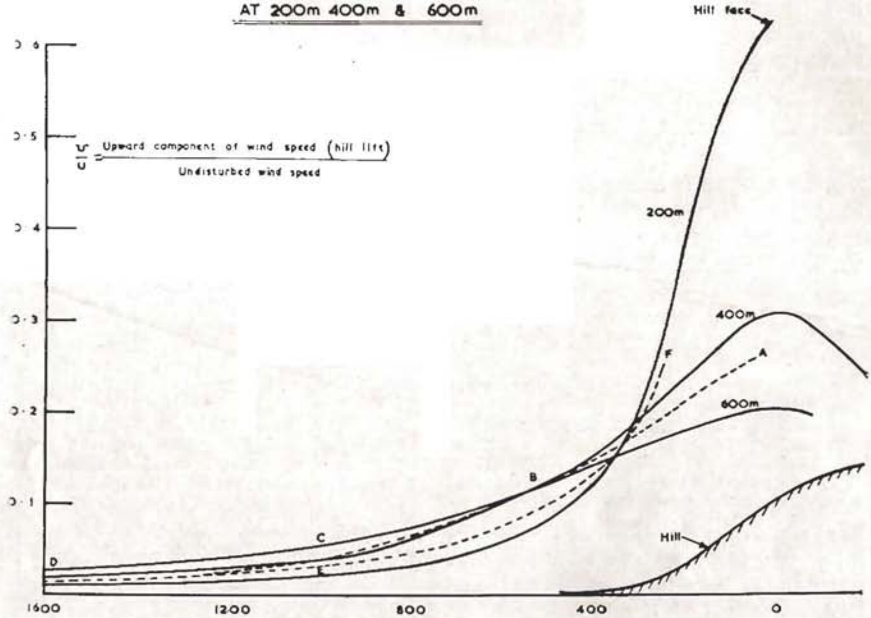


FIG. 3. VARIATION OF THEORETICAL HILL LIFT
AT 200m 400m & 600m



The effect (a) is constant throughout the flight if the pilot keeps the airspeed indicator reading constant. The hill lift (b) is seen from Figure 3 to be rather better at the greater height of the outward flight than on the return journey and (c) the sinking speed is reduced on the outward journey because the wind speed is increasing from B to D.

The effects of the hill on the wind thus give changes in the glider performance which agree qualitatively with observations, but the quantitative agreement is not so good. The path of a glider starting from the point A, flying out from the hill at an airspeed of 50 m.p.h. in a 20 m.p.h. wind, has been calculated from the effects (a), (b) and (c) enumerated above but assuming the hill lift is only half as strong as the ideal values of Figure 3. This "theoretical path" is shown in Figure 1 and it cannot be said that the agreement is very good. However, Dr. Scorer's conclusion was that there was "a need for more observations."

The purpose of this note is to show that the Clwyd observations were not, after all, as "paradoxical" as Dr. Scorer thought, that the wind does slow up on approaching the hill and that the hill lift variation helps to explain the "sinking return." Nevertheless, the observed effect is greater than we can account for. There is almost certainly some other effect such as that due to the structure of the wind turbulence put forward by Irving and Glaser (Ref. 2.). I think that this is a powerful effect—for the following reason.

When the wind has a component along the ridge as well as normal to it, the hill lift (due to the normal component) permits slope-soaring, but the ground speed of the glider along the slope is much less in one direction than in the other. I have noticed on almost every such occasion that the hill lift is appreciably greater when flying "into" wind than when returning "downwind" through the same point. In other words, height is gained on the slow, upwind beat and lost on the downwind dash. Since the flight is made in almost a fixed position relative to the slope, the effects (b) and (c) above should be absent and any difference must be due to this other turbulence effect—or to something yet unexplained.

References

1. R.S. Scorer: The pressure field in front of a hill. *Gliding*, July 1951.

2. Irving and Glaser: Reported in Gliding Notes, *The Aeroplane*, 13th April 1951.

Dr. Scorer replies

MAY I reiterate that the "sinking return" measurements were based only on comparison of the altimeter reading with the actual height of the glider obtained from theodolite observations. The variometer and actual rate of sink did not come into it. The thesis is that the pilot thought he was higher than he really was when he was out in front of the hill, and so when he got back he ended up lower than he expected. The variometer would, of course, corroborate the erroneous altimeter. If we believe Bernoulli (and I do) then Nifterick's' theory, which I described, shows that in order to obtain the "sinking return" the pressure would have to be lower near to the hill and the wind stronger. In fact there was higher pressure, no evidence of a stronger wind, near to the hill.

Since we believe Bernoulli's theorem we must conclude that it cannot be applied in this case, because the flow is not potential flow. If it were, no-one would ever get lift to the heights above hills that they do, e.g. to four times the height of the hill in a 30 m.p.h. wind. Yates' argument (which has also been sent to me, complete with calculations to show that the effect is not big enough, by Flt. Lt. B. W. Plenderleith) should really use a point source (not a line source) to get a better representation of the hill at Prestatyn; and this would render it less effective, for the vertical motion would be much less.

As for flying parallel to a ridge when the wind is not blowing over it at right angles, I do not believe there would be any sinking return if the ridge were infinitely long. I don't think turbulence has anything to do with it, and since the velocity gradients are too small to produce any effect, I must conclude that horizontal pressure gradients are responsible for the apparent difference in lift when flying up and down wind with the same air speed in the same place.

The September Expedition

Though we have not fully worked out and interpreted the results of the September expedition to the Clwyds, one or two points of interest have emerged. The altimeter is not a good instrument. Four instruments were calibrated, and all showed a periodic error of up to 30 ft. The period was 1,000 ft and so it must have something to do with the gearing or the balance of the needle that rotates once in 1,000 ft. This means that if the altimeter is correct at 1,000 and 2,000 ft. it can easily be 30 ft. out at 1,500 ft.; 30 ft. of the error found at Easter can thus be explained away, leaving only 60 ft. or so for certain. The altimeters also showed a lag of up to 30 ft., so that if you are descending fast, it may read 30 ft. too high. If the rates of descent are big enough to produce lag, then the lag will be bigger on the return (when you get most red ball) and so the sinking return effect is, if anything, bigger than observed. We therefore hope to do the experiments with much better instruments in the coming year.

The potential accuracy of the observations was not fully exploited. With a large number of observations, errors of levelling of the theodolites can be eliminated. In fact the error of the altimeter as an instrument for measuring pressure was between 5 and 10 times the other errors encountered in making the observations.

The observations worked out at the time of writing show much smaller altimeter errors than on the occasion described at Easter, amounting to 15 to 30 ft., but soaring conditions were, on the whole, not nearly as good, until the last day, when there were waves about, but there was too much low cloud for any series of observations to be made above cloud base.

R. S. SCORER.

Safety High

II—The Oxygen Story

by Aesculapius.

ONE day when I was gaily wandering through the merry throng of enthusiasts at the Nationals 1951, my attention was drawn to a small crowd closing in round one of the cockpits. Naturally inquisitive, I could not resist a closer inquiry into the cause of interest. They were examining the cockpit layout of one of the competing sailplanes.

Of particular interest was the instrumentation and the oxygen layout. As I was peering over somebody's shoulder, a small boy pushed his way out from beneath me crying out: "'Ere mister, come 'ere. Wot's this thing 'ere for, eh?" He was pointing to the oxygen installation. The pilot—for I suppose it was he, a tallish, rather suave and soft spoken gentleman—yielded to the insistent youth. "That?" said he in a dejected manner, "Oh, *that* I use to keep me consistently lower and slower than anyone else, and to ensure without exception the quickest and slickest glide to the bottom." "Oh!" said the boy, and he walked away with a puzzled expression on his face.

The Sweet Disaster

Nevertheless, oxygen systems are not solely designed to keep one at ridge top level (or below). Their main purpose is to keep one up, and safer it is by far to sink inadvertently down to the bottom with oxygen on board, than to rise inadvertently up without it. For to be starved of oxygen is as lethal as being starved of food or water. Worse still, in fact, for whereas food and water can be stored by the body, oxygen virtually cannot. So that oxygen starvation makes itself known immediately it occurs.

"Makes itself known?"

Well, no. This is a misstatement, for characteristically it is just what it *doesn't* do. You may ask, "What does happen then?" In answer, I cannot do better than quote an account given by Tissandier in 1875 after that fateful balloon ascent in which his two companions lost their lives for want of oxygen. He writes:—

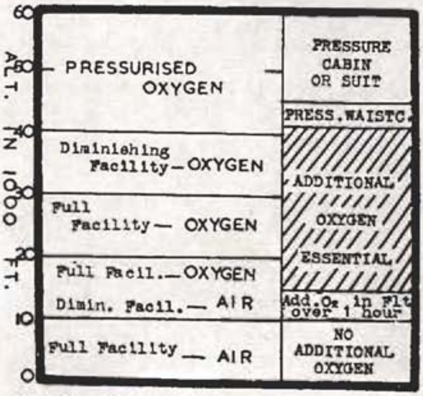


Fig. 1—Diagram to illustrate the roughly equivalent altitudes breathing oxygen and air respectively.

". . . . At 24,600 feet the condition of torpor that overcomes one is extraordinary. Body and mind become feebler, there is no suffering. On the contrary one feels an inward joy. There is no thought of the dangerous position; one rises and is glad to be rising. I soon found myself so weak that I could not even turn my head to look at my companions. I wished to call out that we were now at 26,000 feet, but my tongue was paralysed. All at once I shut my eyes and fell down powerless and lost all further memory."

Where it Happens

Figure 1 gives a pretty fair indication of the relevant altitudes. Notice that the atmosphere is divided into three different blocks. In the lowest block the human frame can cope without additional oxygen. In the middle block, additional oxygen is necessary; and pure oxygen supplied under pressure is essential in the third block, or upper atmosphere.

Without Additional Oxygen.—Above the 10,000 foot level, measurable changes begin to occur in the average individual. These do not interfere unduly with normal ability, though they reduce tolerance to long-term endurance tasks, and interfere with special feats of mental acuity.

It is not until the 15,000 foot level that the subtleties of oxygen lack (or anoxia as it is called) really begin to creep in. Here the average pilot cannot detect any marked defect, though an independent observer would notice a number of changes. He would notice a reluctance to persist with any particular task, and a tendency to discard any opportunity which might demand special effort. Instrument flying becomes less accurate, decisions are less easily made, and a progressive lack of responsibility sets in.

There is, however, a large scatter in resistance of individuals to oxygen starvation, and precise figures, perhaps misleading, are only quoted as a matter of convenience. For at the lower end of the scale are those who will eventually lose consciousness just above 15,000 feet, and at the upper end a few who can react more or less coherently up to 28,000 feet. But to all intents and purposes, unless he is well acquainted with his own tolerance, a pilot should assume he will become dangerous above 15,000 feet, and certainly not consider achieving 20,000 feet, unless equipped with oxygen.

With Additional Oxygen.—Suppose you are equipped with oxygen: what are the relevant altitudes now? Well, somewhere between 10,000 and 15,000 feet the supply should be turned on. If we take the instance when pure oxygen only is breathed, then below 30,000 feet you are, as it were, better off than breathing air at ground level. That is, with each breath, the lung takes in a greater weight of oxygen than it would at ground level breathing air, though as it happens this is no advantage, since the body cannot make use of the excess.

But just above 30,000 feet, even pure oxygen begins to be inadequate. The deficiency becomes progressively worse with altitude, until at about 40,000 feet it again becomes marginal. In fact, 40,000 feet breathing oxygen is equivalent to about 15,000 feet breathing air. None the less, don't be lulled into a false sense of 40,000 feet security by this, for remember always that pure oxygen can be diluted with air, making mask leakage a serious hazard,

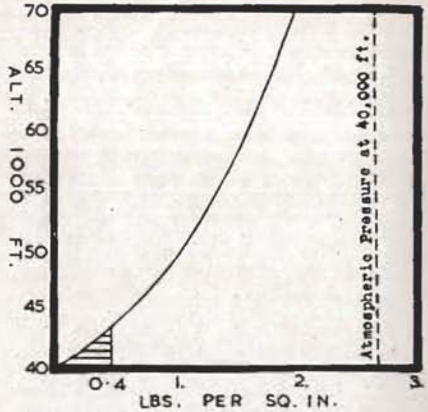


Fig. 2—Graph relating altitude to the extra pressure which must be applied to the inhaled oxygen in order to sustain consciousness. Simple mask pressurisation can be tolerated in the cross-hatched area only.

whereas at 15,000 feet breathing air there is no such risk. Moreover, failure of oxygen at 40,000 feet leaves a long, long stretch of feet before reaching an altitude at which air alone will support consciousness.

With Pressure.—Those who aspire to world altitude records must grasp yet another series of figures. For above 40,000 to 42,000 feet, pure oxygen is the order of the day. Figure 2 illustrates graphically the excess pressure required above 40,000 feet to reduce the equivalent altitude back to this value. For example at, say, 50,000 feet, pure oxygen must be supplied at a pressure of 1.0 lb./sq. in., in order to bring the pilot back to an equivalent of 40,000 feet.

As it happens, the body cannot stand up to a pressure of more than about 0.4 lbs./sq. in. (20 mm. mercury) without it being uniformly applied all over. The reason for this we shall consider below. A limit of 43,000 feet to 44,000 feet is thereby imposed when simple local means of pressurisation are used. Above these altitudes full coverage pressure suits or pressure cabins become an absolute necessity.

Such in brief are the simple findings, disregarding until a later issue those other phenomena associated with high altitudes. It now behoves us to question . . . .

Why it Happens

Pressure is the crux of the matter here. A supply of oxygen alone is not enough to keep the body tissues alive. It must be presented to them at an adequate pressure: that is, with an adequate driving force behind it. The optimum pressure is that exerted by the oxygen in the atmosphere at ground level.

What is this pressure? It is, of course, one fifth of an atmosphere, since air, broadly speaking, is four fifths nitrogen and one fifth oxygen. Where the total pressure is halved (i.e. at about 20,000 feet), the pressure exerted by each constituent gas must be halved also. At this altitude, then, the partial pressure of oxygen (as it is called) is one tenth of an atmosphere, a value too low for the efficient maintenance of life.

Of course, the oxygen can be brought back to its optimum value of one fifth of an atmosphere by casting out some of the nitrogen and supplying extra oxygen in its place. Glancing at Figure 3, you can see

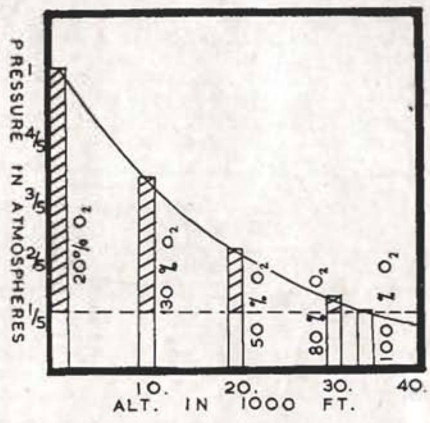


Fig. 3.—Diagrammatic graph to show how the relative amount of oxygen in the inspired air must increase with altitude. Notice that above 33,000 feet even pure oxygen cannot maintain the optimum partial pressure. The unhatched areas represent partial pressure of oxygen: the cross-hatched areas partial pressure of nitrogen.

that at this altitude (i.e. 20,000 feet) enough must be added to bring the oxygen: nitrogen ratio up to unity. The figure illustrates how the "vital quintessence" (viz.: oxygen) can be maintained at its optimum partial pressure by progressively increasing the ratio of oxygen to nitrogen in the gas mixture that is breathed. You can also see how, at just over 30,000 feet, the *total* pressure has fallen to one fifth atmosphere, leaving no room for nitrogen at all.

Moreover, above this altitude, however pure the oxygen, it will not keep the body up to scratch. At just above 40,000 feet, it won't keep it going at all! As pointed out by Robert Boyle way back in the middle of the 17th century, "Air too much dilated is not serviceable for the ends of respiration, the hasty death of the animal we killed in our exhausted receiver seems sufficiently to manifest."

Limitations of Pressurisation.—So from 33,000 feet onwards, only a pressurised supply of oxygen can keep the partial pressure in the lungs up to its optimum value. Although this can be done by a rather complex mechanism, it is not usually employed. The oxygen systems in current use do not attempt to do so, with the result that one begins to sacrifice bodily oxygen saturation above this height. The deficiency becomes critical around the 40,000 feet mark. Between 33,000 and 40,000 feet light pressures would be an asset, but above 40,000 feet pressure becomes essential.

"Surely," you may say, "pressurisation should be easy enough." Well yes, if it is looked at from a mechanical viewpoint. But if you simply apply the pressure to the inside of an ordinary mask, it leaks. "All right," you say, "design a leak-proof mask." Well then, as the pressure rises, remember it only rises in the lungs (i.e. chest). All the great veins of the body return, of course, to the heart, which lies in the chest. These veins have to pass through the chest walls. The blood they contain has to flow, for instance, from the abdomen, through the diaphragm (which separates chest and abdomen) into the chest. Since only the chest will be supplied with pressurised oxygen, there must be a pressure difference between chest and abdomen. This difference tries to force the blood in the great veins back into the abdomen whence it came. Comes the time when there ain't no flow left; the heart gets no blood; got no blood, can't pump; can't pump, gives up the ghost!

What then?

Well, this dilemma is solvent until about 0.4 lbs./sq. in. (or 20 mm. Hg.) is reached, and that takes you up to about 43,000 to 44,000 feet, as we have already seen. Higher than this you cannot go without complete bodily pressurisation, either by pressure cabin, or, what is virtually a tailored pressure cabin, namely a pressure suit.

Aspirants to world altitude records, don't be discouraged—but *beware*.

Practical Issues

Summarising relevant data, we have seen that above 10,000 feet you should be oxygen-conscious; above 15,000 feet definitely turn your oxygen on; above 20,000 feet, make certain the contents gauge is behaving as it should—confirm, in fact, that there are no leaks; above 25,000 feet, consider turning a Mk. XI regulator to "high"; above 30,000 feet, remember your faculties will progressively deteriorate until 40,000 feet is reached; above this pressure must be applied. At Fifty Grand, the full pressure or bust!

I well remember the occasion of a high-ranking official approaching a decompression chamber with considerable complacency. "First World War, no oxygen; all nonsense, never touch the stuff." But I remember more vividly the same officer half an hour later! In fact the key to it all is, *don't be complacent*.

At relevant altitudes keep a constant check on your mental faculties. You can watch your own reactions to a situation. In the early stages of anoxia, you can recognise yourself growing over-confident. Constantly check and cross-check your instruments, to test your powers of correlation. Keep a watch on any writing or figures in the cockpit, for these change shape, or turn upside down, or simply become unreadable. If you have a passenger remember he can't tell you when he is unconscious. Above all *do* something when anything appears wrong and do it quick, whether you understand the fault or not.

Naturally, when you have tried yourself out, and know your limits (for these are very variable from person to person), you can play your cards more accurately. Remember after all that the British height record was achieved without oxygen, at over 20,000 feet. But the pilot was experienced in high-altitude flying. On this occasion he hit lift which was very strong. All the way up he was checking and cross-checking his faculties, and at 20,000 feet, although still going up fast, the great decision was made, the time had come, and down he came at maximum rate of descent.

When aiming high, remember anoxia is a feline creature, who shows great subtlety in her ways. She caresses you and comforts you into a fine elated sense of security and confidence. She seduces you on to hopeless

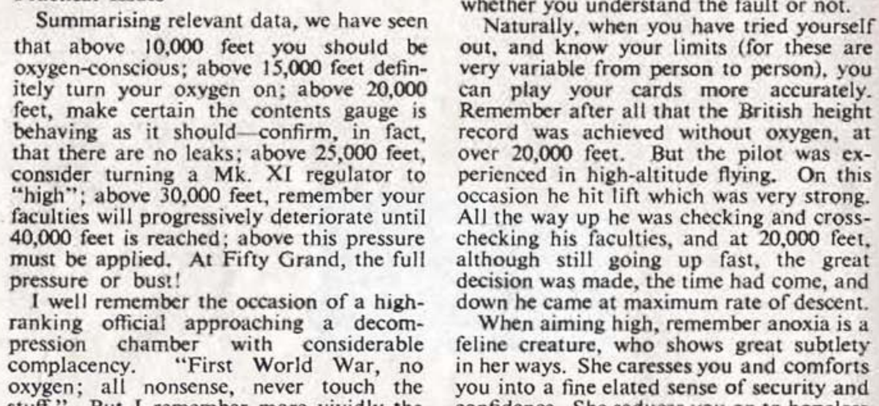


Fig. 4.—A simple continuous-flow oxygen installation (diagrammatic).

achievement, and then . . . . fiendish is the ensuing fall, and none but the fates can decide the outcome thereof.

Appendix

The majority of oxygen systems come under one of two headings:

I. Continuous flow system.
II. Demand regulator system.

I. Continuous Flow System

Nearly all British installations are of this kind. A typical instance is illustrated in Figure 4. Basically, there is nothing more than a high-pressure storage and supply line leading to a regulator. The regulator is a simple reducing valve, bringing the high-pressure oxygen down to a manageable value of about 40 lbs. per sq. in. A copper capillary tube of fixed dimensions allows the oxygen to flow out from the 40 lbs./sq. in. reservoir to the medium-pressure tube leading to the economiser. The principle of this latter is simply a bag big enough to store oxygen during expiration, thus adapting a continuous supply to the intermittent requirements of respiration. If this "bag" is attached to the mask itself, the special requirements of a remotely placed reservoir are not in evidence and it can take the form of a simple rubber bag.

STORAGE CYLINDERS which are available are as follows:

1. 750 litre N.T.P. (normal temperature and pressure) at 1,800 lbs./sq. in. (15 lbs. weight approximately).
2. 150 litre N.T.P. at 1,800 lbs./sq. in. (9 lbs. weight approximately).
3. 75 litre N.T.P. at 1,800 lbs./sq. in. (5 lbs. weight approximately).
4. 55 litre N.T.P. at 1,800 lbs./sq. in. (3 lbs. weight approximately).

HIGH-PRESSURE TUBING is of annealed copper, 3/16 in. o.d. × 22 standard wire gauge thickness.

REGULATORS are numerous. The most satisfactory series is the Mk. XI. These have only two flows, below and above 25,000 feet. Two capillary jets are arranged in parallel, and the switch selects either one (below 25,000 feet) or both (above 25,000 feet). Each jet allows approximately $2\frac{1}{2}$ to 3 litres (N.T.P.) per minute flow at N.T.P. This figure makes duration calculations possible.

The Mk. VIII and X series of regulators have a continuously varying flow control.

MEDIUM-PRESSURE TUBING is of aluminium alloy, 5/16 in. o.d. × 22 s.w.g.

Low pressure flexible tubing from economiser to mask should be wide-bore 5/8 in. i.d. and not more than 9 feet long.

THE R.A.F. H. TYPE MASK is undoubtedly the safest to use.

II. Demand Regulator System

The U.S.A. have adopted the system whereby part of the energy imparted by your lungs in the inspiratory process causes a sensitive tap to turn the supply on and off at the mains. Such a system requires a number of refinements which make it far more complex than the continuous flow system. For use by amateurs, the disadvantage incurred by increased complexity far outweighs any advantage, which is negligible except in specialised instances of service usage.

III. Pressurisation

Both systems can be pressurised. The continuous flow system requires the economiser to be by-passed, and a pressure waistcoat to be installed in its stead. The demand system requires spring forces to be applied to the diaphragm which controls the pressure of the outcoming gases. In either instance, special close-fitting leak-proof masks must be worn.

Pressures in excess of 0.4 lbs. per sq. in. cannot be used for physiological reasons considered in the text.

IV. Notes of Interest

The ideal glider oxygen storage unit would be a light alloy sphere, wire wound, containing oxygen at 2,000 lbs. per sq. in., with capacity of 300 litres at N.T.P. This would weigh approximately 4 lbs. There is no such unit available at present.

The German 200-250 litre capacity high-pressure light alloy cylinders are weight for weight similar, or slightly worse than the R.A.F. 750-litre bottle.

It would be as well to note that the plastic type mask, with rubber bag attached as economiser, was designed for use in transport machine cabins. They are liable to obstruct with frozen moisture condensed from expired air, at temperatures of —5°C or lower.

Finally, bear in mind the importance of making the weighty elements of an oxygen system very easily detachable in order that weight can be reduced to a minimum when conditions are marginal.

Through Waves by Jet Aircraft

by P. G. Mallett

DURING the past few months I have been living a life of forced hibernation from soaring up in North Yorkshire. However, the ever-vigilant eye and ear have been alert for future soaring possibilities in this area—my first interest, of course, being waves over the Pennines and secondly the build-up of cumulus and cumulo-nimbus clouds along the length of this range. The latter makes an interesting study as a Soaring Map, when viewed from 30-40,000 ft.

The wave—one of the British variety!—which we know must exist in favourable conditions over and in the lee of the Pennines, eluded my grasp until Friday, 12th January, 1951.

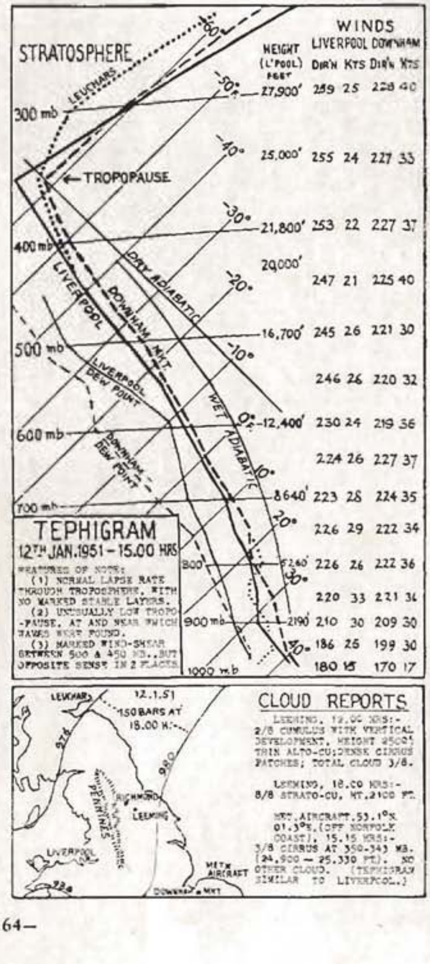
That day, at 15.10, I climbed rapidly into wind towards the Pennines on 250°(M), passing through 4-6/8 stratocumulus at 2,000 ft. To the north were well-formed lenticulars, out of reach in Northumberland; ahead lay a few scrappy pieces of lenticular set in a blue sky and a lowering sun. At 20,000 ft. a dark shadow of cloud appeared in front of me (just as though I hadn't been looking). As I closed with it, an all too evident downdraught of minus 30 ft. per sec. tried its clutching hand. By this time I had taken stock of the situation, so continued on course, which crossed the lenticular at 90 degrees. Over the crest I was greeted with a smooth, wide area of plus 30-35 ft. per sec. updraught.

Upwind and ahead lay another lenticular at 3-5 miles, at the same height and approximately the same size, 10-15 miles long by ¾ mile wide. This I flew over at 25,000 ft., observing almost identical vertical currents as experienced over the first lenticular. Below, the cloud cover had increased in the lee of the Pennines to 8/8ths, apart from an ellipse situated directly beneath and between the two lenticulars. At this height it was impossible to obtain its exact position by pinpoint, the area being approximately 5 miles S.W. of Richmond, Yorks.

Much to my disappointment further exploration above and below the lenticulars had to be abandoned. Turbulence was not encountered throughout the flight.

As you can see from this report, which is far from conclusive, it should be possible to obtain great altitudes in a sailplane in this country. More research is required, however.

The flight was made in a jet aircraft travelling at high speed: this accounts for any lack of detail.



The Air Flow over an Extended Ridge

by August Raspet

(Engineering Research Station, Mississippi State College, U.S.A.)

Read before the third Congress of the International Scientific and Technical Organization for Soaring Flight (O.S.T.I.V.) and reproduced from its Publication No. 1.

Introduction

THE natural air flow over terrestrial obstructions is distinguished from aerodynamic flows over various, relatively small artificial shapes by the fact that the potential flow is rarely found in natural flows. Theoretical solutions for the two-dimensional potential flow over obstacles have been published by Pockels (1901) for a ridge defined by a harmonic function, Defant (1921) for a half-elliptic cylinder, Ackeret (1922) for a shape defined by a source in the uniform stream, and others. In Figure 1 is shown the solution of Pockels for an obstacle 50 metres high. Lines of constant vertical velocity are shown. The influence of the ridge is evident only in the immediate vicinity of the ridge. However, experimental explorations of natural flows (Wagner, 1926, Koch, 1927-28, Landsberg and Riley, 1943) have failed to yield the type of flows or the vertical velocities expected from the theoretical studies. Seeking a more complete description of natural flows,

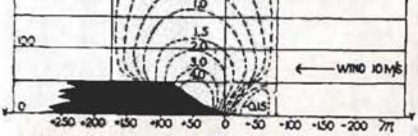


Fig. 1. Pockels' solution for the vertical component of flow over a ridge.

Raethjen (1926) included the effect of the lapse rate on the flow and arrived at the general differential equation for atmospheric flows over small regions assuming frictionless, vortex free, two-dimensional flow,

$$\nabla^2 \psi - C.\psi = O \qquad \text{Equation (1)}$$

where ψ is the stream function,

$$\nabla^2 \psi = \frac{\delta^2 \psi}{\delta^2 x^2} + \frac{\delta^2 \psi}{\delta z^2}$$

$$C = \frac{g}{U^2} \cdot \frac{1}{T} \cdot \frac{\delta \Theta}{\delta z} .$$

U is the horizontal wind at a great distance from the obstacle, Θ is the potential temperature, T the absolute temperature, z the altitude above sea level and g the acceleration of gravity. It is evident that equation 1 reduces to Laplace's equation, the case for potential flow when $C = O$ which is true only when $\frac{\delta \Theta}{\delta z} = O$, i.e., when an adiabatic lapse rate exists.

Examination of Raethjen's equation (Eq. 1) also explains why potential flow is rarely found in nature.

When $C = O$ the convective stability of the air is neutral and consequently any local heating by the sun will have a marked effect on the flow. A steady flow under this condition is hardly possible.

If, on the other hand, the lapse rate is stable, $C > O$, Raethjen (1926) found sinusoidal solutions of the type

$$\psi = \psi_0 \sin \sqrt{C \, x} .$$

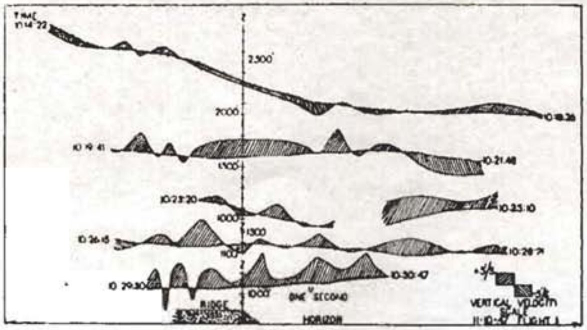


Fig. 2 Vertical velocity of wind over a ridge.

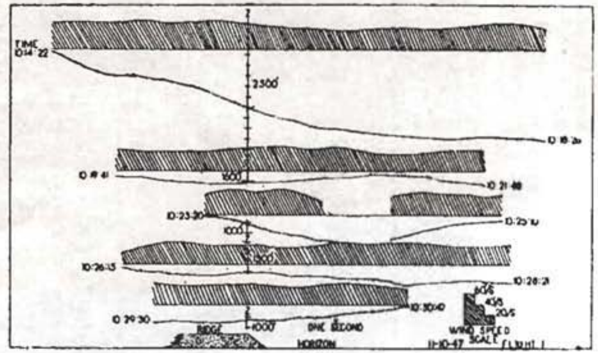


Fig. 3 Horizontal wind component on the ridge

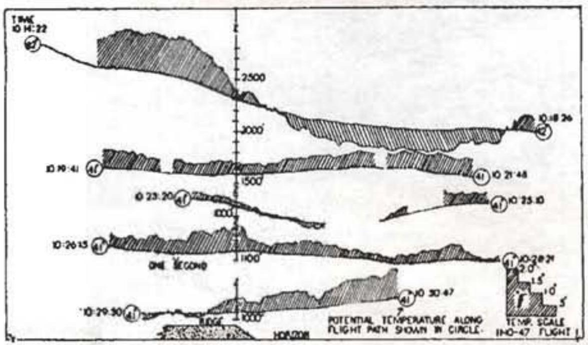


Fig. 4 Potential temperature along flight path.

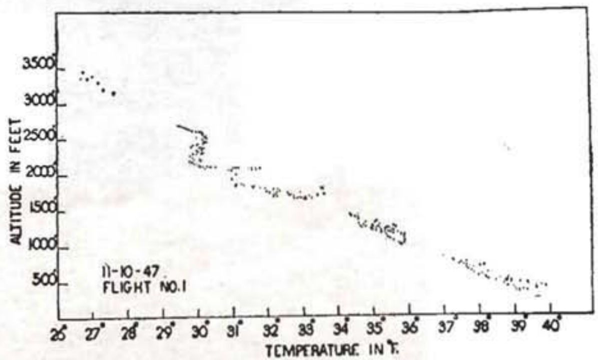


Fig. 5 Lapse rates over ridge.

where the period of the wave is

$$P = 2\pi \sqrt{\frac{T}{g} \cdot \frac{1}{\frac{\delta\theta}{\delta z}}} \cdot \quad \text{Equation (II)}$$

If the propogation velocity of the waves is equal to the wind speed the waves will be stationary. Lyra (1943) has published a solution for the flow of a stratified fluid over an obstacle showing a system of stationary waves to the lee of the obstacle as well as a general region of lifting over the windward portion of the obstruction. Recently Queney (1947 and 1948) has offered a linearized solution based on the theory of small perturbations not only for the gravity waves studied by Lyra but, also, for the inertial and long atmospheric waves. Queney used a factor which he called the coefficient of vertical stability "S" to define the stratification,

$$S = \sqrt{\frac{g}{\theta} \cdot \frac{\delta\theta}{\delta z}} \cdot \quad \text{Equation (III)}$$

It is evident from a comparison of equations (II) and (III) that $\frac{1}{P} \cong \frac{S}{2\pi}$. The wavelength of the stationary waves computed either from the period or the coefficient of vertical stability is, therefore, identical for either Lyra's or Queney's solution for gravity waves.

Experimental Research

With the foregoing theoretical background as a guide, the present study was begun in order to determine if the available theory could predict the type of flow which would take place under the ambient temperature and wind fields. As an approach to an idealized natural obstruction, a terminal morain facing the northwest located on the north shore of eastern Long Island was chosen. The base of the terminal morain is on the Long Island Sound with the plateau about 50 metres above sea level. The slope of the dune is the natural angle of recline of sand, about 30°. Since the ridge is comparatively long, a two-dimensional flow can be expected. The passage of the wind over the smooth and nearly isothermal surface of Long Island Sound yielded a homogeneously stratified atmosphere in most cases.

The apparatus used for exploring the natural flows consisted of a sailplane (Raspet, 1948) equipped with a cine' camera for photographically recording the barometric altimeter, a variometer measuring rate of climb, a low-lag thermometer and a clock. On the ground a mapping of the position of the sailplane synchronized with the recorded data from the sailplane permitted the vertical and horizontal wind fields as well as temperature fields to be plotted along the traverses made by the sailplane. By restricting the sailplane to a plane normal to the ridge and in the surface wind direction, it was possible to map accurately the two-dimensional flow field as well as the ambient wind and temperature fields. In Figures 2, 3, 4, and 5 are shown the various fields made by using the sailplane as a meteorological probe.

Description of the Various Flow Fields Over a Ridge

In Figure 2 is shown the vertical velocity of the air flow over the ridge taken on November 10, 1947. It should be mentioned that the lower two traverses are displaced vertically in order to eliminate confusion. However, the true vertical co-ordinates are shown for each traverse. The flight path of the sailplane is shown as the base line on which are plotted the values of true vertical velocity of the wind. Time intervals of one second are displayed along the flight path. Study of this flow field shows a long wave on the highest traverse and directly below it a wave of nearly equal wavelength but reversed in phase with respect to the upper wave. Superimposed on the waves and predominant on traverses below them are turbulence waves of much shorter wavelength. That the flow is not steady is evident from a comparison of the lower three traverses where within five minutes the flow is seen to change markedly. However, the ridge lift is apparent in all three traverses.

Returning to a consideration of the longer waves in the two uppermost traverses

Table 1 Characteristics of Air Flows

Date	Flow	Wind	$-\frac{\delta \Theta}{\delta z}$	T comp.	T obs.	λ comp.	λ obs.	Z
9-30-47 9.30-47	Wave Wave	17 M/S 17	2.45° C/km 2.45	680 sec. 680	111 sec. 23	11.5 km 11.5	1.8 km 0.36	0.7 km 0.24
11-10-47	Wave	14	2.35	690	60	9.7	0.89	0.65
12-6-47-1	Wave Ridge lift	16 16	6.02 2.16	428 715	57 —	6.8 11.3	0.53 —	0.50 —
12-6-47-2	Ridge lift	15	2.37	680	—	10.4	—	0.1
12-13-47-1	Turbulence Waves	4-8	2.16	690	10.20	2.52	0.06-0.2	0-1
12-13-47-2	Waves	3	1.95	748	10-20	2.28	0.05-02	0-1
8-8-38 Ref. 11	Wave	8-10	—	—	40-50	—	0.4	0.1
5-21-37 Ref. 12	Wave	20	—	—	350	—	7.0	2-5
Ref. 13 + 14	Wave	25	4.28	530	500	13.3	10-15	2-4
Ref. 13 + 14	Wave	20	1.0	1130	2000	22.3	40	6-11

one may compute the amplitudes of the waves from the simple relation

$$A = \frac{w}{U} \frac{\lambda}{2\pi} \qquad \text{Equation (IV)}$$

where w is the vertical velocity of the air motion and λ is the wavelength.

The amplitudes are found to be 50 feet and 33 feet reading from the uppermost traverse down. Evidently from this computation the waves are spaced sufficiently that they do not interfere with each other. It is entirely possible then for them to be reversed in phase.

Figure 3 shows the horizontal winds during the same exploration. Some small variations in wind are present. However, these variations cannot readily be ascribed to the wave motion. The strong anomalies present in the potential temperature field are displayed in Figure 4. The wavelike variation of potential temperature in the upper traverse agrees in wavelength with the vertical flow field. However, no valid conclusions in this respect can be drawn without a better knowledge of the true nature of atmospheric flows. Whether the temperature variations are the cause or result of the wave motion must yet be determined by more refined theoretical and experimental analysis.

In Figure 5 the temperature of the air is displayed as a vertical sounding. The anomalies apparent in Figure 4 as due to horizontal gradients appear as projections from the mean lapse rate of the air. These projections are most prominent at the altitude where the waves occurred. The mean lapse rate obtained from each flight was used as the basis for computing the period of the gravity waves.

Another flow field showing waves is that illustrated in Figure 6. On the two traverses appear two prominent lift regions which might be attributed to convection. However, a computation of the drift of this flow from the lower traverse to the upper proves that the lift is not due to convection. The disturbance is attributed to the ridge and is evidently stationary. In this analysis waves are divided in two classes, the stationary and those moving with the wind. To the latter class belong the short turbulence waves. With two sailplanes probing the flow, following each other by a fixed time interval, it would be possible to filter out all of the non-stationary disturbances. In the flow of Figure 6 the waves on the two traverses were found to be 0.36 and 1.8 km. in wavelength. In Figure 7 a long wave occurs in a layer of very stable air having a potential temperature gradient of 6.02 degrees C/km.

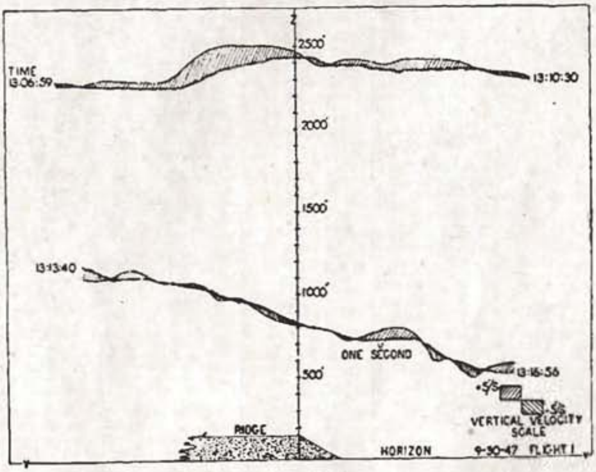


Fig. 6 Vertical velocity of wind over a ridge.

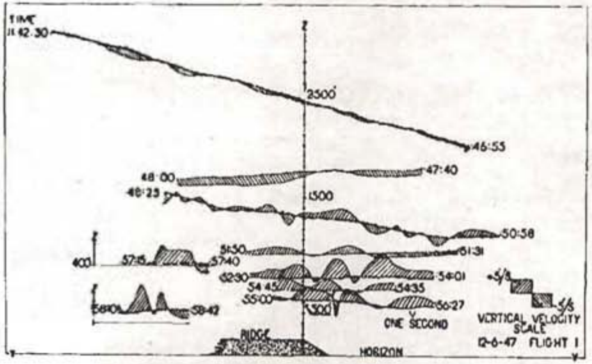


Fig. 7 Vertical velocity of wind over a ridge.

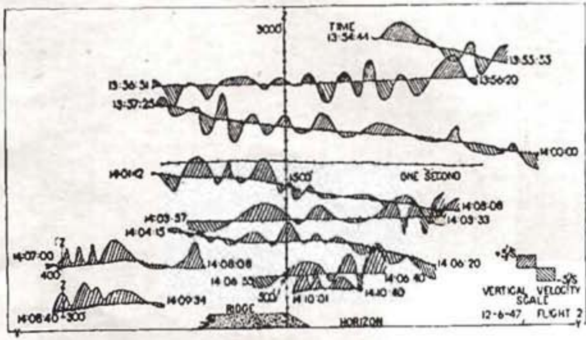


Fig. 8. Vertical velocity of wind over a ridge.

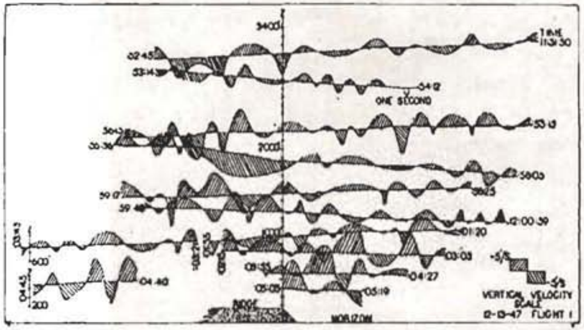


Fig. 9 Vertical velocity of wind over a ridge.

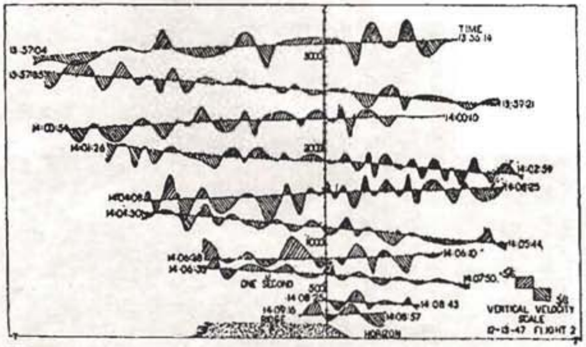


Fig. 10. Vertical velocity of wind over a ridge.

Strong ridge lift seen to be wavelike in character, extends some distance windward of the ridge. This same behaviour is apparent in a later flight on the same day as that in Figure 7. Since neither Lyra or Queney explain the existence of such waves before an obstacle, further research appears advisable in order to determine the nature of these windward waves. It is interesting to remark that F. Ringleb has demonstrated mathematically the symmetry of solutions to Raethjen's equation for a semi-cylindrical obstacle. Ringleb thus finds waves before the obstacle.

The flow fields of Figures 9 and 10 are shown to illustrate the turbulence waves in a stable atmosphere in a comparatively weak wind. The flight paths are seen to be nearly straight lines sloping downward at the glide angle of the sailplane. The small oscillations of vertical velocity are obtained from a differentiation of the theodolite plot of the track of the sailplane. By this method the short period oscillations can be measured without their being damped by instrument lag.

A summary of the data obtained in this exploration as well as some data from the literature (Klanke 1938, Kuettner 1939, Georgii 1947, and Krug-Pielsticker 1942) is shown in Table 1. It is apparent that the potential temperature gradient is nearly identical in several of the flow fields yet the nature of the flow is extremely different. According to Raethjen's theory the temperature field should be expected to exercise the principal control in determining the flow field. If two examples are selected, those of Figures 2 and 8, which have a nearly equal horizontal wind and nearly identical lapse rate, the flows are found to be quite dissimilar. In other words, this data demonstrates that the Raethjen equation is not sufficient to define the flow in the realm explored in this study, that is, up to about one kilometre above the surface.

Examination of the flow data plotted in Figure 11 shows that the period of the waves increases with the altitude. That gravity wave theories are not sufficient to cover this behaviour is shown in Figure 12 where the ratio of the observed period of the waves and that computed from the existing temperature gradient is plotted

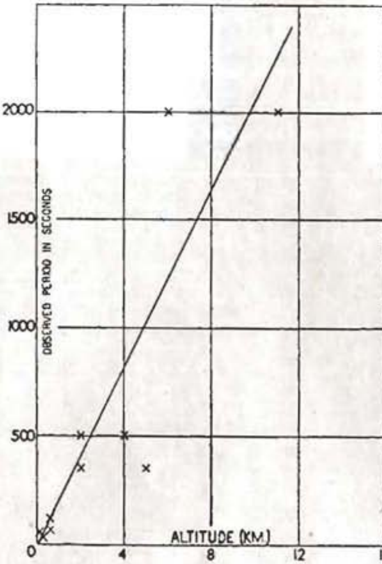


Fig. 11 Period of atmospheric waves vs. altitude.

against altitude. In the level 1 to 4 km, the gravity wave theory holds true, outside this region the theory breaks down.

When the observed wavelengths from Table 1 are plotted logarithmically against

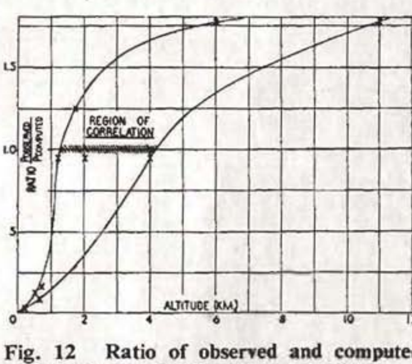


Fig. 12 Ratio of observed and computed periods of atmospheric waves vs. altitude.

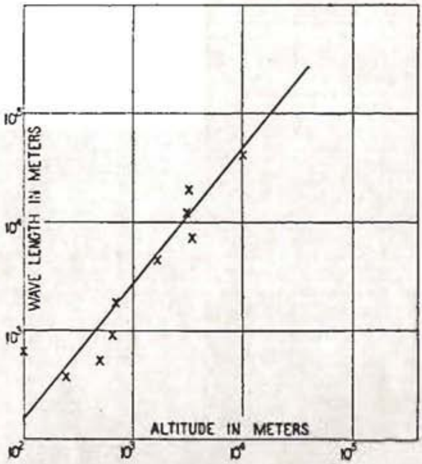


Fig. 13 Wave length vs. altitude.

altitude, Figure 13, the wavelength is found to be given empirically

$$\lambda = K \cdot z^{1,7}$$

where K is the constant of proportionality.

This experimental evidence thus shows that the so-called gravity wave theory should include an air density term. The theoretical airflow patterns determined by the linearized flow equations of Raethjen, Lyra, and Queney, are not, therefore, sufficiently rigorous to explain either high altitude waves or those near the ground. Of interest is the comment by Hess and Wagner (1948) that the observed height of nodal planes showed a discrepancy of 100% over that computed at an altitude of 24,000 feet. From Figure 12 the ratio of $\frac{P\ obs.}{P\ comp.}$ at this altitude is about 1.8 which is in rather close agreement with the result of Hess and Wagner.

As part of this project Ringleb (1948) has attacked the theory of atmospheric flow from a more general viewpoint deriving a following differential equation

$$\nabla^2 \psi + \frac{1}{2}\left[\frac{d}{dz}\left(\frac{\rho'}{\rho}\right) - \frac{1}{2}\left(\frac{\rho'}{\rho}\right)^2\right] \cdot \psi = 0$$

where ρ = air density and ρ' — density gradient with respect to altitude, so that $\rho' = \frac{d\rho}{dz}$. It is evident that this form does include the air density in the term from which would be computed the period or wavelength. However, considerable more study is required to reduce the equation to a form by which its validity may be tested experimentally.

Sekera (1938) has also studied the problem of atmospheric wave motion in a more general manner by including the effect of a vertically changing wind field. Waves have been reported by Ross (1948) under conditions when the wind increased from 0 at the ground to 96 m.p.h. at 25,000 feet. Under such a wind field theory based on a uniform wind cannot be used.

Conclusion

This study has aimed at correlating experimentally determined atmospheric flow fields against available theories for air flow over natural obstacles. As a result of these attempts it may be stated that:

a. From the differential equation of micrometeorological flow of Raethjen, it was not possible to define the type of flow from the parameters included in the theory.

b. For the wave-like airflows, wavelengths computed on the basis of short gravity waves were found to agree with experimental results in the level 1 to 4/km only.

c. The theories of Lyra and Queney predict waves to the lee of the obstacle. In the exploration waves were also found windward of the ridge.

The theories of Ringleb and Sekera appear worthy of experimental test. For the Ringleb theory a special instrument which measures air density directly will be needed. The physical basis for such an instrument is available by utilizing the aerodynamic properties of the sailplane. The extension of the exploration to a larger ridge is indicated by the experiences of this experiment.

The author wishes to acknowledge the support furnished this project by the Office of Naval Research. He is most grateful to the U.S. Navy Bureau of Aeronautics for the services of Drs. A. M. Lippisch and F. O. Ringleb who consulted in this research.

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"OSTIV" Publication No. 1

AN event of note is the issue of the first publication of the International Scientific and Technical Organisation for Soaring Flight, whose initials, in its official French title, spell O.S.T.I.V. With an Introduction by the President, Mr. L. A. de Lange, it contains six papers read at the congress held in Sweden during the International Championships in 1950.

The two longest are by Dr. A. Raspet: "The Air Flow over an Extended Ridge," which is reproduced in the present issue of GLIDING, and "Performance Measurements of a Soaring Bird," which was published in our Autumn issue of 1950. In each, Dr. Raspet shows how sailplanes have been used as a tool for scientific research, sponsored by the Engineering Research Station of Mississippi State College. The other articles are summarized here.

Ways to Record Performances in Soaring—This paper, written in German with a short English summary, is by W. Jucker, member of a small private group in Switzerland called "Segelflugforschung Alp Scheidegg." This body was formed in 1945, with funds from a private source, for research into wave soaring.

One of the problems in setting up a record for gain of height is to start from as low a level as possible. The author describes a flight he made with Siegbert Maurer in a Moswey 6 near Zurich in April, 1950, when they released at 8,690 ft. and climbed up in front of three superimposed rotors, gaining 3,300 ft. thereby. They then reached smooth wave lift and continued the climb to 17,880 ft. Jucker reckons that they could have released at only 5,250 ft., and says that the upper border of the highest foehn cloud appeared to be at about 43,000 ft., adding that other flights have proved it possible to climb above such a wave cloud.

He then discusses examples of wandering waves, one of which he investigated with a Piper Cub near Zurich in January, 1948. As soon as the cloud-wall of a local cold front reached it, the wave cloud began to move with the wind. He and his companion were lifted at 1 metre per sec. through the wave cloud to 1,600 feet above its top. He also observed moving wave clouds above a cold front from the Weissfluhjoch on the day of the triangular race at Samedan (see GLIDING, Summer, 1950, p. 83). The author concludes by suggesting that, since frontal systems move over long distances, their accompanying waves could be used to cross the Atlantic and other seas by sailplane.

Use of the Link Trainer for Preliminary Training in Blind Flight for Glider Pilots.—F. W. Ledermann describes experiences in Switzerland. One must take account of the glider's lack of full equipment by, for instance, putting the A.S.I. out of operation as if it is iced up, or by switching off the gyro compass and using the magnetic compass only. The pupil should have had 30 hours' glider flying before doing 6 hours on the Link, first without the hood and then with it. The Link course must be followed by two-seater flights with the pupil under the hood. Excellent results have been obtained in Switzerland by these methods.

Remarks on Glider Design and Related Subjects.—L. L. T. Huls begins by describing a 16-metre high-performance sailplane which it is hoped to build in quantity in Holland. With a span of 16 metres, it has an NACA wing-section which is expected to provide laminar flow to 40% chord over a range of lift coefficients from 0.3 to 1.1. It is claimed that, if the required smoothness can be achieved, the performance should equal that of the Weihe over the speed range in which laminar flow is effective (60-100 km/h). The sailplane is built of wood except for the front fuselage, which is of welded steel tube for better "crash-worthiness."

Flutter investigations on the rudder of the Fokker-built Olympia type are discussed, but they are not yet completed. Winch launches with c.g. hook are also under investigation, and some winches in Holland have been fitted with dynamometers, but it is suggested that these instruments are apt to distract the winch driver's attention.

Comparison Flight Tests of Orao II and Weihe.—A Raspet and Boris Cijan report on these tests, carried out at Oerebro during the 1950 Championships. The Weihe was that flown by Paul MacCready, which had been somewhat "hotted up."

The minimum sinking speeds were found nearly identical at 58 cm. (22.8 ins.) per sec. At 42 m/s (94 m.p.h.) the Orao had a sink 30 cm/sec (11.8 ins. per sec.) less than the Weihe; this, however, would have been reduced to a difference of 10 cm (4 ins.) if both machines had the same wing loading.

It is to be hoped that this volume will be the first of many.

A.E.S.

Prize Competition in Meteorology

PRIZES have been allotted by Dr. R. S. Scorer to the winners of this competition, announced in our last issue on page 104, as follows:—

1.—£5 to R. D. Roper for essay: "Evening Thermals "

2.—£2 to A. J. Fyfe for "Lee Waves of the Ochils."

3.—£2 to G. O. Smith for "Derbyshire Evening Thermal."

4.—£1 to O. W. Neumark for "Standing Waves at Dunstable."

5.—£1 to G. H. Lee for "Dunstable Standing Wave."

Competition for 1952.—Another competition on the same lines is announced by Dr. Scorer. A prize will be given for the best description of a convection phenomenon experienced during the period January-August 1952: entries to be sent in by 10th September, 1952.

The prize will be awarded for the entry which adds most to our knowledge and understanding of thermals: reference to the questionnaire published in the last issue of GLIDING, page 108, will suggest the kind of information wanted. Full details, including the amount of the prize, will be published in our next issue.

Best Air Speed Indicator and True Air Variometer

by O. W. Neumark.

In "Thermik" of May, 1951, Helmut Klemke, of Nortorf, Holstein, described an instrument which he submitted to the D.F.S. (German Research Institute for Soaring Flight) in 1943. It consists of a variometer and an air-speed indicator, the scale of which is specially arranged in relation to the L/D curve of a particular sailplane. The indications of both instruments are combined on one dial by means not described in the present issue.

The indicator consists of one variometer needle which indicates against the outermost fixed scale the rate of climb or descent experienced by the pilot. Against the inner scale, which is automatically rotated by the air-speed instrument, the aforementioned needle indicates the rate of ascent or descent of the air, as opposed to that of the sailplane. The rotation of the inner scale subtracts the sink due to the speed of the sailplane from the total sink or lift experienced by the pilot. Against the moving inner scale, the needle also indicates the speed to fly to obtain the best gliding angle at all times and irrespective of the present speed of the sailplane. An index mark on the inner moving scale indicates against the fixed outer scale the air speed of the sailplane.

It is evident therefore that this instrument is a great advance on the best-speed-for-best-gliding-angle instrument advocated by G. O. Smith and elaborated by J. C. Neilan, although in its present form it does not apparently allow off-setting for head-wind or distance of glide per unit altitude as in J. C. Neilan's refined scale. It should, however, be possible after some redesign to include this as well as a further optional off-setting facility to give an immediate indication of increment to best cruising speed (higher than the speed for best gliding angle and based on rate of lift expected in the next thermal) when passing through sink. It is therefore academically a far sounder instrument than MacCready's arrangement described in an earlier issue of *Thermik*, although the result when used for best cruising speed will practically be identical.

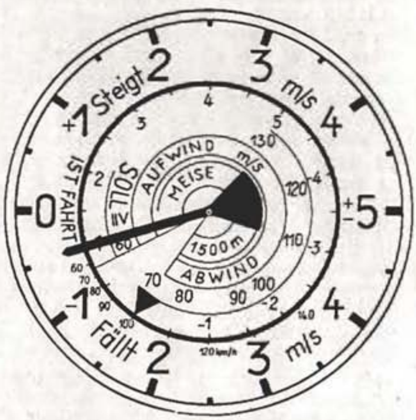

Fig. 1.—Illustrating the interplay of the scales in Helmut Klemke's instrument described in the accompanying article. It is calculated for a Meise flying at 1,500 metres. The inner disc is movable, and in the above illustration its position, and that of the needle, show that the sailplane is losing height at nearly ½ metre per sec. while flying at 100 km/h through air which is rising at 1 metre per sec.: and that in these circumstances the speed for best gliding angle is just under 60 km/h. As the pilot reduces speed to this figure, both the inner disc and the needle will independently move clockwise.

"Steigt, Fallt" = climbs, sinks. "Aufwind Abwind" = upcurrent, downcurrent. "Ist Fahrt" = present speed is. "Soll" = speed should be. (Courtesy of "Thermik").

The instrument has two fundamental functions which it performs automatically, taking into account the sailplane's actual speed.

When flying through sinking air the

needle points out the best speed to fly to attain the best gliding angle. On adopting that optimum speed, the needle still maintains its "command" function. Should there be a further increase in the sink of the air, it would point to a new optimum speed for best gliding angle. Similarly, as the sink reduces, the pointer will correctly indicate that the best air speed for best gliding angle is reduced proportionately.

Its second fundamental function is that the needle will indicate very clearly whether one is hurtling through lift when flying very fast, without the necessity of any mental arithmetic. Due to one's great self-induced sink at high air speeds, small rates of rising air are often not recognised in time.

The writer has for several years waited for someone to perfect a total energy variometer in order to apply it to act as a best air speed indicator. It is evident, however, that a combination of variometer and air-speed indicator automatically actuating a kind of circular slide-rule is the obvious solution that has escaped us for so long. There can be no doubt that a correctly designed instrument of this nature is worth its cost because it enables a pilot to make full use of the high performance of his sailplane.

Some hints for those who wish to construct such an instrument:—

Replace the needle of an A.S.I. with a flat disk. Place the A.S.I. at right angles to the variometer and superimpose the dials of the instruments with a prism or glass plate. A collimating lens can be used for avoiding parallax. Choose a variometer with a linear scale 0-15 ft. per sec., ± 180° dial scale. Inscribe air-speed indicator figures on the variometer dial (fixed) from plots from the sink/speed performance diagram of *your* sailplane. Draw an index mark on the A.S.I. disk. Use this as the 0 ft. per sec. mark and draw a variometer scale on the inner disk to exactly the same linear scale as the variometer. Draw best-air-speed figures on the inner dial by plotting the x and y values at the tangent of the performance curve from origin $x=y=0$

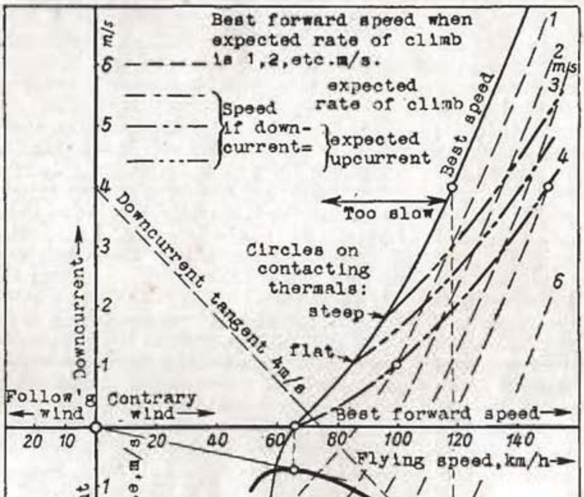

Fig. 2.—Diagram showing data and calculations on which Helmut Klemke's instrument is based. The speed diagram is that for Olympia-Meise, after Spilger's measurements (see "Gliding" for Spring, 1951), assuming wing loading of 16.3 kg/sq.m (3.34 lb/sq. ft). The thick curve applies at an altitude of 1.5 km (4,921 ft) and the broken curves at sea level and 6 km (19,685 ft).

and then from origin $y=+1$, $y=+2$, $y=+3$ ft. per sec. (you are then finding the best air speed for still air ($y=0$), for outside air sink of -1 ft. per sec. ($y=+1$) etc., etc.). When this A.S.I. dial has been very accurately drawn (it would be helpful to colour distinctly the positive and negative semicircles, but not to give great prominence to the variometer figure markings on this A.S.I. dial but to emphasise the best-air-speed figures)—then approach an instrument manufacturer and order an A.S.I. with a light-weight rotating disk instead of a needle and calibrated to indicate exactly according to the air-speed scale established on your variometer (fixed).

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Funnels, Jets and Aerocadabra

R. S. Scorer

THE meteorological problem is to find out how the air is moving. This can be done directly by putting smoke into it or by flying in it, but the method of calculation can sometimes be employed and laboratory experiments have been tried. Anyone who has dabbled in aerodynamics knows that calculations alone go a little way but that experiment is almost inevitably resorted to fairly soon; indeed, such expensive and elaborate apparatus has been built that it is natural to hope that it could be used to solve meteorological problems, and that one could think of the motion of the atmosphere in terms of boundary layers, velocity potential, Bernoulli, shear stresses, funnels, jets and all the other aerocadabra. The danger lies in the fact that most people who fly know quite a bit of aerodynamics and so it is natural for them to think of air motion in terms of it. I will now try to explain the danger of this.

The atmosphere is quite unlike anything that has yet been produced in a laboratory. It has no top, no lid, no walls, only a rather irregular floor. Its pressure is mainly produced by gravity, and any pressure that is not, is transmitted as a sound or shock wave at great speed and does not affect the wind or weather. The important motion in the atmosphere is produced by gravity acting on the density differences that result from uneven heating and cooling. There is no mechanical engine, indeed there is no mechanical influence other than surface drag. "Mechanical lift" is a myth, there is no machine inside the hill over which you soar. The hill is a fixed boundary, fixed relative to gravity and all the sources of heat and cold and everything that makes the air move. Even a cold front is not a shovel lifting the warm air; the warm air goes up all right, but only at the fronts, and not throughout the whole of the warm sector of a depression. If you try lifting water between two wedges of wood, the water rises not only over the wedges but everywhere in between them too—in fact, the surface is kept nearly level by gravity; yet the warm air goes up the wedges of cold air in a depression more than in between them.

Another thing about the atmosphere is that it can keep shear in existence without any stresses. Anyone who has heard of the thermal wind knows that the wind can increase upwards enormously, and stay like that for days; and this is not due to the

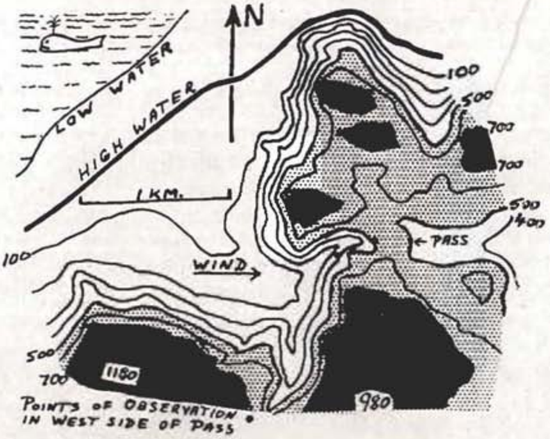


Fig. 1.—The Sychnant pass between Dwygyfylchi and Conway. Land is shaded above 500 ft and black above 700 ft. The shelf at the top of the gully was just above 500 ft. The flow was undoubtedly affected a bit by the 800 ft hill to the W.N.W.

drag of the ground, because it can equally well decrease upwards, and stay like that for days on other occasions. This is all due to the earth's rotation and no-one has yet devised a method of producing the same effect in a laboratory.

Rotating wind tunnels have been thought of, but for another purpose, namely to produce the same effect as the static stability. When the air is statically stable, the layers tend to remain level one above the other, and if displaced tend to come back to level again; and so it is with rotating air—it tends to remain the same distance from the axis of rotation and the layers at different distances tend to keep one outside the other in the same order. Hence the legend of the student who was failed year after year, because the professor found he could withstand 8g on a rotating wind tunnel and he did not want to lose him. It is no easy matter to produce in a wind tunnel the effect of static stability. It could, strangely enough, be done by making the air statically stable, i.e. by heating the upper layers, but to simulate the atmosphere, one would have to use very low air speeds even with the greatest feasible temperature gradient, and even then you cannot get away from the walls and roof with their rapidly thickening boundary layers.

And do not think of thermals as jets, unless you are on top of a volcano. The important thing about hot gases from a chimney is their heat; their vertical velocity of emission is unimportant.

The fact that no-one has yet done a useful model experiment on the atmosphere should be proof enough, for those who cannot perceive the differences, that there are fundamental differences which mean that there are no good analogies between laboratory aerodynamics and atmospheric dynamics.

However, not quite all is lost, for there is a layer of air near the ground that possesses no static stability and often little organised vorticity, which behaves very much like the potential flow solutions to problems. If you stand on a small hump of the ground the wind seems, and really is, stronger, as it would be in potential flow. When this hump is on top of a mountain you are inclined to think that the wind is stronger because it is the mountain, not the hump, that you are on top of; but if you stood in the flat calm behind the hump, would you think the calm was there because you were on the top of the mountain? On the whole, winds on mountain tops are stronger, but so is the wind at the top of Blackpool Tower. On a mountain top there are generally fewer trees and other obstacles which reduce the surface wind so much over level country. Sometimes, in fact often, the wind at 1,000 ft. above a mountain top is stronger than at the same level over the open plain, but it is not for the same reason that the velocity is greater in the narrowest part of a tunnel. There is no obvious reason why the air should not go round the sides: sometimes it does, sometimes it doesn't. But whatever it does, remember that the mountain got there before the air current and it is nonsense to talk about the mountain blocking the flow, for it is only a hypothetical flow that never existed that is blocked, and if the air at one moment is not going over the top it has got to have a special reason for starting to do so. In a wind tunnel there is a large fan or lots of bottles of compressed air on hand, but nothing like that in the atmosphere.

Finally we come to spurs and gullies. Anyone would think casually that the air "impinging on" a ridge with spurs and gullies facing into the wind would go round the spurs and up the gullies, in fact would delay its reluctant ascent till the last moment. But is it, or is it not, true that one finds better lift over spurs than over gullies? I think it is.

We found a good gully in North Wales, the Sychnant pass (Fig. 1). At the top there was a shelf across the V-shaped section, over which the surface wind was blowing at 20 m.p.h. or more. An ideal launching site: just bungey off the shelf into the air coming up the gully; after all if the air is coming up the gully, it must be coming *up*! So we fired a few smoke puffs, and to our astonishment the air in the middle of the gully was moving more slowly than the surface air, and was in places even descending. As the smoke puff approached the ground, it seemed to increase its speed and pass over the shelf very rapidly. We then fired some vertically upwards from the shelf and one puff actually stood still for about two seconds before drifting off slowly downwind, while we stood on the shelf in a fresh wind about 150 ft. below it. It must be admitted that the gully was not facing straight into wind; even so one would scarcely have expected what we saw: and a gully is not much use if it only gives lift when facing exactly into wind, and probably doesn't then.

One can think of good reasons why gliders don't often get into gullies. They might go in to see if there is any lift, but we can hardly recommend it.

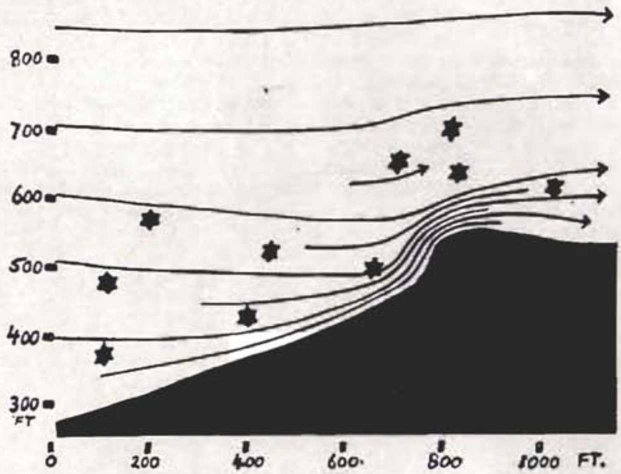


Fig. 2.—A vertical section up the centre of the gully showing how the high velocities were found only close to the surface. In order that the spacing of the streamlines should represent velocities, extra lines are added in the narrower part. Smoke puffs were fired approximately where the stars are shown, and the streamlines deduced from their subsequent motion. The occasion was an hour before dusk on 18th September, 1951.

Ten Thousand Feet in the Mynd Wave

by A. H. Yates

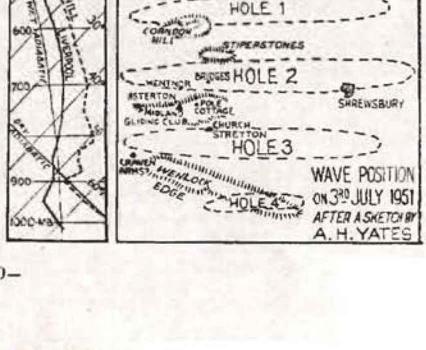
SUNDAY, 1st July, had been very hot and anti-cyclonic, and it was not until midday on Monday, 2nd July, that a light west wind made hill-soaring possible. By breakfast time on Tuesday, 3rd July, the wind, which had remained just north of west, was strong enough for bungy-launching, and at 08.15 hrs. we sent Ramsden off in our Olympia to fly for five hours to complete his "Silver C." There was some cloud with base about 2,000 ft. above Long Mynd (which is 1,400 ft. above sea level). Ramsden landed at 13.25 and, pausing only to re-set the barographs, I took his place in the Olympia and was bungied off at 13.35.

Half an hour earlier, Theo Testar had remarked that the clouds, now only about 1,300 ft. above the Mynd, were sorting themselves out with lanes of clear sky running across wind. Through the gaps, higher clouds could be seen with a similar configuration. Testar had gone off before me and was not to be seen among the hill soarers. I was able to reach 800 ft. in the thermals which studded the hill lift, and was very near to cloud base several times. The nearest hole in the cloud up-wind was some two miles out from the south end of the hill, and although I flew out towards it through mild sink, I had never enough height to reach the wave lift which must have been there. I once arrived back below hill-top and, to avoid an ignominious landing at Asterton, decided to join those hill-soaring at the Pole Cottage (north) end of the slope where the slope lift seemed to be strongest. At about 14.45 hrs., just upwind of Pole Cottage, I reached the base of the ragged strato-cumulus, which had now thickened to a cap with base about 1,200 ft. above the Mynd, but which seemed to thin out in front of the slope, near Bridges. (It is always difficult to see the cloud patterns from near cloudbase).

Although the College of Aeronautics Grunau was close by, I entered the cloud and flew west, upwind, for a few hundred yards until I broke out into the blue between Wentnor and Bridges. The lift was smooth, but only one or two feet per sec., and by flying carefully backward and forward along the leading edge of the cloud mass, the Olympia climbed steadily and the rate of climb rose to three ft./sec.

The blue lane appeared to be about a mile wide and to be roughly north and south, stretching towards Shrewsbury. As I climbed above the gap, I found the lift to be greatest about one third of the way out across the gap; it fell off gradually to zero just behind the cloud leading-edge and further out over the gap. At about 15.30 hrs., 6,000 ft. above the Mynd was reached, and I was then higher than the top of the great cloud mass over the Long Mynd. There was a short break in this long cloud just north of the Mynd, after which it continued northward. The gap above which I was flying stretched from Craven Arms to beyond Shrewsbury. Since the cloud top looked higher north of the break, I flew along the lane toward Shrewsbury and passed the cloud break without much loss of height. The lift proved to continue and the climb was continued near Shrewsbury to nearly 10,000 ft. above the Mynd.

I was very conscious that the gain of height recorded by the altimeter only just exceeded "Gold C" height, but any adjustment of position now gave a slight sink: I was flying at the minimum sinking speed at the greatest height locally obtainable. Increased height might come with time, if the wave strengthened, but the trough was now clouding over and was completely



covered for a length of several miles between me and the Long Mynd. The gap in front of the Mynd itself, through which I must descend to regain the take-off point, was diminishing in size. The cloud rolls immediately upwind and downwind seemed to be of less height than my cloud, so that it was unlikely that a greater height could be obtained by crossing to them.

While I was thus considering the situation with the variometer needle at zero and 9,900 ft. above take-off on the altimeter (11,300 ft. above sea level), I noticed that the trough was filling rapidly and I decided to descend. With brakes out and 70 m.p.h. on the airspeed indicator, I soon reached the gap in the cloud valley floor above Wentnor, and descended through ragged wisps of cloud and landed at 16.45 hrs. The cloud base over the Mynd had descended to only 800 ft., but the sun shining on the ground around Wentnor showed that the hole in the clouds was still there.

Philip Ramsden, now recovered from his five-hour marathon, took my place in the Olympia and managed to contact the wave at about 19.00 hrs., and reached 2,000 ft. above the Mynd just downwind of the cloud hole.

Testar, who had taken off before me, contacted the wave and reached over 2,000 ft. During my climb I had seen two other Olympias below me, the blue one flown by Doc. Cotton reached 5,500 ft., and the cream one flown by John Horrell, 6,200 ft. Horrell flew downwind to the next wave and, after losing only a little height *en route*, climbed back to 6,200 ft. He found traces of other wave lift during a long descent downwind through cloud and eventually reached Wolverhampton—his Silver C distance.

Analysis

The upper air radio-sonde ascent from Liverpool at 14.00 hrs. on 3rd July showed that the temperature lapse rate was unspectacular. Apart from the dry adiabatic lapse rate near the ground, set up by convection, the lapse rate from 1,500 ft. to over 20,000 ft. a.s.l. was fairly uniform at about 3.2 deg. F. per 1,000 ft., a little less than that required to make saturated air unstable.

The wind direction was remarkably constant, with height at between 270° and 280°, but the wind speed increased steadily as height increased, especially between 2,000 ft. and 10,000 ft. above the Mynd (18 to 38 knots). That these wind figures were applicable to the Shrewsbury area is confirmed by the ability of the Olympia to progress upwind easily at 2,000 ft. and less easily at 10,000 ft., with an airspeed indicator reading of 40 m.p.h. (true airspeed 50 m.p.h. at 10,000 ft.).

These conditions (a reasonably stable atmosphere and a wind speed increase with height) are just those needed to make lee waves likely (Scorer: GLIDING, Spring 1951). The high humidity, over 90% up to 6,000 ft., explains the rapid formation of the cloud bars when the air was raised by wave action.

A sketch map is attached showing the approximate position of the cloud gap over which I flew. There was at least one other upwind, and the one immediately downwind was approximately over the Church Stretton valley. (John Horrell flew across this area and I could see Church Stretton down through this gap). Horrell reported that the next downwind gap was small, but that it lay approximately over Wenlock Edge.

Roughly placing these troughs and crests on the map shows that the wavelength was of the order of 5 miles and that the waves lay nearly across wind. It is not possible to say which mountains produced the waves, but the map shows that the 5-mile Long Mountain, near Welshpool, lies about 10 miles (or about two wave-lengths) upwind from the Long Mynd cloud bar, and it is possible that this mountain gave the initial disturbance. The next hill masses of the Stiperstones—Corndon Hill and The Long Mynd at about 5 mile intervals, may have reinforced the initial waves, and the coinciding of the Mynd slope lift with the wave lift, enabled the Olympias to contact wave lift.

SUMMING UP.—Under conditions of a normal lapse rate, with a wind shear (increase with height), waves of wave-length about 5 miles have again been found extending from about 3,000 ft. to 11,000 ft. above sea level.

FOOTNOTE.—Three days later, on Friday, July 6th, 1951, waves occurred again and several machines reached about 5,000 ft. above the Mynd in conditions very similar to those of Tuesday. The upper air readings of temperature and wind velocity at 14.00 hrs. at Liverpool were also remarkably similar to those on Tuesday.

Long Mynd to Wolverhampton in Waves

by J. W. Horrell

THE previous pilot in the Midand Club's Olympia on 3rd July, 1951, was Theo Testar, who landed at 14.45 (B.S.T.) reporting that he had flown northwards in front of a standing wave cloud lying over the Mynd almost as far as Shrewsbury. This gave me the idea of trying a cross-country in that direction although it was cross-wind.

I was launched by bungy at 14.50 with very little preparation—borrowed map and barograph (mine were in my car and T-21 respectively) and very little money—2s. 8d.

By flying in hill lift, I very quickly found that the best place was just south of Pole Cottage, and it was possible to fly well in front of the slope on the line Pole Cottage-Wentnor. I therefore headed outwards in lift of 2½ ft./sec., reaching cloud base at 800-1,000 ft. and passing just in front of the cloud, which could then be seen to follow the shape of the Mynd.

By beating up and down in front of this cloud, lift increased to 3 ft./sec. and I reached 4,300 ft. a.s.l. about 5 miles south of Shrewsbury. At this point there was a break in the wave cloud and I was in sink at 5 ft./sec. and approximately level with the ridge of the cloud.

I decided not to press on northwards to the continuation of the cloud (used by Yates), and returned to the Pole Cottage area (Wentnor could be seen through the hole in the cloud). Here I very soon re-entered lift at 3 ft./sec. and by beating up and down on the downwind side, reached the level of the highest part of the cloud, 7,700 ft. a.s.l. It was possible to remain approximately stationary by flying into wind at 38-40 m.p.h. indicated.

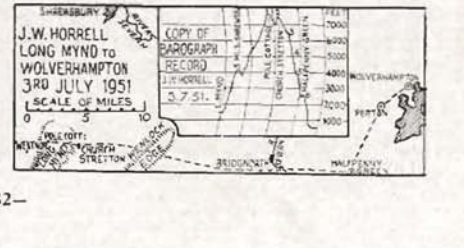
At this point I saw Yates in his Olympia, flying to the N.W. of me and about 3,000 ft. above. Dr. Cotton was also in the same gap in the blue Olympia, but was flying in the upwind side of the hole. He was about 800 ft. below me. The gap suddenly started to close and Cotton immediately flew straight down through what was left of it.

I then turned on compass course east and flew through the crest of the wave cloud over the Mynd, coming out into the next gap over Church Stretton. The rate of sink was 5 ft./sec. and I dropped to 7,000 ft. a.s.l., very quickly regaining lift at 3 ft./sec. on the downwind side of this gap, which was smaller than the previous one. I reached the level of the crest of this cloud at 7,600 ft. a.s.l. and decided to repeat the downwind journey to the next cloud. From here there appeared to be an unbroken area of strato-cumulus down wind. Being level with the crest, I could not see the next hole, if any.

By now I was flying blind at 45 m.p.h. down wind, in sink of 3 ft /sec. I did catch a momentary glimpse of Wenlock Edge as I passed over, but there was no appreciable change in the rate of sink, although the barograph recorded a slight kick.

I continued blind-flying on the same course, finally coming out through cloud base at 4,000 ft. a.s.l., one mile south of Bridgnorth. I could then see only 8/8 strato-cu, but as the rate of sink had dropped to 2½ ft./sec., I then decided to press on down wind, changing course slightly to take me over Halfpenny Green airfield. Here I encountered a small area of lift at 2,600 ft. a.s.l. at 3 ft./sec., but which only gained me about 200 feet. I did not identify the source of this lift, as there was no sun to produce a thermal, but its position does correspond approximately with the wave lengths previously noted.

I next flew to Perton airfield in the same sink of 2½ ft./sec. at 45 m.p.h., but found no more lift. There was a slight check in the descent, probably caused by hill lift from the ridge near Perton, but I decided to ignore this and set course for Wolverhampton airfield at 45 m.p.h., 2½ ft./sec. sink, about 2,000 ft. a.s.l., having seen that my course would take me over a cricket ground and a golf course, just in case I should encounter a violent "down." How-



ever, I was lucky and crossed the airfield boundary at about 300 ft. above ground level, entered the circuit and touched down alongside the control tower at 16.40 hrs., thus completing my "Silver C."

On the barograph chart, apart from the large "kick" over Church Stretton, smaller ones can definitely be noted at regular intervals throughout the flight (marked 1-6), although all the way from Bridgnorth to Wolverhampton the cloud was 8/8 strato-cu with no break visible.

Correspondence

WORLD CHAMPIONSHIPS

APPEAL FOR FUNDS

Dear Sir,

The World Gliding Championships will be held next May in Spain. The British Gliding Association is most anxious to be represented, and provisional plans are being made to send a contingent of five glider teams, but this will not be possible without financial help.

In 1950 our pilots and crews paid their own expenses, but they cannot afford to do so again. The position this year is that if Britain is to be represented, £4,000 must be found. The five gliders of British design, the cars and radio equipment, have already been loaned, and the Spanish authorities will meet all expenses while the teams are in Spain. There are, however, many other costs involved in organising the venture and in carrying it through. These include transport charges, subsistence en route, insurance, repairs, certain hire charges and entry fees.

The efforts which have been made to improve gliding technique and equipment in this country, the importance of this sphere of aeronautics, and the value of British representation at a world event of this nature, are generally appreciated and I am confident that an appeal to the public, and in particular to our loyal friends in the aircraft industry, will meet with immediate support. Donations should be made out to the British Gliding Association Ltd., and forwarded to the British Gliding Association at the Royal Aero Club Aviation Centre, Londonderry House, 19, Park Lane, London, W.1.

KEMSLEY,

President, British Gliding Association.

THE LOW TOW

I have just finished reading the autumn issue of GLIDING, and want to congratulate you on a most interesting issue.

I was particularly interested in Allan Ash's story on "The Low Tow." In the United States, the low tow is known as the Navy tow, since the method was first explored and then adopted as the standard method by my glider test group at the Naval Aircraft Factory, Philadelphia, in 1942. At the time, we were working on an automatic pilot for gliders in tow, using a tow-line direction feeler to transmit elevation and azimuth intelligence to the autopilot. Our efforts to do this in the high position were fruitless. The tow-line information was misleading, particularly as far as elevation control was concerned. If the glider got too high, the downward tending tow line so indicated and down elevator was applied to correct it. Unfortunately, a downward-tending tow line could also indicate that the glider was over-running the tug, causing increased catenary sag, in which case further down elevator only exaggerated the condition.

Exploration of the low tow position developed the fact that there was one position slightly below the slipstream, where the tow line weight was just balanced by the airstream lift so that the catenary curve disappeared, and the tow line lay in a straight line from the tow plane to the glider The angle of the tow line to the horizontal for this situation, as viewed in side elevation, was approximately 8 to 10 degrees, using a 250 to 300 foot manila or nylon tow line. This seemed to be true whether towing a sailplane behind a primary training plane, or a 12-place Waco CG4A behind the amphibious Catalina PBY5A. In this position, the tow line direction indications were correct, and the auto-pilot functioned satisfactorily.

In the course of these experiments, we discovered also that riding that particular position was considerably less fatiguing for the glider pilot. It was found that if well trimmed, the glider would tow "hands off" for considerable periods of time, any deviation from the correct position setting up a corrective "tending" of the tow line, amplified by the building up of a wind catenary which augmented the "tend" in the proper direction. Next, the absence of the usual catenary sag in the tow line removed the spring effect of such a curved line, and prevented surging, which can become very annoying in rough air in the conventional high tow position.

It developed further that for some reason not as yet satisfactorily explained to me, the combination has less drag with the glider in the low position. Mr. Ash indicates this in his statements about improved rate of climb. Here is another simple check. Have the tow plane level off and fly horizontally with the glider in the normal high position until a constant speed is attained. Then let the glider go down into the low position—the throttle setting of the tow plane not being changed, and the tow plane continuing to fly level until equilibrium airspeed again is attained. Read the new airspeed. In the PBY5A—CG4A combination an increase of 5 knots resulted.

As to the best method of getting into the low position, opinion varies. For myself, in a sailplane I prefer the brief but uncomfortable ride down through the slipstream after a safe altitude has been attained, but when flying the large gliders with complete airplane-type landing gears, such as the CG4A, my practice was to keep the glider on the ground until the tow plane was in the air and its slipstream overhead. We used this system also in towing airplanes.

Disadvantages with the low tow, other than that cited by Mr. Ash, are: the glider pilot cannot watch the tow plane and the terrain at the same time; and, should the tow line break near the tow plane, or should the tow plane release the line for any reason, the glider pilot is liable to receive a face-full of tow line mixed with fragments of canopy, which can be most disconcerting. In spite of these objections, I still like the low tow.

RALPH S. BARNABY,
Captain U.S. Navy, Retired

Dear Sir,

Having had the good fortune to do about 500 aero-tows, on "heavies" and "lights," I was interested to read the article entitled "The Low Tow," by Allan Ash, in the Autumn issue of GLIDING.

He goes a long way to point out the advantages of this method of towing, although I should have thought some of his figures were rather optimistic, or else he was operating with an exceptionally good tug.

I think there is room for amplification with regard to the height at which the changeover from high to low tow occurs. A glider combination, similar to all aircraft, is most vulnerable as it becomes airborne and passes low over the upwind boundary. The tug's engine may quit, or the tow rope pull out or break. This is definitely not the phase at which to feel about, as Mr. Ash says, "through the slipstream after he (the glider pilot) has reached a safe height, e.g. 100 feet, or he can hug the ground during the take-off and allow the tug to climb up away from him." At this very unsafe height the sailplane pilot should not be looking *up* at the tug, but *down* at it, with his eyes and mind on the landing fields he will use straight ahead in the event of emergency. Then, having briefed the tug pilot to be prepared for a change in trim, the low tow can be adopted at a reasonable height, when the glider pilot is more settled and the air less turbulent, of 800 to 1,000 feet.

In my opinion, the low tow is well suited to fairly long ferry trips, especially if the track is up-sun, when the sailplane pilot wants the most stable tow possible. Ordinary tows to 1,500 and 2,000 feet, when the tug can be used as a "thermal indicator," hardly make the low tow worth while.

It would not have been amiss if Mr. Ash's article had included a few points on standard towing procedure and emergency signals and drill, e.g. if the sailplane fails to release, etc., or is it thought in these days that pilots are fully conversant with this sort of thing? I was astonished when an Olympia pilot stated the other day that when he did his first aero-tow he was "just flung off without any instructions at all." One would think that tug pilots, as captains of glider combinations, would take the trouble to check this themselves.

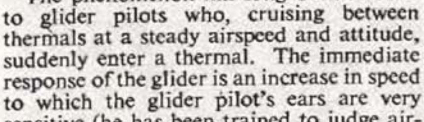
JOHN FREE.

AIRSPEED FLUCTUATIONS WHEN ENTERING WAVES

In the Autumn issue of GLIDING, John Neilan described the increase in speed of an aeroplane flying level when it enters, at constant attitude, the upward-moving air in a standing wave. He asked for a simple-minded explanation.

The phenomenon has long been known to glider pilots who, cruising between thermals at a steady airspeed and attitude, suddenly enter a thermal. The immediate response of the glider is an increase in speed to which the glider pilot's ears are very sensitive (he has been trained to judge airspeed by the noise level).

The cause of the forward acceleration of the glider or aeroplane on hitting an upgust is seen if the forces acting on the wing are examined.



If the aircraft is flying level at speed V, the forces on the wing are lift L, vertically upwards, and drag D, horizontally. If the aircraft now meets an upgust of speed w, then the resultant velocity of the air is V' and the angle of incidence is suddenly increased by $\delta\alpha = w/V$ approximately.

The air forces on the wing are now L' and D', normal to and parallel to V' respectively, and these forces represent a change in drag from

D to $D' \cos \delta\alpha - L' \sin \delta\alpha$.

Now, if the aeroplane is not just about to stall (in which case the upgust would stall it), D' and D are almost equal and so are L' and L. Also since $\delta\alpha$ is small

$\cos \delta\alpha = 1$ approx. and $\sin \delta\alpha = \delta\alpha = w/V$.

Thus, there is a change in drag from

D originally to $D - L.\frac{w}{V}$

i.e. an accelerating force equal to w/V times the aircraft weight. It is this force which increases the speed of the aircraft when it enters the upgust. The fact that a glider is descending and not flying level does not alter the argument.

Having entered the upcurrent the pilot can choose either to continue to fly level in which case he will be able to fly much faster, or to maintain his original airspeed, in which case the upcurrent will give the aircraft a rate of climb. If the airspeed is to be held to the original value the attitude of the aircraft to its flight path must remain the same, and since the new path is inclined upwards relative to the old, then the aircraft attitude to the horizon will be more nose-up.

The reactions of pilots to the increase in airspeed on entering a gust depends on the type of aircraft they are flying. The glider pilot is seeking upcurrents and welcomes the sudden increase in airspeed as an indication of a thermal. He gets the nose up to reduce speed to his best circling speed and begins to circle if the lift is thermal lift. If the upgust is the result of entering a wave, he will also reduce speed to return to the minimum sinking speed in order to extract the best rate of climb from the wave.

The aeroplane, however, is cruising at a constant height and airspeed. The pilot endeavours to maintain the aircraft under these steady conditions but cannot do so if he flies into waves. An attempt to hold height constant results in a fluctuation in airspeed, while an attempt to hold the airspeed constant results in a series of climbs and descents as the waves are traversed. A flight through waves in a Rapide en route from Farnborough to Northern Ireland was recently described to me. The pilot endeavoured to maintain a constant height at constant throttle setting and the hitherto steady A.S.I. reading fluctuated considerably with a period of about one minute. This corresponded to a series of waves with wavelength about three miles.

Mr. Neilan is quite right in pointing out that pilots unprepared for such phenomena may be disconcerted and blame the aircraft. Those in the know may, however, be able to turn their knowledge to advantage. Remember the P.38 pilot in California who climbed his aeroplane with propellers feathered to 30,000 ft. in a wave! All we now need is waves beside each airport, and "stacking" problems are solved.

A. H. YATES.

FORMER DISTANCE RECORD

On 14th November, the Secretary of the B.G.A. wrote as follows to Mr. H. R. Gillman, of the Federation Aeronautique Internationale:

"In connection with the international distance record for gliders, which until recently was held by Miss Klepikova, I am instructed by my Council to ask you if you would be kind enough to let us know from your records at what height Miss Klepikova was released on her record flight on the 6th July, 1939. There has been some discussion concerning this record over here, and we would like to get our facts correctly. I am sorry to trouble you about this."

Mr. Gillman, in reply, gave the following figures for two records set up by Miss Klepikova for distance in a straight line:—

Glider, 1st category (single-seater), 6th July, 1939: height of release, 1,100 metres (3,609 ft.).

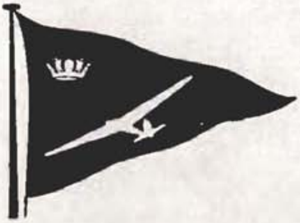
Glider, 2nd category (multi-seater), 10th June, 1940, with Borodina as passenger: height of release, 1,200 metres (3,937 ft.).

EDITORIAL NOTE.—While meeting in Sweden during 1950, many delegates to the F.A.I. Gliding Commission were given to understand that Miss Klepikova, when setting up the former international distance record of 749.2 kms. (465.5 miles) in 1939, started with an aero-tow to 7,000 metres (see GLIDING, Autumn 1950, p. 118).

The second flight mentioned by Mr. Gillman set up a two-seater distance record for women, 443.7 kms. (296.4 miles).

CLUBS & ASSOCIATIONS

Royal Naval Gliding & Soaring Association



Review of 1951 Season

Now that gliding has virtually stopped for the winter, with the exception of a Grunau which the Southdown Club have kindly accepted as a lodger at Friston, it is well to glance back at the year's activities.

The table below shows how the Association's branch clubs have fared over the past three seasons. The figures give the number of launches.

Club	1949	1950	1951
Fulmar	672	1,121	142
Condor	—	146	?
Blackcap	184	2	Disbanded
Gannet	176	196	140
Heron	—	—	92
Portsmouth Naval	1,026	2,150	3,320
	2,058	3,615	3,694+

From these figures it is unmistakably clear that the Portsmouth Naval Club has been expanding steadily, whereas the others, except for Heron which only started in October, 1951, can barely be said to be holding their own. Where lies the reason for their apparent lack of interest in this sport of the air in the modern, supposedly air-minded, Senior Service? It isn't lack of interest! The interest is there, at the air stations, amongst the non-flying personnel who spend their working hours helping their more lucky aircrew companions into the air. They long for a chance to fly themselves. The R.N.G.S.A. makes available two-seater trainers, primaries and intermediate sailplanes, but still little gliding is done. The missing link between the gliders and the prospective glider pilot is—Instructors.

There are all too few glider pilots in the Navy with more than a very little gliding experience, and of those few even fewer can spare the time to instruct regularly at a gliding club.

Cross-country Flying

During the season, members of the Association have flown a total of 1,105 cross-country miles in 21 flights and have achieved a "Gold C" distance leg (which, being a goal flight, was also a Diamond leg) and a "Gold C" height leg.

London Gliding Club

To start from where the last report left off, we must record a goal flight to Colchester on 28th July by J. R. Jeffries in his veteran Scud II of 1932 design. After the Championships, Dodd, Parkin, Hands and Currie hired out one of the club Olympias and took it to the Long Mynd for a week.

August started with a thermal-off-the-winch day on the 2nd, when A. W. Doughty went 62 miles to Lakenheath, and F. E. Allen took an aero-tow at Elstree and went 112 miles to the coast at Great Yarmouth, getting 6,000 ft. Aero-tows were laid on from the club ground on the 5th, when Jack Rice flew in with his Messenger.

On a day of practically no wind, 8th August, H. M. Latto made an interesting series of sorties, after catching a thermal off the winch: N.E. to Barton and back, S.W. to Berkhampsted Bovingdon and back, then finally W. to Oxford. Here a vast mass of cloud, 10-20 miles across, failed to give lift, so he landed at Cowley after 70 miles of multi-directional wandering. The calm was not due to a stable anti-cyclone, but to an unstable "col" between two shallow "lows" centred over the Orkneys and Brittany.

The father of all cloud streets passed over on 12th August, stretching at least 28 miles from W. to E., according to Dudley Hiscox,

who used it to go 16 miles upwind to Wendover and then 57 miles downwind to Southend. He found lift on the north side, where it looked very like a cold front. Among others who used it, L. Wright went upwind to Thame, P. A. Wills climbed 10,000 ft. and went cross-wind to Denham, and M. C. Russell and J. Buckley climbed over 8,000 ft. Next day N. P. Anson made 8,800 ft. on the way to Debden.

A notable day in club history was 22nd September, when an unmistakable lee wave formed just the other side of the Tring Road in an E.S.E. wind blowing off the Downs. It had been suspected in the afternoon, but the T-21 was too busy with pupils till 19.05 hrs., when it took off with G. H. Lee and O. W. Neumark and entered wave lift of 1 ft. per sec. at about 800 feet above ground. This increased to 4 ft. per sec. by the time they had climbed to 2,200 ft. above take-off, when darkness forced them to land. A W. Doughty also picked up the wave in a Tutor, climbing to 1,400 feet before dark. Exploration of the area of lift by these pilots made it clear that the wave extended well beyond the north end of the Downs but not as far as the south end, and the upper wind, which blew from due south at 4,000 feet, seems to have determined its position.

October brought no outstanding flights, but on 3rd November, the Kemsley winter competition having begun, Frank Foster set off towards Cambridge. However, the clouds were poor beyond Hitchin so he returned from there.

Army Gliding Club

THE later summer and early autumn months have been sparing in good gliding weather (apart from Mondays of course, when even the office desk shows ten up, and every martial endeavour is topped by a small white cumulus).

We have had several creditable cross-countries to mark up on the board, however. Notable amongst these are Colin Bennett's "Silver C" height and distance in the blue Grunau, to Hurn, and Deane-Drummond's out-and-return to Dunstable in the Olympia.

The Ministry of Tearing-up has now nearly completed the activities which have so effectively curbed our style for the best of the season, and as the yellow leaves drift from the bough, so emerge new shining stretches of tarmac, and even the tow-car whinnies.

Since the flying year started in March, the club has operated for 150 days, with 3,355 launches for 360 hrs. 8 mins. flying. Of this the resident instructor, John Free, flew 2,063 launches for 175 hours. The club fleet consists of a T-21B two-seater, Olympia, blue Grunau, and a new cream Grunau which replaced the Kirby Cadet in July. After the second Grunau arrived, trials were carried out using auto-powered launches, together with entirely dual *ab initio* air instruction, using air brakes and a limited instrument panel, followed by first solo and conversion on to the Grunau. This system has been found to operate with safety and success, pupils requiring on an average about 40 to 55 launches before attempting first solo.

Surrey Gliding Club

THE club has now settled in at Lasham—at least, flying is proceeding normally, which is the main object of the exercise. The actual move to Lasham took place on Sunday, 19th August, *et seq.* Ted Ashwell was towed over in Red-O by Laurie Vandome and his Auster; Jack Karran had soared his own machine there the previous day, under highly difficult conditions. The rest of the aircraft went by road. The loading of furniture, etc., made the Redhill clubhouse look like the scene of a particularly sensational eviction. Anyhow, after about three weeks, most things had reached Lasham.

Flying commenced on 23rd August and has continued every week-end since. Though Deane-Drummond soared a Surrey machine for 45 minutes that day, it was not till 29th September that Hugh Kendall took Philip Wills's Weihe back to Woodley and became the first member to get away. Wally Kahn stayed up in the club Weihe for 90 minutes on the same day. Various people have soared Daisy for short periods.

On 3rd December, conditions appeared favourable for an attack on the winter cross-country competition. In spite of numerous attempts, only Wally Kahn

managed to remain airborne for more than a few minutes, and he did not do well enough to go away.

The Club's annual Christmas party was held at Lasham on 8th December. Though the forecast weather map showed a wind velocity of about 20 m.p.h. there, the hurricane made even motor cars practically uncontrollable. The party continued until 01.04, when the electricity failed, due to a shortage of fuel, and nobody felt capable of refilling the machine. The debauchery this year took the form of a marathon cocktail party, in that there was positively no organised entertainment.

At the club's Annual General Meeting earlier in the day, the chairman, Ann Douglas, underlined the marginal financial position after the reorganisation to separate the North Downs Gliding Trust from British Air Transport. There is only £70 in hand at present, with the "lean season" just starting. But a Kemsley loan is assured to cover capital equipment, and by hard work and still greater efficiency, we are quite confident that Lasham will become the busiest gliding site in the country.

A.J.L.

Deeside Gliding Association

THIS report covers the first six months' active operations of the club. We have now moved to our permanent hangar, the greater part of which has been cleared of sand and other debris, although there is still sufficient left requiring removal to ensure that members will not suffer from boredom when weather-bound.

Two aircraft are available, an S.G. trainer and a Cadet; Crease's Olympia sailplane is also here till its summer migration to Cambridge, and some of the members have flown it. The total number of launches (winch) has exceeded 300, and 5 A, 5 B and a C certificate have been obtained. The C was attained on a launch from the Clwyd Gate site with a 2,000 ft. altitude gain and 1¼ hours duration. Plans are afoot to have further launches from this and similar sites in the Clwyd range during the winter months. Since, however, the Clwyds are only about 12 miles distant, attainment of 3,000 ft. a.s.l. there would enable the Olympia to land at Sealand, thus avoiding de-rigging.

Club membership is steadily growing, but there are still a few vacancies before the saturation ratio between machines and pilots is reached. It is our good fortune to have exclusive occupation of the large ex-R.A.F. airfield at Sealand.

V.B.

FOR SALE & WANTED

Classified advertisements can now be accepted for this Magazine. Rates on application to The Trade Press Association Ltd., 57-61 Mortimer Street, W.1.

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