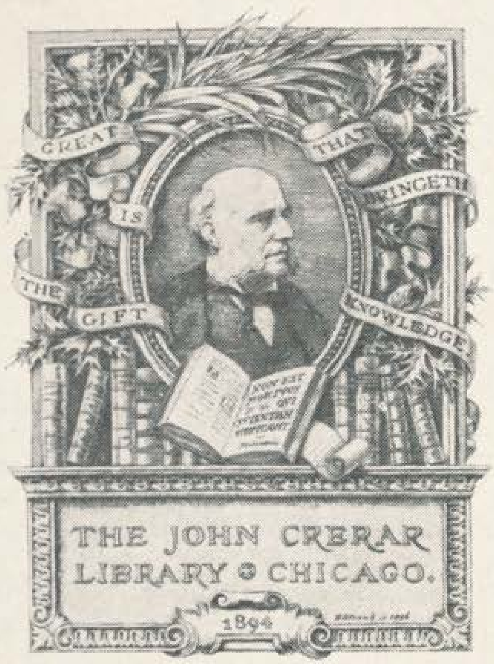
GREAT
IS
THE
GIFT
THAT
BRINGETH
KNOWLEDGE
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1894

EXPERIMENTS WITH GLIDING MODELS

conducted by

E.C. HUFFAKER

for

Mr. O. CHANUTE C.E.

1899

EXPERIMENTS WITH GLIDING MODELS.

Conducted by

E. C. HUFFAKER – of CHUCKY CITY – TENN.

UNDER INSTRUCTIONS FROM

O. CHANUTE –

of

Chicago – Illinois.

1899.

This book is ~~is~~ intended for ~~makin~~ keeping a record of experiments made and proposed, with drawings of models, memoranda, discussions, and such other matters as may seem to have a bearing up-

upon the subject of natural and artificial flight.

OUTLINE OF WORK TO BE DONE

The character of the experiments to be made is indicated in the following extract from a letter from Mr. Chanute, bearing date of December 22 1898.

"The following are my views on the subject of the proposed experiments on automatic stability in a wind.

" You know that I have tested two methods with full sized apparatus. These are, 1st the Penaud tail and 2nd the swinging wings. Both are effective, but both are slow in action. Both are employed by soaring birds, but the frequent rocking of the latter, apparently over aerial billows, when no motion of the tail or wings can be detected, has suggested to me that an automatic arrangement can be devised by which the angle of incidence will be adjusted by the direct pressure upon the wings, without the delay incident to swinging them, or the tardy action of the tail. I may be wrong in my expectations, but I want to test the matter thoroughly.

"The underlying principle has been well stated by both Sir George Cayley and by Mr Drzewiecki. The latter says, on page 245 of l'Aeronaute, Oct 1887:

"' An aeroplane moving horizontally in air is in longitudinal dynamic equilibrium when the centres of gravity and of pressure of the system are on the same vertical line, it is at the two points C and G that the two opposite forces R, the pressure, and P, the weight, are applied, being

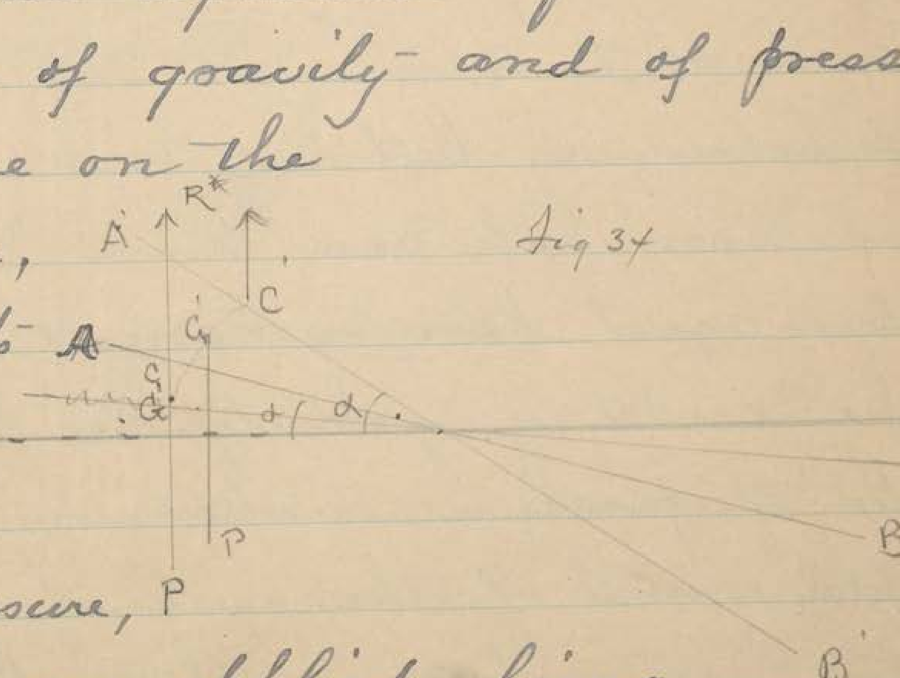
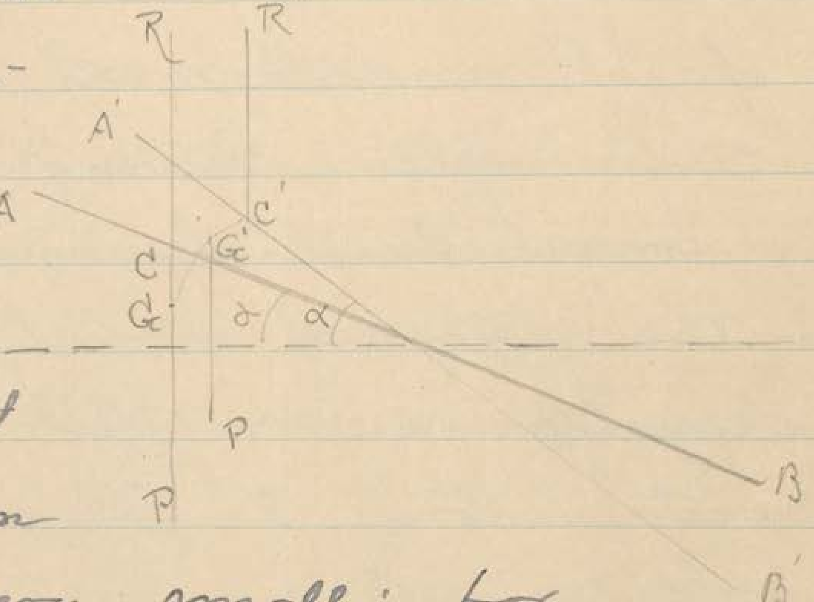


Fig 34

opposite and equal. If now, from any cause, the angle α increases or diminishes, the centre of pressure would move back or forward, and the sustaining force, applied at this centre would form with the weight applied at G, a couple which would bring the point C upon the vertical line passing through G, thus maintaining α constant.

"The position of these two centres (gravity and pressure) must evidently be such that G shall be below C, but the distance between the two must be very small; for



if the centre of gravity is much below the centre of ~~pressure~~ suspension C, upon every displacement of the centre of gravity from the vertical line passing through the centre of pressure there would result a pendular action which would be larger the greater was the distance between those two points. Such a case was observed by myself, when a bird wounded by a shot had his leg hanging down, thus lowering his centre of gravity, and was unable to fly."

"This 'pendular' action evidently results from the inertia of the weight, which is carried past the vertical, and produces a series of oscillations. The remedy proposed by Drzewiecki, i.e.: a high centre of gravity, will do for the bird, who is instinct with life, and can afford an unstable equilibrium, but I do not think it will do for a man, concentrating say 150 pounds in his body, and sustained by an aeroplane of some 40 pounds weight.

My proposal is to disassociate these two elements of weight to the extent of to the extent of pivoting them together, so that only the inertia of the aeroplane need be taken care of in the changes of the angle of incidence, and restoration of the stability. Then the man can be placed low enough to be in no danger of toppling over.

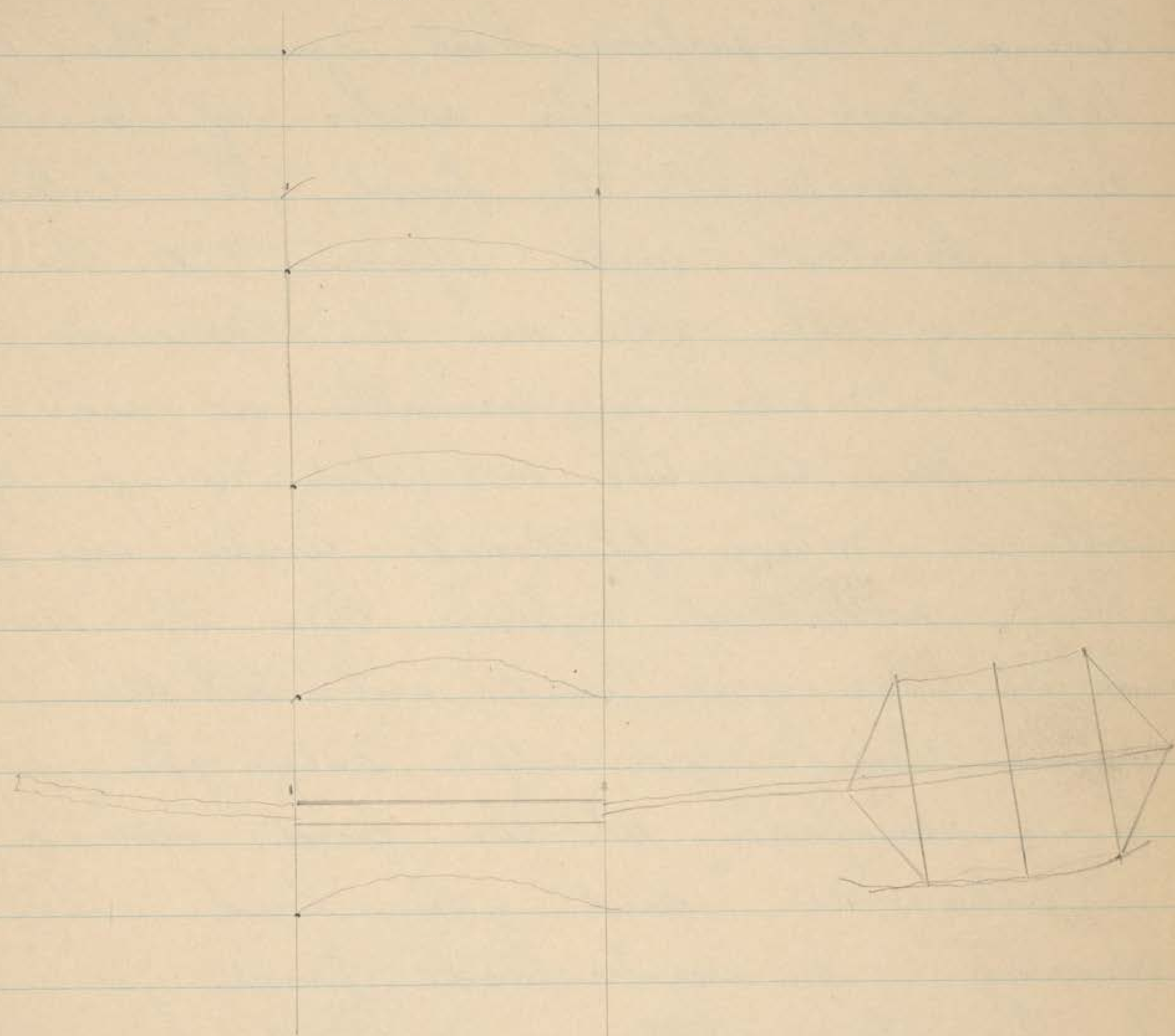
"For a variety of reasons, I have concluded that a successful soaring machine should consist of five superimposed surfaces. The principal of these reasons is, 1st that the surfaces should be as narrow as possible (in the direction of flight) so as to reduce the fore and aft motion of the centre of pressure to a minimum, thus contributing to the fore and aft stability; and 2nd, that the surfaces should have as small a sidewise spread as ~~possible~~ practicable, so as to diminish the leverage of wind gusts. Hence I have designed a soaring machine with five arched surfaces, each 17 feet by 3.06, spreading 260 sq. ft. and weighing 40 pounds, with which I believe that soaring flight can be performed. I have a model and it promises well, but other engagements and local restrictions have prevented me from fully testing it.

" Before proceeding to the construction of the full sized apparatus (and in this I hope that you will participate) I desire to test thoroughly the principle upon which the automatic stability is based.

"This is shown in outline upon the next page. A stout bar D is fastened to the upright posts, and inside of this are oscillating pair of bars B is pivoted at G, which must be under the centre of pressure when the machine presents a negative angle of 3° to 6° to the horizon. Carefully regulated springs S.S. connect the bottom bar of the machine with the pair of bars BB, which carry the weight of the man, either by the arm pits, or on a hanging seat when well under way. Limiting cords LL stop the oscillations within safe limits and may or may not have springs inserted in them.

" Surfaces 17 to 18 feet across and 3 to 3.10 ft wide. Hence utmost travel possible for centre of pressure about 0.9 ft. Travel in practice ((from 0° to 3°) about 0.45 ft. Surfaces arched crosswise like a gull.

It may be preferable to place the rear vertical posts one foot nearer the front, thus allowing the rear of the surface to flex upward. The apparatus is



provided with vertical keels.

"The automatic regulation occurs as follows. If a wind gust brings the centre of pressure in advance of the desired position, it increases the angle of incidence and the C.P. moves back, when the spring brings the surfaces back again. The reverse takes place when the centre of pressure moves back of the pivot, the latter being located with reference to the movements of the c.p. on the concave surfaces, made to resemble as near as may be the two extended wings of a vulture, when flattened under air pressure.

"I need not explain this in greater detail, as you tried substantially the same thing yourself; only you pivoted your surface at the front, instead of at the c.p. and you had only one spring. I understood from you in Washington that you had given it up; wherein I think you were wrong, as it seems to me to promise good results, although it may be tedious to get at the best position of the pivot, and at the regulation of the springs.

"

"It is evident the principle may be tested with one or two surfaces, instead of five. I propose three methods: 1st as a kite, a la Hargrave, whose two last pamphlets I send you; 2nd as a glider, as recommended by Lilienthal on pages 14 & 15 Aeronautical Annual of 1896, and page 97 Ae. Anl of 1897; 3rd as projectiles, either impelled by hand or by stretched rubber ~~bands~~ threads. When, after preliminary models, we proceed to the testing of a full sized machine, we will pursue the same methods; substituting running over the ground to the rubber threads. Should this prove successful, not only will it do for the traveling show I have mentioned, but I expect it will perform soaring flight, under favorable circumstances of a rising trend of air.

"For I think now I know how birds perform the feat. Curiously enough I did not get at the secret by observation or by experiment, the two main sources of human knowledge, but by reasoning and computation.

"You may remember that I stated that the mystery to be explained (A^l Annual 1897, p 124) was how the bird could maintain a speed of about 20 an hour, soaring in a wind of 5 to 10 miles an hour. I think I know now, and I can make the further acknowledgement that he can soar in a dead horizontal calm, if the air is ascending five miles an hour. It would lengthen this letter unduly to give you my reasonings and computations, which are at your service, but I trust that you will establish by observation and measurements that the air does rise 5 miles an hour.

"The secret is that in light winds the bird sails at a negative angle of incidence (3° to 6°) which is so small as to escape observation, and which gives upon computation, far greater propelling components than can be figured from Lilienthal's tables of positive angles. This requires, of course, that there should be an ascending trend of air so long as the bird maintains his elevation; when the wind is horizontal or descends, he must descend and gain speed - Keep this to yourself.

"In all your experiments, load all your models to 0.76 pounds per square foot - (1.31 sq. ft. per pound). This is the proportion for a man soaring machine, if he is placed upright. If he were placed horizontally, he could soar with about 0.95 lbs per square foot, as does the buzzard, but I consider this position too dangerous to begin with: The great surface required (260 sq. ft for 190 lbs) makes it imperative that automatic stability shall be first secured.

"Make your apparatus as light as is consistent with strength, and bring it up to the weight required by loading (concentrated) below the point P. You will note that with this arrangement, i.e. disconnection of the body and wings, the man may be dropped even lower than I show him.

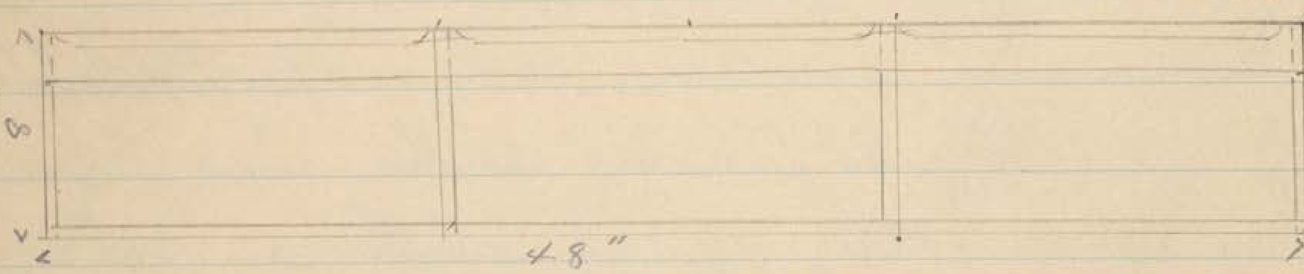
"Let me know whether I have failed to make everything plain, and how you propose to proceed. I have a single surface apparatus to test the principle, but I have not had the opportunity to adjust and try it."

Jan 31 1899

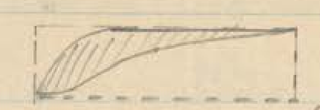
In accordance with instructions from Mr. Chanute, given in the preceding pages, work was begun on the 1st of Jan. 1899 on a double decked model with curved surfaces and a single plane tail. It was Mr. Chanutes intention to have the linear dimensions of this model 1/4 those of the large machine to be built later on. This would have given for the sustaining surfaces a rectangular outline of 54" by 9" each, with an area of 486 sq-in = 3.38 sq-ft; with a spacing between the surfaces of about 8 inches. However before I knew of this I had a model well under way with rectangular outline of surfaces 48" x 8" and a spacing between the two of 7 in, and I proceeded along this line and with these dimensions intending to adapt Mr. Chanute's ~~fig~~ dimensions in the construction model.

These surfaces were constructed by first making a rectangular frame of lynn - a wood remarkably strong for its weight (specific gravity about 60 or 65), fine grained soft, easily worked, and not inclined to warp. Two cross ribs then completed the frame, which had the following appearance and dimensions:

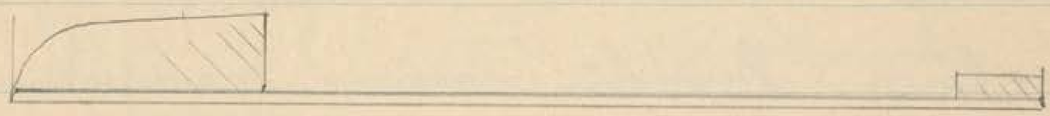
Scale 1/10



The front or leading spar was made from a piece of lynn, having a cross section 2" x 5/8". This was rounded upon the upper front edge, the position of the cross ribs was marked on the lower surface - and the remainder of the wood was cut away from the under front surface to the upper rear surface, the cross section at points between the ribs being as herewith shown.



A cross section through the ribs is shown below



The rear portion of the frame was made of a strip 1/8" x 3/4", to which the cross ribs were fastened. The whole was then covered with Lonsdale cambric, the edges being drawn around the front, rear and end strips and securely fastened with mucilage.

The two middle strips were placed 16" from the ends, were made larger than the end strips, 1/2" x 5/8", set edgewise, and pierced each with 3 holes through which the ~~rotation~~ as wire forming the axis of rotation was to pass. These holes were in pairs and placed rectively 1/4", .3 and .4 the distance from front to rear of the surface.

The upper surface was similar to the lower except in being lighter, the front spar especially being much more so than the one below; being 1½" × 3/8" in rectangular section before being trimmed down.

The two middle lower transverse bars or ribs were, as already stated, pierced with three holes through which a wire, forming the axis of rotation, could be passed to connect the surfaces with the oscillating bars.

These bars were made in accordance with the plan followed by Mr Chanute in his experiments with full sized machines, and together with their connecting bars formed a rectangle, which carried the double surface, the tail and the rudder. Its width was a little short of 16th in., the distance between the two middle bars upon the lower surface. A strong cross bar, set transversely bore a short standard on either end, and these standards were pierced by holes for ~~connecting~~ receiving with the wire forming the ~~[illegible]~~ rotating axis. Cross bars supported the tail and rudder, ~~and~~ diagonal bars were used to hold the rectangular frame in shape. The tail ~~was~~ had the form of a trapezoid, wider in the rear, and the rudder which was rectangular could be put on either above the tail or below it as might be desired. Below ~~are~~ given plan and side elevation of completed model.

Rubber springs were used for connecting the frame with the flying surfaces, their tension being regulated by loosening or tightening the cords by which they were fastened. Stop cords were also put on to prevent too great ~~movement~~ a degree of oscillation.

The tail was set transversely parallel to the wings, but it was found on trial that after a few falls the the frame become distorted so that this ~~tail~~ parallelism, which is an important matter, could not be maintained. A triangular frame was then substituted for the rectangular one, the oscillating bars being brought together in front, ~~the rear~~ and spreading out in the rear. It was found on trial that spruce was too fragile, and oak bars were accordingly substituted in their stead.

These oak bars were at first flattened horizontally, but in springing they tended to alter the setting of the tail, and for these bars flattened vertically were substituted and found to work satisfactorily.

The weight was so regulated as to give about .76 pounds per sq foot of wing surface, but owing to changes in construction it sometimes varied from this, but not materially.

The experiments made with model thus far have been highly encouraging in several respects.

1st In the matter of fore and aft stability. The parts have required no fine nor tedious adjustment. After the first trial has given good results, and usually after a few trials the model has been found to sail with great stead-

2nd and Especially in the matter of lateral stability: This gain has been in part due to the vertical planes, 4 in number, placed between the two leading surfaces, and in part to the superimposed surfaces and the consequent lowering of the centre of gravity.

3rd In the automatic action of the springs. Repeated trials with the ~~strong~~ springs lightly stretched and moderately so and by using cords without springs, and also by trying the ~~surface~~ model without restraining cords of any kind seem to indicate very clearly, that springs moderately stretched give the best results.

The position of the ~~c.g.~~ axis of rotation of the wing surfaces upon the oscillating bars has been shifted from .25 to .4 the distance from front to rear of the surfaces, and the best position seems to be at about 1/3 this distance. Thus if the width of the surfaces be 8 in, the best position of the axis is at about $2\frac{2}{3}$ in from the front margin.

The lifting force has not been as great as it should be, that is if we are to make any approach to the flight of the birds. Thus the buzzard in a calm atmosphere can in gliding maintain a descent of 1 in 10, or even less; whereas the best accomplished as yet in these experiments has been to secure an advance of 4, 5, or 6 feet for a fall of 1 foot. However it is hoped that with a better adjustment of the parts a much better lift can be obtained.

Under date of
~~On~~ the 12th ~~inst~~ Mr Chanute writes an extremely interesting letter showing by computations by means of accepted formulae, including those of Lilienthal on the lift and drift of curved surfaces, that under an assumed upward current, (rising vertically in a calm atmosphere) of 4 miles an hour, which would be scarcely perceptible to the senses, a buzzard can continuously soar without the expenditure of any power on his part, the power being supplied by the buoyant effect of the air. This velocity of 4 miles an hour,* ~~would~~ would be wholly insufficient to sustain the bird in a fixed position, but when coupled with an onward motion of 15 miles an hour, is amply sufficient for support. Mr Chanute does not assume that the bird soars in cir-

cles, and his reason is equally applicable to spiral and rectilinear flight. But it is far easier to explain the existence of the requisite rising current over a small than over an extended area, & perhaps finds a more natural application in the case of ~~circular~~ or spiral flight. (He also assumes that the wings are set at a negative angle of 3° with the horizon.

In conclusion Mr Chanute says:

"The points which are needed to confirm this are:

"1° To establish by observation that the air ~~mass~~ does rise in a horizontal calm, and that its speed is 4 miles an hour.

"2° That the resistance of a buzzard soaring at a negative angle and 15.52 miles an hour is not more than 0.0864 lbs.

"3° That the soaring buzzard in calms has a speed of 15.52 miles an hour.

"4°. That he has a negative angle of incidence."

In regard to the 1st of these points, it does not seem necessary to establish that everywhere the air is rising at the rate of 4 miles an hour during a calm, but only under the circumstances under which the bird is known to soar. As a matter of fact the bird seldom maintains his elevation for any length of time in direct flight during a horizontal calm, but will be found coming down whenever he essays to do so. In such weather he only succeeds in soaring (that is in ~~R.~~ continuously flying without flapping) when moving in circles, varying from perhaps 100 to 200, 300 or even 400 feet in diameter, and when a number of birds are soaring in company, they do not range widely, but keep within com atively narrow bounds. Another significant fact is that when hawks and vultures are seen soaring in circles at the same time and in the same neighborhood they will almost invariably form a part of the same flock, and as these birds have nothing in common we may safely infer that they are drawn together by some cause other than the gregarious instinct, and this ~~cause~~ is most likely a rising current, which they have found or produced or into which they have drifted. It has been generally been argued that one vulture seeing another descend ~~has~~ at once joins him and others follow "until the air is black with pinions." all in quest of prey. But how shall we account for the ~~hawks~~ presence of the hawks? In its daily life the soar-

27 January

ing birds demands rising currents as the fish demands water and it has learned by instinct ~~that~~ to join any flock of birds that are soaring. At any rate almost all my observations have gone to confirm me in the belief that in calm weather rising currents are isolated and much limited in cross section to a small compass. The problem thus resolves itself into determining whether or not there is invariably, or commonly, a rising current of 4 miles an hour where the birds are found soaring.

Almost the only evidence we have on this point, aside from the maneuvers of the birds ~~themselves~~ themselves, is that in exceptional cases leaves or other light materials may be seen floating upward over the same locality where the birds are

Shrouds of silk might be sent up by pulling them apart until the whole formed a "fluffy" mass easily carried about by the currents, and I have many times sent them up to great heights, even out of sight; with a velocity that was often greater than 4 miles an hour; and the current of air must have been greater still, for these shrouds will come down in still air at a rate of ~~4 or 5~~ 3 or 4 feet per second; but I have never tried sending them up where the birds were soaring. This seems to be an experiment worth trying when the weather grows warmer — it would be best tried in June, July, August, Sept. and October.

But there is evidence offered by the bird, which is by no means rarely to be seen, which to my mind offers soaring. I have myself seen this in but few instances. In one case a large number of buzzards were soaring above a hill on a calm day, with their wings drawn back in such way as to indicate the presence of a strong upward current, and I fortunately noticed a silver ~~colored~~ leaf turning over and over in the midst of the flock and steadily rising. The rate of ascent of the air was of course much greater than that of the air; and seemingly must have been at least as great as the rate at which the leaf would have fallen in still air, or perhaps some 8 or 10 feet per second. It is difficult to detect and still more difficult to measure ~~these~~ the rate of ascent of these currents, and I hardly know how the latter is to be accomplished.

almost incontrovertible proof of that the bird soars in a rising current.

And now I think I have Mr Chanute "on the hip"; for when I asked him some years ago how he explained the feat of one buzzard plunging almost vertically downward in pursuit of another, to a distance of 50 feet, and returning without a stroke of the wing to its original position, (misunderstood) he replied that he simply did not believe the feat was even performed. Yet I have since then seen it performed very many times.

In this feat, which is most frequently seen in the breeding season, I see strong evidence of an upward current; for it can hardly be explained otherwise than on such an hypothesis, and is easily explained on the assumption of an upward current.

Another method which suggests itself

is to send up small balloons. But these would rise in still air and little could be learned in this way; though we might learn much in regard to the trend of the currents. On the whole the only feasible method that now suggests itself is that of sending up the silk cords.

To go back a little, another evidence of a strong upward current is not infrequently to be seen in the manner in which the buzzard holds his wings while soaring.

In ordinary descent the position of the wings seen in vertical projection is shown in the figure , as when descending in a calm at 1 in 10, or when soaring ~~when~~ under ordinary circumstances. But when coming down rapidly, either in curves or along a straight line, at a rate of say 1 in 5 — the wings take this position. ; being somewhat bowed and drawn sharply back along the primaries - the effect being to impart to the wing surface a large negative angle, and to decrease the effective sustaining area. Now occasionally when a large number of vultures are collected upon the summit of some hill, especially near sunset, every bird in the flock will be found with the wings thus drawn back, and whenever the wings are straightened out the bird will bound upward; the ap- ~~parently~~ parent difficulty being to keep down. The bird is in fact rapidly descending through a current rising still more rapidly. Invariably the flight in these cases is rapid. The ~~of~~ bird in reality maintains a pretty nearly uniform elevation. How is this to be explained on any other supposition?

I know of no ~~know~~ direct way of determining the resistance encountered by the buzzard when flying at a velocity of 13.32 miles an hour and a negative angle of 3°. I estimate however that a buzzard weighing 5 lbs. can glide downward in a still atmosphere at a velocity of say 18 or 20 miles an hour with a fall of 1 in 10 or even less. The total resistance in this case would then be 1/10 of 5 lbs, or .5 lbs.

A part of this is wing resistance, by which is meant the resistance due to the inclination of the expanded surface to the line of flight; the remaining portion being due to the edge resistance of the wings and the resistance of the body. How great the wing resistance, with curved surfaces we do not know, but it may be said that it is less than .5 lbs, since if it were as great as .5 lbs there would be nothing left for edge resistance. If we consider flight with plane surfaces at 15 miles an hour and a descent of 1 in 10, we must suppose that the wings are set at an angle of less than 1 in 10, say 1 in 12, to the line of flight.

The first gives an angle of 5° 40′
" latter " " " " 4° 44′
The angle of inclination ~~to th~~ above the horizon are equal to 0° 56′,
and we would have a propelling component ~~of~~ of the normal pressure to overcome the head resistance, which would upon this assumption $= (\frac{1}{10} - \frac{1}{12}) \times 0.5 = 0.083$ lbs.
This resistance which is based upon a supposition of $\frac{1}{12}$ made at random is singularly close to the resistance as calculated

by Mr Chanute, namely, 0.0864, with weight assumed at 4.25 lbs, instead of 5 lbs.

But with a plane set at an angle of 4° 44' we have for the normal pressure, which may be taken as the lift (and assuming 5 sq ft of sustaining surface,

$$P = .005 V^2 A . \frac{2 \sin \alpha}{1 + \sin^2 \alpha} = .93$$

This it will be seen is wholly inadequate to sustain a weight of 5 lbs, and apparently the bird must find a very great advantage in the use of curved surfaces. For it is well to emphasize the point that with plane surfaces set at an angle of 4° 44' with the line of flight it is impossible to carry 5 lbs, or anything like it at a velocity of 15 miles an hour and a descent of 1 in 10. Or to put it differently, in order to fly with plane surfaces, even with no ~~edge~~ edge and body resistance, the rate of descent must be much greater than that of the buzzard with surfaces similarly weighted. Of course where there is a rising current the rate of descent is diminished or destroyed and the bird will sail or soar horizontally; but the above reasoning will still hold good, and points to the great advantage over planes. Still it should be added that in practice, as far as the experiments I have witnessed go, the great advantage of the curved surface has not yet been utilized.

Feb. 13:-

February is half gone and our progress thus far has not been as rapid as we seemed to have reason to hope. This has in part been due to the unfavorable weather. A portion of the time there has been rain, but even on the fair days, when everything would appear to be fabrable, a light irregular wind has usually been blowing from the west of south west (west south west) and as the ridges in this section bear south-west and north east, with the steeper slopes upon the eastern sides it has not been easy to find a suitable hill facing the wind. for making experiments with models as heavy as those we have been using. As a consequence on the steeper slopes there has usually been a downward trend of the wind, ~~ona~~ and singularly enough this downward trend has been marked on level ground, especially in the woods on the ^bald^ summit of a certain hill near Chucky City, where these experiments have chiefly been conducted, but even on the slopes facing the wind. It hardly seems credible, but when preparing to cast off the model ~~into~~ the wind has appeared at times to be beating down upon it. But just as on warm summer days the air everywhere seems to be rising, so in this cold weather it seems to be falling, and the one phenomenon is perhaps as easily explained as the other (or ~~with~~ as much difficulty).

Consequently the progress made has been chiefly along the line of stability. A two surface model with a single ^plane^ tail and double wing surface separated by 4 vertical surfaces (plane), wings & tail ^set transversely^ horizontal, seems to possess a greater degree of stability than single surfaces better in a lateral and a fore and aft direction. It seems to possess a good deal of the stability of the Hargrave cellular kite, and due in part I should say, to the vertical sections and in part to the lowered centre of gravity. The great trouble in my experiments hitherto (until I began applying Mr Chanute's ideas, has been to overcome the lateral instability. Many of my models used to fly steadily for considerable distances, as steadily as a hawk in sailing, until some chance turned it aside, when with a surprising loss of sustaining power it circled rapidly to the earth. But now we seem to be overcoming this lateral instability to a noteworthy extent, at the same time that we are maintaining a good fore and aft stability.

One great difficulty continues to be with the lift. The model comes down at too steep an angle - one whose lowgust is 1/6, 1/5 or 1/4. I doubt if ~~any~~ we have attained to anything better than 1/6. This is much less than I used to get with lighter models, and as a consequence our flights now are necessarily shorter. Still my long flights were obtained in warm weather and we are now experimenting in cold weather, which perhaps accounts for much of the difference.

As the result of our correspondence Mr. Chanute has decided (Feb 13) to continue these preliminary experiments until his return from Germany. "By that time" he writes, "May 1st at the latest, we ought to have determined all the points ~~dis~~ discussed, and to have completed a last model with 5 surfaces, 1/4 size of machine."

We may be said to have already set: that the chief point, namely, that the automatic stability is increased by the use of the springs arranged according to Mr. Chanute's idea. Of the advantage of these springs I have no doubt: Whether they offer a complete solution of the problem of fore and aft stability in a gusty wind is one of the points yet to be determined. Another is to find the best position ~~of the axe~~ of the axis of rotation, both in a vertical and fore and aft sense. That is, should the axis lie below or between the (two) surfaces? if between should it lie above or below the middle point? ~~What~~ What point, measured back from the front edge toward the rear will give the best results (so far a point about 1/3 the distance from front to rear seems best.). I have an idea that a double tail, one attached to the horizontal bars, the other to the ~~wing~~ surfaces would increase the stability. It is to be hoped our bad weather is about over. We usually have some fine weather here in February.

Feb 28

The bad weather continues almost uninterruptedly. As a consequence the experiments made since the last entry have been few the time being occupied with indoor work, chiefly in getting out ~~the~~ materials for the 5-surface model. I have gone over the computations of Mr. Chanute in his letter of Jan 12 ~~g~~ on showing that on accepted principles of air pressures the buzzard can soar in a horizontal calm, provided there is a vertical rise in the air of 4 miles an hour. I find them substantially correct. There seems to be two or three arithmetical of minor importance running through the computations. These have been noted in pencil on the margin of the letter.

March 15.

Much bad weather has prevented much experimenting since the last record was made. Still enough has been done to pretty accurately locate the seat of the difficulties to be overcome in seeking after automatic stability. I shall accordingly here give a somewhat extended account of these difficulties and of the action of our models in a wind of variable velocity.

I shall first point out wherein, in my opinion, the celebrated principle of Penaud fails, at least when we substitute ~~plane~~ curved surfaces for plane ones.

Penaud sought to preserve the ~~auto~~ fore-and aft stability by setting the tail at a smaller angle of elevation above the line of flight than that given to the wings. If then the model, with wings and tail so adjusted with reference to the centre of gravity as to be in equilibrium at a certain angles of wings and tail - say for instance 6° for the wings and 2° for the tail, - if, ~~I say~~, the model should for any cause be deflected from its course, so that the angle of both wings and tail should be decreased or increased, the corresponding change in pressure upon the surfaces will be greatest upon the tail and so tend to bring it back to its original position.

To take a numerical example: let the two wings have ~~a~~ combined area of 2 sq. ft; the tail 1 sq. ft; all the surfaces being so narrow as to cause but little shifting of the centre of ~~pr~~ pressure back and forth through changes in the angles of elevation. Let the angle of elevation of the wings be 6°, of the tail 2°, and let the velocity be 15 miles an hour. We then have for the pressure upon the wings

$$P = .005 \times 225 \times 2 \sin 6° \times 2$$

$$= .47 \text{ lbs - very nearly.}$$

For the tail

$$P = .005 \times 225 \times 2 \sin 2°$$
$$= .078 \text{ lbs. approximately}$$

The total pressure ~~weight carried~~ would then be .47 + .078 = .548 lbs. The centre of pressure ~~gravity~~ would then be ~~f~~

$$\frac{.078}{.548} = .143$$ of the distance between the centres of pressure upon wings and tail, measured back from the c.p. upon the wings. If the distance between these centres be assumed at 2 feet, the c.p. of the whole would lie .286 ft = 3.43 in to the rear of the ~~front~~ c.p. of the wings. If a weight of .548 pounds were applied at this point and the model were driven forward horizontally with a velocity of 15 miles an hour & all the pressures upon it would be uniform.

Suppose the angles of elevation are increased from 6° and 2°, to 7° and 3°; we then have for the pressure upon the wings

$P = .005 \times 225 \times 2 \sin 7^\circ \times 2$

$= .549$ lbs.

Upon the tail

$P = .005 \times 225 \times 2 \sin 3^\circ \times 1$

$= .118$ lbs.

The total pressure ~~is~~ now becomes

$.549 + .118 = .667$ pounds.

The c.p. is

$\frac{.118}{.667} \times 2 = .5$ ft $= 6$ in back from the centre of pressure of the wings.

The pressure has thus ~~increased~~ from .548 lbs to .667 pounds and the centre of pressure has moved back $6 - 3.43 = 2.57$ in.

We now have a weight of .548 pounds applied vertically through the original centre of pressure, and a pressure of .667 lbs, which for the present we may assume to be applied ~~horizontally~~ vertically, at the new centre of pressure, 2.57 in. back of the original c.p.

The effect will be

1st To raise the model, as the pressure now exceeds the weight:

2nd To cause the model to so rotate about a transverse horizontal axis as to raise the tail and decrease the angles of elevation of the wings and tail, equilibrium being restored when these angles are reduced to 6° and 2° respectively.

3rd To increase the resistance; upon the wings by about $\frac{1}{6}$, upon the tail by about $\frac{1}{2}$.

Assuming that there is no edge resistance to be over come, the original resistances may be found approximately by the formula

$$R = P \tan a$$

For the wings

$$R = .47 \tan 6° = .049 \text{ lbs.}$$

For the tail $R = .078 \tan 2° = .0027$ "

Total resistance $= .0517$ "

Increasing the above resistances by $\frac{1}{6}$ and $\frac{1}{2}$ we obtain

For the wings

$$R' = .057 \text{ lbs}$$

For the tail

$$R' = .0041$$

Total resistance = .0611

The total resistance has thus been increased from .0517 to .0611 or about 18 percent.

4^th^ To reduce the velocity of the model. This follows from the increased resistance, assuming ~~that~~ the force applied is constant, (a point which will be considered further on). The amount of this retardation is not easily calculated, as the element of time must come in, but assuming that the velocity will vary inversely as the square root of the resistance (force constant). {I am using force here in the sense of pull, not h.p.}. we will have

$$V' = V\sqrt{\frac{.0517}{.0611}} = 15 \times .92 = 13.8 \text{ miles an hour.}$$

When therefore the model has righted itself, the angles of wings and tail being reduced to

6° and 2° and the c.p. brought back over the centre of gravity, the whole ~~would~~ will be in equilibium, except for one thing, which Penaud seems not to have considered, namely that the velocity being reduced to 13.8 miles an hour the pressure is no longer equal to the weight, and the model must fall.

The decreased ~~in~~ pressure amounts to

$$\cancel{.xx4}\ W \times \left(\frac{13.8}{15}\right)^2 = .848 \times .92 = .604 \text{ lbs.}$$

Let us consider what follows from this in accordance with Penaud's principle.

The c.p. lying immediately over the c.g. the model, moving horizontally with a velocity of 13.8 miles an hour, must fall parallel to itself; but in so doing the combination of the vertical decent and the horizontal movement must give an increased angle of elevation relative to the line of flight which is now descending. Suppose as before that this angle is increased 1°, so that the angle of wings and tail are again 7° and 3° with the line of flight. This will cause the centre of pressure to move backward 2.57 in, as before, and we have a weight of .548 lbs pulling the model down, and a pressure of .504 lbs, acting 2.57 in in the rear, pressing it up. The model in consequence alters its angle of elevation relative to the line of flight, which is descending, and finds its equilibrium only when the angles of wings and tail are reduced to 6° and 2°; that is, when the model points downward 1°.

In the mean time the velocity has not increased (assumed pull in horizontal position being constant) but on the contrary has decreased, for the same reason as before, namely the increased resistance due to the increased angles of elevation - and when the model has turned downward 1° the weight is still ~~less than the pressure~~ greater than the pressure; the angles of elevation of 6° & ~~3~~ 2° are therefore again insufficient for the support of the model, which must again fall, again have the angles increased, again have the c.p. moved backward, again tip downward, with the line of flight descending we may say at 2°, and so on indefinitely, until the model plunges head downward to the earth.

From this it appears that with an ideal Penaud model, flying horizontally, with an inclination of 6° and 2° for wings & tail, and an original pull sufficient to produce a velocity of 15 miles an hour, all ~~parts~~ pressures being in equilibrium, this equilibrium if once lost will, upon the Penaud principle, never be regained. For the velocity once altered, as it must be when the angles are altered, cannot be restored until the angles are ~~so~~ brought back to what they were at first; and this cannot happen until the velocity is restored. There will therefore be a pair of forces acting upon the model to turn it over end for end in whatever way it may have at first started. over

I have said that this is the effect of the Penaud principle acting upon an ideal model originally endowed with a uniform motion and a constant pull in a fixed direction. For the Penaud principle takes no account of the righting effect of gravity, which appears in the enunciation of his proposition and its demonstration as dead weight acting vertically. There is no resolution into ~~vert~~ other components and the simple relative changes in the ~~setting of the~~ angles of elevation of the wings and tail are supposed to be all sufficient for fore and aft stability. This is the principle which Secretary Langley describes as "admirable, admirable, admirable! and again admirable!"

So it would be if the resultant of pressure ~~[illegible]~~ varied only in position and not in intensity as well. But no account is taken of its intensity which varies with the angular elevation, nor of the varying velocity which must result from changes in the angles of elevation. There is no practical way ~~of~~ known of maintaining a constant pressure upon surfaces whose angles of elevation vary. The pressure increases as the square of the velocity and for small angles very nearly as the angles them-selves, so if ~~we~~ v be the velocity and θ the angle of elevation then

$$v^2 . \theta = k = \text{a constant}$$

would give a uniform pressure. But how is v^2 to be made to vary inversely as θ, even theoretically? And when we introduce edge resistance, ~~rotary~~ ~~vel~~ inertia and flexing of surfaces, how insuperable the difficulty becomes! The Penaud principle may be advantageously applied perhaps in connection with others, but it is based upon the assumption of a constant vertical pressure, which is unallowable even theoretically; and I have reason to believe, from numerous experiments that I have made (notably those made two years ago with gliding models having single surfaces) that the principle of Pénaud in operation will, as I have reason out, tend to overturn a model in whatever way it may get started. So that it is very far from being a perfect principle when applied in practice. No doubt if a model tends to rear in front and turn over backward, the sudden action of the Pénaud tail under increased pressure will tend to check its rotation; but it cannot prevent the checking of the velocity nor of itself counteract the evil effects that follow; it can neither restore the velocity nor the original angles of incidence, and were it not for the play of gravity and of another principle to be described presently, a principle of others not, well understood it would invariably bring the model either head downward to the earth.

The principle referred to is the shifting of the position of the centre of pressure upon both wings and tail under varying angles of elevation; the principle first enunciated by Sir George Cayley and elaborated by Drzewiecki and already referred to by Mr Chanute on page 3 of this book.

This principle acts much as does that of the Penaud tail, the c.p. shifting backward as the angle of elevation increases and forward as it decreases, and the same objections may be ~~used~~ urged against ~~this principle~~ it as against the other; that acting alone it tends to finally overturn the model.

In order to compare its efficiency with that of the Penaud tail under similar circumstances, let us take the case of the model already described, with wing surfaces of 2 sq.ft. area, tail 1 sq.ft; angles of elevation of wings and tail 6° and 2°; ~~and~~ velocity 15 miles an hour, and let the angles of elevation be increased to 7° and 3°. We have the following formula for computing the centre of pressure

$$d = (0.2 + 0.3 \sin \theta) L.$$

This is for planes

Here d is the distance from the front margin to the centre of pressure of the wing, and l = the width of the wing. Let $l = 4''$. Then for the wings set at 6°

$$d = (0.2 + .3 \sin 6°) \times 4'' = .93''$$

for $\alpha = 7°$ $$= (0.2 + .3 \sin 7°) \times 4'' = .95''$$

Distance shifted = .02″

For the tail

at 2° $$d = (0.2 + 0.3 \sin\theta) 4'' = .84''$$

at 3° $$d = (0.2 + 0.3 \sin\theta) 4'' = .86''$$

The centre of pressure upon each surface is thus moved backward about .02″.

By the preceding calculation, p 43. the resultant centre of pressure to begin with was found to lie 3.43″ to the rear of the c.p. of the front wings, and consequently 20.57″ in front of the rear. Consequently the resultant of pressure will now lie 3.45″ behind that of the front surface, or .02″ behind. the c.g. In case of the second tail the c.p. was moved backward 2.57″ (p 43′). It is evident therefore that for small changes in the angle of elevation, when the angles are small, the change in the c.p. is far greater in case of the Penaud tail, in the ratio of 2.57" to .02". For small angles therefore and small changes in them, the centre of pressure varies very little in the first case, and that the corrective effects of the Penaud tail would be very much greater. For large angles the difference is not so great, but with ~~two~~ narrow surfaces set at the same angle, there is in no case much shifting of the C.P. ~~where the surfaces are widely separated~~

It seems possible, by ~~?~~ setting the tail at a larger angle than the ~~fr~~ wings to reverse Penauds principle and throw the c.p. forward with an increase of the angle of elevation, and as the c.p. with wings set at equal angles always moves back, the angles might be so adjusted that these movements would counteract each other and so maintain a very nearly constant C.P. And that more soaring birds, like the black vulture and the hawks, which are soar with an expanded tail have the latter set at a greater angle than the wings, reversing Panaud's plan.

The question arises as to whether or not we want a combination of surfaces whose centre of pressure shall be constant in position. If it were, then at whatever angle the wind might strike the apparatus there would be no tendency to alter its position except to raise or depress it; the model being always parallel to its first position. If this position were a horizontal one, or one dipping slightly forward, it would seemingly give what we want. But if by any chance it got out of the ~~horz desired~~ the proper position, there would then be no force acting automatically to bring it back into position, and such a model might come down vertically, or slide down backward, or shoot down head foremost, or take any other erratic course. So a fixed centre of pressure does not seem to be altogether a desirable thing. The important thing, and the most difficult to attain, is to secure an approximately ~~fixed~~ constant pressure at small angles. The position of the c.p. is of subsidiary and chiefly important only as it helps us to attain a constant pressure. How difficult this problem is is shown by the fact that the variations in the vertical trend of the wind from second to second ~~are~~ ~~fo~~ average perhaps as much as 2° or 3°, and with a model adjusted to an angle of 6°, this would mean a ~~dif~~ variation in lifting power of from 1/3 to 1/2 - since the ~~dif~~ pressure varies very nearly as the angle. Seemingly the most feasible method of accomplishing this would be by elastic surfaces, and we are therefore probably on the right track.

A still more serious difficulty to be overcome in these experiments lies in the rapid fluctuations in the velocity of the wind, especially in its sudden cessation at times. For instance if the experiments are being conducted in an irregular wind blowing up a steep incline, the model will be found at times to be sustained in a horizontal position for several seconds in succession, it may drift back or advance or turn and fly away sidewise. But it often happens that when brought to a stand still by a strong wind, this wind will suddenly die away and leave the model in almost still air with no velocity and acted on only by gravity, pulling it vertically downward. It will of course at once begin falling and the fall will be broken by the expanded wing and tail surfaces, and if the model remained entirely vertically it would ~~come~~ slowly settle down to the earth, or if a light wind were blowing it might do the same.

Then as the wind freshened it would rise again. If on the contrary when left thus suspended in still air, the head should drop downward a few degrees, the model in falling would begin to glide ~~sop~~ forward with an increasing velocity; and we would have what we most desire, a steady gliding flight downward. If on the contrary the head should go up the tendency would be to slide backward, and if in this position it should be struck by a gust of wind it would be bodily blown away before it. It is necessary then that when left suspended, or what is equivalent when dropped vertically in still air, the model should pitch slightly forward, and only slightly so. This will happen if in this position the centre of gravity lies slightly in advance of the centre of pressure upon wings and tail, and here we have an important matter to consider. For if the wings and tail have the same setting

, within a few degrees, the pressure upon each will be very nearly in proportion to their areas, and the centre of gravity will need to be located accordingly; in other words the tail must bear a little less than its proportional part of the weight. This would turn the model down in front as it descended and it would begin to glide forward.

As soon as it begins to glide forward however the angle made by the wings and tail with the line of flight is no longer approximately 90° as it is when falling vertically, but may be reduced to a very small one as the descent becomes more rapid. The flight should remain steady if the ratio of lift for wings and tail, ~~and this~~ should remain unchanged. But if they are set at different angles this ratio may come to be very different, and the centre of pressure may in consequence be thrown either far forward or far backward, with wholly unsatisfactory results in either case. Let us take again the example with which we set out, namely one having a wing surface of 2 square feet and a tail surface of 1 sq. ft:- the centres of pressure being 2 feet apart, the angle of elevation of the wings differing from that of the tail in any position by 4°. This makes little difference when the model is allowed to drop in still air, for if the wings were set at 90° with the vertical the tail would be set at 86°, or more properly speaking at 94°, its angle with the horizon being negative. The pressure pressure upon the tail would then be to that upon the wings in the ~~rad~~ ratio 1 to 2 very nearly, and the centre of pressure would fall 8" ~~16"~~ behind the centre of pressure of the ~~front~~ wings. If now we place the centre of gravity 2" in front of the centre of gravity, in order to insure the model's tipping forward and gliding, we will find that when it does begin to glide and has attained ~~reached~~ the desired velocity of

15 miles an hour, with a wing elevation of 6° above the line of flight and a tail elevation of 2°, the centre of pressure will have ~~passed~~ moved forward
page 43
from 8"~~.66~~" to 3.43" in the rear of the centre of pressure upon the wings; that is, from being 2" to the rear of the c.g. it would now be 6" ~~6~~" − 3".43 = 2".57" ~~2".59~~ in front of it.
The model would therefore turn up rapidly in front, would glide upward until its velocity were about exhausted, the rotary inertia would carry it beyond the horizontal position, the pressure would ~~become~~ have a negative horizontal component, and the model would begin gliding backward; then it would rise in the rear, tumble forward, spin round like a leaf, and no flight proper would result. An observer would say unhesitatingly that the c.g. was too far back, and so it would be for gliding at small angles;

but to begin with it was properly located.

This follows from the use of the Penaud tail, and also from the shifting forward of the c.p. according to Joessel's law. This latter cause (see p 53) would have shifted the c.p. still further forward than the Penaud tail brought it in the preceding example, by about 1.5" making the final position 4.07" ~~+1.93"~~ in front of the c.g.

If on the contrary we wish to use the Penaud tail and secure stability for fine angles, we shall find that the advanced position ~~xx~~ of the c.p. renders the model wholly unstable for the coarser angles, and in a gusty wind it is very necessary to preserve the equilibrium under all conditions. Here we are confronted with two apparently incompatible conditions: we _must_ have the c.g. placed far in front for gliding at small angles, otherwise we can never gain the requisite velocity through the action of gravity, and we _must_ have it far ~~to the~~ rear in order to ~~maintain~~ the stability at large angles.

What can be done to overcome this difficulty? which is not a theoretical one at all but one we have to contend with in almost all our experiments? It seems useless to try to get around the necessity of placing the centre of gravity far back for flight at coarse angles, and we must therefore look to evading the necessity ~~for~~ placing the c.g. so far in front in case of the finer angles. This can theoretically be accomplished by abandoning the use of the Penaud tail, and setting the ~~angles~~ wings and tail at the same angle, or better still by setting the tail at a greater angle than ~~that~~ of the wings. That is, as the tail will in spite of all we can do (provided it is fixed in position) lift at its proportional part when the angle with the line of flight is coarse, let us make it do the same thing when the angle is fine.

That there is some serious difficulty encountered in attempting this is evidenced by the fact that it is contrary to the practice of all experimenters, my own included. So much have I been impressed with the difficulties attending this arrangement, that I have endeavored to ~~have~~ the tail bear no weight at all, but to ~~act~~ always and only as a guide. Some experimenters have even gone farther and placed the centre of gravity ~~so far~~ forward that of the centre of pressure ~~upon the ~~ ~~forward~~ ~~of the centre of~~ upon the wings, so that the tail necessarily receives its pressure upon the upper surface, and my own experiments have convinced me that surest and steadiest flights can be obtained in this way. But it is wasteful of energy, owing to the downward pressure upon the tail, and it the arrangement has the serious disadvantage that in a lull of the wind the model is liable to plunge head down-ward and if this is followed ~~by~~ a sudden gust, the wind is caught upon the upper surface of the wings and the machine is over-turned. It has been proposed, and the ~~plan~~ proposal has been carried out, to make a hinged tail, which shall receive pressure only upon its upper surface. This prevents the model plunging down but offers no precaution against its tipping up in front, as there is nothing to prevent the front edge tipping upward.

The chief objections to be urged against setting the tail at a steeper angle than the wings and placing the centre of gravity so far back that each surface shall bear a weight ~~somewh~~ nearly proportionate to its area, is ~~that~~ with this arrangement the lateral stability is almost completely lost, and the whole apparatus is liable at any time to swing completely round and go him tail foremost. This must necessarily be the case unless we can arrange to have more lateral resistance in the rear than in front. An Indian's arrow goes straight ahead, because of the heavy point that leads it; whereas an arrow without a point; or one with the small end leading cannot be shot with any degree of accuracy; and the same principle applies to the flying model. What we need is a feather in the rear; vertical surfaces offering resistance but giving no lift; and if a sufficiency of these be supplied there seems to be no theoretical reason to be urged against placing the centre of gravity well to the rear.

March 31.

Continuing the discussion began March 15, which will soon lead to a record of the experiments made (my reason for introducing this long discussion here is to enable us the better to understand the results obtained by experiment):

When in 1894 Secretary Langley had Mr. Herring and myself make some competitive trials with gliding models, one of the tests was to be that the model should quickly right itself when dropped vertically, head downward. This seemed to be an extremely rigid test, and greatly enhanced the difficulty of getting a model that would fly at all. For the reason that the arrangement of surfaces which was found to give good results when the model was properly launched in the horizontal position, was found to fail completely (or almost so so) when the models were dropped head down. (They were dropped from the ceiling of the large western hall of the Smithsonian building). In view of the variable nature of the winds with which we have to deal it seems as if some such test must be applied before a machine can be pronounced altogether safe for all the emergencies of flight. Still much ~~out~~ ought to be left to the skill of the operator, and the test of the Secretary must be regarded as extremely rigid and not perhaps necessary in practice. However I am more and more inclined to believe that the tail must in some way or other be made to lift its proportionate part of the weight under all angles of inclination.

This brings us to a new problem: Suppose a model to be gliding steadily downward with a descent of 1 in 6 in a truly straight line, and that the head resistance is less than the pull of gravity in the line of flight; what course will the model pursue? Without the light of experiment I should say that the increased pull would add to the velocity; this would result in increased pressure upon the flying surfaces, and the model would be lifted from its original course, but would remain parallel to its original position. For if the pressures vary as the square of the velocity they should increase proportionately on all parts as the velocity increased and the resultant of pressure would remain unaltered in position, except that possibly in rising the angle of elevation relative to the line of flight which we may assume would now be curving up-ward, might be slightly increased, throwing the c.p. very slightly forward. Experiment shows that in general the model will not rise bodily and parallel to its first position, but will increase its angle of elevation by rotating about a transverse axis; so that the angle of elevation with reference to the horizon becomes greatly increased, and the rate of descent much less. The model will often assume a horizontal position and even be found sailing upward. The inevitable result must be (and all our recent experiments bear this out) to decrease the velocity and correspondingly the lift; and as the inertia of the model is overcome it is necessary for it to take a fresh start by plunging downward at a steep angle, when the whole process is repeated; but it may be with this difference, that it descends at a steeper angle than before, rises more abruptly, loses more of its velocity; and so goes zig-zag-ing along, the flight getting continually worse. The remedy is to push the centre of gravity forward, which will decrease the undulations and give a steadier flight; provided no gusts of wind are encountered. The course of these undulations seems to me to be the interference with the action of the tail by the wings. Experiments made at the Smithsonian Institution went to show that with plane surfaces the center of pressure ~~went~~ moved very slightly forward as the velocity increased, and that with curved surfaces it actually moved backward. Careful experiments by Mr. Herring & myself also seemed to show conclusively that the relative lift of the following surface, compared with that of the leading surface, varied with the angle of elevation. The experiments were tried on the whirling table, with models having a pair of front and a pair rear wings, equal in area and set at equal angles with the midrod. The whole was carefully ~~bal~~ balanced about an assumed axis of rotation. Thus assuming that the rear wing was one half as effective as the front wing the centre of gravity and of rotation (about a transverse horizontal axis) was placed 1/3 the distance back from the the C.P. of the front wing. This assumption was found to be true for a certain velocity and angle of elevation, while for other angles and other velocities it was very far from being true. In general we found that as the velocity increased the tendency was for the models to rise in front. This was also the case if the angle of elevation increased. and as the models were free to turn any increase of velocity which elevated the front surfaces, at the same time increased the angular elevation as well, and this elevation still further altered the relative pressures upon the front and rear surfaces. The models therefore tended to fly up or down in front somewhat suddenly, according to the direction they might first take. The stability was thus found to be wholly unstable.

When the angle of elevation was small the lift of the leading and following surfaces was nearly the same; say 85 per-cent for the rear surface for an angle of 6° and a velocity of 10 to 15 feet per second. (The exact figures I do not remember)

All this harmonizes with the action of our models in my recent experiments, and in fact in nearly all my experiments. If the model when properly balanced in a rising current (wind ascending a hill) for steady hovering flight is struck by ~~an increased~~ a wind of increased velocity; or by one having a steeper trend, the effect is the same in either case, just the opposite of what we want, ~~is~~ the model rising in front, until it points sharply upward. Under the increased pressure it will then be lifted upward and driven backwards. It may then be ~~driven~~ turned to the right or left, swing round and speed away before the wind; or as it begins drifting the relative velocity of the wind will decrease, or a lull will ensue, or the ~~angle~~ of trend of the wind will be lessened, and the reverse takes place; the ~~model~~ rear surface now lifts relatively more than it should, the model turns down in front and descends into the wind, or the wind may suddenly rise and strike upon the upper surface and bring it down suddenly. This is the general result, and the conclusion to which oft repeated experiments have brought me.

As this is contrary and opposite to what might seemingly have been looked for in applying the principle of Caley and Drzewiecki, it may be asked if that principle is itself incorrect? This does not necessarily follow; for the reason that that principle is ~~[illegible]~~ supposed to hold true for a single plane surface, and does not necessarily apply to a combination of surfaces, as ~~where~~ takes no account of the interference of one plane with another, as where one lies in the wake of the other.

As long as the flight is ~~~~ maintained and the angles of incidence remain small, there will likely be but little interference, and while the difficulties herein pointed out are real ones, it may be that in practice with a full sized machine they will not be so formidable as they now appear.

In a letter bearing date of March 15th Mr Chanute raises some interesting questions in regard to the origin of wind gusts and the existence of rotating waves in the atmosphere "like the boils and eddies of a flowing stream", and says that the existence of such has been confirmed by all his experience with man ridden machines. "But the most interesting ~~part~~, "question," he writes," is the path of the particles; whether they rotate in a vortex, as Hargrave figures them under his kites, or whether the motion is simply up and down," as that of water in a wave of translation? Or whether we must turn to the theory of waves of vibration?"

Without attempting to answer these questions directly, I will not here a circumstance which I have often ~~noticed~~ observed and which may have some bearing upon the ~~first~~ questions. It is that a model will fly at a lower rate of descent in still air when within about two feet of the surface of the ground, than when more elevated; often skimming along for a surprising distance when very near the earth and almost horizontally. I have thought that the air might be rotating about the model, or bird as it advances, ~~the~~ in ~~the~~ form of a cylinder, with transverse axis, the air rising in front and descending in the rear. If this is true it is easy to see that if the diameter of the cylinder is lessened by contact with the earth the supporting power might be noticeably increased. At any rate the fact is as I have stated it.

I have recently obtained some surprisingly good results in the way of lateral stability by placing a two surface cellular structure, in the rear of a single surface, the ~~model swing~~ tail swinging from side to side and returning to its first position in a way to keep the model in an approximately straight line. This may prove to be an excellent arrangement, as it seems to me. 1st because it enables us to put a number of dragging surfaces in the rear of the centre of gravity; and 2nd because ~~a su~~ two such surfaces in the rear would, if they had a narrow space between them, operate more efficiently when set advancing at smaller than at large angles, and so enable us to place the centre of gravity further forward than if a single surface of an area equal to the combined area of the two were used.

A good number of experiments made to determine the best position of the ~~centre~~ axis of rotation with reference to the vertical line seem to indicate clearly that it should lie near the median of the leading surfaces; that is, if two surfaces be employed, the axis of rotation should lie a little below the point bisecting the space between the two, as in this position the drift above and below the axis is very nearly balanced, while there is a safe tendency to throw the front upward.

Mr. Chanute in his letter of Mar 15 proposes 3 methods of constructing a large machine with folding ribs:

"1° Radiating ribs – used by Lilienthal –
2° Inserted struts – " " Hargrave –
3° Collapsing together " " Wenham ("Prospero" p. 101)"

I am hardly competent to express an opinion on so important a matter, but it occurs to me that a fine surface machine might be so constructed that the wings could be removed from a frame made to hold them and piled one upon the other, or perhaps first folded and then piled up, and that the framing and vertical planes could likewise be taken apart and reduced to a small compass. But I question if I know much about the matter – "The cobbler should stick to his shoes."

April 15

Some recent experiments on the proper position of the centre of gravity in a vertical line seem to indicate that with a two surface machine the c.g. should lie a little below the centre of figure of the two surfaces. If it is placed too low the effect will be that if for any reason the angle of elevation of the wings be increased, the resultant of pressure will pass in front of the centre of gravity, when the ~~__~~ inertia of the body, carrying it onward, will cause the wings to tip back and still further increase the angle of elevation, and so augment the trouble. If on the other hand it is placed above the centre of figure the reverse will take place, the model tending to plunge suddenly downward. In either case there is introduced a force which interferes with the free action of the springs which are supposed to give automatic stability.

The steadiest flight should result when the c.g. is so situated as to equally divide in all directions the drift in the direction of the line of flight; but in order to insure ~~safe~~ a safe rising tendency it should lie slightly below the centre of figure.

I am endeavoring in these notes to find out, not imaginary difficulties, but real ones which have been encountered in the course of our experiments, and so arrive at a correct theory of flight; for until we understand the theory of flight our efforts will be for the most part hap hazard trials followed by haphazard results.

In gliding flight we may assume that the model or bird is drawn onward along its course by the force of gravity acting ~~along~~ through the centre of gravity; for while gravity acts always vertically, the effect is the same if we resolve it into two components, one parallel to the line of flight, the other perpendicular to it. Similarly the resistances or pressures upon the model or bird may be resolved into 2 components, the one parallel, ~~&~~ the other perpendicular to the line of flight; and steady uniform flight will be obtained only when the two components of pressure equal and counterbalance the two components of gravity, and act in opposite directions.

Let us suppose that the pressures upon a model, descending 1 in 6, with a velocity of 20 feet per second, are such as to counterbalance the corresponding components of gravity, and that for any reason the angle of elevation of the wing be increased. This will increase the resistance, tending to stop the model in its course and also to turn it from its course. This calls into play a force which before had remained inert, namely the inertia of the model, acting in the line of flight and through the centre of gravity. This new force will tend to drag the model onward in its original course and not to overturn it; provided the new resistances, like the old, are counter balanced about the line of flight of the centre of gravity. This will be the case where the wing and hull resistances are separately counterbalanced about the c.g. But if in the original position the hull resistance was made to counterbalance the wing resistance (in the line of flight) either wholly or in part, then with an increased wing resistance the equilibrium will be destroyed, and ~~the~~ its ~~rate~~ inertia will tend to overturn the model.

That this balancing of the wing resistances about the centre of gravity, it

least approximately, is of the first importance, hundreds of my experiments have shown.
Thus I have many times had a model so adjusted that for a considerable distance, often as much as 500 or 600 feet the flight was perfectly steady, and then from encountering a change in the direction of the wind currents, or other cause, the model has had its equilibrium wholly destroyed in an instant, and has either rosed upward and stopped in mid air, circled sidewise to the earth, or else plunged suddenly head downward almost vertically to the ground.
Hence I very much doubt the advisability of placing the centre of gravity very much below the centre of pressure of the sustaining surfaces, including the tail; especially if we wish to fly at small angles of descent.
Here we may learn a lesson from the birds. When the buzzard ~~tail~~

directly along a steadily descending course glides downward, it is with wings ~~brought~~ downward almost or quite into the horizontal position (transverse to the line of flight), the V form of the wing being almost ~~wholly~~ solely adopted when soaring. I do know of any bird, except ~~the soarers~~ those which soar in circles, which uses ~~resorts~~ ~~to~~ the V form of wings and these usually only when circling; as for instance the Albatross, which while ~~a~~ one of the greatest of soarers, seldom moves in circles, and only uses the V form when turning in its course.

Why does the soaring bird elevate the wings and the gliding bird depress them? The soaring bird elevates them for the reason that it is only by elevating the outer wing that it can be kept from flying off on a tangent, and no such necessity exists in case of the gliding bird. For instance the ~~Th~~ crow soars with the greatest difficulty,

having never learned to elevate the wings proper-
ly for soaring, whereas its gliding flight with
straight wings is unexcelled.
If we assume that the soaring birds seek
out ascending currents we can easily under
stand why the wings may be more elevated
than in gliding flight. Suppose the ascend-
ing current is of such strength as to keep
the bird aloft without the necessity of any
forward motion. then it is evident that the more
elevated the wings the more steadily will he
be sustained, since in this position the weight
he may and should be as far as possible below
the centre of pressure upon the wings. Similarly
if a bird descends vertically, like the Gyps ful-
vus, as described and figured by M. Mouillard, this
extreme elevation of the wings into the form of
an acute V is the best that can be adopted
for steady flight. If now the ascend-
ing current be too feeble for the sup-

port of the bird in a fixed position; but only sufficient to sustain him when ~~in~~ moving forward with a certain velocity; or when in descending in a calm, he comes down, not vertically, but at a steep angle; he may still with advantage maintain the elevated position of the wings, but elevated to a less degree than in the former case. So in gliding flight the more rapid the descent, whether in a direct or spiral course the greater may be the angle of elevation of the wings; or what is equivalent in to the same thing in gliding flight; the lower may the centre of gravity be placed; while if we wish to descend at a fine angle it is necessary to place the centre of gravity relatively high.

All of our recent experiments point this way, but we are laboring under the disadvantage of a heavy weight (.76 lbs per sq. ft.) which for models, though not for birds, and possibly not for large machines, makes a rapid descent essential. Very many experiments have been made in the hope of flying at a finer angle, but without proportionate success. So that as a matter of fact, our lift now is but little better than it was in January and February. The mills of the Gods are grinding very slowly, and I do not see that they are grinding so very fine either.

I several of his letters Mr. Chanute has expressed a desire to have the soaring experiments of Mr. Lawrence Hargrave repeated, and I have accordingly fitted up the necessary apparatus and have made a good number of experiments along the lines indicated by Mr. Hargrave.

A rope has been stretched between two trees standing on a hill side with a single tree intervening, so that the middle of the rope can be drawn up to a height of about 60 feet above the ground. The slope of the hill is something like 1 in 8, and faces the west, so that the winds prevailing in this section have usually an upward trend of 1 in 8 as they pass over it; as they come chiefly from the west, north west, and south-west. A piece of timber 18 in long, bearing a pulley set transverse to its length on the under side has been fastened firmly to the rope at the point from which the soaring model is to be suspended. A common trout line passes over the pulley; one end of which is made fast to the model, while the other may be held in the hand to regulate the height or fastened ~~in~~ to some suitable object and so keep the model at any desired height.

In making these experiments the model has been drawn up to within 20, 15, 10 or 5 feet of the rope and allowed to swing freely under the action of the wind.

The models used ~~have~~ been those already constructed for gliding and carry about .75 pounds per square foot of wing surface. The one chiefly used has had a double surface in front, each surface having a rectangular outline 48" x 8".

The cord has been attached in a variety of ways, 1st by the nose, so that it should at all times be held against the wind unless soaring into it; nothing however could be accomplished with the cord in this position, as the wind was never strong enough to bring the model into the horizontal position necessary for soaring. 2nd by ~~attach~~ means of a bridle fastened to the nose and tail, so that the model in a calm would & swing into a horizontal position, under the action of the wind it was thought the model would be relieved of a portion or all of its weight, and so rise under the elastic pull of the ~~cords~~ rope and trout line to a greater or less height, according to the velocity of the wind. However it was found that the wind gave to the model a pendulum motion, and as a consequence as the mode swung into the wind the angle of elevation increased, and on the other hand decreased as it swung the other way, so that it was not possible to secure any thing like a fixed angle of elevation. 3rd by means of a bridle attached at the extremities of the wings, ~~in~~ ~~[illegible]~~ above ~~a~~ a line passing transversely through the centre of gravity. The model could now swing like a pendulum in any direction, and while the rotation of the model laterally was in a measure restrained, it was free to take up such an angle of elevation as the action of the wind upon its surfaces might give it.

This last ~~position~~ method of suspending the model was the best tried and worked quite satisfactorily. The model rose and fell under the action of the wind, often parallel to itself, sometimes drawing into the wind, sometimes receding from it; at times it held steadily into the wind, at others turned and ran before it; it rose up, plunged down, turned sidewise, and in general displayed but little stability. Once in a strong wind it rose steadily and vertically until some 4 feet above the rope, and then drew steadily forward into the wind, dragging the trout line after it and passing downward over the rope. In a less marked degree this action was several times repeated as far as rising and advancing was concerned; but only on one occasion did it pass steadily over the rope. Usually as it rose the drift was positive in direction and kept the trout line stretched as the model rose. For the most part the model swung back and forth as a pendulum and the result of the experiments was far from establishing Mr. Hargrave's conclusions, since the force of the winds to draw a bird or model into it should be exerted almost constantly instead of at long intervals, if we are to find in this action an explanation of soaring flight.

These experiments however cannot be regarded as disproving ~~Hargraves~~; since in the 1st place his ~~apparatus~~ model was different from ours, and 2nd the winds in which he experimented must have been far more favorable to securing satisfactory results; his winds being steady, and often having a velocity of 22 miles an hour, while ours were exceedingly irregular and seldom exceeded half this velocity; being in fact seldom strong enough to lift the model.

April 30

For the past two weeks my attention has chiefly been given to the completion of the 5 surface machine, which has been largely overhauled so as to reduce the edge resistance, as Mr. Chanute regards the edge resistance (and hull resistance) as important factors in retarding the velocity of flight. In this I think he is right, and this brings us to a very serious problem: How are we to fly with small angles of incidence if the hull and edge resistances are so great as to preclude the attainment of the velocity necessary to sustain the weight at small angles? In our experiments with 2 surface, and single surface models the best we have been able to do has been to secure an advance of some 5 or 6 feet with a fall of 1 foot – usually the rate of descent has greater than these figures ~~would~~ indicate.

If a model descends at an angle of 10° by gliding, I think we may suppose that the angle of inclination to the line of flight is something less than 10°, for if it were 10° with the line of flight, it would have the cords of the surface horizontal, and therefore the resultant of wing pressures would be very nearly vertical; with plane surfaces it would be so, but according to Lilienthal for curved surfaces it would have a forward inclination (possibly), (though for an angle of 10° it might not have.). But even if we admit that it is inclined forward, so that there is an element acting in the direction of flight which drives the model forward; but it can hardly be sufficient to overcome the edge and hull resistance. As a matter of fact, the angle ~~is consid~~- of elevation above the line of flight is much less than the angle of descent, so that the model comes down much more rapidly than plane wings with no edge & hull resistance would require. But though less than 10° the angle of elevation of the wings above the line of flight is still greater than we want. It is possibly 5° or 6°; whereas we should like to ~~have~~ glide with angles as small as 1° or 2° or 3°. (with a negative angle of 1°, 2°, 3° or 4° with the horizon). But the edge resistance is too great to admit of such small angles and the attainment of the necessary velocity, except with a rapid rate of angular descent, For this there are two remedies in sight; we may reduce the weight, ~~and~~ or we may reduce the edge & hull resistances. It is along the latter line that I have been endeavoring to make progress, and it seems to me now, that with our 5 surface machine the resistances are reduced almost to a minimum.

The surfaces are 5 in number, 54"×9", and depth of curvature 1/2, with highest point of curvature something like 1/5 the distance from front to rear. The frames are made of bass wood (which is unequaled for the purpose, being light, strong and easily worked). The spacing between the surfaces is 7 inches.

The tail is 54"×18" and is borne upon a spine, flattened laterally in front and horizontally in the rear. This spine is made of oak and bears ~~the~~ a cross piece 18" long, upon the ends of which short standards are borne, through which passes the axis of rotation. This axis of rotation passes also through two vertical ~~par~~ bars which pass ~~throug~~ between the 2nd and 3rd surfaces, so that it lies slightly below the centre of figure of the 5 surfaces; this position being chosen ~~on account~~ in consequence of experiments made with the 2 surface machine.

A sled, having a single runner is formed by means of strong pieces of oak which pass downward, backward and upward; ~~being~~ joined to the spine in front and rear, and inclosing the lower two surfaces.

The ~~surfaces~~ surfaces are formed by stretching Lonsdale cambric over the frames, lapping them over about the edges and fastening on with mucilage upon the under sides.

This model is virtually complete and I hope to be experimenting with it within two or three days at furthest.

The recent experiments made with the 2 surface models go to emphasize what has gone before, good though not perfect stability with a poor lift: I think we must be near the limit of the possibilities ~~of~~ with the weight we are carrying.

May 15

Upon trial ~~it was~~ of the 5-surface model it was found that the sled attached to the main spine bearing the tail was too weak to resist the shocks ~~of~~ received on alighting, and after a half dozen ~~trials had been made~~ it went to pieces before anything could be learned. This led me to conclude that a different frame ~~than~~ would have to be employed and I accordingly proceeded to construct a triangular frame, similar to those used on the 2 surface models, which have proved to be very strong for their weight and able to sustain severe shocks without injury. This took the place of the single spine and sled runner before used. No sled runner was used with this. It was passed between the 1st and 2nd surfaces, and bore the axis of rotation on short standards at the ends of the transverse bar. Cross pieces were added to furnish points of attachment for the springs and the stop cords. ~~On trial~~ The total ~~On trial~~ weight was about 7½ pounds, the ~~total~~ wing area $\frac{5'' \times 54'' \times 9''}{144}$ = 17 sq.ft. This should carry a weight of $17 \times .76 = 13.1$ pounds, whereas it ~~carr~~ was weighted to but .44 pounds per square foot, which may be considered light-weight.

On trial the first fall broke the stop cords in the rear, and these were broken so often that it became necessary to substitute wire chains, made similar to a surveyor's chain, in place of the cotton and hemp cords before used. These did not break and were so fastened to the two strong oak uprights in front as not to tear loose. Thus arranged it was possible to obtain a semblance of a flight. But owing to the low position of the centre of gravity the springs failed to work automatically; for the drift upon the wings was so great and the resultant so far above the axis of rotation that the surfaces were blown back as soon as the flight began, ~~thus increasing~~ unduly stretching the front springs. This resulted in an increased angle of elevation and this in a further increase in the drift,- which continued until the movement was ~~st~~ checked by the stop cords in front. If the front springs were made sufficiently strong to resist the backward pull of the surfaces, it showed a marked tendency to overdo the matter and to begin with pull the surfaces down in front to such an extent that no flight at all could be obtained. The reliance had thus to be placed almost wholly upon the stop cords.

By running with the model into a light wind very satisfactory flights could be obtained by keeping the fingers pressed against the tail, the model lifting well at small angles of incidence. But as soon as the pressure from the rear was removed the velocity fell off and the model came rapidly down. This goes to show clearly that the ratio of lift to drift in this model must be a large one.

It looks now as if it would be necessary to raise the axis of rotation and pass it between the 2nd and 3rd surfaces, immediately below the 3rd surface. If the triangular frame is passed through at this point it will leave the lower surface exposed to almost certain breakage on the first hard fall the model gets. Altogether the outlook for success with this model, such as we have had with the ~~to~~ one and two surface models seems rather discouraging.

This however does not affect the probable merits of the 5 surface machine when constructed on a large scale where the operator can guide against falls and where the relative drift of the machine may be greatly lessened.

May 31 -
The five surface model has been altered so as to put the axis of rotation between the second and third surfaces, a sled like arrangement passing downward beneath the lower surface to prevent breakages. With this arrangement a good many experiments were made, but the model soon became wrenched and twisted, so that the surfaces presented varying angles of elevation and was wholly unfitted for further experiment. I have accordingly had to bring it into something like shape by means of stays, ~~the~~ placed diagonally across the second and 4^{th} surfaces, and as the model has become tilted to one side, so that the outline of the surfaces is no longer rectangular but has this form  (here much exagerated.) In this shape there is not much hope of accomplishing much. It will have to be

rebuilt.

Mr Chanute having consented to trying some experiments with light weight models, by which I mean models carrying not over .5 pounds per square foot of wing surface, I have cut down a single surface model to this weight by reducing weight of almost every part, shaving the front ~~ribs~~ spars of the wing down to a mere shell. This has reduced the weight so materially that a ~~weight~~ bar of lead, about 1/3 pound, has had to be added to bring it up to the desired .5 pound per square foot. This enables me to shift the centre of gravity to almost any desired degree, which is an additional advantage in our experiments.

With the centre of gravity placed somewhat in front of the C.P. of the wing, excellent results have followed this change to ~~a~~ a light weight. 1st The lift is very much improved, so that the model now

May

descends at the rate of 1 in 8 or 10, against a rate of 1 in 4, 5 or 6 in our previous experiments. 2nd The fore and aft stability is in no wise impaired, this being a point upon which Mr. Chanute has doubts, the model flying with much steadiness when the c.g. and the springs are properly adjusted. 3rd The lateral stability is about as good as with the heavier weight. Here a distinction is to be made. If ~~the~~ a light and a heavy model were made to descend at the same angle, the light model would likely prove the more steady of the two. But as the angle of descent becomes less the lateral stability of any model becomes more delicate, for the reason that there is less force to pull or drive it onward (For instance it would be an easy matter to make a model which would descend vertically head down and be altogether stable, ~~whose~~ because a powerful force directs it straight to the earth; whereas when sailing at a small angle ~~of eleva-~~ ~~tion~~ of descent, the force which drives it forward is small, while the force which draws it downward and so tends to turn it from its course is many times greater, and the smaller the angle of descent the greater will be the discrepancy between these two forces. ~~Hen~~ The delicacy of the equilibrium seems to manifest itself more in a lateral than in a fore and aft direction, for if the fore and aft equilibrium is not perfect we will have an undulating flight, the model alternately rising and falling, but maintaining on an average a steady descent; while we can have no lateral undulations and maintain a good flight. Hence we should expect a loss of lateral stability as the angle of descent becomes less, and if at a small angle we secure as good a degree of stability with a light model as we can secure with a heavy model descending at a larger angle, we may justly account it a considerable gain in stability. Two years ago in my experiments, when I had so much trouble with the lateral stability it was chiefly because I was endeavoring to fly at small angles of descent. I never had much difficulty with the lateral stability when the angle of descent was large. 4th The light model is safer; for we may carry the same weight with a smaller velocity and so the danger on alighting will be less; and we descend at a smaller angle and on this account the danger is also less. The ~~more~~ nearly one descends horizontally the less the danger, as it is possible to get safely on to the ground from a train moving 20 miles an hour, whereas such a velocity in a vertical direction would likely prove fatal or at least injurious.

This action of the single surface is encouraging also in that it enables us to secure longer flights and so have a better opportunity for studying in this way. the problem. I want next to try the 2 surface model

June 15-

Having reduced the weight of the 2 surface model to .5 pounds per square foot of sustaining surface, and having arranged for shifting the centre of gravity forward to a point two or more inches in front of the centre of pressure of the wings, I made a large number of experiments with it, from the top of a high hill just north of Fullnx. depot. These experiments were to my mind highly gratifying, showing as they did that a satisfactory degree of stability is not inconsistent with light weight and a descent of 1 in 8 or even greater. The stability seemed even greater than with the heavier weight of .76 pounds per square foot.

With this light weight it is possible to get a flight of 500 or 600 feet without great difficulty, and we have accordingly a chance to study the problem to far better advantage than when the flights are short and rapidly descending. Another advantage lies in the fact that the ~~wind~~ rising currents of air encountered are often sufficient to buoy the model up to a height at times equal to that from which it started, whereas the heavier model comes down through the rising current. On several occasions the model has held steadily on into a horizontal wind with little or no loss of elevation for short distances, showing the phenomenon of aspiration.

I find the best position of the c.g. to lie slightly in front of the c.p. of the wings. This is a questionable statement, as we do not know with certainty where the c.p. on the wings is located, but I assume that for the surfaces we are using and the angles of descent that it is about 40 per-cent from front to rear.

June 30.

Further experiments go to substantiate the conclusions reached in the last record; namely that excellent flights may be obtained from light models, 1 pound per 2 sq. ft. of surface. This has been tested with both single and double surfaces. The winds have been light or absent, and hence it yet remains to be seen how the models will act in a strong wind. Two years ago my models were light weight, 1 lb per 2 sq. ft. and I obtained excellent results in high winds. It seems to me it is a question of angles. If the wind is strong we can fly at a smaller angle, getting the same lift as when using a heavier model with a larger angle. The practical question remains as to whether, with light model we can main-

June

tain the requisite small angle. I believe it can be done, and that it is easier to maintain an angle of 3° or 4° than one of 8° or 10°. Especially with curved surfaces, where I have almost invariably obtained the best results when the chord of the curve cuts the line of flight at a small angle. The question of construction has also to be considered. But as I understand Mr Chanute, his objection to light weight machines is that they are ~~much~~ unsafe. On this point I can only speak from my experience with small models, which I deem safer than ~~the~~ heavier ones.

Numerous experiments with the 5 surface ~~model~~ have yielded no satisfactory results.

July

July 15–

Experiments made to determine relative merits of broad and narrow wings, seem to show that the broad wing is as stable as the narrow — notes interrupted – will be resumed at close of month –