GLIDING AND SOARING FLIGHT

J. BERNARD WEISS

W. C. Ridgeway.

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GLIDING AND SOARING FLIGHT

A SURVEY OF MAN'S ENDEAVOUR TO FLY BY NATURAL METHODS

BY

J. BERNARD WEISS

With a Preface

BY

C. G. GREY

And an Appendix

BY

W. H. SAYERS

LONDON

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TO THE MEMORY

OF

MY FATHER.

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FOREWORD

No book can be justified solely by the intent of the author. The interest aroused by the recent experiments in gliding must serve as a ground for apology if not as an excuse for endeavouring to supplement the established works on aviation. The material from which the following pages have been compiled is drawn in some measure from the earlier of these books, and from periodicals and pamphlets which have appeared in the time of the events they record. Acknowledgments are due in particular to the staff of *The Aeroplane* for the information concerning the work of the last three years in Germany. The author has also to thank the Royal Aeronautical Society for its generous assistance in providing the photographs of historical gliders

J. BERNARD WEISS.

PREFACE

It is a curious fact that although gliding experiments formed the basis from which all practical flying and a great deal of what is supposed to be aerodynamic science have been developed nobody has taken the trouble to write a history of gliding or to record what the early gliding experimenters did for the progress of aviation. Before power-driven flying became possible each individual gliding enthusiast wrote his own little history as he went along. Lilienthal produced monographs in German; Pilcher wrote of his experiences in English. Ancient numbers of the official organ of the *Aeronautical Society* are full of articles on the experiments of Octave Chanute, the Wright Bros., and others. Contemporary French publications chronicled the work of MM. Archdeacon, Ferber, the Voisins, Blériot and Farman.

Then in an evil moment (from the point of view of pure aerodynamic progress though not as it affected practical rule-of-thumb flying) the Brothers Séguin produced the Gnôme rotary engine which, though it consumed vast quantities of petrol and oil, was so unnaturally light that it made flying possible on aeroplanes which never ought to have flown at all. There is an old saying among aeroplane people to the effect that you can make a tea-tray fly if you push enough power into it. And that was practically what the Gnôme engine did.

The great Aviation Meeting at Reims in July, 1909, saw the first appearance of the Gnôme engine and with it the Farman and Voisin box-kites put up records for duration which before then had seemed impossible. From that day onwards the aerodynamic design of aeroplanes languished and attention was concentrated on purely mechanical design in order that as engine-power increased aeroplanes might be built strongly enough to stand the ever-increasing strains put upon them. The result has been that for thirteen or fourteen years we have simply been building ever-improving box-kites and have almost lost sight of the very idea of perfecting aeroplanes.

There is a popular idea that the advent of the War 1914-18 made flying possible and put life into what otherwise would have been almost a still-born science, industry, sport, or whatever you care to call flying in general. As a matter of fact the War has seriously hindered aeronautical progress and there are many who look to gliding as the best way towards making up for the progress lost during the War.

It is true that in many ways the War solved problems of which the solutions help aeronautical progress. For example we now know just how much the average human being can stand in the way of being thrown about in the air without bursting something inside. Before the war a few professional aviators looped the loop and performed mild acrobatic feats in the air. To-day every pilot does as a matter of course far more than they imagined to be possible, and any village "joy-rider" can for ten shillings a "flip"—as the pilots call it—do all that those pre-war heroes did.

Also we have learned much about the strengths of materials. Money being of no value during the war it was possible to carry out mechanical and chemical experiments purely for aircraft purposes which aviation on a pre-war basis could never have afforded. And in consequence of these experiments we have produced new and improved materials so that we are able to build lighter and stronger aeroplanes than we could possibly have built by now if there had been no war. But against this it is well to note that these same lighter and stronger materials make it possible for very bad aeroplanes and very bad engines to put up what look like very good performances, whereas if we had only had the old pre-war materials we might have achieved a much higher degree of efficiency in our dealings with the air itself and with our engine fuels instead of achieving our results as we do largely by sheer brute force.

Furthermore we have learned a certain, or perhaps one should rather say an uncertain, amount about the science of aerodynamics, which is or attempts to be the science which explains what the air does and why when force is applied to it by some body or when it applies force to something else. Money being cheap during the war, as aforesaid, aeronautical experimenters in all the belligerent nations were able to get hold of almost unlimited sums with which to build research laboratories, testing apparatus, wind-tunnels, instruments and so forth. And money was forthcoming in quantities for the pay of bright-brained mathematicians, aerodynamic theorists, and assorted experts whose objects were to explain why aeroplanes fly and to discover how they could be made to fly better.

Between them they did actually discover many interesting facts but they also evolved a number of theories which so far from helping progress rather retarded it. For example they discovered the best possible wing-forms, and the best possible body forms, and the best possible shapes for landing-carriages, and struts and wires and levers, and all sorts of things individually, and then thought that by building an aeroplane of all the best possible bits and pieces, they would produce the best possible aeroplane. Which has now been proved to be utterly and hopelessly wrong as a theory. And this is precisely where the glider begins to come into its own again.

During the war, again thanks to money being no object, astounding speeds heights distances and durations were attained with aeroplanes by simply cutting off weight and cramming on power. After the war there was little money available for aviation and so economy became of prime importance. And so we of the so-called conquering nations have made little progress towards producing aeroplanes which can pay their way as commercial propositions.

Now it so happens that under the terms of the Treaty of Versailles a body was formed, known as the *Commission Aéronautique Inter-Alliée,* to control German aviation, to see that all German aeroplanes-of-war were destroyed and to see that no aeroplanes of any practical use were built in Germany. So the Germans being forbidden to build aeroplanes above a certain absurd maximum power, but being determined to progress in aeronautical knowledge, returned to the elemental stage and began again experimenting with gliders. By combining the practical knowledge won in the air during the war with that gained by their painstaking research students, they produced by 1920 several aeroplanes without engines which achieved remarkable results. Still better results were achieved in 1921, and so much were aviators in other countries impressed by them that the French organised a gliding and soaring competition for 1922.

This French competition, held near Clermont-Ferrand, was merely funny so far as results were concerned. A week or two afterwards the Germans held their 1922 competition and their best glider remained in the air for over three hours. This performance attracted world-wide attention in the press, and the *Daily Mail* always quick to see the trend of public interest promptly and generously offered a prize of £1,000 for a competition in England under the control of the Royal Aero Club. The knowing ones prophesied failure on the strength chiefly of the widely advertised French opinion that the astonishing German results were due to the specially favourable conditions in which their gliding was done, because—it was said—their gliding place was at the meeting point of two mountain ranges at a height of 5,000 feet, so that they had 20 miles width of air being forced up to that height on which to float their gliders. The fallacy of this beautiful scientific theory was exposed when in October, 1922, the *Daily Mail's* £1,000 was won, and the German soaring record was handsomely beaten by M. Maneyrol at Firle Beacon, near Newhaven, gliding off a hill only 200 or 300 feet above the country to windward of it, which country itself was lower than the country behind it, so that the wind actually came down-hill to the foot of the Beacon.

Since then interest in gliding has increased enormously. The most serious aeroplane designers of all countries are now convinced that carefully observed experiments with gliders will enable us to discover facts about the aerodynamic design of aeroplanes and about the action of air on the lifting surfaces and control surfaces of aeroplanes which cannot be discovered by the laboratory and wind-tunnel methods hitherto in use and have never been suspected by the high-class scientists and mathematicians. We shall now be able to experiment with the aeroplane as a whole and with the aeroplane alone, without the observations being complicated by the actions of the engine and air-screw, or by "scale corrections" as in laboratory work.

Thus we shall work towards increased efficiency and stability and controllability and so towards cheapness and safety in flying.

Also gliding will be of high value to the Royal Air Force in teaching flying. Instead of a pupil having to spend weeks with an instructor in an aeroplane at great cost of labour and fuel before being allowed to fly alone, he can now be taken into the air a few times with an instructor just to teach him the feel of things, and then he can be turned loose on a glider under such conditions that he cannot well hurt himself. If he breaks the glider it costs very little to repair or replace. In a very short time he will show whether he has the right hands, eyes and judgment to make a pilot. And under such circumstances he will gain confidence in his own ability whereas under the critical eye of an instructor or with the responsibility of an expensive aeroplane on his mind he might well take much longer to "find himself."

With so much obvious usefulness before it the glider is evidently a machine about which more should be known than is commonly known to-day. Hence this book. And none is better qualified to write such a book than is Mr. Bernard Weiss. He is the son of the late Mr. José Weiss, who—such is our English custom—is much better known in the United States as a great painter of English landscapes than he is in this country in his more important manifestation as a great pioneer of aviation.

In 1909, when I first had the honour of meeting him José Weiss knew more about the aerodynamic design of an aeroplane than do any but a minute number of our leading aeroplane designers and aeronautical scientists to-day. I say this deliberately and not as a figure of speech, for our brightest brains to-day are constantly announcing with the air of a Christopher Columbus discoveries which to José Weiss were mere commonplace facts. His son Bernard, then a small boy, worked with his father and Mr. Keith, and watched all their experiments with acute and intelligent interest. As soon as he reached military age he joined the Royal Flying Corps and learned to fly, so that he has knowledge of power-flying as well as of the theories and experiments of the early glider experiments. He is now a barrister and the results of his legal training will be observed by readers of the book who will appreciate the clear, concise and well-reasoned method by which the book has been compiled and the style in which it is written.

Captain W. H. Sayers who has done a technical appendix is also a pioneer of aviation. He began experimenting with gliders and models in 1909, or thereabouts, and gave up a good job as an electrical engineer to work as a mechanic at Brooklands in the earliest days of that first of British aerodromes. During the war he served in the Royal Naval Air Service and the Royal Air Force as a technical officer, particularly in charge of design work, and he himself designed and superintended the production of a number of interesting and successful seaplanes and land-machines. Having worked with him off-and-on for some twelve years I can vouch for the reliability of his views on technical matters whether touching practical design or pure theory.

As already suggested, there is much to be learned from gliders and there is much good work to be done with gliders. As an instrument of research, as an implement for training, and possibly as a machine for sport, the glider has a very important future. It is hoped that this account of its past and present will help that future.

C. G. Grey.

Editor of *The Aeroplane* and *All the World's Aircraft.*

INTRODUCTION

In treating of matters connected with gliding, it is often impossible to avoid the discussion of much that is ordinarily dealt with in the many disquisitions on mechanical flight. Nor is it even desirable to define the line that would separate questions which might fall within the realm of the one or the other. The early history of flying being practically entirely a history of gliding it is inevitable that the initial development of both kinds of flight should come under review in any attempt to record the steps which have led to the relations that they bear to each other to-day.

The underlying principles of gliding flight are in common with those of mechanical flight, while the latter, as the outcome of the former, has additional principles peculiar to itself. From laborious investigations into the broader problem, approached only by gliding, was derived the basis of our knowledge of the principles of aeroplanes, which had demonstrated their capacity to sustain a man in the air long before mechanical power was applied to propel them.

To-day's experiments in gliding and soaring are a direct extension of a long chain of a similar type of experiments conducted on a more primitive scale by those in the background not only of the so-called successful aeroplane, but also of a particular school which has ceaselessly urged the promotion of this movement. By following, as nearly as practicable, the chronological order of man's attack on this basic aspect of flight, we may learn to appreciate its great significance in the struggle to conquer the air. Incidentally we may hope to dispel the popular notion that the birth of flying must be somewhere centred in the achievements of the Wrights in America and of Santos Dumont in France.

Our task will be, in the first place, to trace the growth of the practical knowledge concerning the phenomena of flight before it was hurriedly applied to the evolution of the power-driven aeroplane. In recording this progress it will be necessary to outline the work of each pioneer. Sequence is often difficult to find in the early research of an incipient science. Aviation was unusually slow in becoming popularised by a heedless press, while literature on the subject was seldom available. It is not surprising that every thinker who approached the question might well feel alone in a sphere which another was already exploring.

Experience shows that the casual observer is quick to applaud the spectacular attainments of those representing the fortunate instruments which have yielded success. But experience shows that he is correspondingly slow to appreciate the value of the work but for which these earlier heroes of flying would never have even been heard of. A serious endeavour must here be made to notice especially those who may be rightly regarded as the founders and promoters of any particular branch of research that falls within the scope of our interest.

Recognition is due to those rare and adventurous spirits who, in spite of every discouragement and in the face of the most shameful ridicule, persevered in their efforts to discover the road for others to follow. Their only support was their faith in the solution of the problems involved. Scientific conviction and redoubtable courage in laborious endeavour without personal profit are the qualities which enabled the real pioneers to lay the foundations of the science of flight.

Now a salient feature of this uphill fight was the vision that inspired those patient experiments. It is often suggested that the present day power-driven aeroplane must surely have been beyond the range of the dreams of the early inventors. It is safe to conjecture that the recent achievements in gliding would be much more likely to cause exultation. The true pioneers were mostly united in the firm belief that bird-like efficiency might one day be reached. Their gliding experiments which formed the basis of practical flying were conducted on machines originally modelled on the principles of nature. It was always thought that once they had mastered the problems of equilibrium and control, it would lie in their power to construct a machine requiring a very slight force to maintain it in horizontal flight.

But the vision of the low-powered efficient machine vanished with the advent of the partially developed internal combustion engine. The natural desire for immediate results of a sensational order put an end to the development of body and wing design on truly progressive lines. From that time onwards gliding fell into neglect. A certain number of scattered enthusiasts continued to believe that gliding experiments might serve as a path to useful knowledge, but their voice was unheeded, and gliding was held to serve no further purpose in the general advancement of the science.

Many are asking why it should be necessary in view of the accomplishments of an up-to-date aeroplane, to go back to walking on a seemingly primitive glider, after a decade of years of running under power. History has always its lessons, and the history of flight is no exception to the rule. The needs of the hour are continually proving that the apparent triumph—which has been reached by the gradual perfecting of constructional methods and the means of applying mechanical power—is not in reality the practical solution of the ancient problem.

It must not be imagined that an attempt is being made to supplant the power-driven aeroplane by a magical type of engineless aircraft; an engine is as necessary to an aeroplane as are muscles to a bird. But it is believed that careful introspection may reveal a way of harnessing to an appreciable extent the power contained in the air. At the least it will serve as a valuable insight into the laws of natural flight. The existing aeroplane cannot be said to act in accordance with those laws. Indeed it is not so designed. Yet if those laws were more clearly understood by designers and technicians, vast improvements in design would ensue. Man has not yet been able to make use of the air on the principles employed by a bird. If he wishes to do by mechanical methods that which a bird does naturally, he must learn the principles of progression in a medium with which he is still unfamiliar. And the measure of success already attained by the aeroplane is no criterion of our knowledge as to how to comply with those principles.

The circumstances bringing about the present activity are mainly attributable to the pressing demands of commercial aviation. War is conducted regardless of cost, and the factor of expense is hardly considered in designing machines that are out to win. But the commercial machine must not be infected with the vice of inordinate expense. The cost of engines and the price of petrol stand at present as a bar to success. Power must be saved and consumption reduced by producing a machine of higher efficiency. The more marked the improvement, the greater the load that can be carried for a given gradation of power. Experiments in gliding are purposed to yield the particular knowledge that will lead to improvement; for precisely the qualities which make for perfection in gliders, will make for the product of an economical, load-carrying, power-driven aeroplane. By no other method can aviation be rendered commercially possible. Experiments of this or of similar nature would certainly have been requisite sooner or later.

But there is another and more immediate cause of the revival of gliding. The "Commission Aéronautique Inter-Alliée"—a creation of Versailles—had imposed upon Germany a general prohibition of the construction of aeroplanes. This short-sighted attempt by the conquerors to extinguish aviation in the land of the vanquished, had the natural effect of directing aeronautical research into unexplored channels. An incentive was given to the most competent German technicians to apply their Teutonic ingenuity to evolving machines that would not come under the vindictive prohibition. The good which resulted is certainly greater than the harm which the edict was calculated to inflict.

It would be well to envisage the general directions in which gliding experiments may really prove useful. They are of the highest importance to the practical study of aerodynamics. The true effects of the passage of a body through the air can only be gauged when the disturbing forces set up by an engine and airscrew are not introduced. This applies not only to wing forms, but equally to fuselage shapes, and still more to controls. The wind tunnel method, hitherto employed as the means of carrying out tests on scale models only, is not always reliable in determining the performance of a full sized machine. The glider machine in free gliding flight provides a superior method, and yields more accurate results. The successful glider, by its nature a highly developed and refined machine has the additional merit of supplying the means of conducting, for a small capital outlay, elaborate full-scale experiments, which would otherwise entail the costly manufacture of imperfect, engined aircraft.

The aim, then, of gliding is in the first place to advance education in respect of aeronautical construction—in particular by showing the actual behaviour of wings of different sections with different loadings; and generally in the direction of higher efficiency. A further meritorious aspect of *soaring* flight—more fully discussed in a following chapter—is the advancement of knowledge which is bound to result from the study required of meteorological conditions. The knowledge so gained directly contributes to the general development of aviation, as indeed it does to our wider acquaintance with natural phenomena.

As a sport, the merits of gliding have always been amply debated. Its foremost exponents, from even its earliest days, have always "enthused" on this pleasing phase of its technique. It was Lilienthal's cherished belief that if the youth of his country could be urged to practise the art the sporting incentive would alone be sufficient to hasten the progress of flying. And there are many to-day who endorse his belief. The requirements of accurate judgment and precision of movement are certainly elements of sport. In the flying of a high-powered fighting scout the factor that counts is the performance of the machine, whereas in gliding everything is subordinate to the skill of the pilot. But whatever the modicum of pleasure may be, let it not be imagined that true soaring flight can be ever accomplished without the judicious exercise of a high degree of intelligence and skill.

There is also much said about the possible outcome of a small sporting type of auxiliary engined glider, which has already been happily styled "aviette."

It is not disputed that the highly efficient and perfectly controllable glider should properly lead to the eventual production of an economical, low-powered, single, or two-seater, all-purpose aeroplane. But whilst considering the problem of low-powered flight, it is well to remember that we have yet much to learn in the preparatory school of gliding on *gliders*. Having returned to the path of wisdom, let us be warned not to lose it again.

If those adventurous spirits of earlier times had not been deterred from pursuing what is after all proving the road to success, we may safely assume that present-day flying would have long since provided a real "aviette."

THE purpose of this brief historical survey is merely to mark the material steps in the progress of the science of gliding flight. It is therefore expedient to withhold our attention from the follies and failures of those legendary abstractions whose aerial exploits do not constitute a genuine advance in the evolutionary history of the science. There is nothing to gain by confusing the mind with superfluous stories that have no place in the sequence of prominent events.

The famous manuscript of Leonardo da Vinci gives lucid expression to some very rational notions concerning the flight of birds. But it is perhaps of greater value as showing another aspect of the versatility of this extraordinary genius than as a real contribution to the problem's solution.

The picturesque story of Jean Baptiste Dante, a professor of da Vinci's time, and of his daring operations before the spell-bound Perugians is diverting more than instructive. The accounts that we have may be coloured or not; but the fact that a scientist in an age of science should have left no record of the discoveries that he claimed to have made, raises a suspicion that properly justifies our passing him by.

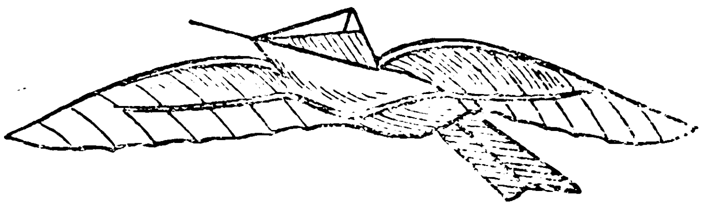
To Sir George Cayley we can fairly ascribe our source of knowledge in the field of aerodynamic science. Here was the first serious attempt to explain mathematically the fundamental principles of flight, his figures being in general accord with those that have since been established. Following his investigations, Cayley constructed a glider having 300 square feet of surface and embodying many of the present devices for maintaining stability and control. He recounts his experiences in Nicholson's Journal in 1809: "When any person ran forward in it with his full speed, taking advantage of a gentle breeze in front, it would bear him up so strongly, as scarcely to allow him to touch the ground, and would frequently lift him up and carry him several yards together."

It is quite probable that Cayley possessed adequate knowledge both theoretical and practical to enable him to build a man-carrying glider, but he does not appear to have realised the necessity of adding to his knowledge by further experiments in this direction. He was carried astray by the mistaken belief that a suitable engine was the *sine qua non* of future progress—a lamentable error of persistent recurrence which so much retarded the advent of flight.

Cayley's work was of real importance. He formulated the laws of equilibrium, and gave such evidence as to their value as eventually led to their practical development. His researches, published in 1810, anticipated in almost every material particular the aeroplane of a century later, and had they not been subsequently neglected, aviation might well have been made a reality very much earlier than it actually was. Yet the progressive aspect of flying as a science was greatly advanced by Cayley's research. The serious nature of his work invested it with the scientific dignity which it had so long lacked, and which was so essential to secure its promotion as a branch of knowledge.

During the thirty years that followed there is no authentic record of another earnest endeavour to further the knowledge that Cayley had gained. And one may even conjecture that the later attempts that were made in France bore no relation to Cayley's lead.

In 1854, Captain Le Bris, a French sailor, who was infatuated with the flight of the albatross, constructed an "artificial bird" of albatross pattern. Arched wings measuring fifty feet in span were articulated to a boat-like body. This craft had a total sail area of 215 square feet and weighed 92 lbs. Placing his long-winged creature on the top of a cart, Le Bris stood erect in the body, and headed against a ten-mile wind. The bird was secured to



LE BRIS' ALBATROSS.

the cart by a rope which the Captain could quickly release. By an arrangement of levers he could vary the inclination of the wings and tail. When the pace was sufficient, he raised the front edges of the wings, at the same time slipping the mooring rope.

On the first attempt this rope whipped accidentally round the waist of the carter—and the story proceeds to relate how the bird soared up to a height of some

300 feet with the sailor triumphantly standing above and the terrified peasant dangling below. Other experiments followed of a more or less similar nature, until on one hapless occasion the creature pitched forward and plunged to the earth, where it lay shattered and torn in a hopeless tangle. Fortunately at the time of the disaster the machine was only loaded with ballast. When flown by Le Bris, it had shown itself capable of a certain degree of control, and had once been successfully landed after a glide of 200 yards from a height of 150 feet.

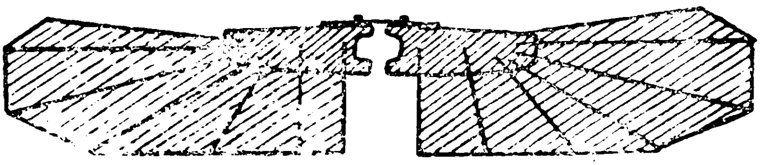
Lack of data concerning the design of this glider and the mechanism employed for its navigation, deprived it of any instructive merits. But the experiments were certainly remarkable for the time, and if adequately reported might quite well have proved of considerable value to the existing state of aeronautical science.

Le Bris was followed a few years later by a compatriot of even greater zeal. The name of Louis Pierre Mouillard was known to only a few of the deeper students of aeronautics until some time after his death, when a collection of his papers on the flight of birds was found in the cellars of the

French Consulate at Cairo. Mouillard was a farmer and poet who had spent much of his life in Algeria and Egypt. From boyhood he mused on the birds with the keenest fascination, and his mind was ever excited by the spectacle of the soaring vulture whose majestic flight he was planning to imitate.

For thirty years he continued these observations with unabated interest. He formulated striking conclusions about weight and wing-spread, agglomeration of mass, resistance and velocity, and the functions of tail and quill feathers. In 1881 he published his *L'Empire de l'Air*, a truly remarkable piece of areonautical literature. The book is devoted almost entirely to his observations relating to birds, and is replete with theories of the highest interest to ornithology and aviation alike.

Mouillard was led by his ardour to the construction of a glider on which he might hope to learn the art of the vulture. It was a tailless monoplane made of curved agave sticks screwed to boards, and covered with muslin. The wings were hinged together, and actuated by rods from the operator's feet in such a way as would vary their relative angle. The operator himself was harnessed in a space in the middle of the hinge. But the whole design was much too crude to yield anything more than a series of hopping glides. A wind gust came and whelmed him over. In his alarm he allowed the wings to fold up like those of a butterfly at rest. He was unpleasantly pinched, and decided to abandon further attempts, being insufficiently skilled in the art of construction to provide the means for adequate control.



MOUILLARD'S WINGS.

As a scientific student of the laws and principles of aerodynamics Mouillard can hardly be classed either with Cayley or with his successors. But as a missionary he was pre-eminent and second to none. The fervour of his book was one of the inspiring causes of the later successful experiments. Mouillard himself was almost fanatical in his enthusiasm. Wilbur Wright described him as a prophet crying out in the wilderness, exhorting the world to repent of its unbelief in the possibility of human flight.

INFLUENTIAL as Mouillard's inductions eventually proved, the problems of flight remained to the world a mystery unsolved. Such scraps of data as at that time existed—obtained from results of desultory efforts to investigate one or another of the aspects of the question—were scattered, disconnected and inconsequent. The evolution of the present day glider, as well as that of the power-driven aeroplane, must date from the time when Lilienthal abstracted some order from an existing chaos and reduced the mystery to a dynamical proposition. He is rightly regarded as the apostle of gliding flight.

Otto Lilienthal, an engineer of Berlin, had devoted practically all his life to a study of the flight of birds. He was joined in this pursuit by his brother Gustave, and in their earlier days they spent much time together constructing models and accumulating knowledge. His practical experiments, which commenced about 1890, were conspicuous among all other attempts by reason of the difference of method adopted. While himself the author of a valuable treatise on the principles of natural flight,*he insisted that to theorise alone was of little avail to the state of affairs that then obtained.

Lilienthal was convinced that it was essential to begin with the lessons of gliding, and that the whole success of aerial navigation depended primarily on the maintenance of equilibrium by the pilot. Man was to be the flyer, the apparatus an adjunct. A lifetime of thought had brought him to the conclusion that a few minutes' actual experience in the air would do very much more to advance aviation than years of labour with unproved formulae. And the credit that fell to him arose from the fact that he was the first to put his belief into practice. In an article entitled "Why is it so Difficult to Learn to Fly ?" which appeared in a Berlin journal shortly before his death, he says that :—

" . . . The method which is to lead to practical flight must be capable of development, be its beginnings ever so primitive, and by it we must

* "Bird Flight as the Basis of Aviation."

be afforded an opportunity of really skimming through the air, by which we may gain experience as to the stability of flight, the action of the wind, and safe landing, in order by continual development gradually to approach permanent free flight. Perfection cannot be forced immediately. It is because inventors require too much from their constructions that the positive results are so little . . . Whoever loses sight of healthy development by continually increased experience, will never attain anything in this sphere . . ."

With this aim in view, Lilienthal resolved to construct a simple apparatus on which he might gain some sort of practical experience in the air without the use of complicated and expensive appliances. The glider with which he commenced his first series of trials was made of willow wood covered with waxed sheeting. It weighed about 40 lbs. and spread 107 square feet of surface. The apparatus was held in flying position by passing the forearms through padded tubes, the hands gripping a cross-bar in front. This was the only way in which he was attached to the machine, so that he could, when standing on the ground

PLATE I.

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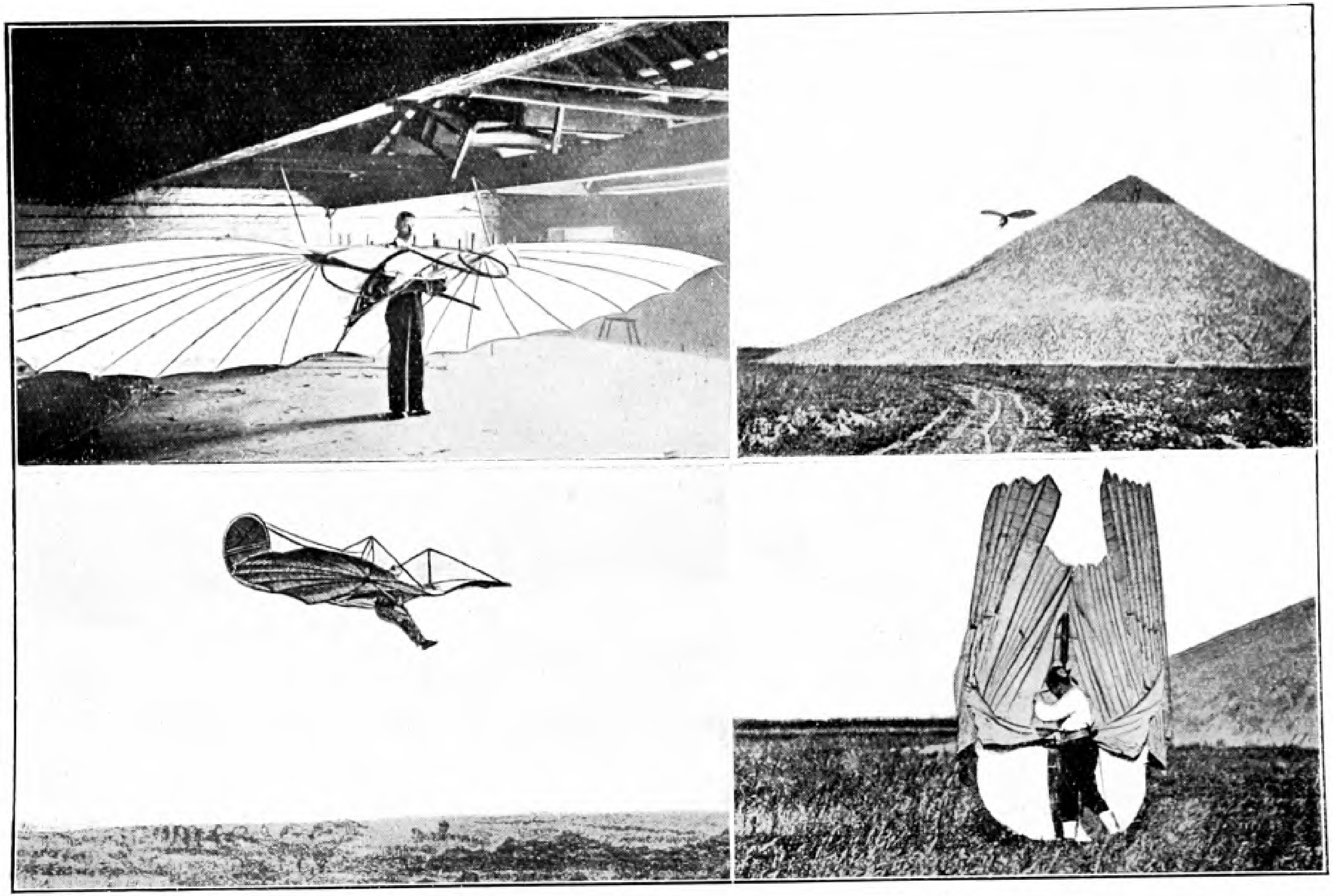

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(Courtesy Royal Aeronautical Society.)

1. LILIENTHAL'S MONOPLANE GLIDER.
2. GLIDING FROM HIS CONICAL HILL.
3. IN A CRITICAL POSITION.
4. THE GLIDER FOLDED AFTER A GLIDE.

place the machine at any angle to the wind, and when supported by the air he could move his body a considerable distance forwards or backwards or sideways, so as to preserve his balance and guide the machine.

At first he used to practise by jumping from a springboard. But he soon found it better to launch himself into the air by running downhill against the wind, and giving a final leap from the ground when he felt a sufficient supporting effect of the wind under the wings. In 1892 a canal was being cut in the suburbs of Berlin. With the surplus earth a conical hill was thrown up in the midst of country that was perfectly flat in every direction. Hundreds of steady glides were accomplished from the slopes of this artificial hill. A good glide would carry him over 100 yards from the centre of the hill at an angle of descent of 1 in 7, or even less. The machine on which these glides were performed had a span of 23 feet, with a total area of 150 square feet, and weighed about 44 lbs.

By using gravity as his motive power Lilienthal was the first to establish the simple and only effectual way of approaching practical flight. Until that time no one had realised that satisfactory gliding was the first requirement on the road to successful flying.

Lilienthal's method of maintaining his balance in the air was merely by moving his weight about in the machine; he supported his weight entirely on his elbows or shoulders. Once having thrown his weight very far back, he was unable to pull himself up again: "I was thrown about just like a sheet of paper when it is caught by the wind. At first I saw only blue sky, and then I saw only green grass, and I thought now it is all over with me." A sprained left hand was the only harmful result of the accident.

The last glides of Lilienthal were made on a biplane. The upper and lower planes were connected in a manner that proved defective, and it was largely owing to the faulty construction of this new double-decked glider that he lost his life. He explains his reasons for introducing the biplane in an article that he wrote for the *Aeronautical Annual* in 1896:—

"My experiments in sailing flight have accustomed me to bring about the steering by simply changing the centre of gravity. The smaller the surface extension of the apparatus is the better control I have over it, and yet if I employ smaller bearing surfaces in stronger winds the results are not more favourable. The idea therefore occurred to me to apply two smaller surfaces, one above the other, which both have a lifting effect when sailing through the air. Thus the same result must follow which would be gained by a single surface of twice the bearing capacity, but on account of its small dimensions this apparatus obeys much better the changes of the centre of gravity.

"Before I proceeded to construct these double sailing machines, I made small models in paper after that system, in order to study the free movements in the air of such flying bodies, and then to construct my apparatus on a large scale, depending on the results thus obtained. The very first experiments with these small models surprised me greatly on account of the stability of their flight. It appears as if the arrangement of having one surface over the other had materially increased the safety and uniformity of the flight . . .

" . . . Relying on this experience, I constructed first a double apparatus, in which each surface contains about 97 square feet. I thus produced a comparatively large bearing surface of about 194 square feet with only about 18 feet span.

"The flights undertaken with such double sailing surfaces are distinguished by their great height . . . I often reach positions in the air which are much higher than my starting-point. At the climax of such a line of flight I sometimes come to a standstill, so that I am enabled while floating to speak with the gentlemen who wish to photograph me regarding the best position for the photographing. At such times I feel plainly that I would remain floating if I leaned a little towards one side, described a circle and proceeded with the wind. The wind itself tends to bring this motion about, for my chief occupation in the air consists in preventing a turn either to the right or left, and I know that the hill from which I started lies behind and underneath me, and that I might come into rough contact with it if I attempted circling."

Before much progress had been made on this later type of machine, Lilienthal met his death, apparently through deserting his old methods of maintaining his balance. He had become convinced of the necessity of adding a horizontal rudder to the machine; this he first tested on one of the old monoplane gliders, the line operating the rudde being attached to his head. During a glide on the biplane on August 10th, 1896, in trying to balance the machine by this means he became confused; he was unable to maintain his equilibrium, and the glider fell to the ground from a height of 25 feet. Lilienthal died a few hours later from injuries received.

The work of Lilienthal gave the first powerful impulse to aviation. The success of his gliding experiments inspired others to continue his work, while his writings were the first to supply reliable data concerning the form of wings. He established the superiority of the cambered plane, about which little was definitely known before. The importance he attached to correct and careful wing design is evident from his dictum that for "soaring" there are three essentials: a correct shape of wing, a right position of wing, and a suitable wind.

Had it not been for the rapid development of the petrol engine, we should have been compelled to exercise a more rigid economy of motive power and might never have left the lines of research so frequently urged by Lilienthal.

* * * *

Percy Pilcher, also an engineer, and a lecturer at the University of Glasgow, was a contemporary and a follower of Lilienthal. Fascinated by the problems of flight, he had built a machine of his own design in 1895. This was constructed to some extent on Lilienthal lines, but the information derived from the inadequate and inaccurate reports of the Press could not have been of much material assistance to him. The chief feature of this machine, which was called "The Bat," was the very pronounced dihedral angle formed by the wings, the tips being raised four feet above the body.

In the same year Pilcher visited Lilienthal in Germany and made several glides on the latter's large biplane glider.

His own experiments on the banks of the Clyde were conducted in a manner similar to those of Lilienthal. On one of his first glides he was picked up by an ascending head wind and rose to a height of 12 feet, remaining in the air some 20 seconds.

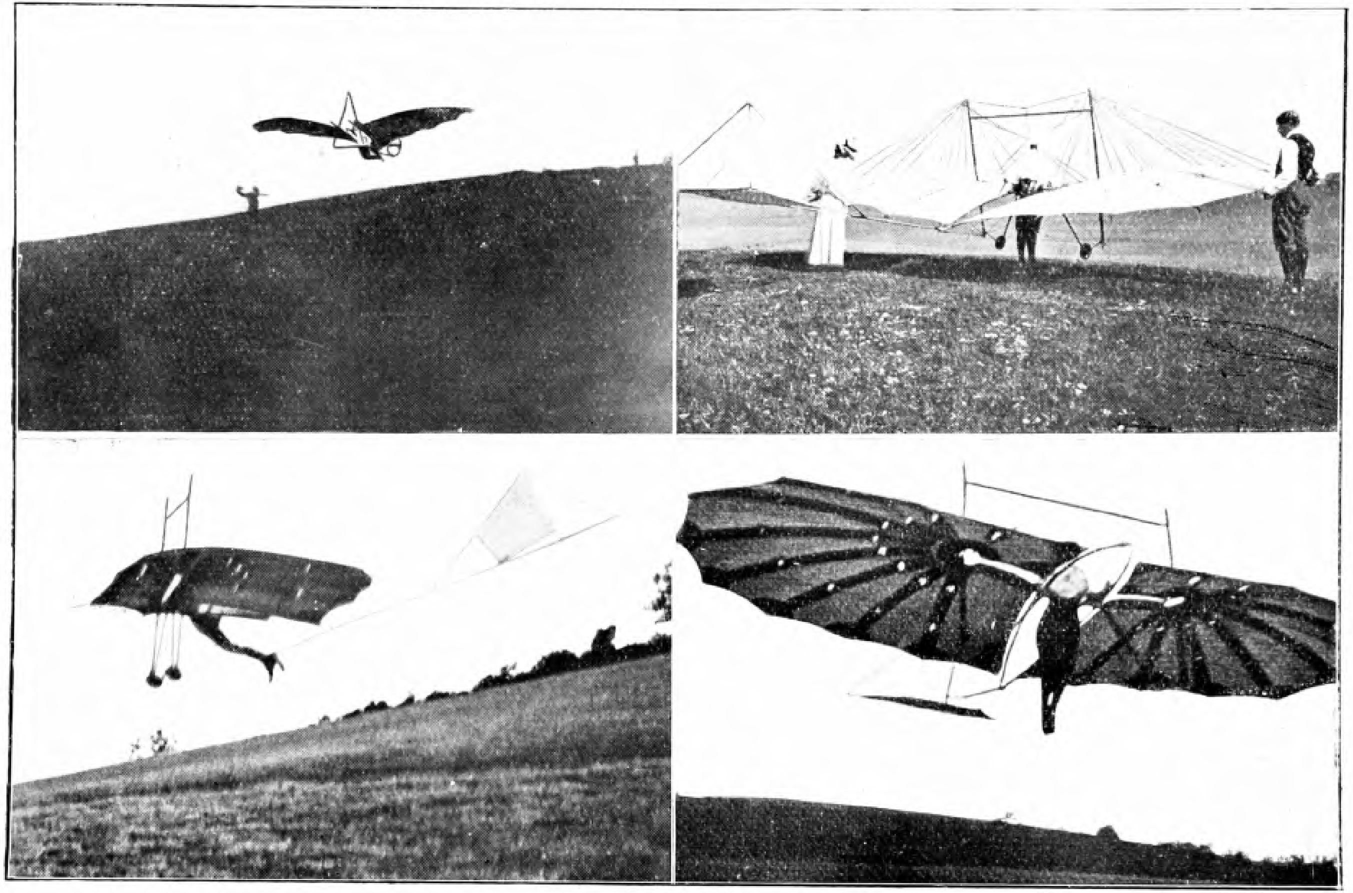
At Cardross he built two more machines. One was never quite finished, as the framework which was constructed to carry an engine proved to be too heavy for gliding. Like all Pilcher's gliders, it was a monoplane. It had square-cut wings which formed an almost continuous plane rigidly fixed to a central body. The other glider, which had an area of 300 square feet and weighed only 55 lbs., was found to be uncontrollable except in the calmest weather.

The "Hawk" was the fourth, the last, and the most successful of Pilcher's gliders. This was built in 1896 at Eynsford in Kent, and proved a great advance on its three predecessors. From the point of view of construction this was in many respects a thoroughly sound machine, when taking into account the serious difficultes arising from such requirements as collapsibility. To detail the construction of every crude apparatus recorded to have flown would entail unprofitable prolixity. But the achievements of the "Hawk" and the singularity of its design are such as to deserve examination.

The wings were attached to two vertical masts, 7 feet high and 8 feet apart, joined at their summit and their centres by two wooden beams. Each wing had nine ribs of pinewood radiating from its mast, which was situated at a distance of 2 feet 6 inches from the forward edge of the wing. Each rib was rigidly stayed to the top of the mast by three tie-wires, and by a similar number to the bottom of the mast. By this means the curve of the wing was maintained uniformly. The tail was formed by a triangular horizontal surface, to which was affixed a triangular vertical surface; and was carried from the body on a high bamboo member.

The body consisted of a narrow aperture formed between the two wings by two bamboo rods bent into a "fair" shape. The operator took up his position by passing his head and shoulders through the body aperture, and resting his forearms on the longitudinal body members. Underneath the body were attached two members fitted with wheels suspended on steel springs. When on the ground the weight of the glider rested on this elementary chassis, which also took the first impact of landing. The total area of the planes was 180 square feet, while the weight was about the same as that with 300 square feet of surface.

PLATE II.



1 2 3 4

(Courtesy Royal Aeronautical Society.)

1. PILCHER'S "BAT" IN FLIGHT.
2. THE "HAWK": REAR VIEW.

3. AND 4. THE "HAWK" IN FLIGHT.

The launching of this glider was effected by a tow-line which was passed over a pulley situated on the top of a hill. Pilcher took up his position on the crest of a neighbouring hill, and thus succeeded in making several long glides. The best of these extended over 250 yards, when he crossed the intervening valley at a considerable height and alighted on the opposite hill.

In common with the practice of Lilienthal, the method of balancing and steering—apart from the slight degree of automatic stability afforded by the tail and the dihedral angle of the wings—was that of altering the position of the operator's body; and, although this method appears to modern eyes crude in the extreme, it must be admitted that Pilcher acquired such dexterity in handling his machine by this means, that he never met with a serious accident until the day of his death.

Pilcher was considering the possibilities of applying mechanical power to his gliders when his experiments were cut short by a fatal accident in 1899. In the course of a demonstration flight the tail was seen to collapse and the machine immediately plunged to the ground. There had been a heavy fall of rain and it was surmised that the shrinking of the canvas caused the breakage of the guy wires of the tail.

In the services which Pilcher rendered to the advancement of flying, he had the distinct advantage of treading in the footsteps of Lilienthal, yet by his own work he drew attention to several matters of practical importance. He pointed out the error of attempting to control the machine with the centre of gravity placed unduly low. His later experiments introduced the system of towing the glider as a kite, and demonstrated the advantage of a wheeled chassis as a means of relieving the pilot of the weight of his machine when landing. To Pilcher lies the honour of being the first English airman to give his life to the cause, and recognition is due to the success he achieved in instilling into his countrymen a portion of the enthusiasm and energy that he himself so signally possessed.

It is impossible to overrate the significance of the experiments of Lilienthal and Pilcher in the development of gliding flight. Prior to the days of Lilienthal investigators of the problem had either confined themselves to the evolution of theories on paper, or to contemplating the construction of elaborate mechanically propelled machines. Only one link separated the experiments of these two pioneers from the more tangible successes of the Wright Brothers ; this link is supplied by Octave Chanute, whose work must now claim our attention.

It was not until late in his life that Octave Chanute gave his active attention to gliding. Before he proceeded to practical experiments he already possessed a knowledge of the principles of dynamic flight, and was closely acquainted with the gliding experiments that were being conducted by Lilienthal in Germany. Chanute was an experienced engineer and had been prominently associated with American railroads before he seriously interested himself in the study of natural flight.

The valuable series of experiments which he undertook were no doubt inspired in the first place by Mouillard's *L'Empire de l'Air.* Chanute's own book, *Progress in Flying Machines,* published in 1893, contains the fullest and most appreciative account of Mouillard that is to be found in the literature of flying.

The first machine that Chanute built was a Lilienthal glider. He secured the services of A. M.

Herring, who had been a pupil of Lilienthal, and had had some experience in the construction and manipulation of these machines. In 1896 he made his first gliding experiments among the sand-hills on the shore of Lake Michigan; but the Lilienthal glider was soon discarded, its design being condemned from the outset as unsound and dangerous. Chanute insisted that the maintenance of equilibrium under all circumstances was at that time the foremost need of flight, and that until automatic stability was acquired it would be premature and unsafe to attempt to fly under power. He wished to improve upon Lilienthal's primitive method of balancing and to acquire a pilot's science on more methodical lines. This was Chanute's great step in advance of his predecessor.

Instead of restoring equilibrium by moving the centre of gravity, he introduced the superior system of correcting loss of balance by making his supporting planes movable and thus restoring the centre of pressure to a condition promoting stability. Several types of gliders were tried, comprising in each case superposed surfaces varying in number from two to twelve. The type finally approved after many empirical modifications in the disposition of the surfaces assumed the form of the biplane with tail, which resolved itself into the prototype of subsequent biplanes in Europe as well as in America.

The Chanute glider weighed only 23 lbs; it spread 135 square feet and readily carried 180 lbs. This craft was found easy to handle on launching, in gliding and on landing; some 700 short glides were performed without a single mishap.

Chanute's gliding experiments were always carried out with a strict scientific purpose, and the data deduced and experience gained proved of the first importance. He had made a distinct advance in the right direction, and it is interesting to glance in retrospect on his predictions of a few years later. He believed "that man would one day 'soar' with a machine weighing about one pound per square foot, perfectly stable, and capable of gliding under gravity at angles of 1 in 10 in still air, and an initial velocity of 25 feet per second at least. The machine would be so constructed that the position of its centre of gravity would give the apparatus a downward inclination fore and aft of about 3°. With such a machine one would 'circle like a bird, rise spirally like a bird,' and soar in any direction."

It was Chanute's belief that in the perfect one-man craft of the future, no motor at all would be needed, and he maintained that there were then indications that such flight would be achieved before long. His work produced a group of disciples in France, while in his own country it was eagerly taken up by younger hands before the close of the century. The progress of Wilbur and Orville Wright was in no small measure advanced by his generous assistance and valuable advice.

* * * *

Before proceeding to the far-famed achievements of the Wright Brothers, we must look back a few years on the labours of a steadfast worker who founded the science of flight in America and gave to the world its first real treatise on aerodynamics. Professor Samuel Pierpoint Langley, working slowly and laboriously with infinite pains and skill, was elucidating some of the most difficult problems of aeronautics. He accomplished a definite and exceedingly useful task, and did more than any had hitherto done to show that human flight was no mere chimera but a real scientific possibility.

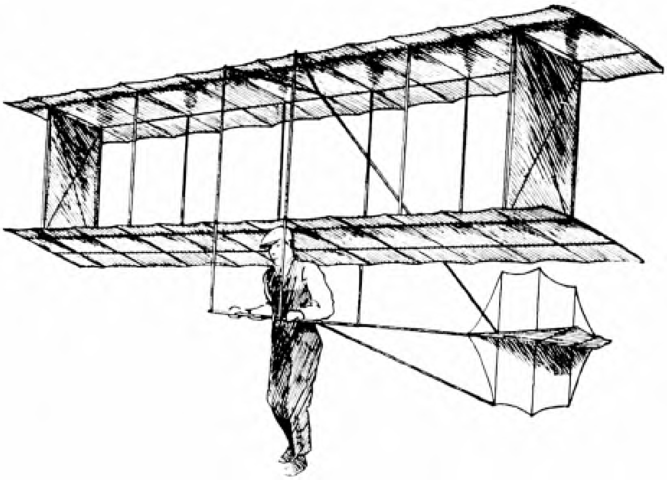
Langley was eminent as an astronomer, and was also an engineer and an architect before he applied his learning and ability to researches in this unexplored field. His *Experiments in Aerodynamics*, published in 1891, is an important mile-stone in aeronautical progress. The purpose of this work was mainly to prove mathematically the feasibility of sustaining and propelling in the air a heavier-than-air machine.

Among the first discoveries he made with his ingenious whirling table were the laws relating to the power required to support and advance an inclined plane at varying angles and at varying speeds. However elementary these laws may appear to our experts now, it is well to remember that they came at that time as a challenge to the Newtonian law of resistance to advance through the air, and had to be conclusively proved.

In 1893, Langley produced his celebrated paper on *The Internal Work of the Wind*. This was written to show the reason why certain species of birds maintain themselves indefinitely in the air without flapping the wing or any other motion than a slight working of the body. No satisfactory mechanical explanation of this anomaly had been given before, and what knowledge there was on the subject of "soaring" was greatly expanded. That

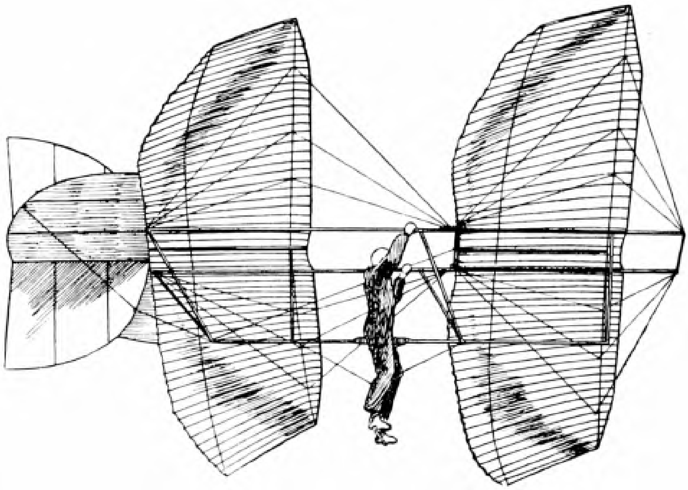
PLATE III.

1



1. CHANUTE S BIPLANE GLIDER.

2



2. THE MONTGOMERY GLIDER.

the ability to soar was in some way connected with the presence of the wind was, to the writer, as certain as any fact of observation could be, though at first the difficulty of reconciling such facts with accepted laws of motion seemed quite insuperable.

Langley had designed a certan special apparatus with which he was able to make observations which showed that the wind in general was not what it was commonly asserted to be, that is, air put into motion with an approximately uniform velocity in the same strata; but that considered in the narrowest practical section wind is always not only not approximately uniform, but variable and irregular in its movement beyond anything which had been expected or foreseen. From which it seemed probable that the smallest part observable could not be treated as approximately homogeneous but that even in this there was an internal motion to be considered distinct from that of the whole body, and from its immediate surroundings. It appeared to follow as a necessary consequence that there might be a possibility of what might be called internal work in the wind. On further study it seemed to him that this internal work might conceivably be so utilised as to furnish a power which should not only keep an inert body from falling but cause it to rise, and while this power was the probable cause of the action of the soaring bird it might be possible through its means to cause any suitably disposed body, animate or inanimate, wholly immersed and wholly free to move, to advance against the direction of the wind itself.

Langley was more than a theorist. In 1896, he constructed his steam-driven models, called "aerodromes," and made almost entirely of steel. Remarkable flights were performed by these models, and the peculiar wisdom displayed in their design is only being fully appreciated after a lapse of twenty-five years.

The value of Langley's work is not to be sought in spectacular results. It lies in the weight of scientific revelation that went far to remove the conservative prejudice that had dulled the eyes even of men of light and learning.

* * * *

There was another pioneer in America, Professor J. J. Montgomery, whose work is apt to be forgotten. His experiments were of a distinct and singular character; yet it is probable that even at the time they were little known and still less understood.

Montgomery had given much attention to the science of aviation, particularly to passive flight, and had constructed several gliders operated by himself or his friends. The most successful of these was of Langley descent, consisting of two curved surfaces fixed on bars in tandem arrangement. Each was 24 feet across and 4 feet wide. These surfaces were curved in the form of a parabola, whereby the curve in front was steep, and that in the back relatively gradual.

After a number of years of experiment with different methods of control, he evolved an effective device for altering the wing curvature during flight, thus varying the lift on each wing, and thereby enabling the operator to control the equilibrium and direction during his glides in the air. This method of preserving balance had been suggested originally by d'Esterno, but was adopted in practice for the first time on this machine. There were also a vertical keel and a tail control plane, the latter being in two semi-circles at right angles to each other, and made movable for both horizontal and vertical steering.

Montgomery tells us that when he commenced his practical demonstrations he had before him three objectives: "First, equilibrium; second, complete self-control; and third, long-continued or soaring flight"—a commendable ambition which was not altogether unrealised.

Whilst conducting these experiments he sustained an injury to his leg which compelled him to look for deputy pilots. So he formed a small school of intrepid enthusiasts, who acquired such confidence in their ability to handle these gliders that they consented to give exhibition descents from balloons. The most spectacular of these daring performances was made in 1905 at Santa Clara in California by Daniel Maloney, who was lifted by a hot-air balloon to a height of 4,000 feet, and then cut loose.

"In the course of the descent," writes one of the pupils, "the most extraordinary and complex manoeuvres were accomplished—spiral and circling turns being executed with an ease and grace almost beyond description, level travel accomplished with the wind and against it, figure-eight evolutions performed without difficulty, and hair-raising dives were terminated by abrupt checking of the movement by changing the angles of the wing surfaces. At times the speed, as estimated by eye-witnesses, was over sixty-eight miles an hour, and yet after a flight of approximately eight miles in twenty minutes the machine was brought to rest upon a previously designated spot, three-quarters of a mile from where the balloon had been released, so lightly that the aviator was not even jarred, despite the fact that he was compelled to land on his feet, not on a special alighting gear."

The facts of this wonderful flight are well attested. It was performed in the presence of thousands of spectators, including a number of responsible reporters. Chanute characterised the flight as "the most daring feat ever attempted." These amazing operations were brought to an end by a disastrous accident, which is the more deplorable in having occurred through no fault of either the machine or the pilot. During the ascent in the balloon a guy rope caught in the framework of the machine and broke the tower that braced the two rear wings and gave control over the tail. The pilot, Maloney, failed to observe the accident. The machine was launched, turned turtle, and settled a little faster than a parachute. Maloney was picked up unconscious, and died half an hour afterwards, although the only mark of any kind on him was a scratch from a wire on the side of his neck. He had descended from an altitude of 2,000 feet, and his death was, therefore, attributed to heart failure.

Montgomery's experiments were probably terminated not so much by this unfortunate mishap as by the San Francisco earthquake, which occurred just when he was arranging to make some other important tests. Public attention and public support were naturally diverted from an undertaking that was so little allied to the immediate needs of that unhappy community.

The work of Montgomery was finally ended by a fatal accident to himself, after resuming experiments in 1911. According to the threadbare local report, "a little whirlwind caught the machine and dashed it head on to the ground; Professor Montgomery landed on his head and right hip. He did not believe himself seriously hurt, and talked with his year-old bride in the tent. He complained of pains in his back, and continued to grow worse until he died."

* * * *

In the meantime the Wright Brothers had come into prominence. Their own experiments did not begin until four years after the death of Lilienthal, but since 1896, when they first devoted their serious attention to matters of gliding, they had been slowly consuming the areonautical information that was then available.

Lilienthal's work and writings especially attracted them, and the fact that a man of Langley's great reputation had declared his belief in the possibility of human flight gave them confidence in attacking the problem which classed all who attempted it with lunatics and believers in perpetual motion.

Accounts of their experiments—both during their progress and after—have been given us by Wilbur and Orville. In their early years, before they were known to the world, they were both engaged on the production of a weekly newspaper, Orville as publisher and Wilbur as editor. Wilbur was at home in the field of journalism no less than he became in the field of flight. No one could better describe the development of their work than he does in an article prepared by himself and his brother for the *Century Magazine* in September, 1908 :—

"It was not till the news of the sad death of Lilienthal reached America in the summer of 1896, that we gave more than a passing attention to the subject of flying. We then studied with great interest Chanute's *Progress in Flying Machines*, Langley's *Experiments in Aerodynamics*, the Aeronautical Annuals of 1905, 1906, 1907, and several pamphlets published by the Smithsonain Institution, especially articles by Lilienthal and extracts from Mouillard's *Empire of the Air*. The larger works gave us a good understanding of the nature of the flying problem and the difficulties in past attempts to solve it, while Mouillard and Lilienthal, the great missionaries of the flying cause, infected us with their own unquenchable enthusiasm and transformed idle curiosity into the active zeal of workers.

" In the field of aviation there were two schools. The first, represented by such men as Professor Langley and Sir Hiram Maxim, gave chief attention to power flight; the second, represented by Lilienthal, Mouillard, and Chanute, to soaring flight. Our sympathies were with the latter school, partly from impatience at the wasteful extravagance of mounting delicate and costly machinery on wings which no one knew how to manage, and partly, no doubt, from the extraordinary charm and enthusiasm with which the apostles of soaring flight set forth the beauties of sailing through the air on fixed wings, deriving the motive power from the wind itself.

"The balancing of a flyer may seem, at first thought, to be a very simple matter, yet almost every experimenter had found in this one point which he could not satisfactorily master. Many different methods were tried. Some experimenters placed the centre of gravity far below the wings, in the belief that the weight would naturally seek to remain at the lowest point. It is true that, like the pendulum, it tended to seek the lowest point; but also, like the pendulum, it tended to oscillate in a manner destructive of all stability. A more satisfactory system, especially for lateral balance, was that of arranging the wings in the shape of a broad V, to form a dihedral angle, with the centre low, and the wing-tips elevated. In theory this was an automatic system, but in practice it had two serious defects: first, it tended to keep the machine oscillating; and second, its usefulness was restricted to calm air . . .

"We resolved to try a fundamentally different principle. We would arrange the machine so that it would not tend to right itself. We would make it as inert as possible to the effects of a change of direction or speed, and thus reduce the effects of wind-gusts to a minimum. We would do this in the fore-and-aft stability by giving the aeroplanes a peculiar shape; and in the lateral balance by arching the surfaces from tip to tip, just the reverse of what our predecessors had done. Then by some suitable contrivance, actuated by the operator, forces should be brought into play to regulate the balance . . . A happy device was discovered whereby the apparently rigid system of superposed surfaces could be worked in a most unexpected way, so that the aeroplanes could be presented on the right and left sides at different angles to the wind. This, with an adjustable, horizontal front rudder, formed the main feature of our first glider.

"We began our active experiments in October, 1900 at Kitty Hawk, North Carolina. Our machine was designed to be flown as a kite, with a man on board, in winds from 15 to 20 miles an hour. But upon trial it was found that much stronger winds were required to lift it. Suitable winds not being plentiful, we found it necessary, in order to test the new balancing system, to fly the machine as a kite without a man on board, operating the levers through cords from the ground. This did not give the practice anticipated, but it inspired confidence in the new system of balance.

"In the summer of 1901, we became personally acquainted with Mr. Chanute. When he learnt that we were interested in flying as a sport, and not with any expectation of recovering the money we were expending on it, he gave us much encouragement. At our invitation, he spent several weeks with us at our camp at Kill Devil Hill, four miles south of Kitty Hawk, during our experiments of that and the two succeeding years . . .

"The machine of 1901 was built with the shape of surface used by Lilienthal, curved from front to rear like the segment of a parabola, with a curvature $^1/_{12}$ the depth of its chord; but to make doubly sure that it would have sufficient lifting capacity when flown as a kite in 15 or 20 mile winds, we increased the area from 165 square feet used in 1900 to 308 square feet—a size much larger than Lilienthal, Pilcher or Chanute had deemed safe. Upon trial, however, the lifting capacity again fell very far short of calculation, so that the idea of securing practice while flying as a kite had to be abandoned.

Mr. Chanute, who witnessed the experiments, told us that the trouble was not due to poor construction of the machine. We saw only one other explanation—that the tables of air-pressures in general use were incorrect.

"We then turned to gliding—coasting downhill on the air—as the only method of getting the desired practice in balancing a machine. After a few minutes' practice we were able to make glides of over 300 feet, and in a few days were safely operating in 27-mile winds. In these experiments we met with several unexpected phenomena. We found that, contrary to the teachings of the books, the centre of pressure on a curved surface travelled backward when the surface was inclined, at small angles, more and more edgewise to the wind. We also discovered that in free flight, when the wing on one side of the machine was presented to the wind at a greater angle than the one on the other side, the wing with the greater angle descended, and the machine turned in a direction just the reverse of what we were led to expect when flying the machine as a kite. The larger angle gave more resistance to forward motion, and reduced the speed of the wing on that side. The decrease in speed more than counter-balanced the effect of the larger angle. The addition of a fixed vertical vane in the rear increased the trouble, and made the machine absolutely dangerous. It was some time before a remedy was discovered. This consisted of movable rudders working in conjunction with the twisting of the wings . . .

"The experiments of 1901 were far from encouraging. Although Mr. Chanute assured us that, both in control and in weight carried per unit of surface, the results obtained were better than those of any of our predecessors, yet we saw that the calculations upon which all flying machines had been based were unreliable, and that all were simply groping in the dark. Having set out with absolute faith in the existing scientific data, we were driven to doubt one thing after another, till finally, after two years of experiment, we cast it all aside, and decided to rely entirely upon our own investigations. Truth and error were everywhere so intimately mixed as to be indistinguishable. Nevertheless the time expended in preliminary study of books was not misspent, for they gave us a good general understanding of the subject, and enabled us at the outset to avoid effort in many directions in which results would have been hopeless.

"The standard for measurements of wind pressures is the force produced by a current of air of one mile per hour velocity striking square against a plane of one square foot area. The practical difficulties of obtaining an exact measurement of this force have been great. The measurements by different recognised authorities vary 50 per cent. When this simplest of measurements presents so great difficulties, what shall be said of the troubles encountered by those who attempt to find the pressure at each angle as the plane is inclined more and more edgewise to the wind? In the eighteenth century the French Academy prepared tables giving such information, and at a later date the Aeronautical Society of Great Britain made similar experiments. Many persons likewise published measurements and formulae; but the results were so discordant that Professor Langley undertook a new series of measurements, the results of which form the basis of his celebrated work, *Experiments in Aerodynamics*. Yet a critical examination of the data upon which he based his conclusions as to the pressures at small angles, shows results so various as to make many of his conclusions little better than guesswork.

" To work intelligently one needs to know the effects of a multitude of variations that could be incorporated in the surfaces of flying machines. The pressures on squares are different from those on rectangles, circles, triangles or ellipses; arched surfaces differ from planes, and vary among themselves according to the depth of the curvature; true arcs differ from parabolas, and the latter differ among themselves; thick surfaces differ from thin, and surfaces thicker in one place than another vary in pressure when the positions of maximum thickness are different; some surfaces are most efficient at one angle, others at other angles. The shape of the edge also makes a difference, so that thousands of combinations are possible in so simple a thing as a wing.

" We had taken up aeronautics merely as a sport. We reluctantly entered upon the scientific side of it. But soon found the work so fascinating that we were drawn into it deeper and deeper. Two testing machines were built, which we believed would avoid the errors to which the measurements of others had been subject. After making preliminary measurements on a great number of different shaped surfaces, to secure a general understanding of the subject, we began systematic measurements of standard surfaces, so varied in design as to bring out the underlying causes of differences noted in their pressures. Measurements were tabulated on nearly fifty of these at all angles from zero to 45 degrees, at intervals of 2½ degrees. Measurements were also secured showing the effects on each other when surfaces are superposed, or when they follow one another.

"Some strange results were obtained. One surface, with a heavy roll at the front edge, showed the same lift for all angles from 7½ to 45 degrees. A square plane, contrary to the measurements of all our predecessors, gave a greater pressure at 30 degrees than at 45 degrees. This seemed so anomalous, that we were almost ready to doubt our own measurements, when a simple test was suggested. A weather-vane, with two planes attached to the pointer at an angle of 80 degrees with each other was made. According to our tables, such a vane would be in unstable equilibrium when pointing directly into the wind; for if by chance the wind should happen to strike one plane at an angle of 39 degrees and the other at 41 degrees the plane with the smaller angle would have the greater pressure, and the pointer would be turned still further out of the course of the wind until the two vanes again secured equal pressures, which would be at approximately 30 and 50 degrees. But the vane performed in this very manner. Further corroboration of the tables was obtained in experiments with the new glider at Kill Devil Hill the next season.

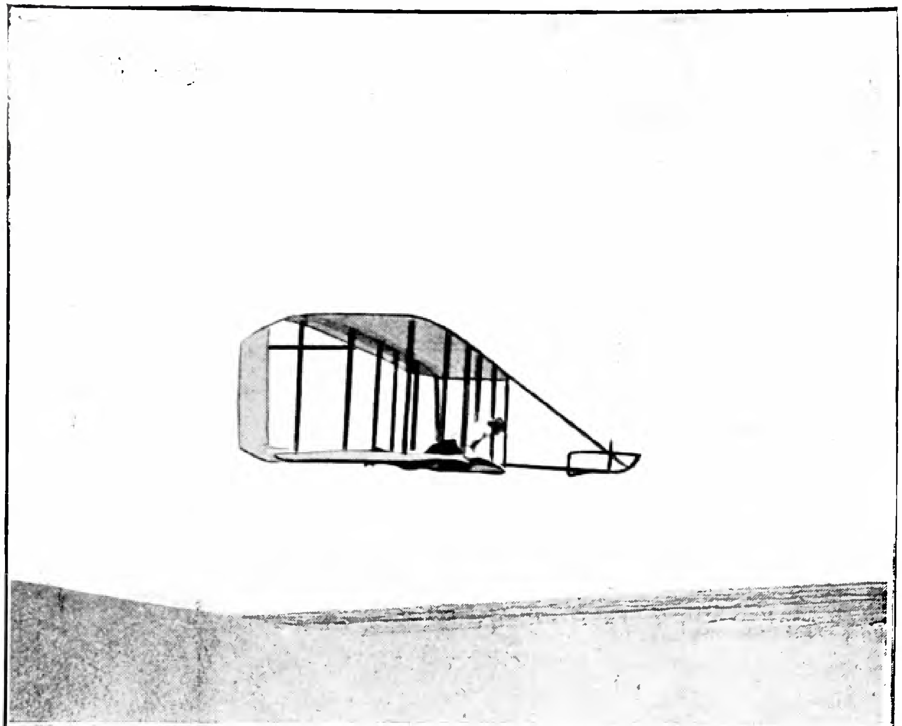
"In September and October, 1902, nearly 1,000 gliding flights were made, several of which covered distances of over 600 feet. Some, made against a wind of 36 miles an hour, gave proof of the effectiveness of the devices for control. With this machine, in the autumn of 1903, we made a number of flights in which we remained in the air for over a minute, often soaring for a considerable time in one spot, without any descent at all. Little wonder that our unscientific assistant should think the only thing needed to keep it indefinitely in the air would be a coat of feathers to make it light! With accurate data for making calculations, and a system of balance effective in winds as well as in calms, we were now in a position, we thought, to build a successful power-flyer . . . "

The rest of the story is a matter of well-known history. The race for priority in the conquest of the air had begun, and it would have been strange indeed if, having attained what seemed to them then a pitch of perfection in gliding, they had not immediately advanced towards their ultimate goal.

The account of their work leaves no room for doubt that their success was due to a combination of scientific skill, undaunted courage, and infinite care. They attempted no new experiment without a reason, and they passed through no new experience without investigating its cause. The fruits of their labour are reflected in gliding to-day. A perplexing puzzle had been resolved into practice, and the practice was raised to the level of an art.

It was the practice of an art in the incipient stage; but a practice from which much could be learnt. Wilbur proclaimed that if the pilot was sufficiently skilful to keep himself from passing beyond the rising current, he would be sustained indefinitely at a higher point than that from which he started:—"Slow glides in rising currents probably hold out greater hope of extensive practice than any other method within man's reach, but they have the disadvantage of requiring rather strong wisnd

PLATE IV.



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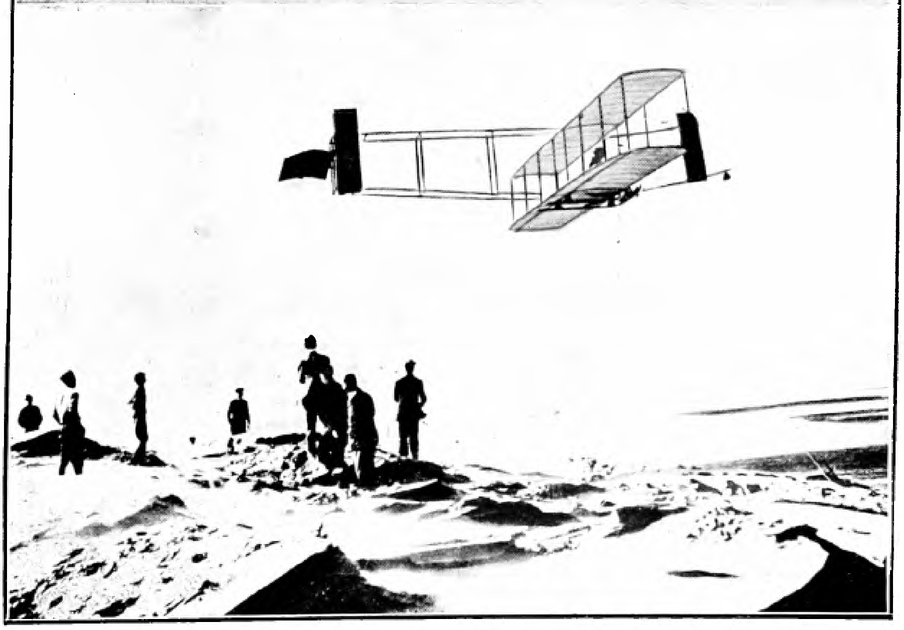


2

(*Courtesy Royal Aeronautical Society*).

1. Wilbur Wright Practising in 1902.
2. Orville Wright "Soaring" in 1911.

or very large supporting surfaces. However, when gliding operators have attained greater skill, they can, with comparative safety, maintain themselves in the air for hours at a time in this way, and thus by constant practice so increase their knowledge and skill that they can rise into the higher air and search out the currents which enable the soaring birds to transport themselves to any desired point by first rising in a circle and then sailing off at a descending angle."

The Wrights had not found themselves at that time expert enough to accomplish anything approaching this; but some years later Orville, practising over the sand-dunes of North Carolina, went far to demonstrate what he knew to be possible. On one occasion in 1911 he is recorded to have remained in the air for a duration of over ten minutes—a performance unrivalled through ten years of subsequent aeronautical progress.

A SCIENCE in its infancy, expanding as it generally does by concurrent research in different parts of the world, seldom admits of a strictly chronological account of its growth. Yet the history of flight, and of gliding in particular, is one of a series of individual efforts, each such effort giving impetus to another, often on the part of a distant observer whose dormant interest might otherwise have never been stirred to animation. On this side of the Atlantic, the prevailing apathy was only dispelled when the rumours from America incited a few to vigorous activity.

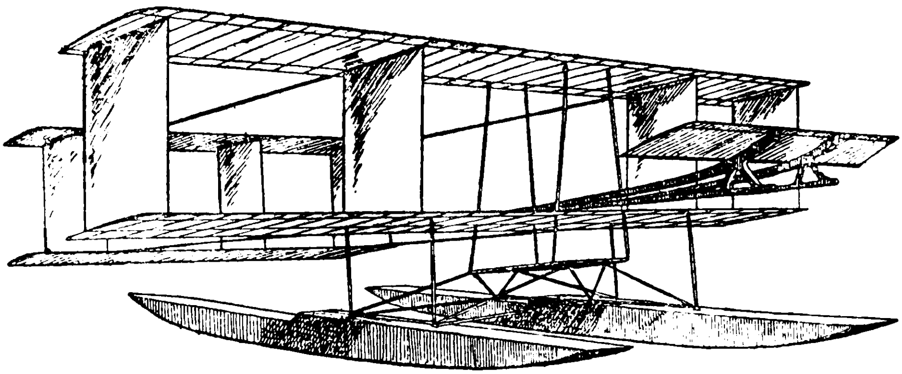
In France some serious experiments in practical gliding were being conducted by Captain Ferber in 1901. Like Lilienthal, he harnessed himself to his glider by the shoulders and arms. By running down a slope into an upward current of air, sufficient lift was obtained to allow a free glide, but these initial attempts produced no results approaching those of his German master. A later machine of the Chanute type brought a higher degree of success, and in 1903, somewhere in Finisterre, a glider, bearing a marked resemblance to the one being used by Wilbur and Orville Wright, gave some true and stable gliding flights.

The visit of Chanute to France about this time, and the particulars he gave of the successes achieved by the Wright Brothers, created a general stir. Archdeacon, Voisin and Blériot, though closely associated in their work, were prompted to tackle the task, each in his particular way. But the men in the background, who pioneered the movement in France, were the two brothers Voisin. From the store of experimental knowledge that these two had acquired, there came the appearance of the early aeroplanes that were known by the name of Farman and Blériot.

Gabriel Voisin, co-operating with Captain Ferber and Ernest Archdeacon, had constructed a box-kite glider, on which some elementary tests were made at Berck-sur-Mer. In order to be nearer their workshops the party returned to Paris, where for want of a suitable hill from which to make glides, this elaborate apparatus was towed by a motor-car across a military drill-ground at Issy-les-Moulineaux.

This method of launching was not satisfactory. An accident destroyed the machine, and the experiments were abandoned.

The next attempts were made on the Seine at Billancourt, the machine being towed in a similar way by a powerful motor-boat, until it rose in the air like a kite. This kite-like floating device had two main superposed planes, 5 feet apart, and



VOISIN'S BOX-KITE GLIDER FITTED WITH FLOATS.

united by four vertical " screens " which provided for lateral stability. In the rear was a tail of similar structure, and in front there projected a supplementary movable surface, fulfilling the function of an ordinary elevator. A vertical rudder was partially enclosed in the huge box-like tail. There was no lateral control ; the pilot could only assist recovery by steering outwards with the rudder, and so increasing the speed and the lift of the lower wing.

On one occasion this machine rose to a height of 55 feet and covered a distance of 160 yards, but it was not long before the experiments on water proved to be as dangerous as those on land. On a subsequent trial a new apparatus, which had been specially constructed for Blériot, was precipitated into the water, Voisin experiencing the greatest difficulty in extricating himself from under the planes.

Later, Archdeacon removed to the Lake of Geneva, where there was greater facility for being towed into a constant head-wind than there was on the comparatively narrow Seine. But no appreciable improvement on their previous performance resulted.

These experiments were conducted only because they served as a useful and immediate stepping-stone to greater achievements with power-driven aeroplanes. They were not regarded as having sufficient intrinsic value, *per se*, to warrant their advancement as an end in itself—except perhaps on their merits as a sport. Voisin declared, in spite of his adversity, that gliding flight was one of the most entrancing forms of sport imaginable:

"Besides the physical development resulting from continued practice, the aviator acquires from this pursuit rapidity of decision, accuracy in movement, and, above all an almost instinctive response to emergencies, that cannot fail to stand him in good stead at some later period, when steering one of the large machines, which demand an unfailing concentration of attention, and extreme quickness of decision at critical moments."

Voisin had certainly paid an exacting toll in forming this opinion. But it remained an opinion on which others, as well as mere seekers of sport, might have acted with profit.

GLIDING was gradually losing its place in the evolution of the successful aeroplane. Soon it became entirely submerged in the advancing tide of premature appetite for imposing displays. Practice in gliding—if practised at all—was only regarded as a gymnastic preliminary of doubtful necessity.

There remained, however, a small and scattered school of the faithful disciples of Lilienthal who firmly believed that ultimate success would only be found in " natural " flight, as opposed to " box-kite-at-any-price " flight. We have so far indicated the historical figures whose pioneer work was the common cause of both these phases of human flight. We must now refer to those whose work is the more immediate forerunner of the movement of to-day.

The quest begun by Lilienthal was continued in Germany by Igo Etrich, and research was pursued independent of progress by contemporary workers in foreign parts. A small group of sympathisers have always supported this school, and their energies have kept it in being until its recent expansion.

In England still fewer were associated with this particular field of research. Foremost among experiments of first importance in this direction were those of José Weiss. They provided the nucleus of that branch of knowledge which the present enthusiasts now seek to extend. As such they deserve our special attention.

The earlier attempts to fly with wings approximating in pattern to those of the soaring bird, had all been defeated by a lack of acquaintance with the principles of the natural stability of birds. Increase of efficiency could not be obtained until this precursory question was solved. The machines of Farman and the Wright Brothers had been designed with a good knowledge of the mechanical principles involved, and the workmanship of the Voisin Brothers was exquisite ; but these machines were at best very crude, and the stability in anything but calm and light winds was exceedingly doubtful. Ignorance on matters of stability was the constant obstacle in the way of the Wright Brothers. They devised mechanical means of control, which devices were later considerably improved. But the maintenance of equilibrium always depended on the skill and constant vigilance of the operator. Wilbur Wright, in relating his experiences in gliding, explained how in every glide variations of wind in velocity and direction had to be encountered. "These variations not only cause those disturbances of the equilibrium which come from the travel of the centre of pressure due to the changed angle of incidence, but also, owing to the fact that the wind changes do not occur simultaneously or uniformly over the whole surface of the machine, set up other disturbances still more troublesome. A sudden gust, we are told, will strike the front of the machine and throw it up before the back part is acted upon at all. Again, the right wing may meet a wind of very different velocity and trend from the left wing, and in this case the machine tends to turn over sideways . . . "

Equilibrium requires the coincidence of the centre of pressure with the vertical axis through the centre of gravity. Natural stability is the quality of maintaining equilibrium under disturbing influences. It was particularly to a search after this elusive quality of the natural stability in natural flying that José Weiss applied his preliminary exertions.

By a systematic series of painstaking experiments he arrived at a design of wing that conferred natural stability in flight. Models ranging in size from that of a rook to a man-carrying glider, and extending in number to upwards of two hundred, were constructed to achieve this purpose. These models had the appearance of headless birds, all bearing a family resemblance. The earliest were made of a framework of tense bamboo, stayed by piano wire, and covered with muslin over which white paper was pasted, the whole being varnished to give a smooth surface. The loading varied, of course, with the size of the model, but the ballast, consisting of a roll of lead fixed between two rubber springs, was generally in the neighbourhood of two-thirds of the total weight of the model.

No fixed curves were accepted at the outset as necessarily correct. The whole design, in fact, was determined by inference drawn from the behaviour of the model in the air. The wing displaying the higher efficiency in flight was adopted as the standard to which the other was "flexed." The empirical knowledge that was gained by this process provided the data on which were based the theories that marked a new era in the evolution of gliding flight.

In the light of the knowledge of to-day these theories may appear to admit of certain modification, but they are of special significance as a solitary attempt to decipher the secret of " sailing " flight—as a separate issue in the general question of air navigation. They are for the most part embodied in papers* read by José Weiss before the Aeronautical Society (not then Royal) in 1907 and 1908, and in the ensuing discussions:—

" Whenever a thinking man begins to consider the question of flight, he is struck by that mysterious paradox of nature, the perpetual movement witnessed in the flight of sailing birds, and on examining the various explanations of the phenomenon brought forward from time to time, one must confess that none of them can be said to be anything like satisfactory. I will mention only the explanation proposed by Professor Langley in his famous work on *The Internal Work of the Wind*, but only to ask if, with all our admiration for Langley's able and patient work, we are not bound to acknowledge that the even and undisturbed course of smoke,

* "Aspects of Sailing Flight."

of clouds, or of a balloon, testify to the even and undisturbed course of the great masses of air and to the total absence of any variations of speed or pulsations whatsoever from which mechanical energy might be derived? I submit that Langley's mistake lies in the fact that he places the pulsations recorded by his instruments in the wind itself, whereas, in fact, these pulsations are produced by the encountering of resistance; that is, by the instrument itself. Air, like water, cannot be touched without a wave being instantly set up. Have you ever seen a flag or similar object exposed to the wind that did not wave?—and we can gather an idea of the mechanical energy of air waves from the fact that they will tear a flag into rags, and that even such minute ones as the sound waves are strong enough to impress the hard substance of a phonograph cylinder.

"I have been experimenting with models of gliders on and off ever since I was a boy, but it is especially since Lilienthal's unfortunate death that I have taken this up as a regular hobby, and in these past five years alone I reckon that I have constructed no less than some 200 of these models. The thousands of launchings made with these models have led me to the discoveiy of a factor, the existence of which I can prove experimentally, and it is that factor, as I shall endeavour to show you, that lies at the very root of the problem.

" In all my reading on this favourite subject I have never come across any suggestion that the reaction which takes place in sailing flight could be due to anything else but air pressure. It is this very point which is misunderstood. I mean to say that there exists a mistaken conception about the nature of the reaction which really takes place. Let me quote a short example which will better explain what I mean by the 'nature' of a reaction. We can press a nail into a piece of wood by means of a hammer, but not without great exertion. By striking the nail with the hammer, we can drive it home with hardly any exertion at all. In both methods there is a reaction of the hammer on the nail, but in each case the reaction is different in its nature and in its properties.

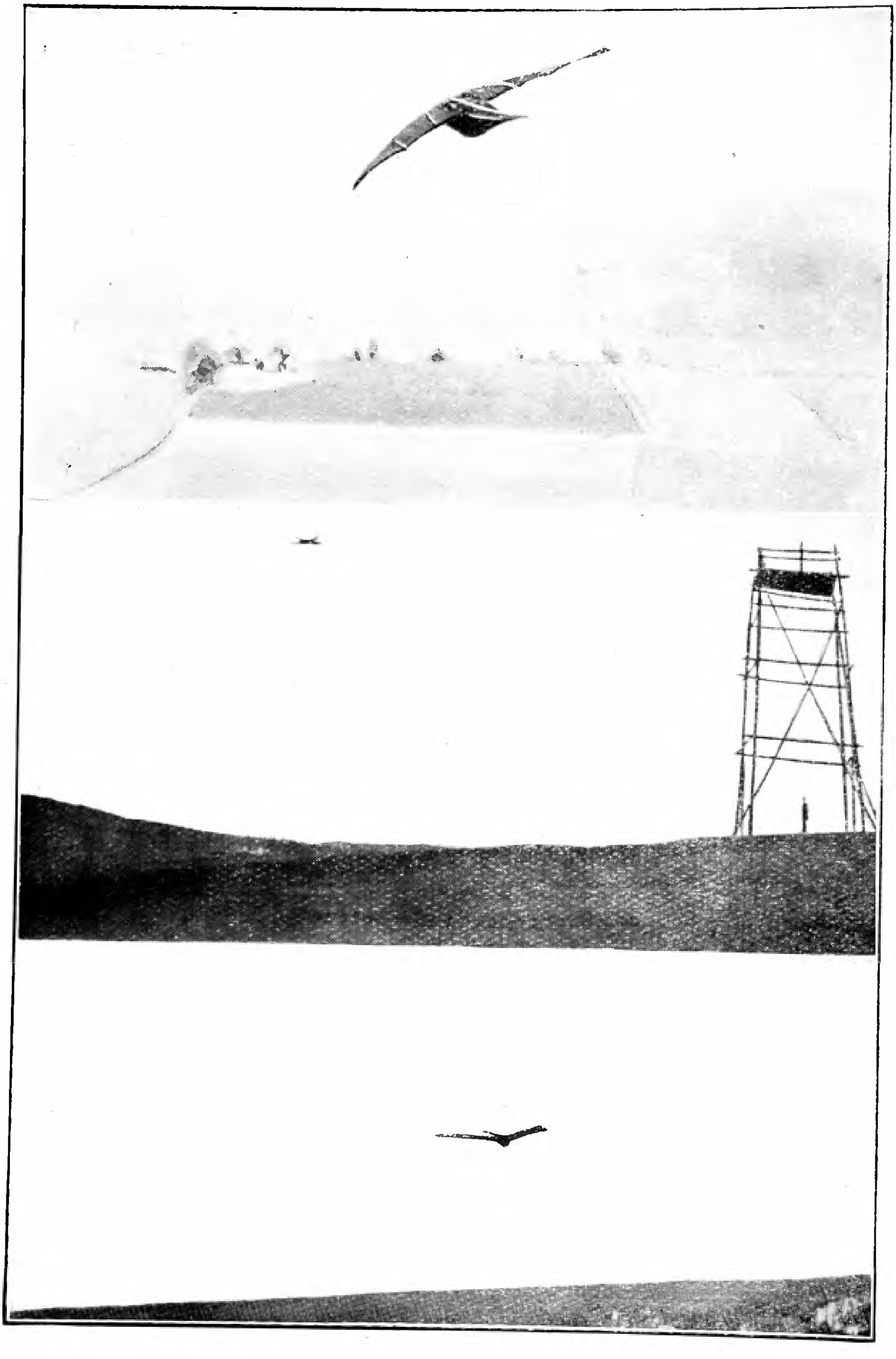
" My contention is that the reaction which obtains in sailing flights is *not* a reaction of pressure ; that reaction is a vibrative one ; that is to say, that by the impact with the air of the bent down front edge of the wing a wave is set up under the wing, and it is the action or the beating of that wave on the under surface of the wing which produces the sustentation or lift. And the properties of that energetic wavy reaction are different to the properties of a reaction due to ordinary air pressure.

"The properties of the reaction due to air pressure are known with certainty from secular experience. They are mainly threefold. In the first place, the reaction due to air pressure is proportional to surface. In the second place its intensity varies as the square of the speed. In the third place, in the case of a plane striking the air at an angle, the reaction is proportional to the sine of the angle which the position of the plane makes with the line of flight, commonly called the angle of incidence.

"I am in a position to show experimentally, by means of experiments made with actual free gliders, and of such a nature that they leave no room whatever for doubt, that :—

"Firstly, in free sailing flight, at equal speed, the reaction is *not* proportional to surface, but varies as the power four-thirds of the surface. I had observed some years ago that in birds of similar shape the relation of weight to surface followed that ratio. In a pamphlet published in September, 1904, M. Goupil relates that he has observed the

PLATE V.



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Weiss Models in Flight.

1. 1910 (*Courtesy Mr. Ronald Graham*).
2. 1906.—Surface, 18 Sq. Ft.; Total Weight, 28 Lbs. Ballast, 16 Lbs.
3. At the R.A.S. Kite Display, July, 1907.

same fact with birds, but draws no conclusion from it. Had M. Goupil been in possession of a type of glider sufficiently perfect to obtain from it accurate data, and had he made experiments with models of graduated sizes, he would have found, as I have found, myself, that at equal speed the relation of weight to surface in artificial free gliders is exactly the same thing as in birds. I can assure you that the experiments by which I can prove this point are very striking and quite conclusive.

"Secondly, whereas we know that reactions due to air pressure always vary as the square of the speed, I can show by experiments equally conclusive that in free sailing flight the reaction does *not* vary as the square of the speed, but is directly proportional to it, neither more nor less.

"And last, but not least, whereas we cannot conceive a reaction due to air pressure unless there be at least a slight incidence, I can prove experimentally that not only in free flight no incidence whatever is required, but that the position in which the maximum of that vigorous reaction is obtained is when the main portion of the wing is dead parallel with its line of flight, and you will observe that in this position, the front edge being bent downwards, the chord drawn from the front to the rear edge is actually at a negative angle with the line of flight. It will be remembered that Lilienthal also revealed the existence of a reaction at a slight negative incidence.

"I cannot enter to-night into the details of the experiments, nor do I expect that my statements will be accepted unreservedly, since they are in direct contradiction with all the accepted formulae and theories, but if an opportunity can be arranged at some large empty hall, where I could use models up to 20 or 30 lbs. in weight, I am willing at some future date to show the truth of my statements in the presence of experts. To see is to believe; when you have seen the experiments, you will know, as I know myself, that these things are not a matter of opinion, but that they are hard facts from which it is impossible to escape.

"Let us now examine the bearing of these facts upon that vexed problem of the motionless sailing flight of birds. In our attempts to figure out the sailing flight of birds, the greatest stumbling-block is 'incidence,' because incidence cannot exist without creating resistance, and involves therefore the exertion of some power to overcome that resistance, and we know that the sailing bird develops none. But if the lift can be obtained without incidence, as I can prove it to be the case, then the problem immediately takes a different aspect.

" If a dead bird with its wings outstretched in the correct flying attitude be exposed well straight to however strong a current of air, it will be found that the resistance offered is absolutely nil. This may seem paradoxical and impossible. It is nevertheless an undeniable fact. To account for it we must bear in mind two things. The first one is that for some unaccountable reason, a curved surface, such as the upper part of the front edge of the wing, not only offers no resistance, but actually produces a sort of suction. Lilienthal had already found this to be the case, and it is also very easily ascertained experimentally. The second point to bear in mind is that from the mere fact of its construction, the wing of a bird produces a forward horizontal component, because the front edge being bent down and rigid, and the rear edge being tapering and yielding, the supporting air must tend to escape from behind, and in doing so, sends the bird forward.

" If we now consider that the lifting reaction is produced by speed only, and that the resistance to penetration is nil, it becomes evident that as soon as the bird begins to glide, the speed, and consequently, the lifting reaction, must increase as long as the line of flight is a descending one, until the mean direction, which in this case is the horizontal, is reached, and this, theoretically, is nothing short of the perpetual movement which we observe in the sailing flight of birds. One might object that the production of the sustaining wave must absorb a certain amount of power and cause a corresponding amount of resistance; this is obviously so. But we must bear in mind that the whole of the weight is acting as motive power, and that with a perfect wing and the absence of all resistance and friction, that weight must naturally produce a reaction equal to itself.

"I think I can safely challenge anyone to figure out from the 'incidence' theory even such results as I have obtained from my own models. I have in some cases in dead calm air obtained glides with 3° of the horizontal; that is with a drop of one in about twenty. Such successful glides cannot, however, be obtained at will withun guided models, as they depend upon an accuracy of conditions which can only result from a living balance. But the mere fact of having obtained them once proves conclusively that the feat is possible as soon as all the necessary conditions are present.

"The question of flight is not one of great power; it is proved by the motionless flight of the larger birds. It is entirely one of perfect shape and material; of perfect relation of weight to surface; of perfect adjustment of centre of weight; of perfect amount of rigidity and elasticity in relation to weight; and last, it is a question of necessary reflex movements on the part of the operator to counteract instantaneously any disturbing effect from outside causes. But all these conditions can be conquered; it is only a matter of dogged perseverance, of time and of money. I firmly believe in the advent of a flying machine within the price of a bicycle, with no other power required than that which a man can develop by means of pedals unaided by machinery . . . "

"I think I may speak with authority on the matter of longitudinal balance, because with my gliders I have obtained results which no man has obtained before. For instance, I have this model, which I made for this occasion, but it would be dangerous to show it in this room, as it is too fast. In order to make this model fly in the wind, I have to put on this weight, which brings the weight up to ½ lb. I launched it yesterday and the results were so extraordinary that I hardly expect to be believed; I do not know that I should believe in them if I had not seen them myself. There was a N. E. wind blowing at the rate of about 20 miles; I had favourable circumstances and I launched it twenty times. Once it stood for 40 seconds quite motionless. It was launched on the ground, rose to 30 or 40 feet, did not turn, did not lose its height and remained hovering like a hawk or a kestrel. It is a positive fact.

"With regard to the weight, if I weighted this model with this weight, the speed will be 10·50 m. per second. Now in order to make this other larger model go at exactly the same rate, viz: 10·50 m. per second, I have to weight it up with 15 lbs. of lead. If I weight it like this it will travel at the same rate. When I put the large weight on this model, which has a surface of 27 square feet and weighs 11 lbs., the total weight becomes 26 lbs. The small one has a surface of 1·37 square feet; when it is weighted for 10·50 m. speed, it will carry not even as much as ½ lb. per square foot, and will travel at the same speed. If that is not a proof that the reaction is not proportionate to the surface, I give it up.

"The theory of longitudinal balance is this. If I hang this model level, it remains so. What is the reason of that? It is because the support is in concordance with the centre of weight. If I move the support only a quarter of an inch below, the thing goes forward; so that we can say with certainty that the only cause that can make an aeroplane keep horizontal is when the two centres of pressure and gravity are in concordance; and inversely when we see a glider keep the horizontal position, it is because the two centres of pressure and gravity are in mathematical concordance. If you start from this fact, what causes them to remain in concordance? We know all the difficulty of longitudinal balance arises from the mobility of the centre of pressure, and when we see a glider keep its balance, we conclude the two centres are in concordance. The reason is, to my mind, that the movements of the centre of pressure, although very nimble, are not erratic; they follow the speed. If the speed alters, the centre of pressure shifts.

On the other hand, if the movements of your centre of pressure are not erratic, the speed also is not erratic. In the same way as the length of a pendulum determines the beat of the pendulum, so in a glider there are certain factors which determine its speed. If, therefore, we know those factors, we can calculate the speed, and if we also know what are the movements of pressure in relation to that speed, we can calculate where the centre of pressure will move at that particular speed. The law is this: That the centre of weight has to be at the point which the centre of pressure reaches at the normal speed. The glider cannot possibly, then, lose its balance, because it tends to its normal speed, whatever the wind in which it may be. I am talking of its relative speed—of the speed compared with the surrounding air. As soon as it goes back to that speed, the centre of pressure moves back into the centre of gravity, and the glider rights itself. You cannot upset it. From my tower, which stands 42 feet from the ground, I can throw my model anywhere and it will right itself . . . "

"In a sailing bird or in a properly made glider, the main portion of the motive power is derived from the weight, the trajectory produced by the weight in following the path of least resistance depending entirely upon the perfection of the aggregate features of the bird or glider. If we assume, for instance, in a perfectly balanced glider, the resistance to horizontal penetration to be equal O, and the lift, that is, the resistance to vertical fall, to increase with the speed, the result must be perpetual motion in the horizontal direction. Now, these are the very conditions that obtain in the sailing bird; we have a form which offers no appreciable resistance to penetration; we have the lift increasing with the speed; and we have also the resulting virtual perpetual motion which we observe daily.

"I have often seen my own roughly-made gliders, when at their best, reach in calm air 3° or even 2° from the horizontal, and in ascending currents they frequently rise to a considerable height whilst travelling against, or circling in, the wind, exactly in the same manner as a bird. What wonder, then, that the sailing bird, in which every feature is absolutely perfect, and which has a living balance, should reach, in 'falling,' a line of flight that is practically the horizontal? It is obvious that the power required to reach the horizontal becomes, in this case, infinitesimal.

"In nature, such additional power as may be wanted is easily supplied by the least ascending currents, of which the bird takes every advantage, and so it is that it sails indefinitely, without expending the least power, as the mere result of gravitation. The supposed mystery of the beautiful ascending orbs of the larger sailing birds is thus fully explained . . ."

"The data available at the present time are inadequate to arrive at anything definite from mathematical deductions, and we have, therefore, no option but to resort to empirical methods. There are at least three factors which, as far as I know, no mathematician has ever brought to bear on his equations. The first is the all-important action of the weight as motive power. The second is the beautiful horizontal component produced, under the action of the weight, by a rigid and bent down front edge and a very flexible rear edge. The third and most important is the powerful vertical suction produced by a current of air striking a convex surface tangentially, and which so far, is not only undefined, but completely ignored . . ."

With a view to defining the properties of this reaction, lengthy experiments were conducted with the later co-operation of Mr. Handley Page. The results obtained were published in sundry pamphlets. They are of a purely technical nature, and lie beyond the scope of our immediate notice.

Prominently associated with the later experiments of José Weiss was Alexander Keith. His peculiar acquaintance with the anatomy and the muscular operation of the wings of soaring birds was a source of much of the valuable data which determined the laws relating to the wing construction of gliders.

By gradual advances the scale of these experiments progressed in 1909 to that of the man-carrying glider. This craft differed only in size from the smaller models. It was a cantilever monoplane constructed of a framework of bamboo over which was stretched a rough fabric of varnished calico. The wings were characterised by a marked change of angle and attitude from shoulder to tip, the leading edge being swept back to the tip, where it joined the trailing edge. This was slightly flexible and, in conjunction with the double curve of the supporting surface, gave automatic stability. The wings were bolted to a small tailless body in which the pilot was seated. Bamboo stays secured them to uprights a little distance above the skid. There was no other bracing employed. The only stabilising control was effected by a slight warping of the flexible wing-tips, reliance being placed in the inherent ability of the machine to preserve its balance.

The operation of launching had already occasioned much difficulty with the models approaching man-carrying size. During the previous two years about a dozen different methods of automatic launching had been tried. One of the most successful was a pole having at its top a cross-bar from which two parallel hawsers ran into the ground at a distance of about three times the height of the pole. The pole was mounted on a pivot and surmounted by a weather-cock, so that the hawsers could always be fixed in the correct position facing the wind. The machine to be launched was placed on a cradle which itself was fitted with grooved wheels running on the hawsers. The initial thrust was obtained by means of a weight and a system of block pulleys fixed on the pole. This gave excellent results, and models weighing over 60 lbs. were repeatedly launched, without a hitch, in calm and in rough weather. The difficulty, however, of stretching the hawsers sufficiently and equally tight, and the time lost in constantly changing their position with each variation in the direction of the wind, proved to be a great drawback, and a possible source of danger if the system were used for a man-carrying machine.

These defects led to the construction of a more elaborate "launching-ways." This was a rigid structure made of light steel joists and resting on a pivot; it was steadied by four rollers running on a circular rail placed on the ground. At the top was a small platform reached by means of a ladder. The cradle was dispensed with, the machine being allowed to run down the floor of the ways on its own wheels. The glider to be launched was brought to the foot of the ways and hauled up backwards by means of a windlass, and whilst it was being hauled up the impelling rope was attached to it by means of a special catch, so made that as soon as the least lift was produced the machine was released automatically and the launching effected.

The last experiments, however, with the piloted glider were made without the aid of any elaborate launching appliance. The machine was simply reposed on an independent pair of wheels on which it would run to the edge of a precipitous slope, when a vigorous push from behind would launch it over the brink well out into ascending currents.

Once in its natural element the glider sailed with the absolute stability of the smaller lead-carrying models. Windy weather would provide a certain measure of true soaring flight, and there can be little doubt that, with modern methods of construction and control, performances approaching those of to-day might soon have been easily accomplished. These flights were performed from the northerly slopes of Amberley Mount in the Sussex Downs by Mr. Gordon England and Mr. Gerald Leake, neither of whom had had any previous experience in the air, and it was largely due to their courage and confidence that remarkable glides were obtained.

The causes which have contributed to the standard of present-day gliding are several. It was at least expected by some to follow these experiments; and it is interesting to recall a contemporary pronouncement in a leading aeronautical journal: "An acme of skill to which we may some day attain, and a forecast of a time when we may perhaps emulate such exploits with no feeling of risk whatever, but with a confidence of safety borne of a sense of knowledge of complete control of machine and element."

But the advent about this time of the light aero engine withdrew the few supporters of the cause of flight on natural principles. Real progress by way of body and wing design has been retarded by over ten years, whilst the box-kite type of aeroplane was refined to a relative but wasteful mechanical perfection.

The only type of British power-driven aeroplane to incorporate the features of these gliders was the first Handley-Page. Mr. Handley-Page, who was then conducting experimental work at Barking, had long been interested in the Weiss principles; and his first machines of 1911 and 1912 were built in general accordance with the design of the gliders. Some notable flights were made on those machines; if the gliding experiments had reached a more practical completion, this type might never have passed out of vogue.

The theories of flight evolved by Weiss and Keith enabled them to compile a table giving the particulars of aeroplanes of unlimited size. Weiss was the first to urge the higher efficiency of large machines when once in the air, and it is striking to note how closely those figures,* deduced from the laws of natural flight, approximate to those of existing large aeroplanes.

To José Weiss it was not granted to see the completion of his work. Lack of support compelled him to abandon experiments, the value of which is only being learnt in the lesson of ten years' experience. But such is the way with all pioneers.

The only other leader in this field of research was Captain J. W. Dunne, an officer of the Royal Engineers, who for some years had been privately experimenting with a gliding machine. It is recorded that Mr. H. G. Wells was associated with him in this enterprise. There is unhappily no authentic chronicle of the early gliding work of Dunne, but it is at least certain that the qualities of these gliders were severely tested, and they were found to be superior in many respects to the famous gliders used by the Wright Brothers.

In 1907 and 1908 identical experiments were carried out at South Farnborough for the purpose of comparison. Their incontestable success led the authorities to give Capt. Dunne permission to pursue

* "Notes on Giant Aeroplanes."

PLATE VI.

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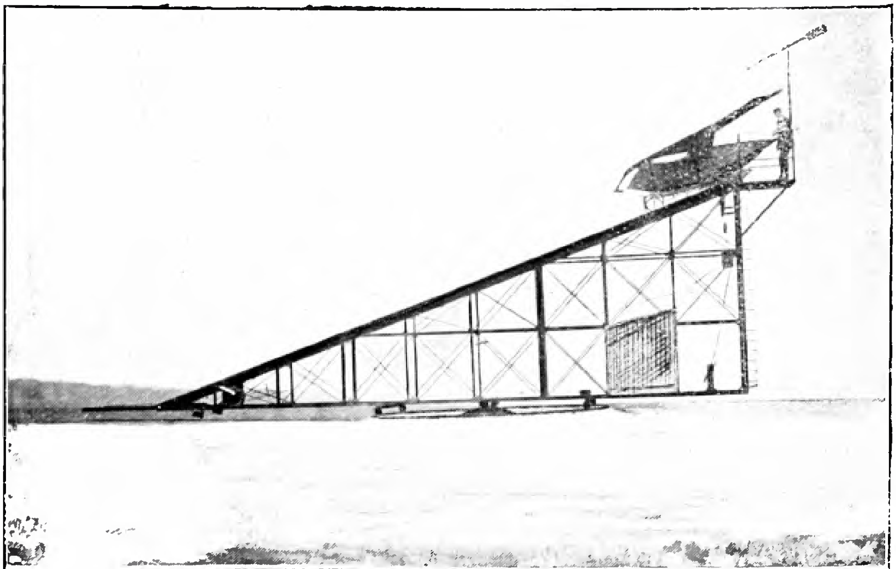

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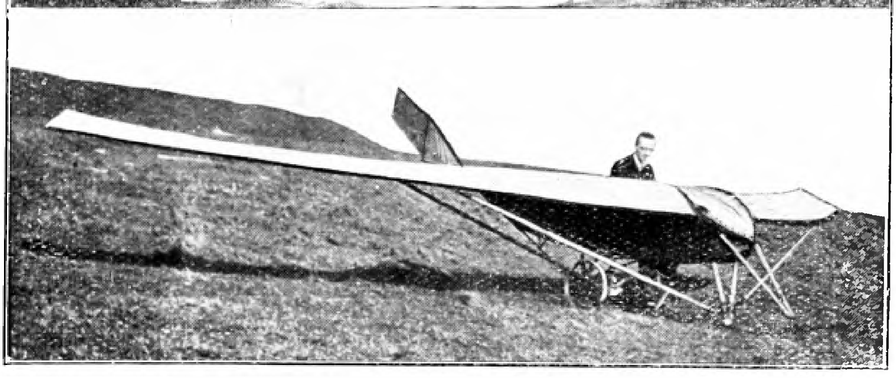

1. WEISS GLIDER ON LAUNCHING WAYS, 1908.
2 and 3. A LATER GLIDER FITTED WITH A VERTICAL TAIL FIN.

them on a larger scale. Accordingly a large motor-driven aeroplane was built and transported to Scotland where further experiments were conducted in the strictest secrecy.

The principle of the Dunne glider differed completely from any that had been hitherto employed. The secret of the soaring powers of the boomerang had baffled scientific investigation, though it had long been recognised that it might prove of the greatest importance to the study of flight. Capt. Dunne found the key to this secret, and it was this principle that he employed in his gliders. It was intended primarily to solve the problem of automatic stability by the shape of the carrying surfaces, and not by any extraneous balancing or controlling device.

Later he evolved a machine of entirely novel design. The planes, in plan view, formed an angle with the apex in the direction of flight. There was no tail or supplementary planes of any description. It was also characterised by a gradually diminishing incidence of the planes from the root to the tip where they presented an actual negative angle, and by a curious downward bend of the trailing edges over a short distance where the two surfaces met in the centre.

Its hall mark was its method of dealing with turning stability. Its tendency was to level up when turning, which was effected by holding it down to a bank against this tendency, and then letting its directional stability take it round in a circle.

* * * *

Reference has already been made to another important source of knowledge, originally quite independent of the pioneer work in England. The contemporary influence of individual research is seldom very widespread, and it is inevitable that investigators in any new field of activity should cover the ground of the other. The aeronautical vision of Etrich and Weiss had not only directed their energies into similar channels, but had led them to ideas of striking resemblance in matters of wing and body design.

During the last few years of the nineteenth century the purchase was made by Igo Etrich, more by chance than through any other motive, of the actual glider on which Lilienthal had met his death. Etrich, it is true, had previously conceived a great interest in the work which had by then been effected towards the accomplishment of human flight, but the acquisition of this historic machine incited him to active efforts. Experiments were cairred out during part of 1899 and 1900, but without a large amount of success, for stability was poor.

The first glider did not incorporate any of the aerodynamic principles which have since made the name of Etrich famous, though it is true that the wings were a trifle swept back from their central entering edge. No tail of any description was employed, nor was there any device for warping the wings, as no pilot had then been carried.

Etrich was giving much attention to the manner in which stability could be obtained by adopting the principle of the Zanonia "Leaf." This is a large two-winged seed; the heavy pod is right in front and the wings curve back on either side. By reason of the contour and plan form of its surface, it immediately rights itself when dropped in any poise, and is enabled to remain perfectly steady both laterally and longitudinally in gliding flight. Ahlborn, of Berlin, had already drawn attention to the gliding qualities of the Zanonia leaf.

Etrich's investigations resulted in the production of another tailless glider, having wings which closely followed the form of the leaf. This, when loaded with sand-ballast, soared to a great height if flown as a kite, and matters were therefore so arranged that the cord could be slipped when once the apparatus had attained sufficient altitude. This was carried out on numerous occasions, the kite—being deprived of what practically amounted to its power-plant—converting itself into an aeroplane and gliding for long distances at small angles with the horizontal.

In 1905 the "Premier Concours d'Aviation de l'Aéro Club de France" was held in Paris. Wels, an assistant of Etrich, was present at this meeting. The performance of the Weiss models which were among the exhibits, had attracted considerable attention, and Wels was naturally much impressed by the results attained by a machine of a design in many respects similar to his own—yet these models were evolved quite independently of the Zanonia principle. Their influence travelled to Germany.

Etrich was producing a machine which it was intended to propel by means of a 3½ H.P. motor-cycle engine. At the instigation of Wels, he then started the manufacture of a man-carrying glider of general resemblance to his last, but modified to conform with the principles of the models that Wels had seen at Paris. This glider was nearly 40 feet in span, and experiments were carried out in 1906 with 150 lbs. of ballast, which was lifted with ease. The glides were made from the slopes of the Giant Mountains at Oberalstadt, near Franknau, Bohemia, down which the machine ran on runners sliding on a greased wooden track.

In these experiments lay the inception of the "Etrich-Taube," the prototype of a singularly successful order of German machines. The question as to whether stability, such as was inherent in the Etrich design, was entirely desirable, has been disputed. But it is clear that in Germany there was scarcely a pilot, prior to the outbreak of war, like those to be found in France or in England, who flew a fast and unstable machine with the idea that it was a form of excellent sport.

The requirements of war impelled an abnormal development of particular features. The demand for performance regardless of cost, and the need for speed on a fighting machine, involved the use of a wing of the low lift variety. Considerations affecting stable and economical flight gave place to the factors of manoeuvrability and speed.

Quite apart from this unfortunate purpose to which flying was being put, there remained in Germany a certain few who adhered to their former faith. They maintained their belief that experiments with gliders might serve as a path to the knowledge of the secret of soaring flight. In the summer of 1912 a gliding competition had been actually held in the Rhön Mountains by an association of gliding clubs of Germany. It does not appear to have been very much more than a sporting affair, and no records exist of any striking results. But this organised meeting had a certain significance, as affairs in that country eventually proved.

The possibilities arising out of motorless flying were appreciated at least by these few—yet expansion of knowledge in this direction, when compared with the triumphant advance of mechanical flight, suffered a check as effective as any interruption in the course of its history.

FROM the conditions prevailing in earlier times, it is hardly surprising that Germany should provide the stage for the revival of gliding. A latent ardour was suddenly roused to a state of active interest. The links with the past had not been entirely severed. Herr Harth, and probably others, had practised the art at Heidelstein in August, 1916.

The causes conducing to this form of aeronautical research have already been noticed. Largely by the efforts of Herr Oskar Ursinus, editor of the journal *Flugsport*, the Rhön meeting of 1912 was reinstituted in the summer of 1920. It was supported, in the matter of prize-money, by the German Air Ministry, by the Directors of German aircraft concerns, and by various aeronautical organisations of the country.

The sincerity of the promoters of these experiments was reflected in the co-operation forthcoming from all parts of Germany. The number of local clubs that entered for the competition testified to a widespread enthusiasm. Among the entrants were ex-war pilots of great experience on fighting machines, and among the machines were many designed by some of the cleverest technical authorities on aeroplane design in Germany.

The most successful machine at this meeting was entered and flown by Herr Klemperer, an aerodynamical expert from the Technical High School of Aachen. It was a highly-efficient cantilever monoplane, built up almost entirely of plywood. Klemperer's longest flight, in point of time, slightly exceeded five minutes, while his machine had shown on several occasions an apparent gliding angle of less than 1 in 30. Among the most interesting craft that appeared was a large monoplane of the "Zanonia Leaf" or "Taube" genus. The wings were constructed largely of bamboo, great flexibility being aimed at. Control was effected by warping the wing tips, these being used for lateral, longitudinal and directional steering.

The results obtained at this competition, and the interest it attracted in Germany and abroad, were distinctly encouraging; and it was decided to make the meeting an annual international fixture.

On the day following the 25th anniversary of the death of Lilienthal, the 1921 competition was opened. This meeting was held between August 10th and 25th, in the Rhön Hills as before. These hills are of considerable magnitude, with gradients that are amply steep. Their slopes are relatively smooth and free from rocks, trees, and other obstructions. The district is, in fact, admirably suited to gliding experiments.

No fewer than forty-five entries were made, though only eleven machines actually succeeded in passing their qualifying tests. During the fifteen days of the competition 128 flights were performed, varying in length from fifty yards to two and a half miles. In the course of these flights the pilots displayed a considerable advance of dexterity in manoeuvring their machines. Herr Klemperer, on the glider which had figured so prominently in the previous year, again provided the best duration flight of the meeting, but owing to unfavourable weather conditions, there was no performance during that time which amounted to a substantial improvement on those of 1920.

A notable machine that was entered was that of Herr Köller. It was a thick-winged monoplane of very simple construction, arranged so that the two halves of the wing could be rotated, either independently, thus giving the effect of a warp, or together, to increase the angle of incidence of the whole. These features provided exceptional manoeuvrability, a quality of first importance to successful "soaring."

Although the competition itself produced no results of much consequence, a great advance was made during the weeks that followed its close. On August 30th Herr Klemperer, on the Aachen monoplane, made the first of a series of flights of historic significance. Leaving the Wasserkuppe, the particular hill selected for use in the competitions, he covered a distance of approximately six miles, remained in the air over 13 minutes, and landed at a point some 1,200 feet below that of the start. The flight was more or less parallel to a range of the hills, and was accomplished largely with the aid of ascending currents.

Of equal or greater significance was a flight of 21 minutes' duration made by Herr Harth a few days later, on a machine somewhat similar to that flown by Kōller. This flight was confined to the neighbourhood of the starting-point, the landing being made within a distance of 150 yards, on a spot that was not more than 40 feet lower. During the flight the machine had risen upwards of 400 feet. The slope of the land below was one of only six degrees, and it was claimed that the flight was made by taking advantage of the energy supplied by gusts, and not due solely to rising currents. It was claimed—probably for the first time—that by manoeuvring in a manner whereby changes in the wind-speed were made to increase the effective air-speed of the machine, the pilot had been able to utilise the gust energy of the wind.

No occasion had arisen hitherto for discrimination between gliding and soaring. In glider flights the motive power is primarily gravity. Being made against a wind blowing up the hillside, they are assisted by a vertical component which naturally tends to prolong the glide. When the upward current is strong, the glider is raised even above its starting-point. True soaring flight, as witnessed in the soaring bird, is attained without the necessity of a windward slope.

By its nature soaring is a complex question. We shall presently examine more closely the explanations and meaning of this phenomenon. But even if the German results marked only a progress in

" gliding," they at least revealed to the eyes of the world a road towards the mastery of motorless flight.

* * * *

Sensational movements in anything new are seldom slow in securing fame. With the spread of popular interest comes the natural desire to outrival that which has already been done. The following year saw a general stir in the aeronautical camps of England and France. In 1922 these contests held in the public eye a central place in aviation.

The elaborate regulations of the Röhn competition testify to the importance attached to progressive development. Soarers are distinguished from gliders ; gliders are grouped into two sub-categories : those fitted with proper control surfaces, and those of the Lilienthal type controlled by movements of the operator. This division into classes of soarers and gliders does not appear to have been based on the scientific distinction considered above, but it serves to illustrate the serious regard that was given to the technicalities of these experiments.

A prize of 100,000 marks had been offered for the longest duration flight that complied with certain conditions. The competitor was required to fly

PLATE VII.

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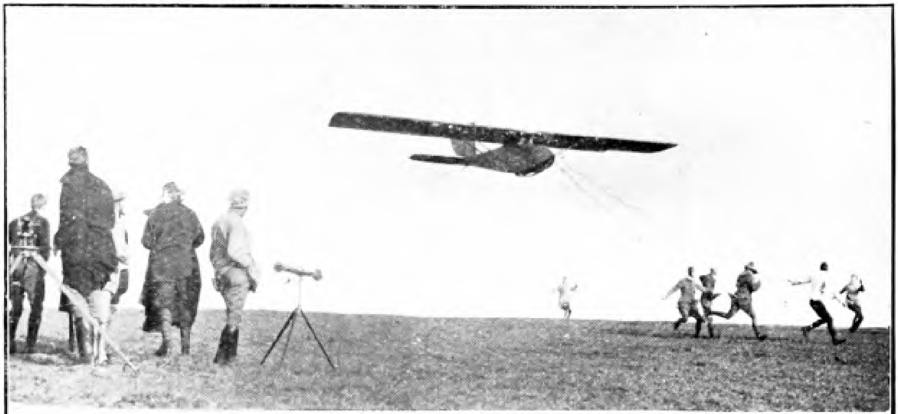

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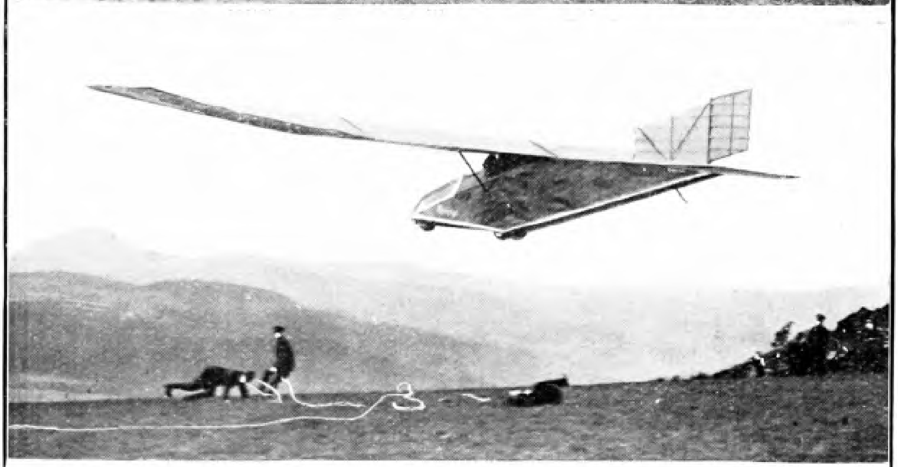

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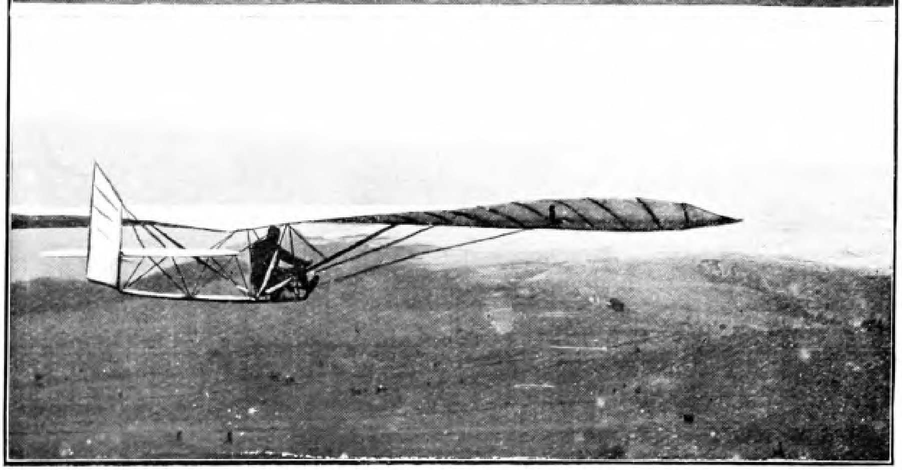


(*Courtesy* "*The Aeroplane.*")

1. The Darmstadt 1922 Glider Taking Off.
2. The Hannover "Vampyr" Flown by Martens and Hentzen.
3. The 1921 Harth-Messerschmitt Machine, with Powerful Warp Control.

for a minimum of forty minutes, returning to the region of his starting-point. Then, without descending, he must fly against the wind between two posts 100 metres apart, and proceed to cover a distance of five kilometres, measured in a straight line, before landing. When these conditions were first made known it was scarcely expected, even in Germany, that they would be fulfilled, yet within nine days of the opening of the competition Herr Martens qualified for the prize in a flight of one hour and six minutes. He crossed the starting line after having been in the air for forty-three minutes, and landed at a point ten kilometres beyond it. On the following day Herr Hentzen, formerly a pupil of Martens, surpassed this performance with a flight of just over two hours, likewise fulfilling the qualifying conditions. On August 24th Hentzen extended his record to over three hours. On this occasion he had started from below the top of the Wasserkuppe, landing eventually higher than he had started, right on the top.

The machine employed by both Martens and Hentzen was built by the Hanover Technical University, and was known as the Hanover "Vampyr" glider. The wings, of thick section, were of uniform chord for more than half the span, but tapered away towards the tips, which were made to " warp " for lateral control. It was the machine on which Martens had flown at the meeting of the previous year, with certain slight modifications.

Before the surprise of the latest achievements, enthusiasts in France had already determined to emulate the performances of their German neighbours. By the institution of the " Premier Congrès Expérimental d'Aviation sans Moteur," the science of gliding, neglected since the crude and, relatively speaking, abortive attempts in the early days of the Voisins, was now re-born in France. It was an international meeting—ex-enemy subjects alone being barred from competing. The site selected for the meeting was the Puy de Combergrasse, a hill of some 3,500 feet to the S.W. of Clermont-Ferrand (Auvergne). Unhappily, nothing of striking importance resulted. M. Boussotrot, on a Farman biplane, made the flight of the longest duration, remaining in the air for five minutes 18 seconds; but the records of the Rhön remained unsurpassed. This may or may not be attributed to inferior aircraft. It was argued that the topographical suitability of the Wasserkuppe was unique, but the later results of the meeting in England proved this contention entirely fallacious.

Following the efforts in France, came the welcome announcement of the lead to be given by the *Daily Mail* to supporters of the movement in England. A genuine encouragement, which had so long been needed, was at last forthcoming. After a lapse of thirteen years, experiments were renewed in the Downs of Sussex.

The topography of Itford was no less suitable than that of the Rhön. The ridge on the south side slopes to the sea. On its northerly face a series of re-entrants formed by erosion deflect the winds into powerful up-draughts, providing the medium in which to "sail." To those sufficiently interested in matters of gliding to penetrate into its earlier history, the events of this meeting must be perfectly familiar. The period available for preparation and practice was too limited to allow either pilot or designer to make any instructive experiment. The conditions that obtained were indeed propitious, but the results that were reached, exceeding the most sanguine expectation, were a very great tribute to all those concerned.

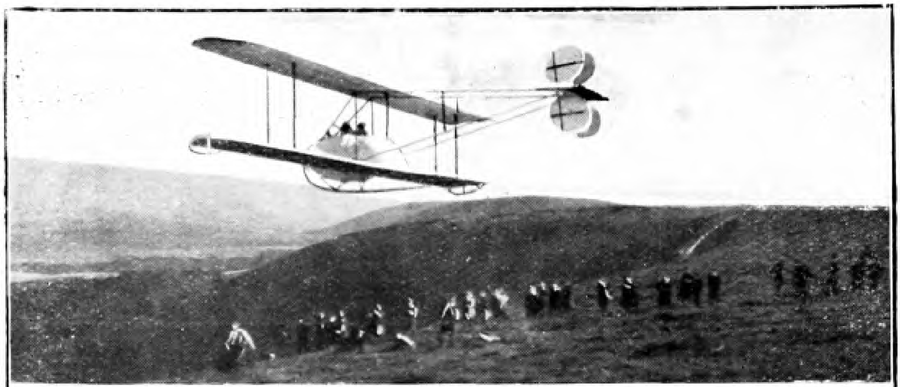
The standard of efficiency cannot be judged by endurance alone in "sitting" on a jet or chimney of air. Regard must be had for the determining factors of direction of wind, contour of ground and course pursued. Yet to set up a record on a first such attempt is at least a notable achievement. M. Maneyrol had never essayed prolonged gliding flight on the type of machine that he used; and he remained in the air for three hours and twenty-one minutes, only the darkness compelling descent.*

But the cream of the conquest at Itford is the historical romance that surrounds this winning machine. Expert and experienced judges were agreed that its chances were small. To these and to all it provided material for thoughtful reflection, and earned for itself a proportionate respect.

The Peyret monoplane defied all convention. It is a pure-bred Langley tandem-monoplane, and its designer appears to inherit his ideas directly from Professor Langley. The evolution of this glider can be traced from the time when Peyret's models of similar pattern competed with the bird-like models of José Weiss at the first competition at Paris in 1905.

*M. Maneyrol, on the same machine, has since succeeded in remaining in the air for over eight hours at Vauville, near Cherbourg

PLATE VIII.



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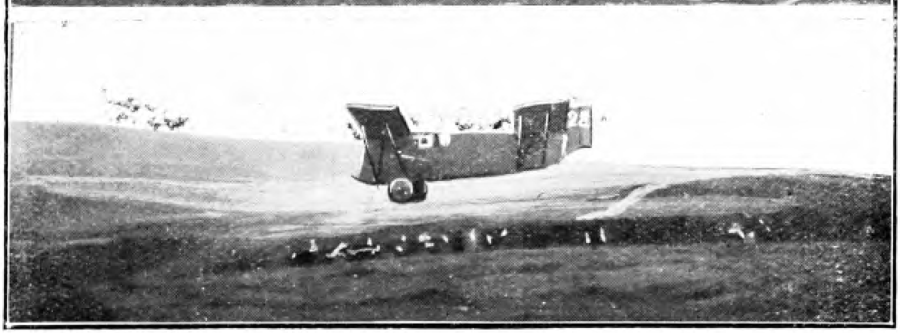

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(*Courtesy "The Aeroplane."*)

1. Mr. Fokker with Passenger Leaving Firle Beacon at the Itford Meeting, 1922.
2. Mr. Raynham on the Handasyde Glider just after Starting.
3. M. Maneyrol, the Winning Competitor, on the Peyret Machine just Lifting from the Edge of the Beacon.

FROM the foregoing sketch of the recent advance, one is led to enquire into the position we hold in relation to the real solution of the problem of human soaring flight. It would seem that under a set of given conditions, the limit to the duration of these flights will resolve itself purely into a question of physical endurance. These conditions are each indispensable, and simple as they may appear when examined individually, it has taken over a quarter of a century to arrive at a method of bringing each into line and all into harmony.

The requirement of wind in an upward trend was always appreciated, or at any rate promptly discovered by every experimenter. It is not yet established what angle of slope and speed of wind make for the best combination. A gentle slope, for example, will give a greater depth of rising current than the vertical face of a cliff. On the other hand, the consequent difference in the angle of the current might well be found less favourable.

Again, while one angle of slope might be preferred in a wind of a certain strength, another might be better in a wind of a greater or lesser strength. But whatever the favoured disposition may be, the presence of a wind with a vertical component has so far been the controlling factor of a gliding performance.

The second essential is a machine of ample aerodynamic efficiency, its particular features being those of low resistance and high manoeuvring qualities.

A third condition is that the piloting should be sufficiently skilful to utilise the energy supplied by the wind. That a certain measure of skill is required is obvious enough. Wilbur Wright, in relating his experiences of 1902, maintained, even at that time, that "it would be easy to soar in front of any kind of hill of suitable slope whenever the wind blew with sufficient force to furnish support, provided the wind were steady. But, by reason of changes in wind velocity, there is more support at times than is needed, while at others there is too little, so that a considerable degree of skill, experience and sound judgment are required, in order to keep the machine exactly in the rising current." The flights of Martens and Hentzen in Germany had led many to believe that only a peculiar type of uncanny skill could produce such amazing results. But the fact that at Itford the flights of longest duration were effected by pilots having little or no such experience, is sufficient to dispose of this faulty assumption.

Before proceeding, it would be well to examine with greater precision the meaning of the terms "soaring" and "gliding," and the relation they bear to each other. Until quite recently no cause has arisen for serious objection to the loose employment of the one for the other. In ordinary parlance, the only distinction made was in the practical difference that in one case the wind moved with an upward trend against a motionless surface, while in the other the surface moved with a downward trend against motionless air. If the rising current of air had a rate of ascent equal to the glider's relative rate of descent, the glider in consequence being sustained at a constant altitude, it was said to be "soaring."

In this sense "soaring" is a *fait accompli.* But such flight is identical aerodynamically and mechanically with ordinary gliding flight, with the sole difference that the wind has a vertical velocity, and to call it "soaring" is, to say the least, inaccurate.

The flight of Harth that has already been noticed, and of others *ejusdem generis*, were performed, in part at any rate, over relatively level ground where the intensity of up-currents encountered could not account for level flight. It was claimed that experts on suitable machines were able to make use of the internal energy of the gusts of a gusty wind to maintain them in horizontal flight, irrespective of the lie of the ground below. To this end a machine of the Aachen monoplane type was specially constructed. It was fitted with sprung wings designed to use automatically the said gust energy—and on this machine soaring flights of two and three minutes' duration have been made over the sea. Theoretically this is perfectly possible, even where the wind is quite horizontal, provided that the relative speeds of the wind and the glider do not remain uniform. It is admitted, however, that evidence of such flight is somewhat scanty, and much more research and experiment in this direction are needed before the art of "soaring" can be claimed as accomplished.

The argument as to the nature of "soaring" and "gliding" is forcibly put, and the distinction between them clearly defined, in a letter from Captain H. S. Wildeblood, a foremost authority on the soaring of birds, which letter appeared in *The Aeroplane* of November 2nd, 1921:—

"I have read the very interesting account in your issue of October 19th of the glider flights in the Rhön Mountains, and, while agreeing that they form a useful object-lesson and are distinctly encouraging, I am afraid I must demur to their being described as *soaring* flights in the sense I understand when speaking of true soaring birds. The German flights are merely glider flights taken downhill, chiefly against a wind blowing up the valleys or hillsides. In such cases the upward wind component naturally helps to prolong the glide, and may even raise the glider above its starting-point. With the true soaring bird, it is not necessary for him to walk to the top of a hill and to wait for an upward hillside current as the Rhön gliders had to do. He just hops off the ground on a level plain like that of the interior of India, when a wind is blowing, and either advances against the wind as far as the eye can follow, or rises in circles until out of sight, without another flap of the wings.

" When our gliders can do this we can safely talk about *soaring*. In the meantime I think it is advisable to refer to the German and similar gliding flights as *gliding*. To confuse the two will, I fear, tend to obscure the fact that we have yet much to learn, possibly an entirely new principle of flight, before we learn how to *soar* I agree with you that the best gliders yet constructed are not yet sufficiently well developed to be of any general practical utility. We must still seek the true explanation of soaring."

The term " soaring flight " must be understood to include the term " sailing flight." If there is a technical distinction, it is one which does not matter. Apparently a vulture " soars " and an albatross " sails."

* * * *

The soaring of birds, the question with which we are now directly concerned, is a question which has exercised the minds of a large number of keen observers. The bird that soars is found to " extract from the existing expansive forces of the air sufficient energy to give an upward thrust so far in excess of the downward vertical pull of gravity, that the resultant force necessary to maintain the initial air speed is in a direction above the horizontal, and this resultant force carries the bird forwards and upwards." The larger birds, though very much smaller than any human glider, are physically obliged to make use of the energy in the wind. It is known that the horse-power of an animal or machine varies as the square of its lineal dimensions, whereas the weight varies as the cube. The power per unit weight is relatively low in the larger birds, many of which would be incapable of prolonged flight if they were unable to capture their energy from the wind.

It is important to appreciate the difference between soaring in air and floating on water. The passive resistance of water permits weight to float on its surface. A bird or a glider cannot be said to float on the air. It must be travelling in relation to the air to obtain the support of the air. A ship is propelled through water in the line of its least resistance, the force exerted by the thrust of the propellor being greater than the resistance of the water to the vessel propelled. A bird when it flaps its wings, does not thrust itself forward by pushing the air in a direction opposite to that in which it desires to progress. The motion of the wings is not downwards and backwards, as might be considered correct for forward propulsion; it is invariably downwards and *forwards*. From the effects of this movement it derives its support. When there is wind, those effects are supplied without the cause. The energy required is captured from the wind. This active energy is capable of exerting considerable force when properly dealt with. A bird is so formed as to be able to utilise that energy and it knows instinctively how to do so. Hitherto aeroplanes have not been so formed, and the pilots that fly them have not that instinct.

As our history has shown, man-made models have been so formed, and by virtue of such form they have been able to soar in the manner of birds; but the variation of air-currents is, of course, such as to make it impossible for an entirely automatic structure to continue to soar indefinitely. There must be intelligence to provide for the regaining of the necessary speed for support, when it is temporarily lost by variations of wind. The intelligence required is inherent in a bird, and we say that the bird has instinct. The creature performs automatically the correct operation without disturbing its progress. Man is now constructing machines on which soaring is seemingly feasible, but he has still to learn instinctive control. Experience is rapidly showing, however, that this is no less possible of attainment than the reflex movements performed by one familiar with the control of an ordinary bicycle.

In practice, of course, winds frequently have an upward trend of sufficient magnitude to account for the soaring of birds. This is always in evidence in the vicinity of cliffs. The conditions essential for human gliding are not essential to the soaring bird. Yet the soaring bird is naturally disposed to seek the conditions which demand the minimum effort. Another example of apparent "soaring" is that of the sea-gull in the wake of a steamship. Here the hull of the boat forms a sufficient obstruction to deflect a horizontal wind into an obliquely upward direction.

In some cases the heat of the sun will warm the earth sufficiently to cause convection to take place in the air so as to give rise to vertical air currents on days when there is no perceptible breeze, and as it does not require a strong vertical component to support a soaring bird, such slight winds even as these may be sufficient to account for many of the remarkable instances of soaring in apparent calm, which have so often been put on record.

About this type of " soaring " there is less to be said. For the purpose of clarifying the question at issue, we have elected to refer to it as " gliding." It admits of a mechanical explanation over which there is no dispute. It is with the " soaring " by means of the energy supplied by a wind of horizontal components that we are mainly concerned.

Mr. Lanchester, in Appendix *V.* of his *Aerodynamics*, says that :—

" Authorities are generally agreed at the present time that one at least of the varieties of soaring practised by the larger birds involves the abstraction of energy from the wind fluctuation, that is to say, the soaring bird can derive the power required for its flight from the energy of turbulence of the wind.

" It is clear that the bird having no horizontal force applied to it *from without* (in contradistinction to a kite which is connected to the earth by a string) is unable to effect any change in the total (horizontal) momentum of the air that comes within its grasp ;

consequently it cannot raise or lower the mean velocity of the wind, although it may be able to cause some parts to move faster and some more slowly.

" It is evident that if a bird can, by altering its angle and altitude, so manipulate the wind coming within its grasp that the portions that are moving in excess of the mean velocity have their velocity reduced, and those that are moving at less than the mean velocity are accelerated, the total energy of the wind will be reduced, and the energy taken from the wind may be available for the purpose of propulsion."

Ever since Mouillard's diligent search for the explanation of soaring, observers from all different quarters have sought to throw light on this abstruse phenomenon. All are agreed on the self-evident fact that energy is drawn from an external source. But on the nature of this source and the method of its capture, they differ—and sometimes widely. Mr. Lanchester suggests that authorities agree on the abstraction of energy from wind fluctuation. Though this may seem incontrovertibly true, it must not be forgotten that others have held on very good grounds that the source of this energy is much more obscure.

Dr. E. H. Hankin is in the front rank of those who maintain that soaring is accomplished by indiscernable means. This school is supported by the somewhat similar theories of Ross and Drury. The theory of Delbruck "that the larger birds may have the faculty of generating at the expense of intra-atomic energy a force capable of striving against gravitation until it renders it null," is one which we cannot discuss. The theories evolved by Mr. Albert Ross more than twenty years past are embodied in those of Dr. Hankin, and do not call for particular notice.

Dr. Hankin's painstaking labours have enabled him to put on record in his *Animal Flight* the precise movements of soaring birds. From his peculiarly patient and minute observation he has been led to believe that the sun stores up energy in the air. The energised air, called "Ergaer," then has the property of "soarability." "Ergaer" "is a state of the atmosphere in which energy from the sun's rays becomes locked up in the molecular structure of the air, to be released by the passage of the bird's wing." It operates in a manner "something in the nature of chemical disintegration resulting in a continuous series of minute explosions." Dr. Hankin does not claim to have discovered the "Ergaer" itself, or all of its properties. But he claims to have discovered the name and the notion, and puts forward a large body of evidence and elaborate arguments in support of his contention.

These arguments, being rather involved, may be best understood by turning to the words of Dr. Hankin himself in a discussion that took place at the Royal Aeronautical Society in 1912 :—

"Looked at from the broadest standpoint, there are only two possible theories of soaring flight. One that we are dealing with a manifestation of kinetic energy, the other that we are dealing with a manifestation of potential energy. The fact that air, in some cases, conducts sound unusually well when it has become unsoarable after a storm, is a fact that obviously supports the idea that we are dealing with kinetic energy. Why has this fact not been quoted by opponents of 'Ergaer?'

"The fact that the soaring of hill crows only occurs in the absence of wind, puts out of court much of the references to the influence of wind that occur so frequently in the ideal soarability of theorists. Admitting that a feather might drop through the air in a wind having an upward component of 2 or 3 feet per second, the kind of soaring flight that would be thereby explained is of the ideal variety, having little relation to actual facts. The bent-up position of the wing-tip feathers in fast flex-gliding could certainly not be explained by an upward component unless this amounted to a speed of some thirty miles an hour. Therefore, if soaring flight is due to kinetic energy, the movement must be of a kind that has nothing to do with wind, that has no visible effect on small cloud-masses and leaves a piece of thistledown apparently unaffected.

"In the change from slow to fast flex-gliding, two different adjustments may be used. If the adjustment is an increase of flexing, the increase of speed goes on during several seconds. If there is also a double dip, the increase of speed may be as much as ten metres per second, and this increase appears to occur almost momentarily. During the double dip there is a transverse axis rotation. But a dive downwards, such as would be necessary to explain the increase of speed on the kinetic energy theory, does not occur. Apparently the double dip causes an increased air disturbance that initiates the increased rate of decomposition of ergaer, as must occur in fast flex-gliding. There are reasons for believing that lateral instability is due to irregular distribution of ergaer, and that transverse axis instability is due to variations in the quality of ergaer, that is to say, of its liability to decompose. The kinetic theory cannot explain the observed facts connected with this latter form of instability.

"At the time of the approach of the monsoon season of 1911, I occasionally observed local and temporary soarability that occurred an hour or more before the time of development of sun soarability. I noticed firstly that this local instability only existed in and during a puff of wind, and, secondly, that especially at its commencement, it was characterised by a great degree of transverse axis instability. I regard this as a proof that in the puff of wind ergaer was changing from a stable condition in which it was unavailable for soaring flight, to a condition in which it was available, and perhaps also that it was completely decomposing, thus causing the wind-puff. One of the differences between 'sun soarability' and 'wind soarability' is that sun soarability usually commences over the houses of the city of Agra a few minutes before it commences over the trees and gardens of Agra Cantonment. Wind soarability, on the other hand, commences anywhere, and only in the presence of wind. 'Disturbed weather' and 'storm soarability' are only special cases of 'wind soarability.'

"When a dust-storm has developed, two winds exist; firstly, the 'attraction wind,' which blows from all quarters towards the storm, and, secondly, the 'displacement wind.' The attraction wind blows with an upward trend, and on nearing the storm, forms an upward current. In the centre of this upward current, a descending current was formed. This, when it reached the earth, spread out in all directions, but especially to leeward, forming the displacement wind. Apparently, the same air goes up in the attraction wind, turns round and comes down again in the displacement wind. The attraction wind, despite the upward trend, lost soarability as it approached the storm, at any rate in the absence of sunshine. The displacement wind, despite its downward trend, was usually highly soarable, and birds in this wind not infrequently showed very great instability, especially instability round the transverse axis. If my views of the nature of a dust storm are correct, we have a case in which soarability decreases and then increases in the same air, a fact very difficult to reconcile with the kinetic energy theory . . .

" . . . The question has been asked why, if ergaer explodes on the under-side of the wind, lifting the bird, why does it not also explode on the upper side of the wind, driving the bird downwards? in the first place, I may point out that the answer to this question does not bear directly on the question now before us, namely, whether the conception of ergaer is one that is true or probable. There are only two possibilities. Soarability is due either to kinetic or potential energy. If the kinetic energy theory is knocked to pieces, only the potential energy theory remains. I have, as I think, done more in that I have given reason for believing that energy is stored in the air. As to how this energy is stored, and as to how it can become available, we must look to future research for an answer. It is a fact that the air under the wing is more compressed than the air over the wing, and that explosive gases are more readily exploded when compressed. But whether or not this has a bearing on the question propounded must be a matter for future research."*

To this statement of his case by Dr. Hankin, the following reply was delivered by José Weiss :—

" From occasional extraordinary results with my gliders, I have come to the conclusion that the difference between gliding and soaring is only a matter of degree, not of principle. I feel that most people, through insufficient investigation, fail to realise how slight a gliding angle may be made; therefore they seek in far-fetched theories the explanation of results that are due only to very great efficiency. The misconception is not as to the fact that the bird obtains energy from the air, but as to the amount that it requires in order to maintain its soaring flight.

" The glider and the soaring bird are both falling bodies following the paths of least resistance. A glider having an infinitely small drift, must follow

* While still maintaining that there is overwhelming evidence that soaring is due to some force quite other than that recognised by aerodynamic experts, and that it is even performed by certain birds in descending currents when the air is " soarable," Dr. Hankin appears to have abandoned the " Ergaer " hypothesis. He prefers to say that he has no theory of any kind ; that the whole affair is so far inexplicable ; and that it must rest with experimenters to solve the problem.

a path very nearly horizontal once it has acquired its natural velocity. A bird approximates very closely to a theoretical glider of this kind, and the energy required to change gliding into soaring is so small that I see no difficulty in accounting for it on the theory of wind. The least undulation in the atmosphere would be quite sufficient, and it is not surprising that soaring birds abound in more tropical climates, where the sun which is the principal agency giving rise to suitable movements of the atmosphere, is so much in evidence. It seems to me that the phenomenon of soaring emphasises the necessity of investigating in the minutest possible detail the causes of high efficiency in bird flight, such as the motion of the air in the vicinity of the wing, and the nature of the disturbance set up by the passage of the bird through the atmosphere . ."

As helpful to the question of soaring by man are the observations of Mr. A. C. Baines (of New Zealand) on the "sailing flight" of the albatross. The following is extracted from a letter to *Nature* in 1889, and often recited before :—

"I will first give a description of the flight of these birds . . . and then attempt an explanation.

The sailing flight is never to my knowledge done in a calm. I once observed the effect of a gradually diminishing wind on their flight. The steamer was going about nine knots. When the wind, which was very nearly aft, became one or two knots slower than the steamer, the birds which had hitherto kept their wings perfectly steady, began to flap at intervals, which became shorter as the wind lessened, and when it ceased they flapped almost without intermission, and soon ceased to follow the vessel.

"The birds go through a series of movements which are related to the direction of the wind. Starting from near the surface, they rise in a slanting direction against the wind to a height which varies with the strength and direction of the wind. The average seems to me about 20 feet. Then comes immediately a turn half round in a rather large circle, followed at once by a rapid descent down the wind. They then take a longer or shorter flight in various directions, almost touching the water. After that another ascent in the same manner, and so on, repeating the series of movements *ad libitum*.

"The interval of time between the ascents evidently depends on the direction of the wind with regard to the course of the vessel. When the wind is ahead, and the birds' velocity through the air great, being necessarily greater than the wind's velocity plus that of the steamer, the interval is short. When the wind is abaft the beam, and the birds' velocity much less, the interval is usually much longer.

"As the bird rises he enters currents of wind which increase in velocity with the height in a direction contrary to his own motion, so that the loss of velocity consequent on rising, and which would take place in still air, is partly—or perhaps, when the wind is strong, wholly—made good. The bird thus gains energy of position, which is converted into energy of motion by descending.

"A bird's ascent against the wind may be compared with the ascent of a particle up an incline, while the incline itself is accelerated in a horizontal direction opposite to that of the particle's motion, thereby enabling it to reach a greater height than that due to the initial velocity. The albatross does not go on rising until his velocity is nearly exhausted, but makes a half turn at great speed previous to his descent.

"I have sometimes seen a number of albatrosses sailing in a peculiar manner, the wind being at right angles to the course of the steamer. They ascend against and descend with the wind, turning alternately right and left, so as to describe an undulating line, not far behind the stern. A number of these sometimes do this for hours."

* * * *

Stress has been laid in the foregoing chapters on the importance attached by early designers to obtaining a form that would give to their gliders a maximum of natural stability. Let us briefly examine the cause.

Lack of stability entails loss of energy. The sideslip involved in the correction of a roll may be negligible on a power-driven machine. But on one which depends on the gusts of the wind for its motive power, the highest efficiency is required. The greater the stability inherent in a glider, the more promptly it is able to overcome disturbances, and the more rapidly disturbances are overcome, the less is the space that is needed for recovery.

A glider which is inherently stable is independent of the pilot's control for its security of balance. In the days when controls were often defective, such security of balance was highly desirable.

Defective controls are rare enough now, but one may rightly suppose that it is no disadvantage to be relieved of the task of maintaining one's balance.

Records show that Sir George Cayley had a practical knowledge of the features primarily necessary for stability, although he does not appear to have published any mathematical investigations on the subject. Exploration in the field of stability has since been tackled by many pioneers. While Professor G. Bryan was producing his *Stability in Aviation* and establishing its laws in a masterly mathematical method, experimenters were learning the lessons of their experiments.

The problem of stability, involving as it does three rotational and three translational motions is exceedingly complex, and does not lend itself to simple explanation. It is, moreover, a technical matter which does not fall within our immediate province. Its question is raised merely because it has always been intimately and inseparably associated with gliding experiments. And there is still this practical point: The uninitiated exponent of soaring flight requires to be freed from preoccupation, in order to give his undivided attention to the best way of handling the energy of the gusts. The higher the factor of natural stability, the greater his freedom from correcting control, and the more his ability to make use of those gusts.

But whatever the qualities of the glider may be, the task of the operator remains to be learnt. The peculiar faculties of a soaring bird cannot be assimilated without much perseverance. German experience has shown that success is in the first place a matter of piloting, and depends only in the second degree on the suitability of the machine employed. Faulty piloting, even with the best machines and under the best conditions, has never produced effective soaring.

Variations in wind speed, as already explained, represent, in the form of gusts, energy available for soaring. By manoeuvring in such a manner as to reduce the variation of the wind in relation to the machine, so rendering the relative wind speed more nearly constant, internal energy is set free from the wind to the use of the pilot. The theory of pilotage, as evolved by Harth and those of his school, has been expounded in the following form :—

"One must so pilot in a wind of great gustiness that that gustiness be reduced as much as possible, and that the energy thus liberated is used to do the work necessary for flight. This still seems quite simple. The ideal of pilotage is to so manoeuvre that there will be as nearly as possible a constant wind speed behind the machine.

"The difficulties of the actual piloting are due to the fact that one does not know beforehand the structure of the wind, the form, periodicity and duration of the gusts. The most desirable type of gust is that which has a long period of increasing speed, followed by one of constant maximum speed, a further period of decreasing speed, and a final period of constant minimum. It is necessary that the difference between maximum and minimum speed be as great as possible.

"When one has such a wind available, piloting a soaring machine is not too difficult. During the first period (increasing wind) one will fly head to wind, and should steadily gain height during his manoeuvre. At the second period of maximum and constant wind, one must turn at right angles to the wind. Generally it will be impossible to avoid some loss of height in this period. At the beginning of the third period, the machine must be turned tail to the wind. As the wind is now diminishing in velocity from behind the machine, the speed of the machine through the air is effectively increasing, and it is again possible to gain height. During the fourth period of constant minimum wind one must again turn at right angles to the wind.

"By flying in this manner one diminishes the differences of wind-speed and utilises the energy of the gusts. This is the essence of piloting a soaring machine.

"Therefore it is necessary to know the structure of the wind, to recognise in advance the nature of the gusts, in fact, to see the gusts before they reach one; and the best preparation for such piloting is the study of the structure of the wind and of gusts."

As knowledge increases it will be possible to predict by close inspection of a contour map the conditions prevailing over the different localities of a given stretch of land. The measure of accuracy will of course depend not only on the information available as to wind directions and other controlling factors, but on the correctness of inference that is drawn from the whole. Duration records on cruises of this kind will be determined by the extent of the knowledge, and the judgment and skill of the pilot. They will be *more* than a test of physical endurance.

It must also be realised that the amount of energy a free glider can draw from the wind under any but the most abnormal conditions, is relatively small, and a machine that soars must be one of peculiar efficiency in its form and construction. But apart from questions of improvement in design—a matter to be dealt with hereafter—these experiments provide a direct incentive to the advancement of knowledge in a field of research so little explored, and of so much importance to the true development of human flight.

THE TECHNICAL ASPECTS OF GLIDING EXPERIMENTS

By W. H. Sayers

In the previous chapters of this book the author has very justly pointed out that the first practical power-driven aeroplane was developed as the result of experimental work carried out by the aid of gliders.

The Wright Brothers by the use of their gliders solved the elementary problems of control and learnt the art of using the control system which they had devised. With the aid of the knowledge which they so acquired they were able straight away to produce a practicable engine-driven aeroplane.

It is worthy of note that in the whole course of their experiments the Wrights made no serious changes in the general form of their machine—their earliest successful glider had precisely the same general arrangement of surfaces as the final Wright engine-driven machine.

Since the Wright machine first proved that power flight was a practicable proposition the general form of aircraft has undergone a very considerable modification, and experience has proved very conclusively that quite apart from details of design the general form of an aeroplane is a matter of great importance. Thus there is not the slightest doubt that the modern standard type of tractor fuselage biplane is a much more efficient aeroplane than the original Wright type of twin propellor machine with front elevator, and that the difference in efficiency is one due largely to the general arrangement of the modern type, and not merely to improved structural design, or to more efficiently shaped wings or other matters of detail.

In the interval between the Wright's original success and the practical standardisation of the present tractor biplane type, many varied general arrangements of the essential components of an aeroplane have been tried. Practically without exception all such experimental machines have been aeroplanes—that is to say power-driven machines. Fourteen years of experience have fairly satisfactorily proved that any serious alteration in the general arrangement of the components of a known type of aeroplane may lead to quite unexpected and unforeseeable results, and that very particularly difficulties in regard to control and to stability are likely to occur.

Quite a large number of experimental types of aeroplanes, many of them possessing highly promising features, have been built and abandoned in the past few years simply and solely because of such difficulties of control and stability which have manifested themselves in early specimens of their type—before the trials had progressed sufficiently far to demonstrate whether or not the type could in fact realise the hopes of its originator.

Now in so far as concerns stability and controllability, the history of the Wright Brothers is ample evidence that gliding is capable of giving very reliable evidence as to the characteristics of a given type of aeroplane. With modern instruments it can also be used to give a very fair indication of the approximate efficiency of the type.

And a glider can be built for at most one quarter of the cost of an engine-driven machine. Over and above this advantage is the fact that the glider, owing to its much lighter loading and consequently slower speed, is much less dangerous if it does prove to be unstable or uncontrollable and the risk both of wrecking the whole machine and of damaging the pilot under such circumstances can be made extremely small by careful methods of design and of experiment.

It is certainly no exaggeration to say that had the designers of new types of aircraft always followed the Wright's original method of testing their machines as gliders first, many valuable lives and much money would have been saved, and the art of aircraft design would have advanced at a considerably more rapid rate.

There is ample evidence that there is still much to learn as to the most efficient general form for an aeroplane—and therefore the case for the use of the glider as an experimental aid to the design of power-driven aeroplanes is as strong to-day as it has ever been.

In so far as the pursuit of gliding as an end in itself is concerned, the case is less clear. The evidence of the past two years—that it is possible on man-made gliders to do what five years ago would have seemed impossible—should however suffice to convince all but the most sceptical of the importance of further research into the subject, We do not yet really begin to know anything about gliding and soaring, and that in itself is sufficient reason to attempt to extend our knowledge.

In this connection the author of this book has attempted to establish a difference between gliding and soaring, and he appears to agree with those who would class as gliding pure and simple flights made with the aid of winds deflected upwards by a hill-side. Within limits one may agree with him, but it is well to point out that short of a complete overthrow of all our present theories on the subject of dynamic flight, all heavier-than-air machines are gliders, and that all gliding requires the expenditure of power.

In " pure " gliding the necessary power is supplied by gravity and gravity alone and the most elementary knowledge of mechanics indicates that in such flight the machine must descend in order that gravity may do the necessary work. It follows that if the machine does not descend the necessary power is supplied by some agency other than gravity. In the case of the engine-driven aeroplane the source of power is obvious. In the case of flight in a rising wind caused by the slope of a hill it is equally obvious.

In the effortless flight of some soaring birds over flat ground, or the sea, the source of power is not so obvious, but it would be extremely unwise therefore to assume that the nature of the flight itself is in any radical degree different from that of the more easily understood types. All the positive evidence which is available seems to indicate that the qualities necessary to an efficient "soaring" or "sailing" aircraft are precisely those necessary for efficient "gliding," and that evidence is very much more important that any argument based on our inability to give an adequate and complete explanation of certain types of bird flight.

It may fairly safely be asserted that our inability to account for all the phenomena of bird flight is to be ascribed to our ignorance of the structure of the wind in which the flights are performed.

The principles of mechanics admit that a glider may be able to obtain power from a wind which is not of uniform velocity—and winds are practically invariably gusty to some extent.

In a horizontal wind of alternately increasing and decreasing strength a glider can certainly take power from the wind by flying into the wind while the strength is increasing and turning round and flying with it while its strength is decreasing. Certain successes appear to have been attained with this method of flight in Germany, but human pilots lack the necessary sense which may tell the bird the precise moment at which he should turn. Very certainly this method of flight is one practised by birds in some circumstances but it is—in essence—pure gliding.

Modern mathematical research in aerodynamics has revealed the fact that a gusty wind cannot possibly be a purely horizontal wind, but must alternately have an upward and a downward component. And further calculations have shown that in a wind having this characteristic it is possible for the average resultant force on an efficient wing to have a forward component—that is for it to tend to move the wing forward into the wind.

This is really only a simple application of the ordinary theory of the relativity of motion—in effect it means that if the wind moves up and down alternately against a wing which is fixed, the effect is the same as if the wing flapped up and down and the wind maintained a constant direction. Model tests in the wind tunnel confirm the calculations, and efforts have already been made to practise flight according to this method with some indications of success. The actual flight is still gliding pure and simple, and the difference in the flight path is one due to the particular type of movement of the wind—just as in the case of flight in a steady up-current.

From this it will be seen that there are various conditions under which it is possible to obtain from the air itself power in a form applicable to the needs of engineless flight. Under certain conditions this power can be utilized to keep a man-carrying glider in the air to-day, and with every increase in the efficiency of the glider the extent to which this is possible will be increased.

What the ultimate possibilities are we do not yet know but we are certainly far from having realised them. The engineless aeroplane drawing its whole power from the energy of the wind may become a practical commercial vehicle—though that seems scarcely likely except possibly in certain tropical regions where vertical convection currents, due to solar radiation superheating the surface of the earth, may be relied upon regularly during the day time.

But since all heavier-than-air flying is in essence gliding, the search for efficiency which is inherent in the development of gliding must necessarily help us to attain efficiency in the design of the engined aeroplane.

To those—and they are many—who desire to know what ultimate useful result is to be obtained by gliding experiments the answer is simple. It is that we as yet know very little indeed about the possibilities of the heavier-than-air type of aircraft, and that gliding is a relatively safe and relatively cheap method of adding to our knowledge.

Introduction to "Notes on Giant Aeroplanes" by JOSÉ WIESS *and* ALEXANDER KEITH, 1916.

A FEW PRELIMINARY CONSIDERATIONS.

FLIGHT as performed by aeroplanes is gliding flight in which the thrust from the propeller supplements the weight, and, at or above the horizontal, substitutes itself to the weight to overcome the head resistance. We do not think that this general statement can be challenged or even qualified. Therefore, in order to form a correct idea of what can be achieved by a machine larger than anything in existence, *we must know first of all how much of the performance, both as to speed and as to lift, will be supplied automatically and without expenditure of power by normal gliding,* and experience shows that the greater the size the more does the gliding contribute to the performance. For instance, a machine having 25 m^2 of surface glides quite well with a load of 20 kgrs. per m^2, whilst a reduced model of the same having only 1 m^2 and weighing 20 kgrs. falls almost like a stone.

If we know what speed and what lift we can get from normal gliding, there is no difficulty in computing the performance of any machine, and for our present purpose we will express the lift in terms of the H.P., just sufficient to keep the trajectory horizontal at gliding speed. We shall know then how much of the available H.P. is left either for accelerating or for climbing.

Now, any apparatus which glides is a falling body, which, like all falling bodies, follows the path of least resistance, and in order to glide at all, it is a question *sine qua non* that the apparatus should be so balanced by its centre of gravity and the disposition of its planes that it is free to assume and to retain the position, in relation to the trajectory, in which it offers edgewise the minimum of resistance. The moment that condition is not present the stability is destroyed or at least impaired. Whether the speed-giving factor be wholly or partially the weight, as when the apparatus is on a downward course, or whether artificial power is substituted to the weight as a speed-giving factor, as when the horizontal is reached or passed, the necessity of the above condition is a permanent one for normal and perfect flying. A well-designed and well-balanced machine finds that position of minimum head resistance automatically, and that is why so many machines in which the wings are set at an angle with the axis of the fuselage, fly tail up, a condition which, by the way, adds to the head resistance. It is not defined, however, which position of a cambered surface offers edgewise the minimum of resistance, and it is by no means necessarily when the chord is parallel to the trajectory. The depth and shape of the camber, the thickness of the wing, the flexibility of the trailing edge, a certain warp in the wing are some of the many factors bearing on this point. No doubt the slight longitudinal dihedral between main plane and elevator, which contributes to automatic longitudinal stability, gives to the main plane a certain amount of incidence on the trajectory, but that incidence is really a minor factor of lift, and in a perfect machine, as in birds, need not exist at all. The Wright Brothers' machine, for instance, flies with its chord at a negative incidence. By a displacement of the centre of gravity rearward—or the raising of the elevator, which, up to a point, has much the same effect by causing downward pressure on the rear—the longitudinal position of the machine is altered only in relation to the horizontal, not to the trajectory, and if the power is then sufficient to keep up the speed in the new direction given, the machine rises normally, without the incidence on the trajectory being changed. Whatever the direction of the trajectory, the machine must retain the position of least resistance, if the stability is to be perfect. A skilful pilot might, of course, fly his machine more or less *cabré* fashion, if he has sufficient margin of power, but this is at all times abnormal flying, because it is not normal gliding, and is always liable to cause a sudden dive or tail slip, unless there is enough spare power to partially helicopter the machine.

Looking at the problem in this light and discarding the contradictory notion that the lift is obtained by driving the planes at a given incidence to the trajectory, when there is no external fulcrum to hold them at that incidence and to prevent the system of planes from finding for itself the position of least resistance, just as does a weathercock, we are led to the conclusion that gliding is in reality a form of imperfect perpetual movement, that is to say that there is in a glider, just as there is in a pendulum, a partial restitution of power, which restitution, if complete instead of partial, would constitute perpetual movement. Our present business is to define the laws which govern that partial restitution in speed and in lift.

We know that a cambered surface when travelling edgewise is powerfully drawn in the direction of its convexity—whatever the explanation of this phenomenon may be—so that the only thing which bars perpetual movement in the horizontal direction is the head resistance, because if there was no head resistance the velocity must increase as long as the trajectory is downwards, and therefore the velocity at which the lift becomes equal to the weight must be reached, when the trajectory must of course become and remain horizontal. The passage of a body through a fluid does not, in theory, offer any resistance other than skin friction from viscosity, if its shape is such that it coincides exactly with the graph—parabolic, by the way, which explains the parabolic forms of birds—of the normal period of compression and expansion of the medium, the effort expended in opening the medium being entirely recouped by the pressure exerted on the rear of the body by the closing medium. This case would be characterised by the total absence of eddies, and there is pretty conclusive evidence, which, however, is beyond the scope of this paper, that the passage of sailing birds through the air leaves the medium behind them quite undisturbed. If some of these birds were endowed with reason and were willing to show off to us what they are capable of, there is every reason to believe that some of them, both land and sea birds, would be able to glide in calm air at an angle of perhaps one in a hundred and certainly one in fifty. That is to say that barring the slight skin friction which must always make absolute perpetual movement impossible, they are practically performing it.

If then we attempt to calculate the power required for aeroplane flight by incidence and weight, we make the same mistake as if we were to calculate the power required to keep a pendulum in motion, from the weight of the pendulum, whereas in both cases the only factor we have to deal with is frictional loss, the weight coming in only in so far as it may affect frictional loss. This, of course, in the case of the aeroplane is true only up to the horizontal, the climbing remaining a pure question of foot pounds. A pendulum weighing, say, one ton and having a length of, say, 4 metres, so as to beat the alternate second, could quite well, if carefully made, be kept in motion by means of a clockwork driven by a weight of 8 or 10 kgrs. falling one metre in 24 hours, which represents net not more than one-millionth of 1 H.P., whereas to accelerate and retard a mass of 1 ton 30 times a minute might take several H.P. according to amplitude of beat. The practical difference between a pendulum and an aeroplane is that it is much simpler to construct a pendulum than an aeroplane approaching theoretical perfection, although Nature has evolved something very near perfection in the larger sailing birds.

It is precisely because, in the normal gliding on which rests all aeroplane flight, gravity gives us gratis nearly nine-tenths of the horizontal performance that we have succeeded in mastering mechanical flight which otherwise seems to be inherently impossible, as was held until a few years ago by some eminent mathematicians who were working on the wrong laws. Until we are definitely certain that we are introducing correctly each and all of the laws and factors coming into play, empirical co-efficients based on practice are much safer than theoretical equations. The force of this point will be shown by the facts and the examples hereafter, and particularly by Facts 3 and 6.

FACTS ABOUT GLIDING.

FROM EXPERIMENTAL RESULTS.

Signs Used.

P = Gross weight in kilogrammes.

S = Surface of main plane or planes across the fuselage, in m^2, *viz.*: span × width.

$S^{1\cdot 33}$ = Is the same as $S \times \sqrt[3]{S}$. The power 1·33 must be obtained in the case of biplanes or multiplanes on each plane separately. Thus $24^{1\cdot 33} = 69\cdot 1$ but $2 \times 12^{1\cdot 33} = 54\cdot 8$ or $10^{1\cdot 33} + 14^{1\cdot 33} = 55\cdot 2$.

Δ = Is the ratio $\frac{P}{S^{1\cdot 32}}$.

G.V. = Gliding speed along trajectory when gliding at least gliding angle. It is also the climbing speed when climbing at maximum rate.

N.H.P. = Nominal H.P.

E.H.P. = Effective H.P. or actual work done in kilogrammetres, all loss deducted.

H.H.P. = E.H.P. just sufficient for horizontal flight at gliding speed.

FACT 1. Assuming an apparatus to be well designed and correctly balanced, the principal factor of the gliding performance is by far the load. Not however, as we shall see, the load per unit of surface expressed by $\frac{P}{S}$, *but the load which we call the* $\triangle$ *load, the one expressed by* $\frac{P}{S^{1\cdot 33}}$.

FACT 2. Two apparatuses of exactly the same pattern but of widely different sizes, and loaded in direct ratio to their surfaces, *viz.*, having the same $\frac{P}{S}$, *cannot be made to glide at the same speed,* and will not glide without undulations with identical balance, that is, with their respective centres of gravity placed at relatively the same point. This latter point is in itself a proof of the different speeds.

FACT 3. To make these two apparatuses glide at the same speed and under identical balance, *it is essential that their respective weights should follow* $S^{1\cdot 33}$ *viz., they must have the same* $\triangle$ *load.* About this fact there is not a shadow of doubt. We have verified it again and again. For instance, two models made exactly alike, but one with 3-ft. span weighing 1 lb., and the other with 12-ft. span weighing $1 \times 16 \times \sqrt[3]{16} = 40$ lb., when launched together, *glide exactly abreast of one another,* and nothing else than this particular relation of weight will make them do so. Probably no man on earth, beyond ourselves, has ever made that simple but conclusive experiment, and the same law is clearly observable in full-size machines. It will also be noted that right through Nature we find that birds, when comparable in their structure and style of flight, are loaded on the same basis. This was pointed out by the late A. Goupil in 1904, and here are 4 typical examples measured by ourselves :

		S (m^2)	P (kgrs.)	$\frac{P}{S}$	Δ
Sea birds	Tern	0·038	0·113	2·93	8·7
	Albatross	0·924	7·720	8·35	8·6
Land birds	Jackdaw	0·131	0·491	3·70	7·3
	Vulture	1·040	7·940	7·63	7·5

Now this fact is in contradiction with the classical formula of air resistance, $R = KSv^2$, which without any doubt applies correctly to all forms of air resistance. But a suction is not the same as a resistance, and we know that in aeroplane flight by far the greater part of the lift arises not from pressure but from suction. At any rate it is very clear that in gliding and in aeroplane flight, at equal speed, the lift follows not S but $S^{1 \cdot 33}$. Whatever the reason of this may be and whether or not it is due to the suction produced by the tangential current, the exact origin and properties of which are still very obscure, we cannot say, but it certainly seems illogical to accept that the lift should be proportional to surface, because it is impossible to conceive any reaction except from the displacement, or inertia, or rarefaction of a given volume of air, and a volume has of necessity three dimensions, whereas a surface has only two. The third dimension appears to be the cube root of the product of the two others, which product is the surface, so that the volume of air corresponding to the reaction is expressed by $S \times \sqrt[3]{S}$ which is the same as $S^{1 \cdot 33}$. This explanation of the phenomenon seems also to be borne out by the fact that in the calculation of a biplane or multiplane, the actual performance will tally with the formulas hereafter only if the power 1.33 is taken not on the total surface but on the surface of the planes taken separately.

From all this it becomes very evident that the only load which has any significance when enlarging or reducing the scale of an existing apparatus is the Δ load, the direct load $\frac{P}{S}$ meaning absolutely nothing.

This is the pith of the theoretical error embodied in Mr. Lanchester's paper in *Engineering*. He states that on the basis of constant velocity and for all designs large and small, the weight of the aerofoil—which we might consider here as including the whole machine barring power plant—varies as the cube of the span, and the gross weight as the square of the span. But if this rule be applied to a gliding model taken as a prototype, it simply leads to the impossibility of a man-carrying glider, and, indeed, of all human flight. If, for instance, we have an 8-ft. gliding model weighing gross 20 lbs., with aerofoil weight 8 lbs.—a very familiar article—and we treble its scale to 24 ft.—also a very familiar article—the aerofoil weight becomes $8 \times 27 = 216$ lbs., but the gross weight only $20 \times 9 = 180$ lbs.

The truth is, as we have seen, that for constant velocity, the gross weight varies as $S^{1 \cdot 33}$, and since S varies as span^2, the gross weight varies as span$^{2 \cdot 66}$. Therefore, the gross weight of our treble-scale glider is not 180 lbs., but $20 \times 3^{2 \cdot 66} = 374$ lbs., which figure corresponds exactly to what we and others have practised for several years. If we make the scale fourfold instead of treble, we have the early small monoplane—*viz.*:

Span	32 ft.
Aerofoil weight 8×64 . .	$=512$ lbs.
Gross weight $20 \times 4^{2 \cdot 66}$ . .	$=800$ lbs.

which already permits of a small engine in addition to a pilot.

FACT 4. If our two apparatuses have the same Δ load and their balance is identical, they will glide at exactly the same speed and in the same manner, *but the gliding angle and also the general stability of the larger one is invariably better than that of the smaller one,* in proportion to the extent of the difference in size. Without being able to define the gain in the gliding angle accurately, we would say that one half point for each unit of the scale of enlargement is a fair estimate, although up to what critical point we cannot say. The reason of this gain is probably that since the gliding speed must be the speed at which the head resistance becomes equal, not to the total weight, but to the total weight minus that portion of it which is absorbed by the lift, although there is the same proportion of unabsorbed weight in each, as a speed-giving factor, the $\frac{P}{S}$ is greater in the larger apparatus, and, the head resistance being only proportional to surface, that proportion of weight acts, of course, with greater effect.

FACT 5. If in a specific glider the weight is altered, the gliding speed follows the square root of the difference. In other words, *the gliding speed is always proportional to* $\sqrt[2]{\triangle}$. So that $\sqrt[2]{\triangle}$ multiplied by a co-efficient based on observation gives us the formula of the gliding speed. In fact, it can be said that $\sqrt[2]{\triangle}$ bears the same relation to the gliding speed as does $\sqrt[2]{\text{length}}$ to the beat of the pendulum, and just in the same way as it takes abnormal power to force a variation on the period of a pendulum, so in flight we cannot prevaricate against this inherent law of gliding without waste of power.

The only factors beyond the $\triangle$ load which seem to affect the gliding speed, although in a much lesser degree, are naturally the depth of the camber and the aspect ratio, because a deep camber and a long entry edge give better lift at equal speed, and therefore leave less weight available as a speed-giving factor. Within the limits of what is practised and practicable, the variations of gliding speed resulting from these two minor factors would very rarely bring the co-efficient below 8 or above 9. We can say that $\sqrt[2]{\Delta}\times 8{\cdot}5$ gives in metres per second a very near estimate of the gliding speed of all modern machines of any type or size. This formula will be found true from a 1-lb. model up to the largest aeroplane in existence. We say advisedly from 1 lb. upwards, because, with very light gliders, viscosity begins to interfere with the free action of gravity, and with such small things, for instance, as little paper gliders weighing only 2 or 3 grammes, although the law remains the same, the co-efficient falls to 7, or even 6, instead of 8·5, and for those, Δ 2 or Δ 3 is quite a heavy load by reason of Fact 4. We must add that a large fuselage, as, for instance, in the case of the old Nieuport, makes also for rather increased gliding speed, however paradoxical this may seem. There is much to say on this fuselage question, which, however, is beyond the scope of this paper.

The most advantageous load—that is, the one corresponding to the greatest economy of power—is undoubtedly in the neighbourhood of Δ 8, probably because that load produces, by the formula just given, a gliding speed of 24 metres per second, which is about the speed at which the force of the tangential current reaches its climax. From a little indoor experiment made in 1908 by Mr. Weiss and Mr. Handley-Page, it seemed pretty clear that the suction produced by the tangential current followed the ordinary law of v^2 up to about 24 metres per second, when the suction or partial vacuum seemed to reach its greatest intensity and to remain constant, however much the speed was increased beyond this. We find even that the long-winged sailing birds have their △ load somewhat higher than the short-wing ones, presumably because the specific speed corresponding to the maximum of efficiency could not be the same in both classes with the same △ load, as, of course, the long-narrow wing would give a little less speed than the short-wide one.

The load of all aeroplanes as they exist also varies between △ 7 and △ 9, the latter being only rarely exceeded, and then only slightly when very strong engines are used. For war machines, however, where high performance is the aim, there is no reason why we should not load up to △ 12 and even △ 14, provided there is enough power and the construction is up to the strain. A very good guide for the H.P. to be fitted is the load per H.P., which for high performance has, of course, to be kept low. We shall see presently in what proportion. The only limitation to the △ load is an excessive gliding speed, which may make starting and landing difficult and dangerous, especially with machines weighing several tons. At the same time, where there is ample spare power, as would be the case here, an experienced pilot can land his machine, as do all birds, by *vol cabré*, at considerably less than gliding speed, and practice would most probably show that the landing is really easier than one expects, especially if the landing is made against the wind. The large machine cited in Example C hereafter has been landed at 38 miles, although its gliding speed is 51 miles. With some species of wild ducks which always descend on water the △ load reaches 24. The wings of these birds are exceedingly muscular, showing that their flight requires exceptional power. With aeroplanes the highest △ load has been reached in the Deperdussin 100 H.P. monocoque at 20, corresponding to a gliding speed of 85 miles.

FACT 6. *If in a specific glider the head resistance is reduced or increased, the gliding speed is not affected by the alteration, but the gliding amgle is altered for the better or the worse respectively.* This is because any reduction of the head resistance immediately tends to increase the speed and therefore the lift, and consequently to reduce the weight available for speed, and so to automatically check the tendency, just as a ball-governor in a steam engine checks the tendency to increased speed from increased pressure without preventing the engine from taking an increased load as the result of increased pressure. Or just as in the pendulum, where friction or variations of power do not affect the period of the oscillations but only their amplitude.

We have proved this fact to the hilt by means of large lumps of loose cotton wool attached to the gliders. It was proved to be an observed fact by Ferber and we believe also by Goupil before 1905. It is a fact of the utmost importance because, when power is applied, it simply acts exactly in the same manner as the weight to overcome the head resistance, and thus the trajectory is raised to the horizontal without the gliding speed being exceeded. It is for that reason that when a machine is climbing at its highest rate, the climbing speed is the same as the gliding speed. In other words, it takes a definite amount of power (H.H.P.) to raise the trajectory to the horizontal at gliding speed, and the surplus of power available can then be converted either into acceleration or into climb, the maximum of climb being, of course, attained when none of that surplus power goes into acceleration, and *vice versa.* If too steep a gradient is attempted the speed must drop below gliding speed, when a dive or at least an undulation, which is an incipient dive, is bound to result—a very familiar experience.

Since to reach the horizontal it is sufficient to completely overcome the head resistance at gliding speed, and since, when the horizontal is reached, no more assistance can be derived from the weight—barring temporary inertia, of course—as a factor to overcome head resistance, it follows that *the H.P. absorbed when the horizontal is reached is exactly that which is required to overcome the whole of the head resistance at gliding speed, independently of the weight carried, but not independently of the △ load which determines the gliding speed.* This law gives us our formula for H.H.P., because the head resistance being proportional to the surface and to the square of the speed, and the speed being proportional to $\sqrt[2]{\Delta}$, H.H.P. must be equal to SΔ multiplied by a co-efficient. It is, of course, obvious that the value of this co-efficient depends on the quality of the machine and on the efficiency of the engine and propeller. For sailing birds, for instance, it would be only a fraction of what we require with our crude machines. But for average machines of the present day, with average engines, and with propellers covering about one-quarter of the span, the value of that co-efficient is just about 0·1 effective H.P. if the value of E.H.P. is taken at 0·66 N.H.P. This will yield figures if anything on the conservative side. Any variations in the co-efficient of H.H.P. naturally falls in the calculations on the value of E.H.P., the two being interdependent.

Our free flight experiments during the last 15 years may not have been conducted with the method and the thoroughness generally expected from professional scientists—which we are not. But it must be borne in mind that the difficulties encountered in free flight experiments with heavy models and gliders are only realised by the few who have tried them, and arise from the innumerable mishaps and breakages which prevent one again and again from obtaining in a reliable manner the data aimed at. It has not been possible to tabulate in orderly fashion the results of this or that specific experiment, and the conclusions have had to crystallise by degrees from the slow accumulation of evidence obtained bit by bit according to luck, and we may say here that these experiments have entailed the construction of over 200 heavy gliders of all sizes, including a number of piloted ones. Nothing else, however, than free flight experiments could possibly reveal the laws which govern gliding flight. The laboratory misses the most essential factor of gliding—namely, the absence of an external fulcrum, which absence causes the glider to be an ordinary falling body following the path of least resistance, and brings thus into operation a whole set of complex laws, the effects of which can only be detected in free flight. Laboratory tests, which otherwise yield such valuable information, are helpless to teach much in regard to gliding results, beyond the famous $\frac{x}{y}$ ratio, which takes no count of the weight and gives no clue as to the resulting gliding speed or as to the limits beyond which, through either lack or excess of weight, an apparatus will refuse to glide. And the gliding angle is not even determined by the $\frac{x}{y}$ ratio found in the laboratory, with the use of an external fulcrum and at a variety of angles arranged at will, but by the $\frac{x}{y}$ ratio, when x is at its absolute minimum, no steady gliding or flying being possible in any other position; and, above all, the gliding angle will depend on how near the load is to its optimum critical point, which point varies with the general quality of the machine and also with its size, by reason of Fact 4.

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