



THE EARLY HISTORY OF THE AIRPLANE

ORVILLE E WILBUR WRIGHT

JVA EDITORA

ORVILLE E WILBUR WRIGHT

The Early History of the Airplane

First published: 1922
Public domain edition

JVA Editora

The **EARLY HISTORY** of the **AIRPLANE**

The Wright Brothers' Aeroplane	1
How We Made the First Flight	<u>9</u>
Some Aeronautical Experiments	<u>16</u>

The
DAYTON-WRIGHT AIRPLANE CO.
DAYTON·OHIO

The Wright Brothers' Aeroplane

[1]

By Orville and Wilbur Wright



HOUGH the subject of aerial navigation is generally considered new, it has occupied the minds of men more or less from the earliest ages. Our personal interest in it dates from our childhood days. Late in the autumn of 1878 our father came into the house one evening with some object partly concealed in his hands, and before we could see what it was, he tossed it into the air. Instead of falling to the floor, as we expected, it flew across the room, till it struck the ceiling, where it fluttered awhile, and finally sank to the floor. It was a little toy, known to scientists as a "helicoptere," but which we, with sublime disregard for science, at once dubbed a "bat." It was a light frame of cork and bamboo, covered with paper, which formed two screws, driven in opposite directions by rubber bands under torsion. A toy so delicate lasted only a short time in the hands of small boys, but its memory was abiding.

Several years later we began building these helicopteres for ourselves, making each one larger than that preceding. But, to our astonishment, we found that the larger the "bat" the less it flew. We did not know that a machine having only twice the linear dimensions of another would require eight times the power. We finally became discouraged, and returned to kite-flying, a sport to which we had devoted so much attention that we were regarded as experts. But as we became older we had to give up this fascinating sport as unbecoming to boys of our ages.

It was not till the news of the sad death of Lilienthal reached America in the summer of 1896 that we again gave more than 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, and 1907, and several pamphlets published by the Smithsonian 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 center 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 center 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.

In a slightly modified form the same system was applied to the fore-and-aft balance. The main aeroplane was set at a positive angle, and a horizontal tail at a negative angle, while the center of gravity was placed far forward. As in the case of lateral control, there was a tendency to constant undulation, and the very forces which caused a restoration of balance in calms caused a disturbance of the balance in winds. Notwithstanding the known limitations of this principle, it had been embodied in almost every prominent flying machine which had been built.

After considering the practical effect of the dihedral principle, we reached the conclusion that a flyer founded upon it might be of interest from a scientific point of view, but could be of no value in a practical way. We therefore 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 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.

Lilienthal and Chanute had guided and balanced their machines, by shifting the weight of the operator's body. But this method seemed to us incapable of expansion to meet large conditions, because the weight to be moved and the distance of possible motion were limited, while the disturbing forces steadily increased, both with wing area and with wind velocity. In order to meet the needs of large machines, we wished to employ some system whereby the operator could vary at will the inclination of different parts of the wings, and thus obtain from the wind forces to restore the balance which the wind itself had disturbed. This could easily be done by using wings capable of being warped, and by supplementary adjustable surfaces in the shape of rudders. As the forces obtainable for control would necessarily increase in the same ratio as the disturbing forces, the method seemed capable of expansion to an almost unlimited extent. A happy device was discovered whereby the apparently rigid system of superposed surfaces, invented by Wenham, and improved by Stringfellow and Chanute, could be warped 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.

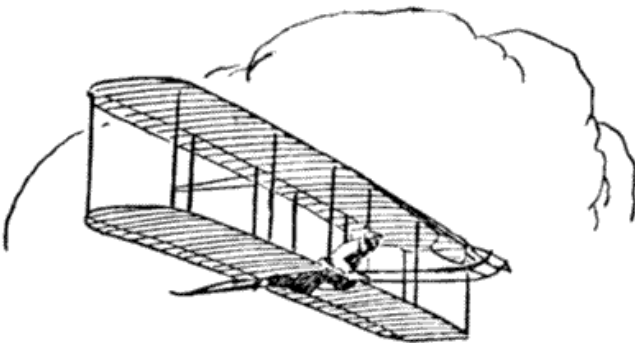
The period from 1885 to 1900 was one of unexampled activity in aeronautics, and for a time there was high hope that the age of flying was at hand. But Maxim, after spending \$100,000, abandoned the work; the Ader machine, built at the expense^[3] of the French Government, was a failure; Lilienthal and Pilcher were killed in experiments; and Chanute and many others, from one cause or another, had relaxed their efforts, though it subsequently became known that Professor Langley was still secretly at work on a machine for the United States Government. The public, discouraged by the failures and tragedies just witnessed, considered flight beyond the reach of man, and classed its adherents with the inventors of perpetual motion.

We began our active experiments at the close of this period, 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 learned 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. He also witnessed one flight of the power machine near Dayton, Ohio, in October, 1904.

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 $\frac{1}{12}$ the depth of its cord; 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 center of pressure on a curved surface traveled 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 counterbalanced 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 details of this arrangement are given in specifications published several years ago.

The experiments of 1901 were far from encouraging. Although Mr. Chanute assured us that, both in control and in weight carried per horse-power, 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 undistinguishable. 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 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 recognized 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 formulas; 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 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 we 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 50 of these at all angles from zero to 45 degrees at intervals of $2\frac{1}{2}$ 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 $7\frac{1}{2}$ 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 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 farther 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 first designs provided for a total weight of 600 lbs., including the operator and an eight horse-power motor. But, upon completion, the motor gave more power than had been estimated, and this allowed 150 lbs. to be added for strengthening the wings and other parts.

Our tables made the designing of the wings an easy matter, and as screw-propellers are simply wings traveling in a spiral course, we anticipated no trouble from this source. We had thought of getting the theory of the screw-propeller from the marine engineers, and then, by applying our tables of air-pressures to their formulas, of designing air-propellers suitable for our purpose. But so far as we could learn, the marine engineers possessed only empirical formulas, and the exact action of the screw-propeller, after a century of use, was still very obscure. As we were not in a position to undertake a long series of practical experiments to discover a propeller suitable for our machine, it seemed necessary to obtain such a thorough understanding of the theory of its reactions as would enable us to design them from calculations alone. What at first seemed a problem became more complex the longer we studied it. With the machine moving forward, the air flying backward, the propellers turning sidewise, and nothing standing still, it seemed