Chapter 1

The Achievement of Flight

> *Success four flights Thursday morning all against twenty-one mile wind started from level with engine power alone average speed through air thirty-one miles longest 57 seconds inform press home Christmas.*

Orville Wright telegram sent on 17 December 1903 from the lifesaving station in Kitty Hawk, North Carolina, to his father, Bishop Milton Wright, in Dayton, Ohio. This document announced humankind's first successful powered flight of a heavier-than-air flying machine.

From Cayley's Triple Paper to Orville Wright's Telegram

Even in a study of NASA's—and its predecessor agency, the NACA's—contributions to aerodynamic development, one must begin by looking at a fruitful pairing, not of two government agencies but of two remarkable individuals, the Wright brothers. It was the inventive genius of Wilbur and Orville Wright, rather than an institutional imperative, that set the stage for aviation's astonishing technological progress in the United States and abroad during the twentieth century. Without retracing the path from the earliest concept of the airplane to the pioneering achievement of the Wrights, readers of this volume will not be able to truly appreciate the formative ideas from which research and development in the field of modern aerodynamics blossomed.

The Wrights accomplished what no one before them had the insight to recognize and tackle—they worked out a means of controlling an aircraft aerodynamically by deflecting the wing and tail surfaces themselves, as Wilbur had seen birds do. In this respect, they were far ahead of their predecessors and contemporaries, including some of the world's top scientists. The secret of their success was that they learned to fly: before they ever attempted to put an engine on their glider, and thus complete the "invention" of the airplane, they learned how to control a flying machine in the dynamic element of the air. They learned about the use of aerodynamic surfaces to control the airplane in a satisfactory manner and, together with the experience they developed as pilots of their kites and gliders, were able to fly their "powered" plane without difficulty. They were marvelously clear and pioneering thinkers, a rare combination. As such, they deserve a place of distinction at the beginning of any history of modern aerodynamics, especially one published during the centennial of their great achievement.

For this first chapter in the first volume of this series, then, there is no more fitting destination than the wind-blown sands of Kitty Hawk, North Carolina, where on 17 December 1903, the Wrights made the world's first successful powered flight of a heavier-than-air craft. The flight of the Wright airplane in 1903 culminated a long, faltering search for basic aerodynamic knowledge, a winding and irregular path that runs far back into the dawn of human experience, back even to our ancestors' prehistoric fascination with the flight of birds. No doubt, humankind dreamed about flying as soon as humans began dreaming. When civilizations arose, men and women imagined gods in the sky and gave them wings—no matter what ancient civilization one looks at, its gods could fly. The power of flight, denied to mortals, was envisioned as an ability of immortal deities. They lived in the heavens above; they moved across the sky in golden chariots; they soared majestically in and above the clouds.

While the materials needed to fabricate a craft resembling a modern hang glider were available thousands of years ago, unquestionably one of the major reasons that mechanical flight did not happen for millennia rested in the basic intuitive misconception that, in order for humans to fly, we needed to imitate birds. But exactly how did birds accomplish flight? Before modern scientific observations with high-speed cameras and, more importantly, before the dawn of a mathematically derived and rational theory of aerodynamics in the early nineteenth century, naked-eye observation of bird flight fostered a wrongheaded belief in flapping wings. From the wings crafted in ancient Greek mythology by Daedalus and Icarus to the ornithopter designed by Leonardo da Vinci during the Renaissance, the best idea anyone could come up with was to mimic a flapping wing structure and adapt it for human use. This path to heavier-than-air flight proved not only ignominiously unsuccessful but also often fatal. Donning makeshift wings of wood, feathers, and cloth, a handful of foolish men in the Middle Ages, flapping away without a whit of aerodynamic understanding, jumped to their deaths from the tops of cathedral towers and castle walls.

Not just popular imagination but also esoteric intellectual conceptions of the age mystified flight rather than rationalized it. Various medieval and Renaissance thinkers ascribed the ability of the wing to produce lift to psychic and occult forces possessed by the birds. This view held that for humans to fly required not some understanding and application of natural laws but rather their circumvention through the invocation of magical powers.[1]

This is not to say that, from ancient times to da Vinci, there was no one who expressed sound aerodynamic ideas. In certain basic respects, aerodynamics

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[1] Charles H. Gibbs-Smith covers the pre-history of flight in *Aviation: An Historical Survey* (London: HMSO, 1970).

began in classical Greece during the fourth century B.C. with the first "scientific" observer of nature, the philosopher Aristotle. He developed a theory of motion that a projectile such as an arrow stayed in motion because the air separated in front of the arrow and then rapidly filled in the region behind it. According to this concept, the arrow kept flying because of the constant application of the medium's force in its wake. Despite being fundamentally wrong (though it was a widely accepted theory until Galileo's time), Aristotle's "medium theory" represented an attempt to explain the phenomena of flight in rational terms with the help of mental pictures of physical operations. This type of visual thinking was a crucial first pathway in the maze leading to mature understanding.

With other aerodynamic concepts, Aristotle proved even more insightful. He was the first thinker to elaborate on the notion of a "continuum." In a radically altered form, this became a basic concept of modern science, the foundation of aerodynamic flow applications today. In his writings, one can also find physical reasoning consistent with a concept of aerodynamic "resistance," inversely expressed through the appreciation that a body in motion, in Aristotle's words, "will move indefinitely unless some obstacle comes into collision with it."[2]

None of Aristotle's ideas prompted anyone to consider designing an airplane, however, nor did any of the concepts developed by Archimedes, the greatest engineering mind of the Hellenistic world. Archimedes, who developed the mechanical lever, built ingenious catapults, and created a screw device for raising water, is also recognized as founder of the science of fluid statics. But not even Archimedes, the author of the world's first "Eureka" experience, ever came close to the idea of mechanical flight. It is one thing to say that the brilliant minds of antiquity produced concepts in the raw, like the continuum or hydrostatic pressure that contributed to the development of aerodynamics, but quite another to say that any of them had technological import.

If science did not stimulate technology, maybe technology stimulated science? One might think that something like the invention of the kite, which took place in China around 1000 A.D., and which appeared in Europe three centuries later in the form of the windsock, would have provoked aerodynamic thinking. Or that

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[2] Aristotle expressed most of the major tenets of his natural philosophy in his *Treatise on the Heavens (De Caelo)*, Book I. An excellent English translation of this entire treatise (in two books) was done by Stuart Leggat in 1995. His translation and commentary help the reader to appraise Aristotle's ideas in relation to the cosmologies of his predecessors. Leggat makes it clear that, while tied to the thinking of his day, Aristotle placed natural philosophy on a new footing, with his use of mathematics-style demonstration and his appeal to observation rather than reliance on what were then more standard forms of argument. John D. Anderson does a good job of interpreting Aristotle's significance in aerodynamics in *A History of Aerodynamics and Its Impact on Flying Machines* (Cambridge, England, and New York, NY: Cambridge University Press/Cambridge Aerospace Series, 1997), pp. 14-17.

the understanding of basic aerodynamics improved with the design of windmills, a powerful new machine that migrated into Western Europe from the Near East around the late thirteenth century. But neither seems to have been the case. History records no serious attempt to bridge the gap between the dream of flight, the actual world of invention, and basic scientific understanding of aerodynamic forces until the work of Leonardo da Vinci in the late fifteenth and early sixteenth centuries. Even then nothing came of da Vinci's thoughts. Leonardo's precious notebooks, encoded with his curious "mirror-like" reverse handwriting, stayed locked up and unknown in the hands of different private collectors for 300 years.

One wonders what might have germinated if da Vinci's ideas had been published and widely discussed during the Scientific Revolution of the sixteenth through eighteenth centuries. Certainly no one before da Vinci, or for three centuries after him, offered anything close to his astonishing vision of flying machines or considered so many of the principles affecting flight. In the same era that Christopher Columbus sailed across the Atlantic Ocean and Vasco da Gama circumnavigated Africa, Leonardo explored his own New World: diagramming the wing structures of birds and bats, developing streamlined shapes and aerodynamically more efficient artillery projectiles, and studying complex flow patterns in streams of water. He drew plans for an ornithopter, a parachute, and a vague sort of helicopter. He meticulously studied the mechanics of bird flight, and shortly before his death in 1519 he was apparently close to the breakthrough concept that a flying machine could be made with fixed instead of flapping wings.

Perhaps most significantly, da Vinci realized that "a bird is an instrument working according to mathematical law, an instrument which is within the capacity of man to reproduce with all its movements." In other words, he realized that applied mathematics was the key not only to understanding basic problems in nature but also to designing advanced technology. "Let no man who is not a mathematician read the elements of my work," he wrote. Immersed in Euclid's geometry and other mathematical texts, and coupling this knowledge with his meticulous observation of natural phenomena, Leonardo came up with the first quantitative expressions for fluid flow conditions, and then applied the same analytical thinking about flow conditions to aerodynamic phenomena. In 1513, he wrote:

> What quality of air surrounds birds in flight? The air surrounding birds is above thinner than the usual thinness of the other air, as below it is thicker than the same, and it is thinner behind than above in proportion to the velocity of the bird in its motion forwards, in comparison with the motion of its wings towards the ground; and in the same way the thickness of the air is thicker in front of the bird than below, in proportion to the said thinness of the two said airs.[3]

Considering this passage, modern aerodynamic engineer and historian John D. Anderson concluded that Leonardo was "three centuries ahead of his time." In his book *A History of Aerodynamics and Its Impact on Flying Machines* (1997), Anderson notes that what Leonardo contributed was "a valid description, expressed in the technical language of the early sixteenth century, of the sources of lift as well as pressure drag (form drag) on an aerodynamic body." Da Vinci's ideas about lift and drag, however, were not disseminated until the late 1790s when the Italian physicist Giovanni Battista Venturi discovered twelve volumes of Leonardo's notes in the Institute of Paris, where they had been moved by Napoleon's troops after the looting of the Ambrosian Library in Milan.

By that time Leonardo's undeniable genius was no longer critical, for other scientists between his life and Venturi's had advanced the understanding of fluid dynamic phenomena beyond da Vinci's own thinking. This started with the work of Galileo Galilei (1564–1642), who developed concepts of inertia and momentum, offered insights into the parabolic trajectory and aerodynamics of ballistic projectiles, and deduced that aerodynamic resistance was proportional not just to velocity, as da Vinci had understood, but also to the air density. Along with Galileo's work there were the pioneering hydrodynamic theories and experiments of the Italian physicist and mathematician Evangelista Torricelli (1608–1647), the inventor of the barometer and the first to measure air pressure, as well as the work of Frenchman Blaise Pascal (1625–1662), who advanced new fundamental principles about the actions of atmospheric pressure on fluid behavior.[4]

Later in the seventeenth century came what is referred to as "the first major breakthrough in the evolution of aerodynamics." This was the formulation of the "velocity-squared law," which states that the force on an object varies as the square of the flow velocity; or in other words, if velocity doubles, then the force acting on it goes up by a factor of four, not of two as Galileo and others had believed. Derived experimentally by Edme Mariotte (1620–1684) in France and Christian Huygens (1625–1695) in Holland, and confirmed theoretically by Isaac Newton (1642–1727) in his *Principia* of 1687, the velocity-squared law lifted scientific understanding of aerodynamics to a new level by adding a crucial, quantifiably exact variable to the mechanical relationships defining aerodynamic force.[5]

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[3] Leonardo da Vinci, Codex Atlanticus Volume E, translated by R. Giacomelli, "The Aerodynamics of Leonardo da Vinci," *Journal of the Royal Aeronautical Society* 34 (1930): 1016–1038, cited in John D. Anderson, Jr., A History of Aerodynamics (New York, NY: Cambridge University Press, 1997), p. 24.

[4] On Galileo, Torricelli, Pascal, and Newton as well, see Richard S. Westfall, *The Construction of Modern Science: Mechanisms and Mechanics* (New York and London: John Wiley & Sons, Inc., 1971).

[5] Anderson, *A History of Aerodynamics*, pp. 32–37.

On the heels of the velocity-squared law came Newton's own "sine-squared law." Though later shown to be flawed, this suggested a means by which to calculate the pressure distribution over a body in motion and make predictions about its lift and drag. Newton's three laws of motion were another major contribution to aerodynamic understanding, especially his second law, which stated that the time rate of change for a moving body (momentum being the product of velocity and mass) was proportional to the force. Applied to fluid flow, this Newtonian law provided a basic equation on which to build further aerodynamic understanding. Other major blocks in the foundation were laid by the great Swiss mathematicians Daniel Bernouilli (1700–1782) and Leonhard Euler (1707–1783), who provided differential equations that, once their solutions could be worked out in the mid-1800s, provided accurate pressure distributions over aerodynamic bodies and, thus, a way of eventually analyzing low-speed flows and reliably calculating lift.[6]

But all this simply provided analytical tools for future use, for none of these scientists and mathematicians of the seventeenth and eighteenth centuries had any interest whatsoever in applying them to the design of a flying machine. Bernouilli and Euler may have developed the mathematics that would be used to show how the pressure differential on the upper and lower surfaces of a wing developed the phenomena of lift, but they themselves never attempted to design an effective shape to achieve this. This, the problem of designing an effective wing or "airfoil," was a fundamental problem that needed a solution if heavier-than-air flight was ever to be achieved.

All that came before the early 1800s served only as prologue to the story leading to the invention of heavier-than-air flight by the Wright brothers. The genuine narrative begins in 1804 when Sir George Cayley (1773–1857), a twenty-one-year-old scientifically educated English country squire, designed and hand-launched a small glider. Not more than a meter long, Cayley's innocent-looking glider of 1804 represented a revolutionary breakthrough, as it essentially incorporated all the elements of the modern airplane. This simple yet profound design effectively combined the three essential features recognized today in the configuration of a modern airplane: a fixed wing, a body or fuselage, and a tail with both horizontal

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[6] A number of fine new books and translations have added considerably to our historical and technical understanding of Isaac Newton's contributions, notably *The Principia: Mathematical Principles of Natural Philosophy*, a translation of Newton's great work of 1687 by Julia Budenz, Anne Whitman, and I. Bernard Cohen (Berkeley, CA: University of California Press, 1999). Other important studies that have recently appeared include Francois De Gandt and Curtis Wilson, *Force and Geometry in Newton's Principia* (Princeton, NJ: Princeton University Press, 1995); Gale E. Christianson, *Isaac Newton and the Scientific Revolution* (Oxford, England: Oxford University Press, 1996); A. Rupert Hall, *Isaac Newton: Adventurer in Thought* (Cambridge, MA, and New York, NY: Cambridge University Press/Cambridge Science Biographies, 1996); Jed. Z. Buchwald and I. Bernard Cohen, *Isaac Newton and Natural Philosophy* (Cambridge, MA: MIT Press/Dibner Institute Studies in the History of Science and Technology, 2000); and David Berlinski, *Newton's Gift: How Sir Isaac Newton Unlocked the System of the World* (New York, NY: Free Press, 2000).

and vertical surfaces. While producing such a glider may seem like child's play today, at the time it was totally at variance with existing flapping wing, or ornithopter, concepts of how to achieve heavier-than-air flight.

Cayley's configuration was the product of some bold new thinking about what it was going to take to fly. For example, Cayley understood how to produce the lift necessary for flight, because he recognized that curved surfaces produced more lift and less drag than flat surfaces. He also understood that the airplane was going to have to be stabilized and controlled, thus the need for tail surfaces. And he understood mathematically what kind of power it was going to take to achieve flight—and this was well in advance of the invention of practical lightweight engines themselves. While he benefited from his knowledge of existing scientific theory, such as Newton's sine-squared law, Cayley's 1804 glider design was essentially "his own." On his own he performed laboratory experiments; conducted flight testing, and analyzed his results. What was so remarkably new about Cayley was the way he broke the elements necessary for flight into what would later be called "systems," i.e., a system to produce lift, a system for control of the craft in the air, and a system to produce propulsion. Before Cayley, people had thought not in terms of separate systems that could be effectively integrated, but of one organic system, like that of a bird, that somehow could do everything.[7]

Though Cayley first arrived at his concept of the airplane in 1799 and integrated it into his 1804 glider, his radically new approach to understanding the airplane is best exemplified in a three-part paper "On Aerial Navigation," written in 1809 and published in three parts in *Nicholson's Journal of Natural Philosophy, Chemistry, and the Arts*.

Without question, this paper, reproduced in its entirety as the inaugural document of this volume, represents "the highlight in the history of aerodynamics"—not just for the beginning of the nineteenth century, but for all human history up to that time. (See Document 1-1.) As John Anderson writes in his history, "the work of all previous investigators pale" compared to Cayley's treatise.[8] For its relationship to the subsequent development of the airplane, this document by George Cayley compares to such seminal publications as Copernicus' 1543 *On the Revolution of Heavenly Bodies*, Darwin's 1859 *Origin of Species*, and Watson and Crick's 1953 paper on their discovery of the structure of DNA. Cayley himself kept working on flying machines up to his death in 1857.

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[7] Anderson does a very complete job of analyzing Sir George Cayley's seminal contributions in *A History of Aerodynamics*, pp. 64–80. There is no monographic treatment of Cayley, though one is much needed. In 1968, the Science Museum of London published a "Science Museum Booklet" entitled *Sir George Cayley*, 1773–1857, authored by C. H. Gibbs-Smith.

[8] Anderson, *A History of Aerodynamics*, p. 79.

Curiously, after the publication of the "Triple Paper," Cayley shifted his attention for four decades to the development of balloons and airships. Most likely he did this because of the vexations of developing an engine for airplane applications. Steam engines with huge boilers simply weighed too much, and innovative new engines, such as gas-fueled, internal combustion engines, were still very experimental and quite limited in their power output. But even without the right engine, in 1843 he refocused again on the airplane and began designing full-scale gliders anew. One of them carried a ten-year-old boy without incident a few yards down a hill in 1849. Four years later another carried his reluctant coachman across a small valley, possibly flying as far as 500 yards. (Unaware of the historic auspices of this 1853 flight, the coachman on landing allegedly remarked, "Please, Sir George, I wish to give notice. I was hired to drive and not to fly.")

With its roots in the scientific revolution, Cayley's work provided the trunk for a growing tree of aerodynamic knowledge and invention that in half a century led to the Wright brothers' achievement in December 1903. From that solid trunk, however, at least three major branches of experimental heavier-than-air flight technology grew in the nineteenth century, and no one at the time could be certain which branch would ultimately bear the most fruit.

The first branch involved trying to learn about flight by experimenting with small-scale models. This proved to be a very useful approach for testing aerodynamic concepts, one that cost little money and did not risk life or limb. The most important individual taking this approach was the young French marine engineer, Alphonse Pénaud (1850–1880), who in the 1860s and 1870s experimented with a number of little flying models powered by twisted rubber bands, much like the toys children still play with today. His most successful model flew 131 feet across the Tuileries Gardens in Paris in August 1871. Pénaud based his model on Cayley's 1804 glider design, but improved it in key respects. He set its horizontal tail to a negative angle of eight degrees relative to the chord line of the wing, which gave his airplane greater longitudinal (or pitching) stability. He also bent its wing tips up in a dihedral angle, thereby providing lateral (or roll) stability. Changes like this, which would have been difficult, expensive, and potentially dangerous if made with a full-scale, manned aircraft, were relatively simple to make and flight test with a small-scale model.[9]

What Pénaud sought was a mechanism that would guarantee absolute inherent stability, so he ignored the possibility of active pilot control. In this he shared the

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[9] Pénaud is discussed in most surveys of the development of flight, but there is no single exhaustive study devoted to his work. Anderson covers Pénaud adequately in *A History of Aerodynamics*, pp. 193–194. See also Tom D. Crouch, *A Dream of Wings: Americans and the Airplane*, 1875–1905 (Washington, DC, and London: Smithsonian Institution Press, 1981), pp. 36, 46, 57–59, 64, and 192; as well as Crouch's *The Bishop's Boys: A Life of Wilbur and Orville Wright* (New York and London: W. W. Norton & Co., 1989), pp. 56–57, 161, 164, 168–169, 249, and 342.

mindset of many aeronautical thinkers at that time, who believed that their first task was to demonstrate how to fly in a simple straight line with a passenger on board and that the issue of control could be dealt with later. Pénaud's emphasis on automatic stability was also a limitation of the scale-model approach. Working with models powered by twisted rubber bands, and without any means of piloting them once launched, Pénaud had little choice but to go after inherent stability. With their inefficient power source, his best flights lasted only thirteen to fourteen seconds, and were characterized by Professor Langley of the Smithsonian Institution as being, "so erratic, and so short, that it was possible to learn very little from them."

At the end of the nineteenth century many people regarded Samuel Pierpont Langley (1834–1906) as the unofficial chief scientist of the United States, which makes it even more significant that Langley, though aware of the limitations of Pénaud's experiments, also focused his own work on scale models. In fact, Langley subscribed to the erroneous and ultimately tragic assumption that results from scale models could be directly scaled up to design a full-scale airplane. In

Although he ultimately failed in his effort to build a successful man-carrying airplane, one should not minimize Professor Samuel P. Langley's many contributions to aerodynamic understanding, especially those embodied in the flights of his large steam-powered models in 1896. In this photograph from December 1895, Langley's *Aerodrome No. 6* (which evolved from *No. 4*) had not yet matured into its final configuration. SI Negative No. A-2854-J

1887, Langley left his work in astronomy and solar science, for which he was very distinguished, to take the prestigious position of Assistant Secretary of the Smithsonian Institution (a few years later he became Secretary). There he embarked on a new course of remarkable aeronautical experiments. He followed the approach of Pénaud but with models that quickly grew much larger, more complicated, and were powered by small steam and gasoline engines rather than rubber bands. His research paid off, up to a point. On 6 May 1896, his *Aerodrome No. 5*, a steam-powered model weighing thirty pounds and with tandem wings spanning over thirteen feet, made two long flights from a launching platform atop a houseboat in the Potomac River south of Washington, D.C. The best of the flights that day covered 4,200 feet in forty-five seconds. Celebrated inventor Alexander Graham Bell, a close friend of Langley's, observed these flights first-hand and published ecstatic accounts of what Langley had achieved as a major step on the way toward powered flight.[10] (See Document 1-8.)

In terms of the history of aerodynamics, Langley made other major contributions. His work in the late 1880s and 1890s has been called "the first meaningful aerodynamic research" in America, a systematic program of tests involving whirling arms and other sophisticated experimental instruments of his own design, the purpose of which was to measure the various aerodynamic forces at work on different surfaces, including propeller shapes. Interestingly, however, this program of basic research "contributed little of practical value" to the design of either his or any other flying machine of the day. Moreover, contemporary scientists, including British giants Lord Kelvin and Lord Rayleigh, condemned his most provocative aerodynamic conclusion, the so-called "Langley's law," which stated that the power required for a vehicle to fly through the air decreased as the velocity increased.[11] (See Document 1-4.) Lilienthal, the Wrights, and others trying to fly believed that this counterintuitive idea had to be wrong—which it was, except for in the narrow range of Langley's test velocities (20 meters per second or less).

A difficult personality at best, Langley today is chiefly remembered as the one who failed where the Wrights succeeded. Yet perhaps the best commentary on his role and achievements came from the Wrights themselves. Writing to a friend shortly after Langley's death, Wilbur Wright said of him:

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[10] No comprehensive biography has yet been written of Dr. Samuel P. Langley. The books by Tom Crouch cover Langley's importance in some significant detail, as does Anderson's *A History of Aerodynamics*. But as much as any other figure in the early history of aviation that has not yet received one, Langley deserves an exhaustive, singular treatment.

[11] For an analysis of "Langley's law" and how it erred, see Anderson, *A History of Aerodynamics*, pp. 179–181.

The successful launch of Langley's *Aerodrome No. 5* from atop a houseboat in the Potomac River, on 6 May 1896. SI Negative No. A-18870

> The knowledge that the head of the most prominent scientific institution of America believed in the possibility of human flight was one of the influences that led us to undertake the preliminary investigation that preceded our active work. He recommended to us the books which enabled us to form sane ideas at the outset. It was a helping hand at a critical time and we shall always be grateful. . . . When scientists in general considered it discreditable to work in the field of aeronautics he possessed both the discernment to discover possibilities there and the moral courage to subject himself to the ridicule of the public and the apologies of his friends. He deserves more credit for this than he has yet received. . . . Though we have rarely followed his lead, we have always found a study of his writings very profitable, especially at the time when we were trying to find out what the real sticking points of flying were.[12]

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[12] Wilbur Wright, letter to Octave Chanute from Dayton, Ohio, 8 November 1906, found in *The Papers of Wilbur and Orville Wright*, ed. Marvin W. McFarland, Vol. 2 (New York, NY: McGraw-Hill Book Company, Inc., 1953), pp. 736–738.

Langley's contemporary impact on the development of aerodynamics extended beyond his influence on the Wrights; as the culmination of the nineteenth century scale-model approach to aeronautical knowledge, Langley's experiments also had a much broader significance. First, his publications, beginning with his 1891 book *Experiments in Aerodynamics*, inspired others to step up their experimental efforts and to be systematic in carrying them out. Second, and perhaps even more importantly, the fact that one of the greatest scientists in the country had decided to devote his efforts to the problems of flight convinced a great number of people that "aeronautics was no longer the past-time of fools."

The notion that only fools tried to fly was certainly exacerbated by the second major branch of technological experimentation in heavier-than-air flight during the nineteenth century. This method involved venturesome attempts to build the real thing, full size, and then try to fly it with a person on board. This approach offered the advantage of more meaningful, immediate, and sensational results—not to mention the possibility of great notoriety for the successful inventor. But full-scale flying machines also meant considerably greater cost in construction, dramatically increased danger in testing, and the inability to experimentally vary design parameters quickly and easily.

Given the general impatience of human nature, though, it is not surprising that a number of individuals pursued this course anyway. In the 1840s, two Englishmen, William Henson and John Stringfellow, designed an "aerial steam carriage" with a huge 150-foot wingspan, powered by a thirty-horsepower steam engine they themselves planned to build. Their objective was to develop an "Aerial Transport Company" to haul goods commercially worldwide, an idea far ahead of its time. Their ambitious machine, however, never flew—in fact, it was never built, and all they constructed was a twenty-foot model that never made it into the air. Yet so many fanciful pictures of their proposed machine appeared in contemporary newspapers and magazines that many people believed the Henson-Stringfellow airplane flew over the Tower of London, the Pyramids, and even the Taj Mahal. These drawings cemented in the public consciousness the image of what an airplane should look like, with rectilinear wings, an enclosed cabin fuselage directly under the wing, twin screw propellers, and pilot-controlled tail surfaces. Thanks to the publicity surrounding this "machine that never flew," George Cayley's basic formula for configuring an airplane became the firmly established technological norm. After about 1845, when anyone thought of an airplane, they pictured it in terms resembling the Henson-Stringfellow vision of the basic Cayley design.[13]

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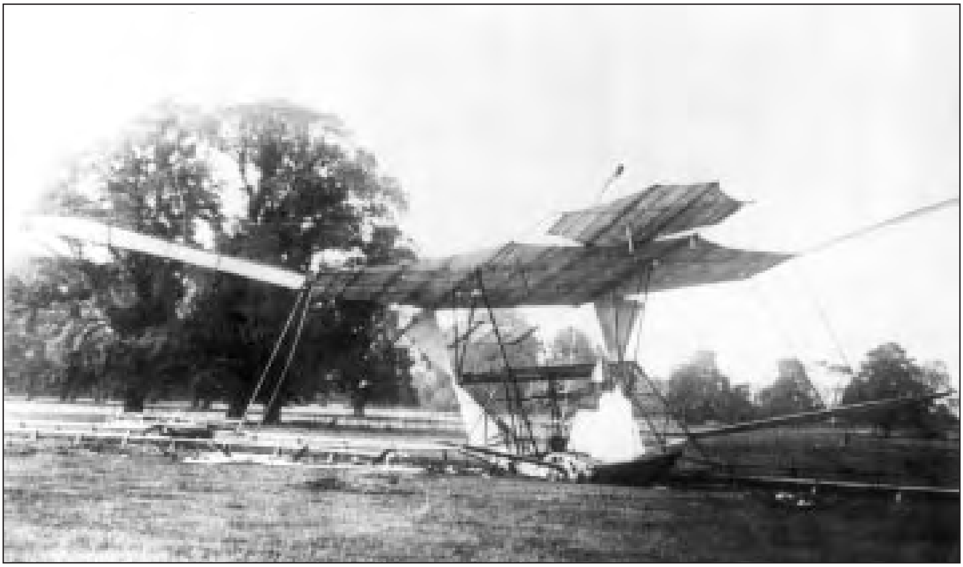
[13] On the aerodynamics involved with the "aerial steam carriage" conceived by Henson and Stringfellow in the 1840s, see Anderson, *A History of Aerodynamics*, p. 194. See also Crouch, *A Dream of Wings*, pp. 28 and 89.



Maxim's huge four-ton biplane, with its eighteen-foot propeller, sits on its track ready for flight testing (unsuccessful) in 1894. Ultimately the monster rose slightly from its guardrails, but only after traveling some 600 feet down the track. It possessed no truly redeemable features and represented, in its two very efficient steam engines, a brute force approach to getting an airplane aloft. SI Negative A-212-A

In the second half of the nineteenth century there were other full-scale attempts at flying machines. Two naval officers, Felix du Temple in France in 1874 and Alexander Mozhaiski in Russia ten years later, sent subordinates in steam-engined machines down inclined planes to achieve short powered hops, but neither of these attempts did much to advance either aerodynamics or the practical development of the airplane. The most elaborate full-scale attempt was made by Hiram Maxim (1840–1916), the American engineer in London famous for his invention of the machine gun. In the early 1890s Maxim spent a good part of his new fortune on the three-year development of a large biplane called the *Leviathan*, with a wingspan of 104 feet, powered by two 180 horsepower steam engines driving eighteen-foot propellers, and weighing nearly four tons. The design, however, was structurally weak, aerodynamically unsound, and lacked

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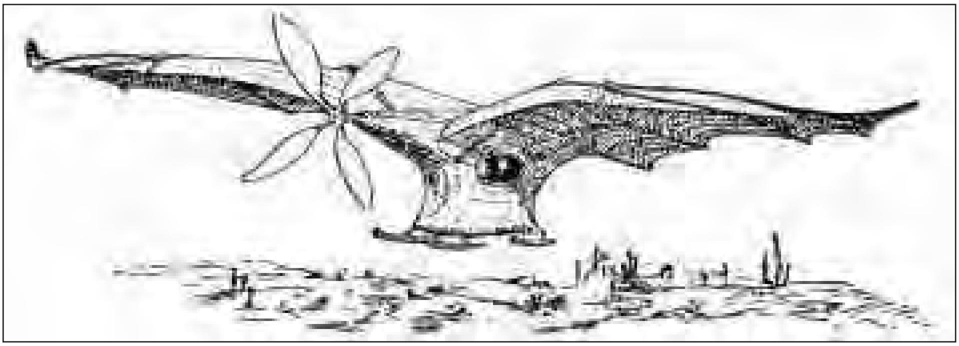
[14] See Iain McCallum, *Blood Brothers: Hiram and Hudson Maxim—Pioneers of Modern Warfare* (Chatham Publishers, 1999). Though this book focuses on weapons development, notably the "Maxim gun," it also covers Hiram S. Maxim's unsuccessful attempts to fly. For first-hand accounts of his experiments in flight, see Hiram S. Maxim, "Natural and Artificial Flight," *Aeronautical Annual* (1896); and "Aerial Navigation by Bodies Heavier Than Air," *The Aeronautical Journal* 6 (January 1902): 2–7. On Maxim's views of the early work of the Wright brothers, see "The Recent Experiments Conducted by the Wright Brothers," *The Aeronautical Journal* 10 (January 1906): 37–39.

effective controls. In 1894 the *Leviathan* crashed into a guide rail after briefly rising mere inches from the ground.[14] (See Document 1-6.)

Perhaps the closest thing to a successful powered flight of a full-scale heavier-than-air machine took place in France four years prior to Maxim's debacle, when Clement Ader's bat-winged *L'Eole* rose twelve inches off the ground in a "flight" of possibly 165 feet. But Ader's achievement was not in the same league as the historic Wright flight of 1903, no matter what French enthusiasts, then and now, may claim about it. All *L'Eole* really managed was a short, uncontrolled, powered hop and nothing more, which was fortunate given that the craft could not have sustained itself, as it had no tail and no method of lateral control, a formula for disaster.

A second Ader machine, the *Avion II*, was never completed, and a third in 1897, the *Avion III*, refused to leave the ground. Neither Du Temple, Mozhaiski, Maxim, nor Ader gave serious thought to how to control their aircraft in flight or what sort of skill the pilot would need to have. All they wanted to do was simply get a machine carrying a person into the air—and they did not truly accomplish even that.[15]



This is Ader's own perspective sketch of the *Éole*. The craft's wing spanned fourteen meters, with the length of the fuselage only 6.5 meters. The airframe weighed just over 175 kilograms. The bat-shaped wings could be folded for storage. Notice that at this point in time Ader considered fitting his plane with narrow caterpillar-like tracks rather than wheels. NASM File AA-006502-01

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[15] One can divide the works on Clement Ader into those that support the claim that his *L'Eole* successfully flew and those who refute or seriously qualify it. Among the works favorable to Ader are: Jacques May, *Ader* (Librairie Aeronautique, 1910); Georges de Manthe, *Clement Ader, sa vie, son oeuvre* (Edition Privat, 1936); Louis Castex, *L'homme qui donna des ailes au monde* (Editions Plon, 1947); Louis Castex, *Clément Ader, ou l'homme qui voulait se faire oiseau* (Presse de la cite, 1967); Raymond Cahisa, *L'aviation d'Ader et des temps héroiques* (Albin Michel, 1950); and Pierre Lissaargue, *Clément Ader, Inventeur d'avions* (Edition Privat, collection Bibliotheque Historique, 1990). For a more critical analysis, see Charles H. Gibbs-Smith, *Clément Ader, his claims and his place in history* (London/Science Museum, Her Majesty's Stationery Office, 1968). There are a number of Web sites that are devoted to Ader, notably that administered by Le musée Clement Ader at *http://le-village.ifrance.com/eole/squeletteader.html* There is information about the *Avion III* at *www.cham.fr/museum/revue/ref/r13a04_a.html*, which is maintained by the Musée des arts et métiers in Paris. Restored in the 1980s, the *Avion III* serves as one of the centerpieces of this museum.

Fortunately, there existed a third approach, one pursued earlier by Cayley himself, which investigated the problems of flight with full-scale manned gliders. This approach may have seemed the most foolhardy to some, involving jumping off hills and crashing nose first into the dirt; or the least sophisticated to others, because it did not involve steam engines or other kinds of power plants. But learning what type of wing shape and aerodynamic configuration best lifted a man into the air, and what sort of system and piloting maneuvers then allowed that man to control his flight in three dimensions, proved absolutely essential to solving what came to be recognized in the late 1800s, not just by aeronautical enthusiasts but by the engineering community as a whole, as "The Problem of the Century."[16]

Certainly, if there was one secret to the Wrights' success, it was that they learned to fly and control their airplane in glider form before they ever put power into it. But these brothers from Dayton, Ohio, were not the first to understand the fundamental importance of this human/machine interface as part of the invention of powered, heavier-than-air flight. Before the Wrights, there were two key individuals who provided technological breakthroughs in this regard that built significantly on the work started by Cayley and laid out a clear path to the future. They were two professional engineers: Otto Lilienthal (1848–1896) and Octave Chanute (1832–1910).

German mechanical engineer Otto Lilienthal made his living manufacturing small steam engines and marine foghorns in a little factory he operated on the outskirts of Berlin. With an unbridled passion for flying machines, Lilienthal conducted laboratory experiments and gathered data throughout the 1880s, trying to grasp and measure the way wings generated lift and generally searching for some real understanding of aerodynamic principles. Feeling that he had learned all he could from unmanned gliders, in 1891 he built and flew his first full-scale manned hang glider. From then until his death, the result of a gliding accident in October 1896, Lilienthal made about 2,000 glides.[17] (See Document 1-5.)

Lilienthal was not the first human being to fly. At least five other individuals had flown in gliders, beginning with Cayley's two hapless "test pilots." In France in the

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[16] This phrase was used by Augustus M. Herring as the title of a 1897 paper about his flying experiments with Octave Chanute.

[17] There is no satisfactory biography of Lilienthal in English, nor have any of the monographic treatments written in German ever been translated. In German, one should start with the works by Gerhard Halle: *Otto Lilienthal und seine Flugzeug-Konstructionen* (Dusseldorf: VDI-Verlag, 1962) and *Otto Lilienthal: die erste Flieger* (Dusseldorf: VDI-Verlag, 1976). Other studies of Lilienthal have been done in German by Werner Schwipps, Jutta Wegener, Karl-Dieter Seifert, and Stephan Nitsch. Anyone interested in Lilienthal's life who does not read German should start with the British translation of his autobiographical *Birdflight As the Basis of Aviation: A Contribution Towards a System of Aviation* (London, 1911). Tom Crouch's *A Dream of Wing* also offers an excellent chapter on Lilienthal's legacy in America, and the Smithsonian Institution Press/National Air and Space Museum published a booklet on *Otto Lilienthal and Octave Chanute: Pioneers of Gliding*, 1980.

1860s Jean Marie le Bris and Louis Mouillard both flew in gliders, while American John Joseph Montgomery flew three different hang gliders of his own design off a hill near San Diego harbor in the 1880s.[18] Universally, however, these predecessors became so frightened after making a few flights that they either refused to go up again or went back to the drawing board. Lilienthal was the first to persist. He continued making gliding flights, incorporating what he learned in his flights into the design of subsequent gliders. He even arranged for a cone-shaped hill to be built for him so he could fly his gliders no matter which direction the wind came from. He became the first man to really know how to fly, earning the nickname “The Flying Man” in accounts of his exploits. His machines were very well made and included both monoplane and biplane configurations. Some of his flights covered over 1,000 feet and lasted as long as fifteen seconds. Stories about his wonderful flights and piloting abilities appeared in newspapers and magazines around the world, and Lilienthal himself wrote a book and published a number of articles explaining his techniques and understanding of aerodynamic principles. These publications educated people about flight and motivated countless people, including the Wright brothers, to consider taking on the problem of flight for themselves.

On 8 October 1896, Lilienthal was gliding about fifty feet in the air when a strong gust of air caused his craft to nose up and stall. He was unable to regain control and his hang glider entered a terminal spin and crashed to the ground. Lilienthal suffered a broken back in the crash and died the next day in a Berlin hospital. The tragedy came as a terrible blow to all would-be aviators, for if not even the great “Flying Man” could avoid a fatal accident, how could anyone else expect to do it?

As much as Lilienthal had worked to address the matter of aerodynamic control in his gliders, what killed him was nonetheless insufficient control. In essence, Lilienthal’s method for control depended solely on the pilot shifting his weight under his glider in a type of “negative guidance” whereby he countered any unwanted aircraft movement by using his own body to shift the center of gravity of the whole machine. If the craft started to stall, all Lilienthal could do was throw his legs forward to try to pull the nose down and regain flying speed. Such a control system was fatally flawed because if the pilot did not shift his weight in just the right way and at just the right time his movements could actually make things worse. Furthermore, this system held little promise for purposes of subsequent development, because the ability of a pilot to control an airplane by shifting his own weight diminished rapidly as the size of the airplane increased.[19]

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[18] See Crouch, *A Dream of Wings*, p. 90 on Le Bris; pp. 20–21, 65–72, 83, 90, 176–178, and 228 on Mouillard; and pp. 87–100 and 307–309 on Montgomery.

[19] For a more detailed technical analysis of Lilienthal’s aerodynamics and the reasons behind his fatal crash in 1896, see Anderson, *A History of Aerodynamics*, pp. 138–164.

At the time of his death at age forty-eight, Lilienthal was thinking about propulsion systems and the problems that needed to be solved if he was to move forward to a powered airplane. There was no telling what he might have accomplished if he had survived.

Aviation's first martyrdom proved catalytic. Although pessimists took it as proof that humankind was not meant to fly, more optimistic individuals reacted to news of Lilienthal's demise with even greater determination to get up into the air. In America, Octave Chanute had just finished a successful summer season test flying a series of different gliders at the Indiana Dunes on the south shore of Lake Michigan, east of Chicago. By the time he heard the horrible news of Lilienthal's death, Chanute and a small group of young colleagues had evolved an advanced biplane glider incorporating a new approach to the problem of airplane stability. This glider proved to be "the most significant and influential aircraft of the pre-Wright era."[20]

Given the depth of Chanute's talents as an engineer and his encylopedic awareness of what was being tried aeronautically in America and around the world, it is not surprising that his 1896 glider represented the most advanced state of the art of that time. He emigrated to the United States with his family from France in 1838 when he was six, by the mid-1870s Chanute was one of the best known and most respected civil engineers in the country, renowned for his bridges and railway structures. At the peak of his career, Chanute was chief engineer of the Erie Railroad and president of the American Society of Civil Engineers and the engineering section of the American Association for the Advancement of Science. Interested in flight since his childhood, in the 1880s Chanute entered a period of semiretirement that allowed him to devote his energies to the problems of "aeronautical navigation." He initiated a regular correspondence with dozens of people

An accomplished railroad engineer, when Octave Chanute (1832–1910) turned his attention to the design of flying machines in the 1880s and 1890s and began corresponding with others with similar interests, he inadvertently created the first international aviation community, a creation essential to the successful invention of the airplane. SI Negative A-21147-B

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[20] See Crouch, *A Dream of Wings*, pp. 175–202, and Anderson, *A History of Aerodynamics*, pp. 192–197.

all over the world who were interested in flying machines, including prominent members of the Aeronautical Society of Great Britain, the most important of the early organizations devoted to the study of flight. He also published what he learned, starting with a series of critically important essays on "Progress in Flying Machines," that appeared in twenty-seven installments in the *Railroad and Engineering Journal*, beginning in October 1891, and was reprinted three years later as a single volume.[21]

Not satisfied with the important role he filled in the aeronautics community, by serving as an active clearinghouse for aeronautical information and by making aeronautics a respectable concern for engineers, Chanute also wanted to test his own ideas about flying. In 1888, he laid out an idea for the design of a flying machine combining features of an airship and an airplane. Abandoning this hybrid, he quickly turned to manned gliders, unveiling a bold new design in late 1894 that was to serve as a test bed for his own novel concepts. Chanute's original glider design featured an approach to control problems that was significantly different from Lilienthal's negative or reactive stability, in which the pilot shifted his body weight. The veteran engineer envisioned instead a system of automatic stability that did not require much from a pilot—in essence, he wanted the structure itself to stabilize the airplane. To do this he proposed an arrangement of multiple wings set in tandem, suggesting as many as four pairs of tandem biplane wings, or sixteen in all. Such a redundant structure, he calculated, would reduce the movement of the center of pressure on each surface by adjusting automatically to shifts in the airplane's center of gravity. The "operator," using Chanute's term, "need only intervene when he wants to change direction, either up or down, or sideways." Furthermore, Chanute added yet another element of automatic stability by designing each one of his tandem wings so that they could rotate slightly. When hit by a strong gust of wind, explained Chanute, "the wings are blown backward . . . the aeroplane tips slightly to the front, thus decreasing the angle of incidence,

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[21] It is truly amazing that there is yet no published biography of Chanute, considering how important he was to not only the development of flight technology but also of American engineering generally. The seminal figure in Crouch's *A Dream of Wings* is unquestionably Chanute rather than Langley or the Wrights. The main collection of Chanute Papers rests in the Manuscript Division of the Library of Congress. The Denver (CO) Public Library holds a large collection of Chanute materials assembled by Ms. Pearl I. Young, who for many years, starting in the early 1920s, worked for the National Advisory Committee for Aeronautics (NACA) at Langley Field, Virginia. Young grew fascinated with the early history of flight, particularly Chanute's contributions. At the Denver Public Library, one will find Young's *Bibliography of Items About Octave Chanute*, *Complete Writings of Octave Chanute*, the *Chanute-Mouillard Corespondence*, and two essays by Young: "Octave Chanute and New England Aeronautics" and on "The Contributions of Octave Chanute, 1832–1910." Anyone reconstructing Chanute's story would also want to see Marvin McFarland, ed., *The Papers of Wilbur and Orville Wright, Including the Chanute-Wright Letters and Other Papers of Octave Chanute*, recently reissued by McGraw Hill, in January 2001.

so that the aggregate 'lift' is diminished." When the wind subsided, a coil spring pulled the wings forward to return to "their normal adjusted position."

Flight trials with different gliders at the Indiana Dunes in the summer of 1896 showed Chanute the error of at least some of his concepts and stimulated a major re-rigging of the multiplane glider, dubbed the *Katydid* for its insect-like appearance. The redundant wings caused far too much lift, and with some surfaces creating more lift than others this caused dangerous imbalances. No one, including the sixty-four-year-old Chanute, wanted to risk flying it at first, so the glider was initially flown tethered like a kite. After numerous design changes, a young fellow engineer named Augustus M. Herring made a series of flights in the glider, the longest a glide of eighty-two feet. Word of the flights spread to neighboring Chicago and reporters from a number of newspapers and the national wire services trekked to the southeast shore of Lake Michigan to observe the action. Not wanting the attention, Chanute and his men packed up and went back to a workshop in Chicago. There they applied what they had learned to the design of a brand-new glider.

One should view the work of Chanute and Herring as collaborative. In the truss bracing used on the gliding machines, one detects Chanute's engineering skills; in the cruciform tail and overall design schemes lay Herring's major contributions. In terms of the fundamental aerodynamics, it is unclear whose understanding was superior. Chanute knew much more mathematics, but Herring's physical intuition was in many ways sharper. Here, Herring stands ready to take off in his two-surface gliding machine in 1896. SI Negative A-30907-H2

The result was a simple, dramatic improvement: a far simpler, triplane glider made with a single rigid box frame. Much less cumbersome than the *Katydid*, it flew at the Indiana Dunes in 1896 as the first modern aeronautical structure, of which Chanute's team had evaluated the forces, figured the strengths, and precisely calculated the performance. Looking at its external bracing of crossed diagonal wires and upright struts, one could see the face of airplane structures to come, beyond the World War I era. The new glider also featured an effective combination of horizontal and vertical tail surfaces put into a cruciform (or cross) shape. That such a tail, appropriately positioned, enhanced the pitch stability of an airplane was understood as far back as Cayley. But Chanute and his men were not satisfied with just the advantages of a fixed tail. Under young Herring's direction, they made their tail moveable (or "regulating") so that it could react positively to gusts, keeping the airplane straight and level. At the suggestion of one of Chanute's men, carpenter and electrician William Avery, the triplane soon metamorphosed into a biplane simply by removing the bottom wings. The resulting performance of this "two-surface" machine was phenomenal—the equal of anything flown by Lilienthal and, in its design features, significantly more progressive. (See Document 1-9.)

One would think that Chanute and his men would have followed up immediately on their sensational success; after all, it was the season-after-season buildup of knowledge and know-how that proved so essential to the Wright brothers' eventual achievement at Kitty Hawk. But it did not happen this way with Chanute. When the summer of 1896 ended, so did his experimental flying. The following summer he and his men did not return to the Indiana Dunes. Herring went to explore the possibility of powered hang gliders in the vicinity of St. Joseph, Michigan.[22] Chanute kept collecting aeronautical information, writing articles for publication and corresponding with dozens of others in pursuit of the dream. Perhaps due to his age, he did not persist with flight trials. It was certainly clear from his 1897 article, "Recent Experiments in Gliding Flight," that he was not sure that powered flight was imminent. In fact, without Herring around to push him in bold new technological directions and describe what it was like to pilot a plane, Chanute reverted to old ideas, notably the absolute priority of automatic stability. He set aside the world's most advanced glider and reverted to the design of yet another monstrously complex multiplane.

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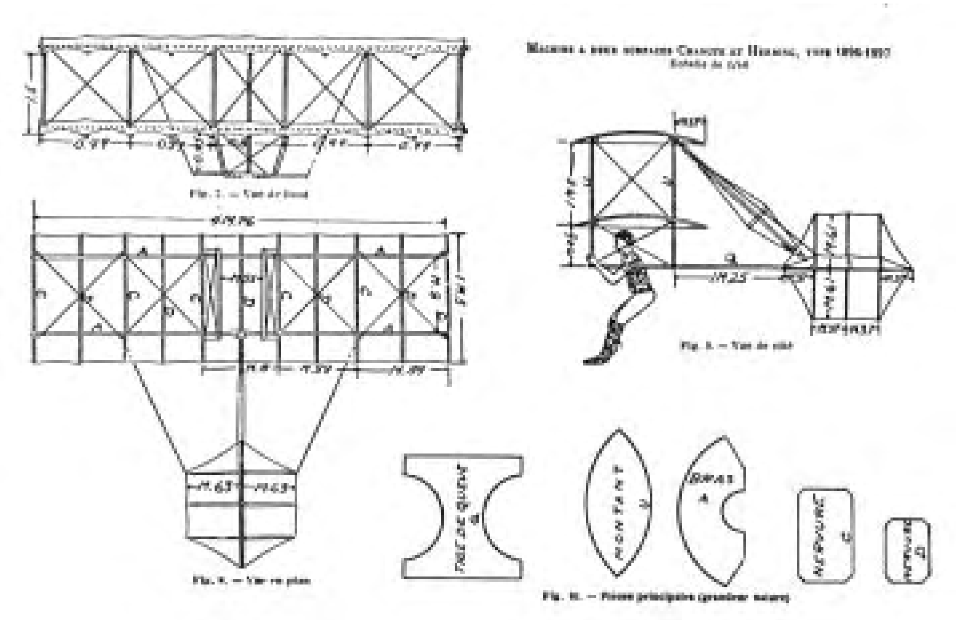
[22] On Herring, see Crouch, *A Dream of Wings*, pp. 203–223. His ancestors to this day continue to believe that Augustus Herring made a powered flight in an airplane before the Wrights; see, for example, Lou Mumford, "Family claim historical accounts of first flight aren't 'Wright,'" *South Bend Tribune*, 11 October 1998. This story can be accessed at *http://www.southbendtribune.com/98/oct/101l98/local_ar/117176.htm*

The year 1896 was thus a pivotal time in the history of aeronautics, and in that year two brothers in Ohio, self-trained designers without high-school diplomas, turned their attention away from their print shop and bicycle business in order to take a crack at inventing the airplane. These two unique individuals, Wilbur Wright (1867–1912) and Orville Wright (1871–1948), brought not only fresh perspectives and new energy to the fledgling field of flight research, but also one of the most remarkable collaborations of genuine talent in the history of invention. Writing shortly before his death, Wilbur Wright offered the following insight into the fruitful relationship that sustained them in their work and jointly magnified their respective abilities:

> From the time we were little children my brother Orville and myself lived together, played together, worked together and, in fact, thought together. We usually owned all of our toys in common, talked over our thoughts and aspirations so that nearly everything that was done in our lives has been the result of conversations, suggestions and discussions between us.[23]



Views of Herring's three-surface glider from 1896. Smithsonian Institution

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[23] Wilbur Wright, *Papers of Wilbur and Orville Wright*, 3 April 1912.

Without this creative synergism between them, it is impossible to imagine them inventing the airplane.

Another thing that is very important to understand about the Wright brothers is how they followed their own path in pursuit of the invention of the airplane. Not that they ignored all the work of others. In fact, one of the first things they did after deciding to address the problems of flight in 1896 was collect all the aeronautical information that was available. First, they exhausted the collections at the Dayton Public Library, then they wrote to Chanute, who was kind enough to write back and advise them to learn how to glide before they tried a powered flight. Aware of Langley's work, they also wrote to the Smithsonian Institution, which replied by sending them a number of articles, including works by both Langley and Lilienthal. (See Document 1-8.) But once they had digested this knowledge, they were not to be boxed in by it. Instead they approached technical problems from their own unique perspective and came up with equally unique solutions.

The most significant example of this crucial characteristic of the Wrights concerned the decisive matter of control. They did something that no airplane pioneer up to that time had done: they isolated the control of an airplane as the main problem that needed attention. Lilienthal, Chanute, and Langley had obviously designed wings that were able to lift machines into the air, and while there were many aerodynamic improvements yet to come regarding lift, the invention of the airplane did not have to wait for them. Neither did the problem of propulsion overly concern the Wrights, because all over the country mechanics were developing small, lightweight internal combustion engines for automobiles and motorcycles. When they needed an engine, the Wrights thought, the technology would be there. That left the issue of pilot control, the very problem that had killed Lilienthal and the aspect on which nobody else had placed such clear priority.[24]

The Wrights' thought process about control ran against the grain of current thinking, and in hindsight it appears that this was precisely why they eventually succeeded where others failed. Because Lilienthal, the great "Flying Man," had died because not even he was able to control his aircraft, conventional wisdom reasoned that what was needed was an inherently stable machine—for example, the line of thinking followed by both Chanute and Langley. The Wrights turned this logic on its head. They cared little about a flying machine's stability, but focused from the start on the critical aspect of making it controllable. No doubt,

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[24] For an extremely insightful analysis of how the Wrights defined their problems and solved them, see Peter L. Jakab, *Visions of a Flying Machine: The Wright Brothers and the Process of Invention* (Washington, DC: Smithsonian History of Aviation Series, 1990). Jakab takes his reader step by step through the thought processes that led the Wright brothers to their successful invention of the airplane.

their intimacy with bicycle technology influenced them greatly in this matter, for a bicycle is an inherently unstable machine. The Wrights felt that the airplane would need to be the same sort of dynamically interactive device as a bicycle: unstable on its own but completely controllable and virtually automatic in the hands of an experienced operator. The key was designing control features into the airplane that a pilot could easily and effectively manage.

In 1899, they took their first important step toward that goal by experimenting in Dayton with what came to be known as their "wing-warping kite." The basic idea was a mechanism that allowed the operator to control the kite in its roll axis, considered the most challenging axis of motion to master in a flying machine. The Wrights developed a set of controls that enabled them to induce a helical twist across the wing surface that increased the lift on one side and decreased it on the other, thereby changing the aerodynamic balance of the machine. They did not realize at the time that others, like Yale physics instructor Edson Fessendon Gallaudet, had flown large wing-warping kites already. Instead they came up with the idea on their own, based on their observations of buzzards employing a similar technique, regaining "their balance, when partly overturned by a gust of wind, by a torsion of the tips of the wings." Starting with the wing-warping kite of 1899, one can follow the links in an evolutionary chain of ever more sophisticated flying machines, right up to the historic Wright *Flyer* of 1903.[25]

In 1900, the brothers built their first full-scale machine, a biplane glider large enough to carry a person. Though it resembled the Chanute-Herring glider of 1896 in many of its structural features, the Wright glider of 1900 differed in that it had a large elevator (or canard) set directly in front of the lower wing. This horizontal surface moved up and down and was the Wrights' way of managing pitch control and preventing nose dives in case of stalls. They built this machine in accordance with the tables of aerodynamic coefficients compiled by Otto Lilienthal, and estimated the amount of wing surface needed to lift the weight of their fifty-pound machine plus a pilot into the air. It was this machine that first flew at Kitty Hawk, on the windy Outer Banks of North Carolina, for two weeks in the early autumn of 1900, during what was to the Wrights a short "scientific vacation." (See Document 1-10.)

What concerned the Wright brothers most about their first season of flight tests at Kitty Hawk was the fact that their glider simply did not generate the lift

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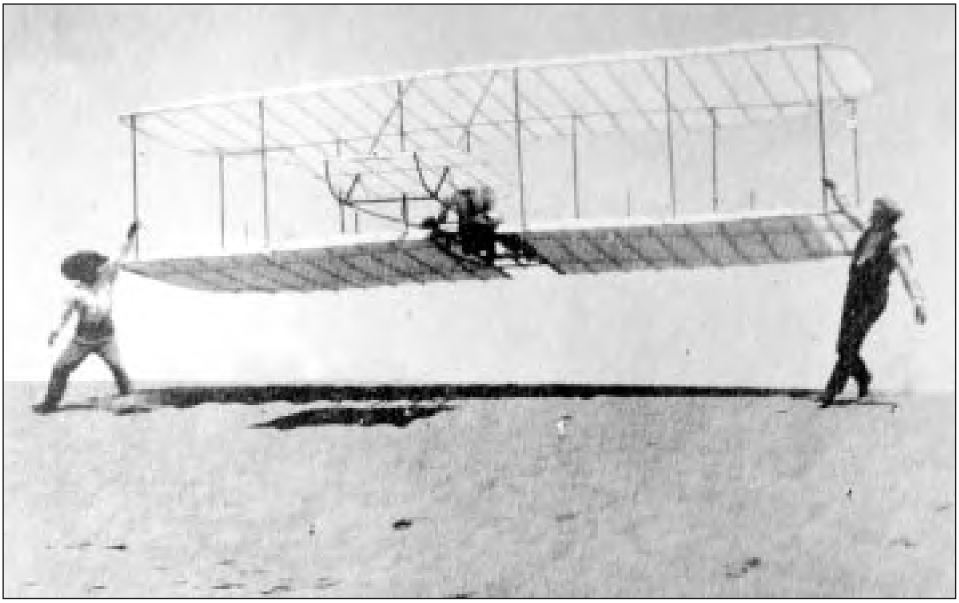
[25] The analysis presented in this chapter of the evolutionary technological development from the Wrights's kites to their historic 1903 *Flyer* is based largely on the narrative and interpretations of Tom Crouch, the world's preeminent Wright brothers' scholar and author of the prizewinning *The Bishop's Boys*. As with other sections of this chapter, a more detailed analysis of the Wright's aerodynamics relied on Anderson's *A History of Aerodynamics*, pp. 201–243. Of course, there are many more works on the Wrights, some of which were consulted in preparation of this chapter.

that their calculations indicated it should. Something was obviously wrong with their calculations. While they did manage to fly their machine as a kite, and finally on the last day to make a few very short free glides, overall, the results were deflating. They tried the glider "with tail in front, behind, and every other way," but with no real success. After their return to Dayton, Orville Wright noted of his brother's mood that, "When we got through Will was so mixed up he couldn't even theorize. It has been with considerable effort that I have succeeded in keeping him in the flying business at all."

The flying season in the next year proved equally frustrating. The machine they took to Kitty Hawk in the summer of 1901 was a good deal larger than their previous glider—in fact, with a twenty-two-foot wingspan, it was the largest glider anyone had ever tried to fly. Basically, they hoped to solve the problem of insufficient lift by creating more wing surface and by increasing the curvature or camber of the wings, but none of these changes worked. The glider produced much less lift than predicted, and was plagued with serious control problems. The disappointment of this season was a critical point for the Wrights, for they recognized that if they failed to solve these lift problems, sooner or later one of them was going to be killed.

Rather than giving up, though, the two brothers reevaluated their basic assumptions and realized that something was seriously wrong with the scientific tables on which they were basing their calculations. Back in Dayton they



The Wrights experienced frustration after frustration in their 1901 glider flying season at Kitty Hawk. But in battling through their many problems and failures they learned lessons extremely valuable to their successful invention of the airplane two years later. Assistants Dan Tate and Edward Huffaker launch Wilbur in his 1901 glider. SI Negative 84-12143 embarked on a new course to conduct their own basic research and find out what was wrong with the Lilienthal data. They constructed a small six-foot-long wind tunnel in the back room of their bicycle shop and tested a whole new range of airfoil shapes, about two hundred in all. They also experimented with changes in aspect ratio, with different wing tip shapes, and with varying gaps between the wings of a biplane. At the conclusion of this intense period of original investigation they sat on a wealth of new aerodynamic information that would quickly enable them to solve the problems that had hitherto blocked their progress. (For material related to the invention of the wind tunnel by British engineer Francis H. Wenham in the early 1870s, see Document 1-3.)

The resulting 1902 Wright glider flew beautifully. Besides incorporating new wing shapes that gave them the lift they wanted, the airplane also featured the Wright's most recent significant discovery, the benefits of a moveable tail rudder linked directly to the wing-warping mechanism, enabling the pilot to manage both controls easily and in concert. With this machine the Wrights really learned to fly. They experienced a magnificent season of soaring and making banks, turns, and recoveries in every direction. Appropriately, it was this machine that these intrepid aviation pioneers patented, not their powered airplane of 1903. They had set out to invent an airplane that was controllable in the air, and by the end of 1902 they had done it. (See Document 1-11.) All that was left was to add a propulsion system to the airplane.

They did encounter one other aerodynamic problem related to propulsion in the design of an effective propeller. In the beginning this was a problem that the Wrights had not anticipated, since they believed it would be sufficient to find a good marine propeller and simply adapt it for use in the air. After the surprises of 1900 and 1901, however, they did not want to take any chances, and determined to calculate precisely the performance requirements of an aviation propeller. In what proved to be another case of their marvelous talent for applying clear engineering logic to a problem that others had either overlooked or ignored, they reasoned that the propeller was no more than a rotating wing—and as a result of their wind tunnel tests they knew a lot about wings. For a propeller, therefore, they selected an appropriate airfoil shape from their own aerodynamic tables, with the camber best suited for the speed at which their machine would be flying through the air, and carved their own propeller blades. "The result," according to aviation historian and Wright biographer Tom Crouch, "was the world's first true aircraft propeller, a device whose performance could be precisely calculated."[26]

The story of the design of the homemade engine for their airplane, although interesting, does not concern aerodynamic development, save perhaps in one

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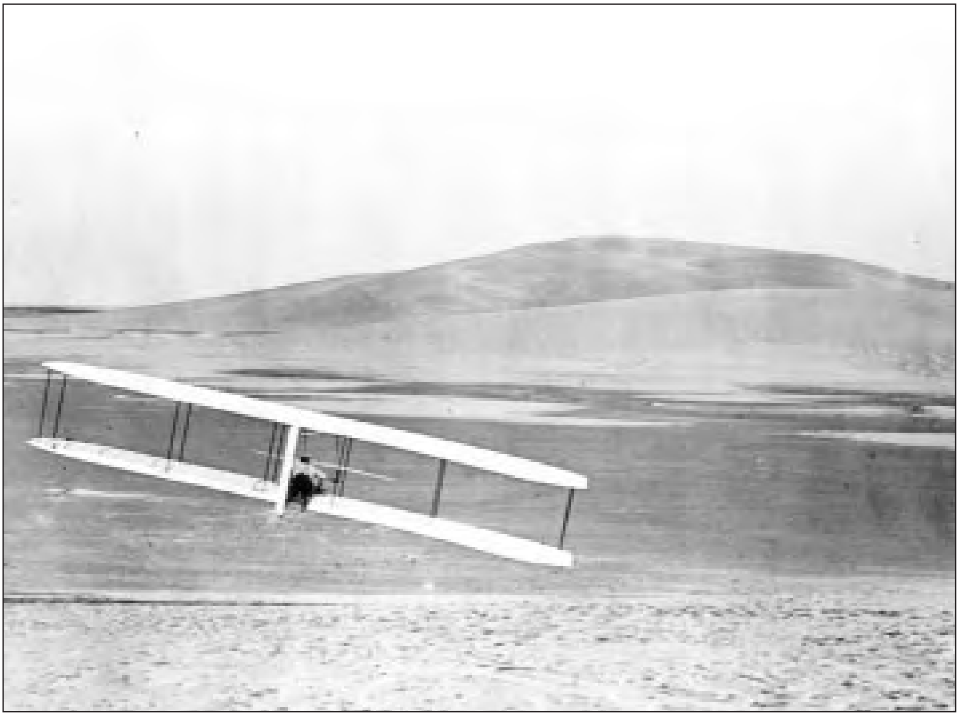
[26] Crouch, *A Dream of Wings*, p. 294.



This photo from 24 October 1902 shows Wilbur maneuvering the glider (fitted with a single moveable vertical rudder) through a gentle right turn. By the end of the 1902 season the brothers had taken their machine through a remarkable series of banks, turns, and recoveries. SI Negative A-43395-A

respect. The Wrights wanted an engine that provided just enough power to meet their calculated requirements, with no extraneous weight or extra resources devoted to redundant capability. They knew exactly from their calculations how much power it would take to get them into the air, and once they had an engine that could deliver that precise amount of power—a mere 12 ½ horsepower from a 200-pound motor—they concluded engine development right there. This efficient approach contrasted sharply with the design overkill of Professor Langley, who had no practical idea how much power it was going to take to get his aerodrome up into the air, and wasted inordinate time, money, and energy on the development of an overly powerful fifty-two horsepower engine while neglecting the more critical areas of aerodynamic structure and control.

The Wrights spent nearly three months at Kitty Hawk in late 1903 before they were ready to try their first powered flight. Besides assembling, testing, and rigging their new machine, they also practiced extensively with the 1902 glider. After a false start on 14 December using a downhill launch rail that produced a take-off speed too high for effective control, Orville and Wilbur were ready to take

The secret of the Wrights' success was that they learned to fly and control the airplane in glider form before they put power into it. They learned a great deal about the use of aerodynamic surfaces to control the airplane in a satisfactory manner, and together with the experience they had as pilots, they were able to fly the plane without too much difficulty when they did put power into it. That did not preclude a false start or two, however. In attempting to fly on 14 December 1903, pictured here, Wilbur nosed up too rapidly and spun back into the ground, damaging the front horizontal rudder, lower rear wing spar, and skid. SI Negative A-38618-A

to the air with their airplane from a level launch rail on 17 December 1903. The brothers made a total of four flights that day. The first flight, made by Orville, covered only 120 feet in a little over twelve seconds. The last, piloted by Wilbur, covered 850 feet and lasted nearly a full minute. The telegram sent by Orville to his father telling the story of this historic achievement is appropriately the destination document for this chapter, marking the realization of humankind's long-sought goal of sustained powered flight. (See Document 1-13.)

The Wrights went home to Ohio and in 1904 and 1905 built two new airplanes and tested them as they flew over a cow pasture known as Huffman Prairie, today a part of Wright-Patterson Air Force Base. Their airplanes got better, as did their piloting skills, and by the end of 1905 they were making flights of forty-five minutes, flying repeated circles and other maneuvers over their "air field." The world surprisingly took little notice of these truly revolutionary achievements. News of their initial flights produced only a scattering of stories, and those at

Huffman Prairie a brief flurry of interest. The press soon forgot about the Wrights and allowed them to continue their refinement of the airplane undisturbed.

The Wright brothers were masters at thinking visually, using what is sometimes called "the mind's eye," and translating abstractions into hardware. Their flying machine had to "look right," in both an aesthetic and a practical sense, before they were convinced that they had everything right in their invention of the airplane. Certainly everything looks right in this picture of the 1905 *Flyer* in flight above Huffman Prairie near Dayton. SI Negative No. A-317-B

Part of the explanation for the lack of greater publicity was due to the ignominious public failure of Professor Langley's full-size manned *Great Aerodrome* in 1903. Using the familiar launch platform atop the houseboat on the Potomac River, in October Langley tried to fly his craft with an assistant aboard, but the machine crashed in the attempt. A second climactic public attempt to fly the machine on 8 December, a mere nine days before the Wright's epochal achievement in isolated Kitty Hawk, also resulted in utter failure. These tests were covered by a corps of newspapermen and photographers who scathingly reported the failure of a government program that had spent $75,000 (the equivalent of about $1.5 million today) to produce an aircraft whose flying characteristics were compared to "a handful of mortar" or "a block of cement." The nation's leading scientist was humiliated and became the target of public scorn and ridicule. With Langley's disgrace capturing the headlines, the press took little notice of the work of two obscure inventors from Dayton.

The coincidence of the Wrights' success and Langley's failure in the month of December 1903 highlights a factor that proved to be critically important, not just to the invention of the airplane, but for the entire course of aerodynamic research and development in the twentieth century. The Wrights were engineers, not scientists—practical-minded and realistic men, who time and time again found simple solutions to what turned out to be key problems. Langley, the scientist, on the other hand, proved an inept technologist because he could not turn what he knew into an accomplishment of his goal. The Wrights had clear vision and demonstrated their genius as creative technologists par excellence.

What they also did, once the world and particularly the different fields of engineering became aware of what the Wrights achieved through their systematic

What the public acknowledged about powered flight after December 1903 was the ignominious, highly publicized failure of Samuel P. Langley's *Great Aerodrome*, not the successful flights of the Wrights, which few knew about and even fewer believed had actually happened. Here, Langley's *Great Aerodrome* rests on its catapult atop a houseboat, ready for flight, on 8 December 1903. SI Negative No. A-18789

engineering approach, was help prepare the ways and means for the future evolution of the airplane. The form of the airplane as the Wrights conceived it in 1903 was only the beginning—a fact they themselves perhaps did not appreciate enough. In all respects, save the decisive one of control, the performance of their 1903 airplane was highly marginal. As one analyst has observed, it really amounted to "the first aeronautical 'proof of concept' design, and could not be used for anything else, including repeated flying."[27] (The historic 1903 *Flyer* was never flown again after the 17 December flights.) If the airplane was to ever become truly practicable, its technology had to improve dramatically and become much more capable and versatile.

Over the course of the next decades, the airplane would experience a number of "reinventions" in many ways as remarkable as the original Wright invention. And in all of them, a systematic engineering approach similar to that of the Wrights proved critical. As much as the invention of the original airplane itself, this was the Wrights' legacy.

Ironically, the country's first civilian aeronautical research facility, under the auspices of the National Advisory Committee for Aeronautics (NACA), was named after Langley, not the Wrights. The NACA Langley Memorial Aeronautical Laboratory

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[27] E. K. Liberatore, *Helicopters before Helicopters* (Malabar, FL: Krieger Publishing Co., 1998), pp. 158–159.

Observers compared the aerodynamic qualities of Langley's *Great Aerodrome*, seen here crashing into the Potomac an instant after launch, to "a sackful of mortar." SI Negative No. A-18853 was founded near Hampton, Virginia, in 1917, eleven years after the death of the discredited Smithsonian scientist. Fortunately, for the sake of the aeronautical research investigations that took place at the NACA and for the positive impact that NACA research would have on the nascent U.S. aircraft industry, the influence of Langley's devoted colleagues and friends in the Smithsonian Institution (enough to get the first NACA laboratory named after him) was not compelling enough to stamp the character of the once-fledgling organization with Langley's unsuccessful approach. The researchers at Langley followed the technological lead and the model of the Wrights. For the study of aerodynamics to affect flying machines, and for flying machines to indeed change the world, the ways of the Wrights ruled the day.

European skeptics watched in awe as Wilbur Wright unveiled what his airplane could do during his trip to France in 1908. In one sensational flight made on 13 August 1908 above the Les Hanaudières race track near Le Mans, France, Wilbur made seven circles of the track in 8 minutes, 13 ⅖ seconds. Four months later, in December, he made two stunning nonstop flights across France—one a distance of 99 kilometers in 1 hour, 54 minutes, 22 seconds, and the other of 124.7 kilometers in 2 hours, 20 minutes. SI Negative No. A-42962-A

The Documents

Document 1-1(a–c)

(a) George Cayley, "On Aerial Navigation," Part One, *Nicholson's Journal*, November 1809.

(b) George Cayley, "On Aerial Navigation," Part Two, *Nicholson's Journal*, February 1810.

(c) George Cayley, "On Aerial Navigation," Part Three, *Nicholson's Journal*, March 1810.

Note: All three parts were republished in James Mead, ed., *The Aeronautical Annual* 1, (Boston: W.B. Clarke & Co., 1895): 16–48.

There is perhaps no more important individual paper in the entire history of aeronautics than Sir George Cayley's famous three-part treatise "On Aerial Navigation," written in 1809. In this paper, reproduced here in its entirety, Cayley reported all his findings on airplane aerodynamics and provided a thorough explanation of the potential of a fixed-wing flying machine. In addition to outlining a systems approach to solving the problems of lift, control, and power, he demonstrated a clear understanding of the advantages of wing camber, the first person ever to appreciate the subtleties of the effects of curvature on lift. He did not fail to address the matter of drag, and in various passages of his treatise he even expressed a modern concept of aerodynamic "streamlining," before the term was invented. The total effect of his presentation was momentous, although it may not come across as such to the modern reader who takes for granted the basic operating principles of the airplane. Cayley's delineation of the form and function of the airplane, based on his intuitive genius and solid experimental approach, is classic.

Cayley's document was written in three parts for *Nicholson's Journal*, and is often referred to as the "Triple Paper."

Document 1-1(a), George Cayley, "On Aerial Navigation," Part One, 1809.

BROMPTON, Sept. 6, 1809.

SIR, I observed in your Journal for last month, that a watchmaker at Vienna, of the name of Degen, has succeeded in raising himself in the air by mechanical means. I waited to receive your present number, in expectation of seeing some farther account of this experiment, before I commenced transcribing the following essay upon aerial navigation, from a number of memoranda which I have made at various times upon this subject. I am induced to request your publication of this essay, because I conceive, that, in stating the fundamental principles of this art, together with a considerable number of facts and practical observations, that have arisen in the course of much attention to this subject, I may be expediting the attainment of an object, that will in time be found of great importance to mankind; so much so, that a new era in society will commence, from the moment that aerial navigation is familiarly realized.

It appears to me, and I am more confirmed by the success of the ingenious Mr. Degen, that nothing more is necessary, in order to bring the following principles into common practical use, than the endeavours of skilful artificers, who may vary the means of execution, till those most convenient are attained.

Since the days of Bishop Wilkins the scheme of flying by artificial wings has been much ridiculed; and indeed the idea of attaching wings to the arms of a man is ridiculous enough, as the pectoral muscles of a bird occupy more than two-thirds of its whole muscular strength, whereas in man the muscles, that could operate upon wings thus attached, would probably not exceed one-tenth of his whole mass. There is no proof that, weight for weight, a man is comparatively weaker than a bird; it is therefore probable, if he can be made to exert his whole strength advantageously upon a light surface similarly proportioned to his weight as that of the wing to the bird, that he would fly like the bird, and the ascent of Mr. Degen is a sufficient proof of the truth of this statement.

The flight of a strong man by great muscular exertion, though a curious and interesting circumstance, in as much as it will probably be the first means of ascertaining this power and supplying the basis whereon to improve it, would be of little use. I feel perfectly confident, however, that this noble art will soon be brought home to man's general convenience, and that we shall be able to transport ourselves and families, and their goods and chattels, more securely by air than by water, and with a velocity of from 20 to 100 miles per hour.

To produce this effect, it is only necessary to have a first mover, which will generate more power in a given time, in proportion to its weight, than the animal system of muscles.

The consumption of coal in a Boulton and Watt's steam engine is only about 5 ½ lbs. per hour for the power of one horse[power]. The heat produced by the combustion of this portion of inflammable matter is the sole cause of the power generated; but it is applied through the intervention of a weight of water expanded into steam, and a still greater weight of cold water to condense it again. The engine itself likewise must be massy enough to resist the whole external pressure of the atmosphere, and therefore is not applicable to the purpose proposed. Steam engines have lately been made to operate by expansion only, and those might be constructed so as to be light enough for this purpose, provided the usual plan of a large boiler be given up, and the principle of injecting a proper charge of water into a mass of tubes, forming the cavity for the fire, be adopted in lieu of it. The strength of vessels to resist internal pressure being inversely as their diameters, very slight metallic tubes would be abundantly strong, whereas a large boiler must be of great substance to resist a strong pressure. The following estimate will show the probable weight of such an engine with its charge for one hour.

	lb.
The engine itself from 90 to	100
Weight of inflamed cinders in a cavity presenting about 4 feet surface of tube:	25
Supply of coal for one hour:	6
Water for ditto, allowing steam of one atmosphere to be 1/1800 the specific gravity of water:	32
[Total weight in pounds:]	163

I do not propose this statement in any other light than as a rude approximation to truth, for as the steam is operating under the disadvantage of atmospheric pressure, it must be raised to a higher temperature than in Messrs. Boulton and Watt’s engine; and this will require more fuel; but if it take twice as much, still the engine would be sufficiently light, for it would be exerting a force equal to raising 550 lb. one foot high per second, which is equivalent to the labour of six men, whereas the whole weight does not much exceed that of one man.

It may seem superfluous to inquire farther relative to first movers for aerial navigation; but lightness is of so much value in this instance, that it is proper to notice the probability that exists of using the expansion of air by the sudden combustion of inflammable powders or fluids with great advantage. The French have lately shown the great power produced by igniting inflammable powders in close vessels; and several years ago an engine was made to work in this country in a similar manner, by the inflammation of spirit of tar. I am not acquainted with the name of the person who invented and obtained a patent for this engine, but from some minutes with which I was favoured by Mr. William Chapman, civil engineer in Newcastle, I find that 80 drops of the oil of tar raised eight hundred weight to the height of 22 inches; hence a one horse power many consume from 10 to 12 pounds per hour, and the engine itself need not exceed 50 pounds weight. I am informed by Mr. Chapman, that this engine was exhibited in a working state to Mr. Rennie, Mr. Edmund Cartwright, and several other gentlemen, capable of appreciating its powers; but that it was given up in consequence of the expense attending its consumption being about eight times greater than that of a steam engine of the same force.

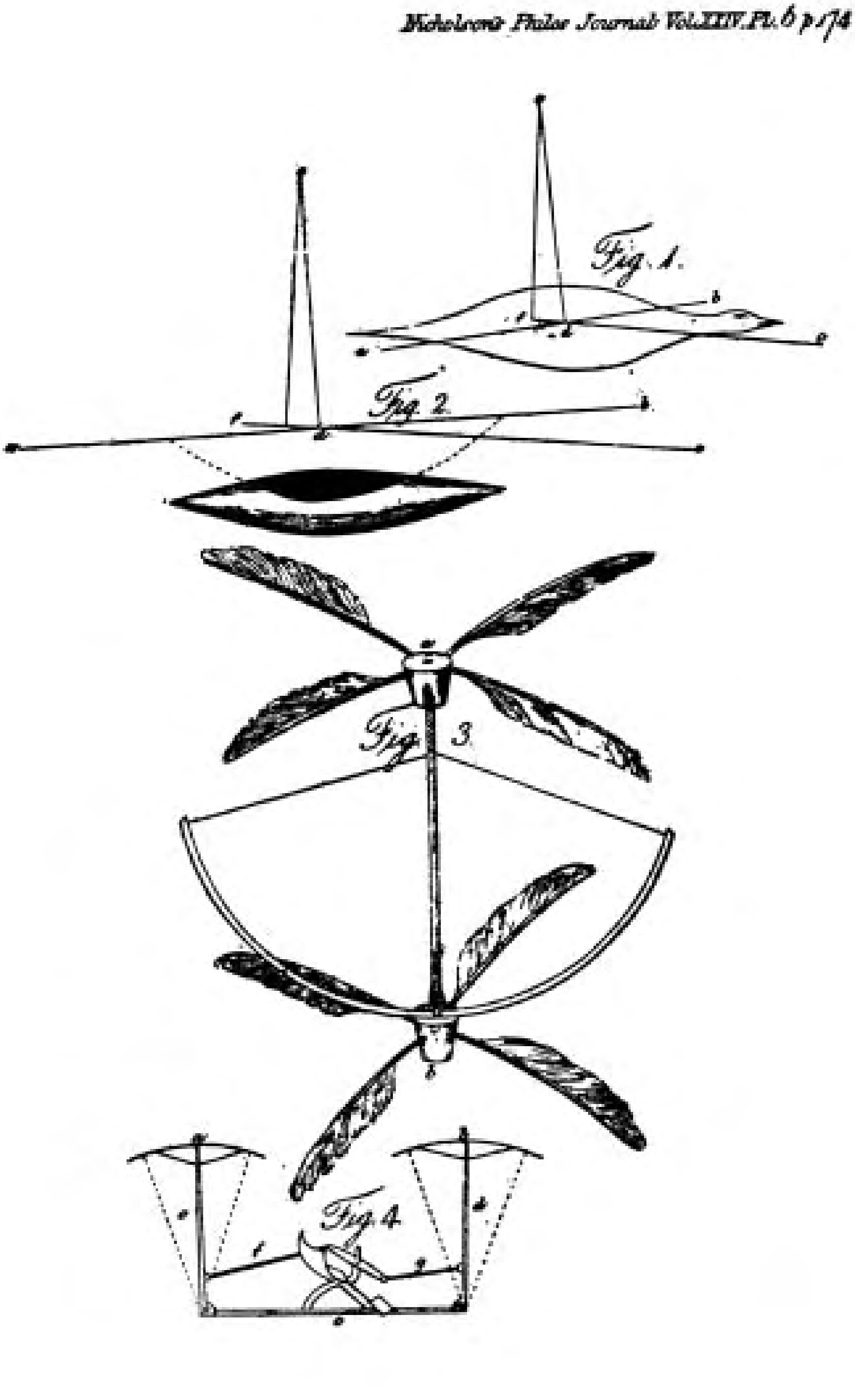
Probably a much cheaper engine of this sort might be produced by a gas-light apparatus, and by firing the inflammable air generated, with a due portion of common air, under a piston. Upon some of these principles it is perfectly clear, that force can be obtained by a much lighter apparatus than the muscles of animals or birds, and therefore in such proportion may aerial vehicles be loaded with inactive matter. Even the expansion steam engine doing the work of six men, and only weighing equal to one, will as readily raise five men into the air, as Mr. Degen can elevate himself by his own exertions; but by increasing the magnitude of the engine, 10, 50, or 500 men may equally well be conveyed; and convenience alone, regulated by the strength and size of materials, will point out the limit for the size of vessels in aerial navigation.

Having rendered the accomplishment of this object probable upon the general view of the subject, I shall proceed to point out the principles of the art itself. For the sake of perspicuity I shall, in the first instance, analyze the most simple action of the wing in birds, although it necessarily supposes many previous steps. When large birds, that have a considerable extent of wing compared with their weight, have acquired their full velocity, it may frequently be observed, that they extend their wings, and without waving them, continue to skim for some time in a horizontal path. Fig. 1, in the Plate, represents a bird in this act.

Let *a b* be a section of the plane of both wings opposing the horizontal current of the air (created by its own motion) which may be represented by the line *c d*, and is the measure of the velocity of the bird. The angle *b d c* can be increased at the will of the bird, and to preserve a perfectly horizontal path, without the wing being waved, must continually be increased in a complete ratio, (useless at present to enter into) till the motion is stopped altogether; but at one given time the position of the wings may be truly represented by the angle *b d c*. Draw *d e* perpendicular to the plane of the wings, produce the line *e d* as far as required, and from the point *e*, assumed at pleasure in the line *d e*, let fall *e f* perpendicular to *d f*. Then d e will represent the whole force of the air under the wing; which being resolved into the two forces *e f* and *f d*, the former represents the force that sustains the weight of the bird, the latter the retarding force by which the velocity of the motion, producing the current *c d*, will continually be diminished. *e f* is always a known quantity, being equal to the weight of the bird, and hence *f d* is also known, as it will always bear the same proportion to the weight of the bird, as the sine of the angle *b d e* bears to its cosine, the angles *d e f*, and *b d c*, being equal. In addition to the retarding force thus received is the direct resistance, which the bulk of the bird opposes to the current. This is a matter to be entered into separately from the principle now under consideration; and for the present may be wholly neglected, under the supposition of its being balanced by a force precisely equal and opposite to itself.

Before it is possible to apply this basis of the principle of flying in birds to the purposes of aerial navigation, it will be necessary to encumber it with a few practical observations. The whole problem is confined within these limits, viz. To make a surface support a given weight by the application of power to the resistance of air. Magnitude is the first question respecting the surface. Many experiments have been made upon the direct resistance of air by Mr. Robins, Mr. Rouse, Mr. Edgeworth, Mr. Smeaton, and others. The result of Mr. Smeaton's experiments and observations was, that a surface of a square foot met with a resistance of one pound, when it travelled perpendicularly to itself through air at a velocity of 21 feet per second. I have tried many experiments upon a large scale to ascertain this point. The instrument was similar to that used by Mr. Robins, but the surface used was larger, being an exact square foot, moving round upon an arm about five feet long, and turned by weights over a pulley. The time was measured by a stop watch, and the distance travelled over in each experiment was 600 feet. I shall for the present only give the result of many carefully repeated experiments, which is, that a velocity of 11.538 feet per second generated a resistance of 4 ounces; and that a velocity of 17.16 feet per second gave 8 ounces resistance. This delicate instrument would have been strained by the additional weight necessary to have tried the velocity generating a pressure of one pound per square foot; but if the resistance be taken to vary as the square of the velocity, the former will give the velocity necessary for this purpose at 23.1 feet, the latter 24.28 per second. I shall therefore take 23.6 feet as somewhat approaching the truth.

Having ascertained this point, had our tables of angular resistance been complete, the size of the surface necessary for any given weight would easily have been determined. Theory, which gives the resistance of a surface opposed to the same current in different angles, to be as the squares of the sine of the angle of incidence, is of no use in this case; as it appears from the experiments of the French Academy, that in acute angles, the resistance varies much more nearly in the direct ratio of the sines, than as the squares of the sines of the angles of incidence. The flight of birds will prove to an attentive observer, that, with a concave wing apparently parallel to the horizontal path of the bird, the same support, and of course resistance, is obtained. And hence I am inclined to suspect, that, under extremely acute angles, with concave surfaces, the resistance is nearly similar in


Nicholson's Philos Journal Vol.XXIV.Pl.6 p.174
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.

them all. I conceive the operation may be of a different nature from what takes place in larger angles, and may partake more of the principle of pressure exhibited in the instrument known by the name of the hydrostatic paradox, a slender filament of the current is constantly received under the anterior edge of the surface, and directed upward into the cavity, by the filament above it, in being obliged to mount along the convexity of the surface, having created a slight vacuity immediately behind the point of separation. The fluid accumulated thus within the cavity has to make its escape at the posterior edge of the surface, where it is directed considerably downward; and therefore has to overcome and displace a portion of the direct current passing with its full velocity immediately below it; hence whatever elasticity this effort requires operates upon the whole concavity of the surface, excepting a small portion of the anterior edge. This may or may not be the true theory, but it appears to me to be the most probable account of a phenomenon, which the flight of birds proves to exist.

Six degrees was the most acute angle, the resistance of which was determined by the valuable experiments of the French Academy; and it gave $\frac{4}{10}$ of the resistance, which the same surface would have received from the same current when perpendicular to itself. Hence then a superficial foot, forming an angle of six degrees with the horizon, would, if carried forward horizontally (as a bird in the act of skimming) with a velocity of 23.6 feet per second, receive a pressure of $\frac{4}{10}$ of a pound perpendicular to itself. And, if we allow the resistance to increase as the square of the velocity, at 27.3 feet per second it would receive a pressure of one pound. I have weighed and measured the surface of a great many birds, but at present shall select the common rook (*corvus frugilegus*) because its surface and weight are as nearly as possible in the ratio of a superficial foot to a pound. The flight of this bird, during any part of which they can skim at pleasure, is (from an average of many observations) about 34.5 feet per second. The concavity of the wing may account for the greater resistance here received, than the experiments upon plain surfaces would indicate. I am convinced, that the angle made use of in the crow's wing is much more acute than six degrees; but in the observations, that will be grounded upon these data, I may safely state, that every foot of such curved surface, as will be used in aerial navigation, will receive a resistance of one pound, perpendicular to itself, when carried through the air in an angle of six degrees with the line of its path, at a velocity of about 34 or 35 feet per second.

Let *a b*, fig. 2, represent such a surface or sail made of thin cloth, and containing about 200 square feet (if of a square form the side will be a little more than I4 feet); and the whole of a firm texture. Let the weight of the man and the machine be 200 pounds. Then if a current of wind blew in the direction *c d*, with a velocity of 35 feet per second, at the same time that a cord represented by *c d* would sustain a tension of 21 pounds, the machine would be suspended in the air, or at least be within a few ounces of it (falling short of such support only in the ratio of the sine of the angle of 94 degrees compared with radius; to balance which defect, suppose a little ballast to be thrown out) for the line *d e* represents a force of 200 pounds, which, as before, being resolved into *d f* and *f e*, the former will represent the resistance in the direction of the current, and the latter that which sustains the weight of the machine. It is perfectly indifferent whether the wind blow against the plane, or the plane be driven with an equal velocity against the air. Hence, if this machine were pulled alone by a cord *c d*, with a tension of about 21 pounds, at a velocity of 35 feet per second, it would be suspended in a horizontal path; and if in lieu of this cord any other propelling power were generated in this direction, with a like intensity, a similar effect would be produced. If therefore the waft of surfaces advantageously moved, by any force generated within the machine, took place to the extent required, aerial navigation would be accomplished. As the acuteness of the angle between the plane and current increases, the propelling power required is less and less. The principle is similar to that of the inclined plane, in which theoretically one pound may be made to sustain all but an infinite quantity; for in this case, if the magnitude of the surface be increased ad infinitum, the angle with the current may be diminished, and consequently the propelling force, in the same ratio. In practice, the extra resistance of the car and other parts of the machine, which consume a considerable portion of power, will regulate the limits to which this principle, which is the true basis of aerial navigation, can be carried; and the perfect ease with which some birds are suspended in long horizontal flights, without one waft of their wings, encourages the idea, that a slight power only is necessary.

As there are many other considerations relative to the practical introduction of this machine, which would occupy too much space for any one number of your valuable Journal, I propose, with your approbation, to furnish these in your subsequent numbers; taking this opportunity to observe, that perfect steadiness, safety, and steerage, I have long since accomplished upon a considerable scale of magnitude; and that I am engaged in making some farther experiments upon a machine I constructed last summer, large enough for aerial navigation, but which I have not had an opportunity to try the effect of, excepting as to its proper balance and security. It was very beautiful to see this noble white bird sail majestically from the top of a hill to any given point of the plane below it, according to the set of its rudder, merely by its own weight, descending in an angle of about 18 degrees with the horizon. The exertions of an individual, with other avocations, are extremely inadequate to the progress, which this valuable subject requires. Every man acquainted with experiments upon a large scale well knows how leisurely fact follows theory, if ever so well founded. I do therefore hope, that what I have said, and have still to offer, will induce others to give their attention to this subject; and that England may not be backward in rivalling the continent in a more worthy contest than that of arms.

As it may be an amusement to some of your readers to see a machine rise in the air by mechanical means, I will conclude my present communication by describing an instrument of this kind, which any one can construct at the expense of ten minutes labour.

a and *b*, fig. 3, are two corks, into each of which are inserted four wing feathers from any bird, so as to be slightly inclined like the sails of a windmill, but in opposite directions in each set. A round shaft is fixed in the cork *a*, which ends in a sharp point. At the upper part of the cork *b* is fixed a whalebone bow, having a small pivot hole in its centre, to receive the point of the shaft. The bow is then to be strung equally on each side to the upper portion of the shaft, and the little machine is completed. Wind up the string by turning the flyers different ways, so that the spring of the bow may unwind them with their anterior edges ascending; then place the cork with the bow attached to it upon a table, and with a finger on the upper cork press strong enough to prevent the string from unwinding, and taking it away suddenly, the instrument will rise to the ceiling. This was the first experiment I made upon this subject in the year 1796. If in lieu of these small feathers large planes, containing together 200 square feet, were similarly placed, or in any other more convenient position, and were turned by a man, or first mover of adequate power, a similar effect would be the consequence, and for the mere purpose of ascent this is perhaps the best apparatus; but speed is the great object of this invention, and this requires a different structure.

P. S. In lieu of applying the continued action of the inclined plane by means of the rotative motion of flyers, the same principle may be made use of by the alternate motion of surfaces backward and forward; and although the scanty description hitherto published of Mr. Degen's apparatus will scarcely justify any conclusion upon the subject; yet as the principle above described must be the basis of every engine for aerial navigation by mechanical means, I conceive, that the method adopted by him has been nearly as follows. Let A and B, fig. 4, be two surfaces or parachutes, supported upon the long shafts C and D, which are fixed to the ends of the connecting beam E, by hinges. At E, let there be a convenient seat for the aeronaut, and before him a cross bar turning upon a pivot in its centre, which being connected with the shafts of the parachutes by the rods F and G, will enable him to work them alternately backward and forward, as represented by the dotted lines. If the upright shafts be elastic, or have a hinge to give way a little near their tops, the weight and resistance of the parachutes will incline them so, as to make a small angle with the direction of their motion, and hence the machine rises. A slight heeling of the parachutes toward one side, or an alteration in the position of the weight, may enable the aeronaut to steer such an apparatus tolerably well; but many better constructions may be formed, for combining the requisites of speed, convenience and steerage. It is a great point gained, when the first experiments demonstrate the practicability of an art; and Mr. Degen, by whatever means he has effected this purpose, deserves much credit for his ingenuity.

Document 1-1(b), George Cayley, "On Aerial Navigation," Part Two, 1810.

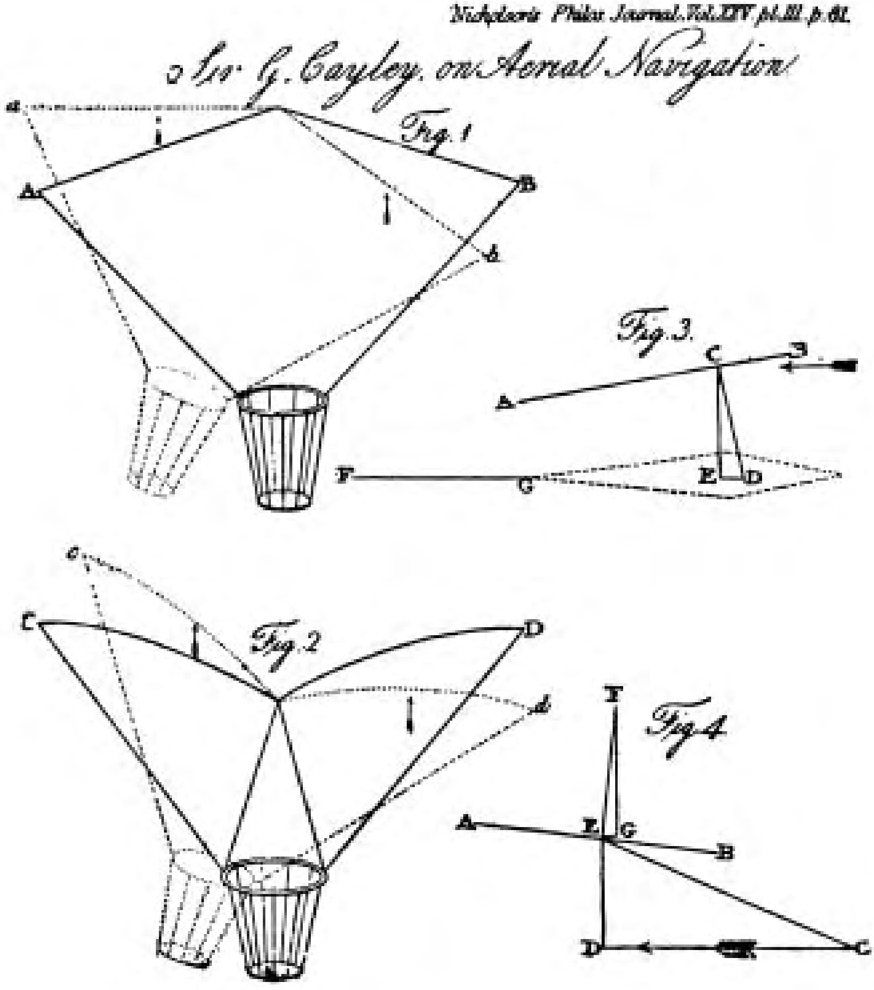
HAVING, in my former communication, described the general principle of support in aerial navigation, I shall proceed to show how this principle must be applied, so as to be steady and manageable.

Several persons have ventured to descend from balloons in what is termed a parachute, which exactly resembles a large umbrella, with a light car suspended by cords underneath it.

Mr. Garnerin's descent in one of these machines will be in the recollection of many; and I make the remark for the purpose of alluding to the continued oscillation, or want of steadiness, which is said to have endangered that bold aeronaut. It is very remarkable, that the only machines of this sort, which have been constructed, are nearly of the worst possible form for producing a steady descent, the purpose for which they are intended. To render this subject more familiar, let us recollect, that in a boat, swimming upon water, its stability or stiffness depends, in general terms, upon the *weight* and distance from the centre of the section elevated above the water, by any given heel of the boat, on one side; and on the *bulk*, and its distance from the centre, which is immersed below the water, on the other side; the combined endeavour of the one to fall and of the other to swim, produces the desired effect in a well-constructed boat. The centre of gravity of the boat being more or less below the centre of suspension is an additional cause of its stability.

Let us now examine the effect of a parachute represented by A B, fig. 1, PI. III. When it has heeled into the position *a b*, the side *a* is become perpendicular to the current, created by the descent, and therefore resists with its greatest power; whereas the side b is become more oblique, and of course its resistance is much diminished. In the instance here represented, the angle of the parachute itself is 144°, and it is supposed to heel 18°, the comparative resistance of the side *a* to the side *b*, will be as the square of the line *a*, as radius, to the square of the sine of the angle of *b* with the current; which, being 54 degrees, gives the resistances nearly in the ratio of 1 to 0.67; and this will be reduced to only 0.544, when estimated in a direction perpendicular to the horizon. Hence, so far as this form of the sail or plane is regarded, it operates directly in opposition to the principle of stability; for the side that is required to fall resists much more in its new position, and that which is required to rise resists much less; therefore complete inversion would be the consequence, if it were not for the weight being suspended so very much below the surface, which, counteracting this tendency, converts the effort into a violent oscillation.

On the contrary, let the surface be applied in the inverted position, as represented at C D, fig. 2, and suppose it to be heeled to the same angle as before, represented by the dotted lines *c d*. Here the exact reverse of the former instance takes place; for that side, which is required to rise, has gained resistance by its new position, and that which is required to sink has lost it; so that as much power oper-



ates to restore the equilibrium in this case, as tended to destroy it in the other: the operation very much resembling what takes place in the common boat. (A very simple experiment will show the truth of this theory. Take a circular piece of writing paper, and folding up a small portion, in the line of two radii, it will be formed into an obtuse cone. Place a small weight in the apex, and letting it fall from any height, it will steadily preserve that position to the ground. Invert it, and, if the weight be fixed, like the life boat, it rights itself instantly.)

This angular form, with the apex downward, is the chief basis of stability in aerial navigation; but as the sheet which is to suspend the weight attached to it, in its horizontal path through the air, must present a slightly concave surface in a small angle with the current, this principle can only be used in the lateral extension of the sheet; and this most effectually prevents any rolling of the machine from side to side. Hence the section of the inverted parachute, fig. 2, may equally well represent the cross section of a sheet for aerial navigation.

The principle of stability in the direction of the path of the machine, must be derived from a different source. Let A B, fig. 3, be a longitudinal section of a sail, and let C be its centre of resistance, which experiment shows to be considerably more forward than the centre of the sail. Let C D be drawn perpendicular to A B, and let the centre of gravity of the machine be at any point in that line, as at D. Then, if it be projected in a horizontal path with velocity enough to support the weight, the machine will retain its relative position, like a bird in the act of skimming; for, drawing C E perpendicular to the horizon, and D E parallel to it, the line C E will, at some particular moment, represent the supporting power, and likewise its opponent the weight; and the line D E will represent the retarding power, and its equivalent, that portion of the projectile force expended in overcoming it: hence, these various powers being exactly balanced, there is no tendency in the machine but to proceed in its path, with its remaining portion of projectile force.

The stability in this position, arising from the centre of gravity being below the point of suspension, is aided by a remarkable circumstance that experiment alone could point out. In very acute angles with the current it appears, that the centre of resistance in the sail does not coincide with the centre of its surface, but is considerably in front of it. As the obliquity of the current decreases, these centres approach, and coincide when the current becomes perpendicular to the sail. Hence any heel of the machine backward or forward removes the centre of support behind or before the point of suspension; and operates to restore the original position, by a power, equal to the whole weight of the machine, acting upon a lever equal in length to the distance the centre has removed.

To render the machine perfectly steady, and likewise to enable it to ascend and descend in its path, it becomes necessary to add a rudder in a similar position to the tail in birds. Let F G be the section of such a surface, parallel to the current;

and let it be capable of moving up and down upon G, as a centre, and of being fixed in any position. The powers of the machine being previously balanced, if the least pressure be exerted by the current, either upon the upper or under surface of the rudder, according to the will of the aeronaut, it will cause the machine to rise or fall in its path, so long as the projectile or propelling force is continued with sufficient energy. From a variety of experiments upon this subject I find, that, when the machine is going forward with a superabundant velocity, or that which would induce it to rise in its path, a very steady horizontal course is effected by a considerable depression of the rudder, which has the advantage of making use of this portion of sail in aiding the support of the weight. When the velocity is becoming less, as in the act of alighting, then the rudder must gradually recede from this position, and even become elevated, for the purpose of preventing the machine from sinking too much in front, owing to the combined effect of the want of projectile force sufficient to sustain the centre of gravity in its usual position, and of the centre of support approaching the centre of the sail.

The elevation and depression of the machine are not the only purposes, for which the rudder is designed. This appendage must be furnished with a vertical sail and be capable of turning from side to side in addition to its other movements, which effects the complete steerage of the vessel.

All these principles, upon which the support, steadiness, elevation, depression, and steerage of vessels for aerial navigation, depend, have been abundantly verified by experiments both upon a small and a large scale. Last year I made a machine, having a surface of 300 square feet, which was accidentally broken before there was an opportunity of trying the effect of the propelling apparatus; but its steerage and steadiness were perfectly proved, and it would sail obliquely downward in any direction, according to the set of the rudder. Even in this state, when any. person ran forward in it, with his full speed, taking advantage of a gentle breeze in front, it would bear upward so strongly as scarcely to allow him to touch the ground; and would frequently lift him up, and convey him several yards together.

The best mode of producing the propelling power is the only thing, that remains yet untried toward the completion of the invention. I am preparing to resume my experiments upon this subject, and state the following observations, in the hope that others may be induced to give their attention towards expediting the attainment of this art.

The act of flying is continually exhibited to our view; and the principles upon which it is effected are the same as those before stated. If an attentive observer examines the waft of a wing, he will perceive, that about one third part, toward the extreme point, is turned obliquely backward; this being the only portion, that has velocity enough to overtake the current, passing so rapidly beneath it, when in this unfavourable position. Hence this is the only portion that gives any propelling force.

To make this more intelligible, let A B, fig. 4, be a section of this part of the wing. Let C D represent the velocity of the bird's path, or the current, and E D that of the wing in its waft: then C E will represent the magnitude and direction of the compound or actual current striking the under surface of the wing. Suppose E F, perpendicular to A B, to represent the whole pressure; E G being parallel to the horizon, will represent the propelling force; and G F, perpendicular to it, the supporting power. A bird is supported as effectually during the return as during the beat of its wing; this is chiefly effected by receiving the resistance of the current under that portion of the wing next the body where its receding motion is so slow as to be of scarcely any effect. The extreme portion of the wing, owing to its velocity, receives a pressure downward and obliquely forward, which forms a part of the propelling force; and at the same time, by forcing the hinder part of the middle portion of the wing downward, so increases its angle with the current, as to enable it still to receive nearly its usual pressure from beneath.

As the common rook has its surface and weight in the ratio of a square foot to a pound, it may be considered as a standard for calculations of this sort; and I shall therefore state, from the average of many careful observations, the movements of that bird. Its velocity, represented by C D, fig. 4, is 34.5 feet per second. It moves its wing up and down once in flying over a space of 12.9 feet. Hence, as the centre of resistance of the extreme portion of the wing moves over a space of 0.75 of a foot each beat or return, its velocity is about 4 feet per second, represented by the line E D. As the wing certainly overtakes the current, it must be inclined from it in an angle something less than 7°, for at this angle it would scarcely be able to keep parallel with it, unless the waft downward were performed with more velocity than the return; which may be and probably is the case, though these movements appear to be of equal duration. The propelling power, represented by E G, under these circumstances, cannot be equal to an eighth part of the supporting power G F, exerted upon this portion of the wing; yet this, together with the aid from the return of the wing, has to overcome all the retarding power of the surface, and the direct resistance occasioned by the bulk of the body.

It has been before suggested, and I believe upon good grounds, that very acute angles vary little in the degree of resistance they make under a similar velocity of current. Hence it is probable, that this propelling part of the wing receives little more than its common proportion of resistance, during the waft downward. If it be taken at one-third of the whole surface, and one-eighth of this be allowed as the propelling power, it will only amount to one twenty-fourth of the weight of the bird; and even this is exerted only half the duration of the flight. The power gained in the return of the wing must be added, to render this statement correct, and it is difficult to estimate this; yet the following statement proves, that a greater degree of propelling force is obtained, upon the whole, than the foregoing observations will justify. Suppose the largest circle that can be described in the breast of a crow, to be 12 inches in area. Such a surface, moving at the velocity of 34.5 feet per second, would meet a resistance of 0.216 of a pound, which, reduced by the proportion of the resistance of a sphere to its great circle (given by

Mr. Robins as 1 to 2.27) leaves a resistance of 0.095 of a pound, had the breast been hemispherical. It is probable however, that the curve made use of by Nature to avoid resistance, being so exquisitely adapted to its purpose, will reduce this quantity to one half less than the resistance of the sphere, which would ultimately leave 0.0475 of a pound as somewhat approaching the true resistance. Unless therefore the return of the wing gives a greater degree of propelling force than the beat, which is improbable, no such resistance of the body could be sustained. Hence, though the eye cannot perceive any distinction between the velocities of the beat and return of the wing, it probably exists, and experiment alone can determine the proper ratios between them.

From these observations we may, however, be justified in the remark—that the act of flying, when properly adjusted by the Supreme Author of every power, requires less exertions than, from the appearance, is supposed.

Document 1-1(c), George Cayley, "On Aerial Navigation," Part Three, 1810.

BROMPTON, Dec. 6, 1809.

NOT having sufficient data to ascertain the exact degree of propelling power exerted by birds in the act of flying, it is uncertain what degree of energy may be required in this respect in vessels for aerial navigation: yet, when we consider the many hundred miles of continued flight exerted by birds of passage, the idea of its being only a small effort is greatly corroborated. To apply the power of the first mover to the greatest advantage in producing this effect, is a very material point. The mode universally adopted by nature is the oblique waft of the wing. We have only to choose between the direct beat overtaking the velocity of the current, like the oar of a boat; or one, applied like the wing, in some assigned degree of obliquity to it. Suppose 35 feet per second to be the velocity of an aerial vehicle, the oar must be moved with this speed previous to its being able to receive any resistance; then, if it be only required to obtain a pressure of 1/10th of a pound upon each square foot, it must exceed the velocity of the current 7.5 feet per second. Hence its whole velocity must be 42.5 feet per second. Should the same surface be wafted downward, like a wing, with the hinder edge inclined upward in an angle of about 50° 40' to the current, it will overtake it at a velocity of 3.5 feet per second; and as a slight unknown angle of resistance generates a pound pressure per square foot at this velocity, probably a waft of little more than 4 feet per second would produce this effect; one tenth part of which would be the propelling power. The advantage in favour of this mode of application, compared with the former, is rather more than ten to one.

In combining the general principles of aerial navigation for the practice of the art many mechanical difficulties present themselves, which require a considerable course of skilfully applied experiments, before they can be overcome. But to a certain extent the air has already been made navigable; and no one, who has seen the steadiness with which weights to the amount of ten stone (including four stone, the weight of the machine) hover in the air, can doubt of the ultimate accomplishment of this object.

The first impediment I shall take notice of is the great proportion of power, that must be exerted previous to the machine's acquiring that velocity, which gives support upon the principle of the inclined plane; together with the total want of all support during the return of any surface used like a wing. Many birds, and particularly water fowl, run and flap their wings for several yards before they can gain support from the air. The swift (*hirundo apus Lin.*) is not able to elevate itself from level ground. The inconvenience under consideration arises from very different causes in these two instances. The supporting surface of most swimming birds does not exceed the ratio of 4⁄10ths of a square foot to every pound of their weight: the swift, though it scarcely weighs an ounce, measures eighteen inches in extent of wing. The want of surface in the one case, and the inconvenient length of wing in the other, oblige these birds to aid the *commencement* of their flight by other expedients; yet they can both fly with great power, when they have acquired their full velocity.

A second difficulty in aerial navigation arises from the great extent of lever, which is constantly operating against the first mover, in consequence of the distance of the centre of support in large surfaces, if applied in the manner of wings.

A third and general obstacle is the mechanical skill required to unite great extension of surface with strength and lightness of structure; at the same time having a firm and steady movement in its working parts, without exposing unnecessary obstacles to the resistance of the air. The first of these obstacles, that have been enumerated, operates much more powerfully against aerial navigation upon a large scale, than against birds; because the small extent of their wings obliges them to employ a very rapid succession of strokes, in order to acquire that velocity which will give support; and during the small interval of the return of the wing, their weight is still rising, as in a leap, by the impulse of one stroke, till it is again aided by another. The large surfaces that aerial navigation will probably require, though necessarily moved with the same velocity, will have a proportionably longer duration both of the beat and return of the wing; and hence a greater descent will take place during the latter action, than can be overcome by the former.

There appears to be several ways of obviating this difficulty. There may be two surfaces, each capable of sustaining the weight, and placed one above the other, having such a construction as to work up and down in opposition when they are moved, so that one is always ready to descend, the moment the other ceases. These surfaces may be so made, by a valvelike structure, as to give no opposition in rising up, and only to resist in descent.

The action may be considered either oblique, as in rotative flyers; alternately so, without any up and down waft, as in the engine I have ascribed to Mr. Degen; by means of a number of small wings in lieu of large ones, upon the principle of the flight of birds, with small intervals of time between each waft; and lastly by making use of light wheels to preserve the propelling power both of the beat and the return of the wings, till it accumulates sufficiently to elevate the machine, upon the principle of those birds which run themselves up. This action might be aided by making choice of a descending ground like the swift.

With regard to another part of the first obstacle I have mentioned, viz. the absolute quantity of power demanded being so much greater at first than when the full velocity has been acquired; it may be observed, that, in the case of human muscular strength being made use of, a man can exert, for a few seconds, a surprising degree of force. He can run up stairs, for instance, with a velocity of from 6 to 8 feet perpendicular height per second, without any dangerous effort; here the muscles of his legs only are in action; but, for the sake of making a moderate statement, suppose that with the activity of his arms and body, in addition to that of his legs, he is equal to raising his weight 8 feet per second; if in this case he weighs 11 stone, or 154 pounds, he will be exerting, for the time, an energy equal to more than the ordinary force of two of Messrs. Boulton and Watt's steam horses; and certainly more than twelve men can bestow upon their constant labour.

If expansive first movers be made use of, they may be so constructed, as to be capable of doing more than their constant work; or their power may be made to accumulate for a few moments by the formation of a vacuum, or the condensation of air, so that these expedients may restore at one time, in addition to the working of the engine, that which they had previously absorbed from it.

With regard to the second obstacle in the way of aerial navigation, viz. the length of leverage to which large wing-like surfaces are exposed, it may be observed, that, being a constant and invariable quality, arising from the degree of support such surfaces give, estimated at their centres of resistance, it may be balanced by any elastic agent, that is so placed as to oppose it. Let A and B, PI. IV, fig. 1, be two wings of an aerial vehicle in the act of skimming; then half the weight of the vessel is supported from the centre of resistance of each wing; as represented by the arrows under them. If the shorter ends of these levers be connected by cords to the string of a bow C, of sufficient power to balance the weight of the machine at the points A and B, then the moving power will be left at full liberty to produce the waft necessary to bend up the hinder edge of the wing, and gain the propelling power. A bow is not in fact an equable spring, but may be made so by using a spiral fusee. I have made use of it in this place merely as the most simple mode of stating the principle I wished to exhibit. Should a counterbalancing spring of this kind be adopted in the practice of aerial navigation, a small well polished cylinder, furnished with what may be termed a bag piston (upon the principle made use of by nature in preventing the return of the blood to the heart, when it has been driven into the aorta, by the intervention of the semilunar valves) would, by a vacuum being excited each stroke of the wing, produce the desired effect, with scarcely any loss by friction. These elastic agents may likewise be useful in gradually stopping the momentum of large surfaces when used in any alternate motion, and in thus restoring it during their return.

(I have made use of several of these pistons, and have no scruple in asserting, that for all blowing engines, where friction is an evil, and being very nearly air-tight is sufficient, there is no other piston at all comparable with them. The most irregular cylinder, with a piston of this kind, will act with surprising effect. To give an instance; a cylinder of sheet tin, 8 inches long and 3 ½ in diameter, required 4 pounds to force the piston down in 15 minutes; and in other trials became perfectly tight in some positions, and would proceed no farther. The friction, when the cylinder was open at both ends, did not exceed ½ an ounce.)

Another principle, that may be applied to obviate this leverage of a wing, is that of using such a construction as will make the supporting power of the air counterbalance itself. It has been before observed, that only about one third of the wing in birds is applied in producing the propelling power; the remainder, not having velocity sufficient for this purpose, is employed in giving support, both in the beat and return of the wing.

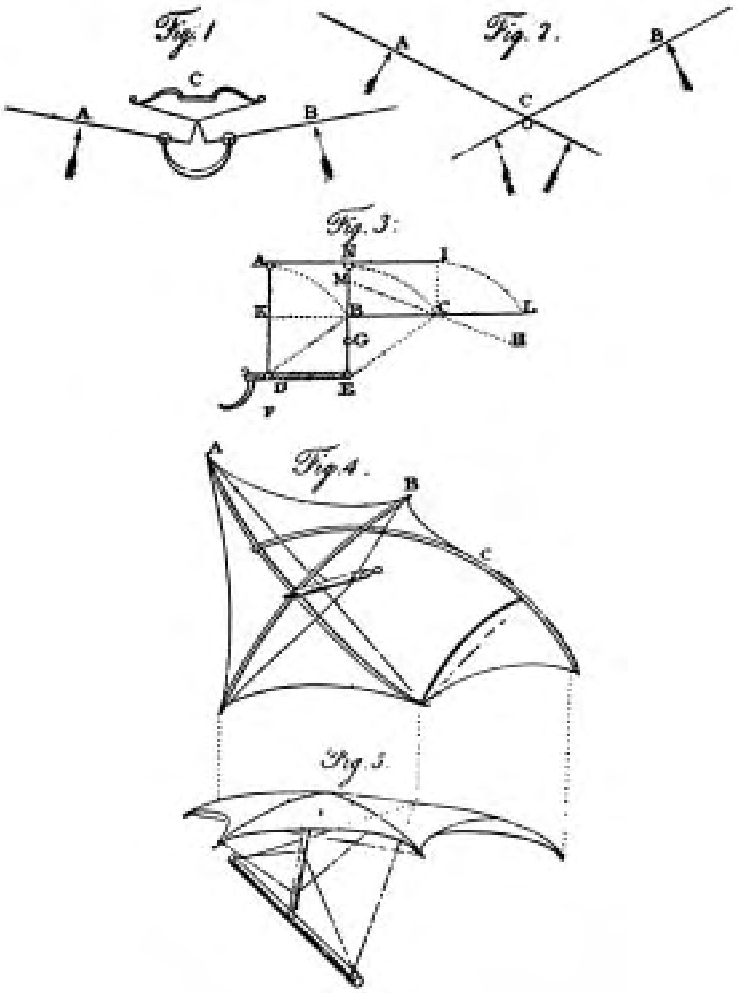
Let A and B, fig. 2, be two wings continued beyond the pole or hinge upon which they turn at C. If the extreme parts at A and B be long and narrow, they may be balanced, when in the act of skimming, by a broad extension of less length on their opposite sides; this broad extension, like the lower part of the wing, will always give nearly the same support, and the propelling part of the surface will be at liberty to act unincumbered by the leverage of its supporting power. This plan may be modified many different ways; but my intention, as in the former case, is still the principle in its simplest form.

A third principle upon which the leverage of a surface may be prevented is by giving it a motion parallel to itself, either directly up and down, or obliquely so. The surface A 1, fig. 3, may be moved perpendicularly, by the shaft which supports it, down to the position K C: or, if it be supported upon two shafts with hinges at D and E, it may be moved obliquely parallel to itself into the position B L.

A fourth principle upon which the leverage may be greatly avoided, where only one hinge is used, is by placing it considerably below the plane of the wing, as at the point D, fig. 3, in respect to the surface A. It may be observed in the heron, which is a weak bird with an extended surface, that its wings curve downward considerably from the hinge to the tip; hence the extreme portion, which receives the chief part of the stroke, is applied obliquely to the current it creates; and thus evades in a similar degree the leverage of that portion of the supporting power, which is connected with the propelling power. These birds seldom carry their waft much below the level of the hinge of the wing, where this principle, so far as respects the supporting power, would vanish.

Nicholsons Philos Journal. Vol. XXIV pl. IV

Sir G. Cayley on Aerial Navigation.



By making use of two shafts of unequal length, the two last mentioned principles may be blended to any required extent. Suppose one hinge to be at F, and the other at G, fig. 3, then the surface, at the extent of its beat, would be in the position of the line H M. If the surface A 1, fig. 3, be supported only upon one shaft, N E, be capable of being forced in some degree from its rectangular position in respect to the shaft, and be concave instead of flat as here represented; then the waft may be used alternately backward and forward, according to the principles of the machine I have ascribed to Mr. Degen. This construction combines the principles of counterpoising the supporting power of one part of the surface, by that of an opposite part, when the machine is in the act of skimming; and likewise the advantages of the low hinge, with the principle of leaving little or no interval without support.

All that has hitherto appeared respecting Mr. Degen's apparatus is, that it consisted of two surfaces, which were worked by a person sitting between them. This statement communicates no real information upon the subject; for scarcely anyone would attempt to fly without *two* wings; without these being equally poised by placing the weight *between* them; and also, without these surfaces being capable of receiving motion from his muscular action. I may be altogether mistaken in my conjecture; my only reason for ascribing this structure of mine to Mr. Degen's machine is, that, if it were properly executed upon this principle, it would be attended with success. The drawing, rather diagram, which is given of this machine in the first part of my essay, is only for the purpose of exhibiting the principle in a form capable of being understood. The necessary bracings, etc., required in the actual execution of such a plan, would have obscured the simple nature of its action; and were therefore omitted. The plan of its movement is also simply to exhibit, in a tangible form, the possibility of effecting the intended alternate motion of the parachutes. The seat is fronted lengthwise for the purpose of accommodating the mode of communicating the movement.

A fifth mode of avoiding leverage is by using the continued action of oblique horizontal flyers, or an alternate action of the same kind, with surfaces so constructed as to accommodate their position to such alternate motion; the hinge or joint being in these cases vertical. In the construction of large vessels for aerial navigation, a considerable portion of fixed sail will probably be used; and no more surface will be allotted, towards gaining the propelling power, than what is barely necessary, with the extreme temporary exertion of the first mover, to elevate the machine and commence the flight. In this case the leverage of the fixed surface is done away.

The general difficulties of structure in aerial vehicles, (arising from the extension, lightness, and strength required in them; together with great firmness in the working parts, and at the same time such an arrangement as exposes no unnecessary obstacles to the current,) I cannot better explain than by describing a wing, which has been constructed with a view to overcome them.

Fig. 4 represents the shape of the cloth, with a perspective view of the poles upon which it is stretched with perfect tightness. Upon the point where the rods A and B intersect is erected an oval shaft; embracing the two cross poles by a slender iron fork; for the purpose of preserving their strength uninjured by boring. To this shaft are braced the ends of the pole B, so as to give this pole any required degree of curvature. The pole A is strung like a common bow to the same curve as the pole B; and is only connected with the upright shaft by what may be called a check brace; which will allow the hinder end of this pole to heel back to a certain extent, but not the fore end. The short brace producing this effect is shown in fig. 4. Fig. 5 exhibits the fellow wing to that represented in fig. 4, erected upon a beam, to which it is so braced, as to convert the whole length of it into a hinge. The four braces coming from the ends of this beam are shown: two of them terminate near the top of the centre of the other shaft; the others are inserted into the point C, fig. 4, of the bending rod. A slight bow, not more than three-eighths of an inch thick, properly curved by its string, and inserted between the hinder end of the pole A, and the curved pole C, completes the wing.

This fabrick contained 54 square feet, and weighed only eleven pounds. Although both these wings together did not compose more than half the surface necessary for the support of a man in the air, yet during their waft they lifted the weight of nine stone. The hinder edge, as is evident from the construction, being capable of giving way to the resistance of the air, any degree of obliquity, for the purpose of a propelling power, may be used.

I am the more particular in describing this wing, because it exemplifies almost all the principles that can be resorted to in the construction of surfaces for aerial navigation. Diagonal bracing is the great principle for producing strength without accumulating weight; and, if performed by thin wires, looped at their ends, so as to receive several laps of cordage, produces but a trifling resistance in the air, and keeps tight in all weathers. When bracings are well applied, they make the poles, to which are attached, bear endwise. The hollow form of the quill in birds is a very admirable structure for lightness combined with strength, where external bracings cannot be had; a tube being the best application of matter to resist as a lever; but the principle of bracing is so effectual, that, if properly applied, it will abundantly make up for the clumsiness of human invention in other respects; and should we combine both these principles, and give diagonal bracing to the tubular bamboo cane, surfaces might be constructed with a greater degree of strength and lightness, than any made use of in the wings of birds.

The surface of a heron's wing is in the ratio of 7 square feet to a pound. Hence, according to this proportion, a wing of 54 square feet would weigh about 7 ¾ pounds: on the contrary the wings of water fowl are so much heavier, that a surface of 54 square feet, according to their structure, will weight 18 ½ lb. I have in these instances quoted nearly the extreme cases among British birds; the wing I have described may therefore be considered as nearly of the same weight in proportion to its bulk as that of most birds.

Another principle exhibited in this wing is that of the poles being couched within the cloth, so as to avoid resistance. This is accomplished by the convexity of the frame, and the excessive lightness of the cloth. The poles are not allowed to form the edge of the wing, excepting at the extreme point of the bow, where it is very thin, and also oblique to the current. The thick part of this pole is purposely conveyed considerably within the edge. In birds, a membrane covered with feathers is stretched before the thick part of the bone of the wing, in a similar manner, and for the same purpose. The edge of the surface is thus reduced to the thickness of a small cord, that is sown to the cloth, and gives out loops whenever any fastening is required. The upright shaft is the only part that opposes much direct resistance to the current, and this is obviated in a great degree by a flat oval shape, having its longest axis parallel to the current.

The joint or hinge of this wing acts with great firmness, in consequence of its being supported by bracings to the line of its axis, and at a considerable distance from each other; in fact the bracings form the hinge.

The means of communicating motion to any surfaces must vary so much, according to the general structure of the whole machine, that I shall only observe at present, that where human muscular action is employed, the movement should be similar to the mode of pulling oars; from which any other required motion may be derived; the foot-board in front enables a man to exert his full force in this position. The wings I have described were wafted in this manner; and when they lifted with a power of 9 stone, not half of the blow, which a man's strength could have given, was exerted, in consequence of the velocity required being greater than convenient under the circumstances. Had these wings been intended for elevating the person who worked them, they should have contained from 100 to 150 square feet each; but they were constructed for the purpose of an experiment relative to the propelling power only.

Avoiding direct resistance is the next general principle, that it is necessary to discuss. Let it be remembered as a maxim in the art of aerial navigation, that every pound of direct resistance, that is done away, will support 30 pounds of additional weight without any additional power. The figure of a man seems but ill calculated to pass with ease through the air, yet I hope to prove him to the full as well made in this respect as the crow, which has hitherto been our standard of comparison, paradoxical as it may appear.

The principle, that surfaces of similar bodies increase only as the squares of their homologous lines, while their weights, or rather solid contents, increase as the cubes of those lines, furnishes the solution. This principle is unanimously in favour of large bodies. The largest circle that can be described in a crow's breast is about 12 square inches in area. If a man exposes a direct bulk of 6 square feet, the ratio of their surfaces will be as 1 to 72; but the ratio of their weight is as 1 to 110; which is 1 ½ to 1 in favour of the man, provided he were within a case as well constructed for evading resistance, as the body of the crow; but even supposing him to be exposed in his natural cylindric shape, in the foreshortened posture of sitting to work his oars, he will probably receive less resistance than the crow.

It is of great importance to this art, to ascertain the real solid of least resistance, when the length or breadth is limited. Sir Isaac Newton's beautiful theorem upon this subject is of no practical use, as it supposes each particle of the fluid, after having struck the solid, to have free egress; making the angles of incidence and reflection equal; particles of light seem to possess this power, and the theory will be true in that case; but in air the action is more like an accumulation of particles, rushing up against each other, in consequence of those in contact with the body being retarded. The importance of this subject is not less than the difficulties it presents; it affects the present interests of society in its relation to the time occupied in the voyages of ships; it will have still more effect when aerial navigation, now in its cradle, is brought home to the uses of man. I shall state a few crude hints upon this point, to which my subject has so unavoidably led, and on which I am so much interested, and shall be glad if in so doing I may excite the attention of those, who are competent to an undertaking greatly beyond my grasp.

Perhaps some approach toward ascertaining the actual solid of least resistance may be derived from treating the subject in a manner something similar to the following. Admit that such a solid is already attained (the length and width being necessarily taken at pleasure). Conceive the current intercepted or disturbed, by the largest circle that can be drawn within the given spindle, to be divided into concentric tubular laminae of equal thickness. At whatever distance from this great circle the apex of the spindle commences, on all sides of this point the central lamina will be reflected in diverging pencils, (or rather an expanding ring,) making their angles of incidence and reflection equal. After this reflection they rush against the second lamina and displace it: this second lamina contains three times more fluid than the first; consequently each pencil in the first meets three pencils in the second; and their direction, after the union, will be one fourth of the angle, with respect to the axis, which the first reflection created. In this direction these two laminae proceed till they are themselves reflected, when they (considered as one lamina of larger dimensions) rush against the third and fourth, which together contain three times the fluid in the two former laminae, and thus reduce the direction of the combined mass to one fourth of the angle between the axis and the line of the second reflection. This process is constant, whatever be the angles formed between the surface of the actual solid of least resistance at these points of reflection, and the directions of the currents thus reflected.

From this mode of reasoning, which must in some degree resemble what takes place, and which I only propose as a resemblance, it appears, that the fluid keeps creeping along the curved surface of such a solid, meeting it in very acute angles. Hence, as the experiments of the French Academy show, that the difference of resistance between the direct impulse, and that in an angle of six degrees, on the same surface, is only in the ratio of 10 to 4, it is probable, that in the slight difference of angles that occur in this instance, the resistances may be taken as equal upon every part, without any material deviation from truth. If this reasoning be correct, it will reduce the question, so far as utility is concerned, within a strictly abstract mathematical inquiry.

It has been found by experiment, that the shape of the hinder part of the spindle is of as much importance as that of the front, in diminishing resistance. This arises from the partial vacuity created behind the obstructing body. If there be no solid to fill up this space, a deficiency of hydrostatic pressure exists within it, and is transferred to the spindle. This is seen distinctly near the rudder of a ship in full sail, where the water is much below the level of the surrounding sea. The cause here, being more evident, and uniform in its nature, may probably be obviated with better success; in as much as this portion of the spindle may not differ essentially from the simple cone. I fear however, that the whole of this subject is of so dark a nature, as to be more usefully investigated by experiment, than by reasoning; and in the absence of any conclusive evidence from either, the only way that presents itself is to copy nature; accordingly I shall instance the spindles of the trout and woodcock, which, lest the engravings should, in addition to the others, occupy too much valuable space in your Journal, must be reserved to a future opportunity.

Document 1-2

Thomas Jefferson, letter to William D. B. Lee, 27 April 1822.

The remarkable farsightedness of George Cayley's understanding of aerodynamics can be gauged by the lack of confidence many of the leading intellectuals and scientists of his day had in the future of "aerial navigation" with flying machines. Responding to this issue just four years before his death, an elderly Thomas Jefferson, principal author of the Declaration of Independence (1776) and third President of the United States (from 1801 to 1809), took the following cautiously pessimistic view about the possibility of mechanical flight. Jefferson possessed a wide-ranging curiosity about all things, especially science and technology. But here in 1822, in his advanced years, he seems to be admitting that they have progressed well beyond his capacity to understand them fully or to assess the potential of such radically new concepts as mechanical flight. (The spelling in this document is Jefferson's.)

Document 1-2, Thomas Jefferson, letter to William D. B. Lee, 27 April 1822.

Your letter of the 15th is received, but age has long since obliged me to withdraw my mind from speculations of the difficulty of those of your letter. That there are means of artificial buoyancy by which man may be supported in the air, the balloon has proved, and that means of directing it may be discovered is against no law of nature and is therefore possible as in the case of birds. But to do this by mechanical means alone in a medium so rare and unresisting as air must have the aid of some principle not yet generally known. However I can really give no opinion understandingly on the subject and with more goodwill than confidence wish you success.

Document 1-3(a–c)

(a) Francis H. Wenham, "On Aerial Locomotion and the Laws by Which Heavy Bodies Impelled Through Air are Sustained," *First Annual Report of the Aeronautical Society of Great Britain for the Year 1866*, pp. 10–47.

(b) Report on the first wind tunnel built by Francis H. Wenham, *Sixth Annual Report of the Aeronautical Society of Great Britain, for the Year 1871*, pp. 75–78.

(c) Minutes of Aeronautical Society, *Seventh Annual Report of the Aeronautical Society of Great Britain, for the Year 1872*, pp. 5–12.

A crucial step forward for aeronautics happened in January 1866 when a group of serious aviation enthusiasts met for the first time, creating the Aeronautical Society of Great Britain. Later this organization of gentleman amateurs and professional engineers interested in flight became the Royal Aeronautical Society. Through its regular periodic meetings and publications, the study of human flight received an important early measure of scholarly credence and viability.

The first document below, from the year 1866, is one of the most interesting of the very early papers presented at a meeting of the Aeronautical Society of Great Britain. Its author, Francis H. Wenham (1824–1908), the son of an army surgeon with no college education, rose to great prominence in the emerging field of aeronautics; some even came to consider him "the father of aeronautics" in Britain. In this 1866 paper, published in the society's first annual report, Wenham focused on a principle that its author called "sustainment," by which he meant principally aerodynamic lift. Although his paper contributed an important idea in the form of his favorable analysis of what we would call a "high aspect-ratio" wing (with multiplane layout and propeller propulsion), most of Wenham's presentation suffered from a serious lack of clarity and depth regarding the basic sources of aerodynamic force. One of the reasons for this was Wenham's weakness in higher mathematics. As readers will see, his 1866 paper contained no mathematical equations; his basic inclination was always to solve technical problems through experimental means.

This experimental approach can be clearly seen in the second document below, from the Aeronautical Society of Great Britain's report of 1871, which discusses Wenham's design of what historians now recognize to be the world's first wind tunnel. Though Wenham used his primitive, steam-engine-driven wind tunnel (maximum velocity was forty miles per hour) solely to explore the lift and drag characteristics of flat surfaces, the type of device he pioneered showed great promise. In the following decade, in 1884, another member of the Aeronautical Society of Great Britain, Horatio Phillips, built a second, improved tunnel to demonstrate the improved lifting qualities of mildly cambered surfaces. Together, these two tunnels paved the way for what, by the Wright brother era, became the most basic and versatile tool in modern aeronautical experimentation.

Document 1-3(a), Francis H. Wenham, "On Aerial Locomotion," 27 June 1866.

The resistance against a surface of a defined area, passing rapidly through yielding media, may be divided into two opposing forces. One arising from the cohesion of the separated particles; and the other from their weight and inertia, which, according to well-known laws, will require a constant power to set them in motion.

In plastic substances, the first condition, that of cohesion, will give rise to the greatest resistance. In water this has very little retarding effect, but in air, from its extreme fluidity, the cohesive force becomes inappreciable, and all resistances are caused by its weight alone; therefore, a weight, suspended from a plane surface, descending perpendicularly in air, is limited in its rate of fall by the weight of air that can be set in motion in a given time.

If a weight of 150 lbs. is suspended from a surface of the same number of square feet, the uniform descent will be 1,300 feet per minute, and the force given out and expended on the, air, at this rate of fall, will be nearly six horse-power; and, conversely, this same speed and power must be communicated to the surface to keep the weight sustained at a fixed altitude. As the surface is increased, so does the rate of descent and its accompanying power, expended in a given time, decrease. It might, therefore, be inferred that, with a sufficient extent of surface reproduced, or worked up to a higher altitude, a man might by his exertions raise himself for a time, while the surface descends at a less speed.

A man, in raising his own body, can perform 4,250 units of work—that is, this number of pounds raised one foot high per minute—and can raise his own weight—say, 150 lbs.—twenty-two feet per minute. But at this speed the atmospheric resistance is so small that 120,000 square feet would be required to balance his exertions, making no allowance for weight beyond his own body.

We have thus reasons for the failure of the many misdirected attempts that have, from time to time, been made to raise weights perpendicularly in the air by wings or descending surfaces. Though the flight of a bird is maintained by a constant reaction or abutment against an enormous weight of air in comparison with the weight of its own body, yet, as will be subsequently shown, the support upon that weight is not necessarily commanded by great extent of wing-surface, but by the direction of motion.

One of the first birds in the scale of flying magnitude is the pelican. It is seen in the streams and estuaries of warm climates, fish being its only food. On the Nile, after the inundation, it arrives in flocks of many hundreds together, having migrated from long distances. A specimen shot was found to weigh twenty-one pounds, and measured ten feet across the wings, from end to end. The pelican rises with much difficulty, but, once on the wing, appears to fly with very little exertion, notwithstanding its great weight. Their mode of progress is peculiar and graceful. They fly after a leader, in one single train. As he rises or descends, so his followers do the same in succession, imitating his movements precisely. At a distance, this gives them the appearance of a long undulating ribbon, glistening under the cloudless sun of an oriental sky. During their flight they make about seventy strokes per minute with their wings. This uncouth-looking bird is somewhat whimsical in its habits. Groups of them may be seen far above the earth, at a distance from the river-side, *soaring*, apparently for their own pleasure. With outstretched and motionless wings, they float serenely, high in the atmosphere, for more than an hour together, traversing the same locality in circling movements. With head thrown back, and enormous bills resting on their breasts, they almost seem asleep. A few easy strokes of their wings each minute, as their momentum or velocity diminishes, serves to keep them sustained at the same level. The effort required is obviously slight, and not confirmatory of the excessive amount of power said to be requisite for maintaining the flight of a bird of this weight and size. The pelican displays no symptom of being endowed with great strength, for when only slightly wounded it is easily captured, not having adequate power for effective resistance, but heavily flapping the huge wings, that should, as some imagine, give a stroke equal in vigour to the kick of a horse.

During a calm evening, flocks of spoonbills take their flight directly up the river's course; as if linked together in unison, and moved by the same impulse, they alter not their relative positions, but at less than fifteen inches above the water's surface, they speed swiftly by with ease and grace inimitable, a living sheet of spotless white. Let one circumstance be remarked,—though they have fleeted past at a rate of near thirty miles an hour, so little do they disturb the element in which they move, that not a ripple of the placid bosom of the river, which they almost touch, has marked their track. How wonderfully does their progress contrast with that of creatures who are compelled to drag their slow and weary way against the fluid a thousand fold more dense, flowing in strong and eddying current beneath them.

Our pennant droops listlessly, the wished-for north wind cometh not. According to custom we step on shore, gun in hand. A flock of white herons, or "buffalo-birds," almost within our reach, run a short distance from the pathway as we approach them. Others are seen perched in social groups upon the backs of the apathetic and mud-begrimed animals whose name they bear. Beyond the ripening dhourra crops which skirt the river-side, the land is covered with immense numbers of blue pigeons, flying to and fro in shoals, and searching for food with restless diligence. The musical whistle from the pinions of the wood-doves sounds cheerily, as they dart past with the speed of an arrow. Ever and anon are seen a covey of the brilliant, many-coloured partridges of the district, whose *long and pointed wings* give them a strength and duration of flight that seems interminable, alighting at distances beyond the possibility of marking them down, as we are accustomed to do with their plumper brethren at home. But still more remarkable is the spectacle which the sky presents. As far as the eye can reach it is dotted with birds of prey of every size and description. Eagles, vultures, kites and hawks, of manifold species, down to the small, swallow-like, insectivorous hawk common in the Delta, which skims the surface of the ground in pursuit of its insect prey. None seem bent on going forward, but all are soaring leisurely round over the same locality, as if the invisible element which supports them were their medium of rest as well as motion. But mark that object sitting in solitary state in the midst of yon plain: what a magnificent eagle! An approach to within eighty yards arouses the king of birds from his apathy. He partly opens his enormous wings, but stirs not yet from his station. On gaining a few feet more he begins to walk away, with half-expanded, but motionless wings. Now for the chance fire! A charge of No. 3 from 11 bore rattles audibly but ineffectively upon his densely feathered body; his walk increases to a run, he gathers speed with his slowly-waving wings, and eventually, leaves the ground. Rising at a gradual inclination he mounts aloft and sails majestically away to his place of refuge in the Lybian range, distant at least five miles from where he rose. Some fragments of feathers denote the spot where the shot had struck him. The marks of his claws are traceable in the sandy soil, as, at first with firm and decided digs, he forced his way, but as he lightened his body and increased his speed with the aid of his wings, the imprints of his talons gradually merged into long scratches. The measured distance from the point where these vanished, to the place where he had stood, proved that with all the stimulus that the shot must have given to his exertions, he had been compelled to run full twenty yards before he could raise himself from the earth.

Again the boat is under weigh, though the wind is but just sufficient to enable us to stem the current. An immense kite is soaring overhead, scarcely higher than the top of our lateen yard, affording a fine opportunity for contemplating his easy and unlaboured movements. The cook has now thrown overboard some offal. With a solemn swoop the bird descends and seizes it in his talons. How easily he rises again with motionless expanded wings, the mere force and momentum of his *descent* serving to raise him again to more than half-mast high. Observe him next, with lazy flapping wings, and head turned under his body; he is placidly devouring the pendant morsel from his foot, and calmly gliding onwards.

The Nile abounds with large aquatic birds of almost every variety. During a residence upon its surface for nine months out of the year, immense numbers have been seen to come and go, for the majority of them are migratory. Egypt being merely a narrow strip of territory, passing through one of the most desert parts of the earth, and rendered fertile only by the periodical rise of the waters of the river, it is probable that these birds make it their grand thoroughfare into the rich districts of Central Africa.

On nearing our own shores, steaming against a moderate head-wind, from a station abaft the wheel the movements of some half-dozen gulls are observed, following in the wake of the ship, in patient expectation of any edibles that may be thrown overboard. One that is more familiar than the rest comes so near at times that the winnowing of his wings can be heard; he has just dropped astern, and now comes on again. With the axis of his body exactly at the level of the eyesight, his every movement can be distinctly marked. He approaches within ten yards, and utters his wild plaintive note, as he turns his head from side to side, and regards us with his jet black eye. But where is the angle or upward rise of his wings, that should compensate for his descending tendency, in a yielding medium like air? The incline cannot be detected, for, to all appearance, his wings are edgewise, or parallel to his line of motion, and he appears to skim along a *solid* support. No smooth-edged rails, or steel-tired wheels, with polished axles revolving in well oiled brasses, are needed here for the purpose of diminishing friction, for Nature's machinery has surpassed them all. The retarding effects of gravity in the creature under notice, are almost annulled, for he is gliding forward upon a *frictionless* plane. There are various reasons for concluding that the direct flight of many birds is maintained with a much less expenditure of power, for a high speed, than by any mode of progression.

The first subject for consideration is the proportion of surface, to weight, and their combined effect in descending perpendicularly through the atmosphere. The datum is here based upon the consideration of *safety*, for it may sometimes be needful for a living being to drop passively, without muscular effort. One square foot of sustaining surface, for every pound of the total weight, will be sufficient for security.

According to Smeaton's table of atmospheric resistances, to produce a force of *one pound* on a square foot, the wind must move against the plane (or, which is the same thing, the plane against the wind), at the rate of twenty-two feet per second, or 1,320 feet per minute, equal to fifteen miles per hour. The resistance of the air will now balance the weight on the descending surface, and, consequently, it cannot exceed that speed. Now, twenty-two feet per second is the velocity acquired at the end of a fall of eight feet—a height from which a well-knit man or animal may leap down without much risk of injury. Therefore, if a man with parachute weigh together 143 lbs., spreading the same number of square feet of surface contained in a circle fourteen and a half feet in diameter, he will descend at perhaps an unpleasant velocity, but with safety to life and limb.

It is a remarkable fact how this proportion of wing-surface to weight extends throughout a great variety of the flying portion of the animal kingdom, even down to hornets, bees, and other insects. In some instances, however, as in the gallinaceous tribe, including pheasants, this area is somewhat exceeded, but they are known to be very poor flyers. Residing as they do chiefly on the ground, their wings are only required for short distances, or for raising them or easing their descent from their roosting-places in forest trees, the *shortness* of their wings preventing them from taking extended flights. The wing-surface of the common swallow is rather more than in the ratio of *two* square feet per pound, but having also great length of pinion, it is both swift and enduring in its flight. When on a rapid course this bird is in the habit of furling its wings into a narrow compass. The greater extent of surface is probably needful for the continual variations of speed and instant stoppages requisite for obtaining its insect food.

On the other hand, there are some birds, particularly of the duck tribe, whose wing-surface but little exceeds *half* a square foot, or seventy-two inches per pound, yet they may be classed among the strongest and swiftest of flyers. A weight of one pound, suspended from an area of this extent, would acquire a velocity due to a fall of 16 feet—a height sufficient for the destruction or injury of most animals. But when the plane is urged forward horizontally, in a manner analogous to the wings of a bird during flight, the sustaining power is greatly influenced by *the form and arrangement* of the surface.

In the case of *perpendicular* descent, as a parachute, the sustaining effect will be much the same, whatever the figure of the outline of the superficies may be, and a circle perhaps affords the best resistance of any. Take for example a circle of 20 square feet (as possessed by the pelican) loaded with as many pounds. This, as just stated, will limit the rate of perpendicular descent to 1,320 feet per minute. But instead of a circle 61 inches in diameter, if the area is bounded by a parallelogram 10 feet long by 2 feet broad, and whilst at perfect freedom to descend perpendicularly, let a force be applied exactly in a horizontal direction, so as to carry it edgeways, with the long side foremost, at a forward speed of 30 miles per hour—just double that of its passive descent: the rate of fall under these conditions will be decreased most remarkably, probably to less than one-fifteenth part, or 88 feet per minute, or one mile per hour.

The annexed line represents transversely the plane 2 feet wide and 10 feet long, moving in the direction of the arrow with a forward speed of 30 miles per hour, or 2,640 feet per minute, and descending at 88 feet per minute, the ratio being as 1 to 30. Now, the particles of air, caught by the forward edge of the plane, must be carried down eight-tenths of an inch before they leave it. This stratum, 10 feet wide and 2,640 long, will weigh not less than 134 lbs.; therefore the weight has continually to be moved downwards, 88 feet per minute, from a state of absolute rest. If the plane, with this weight and an upward rise of eight-tenths of an inch, be carried forward at a rate of 30 miles per hour, it will be maintained at the same level without descending.

The following illustrations, though referring to the action of surfaces in a denser fluid, are yet exactly analogous to the conditions set forth in air:—

Take a stiff rod of wood, and nail to its end at right angles a thin lath or blade, about two inches wide. Place the rod square across the thwarts of a rowing-boat in motion, letting a foot or more of the blade hang perpendicularly over the side into the water. The direct amount of resistance of the current against the flat side of the blade may thus be felt. Next slide the rod to and fro thwart ship, keeping all square; the resistance will now be found to have increased enormously; indeed, the boat can be entirely stopped by such an appliance. Of course the same experiment may be tried in a running stream.

Another familiar example may be cited in the lee-boards and sliding keels used in vessels of shadow draught, *which act precisely on the same principle as the plane or wing-surface of a bird when moving in air*. These surfaces, though parallel to the line of the vessel's course, enable her to carry a heavy press of sail without giving way under the side pressure, or making lee-way, so great is their resistance against the rapidly passing body of water, which cannot be deflected sideways at a high speed.

The succeeding experiments will serve further to exemplify the action of the same principle. Fix a thin blade, say one inch wide and one foot long, with its plane exactly midway and at right angles, to the end of a spindle or rod. On thrusting this through a body of water, or immersing it in a stream running in the direction of the axis of the spindle, the resistance will be simply that caused by the water against the mere superficies of the blade. Next put the spindle and blade in rapid rotation. The retarding effect against direct motion will now be increased near *tenfold*, and is equal to that due *to the entire area of the circle of revolution*. By trying the effect of blades of various widths, it will be found that, for the purpose of effecting the maximum amount of resistance, the more rapidly the spindle revolves the narrower may be the blade. There is a specific ratio between the *width* of the blade and its *velocity*. It is of some importance that this should be precisely defined, not only for its practical utility in determining the best proportion of width to speed in the blades of screw-propellers, but also for a correct demonstration of the principles involved in the subject now under consideration; for it may be remarked that the swiftest-flying birds possess extremely long and *narrow* wings, and the slow, heavy flyers short and wide ones.

In the early days of the screw-propeller, it was thought requisite, in order to obtain the advantage of the utmost extent of surface, that the end view of the screw should present no opening, but appear as a complete disc. Accordingly, some were constructed with one or two threads, making an entire or two half-revolutions; but this was subsequently found to be a mistake. In the case of the two blades, the length of the screw was shortened, and consequently the width of the blades reduced, with increased effect, till each was brought down to considerably less than *one-sixth* of the circumference or area of the entire circle; the maximum speed was then obtained. Experiment has also shown that the effective propelling area of the two-bladed screw is tantamount to its entire circle of revolution, and is generally estimated as such.

Many experiments tried by the author, with various forms of screws, applied to a small steam-boat, led to the same conclusion—that the two blades of one-sixth of the circle gave the best result.

All screws reacting on a fluid such as water, must cause it to yield to some extent; this is technically known as "slip," and whatever the ratio or percentage on the speed of the boat may be, it is tantamount to *just so much loss of propelling power*—this being consumed in giving motion to the water instead of the boat.

On starting the engine of the steam-boat referred to, and grasping a mooring-rope at the stern, it was an easy matter to hold it back with one hand, though the engine was equal in power to five horses, and the screw making more than 500 revolutions per minute. The whole force of the steam was absorbed in "slip," or in giving motion to the column of water; but let her go, and allow the screw to find an abutment on a fresh body of water, not having received a gradual motion, and with its *inertia undisturbed* when running under full way, the screw worked almost as if in a solid nut, the "slip" amounting to only eleven per cent.

The laws which control the action of inclined surfaces, moving either in straight lines or circles in air, are identical, and serve to show the inutility of attempting to raise a heavy body in the atmosphere by means of rotating vanes or a screw acting vertically; for unless the ratio of surface compared to weight is exceedingly extensive, the whole power will be consumed in "slip," or in giving a downward motion to the column of air. Even if a sufficient force is obtained to keep a body suspended by such means, yet, after the desired altitude is arrived at, *no further ascension* is required; there the apparatus is to remain stationary as to level, and its position on the constantly yielding support can only be maintained at an enormous expenditure of power, for the screw cannot obtain a hold upon a *fresh and unmoved* portion of air in the same manner as it does upon the body of water when propelling the boat at full speed; its action under these conditions is the same as when the boat is held fast, in which case, although the engine is working up to its usual rate, the tractive power is almost annulled.

Some experiments made with a screw, or pair of inclined vanes acting vertically in air, were tried, in the following manner. To an upright post was fixed a frame, containing a bevil wheel and pinion, multiplying in the ratio of three to one. The axle of the wheel was horizontal, and turned by a handle of five-and-a-half inches radius. The spindle of the pinion rotated vertically, and carried two driving-pins at the end of a cross-piece, so that the top resembled the three prongs of a trident. The upright shaft of the screw was bored hollow to receive the middle prong, while the two outside ones took a bearing against a driving-bar, at right angles to the lower end of the shaft, the top of which ended in a long iron pivot, running in a socket fixed in a beam overhead; it could thus rise and fall about two inches with very little friction. The top of the screw-shaft carried a cross-arm, with a blade of equal size at each extremity, the distance from end to end being six feet. The blades could be adjusted at any angle by clamping-screws. Both their edges, and the arms that carried them, were bevilled away to a sharp edge to diminish the effects of atmospheric resistance. A wire stay was taken from the base of each blade to the bottom of the upright shaft, to give rigidity to the arms, and to prevent them from springing upwards. With this apparatus experiments were made with weights attached to the upright screw-shaft, and the blades set at different pitches, or angles of inclination. When the vanes were rotated rapidly, they rose and floated on the air, carrying the weights with them. Much difficulty was experienced in raising a heavy weight by a comparatively small extent of surface, moving at a high velocity; the "slip" in these cases being so great as to absorb all the power employed. The utmost effect obtained in this way was to raise a weight of six pounds on one square foot of sustaining surface, the planes having been set at a coarse pitch. To keep up the rotation, required about half the power a man could exert.

The ratio of weight to sustaining surface was next arranged in the proportion approximating to that of birds. Two of the experiments are here quoted, which gave the most satisfactory result. Weight of wings and shaft, 17 ½ oz.; area of two wings, 121 inches—equal to 110 square inches per pound. The annexed figures are given approximately, in order to avoid decimal fractions:—

	No. of revolutions per minute.	Mean sustaining speed. Miles per hour.	Feet per minute.	Pitch or angle of rise in one revolution. Inches.	Ratio of pitch to speed.	Slip per cent.
1st experiment	210	38	3,360	26	⅛ nearly	12 ½
2nd Do.	240	44	3,840	15	1/13 Do.	8

The power required to drive was nearly the same in both experiments—about equal to one-sixteenth part of a horse-power, or the third part of the strength of a man, as estimated by a constant force on the handle of twelve pounds in the first experiment, and ten in the second, the radius of the handle being five-and-a-half inches, and making seventy revolutions per minute in the first case, and eighty in the other.

These experiments are so far satisfactory in showing the small pitch or angle of rise required for sustaining the weight stated, and demonstrating the principle before alluded to, of the slow descent of planes moving horizontally in the atmosphere at high velocities; but the question remains to be answered, concerning the disposal of the excessive power consumed in raising a weight not exceeding that of a carrier pigeon, for unless this can be satisfactorily accounted for, there is but little prospect of finding an available power, of sufficient energy in its application to the mechanism, for raising apparatus, either experimental or otherwise, in the atmosphere. In the second experiment, the screw-shaft made 240 revolutions, consequently, one vane (there being two) was constantly passing over the *same spot* 480 times each minute, or eight times in a second. This caused a descending current of air, moving at the rate of near four miles per hour, almost sufficient to blow a candle out placed three feet underneath. This is the result of “slip,” and the giving both a downward and rotary motion to this column of air, will account for a great part of the power employed, as the whole apparatus performed the work of a blower. If the wings, instead of travelling in a circle, could have been urged continually forward in a straight line in a fresh and unmoved body of air the “slip” would have been so inconsiderable, and the pitch consequently, reduced to such a small angle, as to add but little to the direct forward atmospheric resistance of the edge.

The small flying screws, sold as toys, are well known. It is an easy matter to determine approximately the force expended in raising and maintaining them in the atmosphere. The following is an example of one constructed of tin-plate with three equidistant vanes. This was spun by means of a cord, wound round a wooden spindle, fitted into a forked handle as usual. The outer end of the coiled string was attached to a small spring steelyard, which served as a handle to pull it out by. The weight, or degree at which the index had been drawn, was *afterwards* ascertained by the mark left thereon by a pointed brass wire. It is not necessary to know the time occupied in drawing out the string, as this item in the estimate may be taken as the duration of the ascent; for it is evident that if the same force is re-applied at the descent, it would rise again, and a repeated series of these impulses will represent the power required to prolong the flight of the instrument. It is, therefore, requisite to know the length of string, and the force applied in pulling it out. The following are the data:

Diameter of screw	81 inches
Weight of ditto	396 grains

Length of string drawn out	2 feet
Force employed	8 lbs.
Duration of flight	16 seconds

From this it may be computed that, in order to maintain the flight of the instrument, a constant force is required of near sixty foot-pounds per minute—in the ratio of about three horse-power for each hundred pounds raised by such means. The force is perhaps over-estimated for a larger screw, for as the size and weight is increased, the power required would be less than in this ratio. The result would be more satisfactory if tried with a sheet-iron screw, impelled by a descending weight.

Methods analogous to this have been proposed for attempting aerial locomotion; but experiment has shown that a screw rotating in the air is an imperfect principle for obtaining the means of flight, and supporting the needful weight, for the power required is enormous. Suppose a machine to be constructed, having some adequate supply of force, the screw rotating vertically at a certain velocity will raise the whole. When the desired altitude is obtained, nearly the same velocity of revolution, and the same excessive power, must be continued, and consumed *entirely in "slip,"* or in drawing down a rapid current of air.

If the axis of the screw is slightly inclined from the perpendicular, the whole machine will travel forward. The "slip," and consequently the power, is somewhat reduced under these conditions; but a swift forward speed cannot be effected by such means, for the resistance of the inclined disc of the screw will be very great, far exceeding any form assimilating to the edge of the wing of a bird. But, arguing on the supposition that a forward speed of thirty miles an hour might thus be obtained, even then nearly all the power would be expended in giving an unnecessary and rapid revolution to an immense screw, capable of raising a weight, say of 200 pounds. The weight alone of such a machine must cause it to fail, and every revolution of the screw is a subtraction from the much-desired direct forward speed. A simple narrow blade, or inclined plane, propelled in a direct course at *this* speed—which is amply sufficient for sustaining heavy weights—is the best, and, in fact, the only means of giving the maximum amount of supporting power with the least possible degree of "slip," and direct forward resistance. Thousands of examples in Nature testify its success, and show the principle in perfection;—apparently the only one, and therefore beyond the reach of amendment, the wing of a bird, combining a propelling and supporting organ in one, each perfectly efficient in its mechanical action.

This leads to the consideration of the amount of power requisite to maintain the flight of a bird. Anatomists state that the pectoral muscles for giving motion to the wings are excessively large and strong; but this furnishes no proof of the expenditure of a great amount of force in the act of flying. The wings are hinged to the body like two powerful levers, and some counteracting force of a *passive* nature, acting like a spring under tension, must be requisite merely to balance the weight of the bird. It cannot be shown that, while there is no active motion, there is any real exertion of muscular force; for instance, during the time when a bird is soaring with motionless wings. This must be considered as a state of equilibrium, the downward spring and elasticity of the wings serving to support the body; the muscles, in such a case, performing like stretched india-rubber springs would do. The motion or active power required for the performance of flight must be considered exclusive of this.

It is difficult, if not impossible, by any form of dynamometer, to ascertain the precise amount of force given out by the wings of birds; but this is perhaps not requisite in proof of the principle involved, for when the laws governing their movements in air are better understood, it is quite possible to demonstrate, by isolated experiments, the amount of power required to sustain and propel a given weight and surface at any speed.

If the pelican referred to as weighing twenty-one pounds, with near the same amount of wing-area in square feet, were to descend perpendicularly, it would fall at the rate of 1,320 feet per minute, being limited to this speed by the resistance of the atmosphere.

The standard generally employed in estimating power is by the rate of descent of a weight. Therefore, the weight of the bird being 21 pounds, which, falling at the above speed will expend a force on the air set in motion nearly equal to one horse (.84 HP.) or that of 5 men; and conversely, to raise this weight again perpendicularly upon a yielding support like air, would require even more power than this expression, which it is certain that a pelican does not possess; nor does it appear that any *large* bird has the faculty of raising itself on the wing *perpendicularly* in a still atmosphere. A pigeon is able to accomplish this nearly, mounting to the top of a house in a very narrow compass; but the exertion is evidently severe, and can only be maintained for a short period. For its size, this bird has great power of wing; but this is perhaps far exceeded in the humming-bird, which, by the extremely rapid movements of its pinions, sustains itself for more than a minute in still air in one position. The muscular force required for this feat is much greater than for any other performance of flight. The body of the bird at the time is nearly vertical. The wings uphold the weight, not by striking vertically downwards upon the air, but as inclined surfaces reciprocating horizontally like a screw, but wanting in its continuous rotation in one direction, and, in consequence of the loss arising from rapid alternations of motion, the power required for the flight will exceed that specified in the screw experiment before quoted, viz.: three horse-power for every 100 pounds raised.

We have here an example of the exertion of enormous animal force expended in flight, necessary for the peculiar habits of the bird, and for obtaining its food; but in the other extreme, in large heavy birds, whose wings are merely required for the purposes of migration or locomotion, flight is obtained with the least possible degree of power, and this condition can only be commanded by a rapid straightforward course through the air.

The sustaining power obtained in flight must depend upon certain laws of action and reaction between relative weights; the weight of a bird, balanced, or finding an abutment, against the fixed inertia of a far greater weight of air, continuously brought into action in a given time. This condition is secured, not by extensive surface, but by great length of wing, which, in forward motion, takes a support upon a wide stratum of air, extending transversely to the line of direction.

The pelican, for example, has wings extending out 10 feet. If the limits of motion imparted to the substratum of air, acted upon by the incline of the wing, be assumed as one foot in thickness, and the velocity of flight as 30 miles per hour, or 2,640 feet per minute, the stratum of air passed over in this time will weigh nearly one ton, or 100 times the weight of the body of the bird, thus giving such an enormous supporting power, that the comparatively small weight of the bird has but little effect in deflecting the heavy length of stratum downwards, and, therefore, the higher the velocity of flight the less the amount of "slip," or power wasted in compensation for descent.

As noticed at the commencement or this paper, large birds may be observed to skim close above smooth water without ruffling the surface; showing that during rapid flight the air does not give way beneath them, but approximates towards a solid support.

In all inclined surfaces, moving rapidly through air, the whole sustaining power approaches toward the front edge; and in order to exemplify the inutility of surface alone, without proportionate length of wing, take a plane, ten feet long by two broad, impelled with the narrow end forward, the first twelve or fifteen inches will be as efficient at a high speed in supporting a weight as the entire following portion of the plane, which may be cut off, thus reducing the effective wing-area of a pelican, arranged in this direction, to the totally inadequate equivalent of two-and-a-half square feet.

One of the most perfect natural examples of easy and long-sustained flight is the wandering albatross. "A bird for endurance of flight probably unrivalled. Found over all parts of the Southern Ocean, it seldom rests on the water. During storms, even the most terrific, it is seen now dashing through the whirling clouds, and now serenely floating, without the least observable motion of its outstretched pinions." The wings of this bird extend fourteen or fifteen feet from end to end, and measure only eight-and-a-half inches across the broadest part. This conformation gives the bird such an extraordinary sustaining power, that it is said to *sleep* on the wing during stormy weather, when rest on the ocean is impossible. Rising high in the air, it skims slowly down, with absolutely motionless wings, till a near approach to the waves awakens it, when it rises again for another rest.

If the force expended in actually sustaining a long-winged bird upon a wide and unyielding stratum of air, during rapid flight, is but a small fraction of its strength, then nearly the whole is exerted in overcoming direct forward resistance. In the pelican referred to, the area of the body, at its greatest diameter, is about 100 square inches; that of the pinions, eighty. But as the contour of many birds during flight approximates nearly to Newton's solid of least resistance, by reason of this form, acting like the sharp bows of a ship, the opposing force against the wind must be reduced down to one third or fourth part; this gives one-tenth of a horse-power, or about half the strength of a man, expended during a flight of thirty miles per hour. Judging from the action of the living bird when captured, it does not appear to be more powerful than here stated.

The transverse area of a carrier pigeon during flight (including the outstretched wings) a little exceeds the ratio of twelve square inches for each pound, and the wing-surface, or sustaining area, ninety square inches per pound.

Experiments have been made to test the resisting power of conical bodies of various forms, in the following manner:—A thin lath was placed horizontally, so as to move freely on a pivot set midway; at one end of the lath a circular card was attached, at the other end a sliding clip traversed, for holding paper cones, having their bases the exact size of the opposite disc. The instrument acted like a steelyard; and when held against the wind, the paper cones were adjusted at different distances from the centre, according to their forms and angles, in order to balance the resistance of the air against the opposing flat surface. The resistance was found to be diminished nearly in the ratio that the height of the cone exceeded the diameter of base.

It might be expected that the pull of the string of a flying kite should give some indication of the force of inclined surfaces acting against a current of air; but no correct data can be obtained in this way. The incline of the kite is far greater than ever appears in the case of the advancing wing-surface of a bird. The tail is purposely made to give steadiness by a strong pull backwards from the action of the wind, which also exerts considerable force on the suspended cord, which for more than half its length hangs nearly perpendicularly. But the kite, as a means of obtaining unlimited lifting and tractive power, in certain cases where it might be usefully applied, seems to have been somewhat neglected. For its power of raising weights, the following quotation is taken from Vol. XLI. of the *Transactions of the Society of Arts*, relating to Captain Dansey's mode of communicating with a lee-shore. The kite was made of a sheet of holland exactly nine feet square, extended by two spars placed diagonally, and as stretched spread a surface of fifty-five square feet. "The kite, in a strong breeze, extended 1,100 yards of line five-eighths in circumference, and would have extended more had it been at hand. It also extended 360 yards of line, one and three-quarters of an inch in circumference, weighing sixty pounds. The holland weighed three and a half pounds; the spars, one of which was armed at the head with iron spikes, for the purpose of mooring it, six and three-quarter pounds; and the tail was five times its length, composed of eight pounds of rope and fourteen of elm plank, weighing together twenty-two pounds."

We have here the remarkable fact of ninety-two and a quarter pounds carried by a surface of only fifty-five square feet.

As all such experiments bear a very close relation to the subject of this paper, it may be suggested that a form of kite should be employed for reconnoitring and exploring purposes, in lieu of balloons held by ropes. These would be torn to pieces in the very breeze that would render a kite most serviceable and safe. In the arrangement there should be a smaller and upper kite, capable of sustaining the weight of the apparatus. The lower kite should be as nearly as practicable in the form of a circular flat plane, distended with ribs, with a car attached beneath like a parachute. Four guy-ropes leading to the car would be required for altering the angle of the plane—vertically with respect to the horizon, and laterally relative to the direction of the wind. By these means the observer could regulate his altitude, so as to command a view of a country in a radius of at least twenty miles; he could veer to a great extent from side to side, from the wind's course, or lower himself gently, with the choice of a suitable spot for descent. Should the cord break, or the wind fail, the kite would, in either case, act as a parachute, and as such might be purposely detached from the cord, which then being sustained from the upper kite, could be easily recovered. The direction of descent could be commanded by the guy-ropes, these being hauled taut in the required direction for landing.

The author has good reasons for believing that there would be less risk associated with the employment of this apparatus, than the reconnoitring balloons that have now frequently been made use of in warfare.

The wings of all flying creatures, whether of birds, bats, butterflies, or other insects, have this one peculiarity of structure in common. The front, or leading edge, is rendered rigid by bone, cartilage, or a thickening of the membrane; and in most birds of perfect flight, even the individual feathers are formed upon the same condition. In consequence of this, when the wing is waved in air, it gives a persistent force in one direction, caused by the elastic reaction of the following portion of the edge. The fins and tails of fishes act upon the same principle. In most rapid swimmers these organs are termed "lobated and pointed." The tail extends out very wide transversely to the body, so that a powerful impulse is obtained against a wide stratum of water, on the condition before explained. The action is imitated in Macintosh's screw-propeller, the blade of which his made of thin steel, so as to be elastic. While the vessel is stationary, the blades are in a line with the keel, but during rotation they bend on one side, more or less, according to the speed and degree of propulsion required, and are thus self-compensating; and could practical difficulties be overcome, would prove to be a form of propeller perfect in theory.

In the flying mechanism of beetles there is a difference of arrangement. When the elytra, or wing-cases, are opened, they are checked by a stop, which sets them at a fixed angle. It is probable that these serve as "aeroplanes," for carrying the weight of the insect, while the delicate membrane that folds beneath acts more as a propelling than a supporting organ. A beetle cannot fly with the elytra removed.

The wing of a bird, or bat, is both a supporting and a propelling organ, and flight is performed in a rapid course, as follows:—During the down-stroke it can be easily imagined how the bird is sustained; but in the up-stroke, the weight is also equally well supported, for in raising the wing, it is slightly inclined upwards against the rapidly passing air, and as this angle is somewhat in excess of the motion due to the raising of the wing, the bird is sustained as much during the up as the down-stroke—in fact, though the wing may be rising, the bird is still pressing against the air with a force equal to the weight of its body. The faculty of turning up the wing may be easily seen when a large bird alights; for after gliding down its aerial gradient, on its approach to the ground it turns up the plane of its wing against the air; this checks its descent, and it lands gently.

It has before been shown how utterly inadequate the mere perpendicular impulse of a plane is found to be in supporting a weight, when there is no horizontal motion at the time. There is no material weight of air to be acted upon, and it yields to the slightest force, however great the velocity of impulse may be. On the other hand, suppose that a large bird, in full flight, can make forty miles per hour, or 3,520 feet per minute, and performs one stroke per second. Now, during every fractional portion of that stroke, the wing is acting upon and obtaining an impulse from a fresh and undisturbed body of air; and if the vibration of the wing is limited to an arc of two feet, this by no means represents the small force of action that would be obtained when in a stationary position, for the impulse is secured upon a stratum of fifty-eight feet in length of air at each stroke. So that the conditions of weight of air for obtaining support equally well apply to weight of air, and its reaction in producing forward impulse.

So necessary is the acquirement of this horizontal speed, even in commencing flight, that most heavy birds, when possible, rise against the wind, and even run at the top of their speed to make their wings available, as in the example of the eagle, mentioned at the commencement of this paper. It is stated that the Arabs, on horseback, can approach near enough to spear these birds, when on the plain, before they are able to rise: their habit is to perch on an eminence, where possible.

The tail of a bird is not necessary for flight. A pigeon can fly perfectly with this appendage cut short off: it probably performs an important function in steering, for it is to be remarked, that most birds that have either to pursue or evade pursuit are amply provided with this organ.

The foregoing reasoning is based upon facts, which tend to show that the flight of the largest and heaviest of all birds is really performed with but a small amount of force, and that man is endowed with sufficient muscular power to enable him also to take individual and extended flights, and that success is probably only involved in a question of suitable mechanical adaptations. But if the wings are to be modelled in imitation of natural examples, but very little consideration will serve to demonstrate its utter impracticability when applied in these forms. The annexed diagram, fig. 1, would be about the proportions needed for a man of medium weight. The wings, *a a*, must extend out sixty feet from end to end, and measure four feet across the broadest part. The man, *b*, should be in a horizontal position, encased in a strong framework, to which the wings are hinged at *c c*. The wings must be stiffened by elastic ribs, extending back from the pinions. These must be trussed by a thin band of steel, *e e*, fig. 2, for the purpose of diminishing the weight and thickness of the spar. At the front, where the pinions are hinged, there are two levers attached, and drawn together by a spiral spring, *d*, fig. 2, the tension of which is sufficient to balance the weight of the body and machine, and cause the wings to be easily vibrated by the movement of the feet acting on treadles. This spring serves the purpose of the pectoral muscles in birds. But with all such arrangements the apparatus must fail—*length of wing is indispensable!* and a spar thirty feet long must be strong, heavy, and cumbrous; to propel this alone through the air, at a high speed, would require more power than any man could command.

In repudiating all imitations of natural wings, it does not follow that the only channel is closed in which flying mechanism may prove successful. Though birds do fly upon definite mechanical principles, and with a moderate exertion of force, yet the wing must necessarily be a vital organ and member of the living body. It must have a marvellous self-acting principle of repair, in case the feathers are broken or torn; it must also fold up in a small compass, and form a covering for the body. These considerations bear no relation to artificial wings; so in designing a flying-machine, any deviations are admissible, provided the theoretical conditions involved in flight are borne in mind.

Having remarked how thin a stratum of air is displaced beneath the wings of a bird in rapid flight, it follows that in order to obtain the necessary *length* of plane of supporting heavy weights, the surfaces may be superposed, or placed in parallel rows, with an interval between them. A dozen pelicans may fly one above the other without mutual impediment, as if framed together; and it is thus shown how two hundred weight may be supported in a transverse distance of only ten feet.

In order to test this idea, six bands of stiff paper, three feet long and three inches wide, were stretched at a slight upward angle, in a light rectangular frame, with an interval of three inches between them, the arrangement resembling an open Venetian blind. When this was held against a breeze, the lifting power was very great, and even by running with it in a calm it required much force to keep it down. The success of this model led to the construction of one of a sufficient size to carry the weight of a man. Fig. 3 represents the arrangement. *a a* is a thin plank, tapered at the outer ends, and attached at the base to a triangle, *b*, made of similar plank, for the insertion of the body. The boards, *a a*, were trussed with thin bands of iron, *c c*, and at the ends were vertical rods, *d d*. Between these were stretched five bands of holland, fifteen inches broad and sixteen feet long, the total length of the web being eighty feet. This was taken out after dark into a wet piece of meadow land, one November evening, during a strong breeze, wherein it became quite unmanageable. The wind acting upon the already tightly stretched webs, their united pull caused the central boards to bend considerably, with a twisting, vibratory motion. During a lull, the head and shoulders were inserted in the triangle, with the chest resting on the baseboard. A sudden gust caught up the experimenter, who was carried some distance from the ground, and the affair falling over sideways, broke up the right-hand set of webs.

In all new machines we gain experience by repeated failures, which frequently form the stepping-stones to ultimate success. The rude contrivance just described (which was but the work of a few hours) had taught, first, that the webs, or aeroplanes, must not be distended in a frame, as this must of necessity be strong and heavy, to withstand their combined tension; second, that the planes must be made so as either to furl or fold up, for the sake of portability.

In order to meet these conditions, the following arrangement was afterwards tried:—*a a*, figs. 4 and 5, is the main spar, sixteen feet long, half an inch thick at the base, and tapered, both in breadth and thickness, to the end; to this spar was fastened the panels *b b*, having a base-board for the support of the body. Under this, and fastened to the end of the main spar, is a thin steel tie-band, *e e*, with struts starting from the spar. This served as the foundation of the superposed aeroplanes, and, though very light, was found to be exceedingly strong; for when the ends of the spar were placed upon supports, the middle bore the weight of the body without any strain or deflection; and further, by a separation at the base-board, the spars could be folded back, with a hinge, to half their length. Above this were arranged the aeroplanes, consisting of six webs of thin holland, fifteen inches broad; these were kept in parallel planes, by vertical divisions, two feet wide, of the same fabric, so that when distended by a current of air, each two feet of web pulled in opposition to its neighbour; and finally at the ends (which were each sewn over laths), a pull due to only two feet had to be counteracted, instead of the strain arising from the entire length, as in the former experiment. The end-pull was sustained by vertical rods, sliding through loops on the transverse ones at the ends of the webs, the whole of which could fall flat on the spar, till raised and distended by a breeze. The top was stretched by a lath, *f* and the system kept vertical by staycords, taken from a bowsprit carried out in front, shown in fig. 6. All the front edges of the aeroplanes were stiffened by bands of crinoline steel. This series was for the supporting arrangement, being equivalent to a length of wing of ninety-six feet. Exterior to this, two propellers were to be attached, turning on spindles just above the back. They are kept drawn up by a light spring, and pulled down by cords or chains, running over pulleys in the panels *b b*, and fastened to the end of a swivelling cross-yoke, sliding on the base-board. By working this cross-piece with the feet, motion will be communicated to the propellers, and by giving a longer stroke with one foot than the other, a greater extent of motion will be given to the corresponding propeller, thus enabling the machine to turn, just as oars are worked in a rowing boat. The propellers act on the same principle as the wing of a bird or bat: their ends being made of fabric, stretched by elastic ribs, a simple waving motion up and down will give a strong forward impulse. In order to start, the legs are lowered beneath the baseboard, and the experimenter must run against the wind.

An experiment recently made with this apparatus developed a cause of failure. The angle required for producing the requisite supporting power was found to be so small, that the crinoline steel would not keep the front edges in tension. Some of them were borne downwards and more on one side than the other, by the operation of the wind, and this also produced a strong fluttering motion in the webs, destroying the integrity of their plane surfaces, and fatal to their proper action.

Another arrangement has since been constructed, having laths sewn in both edges of the webs, which are kept permanently distended by cross-stretchers. All these planes are hinged to a vertical central board, so as to fold back when the bottom ties are released, but the system is much heavier than the former one, and no experiments of any consequence have as yet been tried with it.

It may be remarked that although a principle is here defined, yet considerable difficulty is experienced in carrying the theory into practice. When the wind approaches to fifteen or twenty miles per hour, the lifting power of these arrangements is all that is requisite, and, by additional planes, can be increased to any extent; but the capricious nature of the ground-currents is a perpetual source of trouble.

Great weight does not appear to be of much consequence, *if carried in the body*; but the aeroplanes and their attachments seem as if they were required to be very light, otherwise, they are awkward to carry, and impede the movements in running and making a start. In a dead calm, it is almost impracticable to get sufficient horizontal speed, by *mere running* alone, to raise the weight of the body. Once off the ground, the speed must be an increasing one, if continued by suitable propellers. The small amount of experience as yet gained, appears to indicate that if the aeroplanes could be raised in detail, like a superposed series of kites, they would first carry the weight of the machine itself, and next relieve that of the body.

Until the last few months no substantial attempt has been made to construct a flying-machine, in accordance with the principle involved in this paper, which was written seven years ago. The author trusts that he has contributed something towards the elucidation of a new theory, and shown that the flight of a bird in its performance does not require that enormous amount of force usually supposed, and that in fact birds do not exert more power in flying than quadrupeds in running, but considerably less; for the wing movements of a large bird, travelling at a far higher speed in air, are very much slower; and, where weight is concerned, great velocity of action in the locomotive organs is associated with great force.

It is to be hoped that further experiments will confirm the correctness of these observations, and with a sound working theory upon which to base his operations, man may yet command the air with the same facility that birds now do.

The CHAIRMAN: "I think the paper just read is one of great interest and importance, especially as it points out the true mechanical explanation of the curious problem, as to how and why it is that birds of the most powerful flight always have the longest and narrowest wings. I think it quite certain, that if the air is ever to be navigated, it will not be by individual men flying by means of machinery; but that it is quite possible vessels may be invented, which will carry a number of men, and the motive force of which will not be muscular action. We must first ascertain clearly the mechanical principles upon which flight is achieved; and this is a subject which has scarcely ever been investigated in a scientific spirit. In fact, you will see in our best works of science, by the most distinguished men, the account given of the anatomy of birds is, that a bird flies by inflating itself with warm air, by which it becomes buoyant, like a balloon. The fact is, however, that a bird is never buoyant. A bird is immensely heavier than the air. We all know that the moment a bird is shot it falls to the earth; and it must necessarily do so, because one of the essential mechanical principles of flight is weight, without it there can be no momentum, and no motive force capable of moving through atmospheric currents.

"Until I read Mr. Wenham's paper, a few weeks since, I was puzzled by the fact, that birds with long and very narrow wings seem to be not only as efficient fliers, but much more efficient fliers than birds with very large, broad wings. If you observe the flight of the common heron—which is a bird with a very large wing, disposed rather in breadth than in length—you will notice that it is exceedingly slow, and that it has a very heavy, flapping motion. The common swallow, on the other hand, is provided with a long and narrow wing, and I never understood how it was that long-winged birds, such as these, achieved so rapid a flight, until I read Mr. Wenham's paper. Although I do not profess to be able to follow the elaborate calculations which he has laid before us, I think I now understand the explanation he has given. His explanation of the action of narrow wings upon the air is, that it is precisely like the action of the narrow vanes of the ship's screw in water, and that the resisting power of the screw is the same, or nearly the same, whether you have the total area of revolution covered by solid surface, or traversed by long and narrow vanes in rotation.

"If Mr. Wenham's explanation be nearly correct, that supposing this implement (referring to a model) to be carried forward by some propelling power, the sustaining force of the whole area is simply the sustaining force of the narrow band in front. This, however, is a matter which will have to be decided by experiment. It certainly appears to explain the phenomena of the flight of birds. There are one or two observations in the paper I do not quite agree with. Although I have studied the subject for many years, I have not arrived at Mr. Wenbam's conclusion that the upward stroke of a bird's wing has precisely the same effect as a downward stroke in sustaining. An upward stroke has a contrary effect to the downward stroke; it has a propelling power certainly, but I believe that the sustaining power of a bird's flight is due entirely to the downward stroke. I should be glad to hear what Mr. Wenham may have to say upon this. My belief is, that an upward stroke must have, so far as sustaining is concerned, a reverse action to the downward stroke.

"Then with regard to another observation of Mr. Wenham's, that the tails of birds are used as rudders. I believe this to be an entire mistake; for if the tail of a bird could have the slightest effect in guiding, the vane of it must be disposed perpendicularly, and not horizontally, or nearly so, as at present.

"If you cut off the tail of a pigeon, you will find that he can fly and turn perfectly well without it. He may be a little awkward about it at first, but that is because he has lost his balancing power. We all know that it is a common thing to see a sparrow without his tail, therefore, I do not in the least believe that tails have any effect in guiding. They have an important effect in stopping progress, and, undoubtedly, that is one of the necessary elements of turning. If a bird comes close over your head, and is frightened, you will find his claws distended and his tail spread out as a fan, to stop the momentum of his flight. These are the two only observations with which I cannot agree; but as regards the explanation he has given as to the resistance offered by long and narrow wings, he has made an important discovery."

Mr. WENHAM: "With regard to the wing not affording support to the bird during the upward stroke, some of the largest birds move their wings slowly, that is, with a less number than sixty strokes per minute. Now, as a body free to fall must descend fifteen feet in one second, whether in horizontal motion or not, it appears clear to me that there must be some counter-acting effect to prevent this fall. When the wing has reached the limit of the down-stroke, it is inclined upwards in the direction of motion, consequently the rush of air caused by the forward speed, weight, and momentum of the bird against the under surface of the wing, supports the weight, even though the wing is rising in the up-stroke at the time. In corroboration of my theory, I will read an extract from Sir George Cayley, who made a large number of experiments. He says, in page 83, of Vol. xxv., 'Nicholson's Journal':—'The stability in this position, arising from the centre of gravity, being below the point of suspension, is aided by a remarkable circumstance that experiment alone could point out. In very acute angles with the current, it appears that the centre of resistance in the sail does not coincide with the centre of its surface, but is considerably in front of it. As the obliquity of the current decreases, these centres approach and coincide when the current becomes perpendicular to the plane, hence any heel of the machine backwards or forwards removes the centre of support behind or before the point of suspension.'

"From this discovery, it seems remarkable that Sir George Cayley, finding that at high speeds with very oblique incidences the supporting effect became transferred to the front edge, the idea should not have occurred to him that a narrow plane, with its long edge in the direction of motion, would have been equally effective. I may give another illustration. We all know, from our schoolboy experience, that ice which would not be safe to stand upon, is found to be quite strong enough to bear heavy bodies passing over it, so long as rapid motion is kept up, and then it will not even crack. We know, also, that in driving through a marshy part of road, in which you expect the wheels to sink in up to the axles, you may pass over much more easily by increasing the speed. In both these examples there is a greater weight passed over in a given time, and consequently a better support obtained. The ice will not become deflected; neither has the mud time to give way. At a slow speed the same effect may be obtained by extending the breadth of the wheel. Thus, suppose an ordinary wheel to sink ten inches, if you double this width it will sink only five inches; and so on, until by extending the wheel into a long roller you may pass over a quicksand with perfect safety. Now, Nature has carried out this principle in the long wings of birds, and in the albatross it is seen in perfection."

Document 1-3(b), Report on the first wind tunnel built by Francis H. Wenham, 1871.

By the aid of a special subscription the Society has been enabled to present to its members some data with respect to the action of a current of air upon inclined planes of necessarily limited area, but varying angles. When the instrument, designed for this object by Mr. Wenham, was completed, with the aid of Mr. Browning, every facility was afforded for testing its capacities at Messrs. Penns' Engineering Works at Greenwich.

By means of a fan-blower, a current of considerable force was directed through a trunk ten feet long by eighteen inches square. The plane to be acted upon was fixed to the long end of a horizontal arm, which vibrated like the beam of a balance, and bore upon its shorter end a sliding counter weight, so as to balance the weight of any plane which might be fixed at the opposite extremity. The horizontal or direct pressure was read off by a spring steelyard, which was connected to the end of a lever from a vertical spindle close to the base of the machine. The vertical or sliding force due to the various inclinations was read off by an upright spring steelyard.

ANGLES OF INCLINED PLANES.

Forces in lbs.	0°	15°	20°	45°	60°	Force of wind 7⁄10 in water
Direct	3.24	0.33	0.52	2.4	3.05	Plane 1 square foot.
Vertical	0	1.5	1.8	2.4	1.7	
Direct	3.1	0.43	0.62	2.45	3.05	" 1 circular foot
Vertical	0	1.6	1.75	2.45	1.8	
Direct	2.2	0.23	0.38	1.25	1.62	" 4½in. x 18in. = 81 square inches
Vertical	0	1.05	1.2	1.25	1	
Direct	3.77	0.38	0.57	2.2	2.76	" 18in. x 9in. = 162 square inches.
Vertical	0	1.6	1.85	2.2	1.75	
						Force of wind 1 in water
Direct	4.29	0.62	0.95	3.74	4.75	Plane 1 square foot.
Vertical	0	2.35	2.7	3.47	2.45	
Direct	4.26	0.62	1	3.5	3.24	" 1 circular foot
Vertical	0	2.25	2.85	3.5	1.8	
Direct	2.8	0.3	0.57	1.6	1.52	" 4½in. x 18in.
Vertical	0	1.5	1.8	1.6	1	
						Force of wind 6⁄10 in water
Direct	3.24	0.43	0.76	2.7	2.7	Plane 1 square foot.

Vertical	0	1.6	2.05	2.7	1.6	
Direct	3.24	0.43	1.24	2.45	2.96	" 1 circular foot
Vertical	0	1.75	2.5	2.45	1.8	
Direct	2	0.29	0.38	1.3	1.57	" 4½in. x 18in., long edge to wind
Vertical	0	1.25	1.3	1.3	1	
Direct	2.57	0.24	0.43	1.65	2.1	" 4½in. x 18in., end on to wind,
Vertical	0	0.8	1.3	1.65	1.3	8/10 in water
Direct	*5 mean	1.47	0.76	2.9	3.81	Plane 18in. square = 2¼ square feet
Vertical	0	2	2.4	2.9	2.2	

*Fluctuates 0.57 lbs.

MEAN OF THE ABOVE COLUMNS.

Direct	3.31	0.4	0.68	2.34	2.73	Mean height of water, say 0.73in., which should represent 3.8lbs. per foot; readings are 1/7th too high for actual force per foot.
Vertical	0	1.6	1.96	2.34	1.26	

These experiments, when all the angles are averaged for errors, seem to indicate the law that the lifting force of inclined planes, carried horizontally through air, is increased in the direct ratio that the sine bears to the length of the plane, or the height to the incline to the base; thus, if instead of stating the angles in degrees, we say "one in ten," or "one in three or four," as the case may be, this will at once express the proportion in which the lifting force exceeds the resistance. The average of all results is very near to this, making a little allowance for the surface friction of the plane through the air. At 45° the two forces are equal; above this the proportions are in the inverse ratio, as the lifting force is then less than the direct.

It has been stated that the resistance of wedges or cones through the air is diminished directly in the ratio that the height or diameter of the base bears to the length of the cone. The experiments do not confirm this, but show that the resistance is less in proportion as the angle becomes more acute.

It will be seen on reference to these tables that as the angles become more acute, the lifting force exceeds the horizontal or power required to propel planes through air in an enormous ratio.

More acute angles than 15° were not experimented upon, but even at this the lift is four times greater than the thrust, and alone serves to abate the mystery relating to the support of weight in flight, which at least in the case of easy flying birds, consists in the action of surfaces at acute angles with the line of motion.

The experiments, though at present somewhat crude and incomplete, show that very oblique incidences or angles, with a small rise, have a remarkably strong lifting force compared with the power required to propel the plane, and that the ratio of the lift to the thrust greatly increases as the angle or rise diminishes, in all probability accounting for the long-sustained flight of birds with motionless wings. It is desirable that these important experiments should be verified and continued; the apparatus will be at the service of any member desirous of repeating them or trying others. For example, up to the present time only flat surfaces have been experimented upon, and as all the sustaining surfaces in the wings of birds are curved or hollow, it remains to be proved what is the relative advantage of this form, for it is upon such data that plans of construction must be based, and on which failure or success must depend, and to determine whether flight is practicable or not. The whole endeavour must be to find a support on the air in such a way that it cannot yield, so that there shall be as little mechanical loss as possible from what is known as "slip."

A most evident example of the enormous loss of power arising from "slip" may be cited in the plans that appear, from time to time, for raising men or machinery by means of vertical screws. Taking 200 lbs. as the lightest total weight of the man and machine, the following result will show how utterly inadequate his power is to raise any but the most trifling weight by such means. Assuming the surface or area of revolution at any number of square feet, the fraction of this weight of 200 lbs. is distributed over each foot of that surface. The force per square foot must therefore represent the wind velocity to produce the reaction or resistance necessary to support the weight. A surface of 25 square feet will then stand thus:—for 200 lbs. there is a resistance of 8 lbs. per foot required. To get this there must be a wind velocity of 3,600 ft. per minute, therefore 3,600 x 200 + 33,000 = 22 horse power. The following table will show the comparative results for four different diameters or areas of screw, making no allowance for the friction of the machine, thus demonstrating the hopelessness of any successful arrangement on this system.

Diam of screw	Area of revolution	Weight to be raised	lbs. per ft.	Velocity in ft. per minute	Horse power
5ft. 8in.	= 25 feet	200	= 8	3,600	= 22
8 0	= 50 "	200	= 4	2,600	= 16
11 3	= 100 "	200	= 2	1,800	= 11
16 0	= 200 "	200	= 1	1,300	= 8

The difference of power required to produce the same effect in the last and first case is nearly threefold; this arises from the increased slip of the smaller area screw.

The theory of the strong lifting power of planes at very oblique incidences, moving rapidly through the air, having now to some extent been practically tested, a few words may not be out of place concerning the position or arrangement of those planes. Mr. Wenham, in a paper read at the first meeting of the Aëronautical Society, brought forward a number of examples in evidence of this great lifting force, but without defining any exact law, merely relying upon it as a fact for the reasons given.

Then came the question, how to obtain the large extent of surface in a compass small enough to secure strength with lightness. In that essay he showed that an apparatus intended to support a weight of even 200 lbs. on one long extended plane, like the wings of a bird, would be an impracticable construction, requiring long and heavy spars. He then proposed to cut the planes into lengths, and superpose them, like a Venetian blind. It has been objected that this is wrong in theory, and that the action of one plane will interfere with the other; but the fact is, that there is no theory in the matter. It is simply a question of construction. The planes are equally effective in detachments, and may evidently be so arranged as not to interfere with each other. Thus, a series of planes made of thin silk, or tissue-paper, may be rigged, one above the other, like kites on a cord, and weighing not more than half an ounce each, and yet have ample strength to sustain one pound.

Though another season has elapsed, the Society has no announcement to make of the existence of a successful machine for aërial locomotion, or for travelling in any desired direction. Yet the retrospect of the past year shows a decided improvement in the form of experiments made to elucidate and define a correct law of action and propulsion of bodies sustained in air. We have no longer the extravagant and impossible theories of gravity and the laws of motion to account for this difficult phenomenon, so many of which were sent to the Society in the early days of its existence, the publication of which would in no way have contributed to the end in view, but, on the contrary, being wholly unsupported by any previous facts or experiments, would have brought discredit and ridicule upon Aëronautics, as the science which it may fairly be considered.

Should the problem be solved, the Aëronautical Society will take a high rank amongst its compeers, and cannot now be denounced as useless till flight for man is proved to be impossible. There are some amongst, not the least earnest of our members, who are quite willing to prove its impossibility, calmly regarding it as a mere scientific question; but, in fact, there is more difficulty in proving an impossibility than in constructing a machine having partial or total success, and we are far from being able to demonstrate the certainty of failure by any known laws, either of principle or construction. However plausible a mere argument unsupported by facts may appear, we have only to raise our eyes to the machinery of nature exemplified in large flying birds, and behold a reality yet incompletely explained, and not scientifically accounted for.

The Society may congratulate itself for having been the means of bringing together and recording a number of facts not lost upon its members, which have produced greater unanimity of purpose, and have, it is presumed, directed the efforts of experimentalists in accordance with a more generally recognized and practicable theory.

Document 1-3(c), Minutes of the Royal Aeronautical Society, 1872.

A General Meeting of the members of this Society was held in the Theatre of the Society of Arts, John Street, Adelphi, on Tuesday evening, the 18th inst. Mr. JAMES GLAISHER, F.R.S., presided.

A new machine, constructed under the direction of the Society, for measuring the relation between the velocity and pressure of the wind, was exhibited.

At the request of the CHAIRMAN,

The minutes of the previous meeting were read by Mr. F. W. BREAREY, the Hon. Secretary.

The Chairman: Ladies and Gentlemen,—the subject which will most naturally attract our attention this evening is that of the experiments which have been made by the apparatus now on the table before us. I had almost forgotten that at our last meeting we spoke of this instrument having been designed. It was not completed so soon as we expected; and, although much time has been occupied in making experiments, the results are not quite so conclusive as could be desired; but so far as they go are important—not only in respect to the problem we now wish to solve, but, as bearing upon the pressure of the wind on the surfaces of planes. I will not now engage your time longer, but I will ask Mr. Wenham, under whose care, in conjunction with Mr. Browning, the experiments were carried out, to give a statement respecting the results. It is an instrument of a kind which I have long desired, and it seems calculated to achieve what we require in this direction with greater accuracy than any other instrument I know. I call upon Mr. Wenham to explain the apparatus.

Mr. WENHAM expressed his regret at the absence of Mr. Browning, who had been associated with them in these experiments. To make this instrument understood, he would explain how it acted as an ordinary anemometer, for ascertaining the direct force of the wind on a plane, when in a vertical direction to its surface. This consists mainly of a vertical steel spindle, supported on a hardened steel centre. Through an eye at the upper end of the spindle, a horizontal arm passes, and is secured by a small cross-pin, which allows the arm to vibrate like the beam of a balance. The long end of the arm carries the planes; and the opposite short one has a sliding counter-weight, which is adjusted so as to exactly balance planes of different sizes at the long end of the arm. Each plane is clamped at the end of a tail rod, which is pivoted through the forked end of the arm, by a vertical steel pin, as close to the plane as possible; the other end of the tail passes loosely through a vertical slot, slightly curved as a radius, from the balance centre of the arm. By this arrangement, the surface of the plane is always kept at right angles to the current, throughout the extent of its horizontal motion. A wooden shield is fixed close before the front of the arm, to protect this and the balance weight from the wind, so that the planes only may be exposed to its force. The action of the instrument, as a single anemometer only, or when the planes are set at right angles to the current of air, is obvious. The direct pressure is read off by the spring steel-yard, which is connected to the end of a lever from the vertical spindle, close to the base of the machine. In order to measure the vertical forces, the planes are set at the requisite angles from a divided sector, whose centre coincides with the clamping screw at the back. The raising force due from the various inclines was read off by the upright spring steel-yard. It was found almost impossible for one observer to read off the horizontal and vertical forces simultaneously during fluctuations, therefore the readings were noted by two persons at a given signal—even this was a matter of some difficulty. The arrangement would be far more useful and perfect as a scientific machine, if fitted with a piece of clockwork, moving a paper cylinder, on which the vertical and direct forces would be simultaneously registered by separate pencils, describing two undulating lines, showing at a glance the relative forces; the experimenter would then have nothing else to attend to, but to see that all other conditions were acting properly.

The CHAIRMAN: I think the remarks by Mr. Wenham are important, especially with regard to the effects produced on the planes at different inclinations. When the plane was placed vertical, the pressure of the blast of air was direct, and tended only to move the plane in a horizontal direction—being that of the direction of the air itself—but when the plane was inclined, a part of the pressure was exerted in raising the plate in a vertical direction, and a part only in exerting a horizontal pressure; so that the latter was less than in the previous case. When the plane was placed at an angle of 45°, the horizontal force and the vertical force were found to be identical, as mentioned in the manner described by Mr. Wenham. It was also found that whether the exposed surface was a circle, a square, or a parallelogram, providing the area was the same, the results were identical to the degree of accuracy to which the readings could be determined. Anyone who had not considered with care the nature of the pressure produced by the flow or rush of a fluid, elastic or incompressible, against a plane surface placed in its course, might imagine that the system of parallel forces was merely equivalent to a single resultant force acting at the centre of pressure, and capable of resolution according to the ordinary parallelogram law. But this of course is not the case, for the particles of the fluid which come in contact with the plane have somehow or other to get out of the way, by gliding along the surface of the plane (as they cannot get through it), and this produces a complication in the neighbourhood of the surface of such a kind as cannot be theoretically predicted. One thing, however, is quite clear, and that is, that the directions of all the small forces acting on the surface certainly are not parallel, and that we must therefore have recourse to experiment. Even the fact that when the inclination of the plane to the current (supposed moving horizontally) is 45°, the vertical and horizontal pressures are equal, is not by any means evident; nor in fact can it be *exactly* true; for supposing (to fix the ideas) that the upper part of the plane is bent over so as to point in a direction opposed to that in which the current is moving, and making an angle of 45° with it, then most of the particles of air in the vicinity of the plane will, in order to get out of the way, be moving downwards along its surface; so that compounding this motion with that of the current, we should expect the horizontal force to be greater than the vertical. The experiments have shown that this difference is not appreciable to the extent to which the instrument can measure it. The same qualification also must be understood to apply to these results, from which it would appear that the pressure was independent of the form of the surface. The velocity of the current in these experiments was measured by a Lind's anemometer, an instrument that has never appeared to me to give very satisfactory results; but still the only one available for the purpose. I regret that the apparatus is considered by Mr. Browning to be too delicate to be used in the open air, but I hope that this will not be always found to be the case. As I have said before, difficulties exist only to be overcome, and some day I trust, we may obtain a series of experiments, in which ordinary wind will replace the use of the artificial current. I see Mr. Brooke present, who helped us with the experiments, and he may be able to say something as to the results gained.

Mr. BROOKE said it was not exactly mentioned, but the fact was notorious to everyone acquainted with mechanics, that in whatever position the plane was placed, the horizontal pressure may be resolved into two—one perpendicular to the plane, the other in the direction of the plane. It was clear that the resolved pressure acting in the direction of the plane was wholly effective in raising the plane. The resolution of the pressure into two was well known to everyone acquainted with the principles of mechanics; but it was to be understood that there were many other facts to be considered. The simple geometrical consideration of the action of the pressure upon the plane, did not involve the necessity for the particles of air which had impinged upon the plane, getting out of the way to enable other particles to impinge upon it. This led, in this experiment, to a result which might have been expected, but which it was important to ascertain. There were two rectangular planes of the same shape and area, and one was capable of being inclined lengthwise, in relation to the wind, and the other crosswise. Supposing the wind to be coming in a given direction (indicated as being towards the speaker) it was quite clear, with the plane inclined lengthwise, there would be less surface of the plane impinged upon than there would be in the transverse direction (indicated on the instrument). The particles which impinged upon the former must move along the plane and had much more difficulty in getting out of the way, than particles which impinged on the plane in the latter position. This would show that the effective pressure of the wind at the same velocity was greater upon the one plane than upon the other. And, conversely, a revolving, or oscillating plane, moving in the former direction (indicated), would move with less force than in the latter direction (indicated). And here was an illustration connected with the wings of birds, particularly of those that had powerful flight—where the wing was exceedingly long and narrow, it struck the wind in that direction (indicated). The experiment showed that from the same amount of surface there would be greater effect upon the air by a long narrow wing, than by a short and broad one of the same area. That was one of the results that had been obtained by these experiments.

Mr. WENHAM: I partly neglected to show how this illustrates the flight of birds. You will find that the lifting power of the smallest angle is nearly five times that of the direct force. We were not able to try less angles. The smaller the angle of inclination, in regard to the current, the less the direct force; and, comparatively, the lifting force is scarcely diminished. At 15 degrees, one force is nearly five times that of the other.

Mr. HARTE asked if, in making those experiments, attempts were made to ascertain any pressure of the wind downwards.

Mr. WENHAM: No! I omitted to mention that. A spirit level was laid across, so as to level the instrument. We had a trunk twelve feet long and eighteen inches square, to direct the current horizontally, and in a parallel course.

THE CHAIRMAN: Certain conditions of current were tried by Lind's Anemometer.

Mr. HARTE: Did you notice, in making these experiments, where the centre of pressure came?

Mr. WENHAM: We were not able to ascertain very accurately. In all cases there was a tendency to lift the front edge.

Mr. HARTE: Did you notice whether, according to the angle, the centre of pressure came forward?

Mr. WENHAM: We found as the angle became more acute, the centre of pressure came nearer to the front edge.

Mr. HALL (of Acton): Was the experiment made with a surface larger than one foot?

The CHAIRMAN: We had one eighteen inches square.

Mr. HALL: A different result would, I think, be attained within two feet from what was attained with one foot.

The CHAIRMAN: We have not spoken of two feet, because the shaft was scarcely large enough to give the even pressure required. We did not feel quite so certain with respect to large planes; and, therefore, the experiments with them are not included in these records; but I am ready to believe that the larger the planes, the larger the results. With areas of six inches, twelve inches, or two feet, the larger the area, the larger are the relative results. I have had three or four anemometers together, and always found this to be the case.

Mr. BROOKE: I rise to make an explanation. The 0 in the return ought to be 90. It ought to be 15, 20, 45, and 90.

Mr. F. W. BREAREY (the Secretary): If there is any gentleman here who could give us any advantage with regard to a fan-blower, we should be glad to avail ourselves of it. The area was so small that we could not expose much surface.

The CHAIRMAN: But we ought to give our thanks to Mr. Penn for the blower he lent to us and for the use of his steam power. The entire work of the shop was stopped during part of the time we occupied it. I should like to ask you to thank Mr. Penn for the facilities he gave us on that occasion for making these experiments. (Applause.)

Thanks were accorded to Mr. Penn by acclamation.

Document 1-4

Samuel P. Langley, *Experiments in Aerodynamics* (Washington, DC: Smithsonian Institution, 1891), excerpt "Langley's Law" reprinted in *The Aeronautical Annual* 1, James Means, ed., (Boston: W.B. Clarke & Co., 1895): 127–128.

Professor Samuel Pierpont Langley's paper, *Experiments in Aerodynamics*, published in 1891 by the Smithsonian Institution, has been called the "first substantive American contribution to aerodynamics." The following passage taken from this paper presents the theory that came to be known as "Langley's Law." This "law" stated that the power required for a vehicle to fly through the air decreased as the velocity increased—or, more simply, the higher the speed, the lower the drag. While this optimistic theory was subsequently realized to be in error, at the time it was published it represented an important new scientific approach to the study of aerodynamics. The problem for aeronautical experimenters and designers during Langley's time was that lift and drag forces were very difficult to measure with any precision, though Lilienthal, the Wrights, and other contemporaries came to suspect that Langley's law was badly mistaken.

Document 1-4, Samuel P. Langley, "Langley's Law," 1891.[28]

"To prevent misapprehension, let me state at the outset that I do not undertake to explain any art of mechanical flight, but to demonstrate experimentally certain propositions in aerodynamics which prove that such flight, under proper direction, is practicable. This being understood, I may state that these researches have led to the result that mechanical sustentation of heavy bodies in the air, combined with very great speeds, is not only possible, but within the reach of mechanical means we actually possess, and that while these researches are, as I have said, not meant to demonstrate the art of guiding such heavy bodies in flight, they do show that we now have the power to sustain and propel them.

Further than this, these new experiments (and theory, also, when reviewed in their light) show that if in such aerial motion, there be given a plane of fixed size and weight, inclined at such an angle, and moved forward at such a speed, that it shall be sustained in horizontal flight, then the more rapid the motion is, the *less*

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[28] This document does not include six figures that appeared in the original. All six are photographs of Otto Lilienthal making glider flights.

will be the power required to support and advance it. This statement may, I am aware, present an appearance so paradoxical that the reader may ask himself if he has rightly understood it. To make the meaning quite indubitable, let me repeat it in another form, and say that these experiments show that a definite amount of power so expended at any constant rate, will attain more economical results at high speeds than at low ones, *e.g.*, one horse-power thus employed will transport a larger weight at twenty miles an hour than at ten, a still larger at forty miles than at twenty, and so on, with an increasing economy of power with each higher speed, up to some remote limit not yet attained in experiment, but probably represented by higher speeds than have as yet been reached in any other mode of transport—a statement which demands and will receive the amplest confirmation later in these pages."

Document 1-5(a–d)[29]

(a) Otto Lilienthal, "The Problem of Flying," *Annual Report of the Board of Regents of the Smithsonian Institution*, July 1893 (Washington, DC: Government Printing Office, 1894), pp. 189–194; translated from German article published in Prometheus 4, No. 205 (1893): 769–744.

(b) Otto Lilienthal, "Practical Experiments in Soaring," *Annual Report of the Board of Regents of the Smithsonian Institution*, July 1893 (Washington, DC: Government Printing Office, 1894), pp. 195–199; translated from German article published in *Prometheus* 5, No. 220 (1893).

(c) Otto Lilienthal, "The Best Shapes for Wings," *Aeronautical Annual* (Boston: W.B. Clarke & Co., 1897), pp. 35–37; abridged translation from *Zeitschrift für Luftschiffahrt* 14.

(d) Vernon, "The Flying Man: Otto Lilienthal's Flying Machine," *McClure's Magazine* 3 (September 1894): 323–31.

Otto Lilienthal was one of the most inspiring aviation pioneers of the late nineteenth century. His gliding exploits were widely publicized and the articles about him and photographs of his flights captured the imaginations of people in Europe and America. His work galvanized others to action, and his martyrdom in a flying accident in August 1896 motivated others to increase efforts to develop a flying machine. Wilbur and Orville Wright were two men who were so influenced by him; news of Lilienthal's death sparked their initiation of active research into the problem of flight, and their early work was guided by Lilienthal's research and theories.

Representing Otto Lilienthal's evangelical writings on aviation are the first three documents below, translated from German and published in the United States. "The Problem of Flying" and "Experiments in Soaring" are taken from pieces included in 1893 in the German journal *Prometheus*, which subsequently appeared in English in the Smithsonian Institution's 1894 annual report. "The Best

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[29] All figures for this document are photographss and have been omitted here.

Shapes for Wings" is an abridged translation from the 1897 edition of *Aeronautical Annual*. The final piece, an article on "The Flying Man" that appeared in *McClure's Magazine* in September 1894, captures the technical genius inherent to Lilienthal's designs as well as the widespread popular enthusiasm for his intrepid flying.

Document 1-5(a), Otto Lilienthal, "The Problem of Flying," 1893.

While theoretically no difficulty of any considerable importance precludes flight, the problem can not be considered solved until the act of flying has been accomplished by man. In its application, however, unforeseen difficulties arise of which the theorist can have no conception.

The first obstacle to be overcome by the practical constructor is that of stability. It is an old adage that *"Wasser hat keine Balken"* [Water has no rafters]. What then, shall be said of air?

Leaving out of the question propelling mechanisms which require more than ordinary refinements of construction, theory teaches that a properly constructed flying apparatus may be brought to sail in a sufficiently strong wind; while in still air, such a machine may be made to glide downward upon a slightly inclined path. In the practical application of these two methods, however, it is found that while the apparatus is supported by moving air, it is also subjected to the whims of the wind, which often places it in uncomfortable positions, overturns it, or carries it into higher regions and then precipitates it, headforemost, to the ground. Lowering of the center of gravity is of little avail, nor does the most ingenious change of the wings or the steering surfaces alter the case. There is still no trace of the majestic soaring of the bird, for the wind is a treacherous fellow, who follows his own inclinations and laughs at our art. Therefore let us try the second method, the oblique descent in still air.

According to computation the apparatus should descend at a small angle, reaching the ground at a considerable distance, but this experiment is a success only in short flight. Beyond these the apparatus becomes unmanageable, darts vertically up, turns about, comes to a full stop, stands on its head, and descends with uncomfortable rapidity to the ground, the contact with which will probably have demolished the machine, if it do not turn a lucky somersault and land upon its back. Nor do repeated changes of the center of gravity alter the case beyond making it turn over backward instead of forward, leaving the conditions as unstable as before. Fancy the fate of the man who confides in such an apparatus.

Shall we now give up all hopes of success or shall we try new means to deprive the flying machine of its vicious propensities? This question has been answered in various ways. On the one hand it is thought that it should be possible, by mechanical means, to produce stable flight automatically, and an association of engineers of repute at Augsburg—an excellent proof that investigations of the art of flying have begun to be taken up by willing and self-sacrificing men—has among other things proposed mechanical contrivances for the regulation of soaring.

The apparatus is meant to descend from a captive balloon. By the application of ingenious methods the sailing surfaces (wings) are forced to retain their inclination. According to the report of Engineer M. Von Siegsfeld on the subject, no system has as yet been discovered that would promise sufficient security to any one sailing at a considerable elevation.

As desirable as it is that these investigations should discover safe automatic devices to give stability to soaring, it remains, on the other hand, doubtful whether the dangers attending such flights could even then be obviated. I am of the opinion that the evolution of the flying machine will be similar to that of the bicycle, which was not made in a day, and that this will not be either. Although in soaring the center of gravity may be placed below the center of pressure of the supporting air, it appears that even in this case, on account of the elasticity of the air itself, permanent stability could only be obtained by a constant and arbitrary correction of the position of the center of gravity. This is performed by birds incessantly and it is in virtue of a perfect adaptation of the form of their wings to any aerial motion that their flight appears to us so sure, graceful, and beautiful.

In the same way, a man can move through the air and have the general ability to guide his apparatus by constant shifting of the center of gravity. Descent should not be at first tried from great elevations, for such a feat requires practice. In the beginning, the height should be moderate and the wings not too large, or the wind will soon show that it is not to be trifled with. In fact, under some circumstances, one may be swept off toward still higher regions, the descent front which might well be disastrous. It therefore seems best that the wings should not exceed from 8 to 10 square meters (somewhat over 80 to 100 square feet), or that the experiment should be conducted in any wind blowing more than 5 meters per second (nearly 1,000 feet a minute), which represents a gentle breeze. A good run against the wind, however, and a leap from a safe height of 2 or 3 meters may secure a flight of 15 or 20 meters.

Constant practice will enable the experimenter to withstand a stronger breeze, to increase the surface of the wings to 15 square meters (160 square feet), and to start from a greater elevation, especially if there be a moderate slope beneath him with a soft, yielding surface. After becoming sufficiently expert to deviate from a straight line, the experimenter may enjoy the sensation of flying, but it is always a necessary condition that he should face the wind while descending, as the birds do. If then flight is attempted with the wind, it must be more rapid than the wind, or the result will be very apt to be a dangerous somersault at the time of coming to the ground, so that it is, on the whole, most advisable to follow the lessons of the birds, who ascend and descend against the wind.

I have been experimenting in this way for three years, and the constant progress made in the perfection of my machine, and the increased security it gives has convinced me of the correctness of the plan. At all events, I think it best to perfect the soaring apparatus before attempting flight with moveable wings.

After numerous experiments from low elevations, I gradually ventured to increase the height, and for this purpose I erected a tower-like shed, which, while it gave me room to store my apparatus, enabled me to conduct my experiments from the roof. The illustrations, taken from instantaneous photographs, show one of my securely constructed machines for soaring and the various phases of a soaring experiment.

Figure 1 represents the first leap from the roof, the cut showing the front view of the apparatus, which in some respects resembles the spread wings of a bat, and folds up like those for convenience of storage and transportation. The frame is of willow, covered with sheeting; the entire area contains nearly 150 square feet, and the entire apparatus weighs about 45 pounds. The roof of the tower is rather over 30 feet above the surrounding level, and from this elevation, after sufficient practice, one may glide over a distance of over 50 yards at an angle of descent of from 10 to 15 degrees.

Figures 2, 3, 4 show the porgies of the experiment. While flying freely in the air the proper angle of descent has to be regulated by shifting the center of gravity. Of course, the wind plays a very important part here, and it is only long and constant practice that we can learn to make allowance for its irregularities and to steer the apparatus properly. The capriciousness of the wind may exert unequal pressure on the great expanse of wing, and then it may happen that one wing will be elevated higher than the other.

This is shown in fig. 5. In this case the equilibrium may be restored by a change in the center of gravity, which may be effected by extending the legs as far to the left as possible, and thus adding more weight to the wing on that side. The two steering planes attached to the rear aid in enabling one to keep the face to the wind.

Figure 6 shows the simple manner of grasping the machine. There are no straps or buckles, and yet the connection is perfect. Each arm rests on a cushion attached to the framework, the hands seize a crossbar, and the remainder of the body hangs free.

My recent experiments have been made from hills having an elevation of about 250 feet and sloping uniformly every way at an angle of 10 to 15°. From the lower ridges I have already sailed a distance of over 250 yard. The great difficulty to be encountered in the endeavor to soar comes in learning to guide the flight, rather than in the difficulty of providing power to move the wings.

Progress in the mechanics of flying received at one time a severe check through the utterances of a high authority in physics. Starting with an erroneous hypothesis and putting too high a value on the amount of work required, he claimed that the maximum of possible flight had already been developed in the largest birds, and, as man represented about four times the heaviest of them, human flight was to be discarded as an utter impossibility. Now it must be admitted that the difficulties increase with the size of the flying individual; but flying itself is not the difficulty, for the largest flyers are at the same time the best flyers when once they get going in the air.

The object of this paper is to attempt to dispel old prejudices and to win new adherents for the problem in question. Even considered only as a physical exercise, the sport of flying would create one of the healthiest of all enjoyments and add one of the most effective remedies to the means now adopted for the conquest of those diseases which are now incident to our modern culture.

Document 1-5(b), Otto Lilienthal, "Practical Experiments in Soaring," 1893.

My own experiments in flying were begun with great caution. The first attempts were made from a grass plot in my own garden upon which, at a height of 1 meter from the ground, I had erected a springboard, from which the leap with my sailing apparatus gave me an oblique descent through the air. After several hundred of these leaps I gradually increased the height of my board to 2 1/2 meters, and from that elevation I could safely and without danger cross the entire grass plot. I then went to a hilly section, where leaps from gradually increased elevations added to my skill and suggested many improvements to my apparatus. The readers of *Prometheus* have already been informed of the selection of a piece of ground which enabled me to extend my flights over a distance of several hundred meters. The remainder of the summer since my last publication (in Nos. 204 and 205 of this journal) has sufficed to bring these experiments to a termination and to dispose of some important questions as to the possible results.

Indulging in subtle inquiries and theorizing does not promote our knowledge of flying, nor can the simple observation of natural flight, as useful as it may be, transform men into flying beings, although it may give us hints pointing towards the accomplishment of our purpose. We see buzzards rise skyward without any motion of their wings; we observe how the storks intermingle in the flock with outspread wings and in beautiful spirals; we see, high up in the air, the piratical falcon in quest of booty remain stationary in the wind for minutes at a time. We recognize every spot on his brownish plumage, but we do not perceive the least exertion of his wings to maintain his stationary position, and this small bird of prey is not in the least concerned at our presence. He reciprocates the protection secured for him since Brehm and other naturalists have pointed out his usefulness by undisturbedly precipitating himself to the grass before our eyes, and, seizing a grasshopper, we again see him meters above our heads without having detected the least flapping of his wings during the entire performance.

We notice that constant changes are going on in the force of the wind, but the falcon does not alter his position by a single inch, although having already begun devouring his prey, he can give but divided attention to his flight. Now he bends his head downward and backward, so that the world below must appear to him inverted, and evidently enjoys eating the insect as his talons leisurely pluck it to pieces. In the position in mid-air (which is maintained even during this employment) he appears like an automaton rooted in the wind. Just the faintest balancing motion, apparently serving to compensate for the irregularities of the wind, is perceptible in the extreme points of his wings, which are slightly inclined backwards.

The poise of the falcon in mid-air, which appears to us as a defiance to the law of gravity, may be considered not only the most remarkable, but also the most instructive example of flight.

In observing the majestic, circular soaring of other aërial travellers, one can readily believe that these skillful wing artists understand how to profit by the periodic currents of the air, and in describing spirals instinctively transform the force of the opposing current of air into lifting or suspensive power; but when the bird, without the least movement of his wings, remains stationary in one point of the sky, we are led to infer the existence of a peculiar form of surface which may be held suspended by the application of a uniformly moving wind.

While the existence of this possibility may be demonstrated by elementary experiments, this does not discover the secret of soaring, and though nature conclusively demonstrates that it can not be the want of power that prevents our flying, that knowledge alone does not provide us with wings. Furthermore, while nature points out how it is done, that does not necessarily imply that there may not be found other ways or means of doing it. However we may theorize on the subject, without a practical application of the theory, things will remain unchanged and our flight will only be in imagination or in dreams.

My experiments, then, should form the transition, the first step from theory to practice. Like others, I too have, in the beginning, attempted using machines with moveable wings, but this does not apparently aid in the development of an art of flight. The mark is too high and not immediately attainable, and one's ambition should be fully satisfied by withstanding the wind with wings of the size adapted to flying men. Each flight demands a rising from the ground and a landing; the former is as difficult as the latter is dangerous, and regardless of the most ingeniously constructed apparatus, the art of both will have to be acquired just as the child learns to stand and walk. Anyone desirous of exposing himself unnecessarily to danger and of ruining in a few seconds the carefully constructed apparatus need only expose his machine to the wind without having familiarized himself with its management, and he will soon know what it means to control an apparatus of from 10 to 15 square meters in area, where other people can but with difficulty manage an open umbrella.

To all those who, by their own experience or otherwise, can form a correct idea of the difficulties that present themselves, the instantaneous photographs by Mr. Alexander Krajewsky, accompanying this paper, may be of interest.

In continuation of my formerly published experiments, I endeavor with every new trial to gain more complete control over the wind, and without disregarding any necessary precaution I have already succeeded in at least temporarily retaining a uniform level and even in remaining stationary in the wind for a few seconds. The simplicity of my flying machine, which is controlled by shifting the center of gravity, has compelled me to avoid strong breezes, which however might presumably have aided in securing a stationary position. During my continued flights, however, I have been at times surprised by a sudden increase in the force of the wind which either carried me upward almost perpendicularly or supported me in a stationary position for a few seconds to the great delight of the spectators.

The freedom from accidents in these apparently daring attempts may be considered proof that the apparatus already described offers ample security in carrying out my plan of investigation.

To those who, from a modest beginning and with gradually increased extent and elevation of flight have gained full control over the apparatus, it is not in the least dangerous to cross deep and broad ravines.

It is a difficult task to convey to one who has never enjoyed aerial flight a clear perception of the exhilarating pleasure of this elastic motion. The elevation above the ground loses its terror, because we have learned by experience what sure dependence may be placed upon the buoyancy of the air. Gradual increase of the extent of these lofty leaps accustoms the eye to look unconcernedly upon the landscape below. To the mountain climber the uncomfortable sensation experienced in thrusting his foot into the slippery notch cut in the ice or to a treacherous rubble above deep abysses, with other dangers of the most terrifying nature, may often tend to lessen the enjoyment of the magnificent scenery. The dizziness caused by this, however, has nothing in common with the sensation experienced by him who trusts himself to the air; for the air demonstrates its buoyancy in not only separating him from the depth below, but also in keeping him suspended over it. Resting upon the broad wings of a well-tested flying machine, which, yielding to the least pressure of the body, obeys our directions; surrounded by air and supported only by the wind, a feeling of absolute safety soon overcomes that of danger.

One who has already practiced straight flights for some time will naturally endeavor to next guide his apparatus in a lateral direction, and indeed there is nothing easier than the guiding of the aerodrome, which is accomplished by shifting the center of gravity. The steering blades have nothing to do with this, their function being to keep the machine facing the wind.

Plate XIII, fig. 1 illustrates such a serpentine flight. I started from a hill to the right, the base of which is still visible in the figure, and soared toward the plain below in a somewhat circuitous path. The photograph was taken at the moment when I had almost turned my back to the plain. The view shown in Plate XIII, fig. 2, was taken at a time when I was lifted and carried upward by a suddenly increasing current which impeded progress and rendered me absolutely stationary.

In Plate XIV, flights are represented in geometric perspective. The lowest dotted line, *d*, *e*, was described during a calm. Even the expert flyer must descend during a calm at an angle of from 9° to 10°. The run began upon the top of the hill, near *a*; at *b* I left firm ground and endeavored to glide along the mountain slope, placing the wings at c at such an angle that the pressure of the wind, *L*, would not only support the machine, but also carry it forward. This increased the velocity sufficiently to enter at *d* into the line of stable flight. Such a maneuver is necessary, because a velocity of 9 meters per second is required for a flight in a calm, while but 6 meters were obtained by the run. At *e* the ground has almost been reached, and by raising the wings slightly in front the momentum is diminished and a landing effected without serious jar.

The second line, *e f*, shows a flight in a moderate breeze, in which the proper position with a downward inclination of 6° had been assumed immediately upon starting.

Flight against the wind is slower. The distance to be accomplished may be extended by a carefully determined and properly maintained inclination of the wings; in fact, by careful observation of this the soaring may be extended over a distance equal to ten times the height of the starting point.

During a strong breeze a sinuous line of flight results from the temporary support given by the wind at times. This is shown in the line *b g*, though such experiments should be undertaken only by one fully familiar with the management of the apparatus. The indefinable pleasure however experienced in soaring high up in the air, rocking above sunny slopes without jar or noise, accompanied only by the æolian music issuing from the wires of the apparatus, is well worth the labor given to the task of becoming and expert.

It does not seem at all impossible that the continuance of such flights may lead to free, continuous sailing in agitated air.

The results of our present experiments already furnish an indication of the degree of mechanical energy that must be added to that involved in oblique soaring to enable us to gain independent horizontal flight.

The solution of this problem, however, would exceed the purposes of the present article, and I content myself by stating that the conditions of a motor can easily be met, supposing that the propelling mechanism has been properly chosen, and that extraordinary lightness is not even essential.

The interests of the professional flight-essayer demand further experiments in practical flight and the gain of further efficiency. But even to those who only desire to utilize, as a means of sport, the results already obtained, opportunities are offered to promote the interest of the problem of flight and the way for a more ready prosecution of the subject.

The time has passed when every person harboring thoughts of aerial flight can at once be pronounced a charlatan. If we may hope that our aeronautic publications are eventually to be taken seriously by the majority of those skilled in allied subjects, it is important at the outside to awaken the interest of those whose natural concern this great problem should be, but who now shrug their shoulders. We shall then at least be able to show some practical results, and towards these ends we here take the first step.

Document 1-5(c), Otto Lilienthal, "The Best Shapes for Wings," 1897.

The results which we reach by practical flying experiments will depend most of all upon the shapes which we give to the wings used in experimenting.

Therefore there is probably no more important subject in the technics of flying than that which refers to wing formation.

The primitive idea that the desired effects could be produced by means of flat wings has now been abandoned, for we know that the curvature of birds' wings gives extraordinary advantages in flying.

The experiments on the resistance of air to curved surfaces have shown that even very slight curvatures of the wing-profile increase considerably the sustaining power, and thereby diminish the amount of power required in flight.

The wing of a bird is excellent not only because of the curvature of its cross-section, but the rest of its structure and formation also has influence upon the flight. Therefore the outline of the wing is certainly of importance.

It is probable that the form of the cross-section of the wing and flight-feathers *(Schwungfedern)* has a favorable influence upon the flight.

Experiments have not yet been made to show conclusively whether or not the feather structure of a wing endows it with a special quality whereby the sustaining power is increased. With investigators this has been a subject of conjecture. Therefore it is questionable *(auch fraglich)* whether we are wrong if, in constructing flying apparatus, we keep to the bat's wing, which is easier to construct.

Bats fly much better than is generally thought. Two early bats, which I saw flying this summer in broad sunshine and in somewhat windy weather, sailed along so well without flapping their wings that I thought, at first, they were swallows. Of course on evenings when there is no wind, the bat must flutter continually. The early-flying bat is also called evening-sailer *(Abendsegler)* which indicates that its sailing flight has been marked.

The most important point as regards the form of the wing will always be the curvature of its profile. If we examine any bird's wing we find that the enclosed bones cause a decided thickening at the forward edge. The question now is, What part does this thickening play in the action of the curved surface? The thickening is quite considerable, particularly in birds which have long, narrow wings. An albatross in my possession has a breadth of wing 16 centimetres, the thickened part of which measures 2 centimetres; the thickness is therefore 1/8 of the breadth of the wing. As the albatross is one of the best sailers, we can scarcely assume that the comparatively great thickness of the wing at its outer edge has a detrimental effect upon the bird's flight.

For a long time I have assumed that the thickening which all birds' wings have at the front edge produces a favorable effect in sailing flight. By means of free-sailing models I have now learned that nature makes a virtue of necessity, that the thickened front edge is not only harmless, but in sailing flight is helpful *(sondern den Schwebe-effect nicht unerheblich erhöht)*.

The experiments are easily tried. It is only necessary to make a number of models of equal size and weight, each one having a different curve in its sustaining surfaces. These models I make of strong drawing paper, the size of the surfaces being about 4 inches in width by 20 inches in length.

The experimenter can let these models sail from any tower or roof in front of which there is an open space. Each model must be made to glide through the air many times until it reaches the ground. Experiments must be made in the stillest possible air.

The lengths of flights are all noted down, and from a long series of experiments the arithmetical mean for each design is computed. The models having the best profiles will make the longest flights. In this way a reliable table can be made which will show the relative merits of the profiles, and will also show quite plainly in which direction the most useful form will have to be developed.

Until now I have endeavored to find out the best proportions for wings by constructing different kinds of sailing apparatus. In this way, of course, many important facts have been ascertained. The construction of full-sized apparatus requires a great deal of time and is expensive, therefore we must welcome a method which permits inquiry into the forms of wings in models which fly automatically. Besides that, it is not every one's business to throw himself into space in a sailing apparatus, although he who would succeed in practical flying can scarcely avoid this way.

Considering the fact that the most important thing is to ascertain what are the best qualities of the natural wing, which is in every respect perfect,—these steadily sailing models offer every one an opportunity of engaging in experiments of this kind. Further, any one who takes up this kind of experiment will find great pleasure in watching the manœuvers of his small flyers, which often vie with the best sailers among birds. I can therefore recommend this occupation not only for the furthering of the science of mechanical flight, but also because it affords a most interesting pastime.

The few measurements made so far by this method are too incomplete to be fit, as yet, for publication. I am preparing, however, a systematic series of experiments, the results of which will be stated when the experiments are finished.

Meanwhile, I cherish the hope that this paper may be an incentive to others to make similar experiments, so that we may sooner reach the desired end.

Document 1-5(d), Vernon, "The Flying Man," 1894.

Herr Otto Lilienthal, of Berlin, who has attained some celebrity as "The Flying Man," was born forty-six years ago in the antiquated little city of Anklarn, near the Baltic coast of Pomerania, about sixty miles to the northwest of Stettin. A residence so near the sea afforded him, in early life, many an opportunity of prosecuting his favorite studies and observations. In later years he migrated with his younger brother, Gustav, his enthusiastic coadjutor in all his researches in the domain of aviatics, to Berlin, where he established, and is now conducting, a large manufactory of small steam engines, whose mechanical appliances furnish him with every facility for the construction of his flying apparatus. He is an accomplished mathematician, and a close observer of nature; and is, besides, endowed in large measure with that poetic instinct which nearly always constitutes one side of even the most practical German character.

THE BIRD'S WING ON LILIENTHAL'S MODEL.

For more than twenty years Herr Lilienthal, with his brother's aid, and in the intervals of more serious occupations, has been studying the subject of aërial navigation. He has taken the flying bird as his teacher. After many experiments with flat wings or plane surfaces, he became convinced that it was the gentle parabolic curve of the wing which enables a bird to sustain itself without apparent effort in the air, and even to soar, without a motion of the wings, against the wind. This he has demonstrated not only by experiment, but by an application of the doctrine of the resolution of forces to the action of the wind upon a concave surface. The circling ascents of the carrier-pigeon, as he rises when released, to gain a general view of the landscape, and to take his bearings before starting on his homeward journey, depend upon this principle. He *flies* with the wind, but he *sails* or *soars* against it. The fins of many fishes and. the web feet of aquatic birds, are strikingly analogous in construction. The sails of a ship assume a similar form. It would be impossible to sail so near the wind if the instrument of propulsion were a rigid flat surface. It is the effort of the sail to get away from the wind, which it gathers in its ample bosom and drives the boat forward, almost in the very teeth of the breeze.

"There are still prominent investigators who *will not see*," said Herr Lilienthal to me, "that the arched or vaulted wing includes the secret of the art of flight. As we came upon the track of this idea, my brother and I, who were then young and wholly without means, used to spare from our breakfasts, penny by penny, the money to prosecute our investigations; and often the 'struggle for life' compelled us to interrupt them indefinitely. While we were devoting every moment of our spare time to the solution of the problem, almost every one in Germany regarded the man who would waste his energies in such unproductive labor as a fool. Years ago the most distinguished professor of mathematics in the Berlin Industrial Academy sent me word that of course it could *do no harm* to amuse myself with such pastimes, but warned me earnestly against putting any money into them. A special commission of experts, organized by the state, had, in fact, laid it down as a fundamental principle, once for all, that it was *impossible* for a man to fly. German societies for the promotion of aëronautics did not then exist, and those subsequently formed were devoted almost entirely to the interests of ballooning.

"I have always regarded the balloon, and the exclusive attention which it so long attracted, as a hindrance rather than a help to the development of the art of flight. If it had never been invented, it is probable that more serious investigations would have been prosecuted towards other solutions of the problem. Since the time of Montgolfier nearly all practical efforts have been directed to the improvement of the balloon. But it has nothing in common with the birds, and it is these that we must take as our model and exemplar. What we are seeking is the means of free motion in the air, in any direction. In this the balloon is of no aid; there is no relation between the two systems."

THE WING OF THE BIRD.

The wing of a bird is divided into three parts, corresponding to shoulder-joint, the forearm, and the hands and fingers of the human frame. The two former, composed largely of bones and muscles and tendons, are comparatively heavy, and their rapid movement demands the expenditure of considerable physical force; the last consists almost entirely of "pen-feathers," or pinions, which move to a certain extent automatically. In the larger birds—the "sailers" or "soarers," which alone are to be considered here—the first two members, with their concave under surfaces, furnish the sustaining power; and the last, being at the greatest distance from the shoulder, or axis of motion, the chief propulsive force. The construction of each member is peculiarly adapted to its special purpose, and it is this which Herr Lilienthal has endeavored to imitate.

An oarsman, on his forward stroke, opposes the blade of his oar almost perpendicularly to the resistance of the water. As he lifts it at the beginning of the backward stroke he "feathers" it, or brings it into a nearly horizontal position, so that its edge cuts the air. The pinions of birds act in precisely the same way. There are other analogies between the wing and the oar. The backstroke of the oar occupies only about half the time of the "pull," and the upstroke of the wing bears about the same relation to the downward beat. Moreover, at certain inclinations of the wing, the upward stroke, while detracting little or nothing from the sustaining power, contributes to the forward movement. As the pinions separate in consequence of the action of the air from above, they present their concave surface obliquely to the resisting medium and act like an oar in "sculling," which, whether moved to the right or the left, impels the boat forward. It is evident that this must greatly lighten the physical exertions of a bird in rapid flight, for in whichever direction he moves his wings he gains propulsive force.

To the conviction that concave or vaulted wings were essential to success, Herr Lilienthal was led not only by the examination of a great variety of natural wings, and by theoretical deduction, but by actual experiment. The means adopted for this purpose were ingenious and simple. He fitted up an apparatus in the form of the fly-fans found above the dining tables of clubs and restaurants, with two long arms revolving horizontally, to the ends of which surfaces of different kinds and degrees of curvature could be affixed in any required position. The motive power was furnished by a weight, and could be exactly measured. There was also an adjustment which enabled the observer to measure the lifting force of various surfaces, moving at different angles of inclination through still air. By this means Herr Lilienthal was enabled to reach conclusions which were of great value to him in the construction of his flying machine; and the most important of them was that the most effective form of wing was that whose convexity, as measured by the versed sine of the arc, should be one-twelfth of the breadth of the wing, or of the length of the chord connecting the opposite edges.

HERR LILIENTHAL'S WINGS.

The flying machine devised and now used by Herr Lilienthal is designed rather for *sailing* than for *flying*, in the proper sense of the term; or, as he says, "for being carried steadily and without danger, under the least possible angle of descent, against a moderate wind, from an elevated point to the plain below." It is made almost entirely of closely woven muslin, washed with collodion to render it impervious to air, and stretched upon a ribbed frame of split willow, which has been found to be the lightest and strongest material for this purpose. Its main elements are the arched wings; a vertical rudder, shaped like a conventional palm-leaf, which acts as a vane in keeping the head always towards the wind; and a flat, horizontal rudder, to prevent sudden changes in the equilibrium.

The operator so adjusts the apparatus to his person that, when in the air, he will be either resting on his elbows or seated upon a narrow support near the front. With the wings folded behind him he makes a short run from some elevated point, always against the wind, and when he has attained sufficient velocity launches himself into the air by a spring or a jump, at the same time spreading the wings which are at once extended to their full breath by atmospheric action; whereupon he sails majestically along like a gigantic seagull. In this way Herr Lilienthal has accomplished flights of nearly three hundred yards from the starting point.

"No one," said Herr Lilienthal to me, "can realize how substantial the air is, until he feels its supporting power beneath him. It inspires confidence at once. With flat wings it would be almost impossible to guard against a fall. With arched wings it is possible to sail against a moderate breeze at an angle of not more than six degrees to the horizon."

The principle is recognized in the umbrella-form universally adopted for the parachute. Try to run with an open umbrella held above the head and slightly inclined backwards, and see what a lifting power it exerts. Mechanical birds have been constructed with flat wings, which, so long as the machinery operated, were able to sustain themselves moderately well and to fly rapidly; but no one has yet succeeded in making any practical use of them. Their course has no intelligent direction; when the motive power gives out, they fall heavily to the earth. *Soaring*, in the sense of rising against the wind as the birds do, is possible only with dome-shaped wings. The aeroplane, or flat wing, when inclined at a certain angle to the breeze, may rise while its momentum continues, but once overcome its power is gone and nothing can restore it.

PROPER DIMENSIONS FOR THE WINGS.

"The curve of a bird's wing," said Herr Lilienthal, "is parabolic ; but the simple parabola differs so little from the arc of a circle that I adopted the latter curve as the more practicable, and the wings which I now use are in the main segments of a spherical surface. They are so constructed that they can be folded together like the wings of a bat, and require very little storage room when not in use.

"It was only gradually that I arrived at the proper dimensions. One does not easily gain an adequate conception of the materiality of the air, and my apprehensions led me at first to make the wings too large. I found that the varying force of the atmospheric currents, modified as they are by the undulations of the earth's surface, endangered my equilibrium in direct proportion to the spread of the wings. Those which I now employ are never more than twenty-three feet from tip to tip, and I am thus enabled, by a simple change of posture, so to alter the position of the centre of gravity as to restore the equilibrium.

"There are limits also to the *breadth* of the wings, or their extension backwards. The operator must be able in a moment to transfer the center gravity so far to the rear as to overcome the action of the air, which might otherwise tend to throw him forward, and precipitate him to the earth. When one feels himself falling, the natural impulse is to stretch out the arms and legs in the direction of the fall; but it is one of the peculiarities of this mode of navigation that the movement must be in the contrary direction, or towards the *upper* side. The centre of gravity is thus shifted to the one side or to the other, forward or backward, and the pressure of the air, acting with greater force on the lighter and broader surface, soon restores the equilibrium. It is not easy to realize in practice at first, but after a short experience the movement becomes almost involuntary.

"When there is no wind, the apparatus acts simply as a parachute. The pressure of the air is directly from beneath, and is equal on all parts of the under surface. I have more than once found myself in this position, when I have utilized the speed attained in a gradual descent, in rising to a greater height in order to soar over some obstacle like a tree or a crowd of people. Under favorable circumstances it is easy to mount to a height even greater than that of the starting point, but the forward motion is thereby partially or wholly neutralized, and it may happen that one comes to a complete standstill in mid-air. In such cases it is only necessary to throw the centre of gravity so far back that the air shall act more powerfully on the forward surface, and the gradual gliding descent is resumed. So in landing I bend backward, exactly as a crow does when alighting in a field, and reach the ground without the slightest shock. The worst that is likely to happen in any case is the breaking of the apparatus; there is little danger to life or limb.

"I am far from supposing that my wings, although they afford the means of *sailing*, and even of *soaring* in the air, possess all the delicate and subtle qualities necessary to the perfection of the art of flight. But my researches show that it is well worthwhile to prosecute the investigations farther."

THE LILIENTHAL MOTOR.

Having demonstrated the practicability of sailing and soaring, Herr Lilienthal has sought, in his recent experiments, to reach a practical solution of the problems of actual flight. The first difficulty to overcome was the discovery of a suitable motor, without which all efforts to fly would be hopeless. If we estimate the ordinary weight of a man at one hundred and sixty pounds, and add to that the weight of the flying apparatus, we have a total burden of at least two hundred pounds to be raised and supported simply by aerial resistance. It is calculated that to cover come the attraction of gravity in such a case requires a force of one and one quarter horse power, which no man is able to exert for more than a very short time.

With such an apparatus as Herr Lilienthal's, steam engines and electric motors are not readily available; but he conceived the ingenious idea of employing, as a motive force, the vapor of liquid carbonic acid, which, under ordinary atmospheric pressure, boils at a temperature far below that at which mercury freezes. His engine requires no fire, nor boiler, nor steam-chest; only a diminutive cylinder with the requisite valve arrangements, which may be readily worked by hand, and a small reservoir of the liquid acid lying close beside it.

The one first constructed was of two horse-power, with a receiver to contain enough carbonic acid to last for two hours, and was attached to the front of the flying apparatus. The whole contrivance, with the necessary machinery to impart motion to the wings, added less than twenty-five pounds to the weight, and this will probably be reduced in the future by the use of some alloy of aluminum, instead of iron, in the manufacture of the heavier portions. The wings were also fitted with rotatory pinions, constructed on the principles already indicated, and capable of automatic action under the pressure of the air.

The first experiments with this apparatus were rather too successful, at least in demonstrating the power of the engine. Unfortunately, the inventor had underestimated the energy of his motor, which acted with such unexpected vigor that the wings were broken, and the modifications thus shown to be necessary will require some time for their completion. Herr Lilienthal confidently expects, however, eventually to solve the problem in this way.

Document 1-6

Hiram Maxim, excerpts from "Natural and Artificial Flight," *Aeronautical Annual* (Boston: W.B. Clarke & Co., 1896), pp. 88–117.

Hiram Maxim was an American engineer who went to Europe in the 1880s to produce and sell small electrical appliances, but he instead gained fame and fortune with his invention in 1884 of a weapon capable of firing bullets in a sustained fully automatic mode, which he called a "machine gun." Working in London in the 1890s he turned his attention to the problem of mechanical flight and designed what was arguably the largest and most expensive attempt at a flying machine in the nineteenth century. His airplane was a failure, and in terms of the advancing science of aerodynamics he contributed little, but the association of his name and the many articles he wrote for popular journals did much to stimulate an increased interest in aerodynamic research. The document included here is one of those articles, published in the *Aeronautical Annual* 1896 edition, and consists of portions of a seven-part "thesis" outlining his own research.

Document 1-6, Hiram Maxim, excerpts from "Natural and Artificial Flight," 1896.

I. INTRODUCTORY.

At the time I commenced my experiments in aeronautics it was not generally believed that it would ever be possible to make a large machine heavier than the air that would lift itself from the earth by dynamic energy generated by the machine itself. It is true that a great number of experiments had been made with balloons, but these are in no sense true flying machines. Everyone who attempted a solution of the question by machines heavier than the air, was looked upon in very much the same light as the man is now who attempts to construct a perpetual motion machine. Up to within a few years, nearly all experiments in aerial navigation by flying machines have been made by men not versed in science, and who for the most part have been ignorant of the most rudimentary laws of dynamics. It is only quite recently that scientific engineers have taken up the question and removed it from the hands of charlatans and mountebanks. A few years ago many engineers would not have dared to face the ridicule which they would be liable to receive if they had asserted that it would be possible to make a machine that would lift itself by mechanical means into the air. However, thanks to the admirable work of Professor Langley, Professor Thurston, Mr. Chanute and others, one may now express his opinion freely on this subject and speculate as to the possibilities of making flying machines, without being relegated to the realm of cranks and fanatics.

During the last five years I have had occasion to write a large number of articles for the public press on this subject, and I have always attempted, as far as it is in my power, to discuss the subject in such a manner as to be easily understood by the unscientific, and I believe that my efforts have done something in the direction of popularizing the idea that it is possible to construct practical flying machines.

In preparing my present work, I have aimed as far as possible to discuss the question in plain and simple language, and to abstain from the use of any formulæ which may not be understood by every one. It has been my experience that if a work abounds in formulæ and tables, even only a few of the scientific will take the trouble to read or understand it. I have therefore confined myself to a plain statement of the actual facts, describing the character of my observations and experiments, and giving the results of the same. All experiments made by others in the same direction have been on a very small scale, and, as a rule, the apparatus employed has been made to travel around a circle, the size of which has not been great enough to prevent the apparatus continually encountering air which had been influenced in some way by the previous revolution.

The first experiments which I conducted were with an apparatus which travelled around a circle 200 feet in circumference, and by mounting some delicate anemometers directly under the path of the apparatus I ascertained that after it had been travelling at a high velocity for a few seconds, there was a well-defined air current blowing downward around the whole circle, so that my planes in passing forward must have been influenced and their lifting effect reduced to some extent by this downward current. My late experiments are the first which have ever been made with an apparatus on a large scale moving in a straight line. In discussing the question of aerial flight with Professor Langley before my large experiments had been made, the Professor suggested that there might be some unknown factor relating to size only which might defeat my experiments, and that none of our experiments had at that time been on a sufficiently large scale to demonstrate what the lifting effect of very large planes would be. A flying machine to be of any value must of necessity be large enough to carry at least one man, and the larger the machine the smaller the factor of the man's weight. Moreover, it is possible to make engines of say from 200 to 400 horse-power, lighter per unit of power than very small engines of from one to two horse-power. On the other hand, it is not advisable to construct a machine on too large a scale, because as the machine becomes larger the relative strength of the material becomes less. In first designing my large machine I intended that it should weigh about 5,000 pounds without men, water, or fuel, that the screw thrust should be 1,500 pounds, and that the total area of the planes should be 5,000 square feet. I expected to lift this machine and drive it through the air at a velocity of 35 miles an hour with an expenditure of about 250 horse-power. However, upon completing the machine I found that many parts were too weak, and these had to be supplanted by thicker and stronger material. This increased the weight of the machine about 2,000 pounds. Upon trying my engines I found that if required they would develop 360 horse-power, and that a screw thrust of over 2,000 pounds could be easily attained, but as an offset against this, the amount of power required for driving the machine through the air was a good deal more than I had anticipated.

II. NATURAL FLIGHT.

During the last 50 years a great deal has been said and written in regard to the flight of birds. Perhaps no other natural phenomenon has excited so much interest and has been so little understood. Learned treatises have been written to prove that a bird is able to develop from 10 to 100 times as much power for its weight as other animals, while other equally learned treatises have shown most conclusively that no greater amount of energy is exerted by a bird in flying than by land animals in running or jumping.

There is no question but what a bird has a higher physical development, as far as the generation of power is concerned, than any other animal we know of. Nevertheless, I think that every one who has made a study of the question will agree that some animals, such as rabbits, exert quite as much power in running in proportion to their weight as a sea-gull or an eagle exerts in flying.

The amount of power which a land animal has to exert is always a fixed and definite quantity. If an animal weighing 100 pounds has to ascend a hill 100 feet high, it always means the development of 10,000 foot-pounds. With a bird, however, there is no such thing as a fixed quantity, because the medium in which the bird is moving is never stationary. If a bird weighing 100 pounds should raise itself into the air 100 feet during a perfect calm, the amount of energy developed would be 10,000 foot-pounds plus the slip of the wings. But, as a matter of fact, the air in which a bird flies is never stationary, as I propose to show; it is always moving either up or down, and soaring birds, by a very delicate sense of feeling, always take advantage of a rising column of air. If a bird finds itself in a column of air which is descending, it is necessary for it to work its wings very rapidly in order to prevent a descent to the earth.

I have often observed the flight of hawks and eagles. They seem to glide through the air with hardly any movement of their wings. Sometimes, however, they stop and hold themselves in a stationary position directly over a certain spot, carefully watching something on the earth immediately below. In such cases they often work their wings with great rapidity, evidently expending an enormous amount of energy. When, however, they cease to hover and commence to move again through the air, they appear to keep themselves at the same height with an almost imperceptible expenditure of force.

Many unscientific observers of the flight of birds have imagined that a wind or a *horizontal* movement of the air is all that is necessary in order to sustain the weight of a bird in the air after the manner of a kite. If, however, the wind, which is only air in motion, should be blowing everywhere at exactly the same speed and in the same direction (horizontally), it would offer no more sustaining power to a bird than a dead calm, because there is nothing to prevent the body of the bird being blown along with the air, and whenever it had attained the same velocity as the air, no possible arrangement of the wings would prevent it from falling to the earth.

The wind, however, seldom or never blows in a horizontal direction. Some experimenters have lately asserted that if it were possible for us to ascend far enough, we should find the temperature constantly falling until at about 20 or 25 miles above the earth's surface the absolute zero might be reached. Now, as the air near the earth never falls in temperature to anything like the absolute zero, it follows that there is a constant change going on, the relatively warm air near the surface of the earth always ascending, and, in some cases, doing sufficient work in expanding to render a portion of the water it contains visible, forming clouds, rain, or snow, while the very cold air is constantly descending to take the place of the rising column of warm air.

On one occasion while crossing the Atlantic in fine weather, I noticed, some miles directly ahead of the ship, a long line of glassy water. Small waves indicated that the wind was blowing in the exact direction in which the ship was moving, and I observed as we approached the glassy line that the waves became smaller and smaller until they completely disappeared in a mirror-like surface which was about 300 or 400 feet wide and extended both to the port and starboard in approximately a straight line as far as the eye could reach. After passing the centre of this zone, I noticed that small waves began to show themselves, but in the exact opposite direction to those through which we had already passed. I observed that these waves became larger and larger for nearly an hour. Then they began to get gradually smaller, when I observed another glassy line directly ahead of the ship. As we approached it the waves completely disappeared, but after passing through it I noticed that the wind was blowing in the opposite direction and that the waves increased in size exactly in the same manner that they had diminished on the opposite side of the glassy zone.

This would seem to indicate that directly over the centre of the first glassy zone, the air was meeting from both sides and ascending, and that at the other glassy zone the air was descending in practically a straight line to the surface of the water where it spread out and set up a light wind in both directions.

I spent the winter of 1890–91 on the Riviera, between Hyères les Palmiers and Monte Carlo. The weather for the most part was very fine, and I often had opportunities of observing the peculiar phenomena which I had already noticed in the Atlantic, only on a much smaller scale. Whereas, in the Atlantic, the glassy zones were from 5 to 20 miles apart, I often found them not more than 500 feet apart in the bays of the Mediterranean.

At Nice and Monte Carlo this phenomenon was also very marked. On one occasion, while making observations from the highest part of the promontory of Monaco on a perfectly calm day, I noticed that the whole of the sea presented this peculiar effect as far as the eye could reach, and that the lines which marked the descending air were never more than a thousand feet from those which marked the centre of the ascending column. At about 3 o'clock in the afternoon, a large black steamer passed along the coast in a perfectly straight line, and I noticed that its wake was at once marked by a glassy line which indicated the centre of an ascending column. This line remained almost straight for two hours, when finally it became crooked and broken. The heat of the steamer had been sufficient to determine this upward current of air.

In 1893, I spent two weeks in the Mediterranean, going by a slow steamer from Marseilles to Constantinople and returning, and I had many opportunities of observing the peculiar phenomenon which I have before referred to. The steamer passed over thousands of square miles of calm sea, the surface being only disturbed by large batches of small ripples separated from each other by glassy streaks, and I found that in no case was the wind blowing in the same direction on both sides of these streaks, every one of them either indicating the centre of an ascending or a descending column of air.

If we should investigate this phenomenon in what might be called a dead calm, we should probably find that the air was rising straight up over the centres of some of these streaks, and descending in a vertical line over the centres of the others. But, as a matter of fact, there is no such thing as a dead calm. The movement of the air is the resultant of more than one force. The air is not only rising in some places and descending in others, but at the same time the whole mass is moving forward with more or less rapidity from one part of the earth to another. So we might consider that, instead of the air ascending directly from the relatively hot surface of the earth and descending vertically in other places, in reality it is moving on an incline.

Suppose that the local influence which causes the up and down motion of the air should be sufficiently great to cause it to rise at the rate of 2 miles an hour, and that the wind at the same time should be blowing at the rate of 10 miles an hour; the motion of the air would then be the resultant of these two velocities. In other words, it would be blowing up an incline of 1 in 5. Suppose now, that a bird should be able to so adjust its wings that it advanced 5 miles in falling 1 mile through a perfectly calm atmosphere; it would be able to sustain itself in an inclined wind, such as I have described, without any movement at all of its wings. If it was able to adjust its wings in such a manner that it could advance 6 miles by falling through 1 mile of air, it would then be able to rise as relates to the earth while in reality falling as relates to the surrounding air.

In conducting a series of experiments with artillery and small guns in a very large and level field just out of Madrid, I often observed the same phenomena as relates to the wind, that I have already spoken of as having observed at sea, except that the lines marking the centre of an ascending or a descending column of air were not so stationary as they were over the water. It was not an uncommon thing when adjusting the sights of a gun to fire at a target at very long range, making due allowances for the wind, to have the wind change and blow in the opposite direction before the word of command was given to fire. While conducting these experiments, I often noticed the flight of eagles. On one occasion a pair of eagles came into sight on one side of the plain, passed directly over our heads and disappeared on the opposite side. They were apparently always at the same height from the earth and soared completely across the plain without once moving their wings. This phenomenon, I think, can only be accounted for on the hypothesis that they were able to feel out with their wings an ascending column of air, that the centre of this column of air was approximately a straight line running completely across the plain, that they found the ascending column to be more than necessary to sustain their weight in the air, and that whereas, as relates to the earth, they were not falling at all, they were really falling some 2 or 3 miles an hour in the air which supported them.

Again, at Cadiz in Spain, when the wind was blowing in very strongly from the sea, I noticed that the sea-gulls always took advantage of an ascending column of air. As the wind blew in from the sea and rose to pass over the fortifications, the seagulls selected a place where they could slide down on the ascending current of air, keeping themselves always approximately in the same place without any apparent exertion. When, however, they left this ascending column, I observed that it was necessary for them to work their wings with great vigor until they again found the proper place to encounter the favorable current.

I have often noticed sea-gulls following a ship. I have observed that they are able to follow the ship without any apparent exertion; they simply balance themselves on an ascending column of air and seem to be quite as much at ease as they would be if they were roosting on a solid support. If, however, they are driven out of this position, I find that they generally have to commence at once to work their passage. If anything is thrown overboard which is too heavy for them to lift, the ship soon leaves them, and in order to catch up with it again, they move their wings very much as other birds do; but when once established in the ascending column of air, they manage to keep up with the ship by doing little or no work. In a head wind we find them directly aft of the ship; if the wind is from the port side, they may always be found on the starboard quarter, and *vice versa*.

Every one who has passed a winter on the northern shores of the Mediterranean must have observed the cold wind which is generally called the *mistral*. One may be out driving, the sun may be shining brightly, and the air be warm and balmy, when, suddenly, without any apparent cause, one finds himself in a cold descending wind. This is the much-dreaded mistral, and if at sea, it would be marked by a glassy line on the surface of the water. On land, however, there is nothing to render its presence visible. I have found that the ascending column of air is always very much warmer than the descending column, and that this action is constantly taking place in a greater or less degree.

From the foregoing deductions I think we may draw the following conclusions:

First, that there is a constant interchange of air taking place, the cold air descending, spreading itself out over the surface of the earth, becoming warm, and ascending in other places.

Second, that the centres of the two columns are generally separated from each other by a distance which may be from 500 feet to 20 miles.

Third, that the centres of greatest action are not in spots, but in lines which may be approximately straight but generally abound in many sinuosities.

Fourth, that this action is constantly taking place over both the sea and the land, that the soaring of birds, a phenomenon which has heretofore been so little understood, may be accounted for on the hypothesis that the bird seeks out an ascending column of air, and that, while sustaining itself at the same height in the air without any muscular exertion, it is in reality falling at a considerable speed through the air that surrounds it.

It has been supposed by some scientists that the birds may take advantage of some vibratory or rolling action of the air. I find, however, from careful observation and experiment, that the motion of the wind is comparatively steady, and that the short vibratory or rolling action is always very near to the earth and is produced by the air flowing over the tops of hills, high buildings, or trees. If a kite is flown only a few feet above the ground, it will be found that the current of air is very unsteady. If it is allowed to mount to 500 feet, the unsteadiness nearly all disappears, while if it is further allowed to mount to a height of 1,500 or 2,000 feet, the pull on the cord is almost constant, and, if the kite is well made, it remains practically stationary in the air.

I have often noticed in high winds, that light and fleecy clouds come into view, say, about 2,000 feet above the surface of the earth, and that they pass rapidly and steadily by, preserving their shape completely. This would certainly indicate that there is no rapid local disturbance in the air in their immediate vicinity, but that the whole mass of air in which these clouds are formed is practically travelling in the same direction and at the same velocity. Numerous aeronauts have also testified that, no matter how hard the wind may be blowing, the balloon is always practically in a dead calm, and if a piece of gold-leaf is thrown overboard even in a gale, the gold-leaf and the balloon never part company in a horizontal direction, though they may in a vertical direction.

Birds may be divided into two classes: first, the soaring birds, which practically live upon the wing, and which, by some very delicate sense of touch, are able to feel the exact condition of the air. Many fish which live near the top of the water are greatly distressed by sinking too deeply, while others which live at great depths are almost instantly killed by being raised to the surface. The swim bladder of a fish is in reality a delicate barometer provided with sensitive nerves which enable the fish to feel whether it is sinking or rising in the water. With the surface fish, if the pressure becomes too great, the fish involuntarily exerts itself to rise nearer the surface and so diminish the pressure, and I have no doubt that the air-cells, which are known to be very numerous and to abound throughout the bodies of birds, are so sensitive as to enable soaring birds to know at once whether they are in an ascending or a descending column of air.

The other class of birds consists of those which only employ their wings for the purpose of taking them rapidly from one place to another. Such birds may be considered not to expend their power so economically as the soaring birds. They do not spend a very large portion of their time in the air, but what time they are on the wing they exert an immense amount of power and fly very rapidly, generally in a straight line, taking no advantage of air currents. Partridges, pheasants, wild ducks, geese, and some birds of passage may be taken as types of this kind. This class of birds has relatively small wings, and carries about 21 times as much weight per square foot of surface as soaring birds do.

[. . .]

IV. THE ADVANTAGES AND DISADVANTAGES OF VERY NARROW PLANES.

My experiments have demonstrated that relatively narrow aeroplanes lift more per square foot than very wide ones, but as an aeroplane, now matter how narrow it may be, must of necessity have some thickness, it is not advantageous to place them too near together. Suppose that aeroplanes should be made ¼ in. thick and be superposed 3 inches apart, that is, at a pitch of 3 inches. One-twelfth part of the whole space through which these planes would have to be driven would be occupied by the planes themselves, and eleven-twelfths would be air space (Fig. 1). If a group of planes thus mounted should be driven through the air at the rate of 36 miles an hour, the air would have to be driven forward at the rate of 3 miles an hour, or else it would have to be compressed, or spun out, and pass between the spaces at a speed of 39 miles an hour. As a matter of fact, however, the difference in pressure is so very small, that practically no atmospheric compression takes place. The air, therefore, is driven forward at the rate of 3 miles an hour, and this consumes a great deal of power, in fact, so much that there is a decided disadvantage in using narrow planes thus arranged.

In regard to the curvature of narrow aeroplanes, I have found that if one only desires to lift a large load in proportion to the area, the planes may be made very hollow on the underneath side; but when one considers the lift in terms of screw thrust, I find it advisable that the planes should be as thin as possible and the underneath side nearly flat. I have also found that it is a great advantage to

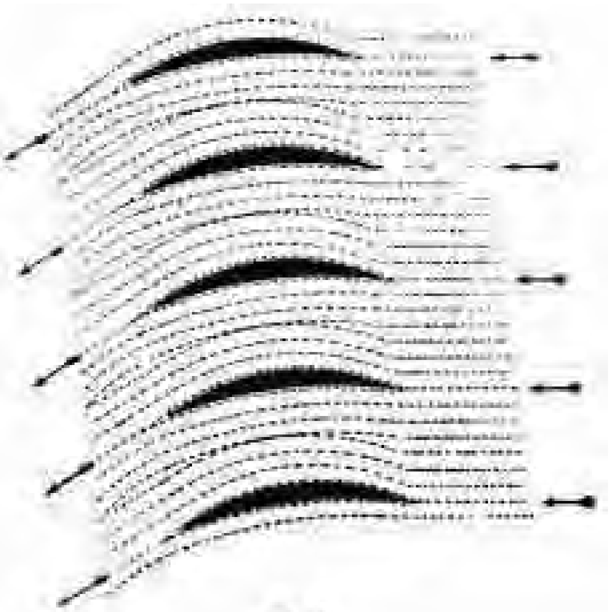


Fig. 1.

arrange the planes after the manner shown in fig. 2. In this manner, the sum of all the spaces between the planes is equal to the whole area occupied by the planes; consequently, the air neither has to be compressed, spun out, or driven forward. I am therefore by this arrangement able to produce a large lifting effect per square foot, and, at the same time, to keep the screw thrust within reasonable limits.

A large number of experiments with very narrow aeroplanes have been conducted by Mr. Horatio Phillips at Harrow, in England. . . . Mr. Phillips is of the opinion that the air in striking the top side of the plane is thrown upward in the manner shown and a partial vacuum is thereby formed over the central part of

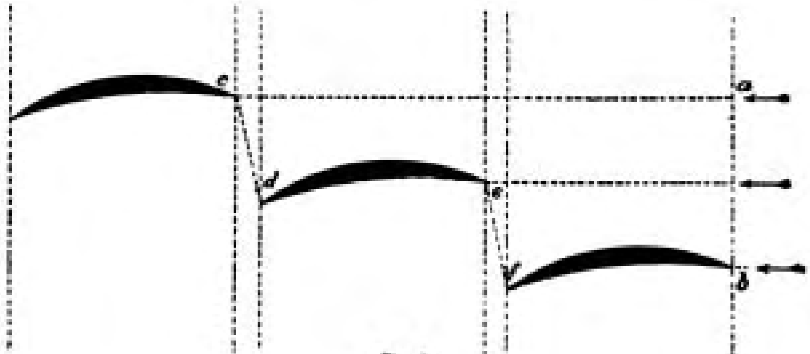


Fig. 2.

Fig. 3.

the plane, and that the lifting effect of planes made in this form is therefore very much greater than with ordinary narrow planes. I have experimented with these "sustainers" (as Mr. Phillips calls them) myself, and I find it is quite true that they lift in some cases as much as 8 lb. per sq. ft., but the lifting effect is not produced in the exact manner that Mr. Phillips seems to suppose. The air does not glance off in the manner shown. As the "sustainer" strikes the air, two currents are formed, one following the exact contour of the top and the other the bottom. These two currents join and are thrown downward as relates to the "sustainer" at an angle which is the resultant of the angles at which the two currents meet (Fig. 4). These "sustainers" may be made to lift when the front edge is lower than the rear edge because they encounter still air, and leave it with a downward motion.

In my experiments with narrow superposed planes, I have always found that with strips of thin metal made sharp at both edges and only slightly curved, the lifting effect, when considered in terms of screw thrust, was always greater than with any arrangement of the wooden aeroplanes used in Phillips' experiments. It would therefore appear that there is no advantage in the peculiar form of "sustainer" employed by this inventor.

If an aeroplane be made perfectly flat on the bottom side and convex on the top . . . and be mounted in the air so that the bottom-side is exactly horizontal, it pro-

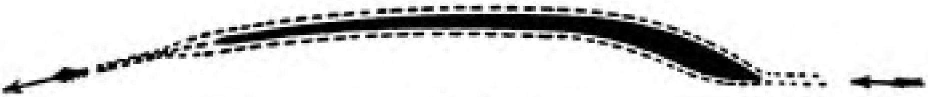


Fig. 4.

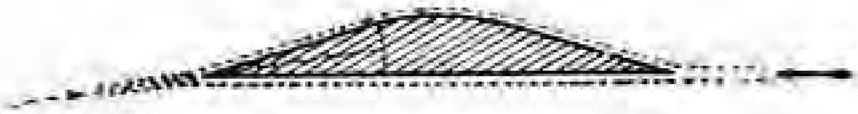


Fig 5.

duces a lifting effect no matter in which direction it is run, because as it advances it encounters stationary air which is divided into two streams. The top stream being unable to fly off at a tangent when turning over the top curve, flows down the incline and joins the current which is flowing over the lower horizontal surface. The angle at which the combined stream of air leaves the plane is the resultant of these two angles; consequently, as the plane finds the air in a stationary condition and leaves it with a downward motion, the plane itself must be lifted. It is true that small and narrow aeroplanes may be made to lift considerably more per square foot of surface than very large ones, but they do not offer the same safeguard against a rapid descent to the earth in case of a stoppage or breakdown of the machinery. With a large aeroplane properly adjusted, a rapid and destructive fall to the earth is quite impossible.

In the foregoing experiments with narrow aeroplanes, I employed an apparatus (Fig. 6) [*omitted*] which enabled me to mount my planes at any angle in a powerful blast of air, and to weigh the exact lifting effect and also the tendency to drift with the wind. This apparatus also enables me to determine with a great degree of nicety the best form of an atmospheric condenser to employ.

V. THE EFFICIENCY OF SCREW PROPELLERS.—STEERING, STABILITY, ETC.

Before I commenced my experiments at Baldwyn's Park, I attempted to obtain some information in regard to the action of screw propellers working in the air. I went to Paris and saw the apparatus which the French Government employed for testing the efficiency of screw propellers, but the propellers were so very badly made that the experiments were of no value. Upon consulting an English experimenter who had made a "lifelong study" of the question, he assured me that I should find the screw propeller very inefficient and very wasteful of power. He said that all screw propellers had a powerful fan-blower action, drawing in air at the centre and discharging it with great force at the periphery. I found that no two men were agreed as to the action of screw propellers. All the data or formulæ available were so confusing and contradictory as to be of no value whatsoever. Some experimenters were of the opinion that in computing the thrust of a screw we should only consider the projected area of the blades, and that the thrust would be equal to a wind blowing against a normal plane of equal area at a velocity equal to the slip. Others were of the opinion that the whole screw disk would have to be considered; that is, that the thrust would be equal to a wind blowing against a normal plane equal to the area of the whole disk at the velocity of the slip. The projected area of the two screw blades of my machine is 94 square feet, and the area of the 2 screw disks is 500 square feet. According to the first system of reasoning, therefore, the screw thrust of my large machine, when running at 40 miles an hour with a slip of 18 miles per hour, would have been, according to the well-known formula, $V^2 \times .005 = P$

$18^2 \times .005 \times 94 = 152.28$ pounds.

If, however, we should have considered the whole screw disk, it would have been—

$18^2 \times .005 \times 500 = 810$ pounds.

However, when the machine was run over the track at this rate, the thrust was found to be rather more than 2,000 lbs. When the machine was secured to the track and the screws revolved until the pitch in feet multiplied by the turns per minute was equal to 68 miles an hour, it was found that the screw thrust was 2,164 lbs. In this case it was of course all slip, and when the screws had been making a few turns they had established a well-defined air-current, and the power exerted by the engines was simply to maintain this air-current, and it is interesting to note that if we compute the projected area of these blades by the foregoing formula, the thrust would be—

$68^2 \times .005 \times 94 = 2173.28$ pounds,

which is almost exactly the observed screw thrust. From this, it would appear when the machine is stationary, and all the power is consumed in slip, that only the projected area of the screw blades should be considered. But whenever the machine is allowed to advance, and to encounter new air, the inertia of which has not been disturbed, the efficiency increases in geometrical progression. The exact rate for all speeds I have not yet ascertained. My experiments have, however, shown that with a speed of 40 miles an hour and a screw slip of 18 miles an hour, a well-made screw propeller is 13.1 times as efficient as early experimenters had supposed and attempted to prove by elaborate formulæ.

When I first commenced my experiments with a large machine, I did not know exactly what form of boiler, gas generator, or burner I should finally adopt; I did not know the exact size that it would be necessary to make my engines; I did not know the size, the pitch, or the diameter of the screws which would be the most advantageous. Neither did I know the form of aeroplane which I should finally adopt. It was therefore necessary for me to make the foundation or platform of my machine of such a character that it would allow me to make the modifications necessary to arrive at the best results. The platform of the machine is therefore rather larger than is necessary, and I find if I were to design a completely new machine, that it would be possible to greatly reduce the weight of the framework, and, what is still more, to greatly reduce the force necessary to drive it through the air.

At the present time, the body of my machine is a large platform, about 8 ft. wide and 40 ft. long. Each side is formed of very strong trusses of steel tubes, braced in every direction by strong steel wires. The trusses which give stiffness to this superstructure are all below the platform. In designing a new machine, I should make the trusses much deeper and at the same time very much lighter, and, instead of having them below the platform on which the boiler is situated, I should have them constructed in such a manner as to completely enclose the boiler and the greater part of the machinery. I should make the cross-section of the framework rectangular, and pointed at each end. I should cover the outside very carefully with balloon material, giving it a perfectly smooth and even surface throughout, so that it might be easily driven through the air.

In regard to the screws, I am at the present time able to mount screws 17 ft. 10 in. in diameter. I find, however, that my machine would be much more efficient if the screws were 24 feet in diameter, and I believe with such very large screws, four blades would be much more efficient than two.

My machine may be steered to the right or to the left by running one of the propellers faster than the other. Very convenient throttle valves have been provided to facilitate this system of steering. An ordinary vertical rudder placed just after the screws may, however, prove more convenient, if not more efficient.

The machine is provided with fore and aft horizontal rudders, both of which are connected with the same windlass. If the forward rudder is placed at an angle considerably greater than that of the main aeroplane, and the rear rudder placed flat so as not to lift at all (Fig. 7) and the machine run over the track at a high speed, the front wheels will be lifted from the steel rails, leaving the rear wheels on the rails. If the rudders are placed in the reverse position so that the front rudder is thrown out of action, and the rear rudder lifts to its full extent (Fig. 8), the hind wheels will be lifted from the steel rails, leaving only the forward wheels touching. If both rudders are placed at such an angle that they both lift (Fig. 9), and the machine is run at a very high velocity, all four of the wheels will be lifted from the steel rails. This would seem to show that these rudders are efficient as far as vertical steering is concerned. If the machine should break down in the air it would be necessary to tilt the rudders in the position shown . . . when it would fall to the ground without pitching or diving.

In regard to the stability of the machine, the centre of weight is much below the centre of lifting effect; moreover, the upper wings are set at such an angle that

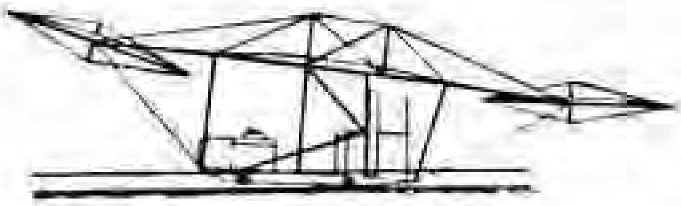


Fig. 7.—The forward wheels off the track.

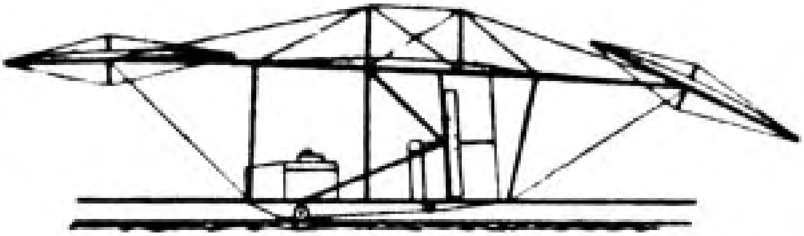


Fig. 8. — The rear wheels off the track.

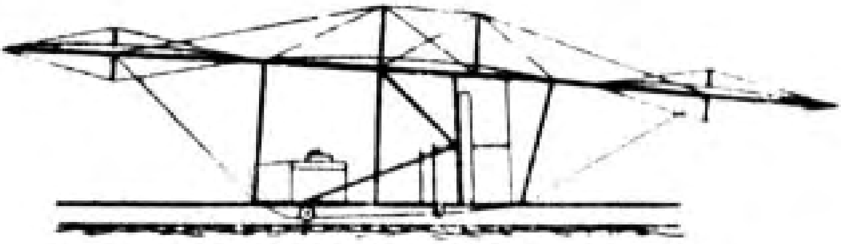


Fig. 9. — All the wheels off the track.

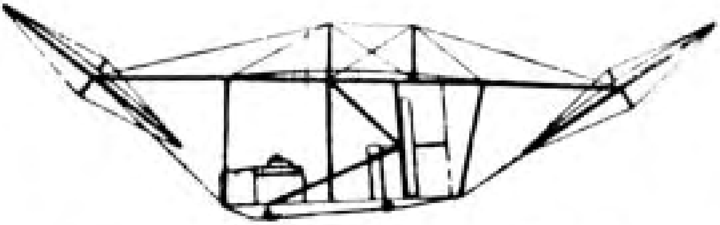


Fig. 10. — Showing the manner of placing the fore and aft rudders in case of a breakage of the machinery.

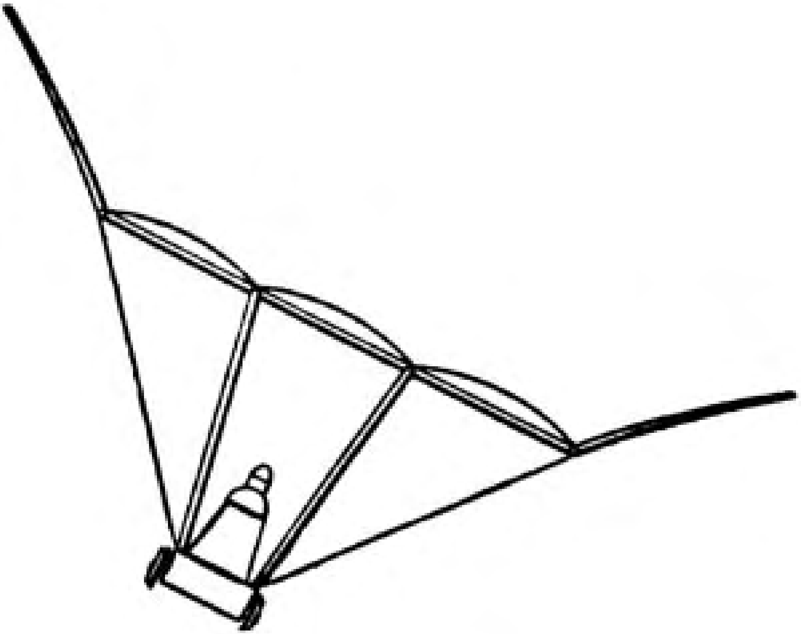


Fig. 11.

whenever the machine tilts to the right or to the left, the lifting effect is increased on the lower side and diminished on the higher side (Fig. 11). This simple arrangement makes the machine automatic as far as rolling is concerned. I am of the opinion that whenever flying machines come into use it will be necessary to steer them in a vertical direction by means of an automatic steering gear controlled by a gyroscope. It will certainly not be more difficult to manœuvre and steer such machines than it is to control completely submerged torpedoes.

When the machine is once perfected, it will not require a railway track to enable it to get the necessary velocity to rise. A short run over a moderately level field will suffice. As far as landing is concerned, the aerial navigator will touch the ground while moving forward, and the machine will be brought to a state of rest by sliding on the ground for a short distance. In this manner very little shock will result, whereas if the machine is stopped in the air and allowed to fall directly to the earth without advancing, the shock, although not strong enough to be dangerous to life or limb, might be sufficient to disarrange or injure the machinery.

[. . .]

VII. CONCLUSION.

My large machine, which was injured in my late experiments, has now been repaired and improved, and is quite ready to be used in any other experiments which I may wish to make on the limited area which I now have at my disposal. The railway track on which my experiments have been made is 1,800 feet long and the land on all sides is thickly studded with large trees. When making experiments about 500 feet of the track is used in getting up the necessary speed and 300 feet is utilized in bringing the machine again to a state of rest. My clear run is therefore limited to 1,000 feet, and the time which the machine takes to pass over this length of rail is at the most only a few seconds. It will therefore be seen that it is not an easy matter to conduct experiments in a satisfactory manner. In addition to these experiments with a large machine, I am also conducting a series of experiments in a blast of air issuing from a trunk 3 feet square. The air is set in motion by the action of screw propellers driven by a steam engine of 60 horsepower, and I am able to obtain any atmospheric velocity that I require, from 5 to 90 miles an hour. This apparatus . . . is constructed in such a manner that it enables me to mount in this current of air any object that I wish to experiment with. For instance, a bar of wood 3 inches square is mounted in the blast of air so that one of its sides forms a normal plane perpendicular to the direction of the blast. The engine is then run until the air is passing through the trunk at a velocity of 50 miles an hour. The tendency of this bar of wood to travel in the direction of the air may then be accurately determined, and this is considered as unity. A cylinder exactly 3 inches in diameter may then be mounted and tested in the same manner. The cylinder will of course have less tendency to travel with the air than the square bar of wood, and whatever this tendency is, will be the coefficient of a cylinder. I have provided oval, elliptical, and various other shaped objects to be experimented with, and when the experiments are finished I shall know the exact coefficient of all shapes that it may be practical to use in the framework of a flying machine, and also what effect is produced by placing two or more bodies in close proximity to each other.

In addition to these experiments, I am also able with the same air blast to ascertain the efficiency of various forms of aeroplanes, superposed or otherwise, and placed at all angles, the apparatus being provided with a scale beam which not only enables me to measure the drift, but also to accurately weigh the lifting effect. The aeroplane, or grouping of aeroplanes, in which the drift will go the greatest number of times into the lift will be considered the most satisfactory for the purpose.

Experiments are also being made in the same air blast with a view of ascertaining the condensing and lifting power of various forms of tubes, steam in the condition of exhaust being passed through the tubes while the air is driven between them at any velocity required. The experiments are being made with pure steam and also with steam contaminated with oil, with a view of ascertaining to what extent the efficiency of the condenser is reduced by a film of oil such as may be expected from exhaust steam. These experiments will enable me to ascertain very exactly the weight and the efficiency of atmospheric condensers, the amount that their tubes may be made to lift at various speeds and atmospheric conditions, and will also enable me to select the form which I find most suitable for the purpose.

In navigating a boat, it is only necessary that one should be able to turn it to the right or to the left (port or starboard), but with a flying machine it is not only necessary to steer it to the right or left (horizontally), but also in a vertical direction to prevent it from rearing up forward or pitching, and this, if it is accomplished by hand, will require the constant vigilance of a man at the wheel who can make observations, think, and act instantly. In order to prevent a too rapid up and down deviation of the machine I have constructed it of great length, so that the man at the helm will have more time to think and act. As before stated, however, I am of the opinion that the steering in a vertical direction should be automatically controlled by a gyroscope, and I have made an apparatus which consists of a steam piston acting directly upon the fore and aft rudders, the steam valve being controlled by a gyroscope. As the rudders are moved by the steam, their movement shuts the steam off in exactly the same manner that the moving of a rudder shuts off the steam in the well-known steam-steering apparatus now universally in use on all large steamers.

Now that it is definitely known that it is possible to construct a large machine which is light enough and at the same time powerful enough to raise its own weight and that of its engineers into the air, the next question which presents itself for solution is to ascertain how to steer and control such a machine when actually free from the earth. When it is considered that the machine is of great size and that it is necessary that it should move through the air at a velocity of at least 35 miles an hour in order to leave the ground, it will be obvious that manœuvring experiments cannot be conducted in a circumscribed place such as I now have. It is therefore necessary for me to obtain new and much larger premises where I shall have a very large and level field at my disposal. It is not an easy matter to obtain a field of this character in England, and it is almost impossible to find a suitable place near London. Moreover, experiments of this character, which are of little value unless conducted on a large scale, are exceedingly expensive, in fact, too expensive to be conducted by private individuals. Nevertheless, as my experiments have shown most conclusively that flying machines are not only possible but practicable, I think I am justified in continuing my experiments until a comparatively perfect flying machine has been evolved. When I have obtained possession of a suitable field, I propose to erect a large building which will contain the machine with all its wings in position. The building which I have at present, notwithstanding that it cost $15,000, is not large enough for the purpose, as the wings all have to be taken off before the machine can be housed.

There are so many points that may be improved that I have determined to build a new machine on a somewhat smaller scale, using about 200 or 250 horsepower. I shall make the engines of a longer stroke in proportion to their diameter so as to get a greater piston speed. I shall construct my screw propellers with 4 long and narrow blades, very sharp and thin, and shall make them large enough so that the pressure on the projected area of the blades will be about 10 lbs. per square foot instead of over 20 lbs. as now. This will greatly reduce the waste of power which is now lost in screw slip. As the present boiler has been found larger than is necessary, mv next boiler will be made lighter and smaller, and instead carrying a pressure of 320 lbs. to the square inch, I shall only carry 275 lbs. But the greatest improvement will be made in the framework of the machine, which will be constructed with a view of enabling everything to be driven through the air with the least possible resistance. The main aeroplane will be the same form as now, but placed at an angle of 1 in 113 instead of 1 in 8, and will be used principally for preventing the machine from accidentally falling to the earth. The principal lifting effect will be derived from a considerable number of relatively narrow aeroplanes placed on each side of the machine and mounted in such a position that the air can pass freely between them. The fore and aft rudders will be the same form as those now employed. The condenser will consist of a large number of small hollow aeroplanes about 2 inches wide, made of very thin and light metal and placed immediately behind the screw propellers. They will be placed at such an angle as to lift about 1,000 pounds in addition to their weight and the weight of their contents. Instead of mounting my machine as now on 4 wheels, I propose to mount it on 3, the two hind wheels being about 40 feet apart and the forward wheel placed about 60 feet in front of these. I propose to lay down a track of 3 rails, the sleepers being embedded in the ground so as to produce a comparatively level surface. This railway track should be oval or circular in form so that the machine may be heavily weighed to keep it on the track and be run at a high speed. This will enable me to test the furnace draught, the burner, the steam, the boiler, the engines, the propelling effects of tile screws, and the efficiency of the condenser while the machine is on the ground.

When all the machinery has been made to run smoothly I shall remove all the weight except that directly over the front wheel, and shall place a device between the wheel and the machine that will indicate the lift on the front end of the machine. I shall then run the machine over the track at a velocity which will just barely lift the hind wheels off the track, leaving the front wheel on the track. If the rear end of the machine lifts into the air it will change the angle of the planes and the lifting effect will be correspondingly diminished. This will prevent rising too high. Special wheels with a wide face suitable for running on either the rails or the earth will be provided for the purpose, and when I find that I can keep the hind wheels in the air and produce a varying lifting effect above and below the normal weight resting on the front wheel, I shall remove the weight from the forward wheel and attempt free flight by running the machine as near the ground as possible, making the first attempt by running against the wind, and it will only be after I find that I can steer my machine and manage it within a few feet of the earth, ascend and descend again at will, that I shall attempt high flight.

My experiments have certainly demonstrated that a steam engine and boiler may be made which will generate a horsepower for every six pounds of weight, and that the whole motor, including the gas generator, the water supply, the condenser, and the pumps may be all made to come inside of 11 lbs. to the horsepower. They also show that well made screw propellers working in the air are fairly efficient, and that they obtain a sufficient grip upon the air to drive the machine forward at a high velocity; that very large aeroplanes, if well made and placed at a proper angle, will lift as much as 2 ½ lbs. per square foot at a velocity not greater than 40 miles an hour; also that it is possible for a machine to be made so light and at the same time so powerful that it will lift not only its own weight but a considerable amount besides, with no other energy except that derived from its own engines. Therefore there can be no question but what a flying machine is now possible without the aid of a balloon in any form.

In order to obtain these results it has been necessary for me to make a great number of expensive experiments and to carefully study many of the properties of the air. Both Lord Kelvin and Lord Rayleigh, after witnessing a series of my experiments, expressed themselves as of the opinion that all the mathematical formulæ relating to planes driven through the air at an angle would have to be completely modified. Lord Kelvin himself has written that in some cases my experiments have proved that the conditions were from 20 to 50 times as favorable to the aerial navigator as had heretofore been shown by accepted formulæ, and that the whole mathematical question would require revision.

Experiments of this character unless conducted with great care are exceedingly dangerous. No makeshift or imperfect apparatus should be employed, but the experimenter should have the advantage of the most perfect appliances and apparatus that modern civilization can afford. The necessary plant for conducting experiments in a proper and safe manner is unfortunately much more expensive than the machine itself. If I find that my experiments require more money than I have at my disposal, I feel sure that some future experimenter more fortunate than myself will commence where I leave off, and with the advantages of the knowledge which has been gained by recent experiments will be able to construct a practical flying machine which cannot fail to be a great advantage to mankind.

The numerous and very expensive experiments, conducted on an unprecedented scale, which have made all this possible, and also brought to light new laws relating to the atmosphere, cannot fail to be of the greatest value to mankind, and it is on this basis that I submit the foregoing thesis.

Document 1-7

B. Baden-Powell (Baden Fletcher Smyth), "Present State of Aeronautics," interview in *The Sunday Times* [London], 14 June 1896, reprinted in *The Aeronautical Journal* (January 1897): 4–5.

Major Baden Fletcher Smyth Baden-Powell, a career officer in the British army and brother of the founder of the Boy Scouts (Robert Stephenson Smyth Baden-Powell, first baron Baden-Powell of Gilwell), joined the Aeronautical Society of Great Britain in 1880, fourteen years after it was established. Although his technical abilities were limited, his inventive spirit and enthusiasm for flight were perhaps unmatched. As secretary of the Aeronautical Society (elected in 1897), and later its president (elected while absent on duty in South Africa), B. Baden-Powell closely followed aviation developments internationally, corresponding regularly with Octave Chanute, in America, and many others.

In this document from 1897, Baden-Powell told a reporter (in a 14 June 1896 interview) from the *Sunday Times* of London what he thought about the "present state of aeronautics." In his opinion, though there were many exaggerated reports of aviation firsts, human flight "will come about, and very soon." In the interview, Baden-Powell reported on the work of Langley, Maxim, Lilienthal, as well as that of fellow Englishman Percy Pilcher, and he also described his own work with man-carrying kites. He was also the first in England to call attention to the work of the Wright brothers in his presidential address to the Aeronautical Society of Great Britain in December 1902, one year before the Wright's historic achievement of powered flight. By 1904, Baden-Powell was corresponding with the Wrights. He spent quite a bit of time with Wilbur during the Wright's tour of France in the fall of 1908 and published the article "A Trip with Wilbur Wright" in the December 1908 issue of *Aeronautics* magazine.

Interestingly, in his 1896 interview Baden-Powell saw the predominant value of flying machines resting in military applications: "It is, of course, in war that its great importance will be apparent." This runs contrary to the peace-bringing theme emphasized by the Wrights in the early years after their invention of the airplane.

Document 1-7, B. Baden-Powell, "Present State of Aeronautics," 1896.

The invention of a flying machine, or rather the announcement of a sanguine inventor who believes he has accomplished this, must be to the editor of a paper almost as valuable as the appearance, or reported appearance, of a sea serpent.

Again and again we see paragraphs in the newspapers announcing some marvellous advancement towards the solution of this intricate problem, but as often as not, or a great deal oftener, the invention proves to be merely a design, or an idea, formed by one who has but little real knowledge of the subject. During the last few years, however, several men of undoubted scientific ability and inventive genius have entered the arena, and have proved, both by figures and by experiment, that a flying machine is a possibility, and has come within the range of practical invention. The careful scientist is, however, just the man who does not, as a rule, announce his device and publish full descriptions of it until he is fully convinced that he is on the right track. It is, for this reason, rather difficult to get at the truth as regards the present position of the subject. It will, therefore, be unnecessary for me to refer to the many vague reports one has seen in the papers, except as regards those I know something about. Some rumours have latterly come out about a machine devised by Professor Langley, of the Smithsonian Institute, at Washington. He is, I know, a most capable man, for he has written a book, "Aerodynamics," which, without doubt, is by far the most elaborate scientific study of the question that has been written. He has for years made the most careful experiments, and has deduced many facts with reference to the action of the air on bodies moving through it of the greatest importance to inventors of aerial machines. He seems now to have made some small machine which is reported to be a great success, though I imagine it is nothing more than a model. No full description of it has, I believe, been published, and in all probability the inventor will not hurry to make common property that which has taken him so many years of study and experiment to evolve, at any events, until he has progressed sufficiently to be sure of success.

Then there is the great Maxim machine. Here again we have a very capable and very careful inventor devoting his attention and his money to the subject. The result has been a truly marvellous machine. Doubtless the inventor himself expected better results than it has proved capable of supplying, but nevertheless, though the machine can scarcely be said to have left the earth, yet it has proved a great success as regards the construction of very large and very light apparatus provided with immensely powerful engines. No one, before this was constructed, could have believed in the practicability of making a steam engine of 300 horse power, and placing it in a flying machine presenting some thousands of square feet of surface to the air, the whole apparatus only weighing some three tons.

However, the affair was altogether too large and unwieldy. A slight gust of wind getting under this enormous awning would be apt to upset the whole thing, and do hundreds of pounds' worth of damage. Probably if the machine had really been shot off into midair it would have gone all right, but there was an awkward uncertainty as to how it might come down! I think myself it ought to have been supplied with air bladders, and sent over a cliff out to sea. Then when it fell in the water it would have floated. Now, however, Mr. Maxim talks of constructing a much smatter machine on the same lines, and I shall certainly look forward with the greatest interest for the result, as I believe it will come very near the goal we are searching for.

This machine, and probably Langley's as well, is on what is known as the aeroplane principle; that is, a large fixed plane presenting a small angle to the air is propelled horizontally at a great speed by means of screw propellers, and is forced upwards by the pressure of the air on its under side. It is really simply a large kite, which, instead of being held by a string for the wind to act on it, is driven through the air, and raised much as a kite is when drawn along by its string. This principle has been proved theoretically to give the best results, that is to say, that with a given amount of power a greater weight can be raised by this system of propelling a plane surface presenting a small upward angle to the horizontal. Lord Kelvin and others have, however, pointed out that, notwithstanding this, a machine with vertical screw propellers to lift it straight upwards might have better practical results, even though more power might be required.

Then a good deal of talk is being made about two other inventors, Lilienthal, in Germany, and Pilcher, in England. These machines are very similar in principle, and though of great interest as experimental apparatus, cannot at present be considered as much more than parachutes. They partake of the nature of large wings attached to the human body, and are, therefore, comparatively well under control, but are only capable of "flying" if launched from some high place against, the wind; both inventors however, propose to apply small engines to propel them through the air, and success depends upon whether they can get sufficiently powerful engines without greatly increasing the size of the apparatus, and if they can get sufficiently efficient propellers to drive them at the requisite speed.

You ask also about the Italian machines. All I know is that reports have appeared of the Italian Government being in possession of a number of "air-ships"—and all I can say is that until I hear more of the details I don't believe a word of it.

Navigable balloons I look upon as a different subject. We know, of course, that the French have some, and, as to other nations, my mouth is closed as regards all I know in this line, but I may say at once that though on certain particular occasions, with favourable, i.e., calm weather, they may prove of the very greatest value in war, yet they are so dependent on the absence of wind and other circumstances that I cannot believe they will ever be very much used.

Then as for my own apparatus. Well, that again, is quite another matter. It is not a flying machine in the ordinary acceptance of the term. It is simply a system of large kites only for captive use. That is to say, it is held by a rope to the ground, and is chiefly for use as a lofty observatory for looking over the enemy's lines, and watching the country round about. I believe there are also other useful purposes to which it may be applied, since the length of the tether line is practically unlimited, but I need not speculate on future possibilities. My present object is to get an apparatus to serve instead of a captive balloon without necessitating the transport of cumbersome filling apparatus.

Kite-flying is not so simple a matter as some may suppose. To make an enormous kite like a toy one would be no easy task. Remember, the tail should be as heavy as the kite itself, which would, in my case, amount to some 60 lbs. or more. And then the liability to dive must be done away with, else I am afraid there might be a difficulty in providing observers. My present apparatus, which is now undergoing its trials at Aldershot, folds up so compactly that it call be carried by one man. It is capable of lifting a man to a height of 200 or 300 ft. One thing may be said about it that, besides the balloon, it is the only means known by which a man has been absolutely raised to any height in the air. And it is not entirely dependent on wind, since in calms it may be made to ascend by towing it with horses.

As for the general question of the possibilities of human flight, I for one firmly believe it will come about, and that very soon. It is, of course, in war that its great importance will be apparent. And if it be found necessary to have a large, complicated, and costly machine to effect the purpose, doubtless it will be used for little else. The first nation which can provide a secret flying-machine—and the secret will probably be more in the smaller details of construction than in the general principle—will undoubtedly possess an incalculable advantage in war.

Document 1-8(a–c)

(a) Samuel P. Langley, "Story of Experiments in Mechanical Flight," *Aeronautical Annual* (1897), reprinted in *Researches and Experiments in Aerial Navigation* (Washington, DC: Government Printing Office, 1908), pp. 169–179.

(b) Samuel P. Langley, excerpts from "The New Flying Machine." *McClure's Magazine* 9, No. 2 (June 1897): 647–660.

(c) Alexander Graham Bell, letter in *McClure's Magazine* 9, No. 2, (June 1897): 659; published as "The Aerodromes in Flight," *Aeronautical Annual* (1897), pp. 140–141.

Dr. Samuel P. Langley's work epitomized the nineteenth century "scale model" development approach to solving the problems of flight. As a practical exercise in applied aerodynamics, however, Langley's experiments were plagued with failure. Finally, in 1897, he achieved a signal success with the test flights of his powered model *Aerodrome No. 5*. The following three documents present the story of his experiments with flying scale models. The first is a selection from his 1896 interim report, which he referred to as a "narrative account of my work in aerodromics." A copy of this article was included in the documents sent to Wilbur Wright in 1899 by the Smithsonian Institution.

Regarding this and other reports by Langley, Wright later noted that, "his accounts of the troubles he had encountered and overcome put us on our guard and enabled us to entirely avoid some of the worst of them. He painted so vividly the troubles resulting from excessive lightness that we have been as men vaccinated against that disease."

Langley's success in 1896 with *Aerodrome No. 5* is recounted in an excerpt from an article he published in 1897 on "The New Flying Machine," along with the report written by his good friend and supporter Alexander Graham Bell, who was present to observe the experiments and took photographs of the model in flight.

Following his success with *Aerodrome No. 5* Langley attempted to progress to a full-scale manned model, his *Great Aerodrome*. The widely reported failure of this flying machine in December 1903, mere days in advance of the Wrights success at Kitty Hawk, led to Langley's disgrace and public rejection. Langley died within three years of this final crushing blow, but his few remaining partisans, including Bell, continued for some time thereafter to tout the potential of Langley's Aerodrome.

Document 1-8(a), Samuel P. Langley, "Story of Experiments in Mechanical Flight," 1897.

The subject of flight interested me as long ago as I can remember anything, but it was a communication from Mr. Lancaster, read at the Buffalo meeting of the American Association for the Advancement of Science, in 1886, which aroused my then dormant attention to the subject. What he said contained some remarkable but apparently mainly veracious observations on the soaring bird, and some more or less paradoxical assertions, which caused his communication to be treated with less consideration than it might otherwise have deserved. Among the latter was a statement that a model, somewhat resembling a soaring bird, wholly inert, and without any internal power, could, nevertheless, under some circumstances, advance against the wind without falling; which seemed to me then, as it did to members of the Association, an utter impossibility, but which I have since seen reason to believe is, within limited conditions, theoretically possible.

I was then engaged in the study of astrophysics at the Observatory in Allegheny, Pa. The subject of mechanical flight could not be said at that time to possess any literature, unless it were the publications of the French and English aeronautical societies, but in these, as in everything then accessible, fact had not yet always been discriminated from fancy. Outside of these, almost everything was even less trustworthy; but though, after I had experimentally demonstrated certain facts, anticipations of them were found by others on historical research, and though we can now distinguish in retrospective examination what would have been useful to the investigator if he had known it to be true, there was no test of the kind to apply at the time. I went to work, then, to find out for myself, and in my own way, what amount of mechanical power was requisite to sustain a given weight in the air and make it advance at a given speed, for this seemed to be an inquiry which must necessarily precede any attempt at mechanical flight, which was the very remote aim of my efforts.

The work was commenced in the beginning of 1887 by the construction, at Allegheny, of a turntable of exceptional size, driven by a steam engine, and this was used during three years in making the "Experiments in Aerodynamics," which were published by the Smithsonian Institution under that title in 1891. Nearly all the conclusions reached were the result of direct experiment in an investigation which aimed to take nothing on trust. Few of them were then familiar, though they have since become so, and in this respect knowledge has advanced so rapidly, that statements which were treated as paradoxical on my first enunciation of them are now admitted truisms.

It has taken me, indeed, but a few years to pass through the period when the observer hears that his alleged observation was a mistake; the period when he is told that if it were true, it would be useless; and the period when he is told that it is undoubtedly true, but that it has always been known.

May I quote from the introduction to this book what was said in 1891?

"I have now been engaged since the beginning of the year 1887 in experiments on an extended scale for determining the possibilities of, and the conditions for, transporting in the air a body whose specific gravity is greater than that of the air, and I desire to repeat my conviction that the obstacles in its way are not such as have been thought; that they lie more in such apparently secondary difficulties as those of guiding the body so that it may move in the direction desired and ascend or descend with safety, than in what may appear to be primary difficulties, due to the air itself," and, I added, that in this field of research I thought that we were, at that time (only six years since), "in a relatively less advanced condition than the study of steam was before the time of Newcomen." It was also stated that the most important inference from those experiments as a whole was that mechanical flight was possible with engines we could then build, as one horsepower rightly applied could sustain over 200 pounds in the air at a horizontal velocity of somewhat over 60 feet a second.

As this statement has been misconstrued, let me point out that it refers to surfaces, used without guys, or other adjuncts, which would create friction; that the horsepower in question is that actually expended in the thrust, and that it is predicated only on a rigorously horizontal flight. This implies a large deduction from the power in the actual machine, where the brake horsepower of the engine, after a requisite allowance for loss in transmission to the propellers, and for their slip on the air, will probably be reduced to from one-half to one-quarter of its nominal amount; where there is great friction from the enforced use of guys and other adjuncts; but, above all, where there is no way to insure absolutely horizontal flight in free air. All these things allowed for, however, since it seemed to me possible to provide an engine which should give a horsepower for something like 10 pounds of weight, there was still enough to justify the statement that we possessed in the steam engine, as then constructed or in other heat engines, more than the indispensable power, though it was added that this was not asserting that a system of supporting surfaces could be securely guided through the air or safely brought to the ground, and that these and like considerations were of quite another order, and belonged to some inchoate art which I might provisionally call *aerdromics*.

These important conclusions were reached before the actual publication of the volume, and a little later others on the nature of the movements of air, which were published under the title of "The Internal Work of the Wind" (Smithsonian Contributions to Knowledge, Volume XXVII, 1893, No. 884). The latter were founded on experiments independent of the former, and which led to certain theoretical conclusions unverified in practice. Among the most striking, and perhaps paradoxical of these, was that a suitably disposed free body might, under certain conditions, be sustained in an ordinary wind, and even advance against it without the expenditure of any energy from within.

The first stage of the investigation was now over, so far as that I had satisfied myself that mechanical flight was possible with the power we could hope to command, if only the art of directing that power could be acquired.

The second stage (that of the acquisition of this art) I now decided to take up. It may not be out of place to recall that at this time, only six years ago, a great many scientific men treated the whole subject with entire indifference, as unworthy of attention, or as outside of legitimate research, the proper field for the charlatan, and one on which it was scarcely prudent for a man with a reputation to lose to enter.

The record of my attempts to acquire the art of flight may commence with the year 1889, when I procured a stuffed frigate bird, a California condor, and an albatross, and attempted to move them upon the whirling table at Allegheny. The experiments were very imperfect and the records are unfortunately lost, but the important conclusion to which they led was that a stuffed bird could not be made to soar except at speeds which were unquestionably very much greater than what served to sustain the living one, and the earliest experiments and all subsequent ones with actually flying models have shown that thus far we cannot carry nearly the weights which Nature does to a given sustaining surface without a power much greater than she employs. At the time these experiments were begun, Pénaud[’s] ingenious but toy-like model was the only thing which could sustain itself in the air for even a few seconds, and calculations founded upon its performance sustained the conclusion that the amount of power required in actual free flight was far greater than that demanded by the theoretical enunciation. In order to learn under what conditions the aerodrome should be balanced for horizontal flight, I constructed over 30 modifications of the rubber-driven model, and spent many months in endeavoring from these to ascertain the laws of “balancing”; that is, of stability leading to horizontal flight. Most of these models had two propellers, and it was extremely difficult to build them light and strong enough. Some of them had superposed wings; some of them curved and some plane wings; in some the propellers were side by side; in others one propeller was at the front and the other at the rear, and so every variety of treatment was employed, but all were at first too heavy, and only those flew successfully which had from 3 to 4 feet of sustaining surface to a pound of weight, a proportion which is far greater than Nature employs in the soaring bird, where in some cases less than half a foot of sustaining surface is used to a pound. It had been shown in the “Experiments in Aerodynamics” that the center of pressure on an inclined plane advancing was not at the center of the figure, but much in front of it, and this knowledge was at first nearly all I possessed in balancing these early aerodromes. Even in the beginning, also, I met remarkable difficulty in throwing them into the air, and devised numerous forms of launching apparatus which were all failures, and it was necessary to keep the construction on so small a scale that they could be cast from the hand.

The earliest actual flights with these were extremely irregular and brief, lasting only from three to four seconds. They were made at Allegheny in March, 1891, but these and all subsequent ones were so erratic and so short that it was possible to learn very little from them. Pénaud states that he once obtained a flight of 13 seconds. I never got as much as this, but ordinarily little more than half as much, and came to the conclusion that in order to learn the art of mechanical flight it was necessary to have a model which would keep in the air for at any rate a longer period than these, and move more steadily. Rubber twisted in the way that Pénaud used it will practically give about 300 foot-pounds to a pound of weight, and at least as much must be allowed for the weight of the frame on which the rubber is strained. Twenty pounds of rubber and frame, then, would give 3,000 foot-pounds, or 1 horsepower for less than six seconds. A steam engine having apparatus for condensing its steam, weighing in all 10 pounds, and carrying 10 pounds of fuel, would possess in this fuel, supposing that but one-tenth of its theoretical capacity is utilized, many thousand times the power of an equal weight of rubber, or at least 1 horsepower for some hours. Provided the steam could be condensed and the water reused, then the advantage of the steam over the spring motor was enormous, even in a model constructed only for the purpose of study. But the construction of a steam-driven aerodrome was too formidable a task to be undertaken lightly, and I examined the capacities of condensed air, carbonic-acid gas, of various applications of electricity, whether in the primary or storage battery, of hot-water engines, of inertia motors, of the gas engine, and of still other material. The gas engine promised best of all in theory, but it was not yet developed in a suitable form. The steam engine, as being an apparently familiar construction, promised best in practice, but in taking it up, I, to my cost, learned that in the special application to be made of it, little was really familiar and everything had to be learned by experiment. I had myself no previous knowledge of steam engineering, nor any assistants other than the very capable workmen employed. I well remember my difficulties over the first aerodrome (No. 0), when everything, not only the engine, but the boilers which were to supply it, the furnaces which were to heat it, the propellers which were to advance it, the hull which was to hold all these—were all things to be originated, in a construction which, as far as I knew, had never yet been undertaken by anyone.

It was necessary to make a beginning, however, and a compound engine was planned which, when completed, weighed about 4 pounds, and which could develop rather over a horsepower with 60 pounds of steam, which it was expected could be furnished by a series of tubular boilers arranged in “bee-hive” form and the whole was to be contained in a hull about 5 feet in length and 10 inches in diameter. This hull was, as in the construction of a ship, to carry all adjuncts. In front of it projected a steel rod, or bowsprit, about its own length, and one still longer behind. The engines rotated two propellers, each about 30 inches in diameter, which were on the end of long shafts disposed at an acute angle to each other and actuated by a single gear driven from the engine. A single pair of large wings contained about 50 square feet, and a smaller one in the rear about half as much, or in all some 75 feet, of sustaining surface, for a weight which it was expected would not exceed 25 pounds.

Although this aerodrome was in every way a disappointment, its failure taught a great many useful lessons. It had been built on the large scale described, with very little knowledge of how it was to be launched into the air, but the construction developed the fact that it was not likely to be launched at all, since there was a constant gain in weight over the estimate at each step, and when the boilers were completed it was found that they gave less than one-half the necessary steam, owing chiefly to the inability to keep up a proper fire. The wings yielded so as to be entirely deformed under a slight pressure of the air, and it was impossible to make them stronger without making them heavier, where the weight was already prohibitory. The engines could not transmit even what feeble power they furnished, without dangerous tremor in the long shafts, and there were other difficulties. When the whole approached completion, it was found to weigh nearer 50 pounds than 25, to develop only about one-half the estimated horsepower at the brake, to be radically weak in construction, owing to the yielding of the hull, and to be, in short, clearly a hopeless case.

The first steam-driven aerodrome had, then, proved a failure, and I reverted during the remainder of the year to simpler plans, among them one of an elementary gasoline engine.

I may mention that I was favored with an invitation from Mr. Maxim to see his great flying-machine at Bexley, in Kent, where I was greatly impressed with the engineering skill shown in its construction, but I found the general design incompatible with the conclusions that I had reached by experiments with small models, particularly as to what seemed to me advisable in the carrying of the center of gravity as high as was possible with safety.

In 1892 another aerodrome (No. 1), which was to be used with carbonic acid gas, or with compressed air, was commenced. The weight of this aerodrome was a little over 4 ½ pounds, and the area of the supporting surfaces 6 ½ square feet. The engines developed but a small fraction of a horsepower, and they were able to give a dead lift of only about one-tenth of the weight of the aerodrome, giving relatively less power to weight than that obtained in the large aerodrome already condemned.

Toward the close of this year was taken up the more careful study of the position of the center of gravity with reference to the line of thrust from the propellers, and to the center of pressure. The center of gravity was carried as high as was consistent with safety, the propellers being placed so high, with reference to the supporting wings, that the intake of air was partly from above and partly from below these latter. The lifting power (i.e., the dead lift) of the aerodromes was determined in the shop by a very useful contrivance which I have called the "pendulum," which consists of a large pendulum which rests on knife edges, but is prolonged above the points of support, and counterbalanced so as to present a condition of indifferent equilibrium. Near the lower end of this pendulum the aerodrome is suspended, and when power is applied to it, the reaction of the propellers lifts the pendulum through a certain angle. If the line of thrust passes through the center of gravity, it will be seen that the sine of this angle will be the fraction of the weight lifted, and thus the dead-lift power of the engines becomes known. Another aerodrome was built, but both, however constructed, were shown by this pendulum test to have insufficient power, and the year closed with disappointment.

Aerodrome No. 3 was of stronger and better construction, and the propellers, which before this had been mounted on shafts inclined to each other in a V-Iike form, were replaced by parallel ones. Boilers of the Serpolet type (that is, composed of tubes of nearly capillary section) were experimented with at great cost of labor and no results; and they were replaced with coil boilers. For these I introduced, in April, 1893, a modification of the ælopile blast, which enormously increased the heat-giving power of the fuel (which was then still alcohol), and with this blast for the first time the boilers began to give steam enough for the engines. It had been very difficult to introduce force pumps which would work effectively on the small scale involved, and after many attempts to dispense with their use by other devices, the acquisition of a sufficiently strong pump was found to be necessary in spite of its weight, but was only secured after long experiment. It may be added that all the aerodromes from the very nature of their construction were wasteful of heat, the industrial efficiency little exceeding half of 1 per cent, or from one-tenth to one-twentieth that of a stationary engine constructed under favorable conditions. This last aerodrome lifted nearly 30 per cent of its weight upon the pendulum, which implied that it could lift much more than its weight when running on a horizontal track, and its engines were capable of running its 50-centimeter propellers at something over 700 turns per minute. There was, however, so much that was unsatisfactory about it, that it was deemed best to proceed to another construction before an actual trial was made in the field, and a new aerodrome, designated as No. 4, was begun. This last was an attempt, guided by the weary experience of preceding failures, to construct one whose engines should run at a much higher pressure than heretofore, and be much more economical in weight. The experiments with the Serpolet boilers having been discontinued, the boiler was made with a continuous helix of copper tubing, which, as first employed, was about three milli-metres internal diameter; and it may be here observed that a great deal of time was subsequently lost in attempts to construct a more advantageous form of boiler for the actual purposes than this simple one, which, with a larger coil tube, eventually proved to be the best; so that later constructions have gone back to this earlier type. A great deal of time was lost in these experiments from my own unfamiliarity with steam engineering, but it may also be said that there was little help either from books or from counsel, for everything was here sui generis, and had to be worked out from the beginning. In the construction which had been reached by the middle of the third year of experiment, and which has not been greatly differed from since, the boiler was composed of a coil of copper in the shape of a hollow helix, through the center of which the blast from the ælopile was driven, the steam and water passing into a vessel I called the "separator," whence the steam was led into the engines at a pressure of from 70 to 100 pounds (a pressure which has since been considerably exceeded).

From the very commencement of this long investigation the great difficulty was in keeping down the weight, for any of the aerodromes could probably have flown had they been built light enough, and in every case before the construction was completed the weight had so increased beyond the estimate, that the aerodrome was too heavy to fly, and nothing but the most persistent resolution kept me in continuing attempts to reduce it after further reduction seemed impossible. Toward the close of the year (1893) I had, however, finally obtained an aerodrome with mechanical power, as it seemed to me, to fly, and I procured, after much thought as to where this flight should take place, a small house boat, to be moored somewhere in the Potomac; but the vicinity of Washington was out of the question, and no desirable place was found nearer than 30 miles below the city. It was because it was known that the aerodrome might have to be set off in the face of a wind, which might blow in any direction, and because it evidently was at first desirable that it should light in the water rather than on the land, that the house boat was selected as the place for the launch. The aerodrome (No. 4) weighed between 9 and 10 pounds, and lifted 40 per cent of this on the pendulum with 60 pounds of steam pressure, a much more considerable amount than was theoretically necessary for horizontal flight. And now the construction of a launching apparatus, dismissed for some years, was resumed. Nearly every form seemed to have been experimented with unsuccessfully in the smaller aerodromes. Most of the difficulties were connected with the fact that it is necessary for an aerodrome, as it is for a soaring bird, to have a certain considerable initial velocity before it can advantageously use its own mechanism for flight, and the difficulties of imparting this initial velocity with safety are surprisingly great, and in the open air are beyond all anticipation.

Here, then, commences another long story of delay and disappointment in these efforts to obtain a successful launch. To convey to the reader an idea of its difficulties, a few extracts from the diary of the period are given. (It will be remembered that each attempt involved a journey of thirty miles each way.)

November 18, 1893. Having gone down to the house boat, preparatory to the first launch, in which the aerodrome was to be cast from a springing piece beneath, it was found impossible to hold it in place on this before launching without its being prematurely torn from its support, although there was no wind except a moderate breeze; and the party returned after a day's fruitless effort.

Two days later a relative calm occurred in the afternoon of a second visit, when the aerodrome was mounted again, but, though the wind was almost imperceptible, it was sufficient to wrench it about so that at first nothing could be done, and when steam was gotten up the burning alcohol blew about so as to seriously injure the inflammable parts. Finally, the engines being under full steam, the launch was attempted, but, owing to the difficulties alluded to and to a failure in the construction of the launching piece, the aerodrome was thrown down upon the boat, fortunately with little damage.

Whatever form of launch was used, it became evident at this time that the aerodrome must at any rate be firmly held up to the very instant of release, and a device was arranged for clamping it to the launching apparatus.

On November 24 another attempt was made to launch, which was rendered impossible by a very moderate wind indeed.

On November 27 a new apparatus was arranged, to merely drop the aerodrome over the water, with the hope that it would get up sufficient speed before reaching the surface to soar, but it was found that a very gentle intermittent breeze (probably not more than 3 or 4 miles an hour) was sufficient to make it impossible even to prepare to drop the aerodrome toward the water with safety.

It is difficult to give an idea in few words of the nature of the trouble, but unless one stands with the machine in the open air he can form no conception of what the difficulties, are which are peculiar to practice in the open, and which do not present themselves to the constructor in the shop, nor probably to the mind of the reader.

December 1, another failure; December 7, another; December 11, another; December 20, another; December 21, another. These do not all involve a separate journey, but five separate trips were made of a round distance of 60 miles each before the close of the season. It may be remembered that these attempts were in a site far from the conveniences of the workshop and under circumstances which took up a great deal of time, for some hours were spent on mounting the aerodrome on each occasion, and the year closed without a single cast of it into the air. It was not known how it would have behaved there, for there had not been a launch even in nine trials, each one representing an amount of trouble and difficulty which this narrative gives no adequate idea of.

I pass over a long period of subsequent baffled effort, with the statement that numerous devices for launching were tried in vain and that nearly a year passed before one was effected.

Six trips and trials were made in the first six months of 1894 without securing a launch. On the 24th of October a new launching piece was tried for the first time, which embodied all the requisites whose necessity was taught by previous experience, and, saving occasional accidents, the launching was from this time forward accomplished with comparatively little difficulty.

The aerodromes were now for the first time put fairly in the air, and a new class of difficulties arose, due to a cause which was at first obscure—for two successive launches of the same aerodrome, under conditions as near alike as possible, would be followed by entirely different results. For example, in the first case it might be found rushing, not falling, forward and downward into the water under the impulse of its own engines; in the second case, with every condition from observation apparently the same, it might be found soaring upward until its wings made an angle of 60 degrees with the horizon, and, unable to sustain itself at such a slope, sliding backward into the water.

After much embarrassment the trouble was discovered to be due to the fact that the wings, though originally set at precisely the same angle in the two cases, were irregularly deflected by the upward pressure of the air, so that they no longer had the form which they appeared to possess but a moment before they were upborne by it, and so that a very minute difference, too small to be certainly noted, exaggerated by this pressure, might cause the wind of advance to strike either below or above the wing and to produce the salient difference alluded to. When this was noticed all aerodromes were inverted, and sand was dredged uniformly over the wings until its weight represented that of the machine. The flexure of the wings under these circumstances must be nearly that in free air, and it was found to distort them beyond all anticipation. Here commences another series of trials in which the wings were strengthened in various ways, but in none of which, without incurring a prohibitive weight, was it possible to make them strong enough. Various methods of guying them were tried, and they were rebuilt on different designs—a slow and expensive process. Finally, it may be said, in anticipation (and largely through the skill of Mr. Reed, the foreman of the work), the wings were rendered strong enough without excessive weight, but a year or more passed in these and other experiments.

In the latter part of 1894 two steel aerodromes had already been built, which sustained from 40 to 50 per cent of their dead lift weight on the pendulum, and each of which was apparently supplied with much more than sufficient power for horizontal flight (the engine and all the moving parts furnishing over one horsepower at the brake weighed in one of these but 26 ounces); but it may be remarked that the boilers and engines in lifting this per cent of the weight did so only at the best performance in the shop, and that nothing like this could be counted upon for regular performance in the open. Every experiment with the launch, when the aerodrome descended into the water, not gently, but impelled by the misdirected power of its own engines, resulted at this stage in severe strains and local injury, so that repairing, which was almost rebuilding, constantly went on, a hard but necessary condition attendant on the necessity of trial in the free air. It was gradually found that it was indispensable to make the frame stronger than had hitherto been done, though the absolute limit of strength consistent with weight seemed to have been already reached, and the year 1895 was chiefly devoted to the labor on the wings and what seemed at first the hopeless task of improving the construction so that it might be stronger without additional weight, when every gram of weight had already been scrupulously economized. With this went on attempts to carry the effective power of the burners, boilers, and engines further, and modification of the internal arrangement and a general disposition of the parts such that the wings could be placed further forward or backward at pleasure, to more readily meet the conditions necessary for bringing the center of gravity under the center of pressure. So little had even now been learned about the system of balancing in the open air, that at this late day recourse was again had to rubber models, of a different character, however, from those previously used; for in the latter the rubber was strained, not twisted. These experiments took up an inordinate time, though the flight obtained from the models thus made was somewhat longer and much steadier than that obtained with the Pénaud form, and from them a good deal of valuable information was gained as to the number and position of the wings and as to the effectiveness of different forms and dispositions of them. By the middle of the year a launch took place with a brief flight, where the aerodrome shot down into the water after a little over 50 yards. It was immediately followed by one in which the same aerodrome rose at a considerable incline and fell backward with scarcely any advance after sustaining itself rather less than ten seconds, and these and subsequent attempts showed that the problem of disposing of the wings so that they would not yield and of obtaining a proper "balance" was not yet solved.

Briefly it may be said that the year 1895 gave small results for the labor with which it was filled, and that at its close the outlook for further substantial improvement seemed to be almost hopeless, but it was at this time that final success was drawing near. Shortly after its close I became convinced that substantial rigidity had been secured for the wings; that the frame had been made stronger without prohibitive weight, and that a degree of accuracy in the balance had been obtained which had not been hoped for. Still there had been such a long succession of disasters and accidents in the launching that hope was low when success finally came.

I have not spoken here of the aid which I received from others, and particularly from Dr. Carl Barus and Mr. J. E. Watkins, who have been at different times associated with me in the work. Mr. R. L. Reed's mechanical skill has helped me everywhere, and the lightness and efficiency of the engines are in a large part due to Mr. L. C. Maltby.

Document 1-8(b), Samuel P. Langley, excerpts from "The New Flying Machine," 1897.

Has the reader had enough of this tale of disaster? If so, he may be spared the account of what went the same way. Launch after launch was successively made. The wings were finally, after infinite patience and labor, made at once light enough and strong enough to do the work, and now in the long struggle the way had been fought up to the face of the final difficulty, in which nearly a year more passed, for the all-important difficulty of balancing the aerodrome was now reached, where it could be discriminated from other preliminary ones, which have been alluded to, and which at first obscured it. If the reader will look at the hawk or any soaring bird, he will see that as it sails through the air without flapping the wing, there are hardly two consecutive seconds of its flight in which it is not swaying a little from side to side, lifting one wing or the other, or turning in a way that suggests an acrobat on a tight-rope, only that the bird uses its widely outstretched wings in place of the pole.

There is something, then, which is difficult even for the bird, in this act of balancing. In fact, he is sailing so close to the wind in order to fly at all, that if he dips his head but the least he will catch the wind on the top of his wing and fall, as I have seen gulls do, when they have literally tumbled toward the water before they could recover themselves.

Beside this, there must be some provision for guarding against the incessant, irregular currents of the wind, for the wind as a whole—and this is a point of prime importance—is not a thing moving along all-of-a-piece, like water in the Gulf Stream. Far from it. The wind, when we come to study it, as we have to do here, is found to be made of innumerable currents and counter-currents which exist altogether and simultaneously in the gentlest breeze, which is in reality going fifty ways at once, although, as a whole, it may come from the east or the west; and if we could see it, it would be something like seeing the rapids below Niagara, where there is an infinite variety of motion in the parts, although there is a common movement of the stream as a whole.

All this has to be provided for in our mechanical bird, which has neither intelligence nor instinct, without which, although there be all the power of the engines requisite, all the rigidity of wing, all the requisite initial velocity, it still cannot fly. This is what is meant by balancing, or the disposal of the parts, so that the airship will have a position of equilibrium into which it tends to fall when it is disturbed, and which will enable it to move of its own volition, as it were, in a horizontal course.

Now the reader may be prepared to look at the apparatus which finally has flown. (See diagram above.) [*Diagram on p. 143*] In the completed form we see two pairs of wings, each slightly curved, each attached to a long steel rod which supports them both, and from which depends the body of the machine, in which are the boilers, the engines, the machinery, and the propeller wheels, these latter being not in the position of those of an ocean steamer, but more nearly amidships. They are made sometimes of wood, sometimes of steel and canvas, and are between three and four feet in diameter.

The hull itself is formed of steel tubing; the front portion is closed by a sheathing of metal which hides from view the fire-grate and apparatus for heating, but allows us to see a little of the coils of the boiler and all of the relatively large smokestack in which it ends. The conical vessel in front is an empty float, whose use is to keep the whole from sinking if it should fall in the water.

This boiler supplies steam for an engine of between one and one and one-half horsepower, and, with its fire-grate, weighs a little over five pounds. This weight is exclusive of that of the engine, which weighs, with all its moving parts, but twenty-six ounces. Its duty is to drive the propeller wheels, which it does at rates varying from 800 to 1,200, or even more, turns a minute, the highest number being reached when the whole is speeding freely ahead.

The rudder, it will be noticed, is of a shape very unlike that of a ship, for it is adapted both for vertical and horizontal steering. It is impossible within the limits of such an article as this, however, to give an intelligible account of the manner in which it performs its automatic function. Sufficient it is to say that it does perform it.

The width of the wings from tip to tip is between twelve and thirteen feet, and the length of the whole about sixteen feet. The weight is nearly thirty pounds, of which about one-fourth is contained in the machinery. The engine and boilers are constructed with an almost single eye to economy of weight, not of force, and are very wasteful of steam, of which they spend their own weight in five minutes. This steam might all be recondensed and the water re-used by proper condensing apparatus, but this cannot be easily introduced in so small a scale of construction. With it the time of flight might be hours instead of minutes, but without it the flight (of the present aërodrome) is limited to about five minutes, though in that time, as will be seen presently, it can go some miles; but owing to the danger of its leaving the surface of the water for that of the land, and wrecking itself on shore, the time of flight is limited designedly to less than two minutes.

I have spared the reader an account of numberless delays, from continuous accidents and from failures in attempted flights, which prevented a single entirely satisfactory one during nearly three years after a machine with power to fly had been attained. It is true that the aërodrome maintained itself in the air at many times, but some disaster had so often intervened to prevent a complete flight that the most persistent hope must at some time have yielded. On the 6th of May of last year I had journeyed, perhaps for the twentieth time, to the distant river station,

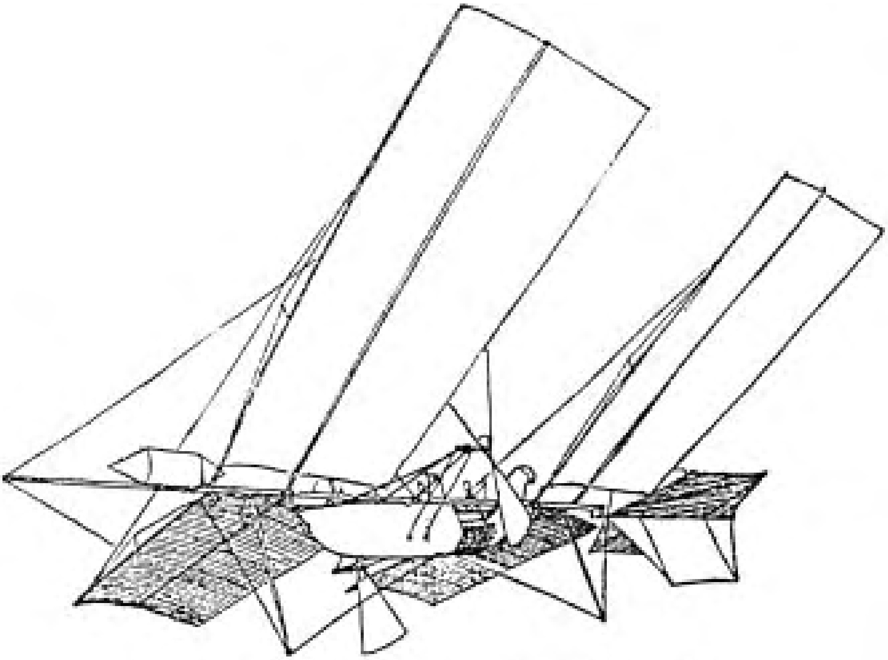


DIAGRAM OF THE AËRODROME AS DESCRIBED BELOW.

and recommenced the weary routine of another launch, with very moderate expectation indeed; and when, on that, to me, memorable afternoon the signal was given and the aërodrome sprang into the air, I watched it from the shore with hardly a hope that the long series of accidents had come to a close. And yet it had, and for the first time the aërodrome swept continuously through the air like a living thing, and as second after second passed on the face of the stop-watch, until a minute had gone by, and it still flew on, and as I heard the cheering of the few spectators, I felt that something had been accomplished at last, for never in any part of the world, or in any period, had any machine of man's construction sustained itself in the air before for even half of this brief time. Still the aërodrome went on in a rising course until, at the end of a minute and a half (for which time only it was provided with fuel and water), it had accomplished a little over half a mile, and now it settled rather than fell into the river with a gentle descent. It was immediately taken out and flown again with equal success, nor was there anything to indicate that it might not have flown indefinitely except for the limit put upon it. . . .

On November 28th I witnessed, with another aërodrome of somewhat similar construction, a rather longer flight, in which it traversed about three-quarters of a mile, and descended with equal safety. In this the speed was greater, or about thirty miles an hour. . . . We may live to see airships a common sight, but habit

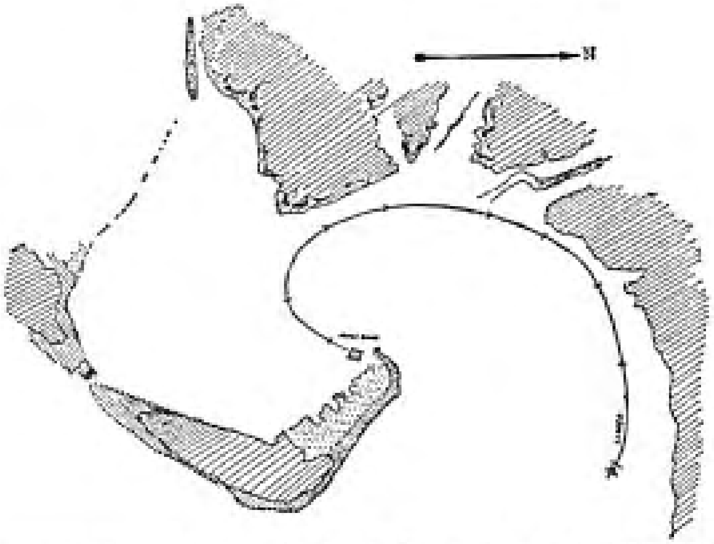


DIAGRAM SHOWING THE COURSE OF THE AËRODROME IN ITS FLIGHT ON THE POTOMAC RIVER AT QUANTICO. SEE PAGE 659.

has not dulled the edge of wonder, and I wish that the reader could have witnessed the actual spectacle. “It looked like a miracle,” said one who saw it, and the photograph, though taken from the original conveys but imperfectly the impression given by the flight itself.

And now, it may be asked, what has been done? This has been done: a “flying-machine,” so long a type for ridicule, has really flown; it has demonstrated its practicability in the only satisfactory way—by actually flying, and by doing this again and again, under conditions which leave no doubt.

There is no room here to enter on the consideration of the construction of larger machines, or to offer the reasons for believing that they may be built to remain for days in the air, or to travel at speeds higher than any with which we are familiar; neither is there room to enter on a consideration of their commercial value, or of those applications which will probably first come in the arts of war rather than those of peace; but we may at least see that these may be such as to change the whole conditions of warfare, when each of two opposing hosts will have its every movement known to the other, when no lines of fortification will keep out the foe, and when the difficulties of defending a country against an attacking enemy in the air will be such that we may hope that this will hasten rather than retard the coming of the day when war shall cease.

I have thus far had only a purely scientific interest in the results of these labors. Perhaps if it could have been foreseen at the outset how much labor there was to be, how much of life would be given to it, and how much care, I might have hesitated to enter upon it at all. And now reward must be looked for, if reward there be, in the knowledge that l have done the best I could in a difficult task, with results which it may be hoped will be useful to others. I have brought to a close the portion of the work which seemed to be specially mine—the demonstration of the practicability of mechanical flight—and for the next stage, which is the commercial and practical development of the idea, it is probable that the world may look to others. The world, indeed, will be supine if it do not realize that a new possibility has come to it, and that the great universal highway overhead is now soon to be opened.

Document 1-8(c), Alexander Graham Bell, "The Aerodromes in Flight," 1897.

Through the courtesy of Dr. S. P. Langley, Secretary of the Smithsonian Institution, I have had, on various occasions, the privilege of witnessing his experiments with aerodromes, and especially the remarkable success attained by him in experiments made upon the Potomac river on Wednesday, May 6, 1896, which led me to urge him to make public some of these results.

I had the pleasure of witnessing the successful flight of some of these aerodromes more than a year ago, but Dr. Langley's reluctance to make the results public at that time prevented me from asking him, as I have done since, to let me give an account of what I saw.

On the date named two ascensions were made by the aerodrome, or so-called "flying-machine," which I will not describe here further than to say that it appeared to me to be built almost entirely of metal, and driven by a steam-engine which I have understood was carrying fuel and a water supply for a very brief period, and which was of extraordinary lightness.

The absolute weight of the aerodrome, including that of the engine and all appurtenances, was, as I was told, about 25 pounds, and the distance from tip to tip of the supporting surfaces was, as I observed, about 12 or 14 feet. The method of propulsion was by aerial screw-propellers, and there was no gas or other aid for lifting it in the air except its own internal energy.

On the occasion referred to, the aerodrome, at a given signal, started from a platform about 20 feet above the water, and rose at first directly in the face of the wind, moving at all times with remarkable steadiness, and subsequently swinging around in large curves of, perhaps, a hundred yards in diameter, and continually ascending until its steam was exhausted, when, at a lapse of about a minute and a half, and at a height which I judged to be between 80 and 100 feet in the air, the wheels ceased turning, and the machine, deprived of the aid of its propellers, to my surprise did not fall, but settled down so softly and gently that it touched the water without the least shock, and was in fact immediately ready for another trial.

In the second trial, which followed directly, it repeated in nearly every respect the actions of the first, except that the direction of its course was different. It ascended again in the face of the wind, afterwards moving steadily and continually in large curves accompanied with a rising motion and a lateral advance. Its motion was, in fact, so steady, that I think a glass of water on its surface would have remained unspilled. When the steam gave out again, it repeated for a second time the experience of the first trial when the steam had ceased, and settled gently and easily down. What height it reached at this trial I cannot say, as I was not so favorably placed as in the first; but I had occasion to notice that this time its course took it over a wooded promontory, and I was relieved of some apprehension in seeing that it was already so high as to pass the tree-tops by 20 or 30 feet. It reached the water 1 minute and 31 seconds from the time it started, at a measured distance of over 900 feet from the point at which it rose.

This, however, was by no means the length of its flight. I estimated from the diameter of the curve described, from the number of turns of the propellers as given by the automatic counter, after due allowance for slip, and from other measures, that the actual length of flight on each occasion was slightly over 3,000 feet. It is at least safe to say that each exceeded half an English mile.

From the time and distance it will be noticed that the velocity was between 20 and 25 miles an hour, in a course which was taking it constantly "up hill." I may add that on a previous occasion I have seen a far higher velocity attained by the same aerodrome when its course was horizontal.

I have no desire to enter into detail further than I have done, but I cannot but add that it seems to me that no one who was present on this interesting occasion could have failed to recognize that the practicability of mechanical flight had been demonstrated.

ALEXANDER GRAHAM BELL.

Document 1-9(a–d)

(a) Octave Chanute, "Conditions of Success in the Design of Flying Machines," (1898) Bulletin No. XXIII, appeared in *American Magazine of Aeronautics* 1 (July 1907).

(b) Augustus M. Herring, "A Solution to the Problem of the Century," *The Aeronautical Annual* 3 (1897): 54–75.

(c) Octave Chanute, "Experiments in Flying: An Account of the Author's Own Inventions and Adventures," *McClure's Magazine* 15 (June 1900): 127–133.

(d) Octave Chanute, excerpt from "Aerial Navigation," *Cassier's Magazine* 20 (May 1901): 111–123.

Octave Chanute was the central figure in the exchange of aeronautical information worldwide at the end of the nineteenth century, and, more than anyone else, he brought the various pioneering researchers, experimenters, promoters, and inventors together into a trans-Atlantic community of aviation enthusiasts. He corresponded extensively and wrote widely in his efforts to advance the state of aeronautics.

Reflecting some of Chanute's own experimental work to perfect a flying machine, the documents presented here include his "Experiments in Flying" from the June 1900 issue of *McClure's Magazine*, and selections from Chanute's report "Aerial Navigation: Balloons and Flying Machines from an Engineering Standpoint." This latter article was printed in the May 1901 issue of *Cassier's Magazine*; Chanute read a revised and updated version with the same title before the American Association for the Advancement of Science on 30 December 1903. The Smithsonian Institution published this paper in its Annual Report of 1904.

Chanute's associate, Augustus Moore Herring is, without doubt, one of the more enigmatic actors in the drama associated with the invention of the airplane. Besides serving as pilot for Chanute's experimental glider flights in 1896, the young Herring made important contributions to Chanute's *Katydid* glider design. Notably this involved Herring's idea for a moveable—or "regulating"—tail that reacted positively to gusts.

After Herring parted company with Chanute and then spent the summer gliding at the Indiana Dunes in 1896, he continued his own aeronautical experiments in southern Michigan. On 11 October 1898, Herring made a powered hop in a machine of his own design. This took place at Silver Beach on Lake Michigan, near the town of St. Joseph. On October 27, the *Benton Harbor News* reported on the event: "During the flight, which lasted some 8 or 10 seconds, Prof. Herring's feet seemed almost to graze the ground while the machine skimmed along on a level path over the beach. The landing was characterized by a slight turning to the left and slowing of the engine, when the machine and operator came as gently to rest on the sand as a bird instinct with life." Matthias Arnot, Herring's financial backer, was present that day on Silver Beach, and he carried a camera. Unfortunately, Arnot was allegedly so surprised by what he saw that he did not take a picture until Herring was in the process of landing. The resulting photo showed the propeller of Herring's airplane revolving but the skids of his machine were not airborne but plowing up the sand, clearly in contact with the ground.

Even if Herring's eighty-eight-pound machine of 1898 did get slightly airborne, it lacked a control system and could only be controlled by the pilot shifting his weight. John Meiler, Herring's great-grandson, who has researched his ancestor's aeronautical career, concedes that the 1898 machine lacked such a system, but he claims that his great-grandfather had equipped a different flying machine with controls as early as 1894. "He realized the potential need for an auxiliary kind of control," Meiler has stated. "But he considered [his 1898 aircraft] an experimental flying machine," one that represented "just one more step in a lifetime of work," not its definitive culmination.

To this day, there are Herring family members and others in the St. Joseph area who believe that Augustus Herring made a powered flight in an airplane before the Wrights and that he deserves far greater recognition. Unfortunately for his reputation, there will always be the Arnot photograph as well as a negative report from his former associate Octave Chanute. After his "successful experiment" at Silver Beach on 11 October, Herring invited Chanute to travel to St. Joseph to witness his next flight attempt, on 16 October. Chanute came, but the air-compressed engine meant to drive the airplane failed to cooperate. The next day, when Herring damaged his plane trying to get it airborne, Chanute left town, thinking Herring a fraud. If he had waited a few more days, his opinion might have changed. According to eyewitness reports, Herring flew his machine successfully on 22 October 1898. Sixty-three years after the event, in 1961, a Mr. Sam Lessing, a hot dog vender at the time in 1898, told the *St. Joseph Herald-Press* that he had been among the witnesses to the event: "I saw it. . . . It couldn't turn, just flew straight ahead."

In his book *A Dream of Wings: Americans and the Airplane (1981)*, historian Tom D. Crouch reviews the fate of Herring's aeronautical career: "Augustus Moore Herring, badly shaken by the news of the Wright success, was unable to believe that these two newcomers to the field had so quickly solved those problems he had been unable to overcome. In 1908 he was a competitor with the Wrights for the first U.S. Army airplane contract, but withdrew his bid without producing a finished machine. The following year he entered a short-lived partnership with Glenn H. Curtiss that ended in a long-running lawsuit with Curtiss and an out-of-court settlement of $500,000 to Herring's heirs. He died in 1926" (p. 307).

Document 1-9(a), Octave Chanute, *"Conditions of Success in the Design of Flying Machines," 1898.*

After many centuries of failure, it is believed that we are at last within measurable distance of success in Aerial Navigation; that there will be two solutions; one with a dirigible balloon, which will chiefly be used in war, and the other with dynamic, bird-like machines which will possess so much greater speed and usefulness that they should preferably engage the attention of searchers.

I have, of late years, experimented with six full-sized gliding machines carrying a man, comprising three different types, and having reached some definite opinions as to the conditions of eventual success with power driven machines, it is ventured to state them briefly for the benefit of other experimenters; for final success will probably come through a process of evolution, and the last successful man will need to add but little to the progress made by his predecessors.

It is true that the most important component of the future flying machine will be the very light motor. It is the lack of this which has hitherto forbidden dynamic flight and restricted dirigible balloons to inefficient speeds, but it is also true that dynamic flight is impossible unless the stability is adequate. The progress made in light motors within the last ten years has been very great; Maxim, Langley, and Hargrave have produced steam engines weighing but about 5 kilogrammes to the horsepower, and hundreds of ingenious men are now improving the gas engine so rapidly that there is good hope that we shall soon be in possession of a prime mover which shall approximate in lightness the motor muscles of birds, which are believed to weigh but 3 to 9 kilogrammes per horsepower developed.

But even with a very light motor, success cannot be attained until we have thoroughly mastered the problem of equilibrium in the air. This fluid is so evasive, the wind so constantly puts it into irregular motion, that it imposes great difficulties even upon a bird, endowed as he is both with an exquisite organization, with life-instinct and with hereditary skill. It is to this one problem of equilibrium that I have devoted all my attention, in the belief that an inanimate artificial machine must be endowed with automatic stability in the air, and that experiments indicate that this can be achieved.

The wind is constantly in turmoil; it strikes the apparatus at different points and angles, and this changes the position of the center of pressure, thus compromising the equilibrium. To re-establish the latter requires either that the center of gravity, (or weight) shall be shifted to correspond, or that the supporting surfaces themselves shall be shifted, thus bringing back the center of pressure over the center of gravity. Birds employ both methods; they shift the weight of parts of their bodies, or they shift either the position or angle of their wings. It is believed that only the shifting of the wings is open for use for an artificial apparatus.

General Conditions.

It is inferred, therefore, that inventors who begin by working upon an artificial motor, and who endeavor to evolve a complete flying machine at once, are beginning at the wrong end, and are leaving behind them two very important prerequisites.

1st. That the apparatus shall possess automatic stability and safety under all circumstances.

2nd. That the apparatus shall be so light and small as to be easily controlled in the wind by the personal strength of the operator.

The general stability in the line of flight, the steering, can be obtained by a rudder, but the automatic equilibrium must be secured in two directions; first transversely to the apparatus, and secondly fore and aft. Very good results have been automatically obtained to the transverse stability by imitating the attitude of the soaring birds, the underlying principle of which consists in a slight dihedral angle of the wings with each other, either upward or downward, but the very best application of this principle is not yet evolved, and it requires more experimenting. Experimenters have found but little difficulty in securing stability in this transverse direction, but it must be worked out thoroughly.

The longitudinal equilibrium is, however, the most precarious and important. I have tested three methods of securing it automatically.

First, by setting the tail at a slight upward angle with the supporting surfaces, so as to change the angle of incidence of the latter through the action of the "relative wind" on the upper or lower surface of the tail. This is known as the "Penaud" tail; it is susceptible to great improvement in details of construction, as has been abundantly proved, but it is not yet certain that it will counteract all movements of the center of gravity in meeting sudden wind gusts.

Second, by pivoting the wings at their roots, so that they may swing backward and forward horizontally, thus bringing back automatically the center of pressure over the center of gravity, whenever a change occurs in the "relative wind." The so-called "multiple-wing" gliding machine was of this type, and it reduced the movement of the aviator required to meet wind gusts to about 25 millimeters. It cannot, however, be said its construction is perfected.

Third, by hinging vertically the supporting surfaces to the mainframe of the apparatus, so that these surfaces shall change their angle of incidence automatically when required. This last method has only been tested in models, other engagements having prevented experiments this year (1898). The other two methods have been applied to full-sized machines carrying a man. They have given such satisfactory results that not the slightest accident has occurred in two years of experimenting, but their adjustment has not yet reached the consummation originally aimed at; i.e., that the aviator on the gliding machine shall not need to move at all, and that the apparatus shall automatically take care of itself under all circumstances except in landing.

I shall be glad to furnish more minute descriptions to those who may want to repeat these experiments, or to apply the principles to machines of their own. The stability of an apparatus is the very first thing to work out before it is attempted to apply an artificial motor. This cannot be too strongly insisted upon, and the best way of accomplishing this prerequisite is to experiment with a full-sized gliding machine carrying a man. This utilizes the reliable force of gravity until such time as the automatic equilibrium is fully attained. Then, and not until then, it becomes safe to apply a motor.

When artificial power comes to be applied, it is probable that the best motor to use at the beginning will be found in a compressed air engine, supplied from a reservoir upon the apparatus. This is not a prime mover, but it is reliable and easily applied. It will probably afford a flight for but a few seconds, but this will enable the aviator to study the effects of the motor and propeller on the equilibrium of his machine. When this is thoroughly ascertained another motor may be substituted, such as a steam or a gasoline engine, which will produce longer flights, but this will require long and costly experimenting to obtain a light and reliable engine.

Another most important requisite is that the first apparatus with a motor shall be of the smallest dimensions which it is possible to design, and shall therefore carry only one man. This requisite is for four reasons: 1st. In order to keep down the relative weight, which increases as the cube of the dimensions, while the supporting surfaces increase approximately as the square; 2nd. In order to secure adequate control of the apparatus in the wind; 3rd. To diminish the power required for the motor; and 4th . To have as little inertia as possible to overcome in landing. The whole apparatus should be so light and small that the aviator shall carry it about on his shoulders and control it in the wind. This can only be accomplished with a gliding machine. My double-decked machine was of ample strength, with 12.5 square meters of supporting surface, weighing 11 kilograms, and carried a man perfectly on a relative wind of 10 meters per second. It showed an expenditure of 2 horsepower obtained from gravity. It is believed that a power machine can be built with 16 square meters of carrying surface, and a weight of 41 kilograms which will carry a man and a motor of 5 horsepower, if the latter with its propellers and shafts does not weigh more than 5 or 6 kilograms per horsepower. In fact, this has been done with a compressed air motor machine, but the apparatus thus far has produced doubtful results, in consequence of the defects of the motor. It is firmly believed that it will be a great mistake to experiment with a large and heavy machine, for it would probably be smashed upon its first landing, before its possibilities could be ascertained.

The speed first aimed at should be about 10 meters per second, and to achieve this the following are good proportions:

Sustaining surfaces	0.15 square meters per kilogram.
Sustaining surfaces	3.00 square meters per horsepower.
Equivalent head surface	0.25 square meters per horsepower.
Weight sustained	20.00 kilograms per horsepower.

Details of Construction.

The general arrangement and detail of the construction will conform, of course, to the particular design to be tested by the experimenter, but some useful hints may be given. There need be no hesitation as to the materials to employ. The frame should be or wood, which although weaker than bamboo is more reliable and permits the shaping of the spars so as to diminish the head resistance. It has been found by experiment that the best cross-section resembles that of a fish, with the greatest thickness about one-third of the distance from the front edge; this reduces the resistance to coefficients of one-sixth to one-tenth that of a plane of equal area, while a round section, such as that of bamboo, gives a coefficient of about one-half. The spars of the frame can best be joined together with lashing of glued twine or with very thin steel tubing, preferably silver or nickel-plated. The stays or tension members should be of the best steel wire, also nickel-plated and oiled to prevent rust. A very important detail, not yet worked out, consists in connecting the wires to the framework so that they shall pull alike. The supporting surfaces should preferably be of balloon cloth or Japanese silk, varnished with two or three coats of Pyroxelene (collodion) varnish which possesses the property of shrinking the fabric upon drying, so as to make it drum-like.

(A good recipe for this varnish is as follows:—Take 60 grams of gun cotton no.1, dampen it with alcohol to make it safe to handle, and dissolve it in a bottle containing a mixture of 1 liter of alcohol and 3 liters of sulphuric ether. When well dissolved, add 20 grams of castor oil and 10 grams of Canadian Balsam. This is to be kept in a corked can, and poured in small quantities into a saucer, whence it is applied thinly with a flat brush. Two coats will generally be sufficient. It dries very quickly, glues together all the laps in the fabric, and shrinks it in drying.)

An expeditious way of fastening the surfaces to the frame consists in stretching them as tight as possible and then doubling them back around the spar, the flap so made to then fasten temporarily with pins; the first coat of varnish will glue the surfaces together, and the pins may be withdrawn if desired.

Although it is preferable that some of the rear portions shall be flexible, the supporting surfaces and the framework must be sufficiently stiff not to change their general shape when under motion. This indicates bridge construction for the framework and therefore the super-imposing of surfaces. Very little supporting or parachute action will be lost by this, for even when struck at right angles by the wind, Thibaut found that a square plane placed behind another of equal size, and spaced at a distance equal to the length of its side, still experienced a pressure of 0.7 that on the front plane. The supporting surfaces will of course be arched in the direction of flight in accordance with the practice inaugurated by Lilienthal, who showed that they possessed angles of incidence of 3 degrees, five times the lifting power of the planes. It is not probable that success will be achieved in Aerial Navigation with flat sustaining surfaces.

Proportion of Parts.

In proportioning the parts, the factor of safety for static loads should generally be 3, never less than 2, and preferably 5 for the parts subject to the more important strains. These are computed in the same way as they are for bridges, with the difference, however, that the support (on the air), is to be considered as uniformly distributed, and the load is to be assumed as concentrated at the center. It is not believed that it is practicable to calculate the strains due to possible shocks upon landing. They must be taken into consideration in a general way, but the utmost efforts will be made to avoid them.

The sustaining power will be calculated in the manner given by Lilienthal in Moedebeek's "Taschenbuch für Flugtechniker und Luftschiffer." He does not, however, fully explain how to calculate the resistance; this consists of the "drift" or horizontal component of normal pressure, plus or minus the tangential pressure, and of the "head resistance" of the framework, or the motor if any, and the body of the operator. As an example how to compute this I give the calculations for the "multiple wing" gliding machine of 1896, which was constructed before experiments, that shows how the head resistance could be further reduced by adopting better cross-sections for the framework.

Area Head Resistance, Multiple Wing Machine.

Description	No.	Dimensions Millimeters	Square Meters	Co-efficient Resistance	Equivalent sq. Meters
Front edge of wings	10	2225 x 12.70	.28257	–	.14128
Main wing arms	10	1956 x 12.70	.24841	⅓	.08280
Ribs of top Aeroplane	3	1346 x 6.35	.02564	1	.02564
Posts of top Aeroplane	4	1829 x 12.70	.09291	⅓	.03097
Posts connecting front wings	8	1280 x 12.70	.12995	⅓	.04332
Posts carrying pivots	2	823 x 19.05	.03135	⅓	.01045
Curved prow pieces	3	914 x 24.50	.06717	1	.06717
Front bow braces	2	731 x 12.70	.01857	⅓	.00619
Rear bow braces	2	841 x 12.70	.02136	⅓	.00712
Cross struts bow & frame	2	670 x 12.70	.01702	⅓	.03613
Rear wing braces	4	2134 x 12.70	.10840	⅓	.03613
Rudder braces	2	1219 x 12.70	.03096	⅓	.01032
Rudder struts	2	548 x 12.70	.01392	⅓	.00464
Wire stays 61 meters		61000 x 1.27	.07747	1-½	.11620
Spring wire stays 8 meters		8000 x 1.27	.01016	1-½	.01524
Rubber springs	6	1300 x 1.00	.00780	1	.00780
Sundry projecting parts		Say	.01198	1	.01198
Aviator's body		Say	.46450	1	.46450
			1.66014		1.08742

In order to calculate the resistance, we must first ascertain the requisite speed for support and the consequent "drift." The front wings measure 13.34 square meters and carry all the weight, they are set at a positive angle of 3 degrees, for which the Lilienthal normal co-efficient η is 0.546. Using the well known formula $W = k\, s\, v^2\, \eta \cos \alpha$ in which W is the weight, k is the air co-efficient, s the surface, v the velocity, η the Lilienthal co-efficient (0.11) and α the angle of incidence, and calling W=86 kilos, we have for the support:

$86 = 0.11 \times 13.34 \times v2 \times 0.546 \times \cos 3°$;

and as cos 3° = 0.9986, we have for the speed:

$v = \sqrt{} 86 / 0.11 \times 13.34 \times 0.546 \times 0.9986 = 10.37$ meters.

Whence we have for the front wings:

Rectangular pressure	0.11 x 10.372 = 11.829 kilos. per square meter.
Normal pressure at 3°	11.829 x 13.34 x 0.546 = 86.16 kilograms.
Lift at 3°	86.16 x 0.9986 = 86 kilograms
Drift at 3°	86.16 x Sine 3° = 4.51 kilograms

The Tangential pressure upon the font wings is zero at 3°. The "drift" on the rear wings, which measures 2.74 sq. meters, and were set at a negative angle of 3°, consists in the product of their surface by the rectangular pressure, (Lilienthal's θ) which at this angle is positive, and the horizontal component of the normal (Lilienthal's η) which is negative at 3°, the latter being obtained by multiplying h by the sine of 3°. We have therefore:

Drift rear wings = 11.829 x 2.74 (0.013 = 0.212 x 0.05233) = 0.98k.

The head resistance is the important factor, and depends upon the shapes which are adopted for the framing to evade air resistance and to secure low co-efficients. It has to be calculated in detail, and the table herewith given recapitulates the various elements of the area of head resistance of the multiple wing machine, reduced by co-efficients to an equivalent area for further calculations.

The rectangular pressure of a speed of 10.37 meters per second being 11.829 kilos per square meter, we have therefore for the whole resistance:

Drift front wings	11.829 x 13.34 x 0.546 [x] 0.52333	= 4.51 kilos
Drift rear wings	11.829 x 2.74 (0.043 – 0.0126)	= 0.98 "
Tangential component at 3°		= 0.00 "
Head resistance	11.829 x 1.087	= 12.86 "
	Total resistance	= 18.35 "

As the speed is 10.37 meters per second, the power required to overcome this total resistance is:

Power 18.35 x 10.37 = 190.28 kilogrammeters or 2.53 horsepower, and as the weight is 86 kilos the angle of descent as a gliding machine ought to be:

Angle 18.35/86 = 0.2134 or tangent of 12°

In point of fact, the apparatus glides generally at this angle and frequently at angles of descent of 10 or 11 degrees, this being probably due to an ascending wind along the hillsides, and fully verifying this mode of calculating the resistance.

In the "double-decker" gliding machine, in which the framing was better designed, the resistance was calculated at 14.46 kilos, and it absorbed a horsepower in gliding in still air. By employing still better cross sections of framework, and especially by placing the aviator in a horizontal position, the head resistance could be reduced by at least one-third, but this particular attitude of the man would involve some risk of accident in landing, and is considered to be too dangerous to be employed in the preliminary experiments. It will be noticed in the table that the resistance of the wire stays is given a co-efficient of 1-½, while theoretically, being cylindrical, their co-efficient should be about ½. This allowance is based upon experience. Wire stays produce undue resistance, and this is probably due to the fact that they vibrate like violin strings when the apparatus is under rapid motion, and thus produce a greater resistance than that due to their rounded cross-section.

The power required will be seen to differ very materially from that indicated by the formula recently proposed in France, which is based on the assumption that the total wing surface, in square meters, multiplied by the co-efficient of air resistance (i.e., the number of kilogrammes carried by a square meter, at a speed of one meter per second) must at least be equal to the cube of the weight of the apparatus in kilogrammes; divided by the square of the power exerted by the motor in kilogrammes, or, $K\ S\ T^2 = P^3$ from which in our own case we would draw:

$0.11 \times 13.34 \times T^2 = 86^3$, or

$T = \sqrt{}\ 86^3 / 0.11 \times 13.34 = 658.4$ klgm

or 8.78 horsepower, which is more than three times the power calculated by the method here given and tested by actual experiment and measuring.

It must be remembered, however, that the 2.53 and the 2 horsepower, which have been found sufficient to sustain 86 kilogrammes in the air, are the *net* horsepower absorbed by the gliding machines. When a propeller and a motor are added, it will be necessary to allow for the losses in efficiency incident to those adjuncts, and so provide about twice the power to the engine which is indicated by the resistance multiplied by the speed. A safe rule of approximation will be to allow that each nominal horsepower at the engine will sustain 20 kilogrammes, and that each kilogramme of the total weight of the apparatus will require 0.15 square meters of surface to sustain it at speeds of about 10 meters per second. When greater speeds become practicable and safe, the surfaces may be reduced below this so that at 20 meters per second they may be but about 0.05 square meters per kilo, instead of 0.15 square meters per kilo indicated above, and this would permit reducing the head area of the framing, but unless the co-efficient for the aviator's body was in some way reduced the resistance and power required would be greater, because of the higher speed.

These are the conditions and considerations which experiments with full-sized gliding machines, carrying a man, have thus far indicated as necessary to observe in order to achieve success with a dynamic flying machine provided with a motor. The most important of them are:

FIRST, that the automatic equilibrium and safety shall first be secured before an attempt is made to apply a motor, and SECOND, that the apparatus shall be made as small and light as possible, so that the aviator may sustain its weight before taking his flights.

Document 1-9(b), Augustus M. Herring,
"A Solution to the Problem of the Century," 1897.

Perhaps no subject offers more scope to the imagination than the benefits and changes for mankind which would result from a practical solution of the problem of manflight. At the same time there is probably no problem which the inventive skill of man has ever attacked which apparently offers, at first sight, more numerous and easy ways of unravelment, and yet which, on careful investigation, develops greater or more unexpected difficulties. In beginning the experiments the methods apparently open might be roughly divided into four classes.

The first of these would comprise all those machines in which the whole or part of the weight was lifted by a balloon or gas-bag; the second, all those forms of apparatus which were intended to sustain or lift their weight with screw propellers revolving on vertical axes; the third, those machines which were intended to sustain their weight (and that of the operator) on flapping or beating wings; the fourth, and last, class would contain the aeroplane, or more properly the aerocurve machines; for the aeroplane may now safely be said to have disappeared from competition with the more efficient form of surface.

The limitations of the navigable balloon are now pretty well recognized. To obtain a speed of even 20 miles per hour, a spindle-shaped envelope of very large size is necessary, and the result at its best is an exceedingly frail and bulky machine, whose ultimate speed capacity is insufficient for wind velocities which frequently occur even near the ground. Its chief defects are great bulk and extreme frailty; for the envelope, in proportion to its relative size, is not many times stronger than a soap bubble. An instance may be cited in support of this in the large navigable balloon built for the Antwerp exposition, which became tilted up during a trial when the rush of gas to the higher end burst the balloon.

The future utility of the navigable balloon is still the subject of differences of opinion; it is, however, certain that whatever may be its ultimate practical advantages as a flying machine, the drawbacks of enormous size and frailty are sure to offer a considerable offset to them.

The vertical screw machines have much to recommend them, but there are far greater difficulties offered to their production than would be supposed. The ability to rise directly into the air from any given spot would be an exceedingly desirable quality. And hence we find that the great majority of experimenters who attack the problem of dynamic flight begin here, starting with a plan of some modification of this type of machine. The stumbling-blocks, however, are soon met. Not least among them is the fact that when the surfaces which form the blades of the screws are revolved over one spot (as they must be to rise directly into the air) they do not give any considerable lifting effect in proportion to the power consumed; for where one might from the theory even of the aeroplane expect a lift of possibly 100 pounds per horsepower, the best result the inventor can produce on a practical scale is pretty sure to be less than one-seventh of that figure. In fact, the lift with the lightest engines we can build is likely to be but little, if any, more than the weight of the machine itself. With engines weighing much more than 4 or 5 pounds per horsepower (250 times as powerful weight for weight as a man), practical success with this type of apparatus is not possible.

The third class, or the beating wing machines, are subject to the same disadvantages in regard to the enormous power required as those of the vertical screw type. In addition to this, the question of maintaining a stable equilibrium in windy weather still further greatly complicates them, so much so, in fact, that there is but small hope of practical machines operated on this principle ever being produced.

It is unnecessary to point out that any combination in a machine of the principles involved in either of the above three classes would still subject it to the fundamental objections of at least one of the classes. These objections are so formidable that, to the great majority of the foremost workers in this field, there now appears but one main principle left, and upon this there is an ever-increasing hope, if not certainty, that flight will be accomplished. This principle is the one which underlies the aeroplane and aerocurve; namely, that when a thin surface is driven rapidly through the air, and is slightly inclined to its path, the equivalent of a pressure is developed on the side which is exposed to the air current—i.e., the under side—which is much greater than the driving force necessary to produce it. If an arched surface (arched in the line of motion) with the hollow side undermost be substituted for a plane, we have an aerocurve. Its chief advantage is that it possesses a higher efficiency. Another, but minor, difference is that it is not necessary to incline an aerocurve in order to develop a pressure on the hollow side when it is moved through the air.

The one advantage which the dynamic or power machine of the aerocurve type has over the vertical screw is the fact that it can, through the agency of the surfaces, convert the relatively small push of the screw propellers into a much larger lifting effect.

It is interesting to note that the first approach to human flight of modern times was attained only by the use of the aerocurve, when early in 1894 the late Otto Lilienthal, of Berlin, Germany, built a huge bat-like machine, with curved rigid wings, on which he was able to “slide” downhill on the air, 150 feet or so at a time.

Practice with this machine soon enabled him to start from very high places, and his flights became correspondingly longer. Early in the beginning of these trials, he became aware, as his writings show, of the enormous power and disturbing effect which those ever-present irregularities in the wind produce, and which, in a large measure, were the cause of his losing his life—a sad accident which has taken from the field of aerodynamics one of, if not, the ablest of its workers; for both the practical and theoretical work of Lilienthal in the new science is of the greatest value, and will be so recognized when more generally understood.

In his first articles Lilienthal repeatedly cautioned others against attempting to glide in winds which exceeded 7 meters a second (about 15 ½ miles an hour), as being excessively dangerous. However, when he made the improvement on his machine of superimposing two smaller surfaces and thereby reduced the "tip to tip" measurement from about 24 feet to 18 feet, the diminished leverage upon which the gusts could act enabled him to sail in stronger winds, so that he even experimented in winds of 22 miles an hour. This, without further improvement in the automatic stability of his machine, was an unwise thing to do, and the accident which occasioned his death, on the 9th of last August, is, more or less correctly, attributed to it; nevertheless, the immediate cause was undoubtedly the result of defects in the machine itself, which had been allowed to deteriorate and get out of repair. In his double-deck machine the upper surface was joined to the lower one by two or three small vertical posts and numerous wires. Probably some of these wires had become rusted or so weakened that they broke when the machine was struck by a heavy puff, and so allowed the upper surface to tilt back and suddenly stop the headway of the machine, but not that of the unfortunate operator, who swung round and round over the apparatus as it pitched to the ground. This tendency to revolve over backward is frequently set up by a very strong sudden gust striking the machine squarely in front; it can, however, be counteracted by a quick movement of the operator's body and legs toward the front. A serious defect in the design of the Lilienthal apparatus is here seen, for on it the operator's position is somewhat strained, and his movement very limited, owing to the fact that he is obliged to hold to a small bar with both hands while his weight is carried on his elbows, which rest, a little farther back, on a portion of the main frame. (See Plate IX, Fig. 2.)

These defects suggested themselves to the writer when, in the summer of 1894, he built a machine similar in many respects to that of Lilienthal. It differed from his in two important particulars: first, the upward movement of the horizontal tail was limited; second, the range through which the operator could shift his weight was nearly three feet instead of about eight inches. To obtain this range of movement the weight of the body, when in flight, rested upon two horizontal bars fitting under the armpits. (See Plate IX, Fig. 3.)

No very startling results, however, were obtained with this machine or with the three subsequent ones, the longest flight attained being only 187 feet in length. Experiment with these machines, nevertheless, furnished a great deal of valuable information. No one who has not experimented with a machine of the Lilienthal type can form any accurate conception of the tremendous power and lifting effect which 130 to 150 square feet of concave surface can exert. It is with an apparatus of this kind that a novice first becomes fully aware that no wind is anything like constant, and that the power of those much-talked-of "gusts" is real, and not imaginary.

Anyone wishing to begin experiment with a gliding machine cannot be too cautious in the selection of an experimental station. Nothing could be more dangerous than to start from a flat roof or a precipitous cliff, or to begin experiment in a locality where surrounding objects, such as hills, buildings, or even large neighboring trees, are likely to break up the wind into swirls and eddies. What is most desirable—in the beginning, at least—is a hill surrounded by country that is as level as possible. Both the starting and landing points should be on comparatively soft earth, free from stones, bushes, and snags. Perhaps the best station of all is to be had where there are high, bare sandhills or dunes, facing a large body of water. The slope of such a hill (to a beginner) is of as much importance as anything else; it must be steep at the top and run off gradually as it nears the bottom so that when he has gained proficiency enough he may start from near the top in calm weather and yet have his flights always close to the hill-side. If this be so and the soil be comparatively soft, the operator can easily save himself from a dangerous fall which might result from a poor start or a breakage of the machine.

In the first experiment with a full-sized gliding machine, a man's natural instincts irresistibly impel him to move in the wrong direction when the balance of the apparatus is disturbed. It is, therefore, at first, impossible to distinguish between effects produced by one's own errors and those produced by wind changes, but in time three separate causes of unsteady flying become easily distinguishable from each other; namely, improper adjustment of the machine, errors of the operator, and changes in the trend, velocity, or direction of the wind.

When the mastery of the machine becomes about as perfect as possible very much of this unsteadiness disappears. Nevertheless, with a wind as steady as winds ever are,—even after having blown for hundreds of miles over absolutely level prairie, or, as in the case of our later experiments, having come in an unobstructed path for several hundred miles over the waters of Lake Michigan,—we found that the effect was by no means a steady one, but was such as to indicate that they were broken up into an inconceivable number of irregularities in pressure, velocity, and direction, in spite of the fact that a light anemometer showed fluctuations in velocity of seldom more than 10 or 12 per cent in readings of 5 seconds duration, taken 10 to 20 seconds apart.

In a wind of 9 to 10 miles per hour with a simple machine of the Lilienthal type the disturbances of the wind are barely noticeable, but at 12 miles they are quite apparent, at 14 miles they require considerable practice to combat, at 16 ½ miles, even with best skill at command, a flight is more or less risky; and when the wind blows above 18 miles per hour it is dangerous, even with a total load of 182 pounds (machine, 27 pounds, operator, 155 pounds) on 130 square feet of surface.

Nothing, perhaps, is more surprising than the power which a gust in even a 14 or 15 mile wind will occasionally exhibit, such, for instance, as sometimes happens to an inexperienced or careless operator, who, in facing the wind with the machine preparatory to making a start, suddenly finds himself lifted anywhere from 2 to 10 feet above his starting place. These flights are invariably backward, and are due to mismanagement in allowing the wind to catch under the surface of the machine while the operator is too far back on it to exert a proper control. In the hands of a skilled person, the flights, in mild winds, generally appear to an on-looker as remarkably smooth, and even in spite of the fact that the operator is seen to frequently shift his position on the machine with considerable rapidity; yet in slightly stronger winds—those of 15 to 16 ½ miles per hour (mean velocity)—the irregularities become very perceptible to the spectator, who may sometimes see the apparatus rock and toss not unlike a ship in a rough sea.

To appreciate the causes which render a gliding machine, or in fact any machine of the aerocurve type, unstable, it is necessary to understand, in a measure, both the peculiarities of the wind and the effect they have on the position of the center of pressure of the surfaces.

In order for any apparatus in free air to be in equilibrium it is necessary, of course, that the center of pressure should be in the same vertical line as the center of gravity. It is not, as many believe, absolutely necessary that the center of weight should be beneath the sustaining surfaces; this may be demonstrated by trying the small paper model shown in Plate XII, Fig. 1, which, if not weighted too heavily, will always fly with the "fin" side up, even if dropped with the weight and fin side undermost.

When a surface is inclined to the air through which it is moving, the lifting pressure is not uniform, but is very much greater toward that edge which is first struck by the current. On a square *plane* 100 inches on a side, the center of all the lifting pressures may be anywhere between the center of the figure and as far forward (apparently) as 14 inches from the front edge, according to the angle and speed at which the surface is presented to the air. The travel sidewise might be even more, granting that gusts may come from either side. With an aerocurve the travel is probably seldom more than ⅕ as much as it would be with a plane. In practice with any ordinary gliding machine of large surface it is found that the gusts come from any quarter: in front of the apparatus, from the extreme left to the extreme right, and in a wind of over 22 miles per hour they follow each other with such suddenness and with such extreme changes that it is absolutely impossible to shift one's weight in time to counteract them. These conditions, which had to be met, made it imperative to seek for automatic stability along very different lines from any that had heretofore been tried.

The changes or gusts which have the most influence in disturbing the machine are seldom of more than half a second duration, and oftentimes they last less than half that time; yet in so short an interval, it has frequently happened to me, in my experiments with my first three gliding machines, that in less than half a second the lateral equilibrium was so far disturbed that the lateral axis of the machine would make an angle of 35 to 40 degrees with the horizontal. At other times, the angle of advance (the angle at which the surfaces are presented to the air) was so much increased that nearly every bit of the headway was destroyed. On two occasions, the change in direction, both vertically and to the side, was so violent and sudden as to shake my hold loose of the machine.

During my last flight on a Lilienthal type of machine, while experimenting in a wind of about 18 miles an hour, the machine was struck twice in quick succession by a gust from the right. The first impulse raised that side until the apparatus stood at an angle of about 40 degrees; the second impulse, which came between ¼ and ¾ of a second later, increased the inclination to nearly a vertical one, so that one wing pointed to the ground and the other to the zenith. Anticipating a complete overturning of the machine (as did happen) I let go my hold and dropped to the sand below, a distance of not more than 12 or 14 feet, where I landed on my feet, but on the left wing of the overturned machine, which had drifted under me as I fell. This accident damaged the machine so much that it was not rebuilt. We recognized from these experiments that the disturbances increased much more rapidly than the mean velocity of the wind; also that in winds of 18 miles or over, it was impossible for a man to shift his weight far enough and rapidly enough on a single surface machine to keep it in proper equilibrium under all circumstances.

In addition, the conclusions were reached, first, that the angle of advance *must* be automatically maintained, with almost absolute certainty, at a very small angle; second, that the lateral equilibrium should also be largely, if not wholly, automatic, and be maintained by some more effective method than a dihedral angle between the surfaces, or by placing the operator far beneath the apparatus. The first of these is considerably the most important, as disturbances of the angle of advance generally entail disturbances of the lateral equilibrium as well.

In the beginning of experiment to obtain longitudinal stability, three clearly defined methods (each with its own limitations developed by experiment) appeared open.

The first and simplest method is to find such a surface, or grouping of surfaces, that the displacement of the center of pressure is very great for very small changes of the angle of incidence; the second method is to find such a form of surface that its center of pressure remains in one spot, no matter from what angle the relative wind may come; the third method is to provide a separate mechanism to either take up or counteract the disturbing effects of the wind changes.

The first method may be said to have been practically attained (as far as it is attainable by such an arrangement) in the various modifications of the Hargrave kite, and also in its predecessor, the Brown biplane, or, a little better still, in what might be called a "bicurve,"—that is, a biplane machine on which slightly arched surfaces have been substituted for planes. Such a model is shown in Plate IX, Fig. 4, B being the front aerocurve. This model will maintain a very good equilibrium for gliding flight so long as the center of gravity is anywhere between C and the rear edge of B; but each change in the position of the center of gravity corresponds, of course, to a different angle of flight, *i.e.*, to an angle at which the center of pressure of the combined surfaces coincides with the vertical line through the center of gravity of the whole apparatus.

The center of pressure travels toward B as the angle of flight is diminished, and in the reverse direction when it is increased. This great range in the possible position of the center of pressure is a measure of the corresponding change in efficiency which the rear surface undergoes at various angles of flight. This efficiency diminishes very rapidly as the angle of flight is diminished; at very flat angles its useful effect disappears altogether. Not only this, but at small angles of incidence, those under .8 degrees, the longitudinal stability of the arrangement disappears as well. From many experiments with gliding models of this type I found it impossible to obtain glides which represented a travel of over 4 lineal feet for each foot of height lost, unless the model was so weighted that no part of the weight rested on the rear surface. Also, that the power required to support any given weight at an angle of 15 degrees or less is about twice as much as would be needed on superimposed surfaces of the same size held at the same angles of inclination.

When the model shown in Plate IX, Fig. 4 (or any similar one) is so loaded that each surface must carry about half the total weight, the apparatus will take up an angle of about 26 degrees with the relative wind. Under this condition a dynamic model would require between 40 and 46 per cent of its weight in thrust to keep it "afloat," or if liberated as a gliding model it will travel forward a little over *twice* as far as it descends vertically. At the flattest angle, about 15 degrees, at which it maintains a good equilibrium, it will glide only 3 to 3 ¼ times as far as it falls.

As the rear surfaces in an apparatus with following surfaces come more and more in the "wake" of the front elements, the relative supporting effect of the rear becomes less and less as the angle of flight is diminished. Thus, through the phenomena of "interference," the relative efficiency (as a lifting factor) of the rear surfaces becomes greater or less (as the angle of flight is increased or diminished)—a corresponding travel of the center of pressure results which maintains the longitudinal equilibrium, but at the expense of extra weight of the apparatus and considerable additional power.

This fundamental principle—that of interference—underlies the stability of the Malay, Eddy, Bazin, Lamson, Chanute, and Hargrave kites. It is still further applied in the three last named, in which the vertical keels form pairs of Brown's "biplanes," which maintain the lateral equilibrium as well.

It would seem that one might be able to avail of the wonderful stability which a system of following surfaces exhibits, by so grouping the surfaces *vertically* as well as horizontally that they could not interfere. It is comparatively easy to so space them that interference is practically avoided, but from several hundred experiments in this direction I have invariably found *that the automatic equilibrium is always impaired in direct proportion as the front and rear surfaces cease to interfere*. The further conclusion arrived at from these experiments was that in following surface machines, a low efficiency is essential to insure safe equilibrium. Quantitively this efficiency is so low that probably less than 30 pounds can be carried per horsepower when the surfaces are loaded to a greater extent than one pound per square foot of area. Consequently, though dynamic models might be made that would work satisfactorily on this plan, a full-sized machine to carry even one man would offer no such encouragement to its projector, chiefly because the weight of a machine increases much more rapidly than its surface or supporting power. Following surfaces therefore are not available.

Just as experiment and careful measurement made this fact clear, a new prospective method of obtaining automatic equilibrium began to open up. A study of the peculiarities in the travel of the center of pressure of variously arched surfaces indicated the possibility of evolving such a form that the center of the lifting pressures would remain in the same spot for all angles of inclination. The result of much investigation in this line is shown in Plate XI, Fig. 1. Strictly speaking, this piece of apparatus is a gliding model, and as such in a wind of 30 miles an hour or less it possesses a perfect equilibrium, owing to the fact that the position of its center of pressure is almost absolutely constant for all angles of incidence between plus 90 degrees and minus 20 degrees; the same is also approximately true whether the wind strikes from in front or "abeam." It will also fly as a tailless kite, but as such is somewhat inferior to the Hargrave, both in steadiness and in the "angle of the string," yet the lifting effect is probably four times as great per square foot of surface. The projected area of the dome is not quite 6 square feet, yet in some experiments it has registered a pull of over 40 pounds on a spring-balance, and on one occasion repeatedly broke a cord tested to 69 pounds.

As a flying machine it would have the advantages of being able to sustain great weight on a very small surface, and would require but a slow speed to do so. A dome machine of only 9 ½ feet in diameter would be of sufficient size to carry a man in gliding flight at a speed (relative to the air) of only 20 to 21 miles per hour. But its drift is so great that it would not carry the operator horizontally more than 2 ¼ to 2 ¾ times the height from which it started. As a great thrust of the screw is far more costly in power than great speed, such a machine could hardly be a practical success. However, owing to the very low sailing speed, a dynamic machine to carry one man might be built which would fly. It would be of little practical value, owing to the excessive power required, and its limited speed capacity. This line of experiment was therefore laid aside in the spring of 1896.

By way of explanation I may here add that from a great number of previous experiments with various devices—such as modifications of the drag rudder, gyrostat, and pendulum regulators—I had come to the belief (which all my more recent experiments have only served to strengthen) that *the action of any device to maintain a machine in safe equilibrium must be such that it prepares the machine for each impending wind change before that change actually occurs*, and that any device which tends to forcibly right the flying machine *after* it has departed from an even keel more often produces (in the open air) greater *unsteadiness* than the reverse. The reason for this is not far to seek. It lies chiefly in the fact that the most formidable, as well as the most frequent, disturbances met within the natural wind are cycloid gusts or rotating masses of air which frequently give a machine (or model) powerful double impulses. These impulses are generally opposite in their effect, and succeed each other by irregular intervals varying from about one-fourth to one second apart; and, therefore, a regulator, such as a pendulum or gyrostat mechanism, which begins to act on the machine after it is disturbed, and continues to do so until it regains an even keel, is often the means of greatly augmenting the second impulse of the pair, or the first of a new gust which may strike the apparatus from a different quarter. I do not mean to say that such devices will not work at all; on the contrary, they can be made to give very good results in fairly mild weather, but they all fail in winds of much less velocity than those which any practical machine must be able to contend with.

This conclusion reached, it would appear that the methods left would be found only through a careful study of the wind changes themselves. As before stated, I had become aware almost from the beginning of my gliding experiments of several distinct kinds of disturbances, the most formidable being very sharp, well-defined changes in the velocity and direction of the wind, which last but a fraction of a second and appear to come in pairs. Their distinguishing characteristic (besides their much greater suddenness and power) is, that in practically all cases they are preceded by a perceptible warning which generally consists of a slight strengthening of the wind followed by a momentary calm, which in turn is followed immediately by the "gust" in its fullest force. During the momentary freshening, the wind either comes from or veers in the direction from which the gust proper will strike. These changes can easily be verified by an observer in a strong wind by noting the effects as he feels them on his face.

There are many observations which might be given to corroborate the theory that practically all the gusts which have any material effect in disturbing the angle of advance or the lateral equilibrium of an apparatus are of a rotary character. They are, in fact, nothing more or less than diminutive tornadoes which travel, however, much more rarely on vertical axes than on diagonal or horizontal ones. In a few cases the axis of a gust is found to be horizontal and parallel with the wind. In the majority they are nearly horizontal, but across the direction of the mean wind. The direction of rotation is usually backward, i.e., in the reverse direction that a wheel would have in rolling over the ground. In what may be called steady winds the swirls are of much greater diameter, and the out-flowing eddies, or the momentary freshening of the wind, precede them by a longer interval.

The observations which led to a recognition of the rotary character of the wind changes also were the means which furnished an explanation of the action of certain simple devices, previously found to work with success as far back as 1890 on a small dynamic model which has been illustrated in a previous issue of this Annual. I was, therefore, not wholly in the dark in commencing experiments to produce a regulator which should prepare the apparatus to meet each particular "gust" before it arrived.

The horizontal regulator of the dynamic model was slightly modified to adapt it more closely to the new theory, and in May, 1896, applied to the kite shown in Plate XI, Fig. 2. This kite, which is here shown in a 28-mile wind, possessed such perfect power in maintaining the surfaces at a small angle with the wind, through changes which would otherwise prevent it from flying at all, that in momentary freshening or changes, it would rise until the strings passed the zenith and made an angle of 6 to 8 degrees beyond the vertical. The average angle maintained by the surfaces with the horizontal varied between such narrow limits that it could not be easily detected by the eye. From a number of observations it was found possible to set the regulator to maintain an angle of between 2 and 3 degrees (above the horizontal), and calculations from the weight and surface of the kite, the pull on the string, and its angle above the horizontal show that the lift and drift of the kite correspond very closely indeed with the theoretical ones computed from the annexed tables.

Later in the summer this regulating device was improved and its use extended so as to counteract the rotating columns whose axes were more or less vertical, and thus preserve the lateral equilibrium of the apparatus. With this change it was applied to the gliding machine shown in Plate XIII, Fig. 1. By its use the safe limit of wind in which experiments could be carried on was raised from 16 ½ miles per hour (with the simple Lilienthal, or 20 miles with the Lilienthal double deck) to over 30 miles, and with it the maximum length of flight was increased from 187 feet to 359 feet; at the same time the rocking and tossing of the apparatus was reduced to such an extent that an on-looker could not in any of the 150 to 200 flights detect that the apparatus in flight ever departed from an even keel, either laterally or longitudinally,—i.e., the angle of advance was maintained perfectly at the very flattest angle. It is evident from repeated measurements that this angle never exceeded 4 degrees with the relative wind.

The difference in the amount of ascending trend of the wind at different times and at various points in front of the hill made great differences in the length of flights. The results of an average flight in calm air are here given:

Net projected area of 2 supporting surfaces, 134 square feet; size, 16 feet 2 inches x 4 feet 4 inches.

Net area of horizontal tail (which receives a pressure on its upper side), 19 square feet.

Weight of machine, 23 pounds.

Weight of operator, 155 pounds.

Press upper side of tail (acting as weight), about 7 pounds.

Total weight carried by 134 square feet, 185 pounds.

Total weight carried per square foot area, 1.37 pounds.

At the time of the following experiment the air was nearly calm, the only trace of wind was from the northeast; the flight was made by running downhill toward the north. Length of flight, 242 feet from last footprint to first at landing; time of flight, 7.4 seconds (in the air); difference in level between points was 42 ½ feet. Speed of machine was therefore practically 22 miles an hour. The wind pressure is 22 x 22 x .005 = 2.42 pounds per square foot. The proportion of this as a sustaining factor was 1.37/2.42, or 57 per cent. By referring to the tables hereto appended it will appear that this amount of lift (57 per cent of the normal pressure) corresponds to a positive angle of the surfaces of between 3 and 4 degrees, and by referring to the third column we find that the drift of the surfaces is (.0525 for 3 degrees and .0582 for 4 degrees) about .056 times the total weight of machine and operator and negative pressure on the tail, or .056 x 185 = 10.36 pounds, which is drift of the surfaces alone; to it we must add the head resistance offered by the framing of the machine and that offered by the operator's body. The framing consists of 64 lineal feet of timber which forms the main arms of the wings; this has a thickness of an inch across the wind, and therefore exposes a cross-section surface of about 3.3 square feet. The upright posts are 64 feet in collective length, being on an average 6⁄10 of an inch in width (across the wind). Their area is therefore practically 3.2 square feet. But as they are sharpened more or less to lessen the resistance they offer to the wind, the total area offered by the woodwork, instead of being 3.3 + 3.2 = 6 ½ square feet, is equivalent to only 2 ½ square feet. Besides this the framings of the tail and vertical rudder expose an equivalent of half a square foot of surface. The regulator and its cords, bands, etc. expose .52 of a square foot, and 160 lineal feet of wire .05 of an inch in diameter, expose the equivalent of .5 square foot more, making the total equivalent area exposed equal to 4.02 square feet. This at 22 miles an hour would offer a resistance of 4.02 x 2.42 = 9.73 pounds; to this must be added the resistance offered by the 5 square feet of the operator's body, arms, and legs. This brings the total resistance to: Resistance of surfaces, or drift, 10.36 pounds; head resistance of machine, 9.73 pounds; and resistance offered by operator, 12.1 pounds; total = 32.19 pounds. This moved over a distance of 242 feet would consume 242 x 32.19 = 7,790 foot-pounds, which would be furnished by the weight of the machine and operator (178 pounds), descending through a vertical distance of 7,790 ÷ 178 = 43 feet 9 inches, against an actual measured height of only 42 ½ feet. The difference in energy can easily be accounted for in either of three ways: First, a slight overestimate of the resistance offered by the operator's body; second, the presence of a slight ascending current of air; or, lastly, that the speed gained in running down the hill at the start was greater than 22 miles an hour. (It is possible to gain a speed of 26 miles if the weight is about half supported on the machine.) It may be interesting to point out in passing that the energy (7,790 foot-pounds) absorbed in keeping the machine and operator afloat during 7.4 seconds represents barely 2-horsepower, but less than one-third of this is drift of the surfaces. It is, however, now pretty well known that it would take at least 3-horsepower to produce a thrust of 32 pounds, even with as large screws as could be conveniently carried on a machine of this size.

The details of the regulating mechanism of neither this nor the "three-deck" machine have been here given, as they are now the subject of applications for patents; nevertheless, to anyone wishing to repeat the experiments I shall be pleased to give all the information necessary.

During October I constructed a new machine of the same general design as the "double deck" but provided with three superimposed surfaces instead of two. In this a considerable change was made in the mechanism which governed the lateral equilibrium. Instead of depending upon the power in the small eddies which precede a rotating gust to operate the machine, their power was used only to work the valves of a mechanism operated by compressed air; in this way the regulation (which is accomplished through a reflex action) became much more powerful and prompt. The tests of the new gliding machine showed that a considerable advance had been made, in that the limit of wind velocity in which flights were safe was raised from 31 ½ miles an hour to over 48, and the maximum length of flight increased from 359 feet to 927 feet (best) and 893 feet (second best); at the same time it was found quite safe to turn the apparatus and fly at a considerable angle with the wind. It was by this means chiefly that the length of flight was increased, as the longer flights were made while "quartering" on the wind; that is, the apparatus after starting (the start must always be made dead against the wind) was kept pointing at an angle from 15 to 35 degrees with the wind, according to the strength of the latter, while the apparatus itself moved along a course nearly, but not quite, at right angles to the wind. This enabled me to keep close to the hillside and take advantage of the rising current of air flowing over the slope. In a few of the flights it would have been possible to have landed on a higher point than the starting one, owing to irregularity of the wind which occasionally raised me, after having gone several hundred feet, to a level above my starting place; these rises were only momentary, and all the flights as a whole were on a descending grade. As the slope both to the right and left had several clumps of small trees which it was necessary to steer over or around (according to the height at which the machine happened to be while it passed near them), these "quartering" flights were not made to any great extent.

With a machine on which the angle of advance is automatically controlled with a fair degree of accuracy, the steering requires but little more effort than a bicycle, and at the same speed, *i.e.*, above 20 miles an hour, I have much doubt in my mind whether a bicycle (on a level road) could be turned on a much shorter radius than a flying machine. It is possible to land within less than 5 feet of any predetermined spot if it be selected well within the range of flight of the starting place.

In a few of the glides with the last machine I attempted to carry an additional weight, in the shape of a bag partially filled with sand. This bag was fastened between the middle and bottom surfaces and, beginning with about 12 pounds, the weight was gradually increased until 41 lbs. were carried without materially shortening the length of flight. The heavier weight considerably increased the difficulty in landing in light wind, owing to the greater speed relative to the ground. In high winds it was of very little hindrance either in starting or landing. The object in view in experimenting with the weight was to ascertain the power required on a dynamic machine and to test the manageability of the apparatus with a weight equivalent to the necessary engines and supplies. The result was so very encouraging that I have since then commenced constructing the engines. One of the pair is shown in Plate XIII, Fig. 2. It develops (alone) about two-thirds of the total thrust-power needed; its weight is only 12 pounds; its action is, however, a little irregular, and on that account is still the subject of experiment; it is a gasolene engine of the Otto cycle type.

Not least among the interesting results brought out by these gliding experiments is the fact, which becomes more and more evident from repeated experiment, that there is a very great difference in the supporting power of the air, whether one faces the natural wind or advances through still air; for while a natural wind of 18 to 19 miles per hour is sufficient (over level ground) to support the double-deck machine and operator, and will even momentarily raise them directly in the air for a foot or two, the same machine requires a minimum speed of 22 miles to support the same weight at the same angle in still air.

On Plate XIV are given drawings of the three-deck machine; another drawing will be found on Plate XII, Fig. 2, giving exact sizes of struts, etc., in cross-section. If built to scale the machine will have 227 square feet (net) surface. This is, however, 40 per cent more than a man of average weight ever requires except in a calm or in winds of less than 10 miles an hour. In winds over 12 miles an hour a beginner will get along better with the upper surface removed. If this machine is built on such a scale that the dimensions are only two-thirds of those given, it will be of ample size (103 square feet surface) for the average operator in any wind of over 25 miles an hour. It is better not to reduce the size of main spars and struts at all from the sizes given for the larger machine when constructing the surfaces on a smaller scale. The sizes given are for the best grade of black or silk spruce only; this wood will stand at least 16,000 pounds to the square inch; it must be straight grain and entirely free from flaws.

There is, perhaps, no better sport imaginable than coasting through the air, especially so where the flights are comparatively long. In moderate winds, of 18 to 25 miles per hour, the path of the machine is often quite horizontal for a hundred feet or so after leaving the hillside, until, in fact, the rising current which flows over the hill has been cleared. If during the first part of the flight an operator wishes to keep near the ground he may do so by moving an inch or two forward on the machine; he will find, however, that in thus sailing downward through an ascending wind the speed increases at a tremendous rate.

Perhaps the most trying ordeal is experienced when the machine unexpectedly encounters a strongly ascending current of air which may raise the operator, in some instances, 40 or 50 feet above his line of flight. Such occurrences are comparatively frequent in a wind of 30 miles or over, but are not dangerous so long as the regulating mechanism remains in working order, as the machine then retains an absolutely level keel. I have twice been raised as much as 40 feet above my starting point without either myself or those who were on the ground being able to detect any change whatever in the inclination of either axis of the machine. Considering the fact that the rise through even such a distance seldom takes more than 1 ¼ to 1 ½ seconds, the automatic stability of the machine would seem to be well attained.

After having adjusted the regulators, and repeatedly tested them in a number of short flights, and at the same time having found the correct position for his weight, all the beginner need do, after starting, is to keep as still as possible and he will make a very creditable flight. If it be necessary to steer to the right or left, moving the body over to that side and a little forward will accomplish the result. For ordinary steering it is seldom necessary to do more than stick out one leg toward the side to which you wish to turn. If you meet a very strongly ascending trend of wind it is manifested by an increase in the weight which appears to rest on your arms; in such a case the vertical rise may be greatly diminished by moving 2 or 3 inches forward of the normal position as long as the rise continues; it is better, though, to simply stick the legs out in front.

Descending currents of air diminish the weight on the arms and give one the sensation experienced in a quick-starting elevator on a down trip. So far, out of, possibly, over 300 trials with the regulated machines, a descending current has never brought the machine quite down but once, but even then the dropping speed was not too great to make a comparatively easy landing possible. On the other hand, the machines have been momentarily raised above their line of flight in probably 2 flights out of every 5. And in winds above 25 miles an hour the machines have risen above the starting point in as many as 75 per cent. of the flights. The highest rise was probably little short of 60 feet. The most difficult thing a beginner has to learn is how to land, *i.e.*, when to move back on the machine in order to check its headway; this knowledge can only be gained by actual experiment.

Document 1-9(c), Octave Chanute, "Experiments in Flying," 1900.

It is considerably over forty years since I first became interested in the problem of flight. This presented the attraction of an unsolved problem which did not seem as visionary as that of perpetual motion. Birds gave daily proof that flying could be done, and the reasons advanced by scientists why the performance was inaccessible to man did not seem to be entirely conclusive, if sufficiently light motors were eventually to be obtained. There was, to be sure, a record of several thousand years of constant failures, often resulting in personal injuries; but it did not seem useless for engineers to investigate the causes of such failures, with a view to a remedy. I, therefore, gathered from time to time such information as was to be found on the subject, and added thereto such speculations as suggested themselves. After a while this grew absorbing, and interfered with regular duties, so that in 1874 all the accumulated material was rolled up into a bundle and red tape tied around it, a resolution being taken that it should not be undone until the subject could be taken up again without detriment to any duty. It was fourteen years before the knot was untied.

Meantime a considerable change had taken place in the public attitude on the question. It was no longer considered proof of lunacy to investigate it, and great progress had been made in producing artificial motors approximating those of the birds in relative lightness. The problem was, therefore, taken up again under more favorable circumstances. A study was begun of the history of past failures, and the endeavor was made to account for them. In point of fact, this produced a series of technical articles which swelled into a book, and also led to the conclusion that, when a sufficiently light motor was evolved, the principal cause of failure would be that lack of stability in the air which rendered all man-ridden flying machines most hazardous; but that, if this difficulty were overcome, further progress would be rapid.

Experiments were, therefore, begun to investigate this question of stability and safety, and, if possible, to render the former automatic. These experiments were hundreds in number, and were, at first, very modest. They consisted in liberating weighted paper models of various shapes, either ancient or new, with gravity as a motive power, and observing their glides downward. This was done in still air. After a while, resort was had to larger models, with muslin wings and wooden frameworks, carrying bricks as passengers; and these were dropped from the house-top in the early morning when only the milkman was about. Very much was learned as to the effect of the wind; and then tailless kites of all sorts of shapes were flown, to the great admiration of small boys. During the seven or eight years within which this work was carried on, some glimmerings were obtained of the principles involved, and some definite conclusions were reached. But it was only after Lilienthal had shown that such an adventure was feasible that courage was gathered to experiment with full-sized machines carrying a man through the air.

Otto Lilienthal was a very able German engineer and physicist. He demonstrated that concave wings afforded, at very acute angles, from three to seven times as much support as flat wings in the air. He made, from 1891 to 1896, more than 2,000 successful glides, the longest being about 1,200 feet, upon machines of his own design, launching himself into the air from a hilltop and gliding down against the wind. In 1895, he endeavored to add a motor, but found that this complicated the handling so much that he went back to his gliding-device. It was while experimenting with a double-decked machine of this character, which probably was in bad order, that he fell and was killed, in August, 1896. Thus perished the man who will probably be credited by posterity with having pointed out the best way to preliminary experiments in human flight through the air.

Just before this dismal accident, I had been testing a full-sized Lilienthal machine. I discarded it as hazardous, and then tested the value of an idea of my own. This was to follow the same general method, but to reverse the principle upon which Lilienthal had depended for maintaining his equilibrium in the air. He shifted the weight of his body, under immovable wings, as fast and as far as the sustaining pressure varied under his surfaces. This shifting was mainly done by moving the feet, as the actions required were small except when alighting. My notion was to have the operator remain seated in the machine in the air, and to intervene only to steer or to alight; moving mechanism being provided to shift the wings automatically, so as to restore the balance when endangered. There are several ways in which this can be done. Two of them have been worked out to a probable success in my experiments, and there is still a third which I intend to test in due course.

To make such experiments truly instructive, they should be made with a full-sized machine and with an operator riding therein. Models seldom fly twice alike in the open air (where there is almost always some wind), and they cannot relate the vicissitudes which they have encountered. A flying-machine would be of little future use if it could not operate in a moderate wind; hence the necessity for an operator to report upon what occurs in flight, and to acquire the art of the birds. My own operations were conducted from that point of view, with the great disadvantage, however, that being over threescore years of age, I was no longer sufficiently young and active to perform any but short and insignificant glides in such tentative experiments; the latter being directed solely to evolving the conditions of stability, and without any expectation of advancing to the invention of a commercial flying-machine. I simply tested various automatic devices to secure equilibrium, and, with great anxiety, employed young and active assistants. The best way to carry on such adventures is first to select a soft place on which to alight. This is well secured on a dry and loose sand-hill, and there ought to be no bushes or trees to run into. Our party found such sand-hills, almost a desert, in which we pitched our tent, on the shore of Lake Michigan, about thirty miles east of Chicago. The main hill selected was ninety-five feet high; but the highest point started from was sixty-one feet above the beach, as the best instruction was to be obtained from short glides at low speeds.

With parties of from four to six persons, five full-sized gliding-machines (one rebuilt) were experimented with in 1896, and one in 1897. Out of these, two types were evolved, the "Multiple-Wing" and the "Two-Surfaced," which are believed to be safer than any heretofore produced, and to work out fairly well the problem of automatic equilibrium. The photographs herewith reproduced, many of them heretofore unpublished, are from snapshots taken of these two types. In 1896, very few photographs were taken, all the attention being devoted to studying the action of the machines, and the one picture shown is the sixth permutation of the "Multiple-Wing" machine, so-called. In 1897, there was more leisure to take snap-shots, as the machine used was a duplication of the "Two-Surfaced" of 1896, supplied with a regulating mechanism designed by Mr. A. M. Herring, my assistant. Each photograph was taken from a different experiment (there were about 1,000 glides); but the point of view was varied, so as to exhibit the consecutive phases of a single flight. The frog-like appearance of some of the legs is due to the speed.

The first thing which we discovered practically was that the wind flowing up a hillside is not a steadily flowing current like that of a river. It comes as a rolling mass, full of tumultuous whirls and eddies, like those issuing from a chimney; and they strike the apparatus with constantly varying force and direction, sometimes withdrawing support when most needed. It has long been known, through instrumental observations, that the wind is constantly changing in force and direction; but it needed the experience of an operator afloat on a gliding-machine to realize that this all proceeded from cyclonic action; so that more was learned in this respect in a week than had previously been acquired by several years of experiments with models. There was a pair of eagles, living in the top of a dead tree about two miles from our tent, that came almost daily to show us how such wind effects are overcome and utilized. The birds swept in circles overhead on pulseless wings, and rose high up in air. Occasionally there was a side-rocking motion, as of a ship rolling at sea, and then the birds rocked back to an even keel; but although we thought the action was clearly automatic, and were willing to learn, our teachers were too far off to show us just how it was done, and we had to experiment for ourselves.

The operator stands on a hill-side. He raises up the apparatus, which is steadied by a companion, and quickly slips under and within the machine. He faces the wind. This wind buffets the wings from side to side, and up or down, so that he has much difficulty in obtaining a poise. This is finally accomplished by bracing the cross-piece of the machine's frame against his back, and depressing the front edge of the wings so that they will be struck from above by the wind. His arm-pits rest on a pair of horizontal bars, and he grasps a pair of vertical bars with his hands. He is in no way attached to the machine, so that he may disengage himself instantly should anything go wrong. Then, still facing dead into the wind, he takes one or two, never more than four, running steps forward, raising up the front edge of the apparatus at the last moment, and the air claims him. Then he sails forward into the wind on a generally descending course. The "Multiple-Wing" machine was provided with a seat, but, goodness ! there was no time to sit down, as each glide of two to three hundred feet took but eight to twelve seconds, and then it was time to alight. The latter phase of the problem had been the subject of meditation for months, and the conclusion had been reached to imitate the sparrow. When the latter approaches the street, he throws his body back, tilts his outspread wings nearly square to the course, and on the cushion of air thus encountered he stops his speed and drops lightly to the ground. So do all birds. We tried it with misgivings, but found it perfectly effective. The soft sand was a great advantage, and even when the experts were racing there was not a single sprained ankle.

The rebuilt "Multiple-wings" were pivoted at their roots, and vibrated backward and forward on ball-bearings, restrained by rubber springs. As the wind varied, they adjusted themselves thereto, and brought back the supporting air pressure over the operator, thus reestablishing the threatened balance. This was done automatically. But in consequence of various defects in construction and adjustment, the operator still had to move one or two inches, as against the from seven to fifteen inches of movement required by the Lilienthal apparatus. Some two or three hundred glides were made with the "Multiple-wing" without any accident to man or machine, and the action was deemed so effective, the principle so sound, that the full plans were published in the *Aëronautical Annual* for 1897, for the benefit of experimenters desiring to improve on this apparatus.

There is no more delightful sensation than that of gliding through the air. All the faculties are on the alert, and the motion is astonishingly smooth and elastic. The machine responds instantly to the slightest movement of the operator; the air rushes by one's ears; the trees and bushes flit away underneath, and the landing comes all too quickly. Skating, sliding, and bicycling are not to be compared for a moment to aërial conveyance, in which, perhaps, zest is added by the spice of danger. For it must be distinctly understood that there is constant danger in such preliminary experiments. When this hazard has been eliminated by further evolution, gliding will become a most popular sport.

The "Two-surfaced" machine, so-called, produced longer and more numerous glides. There were perhaps 700 or 800, at a rate of descent of about one foot in six; so that while the longest distance traversed was 360 feet, we could have sailed 1,200 feet, had we started from a hill 200 feet high. In consequence of the speed gained by running, the initial stage of the flight is nearly horizontal, and it is thrilling to see the operator pass from thirty to forty feet overhead, steering his machine, undulating his course, and struggling with the wind gusts which whistle through the guy wires. The automatic mechanism restores the angle of advance when compromised by variations of the breeze; but when these come from one side and tilt the apparatus, the weight has to be shifted to right up the machine. This is generally done by thrusting out the feet toward the side which has been raised, a movement which is just the reverse of what would be instinctively made on the ground, but which becomes second nature to an expert. These gusts sometimes raise the machine from ten to twenty feet vertically, and sometimes they strike the apparatus from above, causing it to descend suddenly. When sailing near the ground, these vicissitudes can be counteracted by movements of the body of three or four inches; but this has to be done instantly, for neither wind nor gravity will wait on meditation. At a height of 300 or 400 feet the regulating mechanism would probably take care of these wind gusts, as it does, in fact, for their minor variations. The speed of the machine is generally about seventeen miles an hour over the ground, and from twenty-two to thirty miles an hour relative to the air. Constant effort was directed to keep down the velocity, which was at times fifty-two miles an hour. This is the purpose of the starting and gliding against the wind, which thus furnishes an initial velocity without there being undue speed at the landing. The highest wind we dared to experiment in blew at thirty-one miles an hour; when the wind was stronger, we waited and watched the birds.

There was a gull came fishing over the lake, and took up his station over its very edge, about 100 feet high in air. The wind was blowing a steady gale from the north at sixty-one measured miles an hour. The bird breasted it squarely, and without beat of wing maintained for five minutes his position of observation. Occasionally there was a short rocking motion fore and aft, or from side to side. At times he was raised several feet and drifted backward; at others he drooped down; but he never flapped once. It is evident that he derived from the wind alone all the power required to remain afloat and to perforate the blast without drifting back. Whether man will ever be able to perform this feat, which has been termed "aspiration," is perhaps doubtful, but there is no mistake about the observation.

The only thing we could not ascertain was whether our practice hill, 350 feet to his leeward, produced an ascending trend in the wind about the bird, who was level with its summit.

Another day a curious thing occurred. We had taken one of the machines to the top of the hill, and loaded its lower wings with sand to hold it while we went to lunch. A gull came strolling inland, and flapped full-winged to inspect. He swept several circles above the machine, stretched his neck, gave a squawk, and went off. Presently he returned with eleven other gulls, and they seemed to hold a conclave, about 100 feet above the big new white bird which they had discovered on the sand. They circled round after round, and once in a while there was a series of loud peeps, like those of a rusty gate, as if in conference, with sudden flutterings, as if a terrifying suggestion had been made. The bolder birds occasionally swooped downward to inspect the monster more closely; they twisted their heads around to bring first one eye and then the other to bear, and then they rose again. After some seven or eight minutes of this performance, they evidently concluded either that the stranger was too formidable to tackle, if alive, or that he was not good to eat, if dead, and they flew off to resume fishing, for the weak point about a bird is his stomach.

We did not have the slightest accident to lament during all our experiments. These were chiefly performed by two young, active men, who took turns, and who became expert in a week; but then, we attempted no feats and took no chances. Toward the last, we gained such confidence in the machines that we allowed amateurs to try them under guidance. Half a dozen performed fairly well, but awkwardly of course. One of them was our cook, who was by profession a surgeon, and one was a newspaper reporter who had succeeded in finding his way to the camp. Another was a novice; he was picked up by a wind gust, raised forty feet vertically, and gently set down again. Any young, quick, and handy man can master a gliding-machine almost as soon as a bicycle, but the penalties for mistakes are much more severe. After all, it will be by the cautious, observant man—the man who accepts no risks which he can avoid, perhaps the ultra-timid man—that this hazardous investigation of an art now known only to the birds will be most advanced. Not even the birds could have operated more safely than we; but they would have made longer and flatter glides, and they would have soared up into the blue.

In my judgment, neither of the machines above described is as yet perfected, and I believe it is still premature to apply an artificial motor. This is sure to bring about complications which it is preferable to avoid until the equilibrium has been thoroughly evolved. I, therefore, advise that every plausible method of securing stability and safety shall be tested, that many such experiments shall be made, first with models, and then with full-sized machines, and that their designers shall practice, practice, practice; to make sure of the action, to proportion and adjust the parts, and to eliminate hidden defects. If any feat is attempted, it should be over water, in order to break the fall, should any occur. All this once accomplished, it will be time enough to apply a motor; and it seems not improbable that the gliding-machine will furnish the prototype. This step-by-step process is doubtless slow and costly, but it greatly diminishes the chance of those accidents which bring a whole line of investigation into contempt. We have no reason to believe that, contrary to past experience, a practical flying-machine will be the result of the happy thought of one or of two persons. It will come rather by a process of evolution: one man accomplishing some promising results, but stopping short of success; the next carrying the investigation somewhat further, and thus on, until a machine is produced which will be as practical as the "safety" bicycle, which took some eighty years for its development from the original despised velocipede.

Since the above described experiments were tried, another deplorable accident has come to re-inculcate the necessity for extreme caution. Mr. Percy S. Pilcher, a young, accomplished, and enthusiastic English engineer, lost his life September 30, 1899, while making experiments in soaring with a machine of his own design upon Lilienthal principle. He had already formed hundreds of glides since 1894, and had introduced a method of towing the machine with horses, by means of a long cord with multiplying tackle, so that he could rise from level ground. On this occasion, a first successful flight was made; but on the second trial, after a height of some thirty feet had been gained, a snap was heard, the tail was seen to collapse, and the apparatus dived forward, and fell to the ground, Mr. Pilcher receiving injuries from which he died two days later. He doubtless was the victim of his own amiability, for his apparatus had been wet by a shower, so that the canvass of the tail had shrunk, thus producing undue strains upon the bamboo stretcher, the wind was gusty, and the weather very unfavorable; but as many persons had come from a distance to witness the experiments, Mr. Pilcher did not like to disappoint them, and accepted the undue risks which cost him his life. He was less than thirty-four years of age, a skilful and earnest mechanician, who had already built the oil-engine and screw which he meant to apply to his machine.

Notably enough, he had written to me some eighteen months before for leave to copy and test one of my machines, which leave, with instructions, had, of course, been gladly given. The machine had been built, and was to have been tried on the following day. It is a curious coincidence that Lilienthal is said to have also built a machine, quite original with him, upon the same principle as that above alluded to, and that this also was to have been tested within a day or two of the owner's death. It is idle to speculate on what would have been the result; but then accidents might have happened in my own work, and I am profoundly thankful that we were spared such anguish.

Having been compelled, for the last two years, to give all my time and attention to a practical business, I have been unable to experiment; but I have had an expert testing models of a third method of securing automatic stability, which I hope to experiment full-sized.

Aside from the more imaginative and eccentric inventors, there are now a number of scientific investigators who are working to bring about the solution of this difficult problem; and it is not at all improbable that some experimenter will succeed, within a year or so, in making a flight of something like a mile with a motor. This is now fairly feasible, and there are several inventors who are preparing to attempt it. But between this achievement and its extension to a journey, or even to its indefinite repetition, there will intervene many accidents. Nor is there a fortune to be made by the first successful man. Experimenters who wish to advance the final solution of the quest surely and safely must work without expectation of other reward than that of being remembered hereafter; for, in the usual course of such things, it will be the manufacturers who will reap the pecuniary benefits when commercial flying-machines are finally evolved. There will probably be two types of these, one of them a machine for sport, with a very light and simple motor, if any, carrying but a single operator, and deriving most of its power from wind and gravity, as do the soaring birds. This will be used in competitions of skill and speed, and there will be no finer or more exciting sport. The other future machine will probably be of a journeying type. It will be provided with a powerful, but light, motor and with fuel for one or two days' travel. It will preferably carry but a single man, and will be utilized in exploration and in war. Its speed will be from thirty to sixty miles an hour at the beginning, and eventually much greater, for it is a singular fact that the higher speeds require less power in the air, within certain limits, than low speeds. At high velocities, the surfaces may be smaller, lie at flatter angles, and offer less resistance, but the pressure then increases on the framework, and the ultimate speed may not be more than 80 or 100 miles an hour.

Neither of these machines seems likely to compete with existing modes of transportation. But be this as it may, every improvement in transportation, whether in cheapness, in comfort, or in speed, soon develops new and sometimes unexpected uses of its own; so, even with sober anticipation of the benefits to be realized, investigators and public spirited men may well afford to advance the solution of a problem which has so warmly appealed to the imagination of men for the past forty or fifty centuries.

Document 1-9(d), Octave Chanute, excerpt from "Aerial Navigation," 1901.

FLYING MACHINES.

Some imaginative investigators have, therefore, resumed the search, antedating by far the invention of the balloon, for the conditions to be observed in devising a practical flying machine in imitation of the birds. Much of past work has been fanciful and crude, but there are now aeronautical societies and technical publications in most countries which promote sound research for both balloons and flying machines, and an international congress on these subjects was held at Paris in September, 1900. This congress had an elaborate programme for papers and discussions. Only abstracts of the proceedings have been published at this time of writing, but as no great advance or discovery was announced, this paper will not be incomplete if it merely recapitulates what was already known.

Attempts at artificial flight date back to the very dawn of history, but such attempts have been impeded by two main obstacles so prohibitive as at times to cause the investigation to be classed with that into the possibility of perpetual motion. These main obstacles are, first, the extreme danger in man's attempts to acquire the art of the birds, or to fly with an apparatus as yet imperfect; and second, the lack of an artificial motor as light, in proportion the power developed, as bird machinery. We know, approximately, that the motor muscles of birds develop such an output of energy that a full horse-power, if such were produced, would weigh but from six to twenty pounds, while our most powerful locomotives, with their tenders, weigh about two hundred pounds to the horse-power. This great gap has been partly closed within the last ten years, and investigators into flying devices are no longer regarded as visionaries.

It was about 1889 that a number of competent men simultaneously, but independently, took up the problem. Apparently the times were ripe, and already the advance since then has been greater than during the preceding three centuries. Langley, in the United States, and Maxim, in Great Britain, had become convinced that the current coefficients used for oblique air reactions were incorrect, and began experiments of their own to ascertain the facts. They both selected plane surfaces for trial, and reached much the same results as to the lifting power and resistance which are to be obtained from the air at various angles of inclination and at various speeds. These proved to be many times greater than those given by the ancient formulæ, and these experimenters determined upon building actual flying apparatus which should be driven by power.

Aside from the general design and the form and arrangement of the supporting surfaces, the important elements to consider are the ratio of those surfaces to the weight and the comparative power and speed required. There has been considerable range in these elements in the various experiments, and these will be described so as to bring out the important points. Langley began producing working models of flying machines in 1891. He tested various arrangements of wings and of surfaces, and successively applied steam, gasoline, and carbonic acid gas motors, and then steam again, struggling also with various methods of launching the apparatus into the air. At last, in May, 1896, one of his machines made two flights of about half a mile each in about one minute and a half, and later, in November, another machine flew once, more than three quarters of a mile. The whole apparatus weighed 30 pounds and spread 70 square feet of supporting surface, or in the ratio of 2 ⅓ square feet to the pound. The steam-engine was of 1 horse-power and weighed 7 pounds, thus sustaining in flight 30 pounds per horse-power, but running down very soon because the water in the boiler was exhausted.

Maxim began on a much larger scale. He undertook the construction of a full-sized flying machine to carry three men, spreading 4000 square feet of supporting surface, and weighing 8000 pounds, thus affording only half a square foot to the pound. It was provided with a compound steam-engine and a boiler of 363 H. P., a marvel of ingenuity and mechanical skill. This apparatus was provided with wheels, and was placed upon a railway of 8 feet gauge, being restrained from premature flight by a pair of outside wooden rails placed above the wheels. Many experiments were made to test the speed required and the lifting effect, during which various mishaps were encountered and repaired. After several years of this study, the machine, in 1894, unexpectedly undertook free flight, by bursting through the upper rails during one of the tests. It flew, perhaps, 300 feet, but steam was at once shut off, and the apparatus alighted and was broken. The damage was repaired, but as the machine sustained only about 28 pounds per horse power, requiring a speed of 36 miles an hour, and as it had been very costly and other business pressed, Mr. Maxim did not resume his experiments. He is understood to be engrossed by his great manufacturing interests in gun and shipbuilding, some of the profits from which may hereafter be invested in another flying machine. This will probably be provided with a petroleum motor, now being experimented with, from which he expects even better results than from the marvelous steam-engine previously built by him, which latter weighed, with its boiler and a condenser, less than 10 pounds per horse-power.

About contemporaneously with Langley and Maxim, Hargrave, in Australia, Phillips, in Great Britain, and Tatin and Ader, in France, besides many others, experimented with flying devices. Hargrave began in 1885, and produced about twenty working models, propelled by clockwork, by rubber, by compressed air, and by steam. His last steam engine weighed about 10 pounds per H. P., but he hopes to improve upon this with a gasoline engine. He employs for his models comparatively very large surfaces, as much as 5 square feet to the pound to be lifted, a proportion which may not be realised in full-sized machines, and hence requires speeds of only 10 miles per hour, with which he has succeeded sustaining 79 pounds to the horse-power. He has invented the new form of kite which bears his name, and designs to suspend a motor and propeller, as well as himself, below a team of these novel cellular arrangements, and to fly through the air by towing the kites.

Phillips has been experimenting a long while, and has reached the conclusion that very narrow wings, somewhat like slats, are the most effective. He produced, in 1893, a machine looking like a Venetian blind on wheels, driven by a steam-engine. With this he is said to have lifted about 72 pounds to the horse-power, at speeds of 28 miles an hour, with surfaces in the proportion of one-third of a square foot to the pound of weight. The stability was, however, so defective that the apparatus, which weighed 402 pounds in all, could not be trusted in free flight, and made only brief skims. Tatin is a veteran experimenter. In 1879 he produced a flying model, driven by compressed air, with which he made many flights and had some breakages. In 1897 he produced, in connection with Dr. Richet, a model weighing 72 pounds, driven by a steam-engine. He obtained a lift of 55 pounds to the horsepower, with speeds of about 40 miles an hour, and surfaces of about 1 square foot to the pound; but the maximum flight was only 460 feet, much inferior to Langley's, and the equilibrium was defective. He says that he believes that he can overcome this defect, which has hitherto brought to grief every power-driven machine which has flown more than thrice, but this remains to be seen.

Ader is a French electrical engineer who has intermittently been engaged in aerial investigations for thirty years. He has built three full-sized machines, one, in 1872, to be driven by manpower, which, of course, was found inadequate, one steam-driven in 1891, at the expense of a banker, which produced indifferent results; and a third at the expense of the French Government, in 1897, which cost half a million francs. This was tested on the Satory field of manœuvers, with the most rigid precautions to guard the secrets of this war engine. The construction reproduced almost servilely the anatomical structure of birds. The surfaces were in the proportion of one-quarter of a square foot to the pound, and the whole apparatus weighed 1100 pounds. It was driven by a steam-engine of 40 H. P., weighing about 7 pounds per H. P., and was provided with screw propellers. The speed required for support was about 50 miles an hour, and the apparatus sustained 27 pounds per H. P., or somewhat less than Maxim's or Langley's.

No data have been published as to the tests, but it is said that wind squalls produced a quick descent, and that further experiments were abandoned. The equilibrium was probably so defective that it was deemed wise by the French Government to spend no more money on the machine. It was shown at the Paris Exhibition last year. All experts who have seen it agree that it is a wonderful piece of mechanical workmanship, that the motor is adequate, and that the wings are capable of sustaining all the weight, notwithstanding all their comparative exiguity.

The men above mentioned are but a tithe, perhaps a hundredth, of those who have been planning and experimenting with power-driven flying machines, but they are here picked out as the men who have accomplished the more notable successes. We now come upon a small group of investigators who believed that it was premature to apply artificial power to a flying machine until the proper arrangements and shape of the supporting surfaces were evolved, and their management in the air worked out by long practice.

First and chief among these was Lilienthal, a German engineer and physicist. During ten or fifteen years he made a series of elaborate experiments upon the best shapes for artificial wings, published a book on the subject, showing the superiority of arched forms, and, about 1891, brought out his first form of gliding machine, with which, after careful training, he was enabled to make many personal flights from hillsides, using gravity as a motive power. He gradually improved upon this with different machines, the last being a double-decker, weighing about 50 pounds, and carrying his own weight of I70 pounds in addition. The surface was 151 feet square, being thus in the proportion of about three-quarters of a square foot to the pound, and with this he made many glides, at angles of descent of about one in six, the maximum distance being 1200 feet, and depending, of course, upon the height from which he started. The timing of the flights showed that about 110 pounds were sustained per horse-power, at speeds of 23 miles an hour. In 1895 Lilienthal applied to his apparatus a carbonic acid gas motor of 2 ½ H. P.; which was found, however, to affect the equilibrium so seriously that it was given up. He resumed gliding, and had, altogether, made about 2000 flights, with only trifling accidents, when, in August, 1896, he was upset in the air by a wind gust, fell, and was killed, to the great loss of aviation, which he would, doubtless, have advanced further.

Lilienthal was imitated by Pilcher, an English engineer, who modified the apparatus and made hundreds of glides between 1895 and 1899. His machine spread eighty-five hundredths of a square foot to the pound, and showed 100 pounds to be supported to the horse-power, obtained from gravity, at speeds of 25 miles an hour. He provided himself with a gasoline motor, but did not get far enough along to apply it. Towards the last he devised a method of starting up from level ground by towing the apparatus with horses, and in one of these experiments, in September, 1899, taking undue risks in order not to disappoint visitors, he was upset in the air and killed.

The writer of this has emulated Lilienthal and Pilcher, and thus far without disaster. He has confined his endeavours wholly to the evolution of automatic stability, making the supporting surfaces movable instead of the man, an arrangement the reverse of that of his predecessors. He has had about 1000 glides made by assistants, with two different types (five machines) without the slightest accident.

The proportion of surfaces was three-quarters of a square foot to the pound, the speed, 22 miles per hour, and the weight sustained, 89 pounds per horse-power. He has been experimenting by proxy with a third type of movable surfaces, which be intends to test full-sized, and he holds that it is entirely premature to introduce an artificial motor.

Full particulars concerning the last three experimenters will be found in the "Aeronautical Annual" for 1896 and 1897.

Since these experiments a further advance has been achieved by Messrs. Wilbur and Orville Wright, who have produced a double decked gliding machine in which the operator is placed in a horizontal position, thus opposing to forward motion 1 square foot, instead of 5 square feet, when he is upright, and they have further reduced the resistance of the framing by adopting improved shapes, so that the aggregate head resistances are reduced to about one-half of those which previously obtained. The experiments were made on the North Carolina coast, in the United States, in October, 1900.

The above is the record of what has been accomplished within the last decade in an investigation heretofore relegated to what had been termed "cranks." While a journey of 1200 miles has been made with a globular balloon, at the will of the wind, trips of only 5 or 6 miles have been made at the will of the operator, in calm weather, with fusiform balloons. With flying machines a maximum flight of three-quarters of a mile has been made by a model provided with a motor, and thousands of glides, up to 1200 feet, have been made by men-ridden machines with the aid of gravity, which latter power imposes no extra weight upon the apparatus and is always in good order.

The maximum speed of the dirigible balloon thus far is about 18 miles an hour, with eventual possibilities up to 44 miles per hour, while the speed of flying machines has already been about 50 miles an hour, with possibilities to 60 or 100 miles an hour, which speeds are attained by some swift birds. While fusiform balloons will, therefore, constitute one solution, and while they will be gradually improved and will serve in war, and perhaps in exploration, it now seems probable that future developments will chiefly appertain to flying machines. To make these a success the two main problems must be worked out; first, the motor, which must be very light, and, second, the stability, which is even more important, and which should be automatic.

To appreciate the difficulties appertaining to the motor, we may consider the difference in amount of power required for land and for aerial transportation. An American "Consolidation" locomotive will develop about 1000 H. P. and haul 2000 tons upon a level railway. Hence, it will haul 4000 pounds per horse-power. But this 1000 H. P., weighing, say, 100 tons with its tender, could impart a speed of only 57 miles an hour to Count Zeppelin's air ship, were this vessel (a manifest absurdity) able to lift the engine and to bear the resulting air pressure due to the speed. Neither could the locomotive sustain itself in the air if attached to a weightless flying machine.

We have seen that, with motor-driven apparatus, the best that has been positively done thus far has been to sustain from 27 to 55 pounds per horse-power by impact upon the air. Gliding machines, it is true, using gravity as a motive power, show 89 to 110 pounds sustained per horse-power, but these figures must be considerably reduced when an artificial motor is substituted, in order to cover the inevitable mechanical losses in the machinery and in the propeller. Much has been done within the past decade towards reducing the weight of motors. Steam-engines have been produced weighing but 10 pounds per horse-power, and the latest gasoline motor, that of Buchet, is said to weigh only 12 ½ per H. P.; but much remains to be done to render machines working so nearly up to the limit of endurance absolutely reliable and safe in the air. Numerous and costly experiments are required to accomplish this. It now seems probable that the successful aerial motor will be some form of gasoline engine, using air instead of water as a working fluid, and thus saving weight. But those who know how tedious and slow has been the development of the steam-engine will have no very sanguine expectations of the early attaining of perfection in the gasoline motor.

It is still more imperative that the whole apparatus shall not fall by losing its balance in the air. It must maintain its equilibrium and be reasonable safe under all the vicissitudes of flight,—in starting, in sailing, in alighting, and in wind gusts. The bare statement of this requirement meets with ready assent, and yet how few of the investigators have the will or the patience to spend the time, and to take the risk, to learn the art of the birds by personal experiments with gliding machines. The writer has been advocating this method for some years, he has confined his researches to its advance, and he sees no reason to change his views.

The underlying principle of maintaining equilibrium in the air is that the centre of pressure upon the sustaining surfaces shall at all times be upon the same vertical line as the centre of gravity due to the weight of the apparatus. In calm air this is fairly secured, but in a wind the centre of pressure is constantly shifted by the turmoils of the air, for it advances or recedes with the diminution or increase of the angle of incidence. There are several ways of counteracting this difficulty. The centre of gravity may be shifted back or forward to coincide again with the vertical line passing through the new centre of pressure; this is the method employed by Lilienthal and by Pilcher, which they applied by shifting the position of their personal weight. Or the centre of pressure may be brought back into a vertical line with a fixed centre of gravity, either by changing the angle of incidence, or by shifting the surfaces themselves. These latter are the methods which have been experimented upon by the writer in three different ways:—

1st. Affixing a horizontal tail (the Pénaud tail) at an angle to the supporting surfaces. This catches the air on its upper or lower surface, and shifts the angle of incidence of the wings, and, consequently, the centre of pressure.

2d. Pivoting the wings at their roots, so as to move horizontally. These are arranged so that the impact of the air shall bring them back into the proper position.

3d. Pivoting the surfaces so as to rock vertically. This is arranged so that the impinging air shall automatically shift the angle of incidence, and, therefore, the centre of pressure.

The third arrangement is believed to be the best, but one cannot be sure, inasmuch as all the adjustments above indicated are most delicate. Simple as the principles seem to be, it requires years of experiment to apply them properly. The positions of the pivots, the strength and adjustments of restraining springs, and the best position for the centre of gravity involve thousands of cut-and-try experiments, first with models, and then with full-sized gliding machines carrying a man. The important feature is that the man shall remain stationary.

In regard to the shape of surfaces to be employed, Lilienthal demonstrated that concavo-convex wings, like those of birds, are far superior in supporting power to planes, and the latter have now been practically abandoned by aviation experts. The amount of sustaining surfaces experimented with has varied, as above mentioned, from 1/4 square foot to 5 square feet to the pound of weight, this corresponding to speeds of from 50 miles down to 10 miles an hour, to obtain support. The amount required evidently depends upon the speed, but the larger the surfaces the greater is the weight. In the remote future it is probable that small surfaces will obtain, thus reducing the amount of required framework and the consequent head resistance, but until the problem of equilibrium has been fully solved it will be preferable to employ surfaces of about 1 square foot to the pound, involving speeds of 20 to 25 miles an hour in order to promote safety in alighting. With smaller surfaces we may hope to sustain eventually as much as 80 pounds per indicated horse-power, but something will depend upon the efficiency of the propeller.

The propeller is the next thing to be considered after the equilibrium has been secured and a reliable motor worked out. Both Hargrave and Lilienthal gave preference to flapping vanes over the screw propeller, but other experimenters prefer screws. It is yet too soon to draw definite conclusions on this question, and it opens a field for further experimenting.

We can, however, already calculate approximately the proportions, the strength and weight, the supporting efficiency, the speed, and the power required for a projected flying machine, so as to judge of the practicability of a design. Indeed, the mathematics of the subject have been so far evolved that engineering computations may eventually replace vague speculation in the domain of aerial navigation.

But after the problem has been worked out to a mechanical success, the commercial uses of aerial apparatus will be small. The limitations of the balloon have already been mentioned; such craft will be slow, frail, and very costly. We are now sufficiently advanced in the design of flying machines to perceive some of their limitations. They will be comparatively small and cranky, require much power, carry little extra weight, and depend for their effective speed, on each journey, whether they go against the wind or with it, so that they cannot compete with existing modes of transportation in cheapness or in carrying capacity. It is true that high speeds may be attained, and this may serve in war, in exploration, perhaps in mail transportation, and in sport; but the loads will be very small, and the expenses will be great. But flying machines will develop new uses of their own; and as mankind has always been benefited by the introduction of new and faster modes of transportation, we may hope that successful aerial navigation will spread civilization, knit the nations closer together, make all regions accessible, and perhaps so equalize the hazards of war as to abolish it altogether, thus bringing about the predicted era of universal peace and good-will.

Document 1-10(a–c)

(a) Orville Wright, excerpt from deposition of 13 January 1920, *The Papers of Wilbur and Orville Wright*, Marvin W. McFarland, ed. (New York, NY: McGraw-Hill Book Company, 1953), p. 3.

(b) Wilbur Wright, letter to the Smithsonian Institution, 30 May 1899, *The Papers of Wilbur and Orville Wright*, pp. 4–5.

(c) Wilbur Wright, letter to Octave Chanute, 13 May 1900, *The Papers of Wilbur and Orville Wright*, pp. 15–19.

The first document includes a response by Orville Wright, on 13 January 1920, to the question, "When and under what circumstances did you and Wilbur Wright first become interested in the problem of flight?" His brief statement sets up the two letters that follow with comments on how the Wrights were interested in flight even as children and how as young adults they began to tackle the problems of heavier-than-air flight, by first writing to the Smithsonian Institution for information.

The second and third documents are two letters written by Wilbur Wright that express his and his brother's early interest and commitment to flight research. The first of these letters was written in 1899 to the Smithsonian Institution, where Professor Langley was working, to request articles on aeronautics. In response to it, Richard Rathbun, the Assistant Secretary of the Smithsonian Institute, sent the Wrights four pamphlets: *Empire of the Air* (1893) by Louis-Pierre Mouillard; *The Problem of Flying and Practical Experiments in Soaring* (1893) by Otto Lilienthal; *Story of Experiments in Mechanical Flight* (1897) by Samuel P. Langley; and *On Soaring Flight* (1897) by E.C. Huffaker. Rathbun's letter of response also referred the Wrights to a list of other works, including *Progress in Flying Machines* (1894) by Octave Chanute and *Experiments in Aerodynamics* (1891) by Samuel P. Langley, along with the 1895, 1896, and 1897 issues of *The Aeronautical Annual*. The second was a 1900 letter to Octave Chanute. The search for an appropriate location to test their designs mentioned in the letter to Chanute led the Wrights to the Outer Banks of North Carolina and the winds of Kitty Hawk.

Document 1-10(a), Orville Wright on the brothers' interest in flight, 1920.

Our first interest began when we were children. Father brought home to us a small toy actuated by a rubber spring which would lift itself into the air. We built a number of copies of this toy, which flew successfully. By "we" I refer to my brother Wilbur and myself. But when we undertook to build the toy on a much larger scale it failed to work so well. The reason for this was not understood by us at the time, so we finally abandoned the experiments. In 1896 we read in the daily papers, or in some of the magazines, of the experiments of Otto Lilienthal, who was making some gliding flights from the top of a small hill in Germany. His death a few months later while making a glide off the hill increased our interest in the subject, and we began looking for books pertaining to flight. We found a work written by Professor Marey on animal mechanism which treated of the bird mechanism as applied to flight, but other than this, so far as I can remember, we found little.

In the spring of the year 1899 our interest in the subject was again aroused through the reading of a book on ornithology. We could not understand that there was anything about a bird that would enable it to fly that could not be built on a larger scale and used by man. At this time our thought pertained more particularly to gliding flight and soaring. If the bird's wings would sustain it in the air without the use of any muscular effort, we did not see why man could not be sustained by the same means. We knew that the Smithsonian Institution had been interested in some work on the problem of flight, and, accordingly, on the 30th of May 1899, my brother Wilbur wrote a letter to the Smithsonian inquiring about publications on the subject. . . .

Document 1-10(b), letter by Wilbur Wright to the Smithsonian Institution from Dayton, Ohio, 30 May 1899.

I have been interested in the problem of mechanical and human flight ever since as a boy I constructed a number of bats of various sizes after the style of Cayley's and Pénaud's machines. My observations since have only convinced me more firmly that human flight is possible and practicable. It is only a question of knowledge and skill just as in all acrobatic feats. Birds are the most perfectly trained gymnasts in the world and are specially well fitted for their work, and it may be that man will never equal them, but no one who has watched a bird chasing an insect or another bird can doubt that feats are performed which require three or four times the effort required in ordinary flight. I believe that simple flight at least is possible to man and that the experiments and investigations of a large number of independent workers will result in the accumulation of information and knowledge and skill which will finally lead to accomplished flight.

The works on the subject to which I have had access are Marey's and Jamieson's books published by Appleton's and various magazine and cyclopaedic articles. I am about to begin a systematic study of the subject in preparation for practical work to which I expect to devote what time I can spare from my regular business. I wish to obtain such papers as the Smithsonian Institution has published on this subject, and if possible a list of other works in print in the English language. I am an enthusiast, but not a crank in the sense that I have some pet theories as to the proper construction of a flying machine. I wish to avail myself of all that is already known and then if possible add my mite to help on the future worker who will attain final success. I do not know the terms on which you send out your publications but if you will inform me of the cost I will remit the price.

Document 1-10(c), letter from Wilbur Wright to Octave Chanute, 13 May 1900.

For some years I have been afflicted with the belief that flight is possible to man. My disease has increased in severity and I feel that it will soon cost me an increased amount of money if not my life. I have been trying to arrange my affairs in such a way that I can devote my entire time for a few months to experiment in this field.

My general ideas of the subject are similar to those held by most practical experimenters, to wit: that what is chiefly needed is skill rather than machinery. The flight of the buzzard and similar sailers is a convincing demonstration of the value of skill, and the partial needlessness of motors. It is possible to fly without motors, but not without knowledge & skill. This I conceive to be fortunate, for man, by reason of his greater intellect, can more reasonably hope to equal birds in knowledge, than to equal nature in the perfection of her machinery.

Assuming then that Lilienthal was correct in his ideas of the principles on which man should proceed, I conceive that his failure was due chiefly to the inadequacy of his method, and of his apparatus. As to his method, the fact that in five years time he spent only about five hours, altogether, in actual flight is sufficient to show that his method was inadequate. Even the simplest intellectual or acrobatic feats could never be learned with so short practice, and even Methuselah could never have become an expert stenographer with one hour per year for practice. I also conceive Lilienthal's apparatus to be inadequate not only from the fact that he failed, but my observations of the flight of birds convince me that birds use more positive and energetic methods of regaining equilibrium than that of shifting the center of gravity.

With this general statement of my principles and belief I will proceed to describe the plan and apparatus it is my intention to test. In explaining these, my object is to learn to what extent similar plans have been tested and found to be failures, and also to obtain such suggestions as your great knowledge and experience might enable you to give me. I make no secret of my plans for the reason that I believe no financial profit will accrue to the inventor of the first flying machine, and that only those who are willing to give as well as to receive suggestions can hope to link their names with the honor of its discovery. The problem is too great for one man alone and unaided to solve in secret.

My plan then is this. I shall in a suitable locality erect a light tower about one hundred and fifty feet high. A rope passing over a pulley at the top will serve as a sort of kite string. It will be so counterbalanced that when the rope is drawn out one hundred & fifty feet it will sustain a pull equal to the weight of the operator and apparatus or nearly so. The wind will blow the machine out from the base of the tower and the weight will be sustained partly by the upward pull of the rope and partly by the lift of the wind. The counterbalance will be so arranged that the pull decreases as the line becomes shorter and ceases entirely when its length has been decreased to one hundred feet. The aim will be to eventually practice in a wind capable of sustaining the operator at a height equal to the top of the tower. The pull of the rope will take the place of a motor in counteracting drift. I see, of course, that the pull of the rope will introduce complications which are not met in free flight, but if the plan will only enable me to remain in the air for practice by the hour instead of by the second, I hope to acquire skill sufficient to overcome both these difficulties and those inherent to flight. Knowledge and skill in handling the machine are absolute essentials to flight and it is impossible to obtain them without extensive practice. The method employed by Mr. Pilcher of towing with horses in many respects is better than that I propose to employ, but offers no guarantee that the experimenter will escape accident long enough to acquire skill sufficient to prevent accident. In my plan I rely on the rope and counterbalance to at least break the force of a fall. My observation of the flight of buzzards leads me to believe that they regain their lateral balance, when partly overturned by a gust of wind, by a torsion of the tips of the wings. If the rear edge of the right wing tip is twisted upward and the left downward the bird becomes an animated windmill and instantly begins to turn, a line from its head to its tail being the axis. It thus regains its level even if thrown on its beam ends, so to speak, as I have frequently seen them. I think the bird also in general retains its lateral equilibrium, partly by presenting its two wings at different angles to the wind, and partly by drawing in one wing, thus reducing its area. I incline to the belief that the first is the more important and usual method. In the apparatus I intend to employ I make use of the torsion principle. In appearance it is very similar to the "double-deck" machine with which the experiments of yourself and Mr. Herring were conducted in 1896-7. The point on which it differs in principle is that the cross-stays which prevent the upper plane from moving forward and backward are removed, and each end of the upper plane is independently moved forward or backward with respect to the lower plane by a suitable lever or other arrangement. By this plan the whole upper plane may be moved forward or backward, to attain longitudinal equilibrium, by moving both hands forward or backward together. Lateral equilibrium is gained by moving one end more than the other or by moving them in opposite directions. If you will make a square cardboard tube two inches in diameter and eight or ten long and choose two sides for your planes you will at once see the torsional effect of moving one end of the upper plane forward and the other backward, and how this effect is attained without sacrificing lateral stiffness. My plan is to attach the tail rigidly to the rear upright stays which connect the planes, the effect of which will be that when the upper plane is thrown forward the end of the tail is elevated, so that the tail assists gravity in restoring longitudinal balance. My experiments hitherto with this apparatus have been confined to machines spreading about fifteen square feet of surface, and have been sufficiently encouraging to induce me to lay plans for a trial with [a] full-sized machine.

My business requires that my experimental work be confined to the months between September and January and I would be particularly thankful for advice as to a suitable locality where I could depend on winds of about fifteen miles per hour without rain or too inclement weather. I am certain that such localities are rare.

I have your *Progress in Flying Machines* and your articles in the *Annuals* of '95, '96, & '97, as also your recent articles in the *Independent*. If you can give me information as to where an account of Pilcher's experiments can be obtained I would greatly appreciate your kindness.

Document 1-11(a–b)

(a) Wilbur Wright, letter to Bishop Milton Wright from Kitty Hawk, North Carolina, 23 September 1900, *The Papers of Wilbur and Orville Wright*, pp. 25–27.

(b) Wilbur Wright, "Some Aeronautical Experiments," lecture delivered at the meeting of the Western Society of Engineers, Chicago, Illinois, 18 September 1901, *The Papers of Wilbur and Orville Wright*, pp. 99–118.

The following two documents by Wilbur Wright present different perspectives on the Wright brothers' first gliding experiments at Kitty Hawk, North Carolina. The first is an excerpt from a letter written to his father at the beginning of their first season of flight tests there in 1900. The second is a lecture given in Chicago before the Western Society of Engineers after the conclusion of their second season in 1901. Preceding Wilbur Wright's lecture was an introduction by Octave Chanute, who hosted Wilbur during his stay in Chicago.

Chanute played a critical role in boosting the morale of the Wrights, especially Wilbur's declining confidence, after the many frustrations and disappointments of their 1900 and 1901 gliding seasons at Kitty Hawk. While a guest in Chanute's home (which was "cluttered up" with "models of flying machines suspended from the ceiling so thick that you could not see any ceiling at all"), Wilbur had long talks with the veteran engineer, many of them about the merits of Lilienthal's airfoil data. In his Chicago speech, Wilbur had not questioned the validity of any of Lilienthal's data, but he would start doing so soon thereafter, a change of mind reflected in how he edited his speech for subsequent publication. When he returned to Dayton, Wilbur poured over all available data with his brother Orville, including the poor performance of their own wings. They determined that they would need to start over from scratch, building from an experimental research program of their own. This decision led them to construct a makeshift wind tunnel in which they systematically tested a whole new range of airfoil shapes, some 200 of them. From this test program they attained the aerodynamic data they needed to design their highly successful glider of 1902 and the historic 1903 *Flyer* that resulted from it.

Document 1-11(a), letter from Wilbur Wright to Milton Wright, 23 September 1900.

I have my machine nearly finished. It is not to have a motor and is not expected to fly in any true sense of the word. My idea is merely to experiment and practice with a view to solving the problem of equilibrium. I have plans which I hope to find much in advance of the methods tried by previous experimenters. When once a machine is under proper control under all conditions, the motor problem will be quickly solved. A failure of motor will then mean simply a slow descent & safe landing instead of a disastrous fall. In my experiments I do not expect to rise many feet from the ground, and in case I am upset there is nothing but soft sand to strike on. I do not intend to take dangerous chances, both because I have no wish to get hurt and because a fall would stop my experimenting, which I would not like at all. The man who wishes to keep at the problem long enough to really learn anything positively must not take dangerous risks. Carelessness and overconfidence are usually more dangerous than deliberately accepted risks. I am constructing my machine to sustain about five times my weight and am testing every piece. I think there is no possible chance of its breaking while in the air. If it is broken it will be by awkward landing. My machine will be trussed like a bridge and will be much stronger than that of Lilienthal, which, by the way, was upset through the failure of a movable tail and not by breakage of the machine. The tail of my machine is fixed, and even if my steering arrangement should fail, it would still leave me with the same control that Lilienthal had at the best. The safe and secure construction & management are my main improvements. My machine is more simple in construction and at the same time capable of greater adjustment and control than previous machines.

I have not taken up the problem with the expectation of financial profit. Neither do I have any strong expectation of achieving the solution at the present time or possibly any time. My trip would be no great disappointment if I accomplish practically nothing. I look upon it as a pleasure trip pure and simple, and I know of no trip from which I could expect greater pleasure at the same cost. I am watching my health very closely and expect to return home heavier and stronger than I left. I am taking every precaution about my drinking water.

Document 1-11(b), Wilbur Wright, "Some Aeronautical Experiments," Chicago, 18 September 1901.

The difficulties which obstruct the pathway to success in flying machine construction are of three general classes: (1) Those which relate to the construction of the sustaining wings. (2) Those which relate to the generation and application of the power required to drive the machine through the air. (3) Those relating to the balancing and steering of the machine after it is actually in flight. Of these difficulties two are already to a certain extent solved. Men already know how to construct wings or aeroplanes, which when driven through the air at sufficient speed, will not only sustain the weight of the wings themselves, but also that of the engine, and of the engineer as well. Men also know how to build engines and screws of sufficient lightness and power to drive these planes at sustaining speed. As long ago as 1893 a machine weighing 8,000 lbs. demonstrated its power both to lift itself from the ground and to maintain a speed of from thirty to forty miles per hour; but it came to grief in an accidental free flight, owing to the inability of the operators to balance and steer it properly. This inability to balance and steer still confronts students of the flying problem, although nearly ten years have passed. When this one feature has been worked out the age of flying machines will have arrived, for all other difficulties are of minor importance.

The person who merely watches the flight of a bird gathers the impression that the bird has nothing to think of but the flapping of its wings. As a matter of fact this is a very small part of its mental labor. To even mention all the things the bird must constantly keep in mind in order to fly securely through the air would take a considerable part of the evening. If I take this piece of paper, and after placing it parallel with the ground, quickly let it fall, it will not settle steadily down as a staid, sensible piece of paper ought to do, but it insists on contravening every recognized rule of decorum, turning over and darting hither and thither in the most erratic manner, much after the style of an untrained horse. Yet this is the style of steed that men must learn to manage before flying can become an everyday sport. The bird has learned this art of equilibrium, and learned it so thoroughly that its skill is not apparent to our sight. We only learn to appreciate it when we try to imitate it. Now, there are two ways of learning how to ride a fractious horse: one is to get on him and learn by actual practice how each motion and trick may be best met; the other is to sit on a fence and watch the beast a while, and then retire to the house and at leisure figure out the best way of overcoming his jumps and kicks. The latter system is the safest; but the former, on the whole, turns out the larger proportion of good riders. It is very much the same in learning to ride a flying machine; if you are looking for perfect safety, you will do well to sit on a fence and watch the birds; but if you really wish to learn, you must mount a machine and become acquainted with its tricks by actual trial.

Herr Otto Lilienthal seems to have been the first man who really comprehended that balancing was the *first* instead of the *last* of the great problems in connection with human flight. He began where others left off, and thus saved the many thousands of dollars that it had theretofore been customary to spend in building and fitting expensive engines to machines which were uncontrollable when tried. He built a pair of wings of a size suitable to sustain his own weight, and made use of gravity as his motor. This motor not only cost him nothing to begin with, but it required no expensive fuel while in operation, and never had to be sent to the shop for repairs. It had one serious drawback, however, in that it always insisted on fixing the conditions under which it would work. These were that the man should first betake himself and machine to the top of a hill and fly with a downward as well as a forward motion. Unless these conditions were complied with, gravity served no better than a balky horse—it would not work at all. Although Lilienthal must have thought the conditions were rather hard, he nevertheless accepted them till something better should turn up; and in this manner he made some two thousand flights, in a few cases landing at a point more than a thousand feet distant from his place of starting. Other men, no doubt, long before had thought of trying such a plan. Lilienthal not only thought, but acted; and in so doing probably made the greatest contribution to the solution of the flying problem that has ever been made by any one man. He demonstrated the feasibility of actual practice in the air, without which success is impossible. Herr Lilienthal was followed by Mr. Pilcher, a young English engineer, and by Mr. Chanute, a distinguished member of the society I now address. A few others have built machines, but nearly all that is of real value is due to the experiments conducted under the direction of the three men just mentioned.

The balancing of a gliding or flying machine is very simple in theory. It merely consists in causing the center of pressure to coincide with the center of gravity. But in actual practice there seems to be an almost boundless incompatibility of temper which prevents their remaining peaceably together for a single instant, so that the operator, who in this case acts as peacemaker, often suffers injury to himself while attempting to bring them together. If a wind strikes a vertical plane, the pressure on that part to one side of the center will exactly balance that on the other side, and the part above the center will balance that below. This point we call the center of pressure. But if the plane be slightly inclined, the pressure on the part nearest the wind is increased, and the pressure on the other part decreased, so that the center of pressure is now located, not in the center of the surface, but a little toward the side which is in advance. If the plane be still further inclined the center of pressure will move still farther forward. And if the wind blow a little to one side, it will also move over as if to meet it. Now, since neither the wind nor the machine for even an instant maintains exactly the same direction and velocity, it is evident that the man who would trace the course of the center of pressure must be very quick of mind; and he who would attempt to move his body to that spot at every change must be very active indeed. Yet this is what Herr Lilienthal attempted to do, and did do with most remarkable skill, as his two thousand glides sufficiently attest. However he did not escape being overturned by wind gusts several times, and finally lost his life through a breakage of his machine, due to defective construction.

The Pilcher machine was similar to that of Lilienthal, and like it, seems to have been structurally weak; for on one occasion, while exhibiting the flight of his machine to several members of the Aeronautical Society of Great Britain, it suddenly collapsed and fell to the ground, causing injuries to the operator which proved sadly fatal. The method of management of this machine differed in no important respect from that of Lilienthal, the operator shifting his body to make the centers of pressure and gravity coincide. Although the fatalities which befell the designers of these machines were due to the lack of structural strength, rather than to lack of control, nevertheless it had become clear to the students of the problem that a more perfect method of control must be evolved. The Chanute machines marked a great advance in both respects. In the multiple-wing machine, the tips folded slightly backward under the pressure of wind gusts, so that the travel of the center of pressure was thus largely counterbalanced. The guiding of the machine was done by a slight movement of the operator's body toward the direction in which it was desired that the machine should go. The double-deck machine built and tried at the same time marked a very great structural advance, as it was the first in which the principles of the modern truss bridges were fully applied to flying machine construction. This machine in addition to its greatly improved construction and general design of parts also differed from the machine of Lilienthal in the operation of its tail. In the Lilienthal machine the tail, instead of being fixed in one position, was prevented by a stop from folding downward beyond a certain point, but was free to fold upward without any hindrance. In the Chanute machine the tail was at first rigid, but afterward, at the suggestion of Mr. Herring, it was held in place by a spring that allowed it to move slightly either upward or downward with reference to its normal position, thus modifying the action of the wind gusts upon it, very much to its advantage. The guiding of the machine was effected by slight movements of the operator's body, as in the multiple-wing machines. Both these machines were much more manageable than the Lilienthal type, and their structural strength, notwithstanding their extreme lightness, was such that no fatalities, or even accidents, marked the glides made with them, although winds were successfully encountered much greater in violence than any which previous experimenters had dared to attempt.

My own active interest in aeronautical problems dates back to the death of Lilienthal in 1896. The brief notice of his death which appeared in the telegraphic news at that time aroused a passive interest which had existed from my childhood, and led me to take down from the shelves of our home library a book on *Animal Mechanism* by Prof. Marey, which I had already read several times. From this I was led to read more modern works, and as my brother soon became equally interested with myself, we soon passed from the reading to the thinking, and finally to the working stage. It seemed to us that the main reason why the problem had remained so long unsolved was that no one had been able to obtain any adequate practice. We figured that Lilienthal in five years of time had spent only about five hours in actual gliding through the air. The wonder was not that he had done so little, but that he had accomplished so much. It would not be considered at all safe for a bicycle rider to attempt to ride through a crowded city street after only five hours' practice, spread out in bits of ten seconds each over a period of five years; yet Lilienthal with this brief practice was remarkably successful in meeting the fluctuations and eddies of wind gusts. We thought that if some method could be found by which it would be possible to practice by the hour instead of by the second, there would be hope of advancing the solution of a very difficult problem. It seemed feasible to do this by building a machine which would be sustained at a speed of 18 miles per hour, and then finding a locality where winds of this velocity were common. With these conditions, a rope attached to the machine to keep it from floating backward would answer very nearly the same purpose as a propeller driven by a motor, and it would be possible to practice by the hour, and without any serious danger, as it would not be necessary to rise far from the ground, and the machine would not have any forward motion at all. We found, according to the accepted tables of air pressures on curved surfaces that a machine spreading 200 square feet of wing surface would be sufficient for our purpose, and that places could easily be found along the Atlantic coast where winds of 16 to 25 miles were not at all uncommon. When the winds were low, it was our plan to glide from the tops of sand hills, and when they were sufficiently strong, to use a rope for our motor and fly over one spot. Our next work was to draw up the plans for a suitable machine. After much study we finally concluded that tails were a source of trouble rather than of assistance; and therefore we decided to dispense with them altogether. It seemed reasonable that if the body of the operator could be placed in a horizontal position instead of the upright, as in the machines of Lilienthal, Pilcher and Chanute, the wind resistance could be very materially reduced since only one square foot instead of five would be exposed. As a full half horsepower could be saved by this change, we arranged to try at least the horizontal position. Then the method of control used by Lilienthal, which consisted in shifting the body, did not seem quite as quick or effective as the case required; so, after long study, we contrived a system consisting of two large surfaces on the Chanute double-deck plan, and a smaller surface placed a short distance in front of the main surfaces in such a position that the action of the wind upon it would counterbalance the effect of the travel of the center of pressure on the main surfaces. Thus changes in the direction and velocity of the wind would have little disturbing effect, and the operator would be required to attend only to the steering of the machine, which was to be affected by curving the forward surface up or down. The lateral equilibrium and the steering to right or left was to be attained by a peculiar torsion of the main surfaces, which was equivalent to presenting one end of the wings at a greater angle than the other. In the main frame a few changes were also made in the details of construction and trussing employed by Mr. Chanute. The most important of these were: (1) the moving of the forward main cross-piece of the frame to the extreme front edge; (2) the encasing in the cloth of all cross-pieces and ribs of the surfaces; (3) a rearrangement of the wires used in trussing the two surfaces together, which rendered it possible to tighten all the wires by simply shortening two of them.

With these plans we proceeded in the summer of 1900 to Kitty Hawk, North Carolina, a little settlement located on the strip of land that separates Albemarle Sound from the Atlantic Ocean. Owing to the impossibility of obtaining suitable material for a 200 square-foot machine, we were compelled to make it only 165 square feet in area, which, according to the Lilienthal tables, would be supported at an angle of three degrees in a wind of about 21 miles per hour. On the very day that the machine was completed the wind blew from 25 to 30 miles per hour, and we took it out for trial as a kite. We found that while it was supported with a man on it in a wind of about 25 miles, its angle was much nearer twenty degrees than three degrees. Even in gusts of 30 miles the angles of incidence did not get as low as three degrees, although the wind at this speed has more than twice the lifting power of a 21-mile wind. As winds of 30 miles per hour are not plentiful on clear days, it was at once evident that our plan of practicing by the hour, day after day, would have to be postponed. Our system of twisting the surfaces to regulate the lateral balance was tried and found to be much more effective than shifting the operator's body. On subsequent days, when the wind was too light to support the machine with a man on it, we tested it as a kite, working the rudders by cords reaching to the ground. The results were very satisfactory, yet we were well aware that this method of testing is never wholly convincing until the results are confirmed by actual gliding experience.

We then turned our attention to making a series of actual measurements of the lift and drift of the machine under various loads. So far as we were aware this had never previously been done with any full-size machine. The results obtained were most astonishing, for it appeared that the total horizontal pull of the machine, while sustaining a weight of 52 pounds, was only 8.5 lbs., which was less than had previously been estimated for head resistance of the framing alone. Making allowance for the weight carried, it appeared that the head resistance of the framing was but little more than 50 percent of the amount which Mr. Chanute had estimated as the head resistance of the framing of his machine. On the other hand it appeared sadly deficient in lifting power as compared with the calculated lift of curved surfaces of its size. This deficiency we supposed might be due to one or more of the following causes: (1) That the depth of the curvature of our surfaces was insufficient, being only about 1 in 22, instead of 1 in 12. (2) That the cloth used in our wings was not sufficiently airtight. (3) That the Lilienthal tables might themselves be somewhat in error. We decided to arrange our machine for the following year so that the depth of curvature of its surfaces could be varied at will, and its covering airproofed.

Our attention was next turned to gliding, but no hill suitable for the purpose could be found near our camp at Kitty Hawk. This compelled us to take the machine to a point 4 miles south, where the Kill Devil sand hill rises from the flat sand to a height of more than 100 feet. Its main slope is toward the northeast, and has an inclination of 10 degrees. On the day of our arrival the wind blew about 25 miles an hour, and as we had had no experience at all in gliding, we deemed it unsafe to attempt to leave the ground. But on the day following, the wind having subsided to 14 miles per hour, we made about a dozen glides. It had been the original intention that the operator should run with the machine to obtain initial velocity, and assume the horizontal position only after the machine was in free flight. When it came time to land he was to resume the upright position and light on his feet, after the style of previous gliding experimenters. But on actual trial we found it much better to employ the help of two assistants in starting, which the peculiar form of our machine enabled us readily to do; and in landing we found that it was entirely practicable to land while still reclining in a horizontal position upon the machine. Although the landings were made while moving at speeds of more than 20 miles an hour, neither machine nor operator suffered any injury. The slope of the hill was 9.5 deg., or a drop of 1 foot in 6. We found that after attaining a speed of about 25 or 30 miles with reference to the wind, or 10 to 15 miles over the ground, the machine not only glided parallel to the slope of the hill, but greatly increased its speed, thus indicating its ability to glide on a somewhat less angle than 9.5 deg., when we should feel it safe to rise higher from the surface. The control of the machine proved even better than we had dared to expect, responding quickly to the slightest motion of the rudder. With these glides our experiments for the year 1900 closed. Although the hours and hours of practice we had hoped to obtain finally dwindled down to about two minutes, we were very much pleased with the general results of the trip, for setting out as we did, with almost revolutionary theories on many points, and an entirely untried form of machine, we considered it quite a point to be able to return without having our pet theories completely knocked in the head by the hard logic of experience, and our own brains dashed out in the bargain. Everything seemed to us to confirm the correctness of our original opinions: (1) that practice is the key to the secret of flying; (2) that it is practicable to assume the horizontal position; (3) that a smaller surface set at a negative angle in front of the main bearing surfaces, or wings, will largely counteract the effect of the fore and aft travel of the center of pressure; (4)

that steering up and down can be attained with a rudder, without moving the position of the operator's body; (5) that twisting the wings so as to present their ends to the wind at different angles is a more prompt and efficient way of maintaining lateral equilibrium than shifting the body of the operator.

When the time came to design our new machine for 1901, we decided to make it exactly like the previous machine in theory and method of operation. But as the former machine was not able to support the weight of the operator when flown as a kite, except in very high winds and at very large angles of incidence, we decided to increase its lifting power. Accordingly, the curvature of the surfaces was increased to 1 in 12, to conform to the shape on which Lilienthal's table was based, and to be on the safe side, we decided also to increase the area of the machine from 165 square feet to 308 square feet, although so large a machine had never before been deemed controllable. The Lilienthal machine had an area of 151 square feet; that of Pilcher, 165 square feet; and the Chanute double-decker, 134 square feet. As our system of control consisted in a manipulation of the surfaces themselves instead of shifting the operator's body, we hoped that the new machine would be controllable, notwithstanding its great size. According to calculations it would obtain support in a wind of 17 miles per hour with an angle of incidence of only 3 degrees.

Our experience of the previous year having shown the necessity of a suitable building for housing the machine, we erected a cheap frame building, 16 feet wide, 25 feet long, and 7 feet high at the eaves. As our machine was 22 feet wide, 14 feet long (including the rudder) and about 6 feet high, it was not necessary to take the machine apart in any way in order to house it. Both ends of the building, except the gable parts, were made into doors which hinged above, so that when opened they formed an awning at each end, and left an entrance the full width of the building. We went into camp about the middle of July, and were soon joined by Mr. E. C. Huffaker, of Tennessee, an experienced aeronautical investigator in the employ of Mr. Chanute, by whom his services were kindly loaned, and by Dr. G. A. Spratt, of Pennsylvania, a young man who has made some valuable investigations of the properties of variously curved surfaces and the travel of the center of pressure thereon. Early in August, Mr. Chanute came down from Chicago to witness our experiments, and spent a week in camp with us. These gentlemen, with my brother and myself, formed our camping party, but in addition we had in many of our experiments the valuable assistance of Mr. W. J. Tate and Mr. Dan Tate, of Kitty Hawk.

The machine was completed and tried for the first time on the 27th of July in a wind blowing about 13 miles an hour. The operator having taken a position where the center of pressure was supposed to be, an attempt at gliding was made; but the machine turned downward and landed after going only a few yards. This indicated that the center of gravity was too far in front of the center of pressure.

In the second attempt the operator took a position several inches further back but the result was much the same. He kept moving further and further back with each trial, till finally he occupied a position nearly a foot back of that at which we had expected to find the center of pressure. The machine then sailed off and made an undulating flight of a little more than 300 feet. To the onlookers this flight seemed very successful, but to the operator it was known that the full power of the rudder had been required to keep the machine from either running into the ground or rising so high as to lose all headway. In the 1900 machine one fourth as much rudder action had been sufficient to give much better control. It was apparent that something was radically wrong, though we were for some time unable to locate the trouble. In one glide the machine rose higher and higher till it lost all headway. This was the position from which Lilienthal had always found difficulty to extricate himself, as his machine then, in spite of his greatest exertions, manifested a tendency to dive downward almost vertically and strike the ground head on with frightful velocity. In this case a warning cry from the ground caused the operator to turn the rudder to its full extent and also to move his body slightly forward. The machine then settled slowly to the ground, maintaining its horizontal position almost perfectly, and landed without any injury at all. This was very encouraging, as it showed that one of the very greatest dangers in machines with horizontal tails had been overcome by the use of a front rudder. Several glides later the same experience was repeated with the same result. In the latter case the machine had even commenced to move backward, but was nevertheless brought safely to the ground in a horizontal position. On the whole, this day's experiments were encouraging, for while the action of the rudder did not seem at all like that of our 1900 machine, yet we had escaped without difficulty from positions which had proved very dangerous to preceding experimenters, and after less than one minute's actual practice had made a glide of more than 300 feet, at an angle of descent of 10 degrees, and with a machine nearly twice as large as had previously been considered safe. The trouble with its control, which has been mentioned, we believed could be corrected when we should have located its cause. Several possible explanations occurred to us, but we finally concluded that the trouble was due to a reversal of the direction of the travel of the center of pressure at small angles. In deeply curved surfaces the center of pressure at 90 degrees is near the center of the surface, but moves forward as the angle becomes less, till a certain point is reached, varying with the depth of curvature. After this point is passed, the center of pressure, instead of continuing to move forward, with the decreasing angle, turns and moves rapidly toward the rear. The phenomena are due to the fact that at small angles the wind strikes the forward part of the surface on the upper side instead of the lower, and thus this part altogether ceases to lift, instead of being the most effective part of all, as in the case of the plane.

Lilienthal had called attention to the danger of using surfaces with a curvature as great as one in eight, on account of this action on the upper side; but he seems never to have investigated the curvature and angle at which the phenomena entirely cease. My brother and I had never made any original investigation of the matter, but assumed that a curvature of one in twelve would be safe, as this was the curvature on which Lilienthal based his tables. However, to be on the safe side, instead of using the arc of a circle, we had made the curve of our machine very abrupt at the front, so as to expose the least possible area to this downward pressure. While the machine was building, Messrs. Huffaker and Spratt had suggested that we would find this reversal of the center of pressure, but we believed it sufficiently guarded against. Accordingly, we were not at first disposed to believe that this reversal actually existed in our machine, although it offered a perfect explanation of the action we had noticed in gliding. Our peculiar plan of control by forward surfaces, instead of tails, was based on the assumption that the center of pressure would continue to move farther and farther forward, as the angle of incidence became less, and it will be readily perceived that it would make quite a difference if the front surface instead of counteracting this assumed forward travel, should in reality be expediting an actual backward movement. For several days we were in a state of indecision, but were finally convinced by observing the following phenomena: (Figure 1) We had removed the upper surface from the machine and were flying it in a wind to see at what angles it would be supported in winds of different strengths. We noticed that in light winds it flew in the upper position shown in the figure, with a strong upward pull on the cord c. As the wind became stronger, the angle of incidence became less, and the surface flew in the position shown in the middle of the figure, with a slight horizontal pull. But when the wind became still stronger, it took the lower position shown in the figure, with a strong downward pull. It at once occurred to me that here was the answer to our problem, for it is evident that in the first case the center of pressure was in front of the center of gravity and thus pushed up the front edge; in the second case, they were in coincidence, and the surface in equilibrium; while in the third case the center of pressure had reached a point even behind the center of gravity, and there was therefore a downward pull on the cord. This point having been definitely settled, we proceeded to truss down the ribs of the whole machine, so as to reduce the depth of curvature. In Figure 2, line 1, shows the original curvature; line 2, the curvature when supporting the operator's weight; and line 3, the curvature after trussing.

On resuming our gliding, we found that the old conditions of the preceding year had returned; and after a few trials, made a glide of 366 feet and soon after one of 389 feet. The machine with its new curvature never failed to respond promptly to even small movements of the rudder. The operator could cause it to almost skim the ground, following the undulations of its surface, or he could cause it to sail out almost on a level with the starting point, and passing high above the foot of the hill, gradually settle down to the ground. The wind on this day was blowing 11 to 14 miles per hour. The next day, the conditions being favorable, the machine was again taken out for trial. This time the velocity of the wind was 18 to 22 miles per hour. At first we felt some doubt as to the safety of attempting free flight in so strong a wind, with a machine of over 300 square feet, and a practice of less than five minutes spent in actual flight. But after several preliminary experiments we decided to try a glide. The control of the machine seemed so good that we then felt no apprehension in sailing boldly forth. And thereafter we made glide after glide, sometimes following the ground closely, and sometimes sailing high in the air. Chanute had his camera with him, and took pictures of some of these glides, several of which are among those shown.

We made glides on subsequent days, whenever the conditions were favorable. The highest wind thus experimented in was a little over 12 meters per second—nearly 27 miles per hour.

It had been our intention when building the machine to do the larger part of the experimenting in the following manner: When the wind blew 17 miles an hour, or more, we would attach a rope to the machine and let it rise as a kite with the operator upon it. When it should reach a proper height the operator would cast off the rope and glide down to the ground just as from the top of a hill. In this way we would be saved the trouble of carrying the machine uphill after each glide, and could make at least 10 glides in the time required for 1 in the other way. But when we came to try it we found that a wind of 17 miles, as measured by Richard's anemometer, instead of sustaining the machine with its operator, a total weight of 240 lbs., at an angle of incidence of 3 degrees, in reality would not sustain the machine alone—100 pounds—at this angle. Its lifting capacity seemed scarcely one third of the calculated amount. In order to make sure that this was not due to the porosity of the cloth, we constructed two small experimental surfaces of equal size, one of which was airproofed and the other left in its natural state; but we could detect no difference in their lifting powers. For a time we were led to suspect that the lift of curved surfaces little exceeded that of planes of the same size, but further investigation and experiment led to the opinion that (1) the anemometer used by us over recorded the true velocity of the wind by nearly 15 percent; (2) that the well known Smeaton coefficient of .005V2 for the wind pressure at 90 degrees is probably too great by at least 20 percent; (3) that Lilienthal's estimate that the pressure on a curved surface having an angle of incidence of 3 degrees equals .545 of the pressure at 90 degrees is too large, being nearly 50 percent greater than very recent experiments of our own with a special pressure testing machine indicate; (4) that the superposition of the surfaces somewhat reduced the lift per square foot, as compared with a single surface of equal area.

In gliding experiments, however, the amount of lift is of less relative importance than the ratio of lift to drift, as this alone decides the angle of gliding descent. In a plane the pressure is always perpendicular to the surface, and the ratio of lift to drift is therefore the same as that of the cosine to the sine of the angle of incidence. But in curved surfaces a very remarkable situation is found. The pressure instead of being uniformly normal to the chord of the arc, is usually inclined considerably in front of the perpendicular. The result is that the lift is greater and the drift less than if the pressure were normal. Lilienthal was the first to discover this exceedingly important fact, which is fully set forth in his book, *Bird Flight the Basis of the Flying Art*, but owing to some errors in the methods he used in making measurements, question was raised by other investigators not only as to the accuracy of his figures, but even as to the existence of any tangential force at all. Our experiments confirm the existence of this force, though our measurements differ considerably from those of Lilienthal. While at Kitty Hawk we spent much time in measuring the horizontal pressure on our unloaded machine at various angles of incidence. We found that at 13 degrees the horizontal pressure was about 23 lbs. This included not only the drift proper, or horizontal component of the pressure on the side of the surface, but also the head resistance of the framing as well. The weight of the machine at the time of this test was about 108 lbs. Now, if the pressure had been normal to the chord of the surface, the drift proper would have been to the lift (108 lbs.) as the sine of 13 degrees is to the cosine of 13 degrees, or $.22 \times 108 / .97 = 24+$ lbs.; but this slightly exceeds the total pull of 23 lbs. on our scales. Therefore, it is evident that the average pressure on the surface instead of being normal to the chord was so far inclined toward the front that all the head resistance of framing and wires used in the construction was more than overcome. In a wind of 14 miles per hour, resistance is by no means a negligible factor, so that tangential is evidently a force of considerable value. In a higher wind which sustained the machine at an angle of 10 degrees, the pull on the scales was 18 lbs. With the pressure normal to the chord, the drift proper would have been $.17 \times 98 / .98 = 17$ lbs., so that, although the higher wind velocity must have caused an increase in the head resistance, the tangential force still came within one pound of overcoming it. After our return from Kitty Hawk we began a series of experiments to accurately determine the amount and direction of the pressure produced on curved surfaces when acted upon by winds at the various angles from zero to 90 degrees. These experiments are not yet concluded, but in general they support Lilienthal in the claim that the curves give pressures more favorable in amount and direction than planes; but we find marked differences in the exact values, especially at angles below 10 degrees. We were unable to obtain direct measurements of the horizontal pressures of the machine with the operator on board, but by comparing the distance traveled in gliding with the vertical fall, it was easily calculated that at a speed of 24 miles per hour the total horizontal resistances of our machine, when bearing the operator, amounted to 40 pounds which is equivalent to about 2 ½ horsepower. It must not be supposed, however, that a motor developing this power would be sufficient to drive a man-bearing machine. The extra weight of the motor would require either a larger machine, higher speed, or a greater angle of incidence, in order to support it, and therefore more power. It is probable, however, that an engine of 6 horsepower, weighing 200 pounds, would answer the purpose. Such an engine is entirely practicable. Indeed, working motors of one half this weight per horsepower (9 pounds per horsepower) have been constructed by several different builders. Increasing the speed of our machine from 24 to 33 miles per hour reduced the total horizontal pressure from 40 to about 35 pounds. This was quite an advantage in gliding as it made it possible to sail about 15 percent further with a given drop. However, it would be of little or no advantage in reducing the size of the motor in a power-driven machine, because the lessened thrust would be counterbalanced by the increased speed per minute. Some years ago Prof. Langley called attention to the great economy of thrust which might be obtained by using very high speeds, and from this many were led to suppose that high speed was essential to success in a motor-driven machine. But the economy to which Prof. Langley called attention was in foot pounds per mile of travel, not in foot pounds per minute. It is the foot pounds per minute that fixes the size of a relatively low speed, perhaps not much exceeding 20 miles per hour, but the problem of increasing the speed will be much simpler in some respects than that of increasing the speed of a steamboat; for, whereas in the latter case the size of the engine must increase as the cube of the speed, in the flying machine, until extremely high speeds are reached, the capacity of the motor increases in less than simple ratio; and there is even a decrease in the fuel consumption per mile of travel. In other words to double the speed of a steamship (and the same is true of the balloon type of airship) eight times the engine and boiler capacity would be required, and four times the fuel consumption per mile of travel; while a flying machine would require engines of less than double the size, and there would be an actual decrease in the fuel consumption per mile of travel. But looking at the matter conversely, the great disadvantage of the flying machine is apparent, for in the latter no flight at all is possible unless the proportion of horsepower to flying capacity is very high; but on the other hand a steamship is a mechanical success if its ratio of horsepower to tonnage is insignificant. A flying machine that would fly at a speed of 50 miles an hour with engines of 1,000 horsepower, would not be upheld by its wings at all at a speed of less than 25 miles an hour, and nothing less than 500 horsepower could drive it at this speed. But a boat which could make 40 miles per hour with engines of 1,000 horsepower, would still move 4 miles an hour even if the engines were reduced to 1 horsepower. The problems of land and water travel were solved in the 19th century because it was possible to begin with small achievements and gradually work up to our present success. The flying problem was left over to the 20th century, because in this case the art must be highly developed before any flight of any considerable duration at all can be obtained.

However, there is another way of flying which requires no artificial motor, and many workers believe that success will first come by this road. I refer to the soaring flight, by which the machine is permanently sustained in the air by the same means that are employed by soaring birds. They spread their wings to the wind, and sail by the hour, with no perceptible exertion beyond that required to balance and steer themselves. What sustains them is not definitely known, though it is almost certain that it is a rising current of air. But whether it be a rising current or something else, it is as well able to support a flying machine as a bird, if man once learns the art of utilizing it. In gliding experiments it has long been known that the rate of vertical descent is very much retarded and the duration of the flight greatly prolonged, if a strong wind blows *up* the face of the hill parallel to its surface. Our machine, when gliding in still air, has a rate of vertical descent of nearly 6 feet per second, while in a wind blowing 26 miles per hour up a steep hill, we made glides in which the rate of descent was less than 2 feet per second. And during the larger part of this time, while the machine remained exactly in the rising current, *there was no descent at all, but even a slight rise*. If the operator had had sufficient skill to keep himself from passing beyond the rising current, he would have been sustained indefinitely at a higher point than that from which he started. The illustration shows one of these very slow glides at a time when the machine was practically at a standstill. The failure to advance more rapidly caused the photographer some trouble in aiming, as you will perceive. In looking at this picture you will readily understand that the excitement of gliding experiments does not entirely cease with the breaking up of camp. In the photographic darkroom at home we pass moments of as thrilling interest as any in the field, when the image begins to appear on the plate and it is yet an open question whether we have a picture of a flying machine, or merely a patch of open sky. These 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 winds 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. This illustration shows the machine, alone, flying in a wind of 35 miles per hour on the face of a steep hill, 100 feet high. It will be seen that the machine not only pulls upward, but also pulls forward in the direction from which the wind blows, thus overcoming both gravity and the speed of the wind. We tried the same experiment with a man on it, but found danger that the forward pull would become so strong that the men holding the ropes would be dragged from their insecure foothold on the slope of the hill. So this form of experimenting was discontinued after four or five minute trials.

In looking over our experiments of the past two years, with models and full-sized machines, the following points stand out with clearness:

1. That the lifting power of a large machine, held stationary in a wind at a small distance from the earth, is much less than the Lilienthal table and our own laboratory experiments would lead us to expect. When the machine is moved through the air, as in gliding, the discrepancy seems much less marked.

2. That the ratio of drift to lift in well-shaped surfaces is less at angles of incidence of five degrees to 12 degrees than at an angle of three degrees.

3. That in arched surfaces the center of pressure at 90 degrees is near the center of the surface, but moves slowly forward as the angle becomes less, till a critical angle varying with the shape and depth of the curve is reached, after which it moves rapidly toward the rear till the angle of no lift is found.

4. That with similar conditions, large surfaces may be controlled with not much greater difficulty than small ones, if the control is effected by manipulation of the surfaces themselves, rather than by a movement of the body of the operator.

5. That the head resistances of the framing can be brought to a point much below that usually estimated as necessary.

6. That tails, both vertical and horizontal, may with safety be eliminated in gliding and other flying experiments.

7. That a horizontal position of the operator's body may be assumed without excessive danger, and thus the head resistance reduced to about one fifth that of the upright position.

8. That a pair of superposed, or tandem surfaces, has less lift in proportion to drift than either surface separately, even after making allowance for weight and head resistance of the connections.

Document 1-12(a–b)

(a) Octave Chanute, excerpt from remarks at a meeting of the Aéro-Club, Paris, 2 April 1903, translated from Ernest Archdeacon, *La Locomotion*, 11 April 1903, pp. 225–227, in *The Papers of Wilbur and Orville Wright*, pp. 654–673.

(b) Wilbur Wright, excerpts from "Experiments and Observations in Soaring Flight," address at the meeting of the Western Society of Engineers, Chicago, 24 June 1903, published in *Journal of the Western Society of Engineers* (December 1903), in *The Papers of Wilbur and Orville Wright*, pp. 318–330.

One of the most influential reports on the glider experiments done in America, and especially of the recent work of Wilbur and Orville Wright, was an address given in Paris by Octave Chanute in the spring of 1903. At the time Chanute made these remarks, the Wrights were applying what they had learned from their gliding experiments to the design of a powered airplane.

The Wright brothers' third season of flight tests at Kitty Hawk in 1902 crossed a threshold in the development of flight. With their newly designed glider they had, for the first time, a machine in which they could really learn how to fly. In the following document, Wilbur Wright describes this breakthrough season of flight experiments. This passage is taken from an address given at the 1903 meeting of the Western Society of Engineers, as a follow-up to his earlier address presented in September 1901. These remarks were made on the eve of the Wrights' departure for a fourth season of tests at Kitty Hawk, where they planned to fly their new powered airplane based on the glider described here.

Document 1-12(a), Octave Chanute, excerpt from remarks at a meeting of the Aéro-Club, Paris, 2 April 1903.

The strains arise from the irregularities and turmoils of the wind. The current does not come as an evenly flowing stream, but as a series of swirling waves, as shown in the smoke issuing from a chimney. These waves strike the apparatus either on one side or on the other, from below or from above, and constantly tend to upset it. The velocity and force with which the machine is struck by the wave depend both upon the distance from the center of rotation of the latter and upon the speed of the current, and this is probably the reason why anemometers show such varying pressures. A flying machine must meet and overcome all these vicissitudes and this must be done instantly.

In 1896 and 1897, being impressed that the equilibrium of the bird was partly automatic, and that the problem of stability in the wind was the first which must be solved, I undertook some experiments near Chicago, Illinois, with full-sized gliding machines carrying a man, in order to study equilibrium, and that alone. I caused to be built five machines of four different types. The first was a Lilienthal apparatus, in order to start from the known before passing to the unknown, and three of the other machines were based upon the reverse of the Lilienthal type, that is to say that instead of re-establishing the equilibrium (when compromised by the variations of the wind) by displacing the body of the operator, and therefore that of the center of gravity, as did Lilienthal and Pilcher, the new machines were based upon the theory that it was possible so to arrange the carrying surfaces that they should move automatically under the action of the wind and bring back the center of pressure vertically over the center of gravity—a condition absolutely necessary in order to maintain equilibrium.

One of these machines, called the "multiple-wing," is shown in figures 1 to 5. The supporting wings are at the front and are superposed and braced together. They turn upon ball bearings marked B, and are restrained in front by rubber springs which allows a certain amount of horizontal movement. If the *relative wind* increases, thus requiring a lesser angle of incidence to sustain the weight, the wings are blown backward, the apparatus oscillates towards the front, and the angle becomes smaller. Once the squall passed, the springs bring the wings back to their normal position. The rear wings are flexible and merely aid in balancing, and an aeroplane, which might be replaced by an additional pair of wings, surmounts the whole. The whole apparatus weighed 15.2 kilograms, the supporting area of the front wings being 13.33 square meters and that of the rear wings 2.74 m^2. This machine proved very nearly automatic, the operator having to move but 25 millimeters, in ordinary glides, as against 127 mm. with the Lilienthal and 63 mm. with the "two-surface" machine, which is next to be described.

The "two-surface" machine gave the best results and the longest glides. It is shown in figures 6 to 10. The supporting surfaces are of varnished silk and are affixed to a framework similar to a bridge truss, and the operator is below in an upright position, being sustained under the armpits by two horizontal bars. Equilibrium is obtained through a horizontal tail, similar to that invented by Pénaud, but with elastic fastenings devised by Mr. Herring. This tail generally makes an angle of 7 to 8 degrees with the carrying surfaces, and by receiving the relative wind below or above automatically changes the angle of attack according to the exigencies of the moment. The frame was of wood and the weight was 11 kg.

and the surface 12.45 square meters, which easily sustained an operator weighing 71 kg. More than one thousand glides were made with these machines in 1896 and 1897 without accident, the following tables giving an idea of the results with each of the machines:

SOME GLIDES OF MULTIPLE-WING MACHINE—WEIGHT 86 KG., MOUNTED

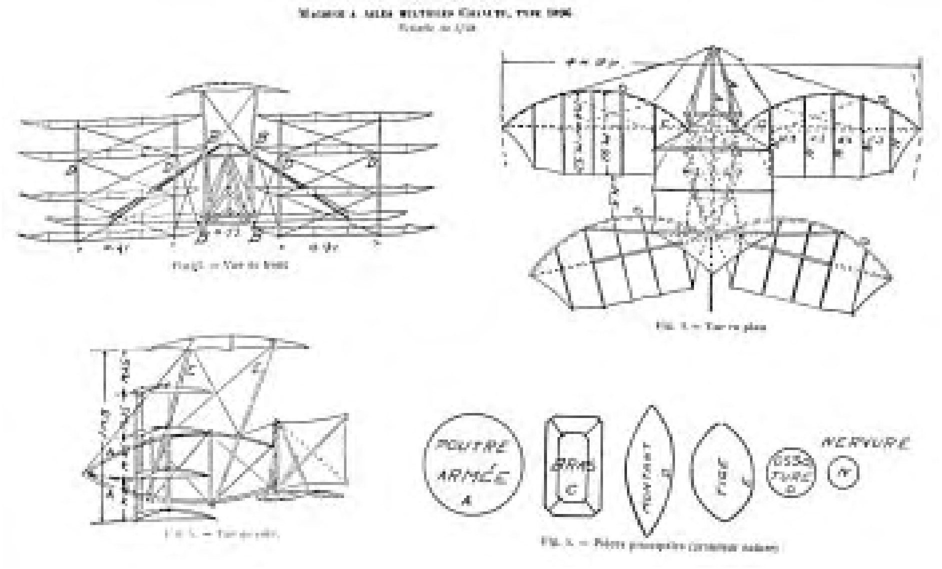
Operator	*Length (meters)*	*Time (Seconds)*	*Speed M. per sec.*	*Remarks*
Herring	45.11	7.0	6.45	
Avery	53.04	7.6	7.00	
Herring	50.60	7.5	6.72	
Avery	55.77	7.9	7.00	Angle of descent not measured but about 10° to 11°
Herring	52.43	7.8	6.70	

SOME GLIDES OF TWO-SURFACE—WEIGHT 82 KG., MOUNTED

Operator	*Length (meters)*	*Time (seconds)*	*Angle descent*	*Height fallen (meters)*	*Speed M. per sec*	*Rate of descent*	*Kg.*	*Remarks*
Avery	60.64	8	10°	10.5	7.62	1 in 5.75	106.	
Herring	71.32	8.7	7 ½°	9.3	8.23	1 in 7.69	87.	
Avery	77.11	....	10 ½°	14	....	1 in 5.50		Time not taken
Herring	72.84	....	11°	14.1	....	1 in 5.24		Time not taken
"	67.06	9	....	....	7.42	....		Angle not taken
"	71.62	10.3	....	....	7.	....		Angle not taken
Avery	78.03	10.2	8°	10.8	7.6	1 in 7.18	86.	
Herring	109.42	14.	10°	18.9	7.8	1 in 5.75	110.	

The slope of the hill being steep and the wind ascending, it was estimated that the resistance required 2 horsepower, instead of the 1 ⅓ H.P. shown by the work done as calculated. The drawings of these various machines were published in the *Aeronautical Annual* for 1897, edited by Jas. Means, Boston, U.S.A., and amateurs were invited to repeat and to improve upon the performances.

In 1902 I caused to be built an apparatus to obtain automatic stability by a third method. This consists in pivoting the sustaining surfaces about 4/10 of their width from the front, and restraining them by springs. If the relative wind increases, the center of pressure moves backward and tends to give the surfaces a smaller



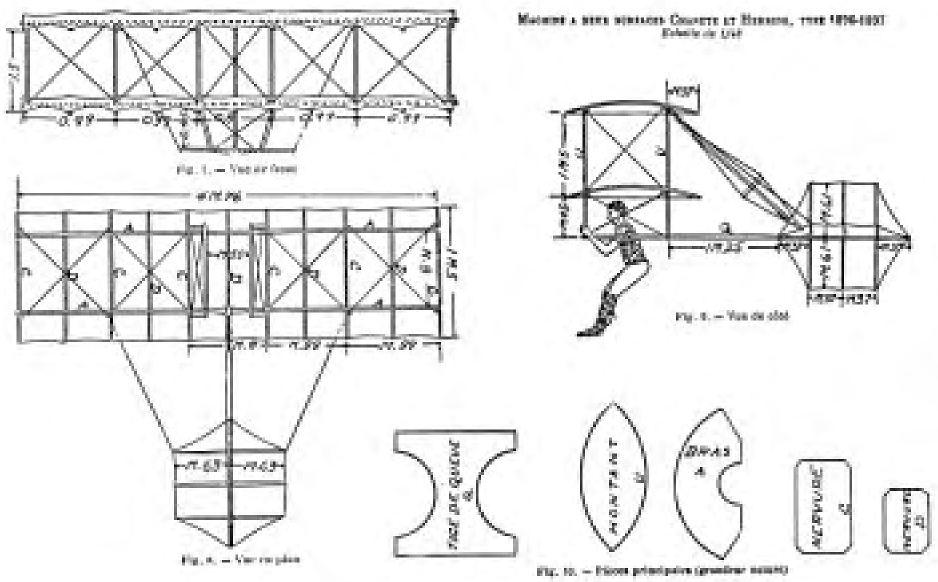
angle of attack. Only a few tests were made in 1902, but more are to be carried on in 1903. The apparatus is shown in figures 11 to 15 for the benefit of amateurs and a photograph of one of the glides is herewith reproduced.

All these experiments were made without accident. It is true that in the beginning all possible precautions were taken. The practice ground selected was on the soft sand hills which border the southern edge of Lake Michigan, some 50 kilometers from Chicago, where there are but few trees or bushes. No glides were made in very gusty winds, or those exceeding 50 kilometers to the hour, and Messrs. Herring and Avery, my assistants, were alone allowed to experiment. Later on, more confidence was gained and visitors were allowed to make short glides under instruction from the experts. All succeeded well, and even the cook became almost an expert in a short time. It must, however, be recognized that this sport is dangerous and that all who would engage in it must take all possible precautions before venturing upon it.

The invitation to amateurs to repeat these experiments remained unacted upon till 1900, when Messrs. Wilbur & Orville Wright of Dayton (Ohio), took up the question. They have accomplished such advance upon all previous practice that the rest of this paper will be devoted to giving an account thereof. The improvements which they have introduced are the following:

1st. Placing the horizontal rudder or tail at the front, a position which proves more efficient in acting upon the air.

2nd. Placing the operator prone on the machine, thus diminishing by ⅘ the resistance due to his body.

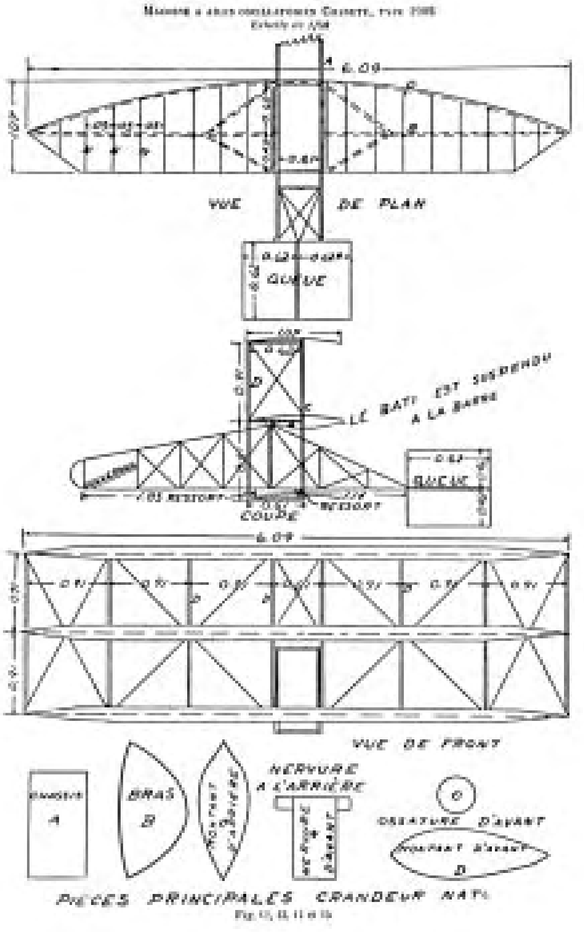


3rd. Warping the wings to steer to right or left.

The practice ground chosen by the Wright brothers, after an inquiry made of the National Signal Bureau of the United States, is far superior to the hills near Chicago. It is situated at Kitty Hawk near the ocean in North Carolina, and consists of a soft sand hill about 30 meters high, on a tongue of land, without a single tree, bush or grass. At its foot a sand beach one kilometer wide extends to the sea, and regular sea breezes blow daily from the ocean. Moreover the spot is a desert and quite inaccessible to the curious, who are always in the way when one is seeking for unknown phenomena.

The Wright apparatus of 1900 measured 5.64 meters across by a width of 1.52 m., there being two surfaces aggregating 15.6 square meters, and weighing 21.8 kg. It chiefly served to study the result of placing the horizontal rudder at the front, the apparatus being loaded with sandbags or weights and flown as a kite. Having ascertained that the machine worked well, the Wrights ventured to make a few glides with good success, and postponed to the next season learning the difficult art of the birds.

The method of conducting the experiments necessarily differed from those adopted by Lilienthal and his imitators. In those machines the operator, being on his feet, carries the apparatus, poises it in the wind, and runs forward until he leaves the ground. With the two-surface machine, for instance, which weighed but 11 kg. (for 12.45 m. surface) the aviator could walk about with his wings on his shoulders. The Wrights, desiring to build a larger and heavier machine and to place the aviator horizontally, had to devise a new method of getting under way.



This consisted in employing two assistants each grasping one extremity of the machine, and running forward against the wind. The aviator, placed in the center, first remains on his feet, runs a few steps and, as speed and supporting power increase, tips forward prone on the framing, hooking his feet on the rear transverse bar, and as soon as he feels himself well carried by the air, he cries, "Let go." From this on gravity serves as a motor; he glides along down the slope of the hill, keeping near the ground, meeting the irregularities of the wind by the action of the front rudder and steering to the right or left, by the torsion of the wings, which are framed loosely, and by the vertical rudder behind. When the foot of the hill is reached, a vigorous action of the front rudder causes the machine to shoot upward a little, thus increasing the resistance and diminishing the speed. The machine then alights upon the sand and stops after a short slide upon its shoes.

The 1901 machine was 6.7 meters across by a width of 2.13 m., the two surfaces being spaced 1.42 m. vertically and giving a surface of 27.1 m^2, with a weight of 45.4 kg. This apparatus performed several hundred glides without accident, but although the head resistance was less, by reason of the horizontal position of the aviator, the angles of descent were nearly the same (8° to 10°) as with the machines in which the aviator stood upright. Now the important thing for a gliding apparatus is that the fall through the air shall be the least possible. We can always lengthen the glides by starting from a higher point, but the angle of descent shows at once the ratio between the propelling force (the weight) and the horizontal resistance, and the latter should be reduced to a minimum before proceeding to other details.

The horizontal resistance consists of three factors:

1st. The *drift*, depending upon the angle of incidence.

2nd. The *head resistance* of the various thicknesses.

3rd. The *tangential* force, recognized by Lilienthal.

The *drift* is the horizontal resultant of the normal pressure and has been thoroughly written upon heretofore, so that it need not be here discussed. Recent experiments seem to show, however, that the friction of the air upon the surfaces is not negligible, and should be separately added.

The *head* resistance is the most important, and is the one in which improvements may be achieved, by shaping the framing so as to give the lowest possible coefficients, by diminishing the number of parts, and by reducing all guy wires and similar vibrating parts to a minimum.

The tangential force only applies to arched surfaces, and presents the curious property that at certain angles of incidence it acts as a propulsive force. Those who may wish to learn more about this are referred to Lilienthal's book, *Der Vogelflug als Grundlage der Fliegekunst*, and to his chapter in Moedebeck's *Taschenbuch für Flugtechniker und Luftschiffer*.

We arrive at the aggregate horizontal resistance by three different processes, as follows:

1st. The speed required for support and the angle of incidence with the relative wind, being first ascertained as accurately as possible, the normal pressure is computed by the aid of Lilienthal's table for arched surfaces, which will be found in Moedebeck's *Taschenbuch*, and this normal is multiplied by the sine of the angle of application to obtain the *drift*. To this is added the *head resistance* due to the framing, the aviator and adjuncts, which is obtained by measuring accurately all the thicknesses and applying to each a coefficient due to its form. It is interesting in this connection to know that beams fishlike in cross section offer but one sixth the resistance on a plane of the same area as their "master section" while vibrating wires offer about twice the resistance due to their "master section." All this is best obtained by drawing up a tabular schedule of all the parts and deducing there from the equivalent or fictive area of resistance of the apparatus, which, when multiplied by the air pressure due to the speed, gives the aggregate *head resistance*; to the sum of the latter with the *drift* is added or deducted the *tangential* force, as obtained from Lilienthal's table, according to whether it is positive or negative. In other words:

Resistance = drift + head resistance ± tangential. This process is tedious and slow, but it is well worth the trouble, for it indicates those parts of the apparatus in which resistance can be reduced.

2nd. The second process furnishes a check upon the first and consists in floating the mounted machine in a wind of sufficient intensity or towing it by a cord at sufficient speed to obtain support and measuring the actual pull with a hand scale. This cannot be done very often as it involves considerable preparation, but it is a check not to be disputed.

3rd. The third process consists in simply multiplying the weight by the sine of the angle of descent. In fact, as the speed of the glides is almost always uniform it is evident that the forces balance, and the resistance along the path is to the weight as the sine of the angle to the radius. It is found as the result of a great many such computations that the three processes agree very closely.

The Messrs. Wright concluded from their experiments in 1901 that there were a number of defects in the apparatus used that year. That the shape of the surfaces was not the best possible and that they became deformed under the pressure of the relative wind. During the succeeding winter they made a whole series of laboratory experiments upon various surfaces, and built a third machine for experiment in 1902.

This last apparatus was 9.75 meters across, by a width of 1.52 m., the two surfaces being spaced 1.42 m. apart, and giving 28.4 m2. of supporting surface. The front rudder was 1.4 m^2. in area, and the weight was 53 kg. Fig. 16 to 20 show this apparatus. The main pieces are shown full size. They were all of wood, the front spar and the main arm were of an American wood for which pine would be a substitute; these parts were either imbedded in the cloth, or in a sheath or strip glued on afterwards. The ribs were of ash, steamed and bent, with a curvature of 1/20 one third of the chord from the front. All fastenings were made by lashing the sticks together, and it is well to dampen the twine with thin glue when wrapping it around. The surfaces consisted of tightly woven cotton cloth such as that used for balloons. It was not varnished but would have been the better for it. The braces were of piano wire and it was generally aimed to have a factor of safety of 10 times the breaking weight. A light pair of shoes or skis was attached to the framing and spar at the front.

The machine was tested on the practice ground already described, in September and October, 1902, and it gave very superior results to those obtained with preceding machines. The angles of descent were flatter and the weight sustained per horsepower was greater. The two brothers glided alternately and they soon attained almost complete mastery over the inconstancies of the wind. They met the wind gusts and steered as they willed. They did not venture to sweep much more than one quarter circle, so as not to lose the advantage of a head wind, but

Machine Wright, vers 1901 [illegible]

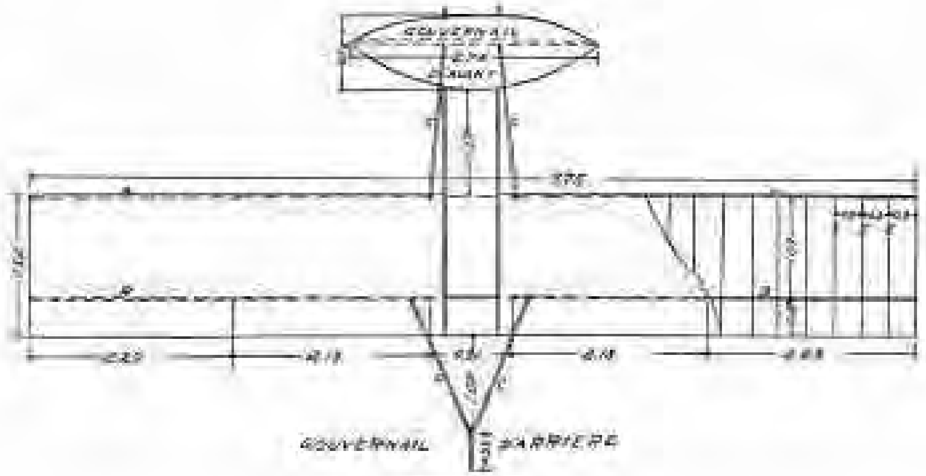


Fig. [illegible]

Machine Wright, vers 1901 — Echelle de [illegible]

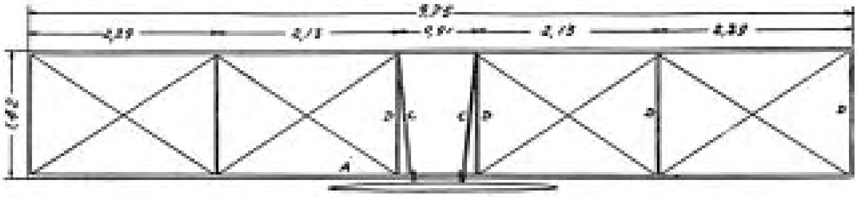


Fig. 8. — Élévation, vue de front

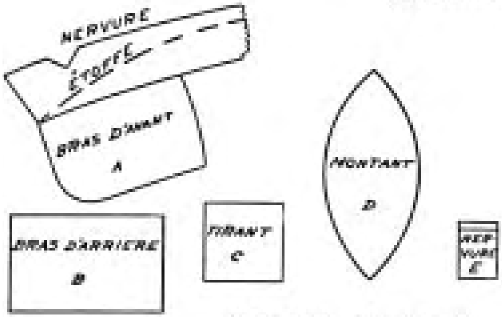


Fig. 11. — Pièces principales (grandeur nature)

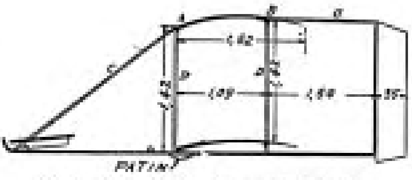


Fig. 10. — Machine Wright, vers 1901 (coupe) (Echelle de [illegible])

they constantly improved in the control of the machine and in learning the art of the birds. Some 800 glides were made of which the following are representative:

The variations mainly result from more or less skill in each glide and consequent slight swerving.

The last column in the table is that which best indicates the advance made over previous practice. The machine of Mr. Maxim supported 12.7 kg., per horsepower, that of Professor Langley 13.6 kg., while previous glides from Lilienthal down showed 45 kg. per horsepower. It is quite true that when a motor is added the weight sustained per horsepower is diminished about one half by reason of the inevitable losses due to the motor, the transmission and the propeller, but Messrs. Wright have so far diminished the resistances that they now sustain 62 kg. per horsepower, and the time is evidently approaching when, the problems of equilibrium and control having been solved, it will be safe to apply a motor and a propeller.

In point of fact the Messrs. Wright are now gliding very nearly as well as the vulture, which generally descends one meter in ten (5°45') in calm air. It is therefore not impossible that man shall eventually master sailing flight and learn, when once well in the air and at speed, to imitate the soaring birds which circle and rise through the force of the wind alone when circumstances are favorable; which conditions chiefly consist in an ascending trend of the wind, such as is frequently found in hot countries, those being the regions which are frequented by sailing birds.

This does not mean that the future flying machine, if such is developed, can entirely do without a motor. This adjunct will be indispensable, not only to fly in those regions where the wind seldom has an ascending trend, but also to restore the speed or the equilibrium very quickly under many circumstances, such as a very sudden wind gust, or a descent too near to the ground. It is easily perceived, however, that when the motor need only be used occasionally the length of possible journeys of flying apparatus will be greatly increased in the countries where circumstances favor sailing flight.

It is thus seen that there has been a gradual evolution from Lilienthal onward. That gliding machines have been made much safer and are now fairly under control. There is doubtless a chance for further improvement, and this can only be accomplished by experiment. The present indications are that such experiments will be extensively undertaken, and a word of warning may well be given in concluding this article.

Let all exert the utmost possible prudence in conducting gliding experiments. First select suitable ground, a soft sand hill is the best, so isolated from other hills as to have no cross currents or whirling winds. Begin practice very gradually, starting from a small elevation and learning by degrees the control of the machine and the best way of alighting safely. Follow the slope of the hill closely, so as to have but a short distance to fall if a false maneuver is made. Examine the apparatus after each glide, and if anything has gotten out of order, repair it at once before making another glide. Try no feats which involve being high in air except over an appropriate sheet of water, but do not resort to that method of experimenting upon ordinary occasions. It involves too much loss of time in drying oneself and the machine, and the latter is likely to become distorted.

More important than all, do not try to beat previous records. This leads to taking risks and to producing accidents. It is well to have competitions at which several amateurs practice together, because they learn from each other and because they are then more disposed to have a surgeon present, a precaution which I have always taken, but we must always remember that the important things to be secured are control of the machine and safe landings, without regard to the distance glided over, the latter mainly depending upon the elevation started from.

Progress and safety will be greatly promoted by beginning with small machines just as the Wrights have done. An apparatus 6 meters by 1.3 meters, giving with two surfaces arched to 1/25 of their width some 15 square meters of supporting area, is ample to carry a man and will be found far more manageable than the last machine of Messrs. Wright of which the drawings are here given. As the cost is not great, being from 500 to 1500 francs, according to the amount of care bestowed on the apparatus, it is feasible to build new machines from time to time in order to introduce the improvements which constantly suggest themselves. It is hoped that when many experimenters get at work such progress shall be made as materially to advance the time when aviation shall become practical.

Document 1-12(b), Wilbur Wright, excerpts from "Experiments and Observations in Soaring Flight," Chicago, 24 June 1903.

The prime object in these experiments was to obtain practice in the management of a man-carrying machine, but an object of scarcely less importance was to obtain data for the study of the scientific problems involved in flight. Observations were almost constantly being made for the purpose of determining the amount and direction of the pressures upon the sustaining wings; the minimum speed required for support; the speed and angle of incidence at which the horizontal resistance became least; and the minimum angle of descent at which it was possible to glide. To determine any of these points with exactness was found to be very difficult indeed, but by careful observations under test conditions it was possible to obtain reasonably close approximations. . . .

In addition to the work with the machine we also made many observations on the flight of soaring birds, which were very abundant in the vicinity of our camp. Bald eagles, ospreys, hawks, and buzzards gave us daily exhibitions of their powers. The buzzards were the most numerous and were the most persistent soarers. They apparently never flapped except when it was absolutely necessary, while the eagles and hawks usually soared only when they were at leisure. Two methods of soaring were employed. When the weather was cold and damp and the wind strong, the buzzards would be seen soaring back and forth along the hills or at the edge of a clump of trees. They were evidently taking advantage of the current of air flowing upward over these obstructions. On such days they were often utterly unable to soar except in these special places. But on warm clear days when the wind was light they would be seen high in the air soaring in great circles. Usually however it seemed to be necessary to reach a height of several hundred feet by flapping before this style of soaring became possible. Frequently a great number of them would begin circling in one spot, rising together higher and higher till finally they would disperse, each gliding off in whatever direction it wished to go. At such times other buzzards only a short distance away found it necessary to flap frequently in order to maintain themselves. But when they reached a point beneath the circling flock they too began to rise on motionless wings. This seemed to indicate that rising columns of air do not exist everywhere, but that the birds must find them. They evidently watch each other and when one finds a rising current the others quickly make their way to it. One day when scarce a breath of wind was stirring on the ground, we noticed two bald eagles sailing in circling sweeps at a height of probably 500 feet. After a time our attention was attracted to the flashing of some object considerably lower down. Examination with a field glass proved it to be a feather which one of the birds had evidently cast. As it seemed apparent that it would come to earth only a short distance away some of our party started to get it. But in a little while it was noticed that the feather was no longer falling but on the contrary was rising rapidly. It finally went out of sight upward. It apparently was drawn into the same rising current in which the eagles were soaring, and was carried up like the birds.

The days when the wind blew horizontally gave us the most satisfactory observations, as then the birds were compelled to make use of the currents flowing up the sides of the hills and it was possible for us to measure the velocity and trend of the wind in which the soaring was performed. One day four buzzards began soaring on the northeast slope of the Big Hill at a height of only ten or twelve feet from the surface. We took a position to windward and about 1,200 feet distant. The clinometer showed that they were 4 ½ to 5 ½ degrees above our horizon. We could see them very distinctly with a field glass. When facing us the under side of their wings made a broad band on the sky, but when in circling they faced from us we could no longer see the underside of their wings. Though the wings then made little more than a line on the sky the glass showed clearly that it was not the under side that we saw. It was evident that the buzzards were soaring with their wings constantly inclined above five degrees above the horizon. They were attempting to gain sufficient altitude to enable them to glide to the ocean beach three fourths of a mile distant, but after reaching a height of about 75 feet above the top of the hill they seemed to be unable to rise higher, though they tried a long time. At last they started to glide toward the ocean but were compelled to begin flapping almost immediately. We at once measured the slope and the wind. The former was 12 ½ degrees; the latter was six to eight meters per second. Since the wings were inclined 5 degrees above the horizon and the wind had a rising trend of fully 12 degrees, the angle of incidence was about 17 degrees. The wind did not average more than seven meters, 15 miles an hour. For the most part the birds faced the wind steadily, but in the lulls they were compelled to circle or glide back and forth in order to obtain speed sufficient to provide support. As the buzzard weighs about .8 pounds per square foot of wing area, the lifting power of the wind at 17 degrees angle of incidence was apparently as great as it would have been had it been blowing straight upward with equal velocity. The pressure was inclined 5 degrees in front of the normal, and the angle descent was 12 ½ degrees.

On another day I stood on top of the West Hill directly behind a buzzard which was soaring on the steep southern slope. It was just on a level with my eye and not more than 75 feet distant. For some time it remained almost motionless. Although the wings were inclined about five degrees above the horizon, it was not driven backward by the wind. This bird is specially adapted to soaring at large angles of incidence in strongly rising currents. Its wings are deeply curved. Unless the upward trend amounts to at least eight degrees it seems to be unable to maintain itself. One day we watched a flock attempting to soar on the west slope of the Big Hill, which has a descent of nearly nine degrees. The birds would start near the top and glide down along the slope very much as we did with the machine but we noticed that whenever they glided parallel with the slope their speed diminished, and when their speed was maintained the angle of descent was greater than that of the hill. In every case they found it necessary to flap before they had gone two hundred feet. They tried time and again but always with the same results. Finally they resorted to hard flapping till a height of about 150 feet above the top of the hill was reached, after which they were able to soar in circles without difficulty. On another day they finally succeeded in rising on almost the same slope, from which it was concluded that the buzzard's best angle of descent could not be far from eight degrees. There is no question in my mind that men can build wings having as little or less relative resistance than that of the best soaring birds. The bird's wings are undoubtedly very well designed indeed, but it is not any extraordinary efficiency that strikes with astonishment but rather the marvelous skill with which they are used. It is true that I have seen birds perform soaring feats of almost incredible nature in positions where it was not possible to measure the speed and trend of the wind, but whenever it was possible to determine by actual measurement the conditions under which the soaring was performed, it was easy to account for it on the basis of the results obtained with artificial wings. The soaring problem is apparently not so much one of better wings as of better operators.

Document 1-13(a–b)

(a) Orville Wright, diary entry for 17 December 1903, published as "Diary of the First Flight," *Collier's* 25 (December 1948): 33.

(b) Wilbur and Orville Wright, statement to the Associated Press, Dayton, Ohio, 5 January 1904.

The following documents are companions to the Wright brothers' telegram, sent to their father in Dayton, announcing the historic achievement of powered flight on 17 December 1903. The first is the diary entry for that historic day, composed by Orville, and the second is the public statement of their achievement, subsequently prepared by the Wrights for release to the press. The latter was given to the local representative of the Associated Press in Dayton (a man by the name of Frank Tunison), but he was not interested. Articles about the Wrights containing part of this statement did appear in miscellaneous newspapers, beginning on 6 January 1904. But only the *Dayton Press* carried the full statement contained here. Instead of the carefully crafted, low-key statements deliberately thought out by the Wrights, what appeared in newspapers about their flying—when anything appeared at all—were imaginative, inaccurate accounts that disturbed the Wrights and misled the public into thinking that this story was no different than all the others that came before—that was, mostly fanciful and not worthy of serious attention.

It is truly astonishing how the press bungled the story of the Wright's achievement. Not only did the press botch the story in 1903, but it continued to neglect or completely distort it for the next five years, well into 1908. One aviation historian, the distinguished Richard K. Smith, has gone so far as to say that "the relationship between the Wrights and the news medium of their day is one of the most grotesque stories of the 20th century and it was by no means the fault of the Wrights" ["Not a Success—But a Triumph: 80 Years Since Kitty Hawk," *Naval War College Review* 36 (November–December 1983): 13]. As indicated in the historic telegram itself, the Wrights chose to inform the press with carefully worded statements and continued to do so for months, but the press paid no attention. December 1903 and early 1904 were not good times for the press to give much credence to anyone's claims of a successful flying machine. Just nine days before the alleged achievement at Kitty Hawk, the press had seen, with its own eyes, the debacle in the Potomac River involving Dr. Samuel P. Langley's full-scale aerodrome. If the great Professor Langley, one of America's greatest scientists, could not solve the problems of flight, then how could a couple of unknown bicycle mechanics from Dayton, Ohio?

Document 1-13(a), Orville Wright, diary entry for 17 December 1903.

Thursday, December 17, 1903

When we got up a wind of between 20 and 25 miles was blowing from the north. We got the machine out early and put out the signal for the men at the station. Before we were quite ready, John T. Daniels, W. S. Dough, A. D. Etheridge, W. C. Brinkley of Manteo, and Johnny Moore of Nags Head arrived. After running the engine and propellors a few minutes to get them in working order, I got on the machine at 10:35 for the first trial. The wind, according to our anemometers at this time, was blowing a little over 20 miles (corrected) 27 miles according to the Government anemometer at Kitty Hawk. On slipping the rope the machine started off increasing in speed to probably 7 or 8 miles. The machine lifted from the track just as it was entering on the fourth rail.

Mr. Daniels took a picture just as it left the tracks. I found the control of the front rudder quite difficult on account of its being balanced too near the center and thus had a tendency to turn itself when started so that the rudder was turned too far on one side and then too far on the other. As a result the machine would rise suddenly to about 10 ft. and then as suddenly, on turning the rudder, dart for the ground. A sudden dart when out about 100 feet from the end of the tracks ended the flight. Time about 12 seconds (not known exactly as watch was not promptly stopped). The lever for throwing off the engine was broken, and the skid under the rudder cracked. After repairs, at 20 min. after 11 o'clock Will made the second trial. The course was about like mine, up and down but a little longer over the ground though about the same in time. Dist. not measured but about 175 ft. Wind speed not quite so strong. With the aid of the station men present, we picked the machine up and carried it back to the starting ways. At about 20 minutes till 12 o'clock I made the third trial. When out about the same distance as Will's, I met with a strong gust from the left which raised the left wing and sidled the machine off to the right in a lively manner. I immediately turned the rudder to bring the machine down and then worked the end control. Much to our surprise, on reaching the ground the left wing struck first showing the lateral control of this machine much more effective than on any of our former ones. At the time of its sidling it had raised to a height of probably 12 to 14 feet. At just 12 o'clock Will started on the fourth and last trip. The machine started off with its ups and downs as it had before, but by the time he had gone three or four hundred feet he had it under much better control, and was traveling on a fairly even course. It proceeded in this manner till it reached a small hummock out about 800 feet from the starting ways, when it began its pitching again and suddenly darted into the ground. The front rudder frame was badly broken up, but the main frame suffered none at all. The distance over the ground was 852 feet in 59 seconds. The engine turns was 1071, but this included several seconds while on the starting ways and probably about a half second after landing. The jar of landing had set the watch on machine back so that we have no exact record for the 1071 turns. Will took a picture of my third flight just before the gust struck the machine. The machine left the ways successfully at every trial, and the tail was never caught by the truck as we had feared.

After removing the front rudder, we carried the machine back to camp. We set the machine down a few feet west of the building, and while standing about discussing the last flight, a sudden gust of wind struck the machine and started to turn it over. All rushed to stop it. Will who was near the end ran to the front, but too late to do any good. Mr. Daniels and myself seized spars at the rear, but to no purpose. The machine gradually turned over on us. Mr. Daniels, having had no experience in handling a machine of this kind, hung on to it from the inside, and as a result was knocked down and turned over and over with it as it went. His escape was miraculous, as he was in with the engine and chains. The engine legs were all broken off, the chain guides badly bent, a number of uprights, and nearly all the rear ends of the ribs were broken. One spar only was broken.

After dinner we went to Kitty Hawk to send off telegram to M. W. While there we called on Capt. and Mrs. Hobbs, Dr. Cogswell and the station men.

Document 1-13(b), statement by the Wright brothers to the Associated Press, 5 January 1904.

It had not been our intention to make any detailed public statement concerning the private trials of our power "Flyer" on the 17th of December last; but since the contents of a private telegram, announcing to our folks at home the success of our trials, was dishonestly communicated to the newspapermen at the Norfolk office, and led to the imposition upon the public, by persons who never saw the "Flyer" or its flights, of a fictitious story incorrect in almost every detail; and since this story together with several pretended interviews or statements, which were fakes pure and simple, have been very widely disseminated we feel impelled to make correction. The real facts were as follows:

On the morning of December 17th, between the hours of 10:30 o'clock and noon, four flights were made, two by Orville Wright and two by Wilbur Wright. The starts were all made from a point on the level sand about two hundred feet west of our camp, which is located a quarter of a mile north of the Kill Devil sand hill, in Dare County, North Carolina. The wind at the time of the flights had a velocity of 27 miles an hour at ten o'clock, and 24 miles an hour at noon, as recorded by the anemometer at the Kitty Hawk Weather Bureau Station. This anemometer is thirty feet from the ground. Our own measurements, made with a hand anemometer at a height of four feet from the ground, showed a velocity of about 22 miles when the first flight was made, and 20 1/2 miles at the time of the last one. The flights were directly against the wind. Each time the machine started from the level ground by its own power alone no assistance from gravity, or any other source whatever. After a run of about 40 feet along a monorail track, which held the machine eight inches from the ground, it rose from the track and under the direction of the operator climbed upward on an inclined course till a height of eight or ten feet from the ground was reached, after which the course was kept as near horizontal as the wind gusts and the limited skill of the operator would permit. Into the teeth of a December gale the "Flyer" made its way forward with a speed of ten miles an hour over the ground and thirty to thirty-five miles an hour through the air. It had previously been decided that for reasons of personal safety these first trials should be made as close to the ground as possible. The height chosen was scarcely sufficient for maneuvering in so gusty a wind and with no previous acquaintance with the conduct of the machine and its controlling mechanisms. Consequently the first flight was short. The succeeding flights rapidly increased in length and at the fourth trial a flight of fifty-nine seconds was made, in which time the machine flew a little more than a half mile through the air, and a distance of 852 feet over the ground. The landing was due to a slight error of judgment on the part of the aviator. After passing over a little hummock of sand, in attempting to bring the machine down to the desired height, the operator turned the rudder too far; and the machine turned downward more quickly than had been expected. The reverse movement of the rudder was a fraction of a second too late to prevent the machine from touching the ground and thus ending the flight. The whole occurrence occupied little, if any, more than one second of time.

Only those who are acquainted with practical aeronautics can appreciate the difficulties of attempting the first trials of a flying machine in a twenty-five mile gale. As winter was already well set in, we should have postponed our trials to a more favorable season, but for the fact that we were determined, before returning home, to know whether the machine possessed sufficient power to fly, sufficient strength to withstand the shocks of landings, and sufficient capacity of control to make flight safe in boisterous winds, as well as in calm air. When these points had been definitely established, we at once packed our goods and returned home, knowing that the age the flying machine had come at last.

From the beginning we have employed entirely new principles of control; and as all the experiments have been conducted at our own expense without assistance from any individual or institution, we do not feel ready at present to give out any pictures or detailed description of the machine.

Document 1-14(a–d)

(a) Wilbur and Orville Wright, letter to the Secretary of War, Dayton, Ohio, 9 October 1905, *The Papers of Wilbur and Orville Wright*, pp. 514–515.

(b) U.S. Army Signal Corps, "Specification No. 486, Advertisement and specification for a heavier-than-air flying machine," issued 23 December 1907, copy in Milton Ames Collection, Historical Archives, NASA Langley Research Center, Hampton, Virginia.

(c) Wilbur and Orville Wright, letter to General James Allen, U.S. Army Signal Corps, 27 January 1908, *The Papers of Wilbur and Orville Wright*, p. 856.

(d) Octave Chanute, excerpts from "Recent Aeronautical Progress in the United States," *The Aeronautical Journal* [London] (12 July 1908): 52–55.

While the anniversary of the invention of the airplane is marked from the Wright brothers' successful test flights of 1903, the inaugural of the airplane's development into a practical, useable device was marked by a subsequent improved Wright airplane's successful completion of government trials, which led to the Wright's first sale of a flying machine on 2 August 1909.

This significant step in the development of aviation is marked through the following four documents, and conclude this chapter. In 1905, Wilbur and Orville Wright initiated a correspondence with the Secretary of War offering their aircraft for sale to the government after a public demonstration of its capabilities. After a further bit of prodding, in 1907 the United States Army issued the second document, an official statement of specifications regarding the aerodynamic performance of a flying machine to be used by the Signal Corps as an observation aircraft. The third document is the Wright brothers' responding bid. Rounding out this group of documents is a summary by Octave Chanute published in Great Britain reviewing the "Recent Aeronautical Progress in the United States."

With Wilbur away in Europe demonstrating one of their airplanes to astonished crowds, Orville was left to pilot the airplane for the Army. The military tests were held at Fort Myer, Virginia, just outside of Washington, D.C., beginning on 3 September 1908. A week of impressive demonstrations, in which the aircraft met or exceeded requirements, dramatically opened the eyes of America to the amazing technological achievement of the Wright brothers and the promise of aviation. Thereafter, not even the unfortunate crash on 17 September that injured Orville and killed his official passenger, Lieutenant Thomas E. Selfridge, could stifle the growing appreciation that the day of the airplane was finally at hand.

Orville recovered from his injuries, and with Wilbur's help the tests resumed in July 1909. In the government contract awarded to the Wrights following the acceptance of their flying machine by the U.S. Army, the brothers received $30,000 for the delivery of one aircraft and the training of two pilots. The price included a $5,000 bonus for exceeding the stipulated speed requirement by a recorded average speed of 42.583 miles per hour over the 10-mile test course.

Document 1-14(a), letter from the Wright brothers to the Secretary of War, 9 October 1905.

Some months ago we made an informal offer to furnish to the War Department practical flying machines suitable for scouting purposes. The matter was referred to the Board of Ordnance and Fortification, which seems to have given it scant consideration. We do not wish to take this invention abroad, unless we find it necessary to do so, and therefore write again, renewing the offer.

We are prepared to furnish a machine on contract, to be accepted only after trial trips in which the conditions of the contract have been fulfilled; the machine to carry an operator and supplies of fuel, etc., sufficient for a flight of one hundred miles; the price of the machine to be regulated according to a sliding scale based on the performance of the machine in the trial trips; the minimum performance to be a flight of at least twenty-five miles at a speed of not less than thirty miles an hour.

We are also willing to take contracts to build machines carrying more than one man.

Document 1-14(b), U.S. Army Signal Corps, "Specification No. 486, Advertisement and specification for a heavier-than-air flying machine," 23 December 1907.

Signal Corps Specification, No. 486

Advertisement and specification for a heavier-than-air flying machine.

To the Public:

Sealed proposals, in duplicate, will be received at this office until 12 o'clock noon on February 1, 1908, on behalf of the Board of Ordnance and Fortification for furnishing the Signal Corps with a heavier-than-air flying machine. All proposals received will be turned over to the Board of Ordnance and Fortification at its first meeting after February 1 for its official action.

Persons wishing to submit proposals under this specification can obtain the necessary forms and envelopes by application to the Chief Signal Officer, United States Army, War Department, Washington, D.C. The United States reserves the right to reject any and all proposals.

Unless the bidders are also the manufacturers of the flying machine they must state the name and place of the maker.

Preliminary.—This specification covers the construction of a flying machine supported entirely by the dynamic reaction of the atmosphere and having no gas bag.

Acceptance.—The flying machine will be accepted only after a successful trial flight, during which it will comply with all requirements of this specification. No payments on account will be made until after the trial flight and acceptance.

Inspection.—The Government reserves the right to inspect any and all processes of manufacture.

General Requirements

The general requirements of the flying machine will be determined by the manufacturer, subject to the following conditions:

1. Builders must submit with the proposals the following:

(a) Drawings to scale showing the general dimensions and shape of the flying machine which they propose to build under this specification.

(b) Statement of the speed for which it is designed

(c) Statement of the total surface area of the supporting planes

(d) Statement of the total weight.

(e) Description of the engine which will be used for motive power.

(f) The material of which the frame, planes, and propellers will be constructed. Plans received will not be shown to other bidders.

2. It is desirable that the flying machine should be designed so that it may be quickly and easily assembled and taken apart and packed for transportation in army wagons. It should be capable of being assembled and put in operation in about one hour.

3. The flying machine must be designed to carry two persons having a combined weight of about 350 pounds, also sufficient fuel for a flight of 125 miles.

4. The flying machine should be designed to have a speed of at least forty miles per hour in still air, but bidders must submit quotations in their proposals for cost depending upon the speed attained during the trial flight, according to the following scale:

40 miles per hour, 100 per cent.

39 miles per hour, 90 per cent.

38 miles per hour, 80 per cent.

37 miles per hour, 70 per cent.
36 miles per hour, 60 per cent.
Less than 36 miles per hour rejected
41 miles per hour, 110 per cent.
42 miles per hour, 120 per cent.
43 miles per hour, 130 per cent.
44 miles per hour, 140 per cent.

5. The speed accomplished during the trial flight will be determined by taking an average of the time over a measured course of more than five miles, against and with the wind. The time will be taken by a flying start, passing the starting point at full speed at both ends of the course. This test subject to such additional details as the Chief Signal Officer of the Army may prescribe at the time.

6. Before acceptance a trial endurance flight will be required of at least one hour during which time the flying machine must remain continuously in the air without landing. It shall return to the starting point and land without any damage that would prevent it immediately starting upon another flight. During this trial flight of one hour it must be steered in all directions without difficulty and at all times under perfect control and equilibrium.

7. Three trials will be allowed for speed as provided for in paragraphs 4 and 5. Three trials for endurance as provided for in paragraph 6, and both tests must be completed within a period of thirty days from date of delivery. The expense of the tests to be borne by the manufacturer. The place of delivery to the Government and trial flights will be at Fort Myer, Virginia.

8. It should be so designed as to ascend in any country which may be encountered in field service. The starting device must be simple and transportable. It should also land in a field without requiring a specially prepared spot and without damaging its structure.

9. It should be provided with some device to permit of a safe descent in case of an accident to the propelling machinery.

10. It should be sufficiently simple in its construction and operation to permit an intelligent man to become proficient in its use within a reasonable length of time.

11. Bidders must furnish evidence that the Government of the United States has the lawful right to use all patented devices or appurtenances which may be a part of the flying machine, and that the manufacturers of the flying machine are authorized to convey the same to the Government. This refers to the exclusive purchase of patent rights for duplicating the flying machine.

12. Bidders will be required to furnish with their proposal a certified check amounting to ten per cent of the price stated for the 40-mile speed. Upon making the award for this flying machine these certified checks will be returned to the bidders, and the successful bidder will be required to furnish a bond, according to Army

Regulations, of the amount equal to the price stated for the 40-mile speed.

13. The price quoted in proposals must be understood to include the instruction of two men in the handling and operation of the flying machine. No extra charge for this service will be allowed.

14. Bidders must state the time which will be required for delivery after receipt of order.

James Allen
Brigadier General, Chief Signal Officer of the Army.
Signal Office,
Washington, D. C., *December 23, 1907*.

Document 1-14(c), letter from Wilbur and Orville Wright to General James Allen, 27 January 1908.

We herewith inclose a bid for furnishing the Signal Corps with a heavier-than-air flying machine, in accordance with Specification No. 486, of December 23, 1907, together with a certified check for two thousand five hundred dollars ($2,500.00).

The machine we propose to deliver is designed to weigh between 1,100 and 1,250 lbs. with two men on board, and for a speed of forty miles an hour. It will have an area of 500 square feet in the supporting planes; and will be propelled by a four-cycle, water-cooled gasoline motor. The frames of the planes will be constructed of spruce and ash covered with cotton muslin; the propellers of spruce and linen.

We have made the date of delivery of the machine 200 days, in order to provide sufficient time for increasing the speed of the machine now under construction, in case Requirement No. 5 is to be interpreted literally. If, however, Requirement No. 5 is interpreted to mean an average of the speeds with and against the wind over a measured course, which is the correct method to give and average corresponding to flight made in still air, as specified in Requirement No. 4, we would be able to make delivery at a much earlier date.

We inclose a photograph of our machine of 1905, which was similar to the one we now propose to furnish. We would request that this, as well as the drawings, be kept confidential.

Document 1-14(d), Octave Chanute, excerpts from "Recent Aeronautical Progress in the United States," London, 1908.

The public attitude in the United States in 1906 and 1907 concerning aërial navigation has been one of expectancy and apathy. The announcements of the marvellous success achieved by Wright Brothers, which every investigation seemed to confirm, must have deterred many searchers from experimenting at all, until they know how much remained to be accomplished in aviation. . . .

Meanwhile Aëro Clubs have sprung up like mushrooms all over the country. The leading one still is the Aëro Club of America in New York, which was organized in 1905. It has held several exhibitions, has promoted the publication of an interesting book, "Navigating the Air," the establishment of a correspondence School of Aëronautics by Mr. Triaca, and an effort is now being made in connection with the Club to raise a fund of £5,000 to be offered in prizes for Aviation.

In St. Louis and Chicago local aëro clubs propose to organize balloon races to be held in 1908. In these and other cities, members are encouraged to engage in the sport by owning and riding balloons themselves, and it remains to be seen how long the enthusiasm will last.

Two monthly Aëronautical Magazines have been started, one in New York and one in St. Louis, but it is yet to be ascertained how well they will be supported.

The Jamestown Exposition of 1907 organized an aëronautical exhibit which amounted to but little, as well as an Aëronautical Congress which brought out few papers, but searchers have been building apparatus to be experimented with in the summer of 1908.

The most distinguished of these is Dr. Alexander Graham Bell, the inventor of the Telephone, who has been experimenting with his tetrahedral kite. He tested on December 6th, 1907, his gigantic man-lifting kite "Cygnet," consisting of 3,393 wing cells, presenting 1,966 square feet of oblique surfaces, and weighing with the floats and passenger an aggregate of 600 pounds. This was towed into the middle of a lake and raised against a wind of 21 miles an hour by a tug-boat. It exhibited that perfect stability which all previous experiments indicated, and upon the wind's dying away it descended gently from a height of 168 feet, but was broken on striking the water. It is to be tested again during the summer of 1908 with a view to eventually apply a motor. There is no question as to the automatic equilibrium of this form of apparatus, but it is possible that the inferior lifting power of the oblique surfaces and the resistance of so many front edges will make the design less favorable for a flying machine than other forms.

Dr. Bell then generously provided the means (he gives credit to his wife) and organized the so-called "Aërial Experiment Association" with its headquarters at Hammondsport, New York, to give his assistants a chance to experiment their own ideas. The first result was the construction of the motor-driven aëroplane "Red Wing," chiefly designed by Lieutenant T. Selfridge, which made its trial trip on sleigh runners on the ice of Lake Leuka on March 9th, 1908. It is a double-decked apparatus 43 feet across, the surfaces being arched both fore and aft and from tip to tip; the upper aëroplane being bowed downward somewhat in the attitude of the gull and the lower aëroplane, which is 6 feet shorter, being bowed upward somewhat to the attitude of the vulture when soaring. The total surface is 386 square feet and the total weight, including the aviator, is 570 pounds. It is driven by a Curtiss motor of 40-horse power, actuating a screw propeller.

At the very first attempt the apparatus left the ice after travelling only 200 feet and flew a total distance of 319 feet from the point where it left the ice to the point of descending. It alighted somewhat clumsily and broke one strut, this being the first public exhibition of the flight of a heavier-than-air machine in America. . . .

The main interest, however, attaches to the pending United States Government tests of flying machines which are under contract for delivery next August. On December 23rd, 1907, the United States Army Signal Corps issued invitations to tenders, which produced much amazement. European and American journals have said that these specifications assume that flying machines are almost a usual method of transportation, and that the terms are so exacting as to seem unreasonable. The Signal Services officers answer that the specifications were drawn up after interviews with some of the inventors and merely cover what they said they could perform, while some clauses were added to prevent the Government's being trifled with, and that the tests will be conducted with judicious reason and liberality. More especially does this apply to the granting but three trials each for the speed test and the endurance test of one hour, which might be defeated on each occasion by some fortuitous and trifling circumstance. . . .

The performances of the Wright Brothers have been viewed with incredulity because of the mystery with which they have been surrounded in the hope of a rich money reward, yet it is now generally conceded that they have accomplished all that they have claimed, i.e., to have made a first dynamic flight in 1903, to have mastered circular courses in 1904, making 105 flights, the longest of which was three miles, and to have obtained thorough control over their apparatus in 1905, making 49 flights, the longest of which was 24 miles, consisting of 30 sweeps over a circular course at an average speed of 38 miles an hour. Since then they have made no flights, having been engaged in negotiations with a view to marketing their invention.

Now they have made a contract with the United States Government to furnish a flying machine under those formidable specifications. They set to work at once. They have built parts of more than one machine, so as to guard against bad breakages, and have returned to their old experimental grounds near Kitty Hawk, North Carolina. This is situated on a long sand spit, two or three miles wide, between the waters of Pamlico Sound and the Atlantic Ocean. It is about as inaccessible a spot near civilisation [sic] as can well be, being almost a desert, occupied by a few fishermen and a Government lifesaving station. Near the camp is "Kill Devil Hill," a cone of drifted sand about 100 feet high, on which former gliding experiments were made.

Here the Wrights have established themselves and begun their practice, for it is only by strenuous practice that the mastery of the air is to be obtained. They are said to be proceeding with great caution, testing every part and peculiarity of the machine, the longest flight yet reported (May 14th, 1908) being eight miles, followed, however, by a serious breakage on landing, said to be due to a false maneuvre, in consequence of some change in the location of the levers which control the rudders. It is stated that the wreck was so complete that the parts will be shipped back to Dayton, Ohio, where the craft will be rebuilt.

The Wrights are understood to have until August 27th to deliver their machine to the Signal Corps for testing, so that there will be sufficient time to resume practice after the machine is repaired. Whether this practice will take place on the same ground or elsewhere is not known. The spot is very secluded, but the ubiquitous reporter has found the camp and is sending "news" both true and untrue, to the great annoyance of [the] Wright Brothers. . . .

An amusing struggle has resulted. The reporters are frantic for information, and the Wrights most determined that no description be given of their apparatus. It is probable that many contradictory cablegrams will have been received in Great Britain when the present paper reaches the Hon. Secretary.

The Wright brothers stand a fair chance of passing the tests and having their machine accepted. They may be defeated by some accident during the preliminary trials or the formal tests, but the present writer is sure that all the members of the Aëronautical Society of Great Britain will join him in the hope that the best of luck will attend the demonstration.

Document 1-15

Simon Newcomb, "Aviation Declared a Failure," *Literary Digest* 37, 17 October 1908, 549.

The press was not the only group to bungle the Wright brothers' story and to minimize the importance of what they had achieved and what the flying machine signified for society. One of the most vocal "bah-humbugs" in the early 1900s happened to be one of the greatest scientists of his day, Professor Simon Newcomb (1835–1909).

Newcomb was a giant in the field of celestial mechanics. His work in the late nineteenth century on the orbital motion of the planets of the Solar System provided the cornerstone of the nautical and astronomical almanacs of the United States and Great Britain, not just in the early 1900s but as recently as 1984. Albert Einstein acknowledged the importance of Newcomb's work in the development of his own theory of relativity. Born in England, he immigrated in 1854 at age 19 to Maryland to join his father. Newcomb taught himself most of the mathematics and astronomy he knew, taking a job in 1857 in the American Nautical Almanac Office, which was located in Cambridge, Massachusetts. Admitted to Harvard, he graduated in 1858, and three years later he took an appointment to the Naval Observatory at Washington, D.C. The next ten years he spent determining the positions of celestial objects using various telescopes, some of his own design. In 1877, at age forty-two, he became director of the American Nautical Almanac Office, now located in Washington, D.C., and initiated the work on celestial motion for which he would become most famous. In 1884, Newcomb became professor of mathematics and astronomy at The Johns Hopkins University, staying there until 1893. He served as editor of the *American Journal of Mathematics* for many years and was a founding member and the first president (1899–1905) of the American Astronomical Society. He also served as president of the American Mathematical Society from 1897 to 1898. By the time the Wright brothers made their historic first flight in 1903, Newcomb had risen to the top of the astronomical community in the United States, the recipient of many of the highest national and international awards given to scientists, including Fellow of the British Royal Society.

Newcomb loved to travel. He spoke French and German fluently and enough Italian and Swedish to travel easily in those countries. An avid hiker, at age seventy (in 1905) he climbed to the chalet high up the side of the Matterhorn in the Swiss Alps, a feat almost unprecedented for a man of his age. One may not expect that such a brilliant and adventurous man would be so pessimistic about the future of aviation, but no one was more outspoken or pungent in his pessimism about the usefulness of flying than Newcomb.

What follows is an article published in the *Literary Digest* in October 1908 revealing Newcomb's ongoing pessimism. Newcomb in fact had been writing about aviation in condescending terms since the 1890s. In one article published in *McClure's Magazine* around the turn of the century, Newcomb had written: "Man's desire to fly like a bird is inborn in our race, and we can no more be expected to abandon the idea than the ancient mathematician could have been expected to give up the problems of squaring the circle." In other words, he believed the problems of flight could never be solved. In the article below, Newcomb's skepticism about the future of aviation is quoted at length. The caption under the picture of Newcomb that ran with the story read "The eminent American astronomer. He doubts if aviation will ever be of much practical value."

Newcomb's was not the only voice still expressing such negativism. In 1910, the *New York Telegraph* called a proposal from a Texas congressman for a study of the possibility of U.S. airmail operations "ludicrous." The paper mocked: "Love letters will be carried in rose-pink aeroplane, steered with Cupid's wings and operated by perfumed gasoline" [quoted in Roger E. Bilstein, *Flight in America: From the Wrights to the Astronauts* (Baltimore, MD: Johns Hopkins University Press, 1984), pp. 16–17].

Though Newcomb died in 1909, his attitudes and others like them did not fade as quickly as one might think. But as aviation caught Americans' attention in the early 1910s, the public's misgivings greatly diminished. When the airplane proved a dynamic instrument in the Great War (1914–1918), most doubt about the practicality of aviation disappeared. Still today, though, there are millions of people who share the basis of Newcomb's resistance to flying. As he is quoted as saying in the article below: "Is it not evident, on careful consideration, that the ground affords a much better bade than air ever can? Resting upon it we feel safe and we know where we are. In the air we are carried about by every wind that blows."

Though the world's emerging aerodynamic specialists might surely have argued with Newcomb's point of view, not even they could undervalue the many dangers, uncertainties, and inefficiencies of machines flying through the air. In an important sense, aerodynamicists would be spending the next 100 years answering Newcomb's complaint.

Document 1-15, Simon Newcomb, "Aviation Declared a Failure," 1908.

A pessimist has been defined as a man who, when offered the choice of two evils, takes both. Prof. Simon Newcomb in his recent article on "The Problem of Aerial Navigation" (*The Nineteenth Century*, London, September) varies the formula somewhat. When offered two perfectly good methods of navigating the air, he rejects both. He concludes that the disadvantages of both dirigible balloons and aeroplanes outweigh all existing and possible advantages, and that the solid earth is good enough for him, anyhow. We have not space for the professor's extended bill of particulars, but will proceed at once to his general statement of the case against aviation. First summing up the advantages in one paragraph, only to overwhelm them in the next, be says:

"Let us . . . in fairness see what is to be placed on the credit side. First and almost alone among these is the fact that steam transportation on land requires the building of railways, which are so expensive that the capital involved in them probably exceeds that invested in all other forms of transportation. Moreover, there are large areas of the earth's surface not yet accessible by rail, among which are the poles and the higher mountains. All such regions, the mountains excepted, we may suppose to be attainable by the perfected air-ship of the future. The more carefully we analyze these possible advantages, the more we shall find them to diminish in importance. Every part of the earth's surface on which men now live in large numbers, and in which important industries are prosecuted, can now be reached by railways, or will be so reached in time. True, this will involve a constantly increasing investment of capital. But the interest on this investment will be a trifle in comparison with the cost and drawbacks incident to the general introduction of the best system of aerial transportation that is even ideally possible in the present state of our knowledge. . . . "

"May we not say . . . that the efforts at aerial navigation now being made are simply most ingenious attempts to substitute, as a support of moving bodies, the thin air for the solid ground? And is it not evident, on careful consideration, that the ground affords a much better base than air ever can? Resting upon it we feel safe and know where we are. In the air we are carried about by every wind that blows. Any use that we can make of the air for the purpose of transportation, even when our machinery attains ideal perfection, will be uncertain, dangerous, expensive, and inefficient, as compared with transportation on the earth and ocean. The glamour which surrounds the idea of flying through the air is the result of ancestral notions, implanted in the minds of our race before steam transportation had attained is present development. Exceptional cases there may be in which the air-ship will serve a purpose, but they are few and unimportant."

Professor Newcomb admits that in certain special cases flyers or balloons may accomplish what could be done in no other way. For instance, he thinks it not unlikely that the pole may be first reached by a dirigible balloon. The balloon as an engine of war, however, he regards as an impossibility, and he reassures the Englishmen who have been looking forward apprehensively to a vertical bombardment of British towns by a fleet of German dirigibles. He says in conclusion:

"In presenting the views set forth in the present article the writer is conscious that they diverge from the general trend, not only of public opinion, but of the ideas of some able and distinguished authorities in technical science, who have given encouragement to the idea of aerial navigation. Were it a simple question of weight of opinion he would frankly admit the unwisdom of engaging in so unequal a contest. But questions of what can be done through the application of mechanical power to bodies in motion have no relation to opinion. They can be determined only by calculations made by experts and based upon the data and principles of mechanics."

"If any calculations of the kind exist, the writer has never met with them, nor has he ever seen them either quoted or used by any author engaged in discussing the subject. So far as his observation has extended, the problem has been everywhere looked upon as merely one of experiments ingeniously conducted with all the aid afforded by modern apparatus. He has seen no evidence that any writer or projector has ever weighed the considerations here adduced, which seem to him to bring out the insuperable difficulties of the system he has been discussing, and the small utility to be expected from it even if the difficulties were surmounted."

If he is wrong in any point—and he makes no claim to infallibility—it must be easy to point out in what his error consists. He therefore concludes with the hope that if his conclusions are ill-founded their fallacy will be shown, and that if well-founded they may not be entirely useless in affording food for thought to those interested in the subject.

Experts in aviation and the members of aero-clubs will not be apt to agree with all this, but it will certainly be useful in counteracting the ardor of those enthusiasts who think that we are all going to fly to Europe before the end of 1909.