AERONAUTICS

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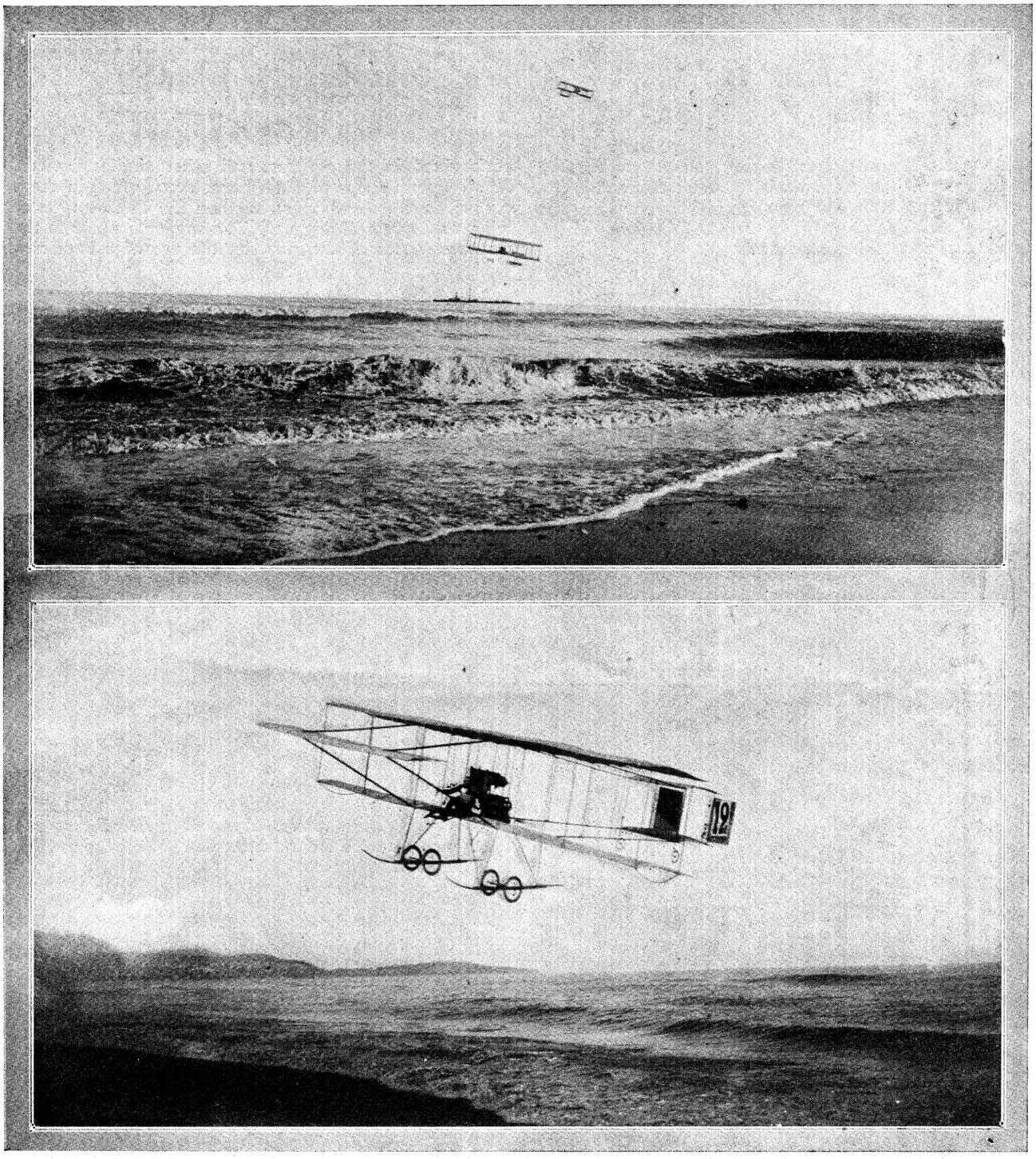


The Nice Aviation Meeting
Above, Rougier on a Voisin biplane and Duray on a Farman biplane passing over the torpedo-boat.
Below, Duray on a Farman biplane turning over the sea.

machine. It only carried ailerons on the top plane. Only in the further particular that it is provided with a double rudder, does it otherwise differ from Grahame-White's biplane. Both machines were fitted with an elevator-flap forming the continuation of the upper tail surface and working in conjunction with the forward elevator. The combined chassis is identical; as is the 50 H.P. rotary Gnome engine driving a 6 ft. 8 in. Chauvière propeller.

The arrangements in connection with the flights were excellent. On his first flight Grahame-White was convoyed by several motor cars, only one of which, Mr. Coleman's white steam car, carrying Mr. Perrin and the mechanics, reached Rugby before him. Paulhan throughout was piloted by a special train. During his night flight again Grahame-White was assisted by several motor cars.

The significance of the achievement is so obvious that little more remains to be said, save to pay a tribute to Mr. Grahame-White's splendid sportsmanship and the requisite meed of congratulation to M. Paulhan. To the cause of aviation in general the issue of the London to Manchester flight has rendered invaluable service: henceforward the aeroplane enters upon its career of practical utility.

The Origin of "Warping"

Professor Montgomery's Experiments

Professor J. J. Montgomery delivered a most interesting address before the members of The Aeronautical Society, New York, on April 21. He gave the history of his experiments with wing-warping gliders and illustrated the movements of air currents about an aeroplane surface.

The following is an abstract of his lecture:*

My first practical experiments commenced about 1883 and were continued until 1886. The first machines which I attempted to build were of the flapping order, hoping to rise from the ground by some flapping movement. I built three of these machines without any indication of success with man-power. Giving this up for the time, I turned my attention to the study of gliding, hoping to solve some of the mysteries of the phenomenon of soaring.

The first machine that was constructed was modelled after the gull's wing—following it blindly, going against my reason in the matter, but following Nature. The wings of birds, as you know, are curved and if properly placed at a small angle to the wind, or to the direction of movement, the front surface is inclined down. This was a stumbling block to me.

The first experiments with this crude device were a success. The apparatus measured 20 ft. spread and an average depth, fore and aft, of 4½ ft. I took this apparatus to the top of a hill, facing a gentle wind. There was a little run and a jump and I found myself launched in the air. I proceeded against the wind, gliding down hill for a distance of about 600 ft. In this experience I was able to direct my course at will.

A peculiar sensation came over me. The first, in placing myself at the mercy of the wind, was that of fear. Immediately after came a feeling of security when I realised the solid support given by the wing-surface—and the support was of a very peculiar nature. There was a cushiony softness about it, yet it was firm. When I found the machine would follow my movements in the seat for balancing, I felt I was self-buoyant.

This experience led to what is now a very important question, one that is agitating the whole country, the question of wing-warping.

Wing-warping was born at this moment. I say this, because it is important in the study of aeronautics to have the problems thoroughly fixed. I commenced then to study the question of control of the machine. You will notice from what I have said that the first machine I made was successful as a glider, that is, it had great power because I blindly followed the surface provided by Nature. It was defective in its equilibrium or control. I went to Nature to study the principle of control. I watched the movements of the vultures and detected in their actions the twisting of the wing. That gave the solution. Then I resumed my work. I was not able to build the wing as the bird's is built, so resorted to the first simple device. But, also, while I followed the principle of equilibrium as presented in Nature, I departed from the form of surface because it seemed unreasonable that the wing should be inclined downward at the front.

Therefore, the second machine was made with flat surfaces. In 1885 I built the second machine, somewhat larger than the one previous, and to afford side equilibrium each wing was hinged diagonally. This diagonal hinge allowed the "flaps" thus formed to yield to undue pressure on either side. These flaps were held by springs in a normal position. If the wind pressure became excessive on one side, the flap of that wing would yield up a little.

Before making this machine, however, I tried the experiment with kites. By the way, I performed all my first experiments with kites. I built a kite after this manner and found it had perfect equilibrium in gusty winds, and concluded it was a practical device and a proper means of equilibrium.

But, in addition to the spring, I had a saddle, which was so constructed that by leaning to one side or the other the rear portion of that wing would yield (the saddle had an upright piece and this was attached by wires running to the rear portions of the wings). If a gust of wind came from the left and I wished to relieve that side, my only inclination would be to lean to the left, and *vice versa*.

I found that when I took that machine and faced the wind that its equilibrium was perfect, that is, I found no conditions under which I could not control it so it would not upset me, a thing I could not do with the first machine. When I attempted to glide I found its power of gliding was far inferior to that of the first. Immediately I concluded I had not found the right surface.

Then I built the third machine. In this machine, in a way, I copied Nature in regard to surface and in a way I departed from it. The wings were formed more or less like those of the soaring vulture, with this exception: I could not bring myself to the belief that the surface curved down in the front was the proper surface. Therefore, I compromised by turning the front edge up a little and the rest of the wing was more or less like that of the vulture. The two wings were placed at a dihedral angle.

Now in this machine I carried out the warping principle in a different way. There was a lateral beam along the front of the wings. These two beams were capable of being rotated in a socket in the frame extending backward to the tail. Wires from the rear of each wing ran to levers, one for each wing, placed at the right and left hands of the operator, who sat on a seat as in the other machines. By these levers I could bring both wings down together, or independently. That machine was perfect in control; whether the wind was regular or gusty I had the machine under control by changing the angles of the wings. This had larger surface even than the second, but was inferior in lifting power.

Immediately I found I did not have the proper form of surface as it did not have the same lifting power under the same conditions as the first machine.

The account I have just given I gave to the Chicago Congress in 1893, and is more briefly stated in Mr. Chanute's book "Progress in Flying Machines." He describes the experiments and the machines. From this you will see that warping of surfaces is not a new question.

But I was not at all satisfied with my work because I was floundering in the darkness, and didn't know where to turn in order to determine a true surface. It was all mystery to me. I concluded we knew little or nothing of aerodynamics, for I had searched the books and read magazines and papers for suggestions. I took the machine apart and commenced at the bottom to study if possible the laws of aerodynamics, and determine the proper form of surface to give such phenomena as the soaring of birds.

In 1885 or 1886, I constructed a whirling table. This consisted of a couple of rails fastened together and mounted on a pivot. On the end of this I fastened surfaces of different forms and whirled the table so as to study the movements of these surfaces. I no sooner had commenced than I detected a peculiar phenomenon which suggested there was something taking place in advance of the surface. In order to test this I resorted to a number of experiments, particularly one which I described to the Chicago Congress in 1893. I had thistle-down scattered in the wind so as to detect the direction of the wind. Having done this I took a large barn door and set it on the ground at an angle of about ten degrees. Immediately I noticed a reaction on the wind in front. Instead of the wind coming in a straight line it came in a gradual curve and rose to strike the surface, indicating that the surface had an action on the wind in front of it. Then I readily saw the reason for the curving of the surface of a bird's wing. I made this known to the Congress and also a series of studies relative to the forms of a bird's wings, the ratio between weight carried and the curvature of a bird's wing. Mr. Chanute and Dr. Zahm were much interested in my work and gave me such encouragement that I continued and completed the whole series, but owing to various circumstances was not able to publish the results.

In 1903 I was able to commence my investigations again and, having discovered some of the fundamental laws, I was able to put them into practice in the machines which I built. These were built strictly on the lines of science. I simply studied my own figures and made the first model. These were tested in various ways. I stretched a cable between two mountains so that it was 150 ft. high above the valley. With cords I would

* In publishing this report we are indebted to the courtesy of the editor of *Aeronautics* (New York).

elevate these models and liberate them in all possible ways, upside down, tail down, and in every conceivable manner. They would glide safely to the ground no matter how they were liberated. In these I simply used the warping idea which I had developed in 1885 and 1886.

After I found these models were perfect in their equilibrium and would follow any direction that I chose by giving them the proper warping, I built a large machine patterned exactly after them. I did not change one iota from the plans which I had drawn after studying my own papers following out the theory.

In order to make the test practical with the large machine, in 1904 I took them down to the mountains about 100 miles below Santa Clara, to San Juan, and with the assistance of three cowboy friends I performed a series of experiments. I elevated these between poles on a cable and dropped them with and without weight. Finding them perfect I got in, and with a running jump glided down the hill. A peculiar thing I found was that it would respond very rapidly to a change of the wind. I discovered this very unexpectedly. The long hill which I was in the habit of using had at its base a sort of canyon. At the top of the hill the wind came in the direction that I faced. Below, it blew up the canyon directly at right angles to the wind above. I was gliding down the hill when quick as a flash I was whirled at right angles to the first wind, but was not upset.

Then I attempted to give a series of exhibitions and develop the machine further. For that purpose I secured a hot-air balloon-man and parachute jumper. I was anxious to commence the experiments of raising a man in the air and dropping him short distances for the first flights. But my parachute jumper had his own ideas. He insisted upon being raised at least 1,000 ft. high the first time. It was an ordeal for me. But there was nothing left for me to do. I either had to give up or let him go up. So I made the adjustments with my machine in such a way that it was impossible for him to get control of the machine and make a mistake and hurt himself. There were certain clamps that controlled the tail and wings that gave him limited action.

So he went up a thousand feet, cut loose and made the first time a very beautiful glide. Then the second time I gave him a little more liberty and he made probably one of the finest glides I ever saw.

He went up about 3,000 ft. in the mountain regions of Santa Cruz. As he cut loose from the machine he lost his direction. We told him to come back to the starting point. He started to fly towards a distant city. In five or six minutes he detected his mistake, turned round and started to fly towards us, and in coming towards us he passed through two or three clouds. This was a beautiful sight. Finally, he came back near the point of starting. He could not make the exact point for he had lost a great deal of elevation in making his flights and there was an intervening forest of tall trees which he did not like to try crossing without good headway, so he made a circle and came to the earth. After that I continued my experiments at Santa Clara.

In 1905, one of my riders (Maloney) was killed. Hot-air balloons rise very quickly, and it was necessary to provide some means for retarding the upward rush. This was effected by ropes running through rings. In Maloney's last flight, one of these ropes caught in part of the machine. We called out to Maloney that the aeroplane was broken, but evidently he did not hear. When he got up about 3,000 ft. high he cut loose, the machine turned over and he descended with the machine upside down. He did not seem to be going any faster than a man dropping in a parachute. When we got to him the machine was broken and he was senseless. Six physicians examined him, but found no mark on him except a scratch on the head from a wire. There was no blood and no bones broken. The physicians concluded he had heart trouble. I continued to build other aeroplanes, giving other exhibitions until the San Francisco earthquake. This wrought such a disaster that I had to turn my attention to other subjects and let the aeroplane rest for a time.

In concluding his talk, Professor Montgomery gave credit to the other pioneers, Langley, Chanute and Zahm. He said: We have given our work for the good of mankind and we want to see mankind benefit, and we deprecate the idea of anybody monopolising the subject. Right here is one thing I want to state. Mr. Chanute is a patriot, and I remember well the words he said to me in 1893. We held long conferences at his residence. I had developed this principle of warping. I was rather slow in making it known to Mr. Chanute. I gave him to understand that I had developed certain ideas regarding equilibrium that I wished to patent. Then Mr. Chanute said to me he did not believe anyone should take out patents on any devices because he said this was a problem of humanity. No one man was going to solve it. All should lend their work to the solution of this great problem and anyone taking out patents might interfere with the progress of the science. When Mr. Chanute expressed himself that way to me I saw the full truth of what he said, and appreciated it. Then for the first time I made known to him the device by which I controlled the lateral equilibrium, the warping.

In reply to a question as to why Mr. Chanute and himself did take out patents, Prof. Montgomery said that while patents are generally considered a form of monopoly, there is another side to the question. A patent is a protection to the inventor permitting him to use his own invention without hindrance.

Forthcoming Meetings

British Meetings

May 14–21 ...	...	Huntingdon
June 4–11 ...	...	Doncaster
June 25–July 2	...	Wolverhampton
*July 11–17 ...	...	Bournemouth
*Aug. 6–13 ...	...	Lanark

Foreign Meetings

*April 30–May 5	...	Tours ...	£1,800
*May 7–15 ...	...	Lyon ...	£8,000
*May 10–16 ...	...	Berlin ...	—
May 15–23 ...	...	Marseille	£1,000
*May 16–25 ...	...	St. Petersburg	£4,000
May 17–21 ...	...	Palermo	£5,000
*May 20–30 ...	...	Verona	£8,400
May 27–31 ...	...	Limoges	£1,200
June 4–7 ...	...	Angers	£1,200
June 4–12 ...	...	Munich	—
June 5–12 ...	...	Vichy ...	£1,200
*June 5–15 ...	...	Budapest	£18,400
June 10–26 ...	...	Rouen ...	£8,000
June 18–26 ...	...	Danzig	—
*July 3–10 ...	...	Reims ...	£10,000
July 10–17 ...	...	Leipzig	—
*July 24–Aug. 4	...	Brussels	£8,000
July 25–Aug. 2	...	Caen ...	£2,000
Aug. 7–13 ...	...	Berlin ...	—
*Aug. 25–Sept. 4	...	Le Havre	£9,600
Sept. 8–13 ...	...	Munich	—
*Sept. 9–18 ...	...	Bordeaux	£8,000
Sept. 24–Oct. 3	...	Milan ...	£6,000
Sept. 25–Oct. 3	...	Biarritz	£1,000
Oct. 7–13 ...	...	Berlin ...	—
*Oct. 25–Nov. 2	...	New York	—
Dec. 4–18 ...	...	Marseille	£1,000

* International Meeting

The Aëronautical Society of Great Britain

(Founded 1866)

53, Victoria Street, London, S.W.

THE next Minor Meeting of the Society will be held at the Society's offices on Thursday, May 26, at 8.30 p.m.

The Council has sanctioned the publication of the following:—

NOTICE TO INVENTORS

The Council of the Aëronautical Society of Great Britain are prepared to examine proposals from inventors with regard to Aeronautics. In sending in proposals of the kind, inventors should clearly understand that the Society undertakes no responsibility whatever in connection with them, and it must also be understood that they are only considered on the express condition that no report or communication emanating from the Aëronautical Society in reply thereto should be used for the purpose of advertisement or publication, without the written consent of the Council. Any proposals should be addressed in the first instance to the Council, who will submit them to Committees nominated by them for the purpose.

Inventions will be considered in the order in which they are received, and all communications will be regarded as strictly confidential.

No fee or charge of any kind will be made. Copies of documents only should be sent, and not originals.

With regard to models, the Council will not hold themselves responsible for any damage sustained by them.

Inventors are reminded that the funds of the Society are not available for the financing or developing of individual inventions.

more accurately of a tail-first, aeroplane. In the Wright machine, in fact, the "stability surfaces," that are in every other case comprised in the tail, are formed by the forward elevator; there can, in consequence, be no question that this aeroplane depends for its longitudinal stability almost entirely on the manipulation of the driver. The tail surface that has now been added to the latest machines is designed to produce a certain amount of independent automatic stability. It is situated aft of the rudder and is fixed in the central plane of the machine. The accompanying rough sketches illustrate the curious position of this new tail surface which has an area of 24 sq. ft.; and measures 12 ft. in span by 2 ft. in section.

"Warping": Professor Montgomery's Claim

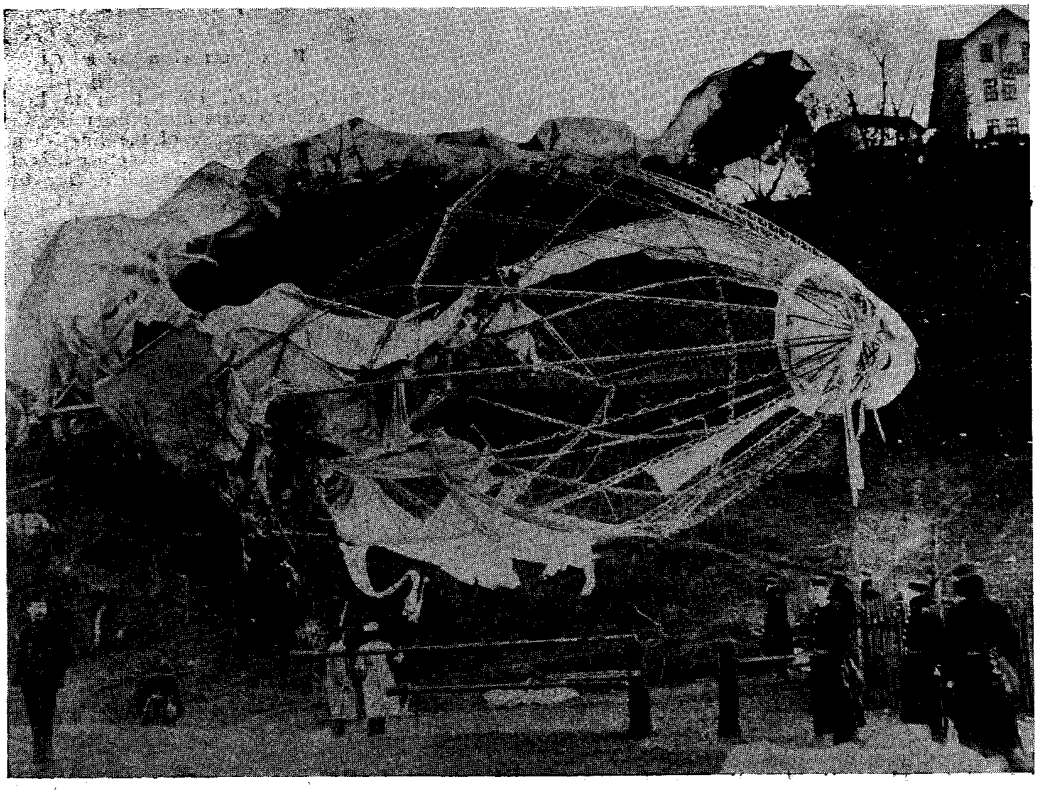
ELSEWHERE we publish a report of a lecture delivered by Professor J. J. Montgomery, of Santa Clara College in California, wherein is contained a detailed account of his early experiments, carried out in 1884-5 and again in 1903-5. These experiments, of great importance in themselves, are of particular interest at the present time, in that they bore for the first time on the vexed question of the "warping" of aeroplane surfaces. It is known, of course, that the brothers Wright in 1904 were granted a patent for the warping of planes in conjunction with the operation of a vertical rudder. During the last few months the Brothers Wright have given effect to their claims by seeking, and obtaining, injunctions preventing alleged infringements of this patent, and prohibiting the flying of aeroplanes making use of wing "warping." Such injunctions were actually granted against Curtiss and Paulhan. The same method has been followed in France, where the Compagnie Aérienne, the owners of the French rights, have successfully brought a number of similar actions. The whole question is of considerable importance. It has been claimed before now that the credit of inventing "warping" is due, not to the brothers Wright, but to as early an experimenter as Mouillard; at any rate, Professor Montgomery's lecture certainly makes it perfectly clear that he himself adopted this device in his experiments made in 1884 and 1885, which were detailed before the Aeronautical Congress held in Chicago in 1893. His own patent dates from April, 1905. It would appear, then, that priorty belongs to Professor Montgomery. It is certainly deeply interesting to learn that at the present time Professor Montgomery is engaged in building a full-size machine on his previous lines, and that this aeroplane is likely to undergo its first trials in California before long. It may, at any rate, serve to settle a controversy that gives signs of becoming acute.

Death of Hubert Le Blon

IT is with deep regret that we chronicle the death of Hubert Le Blon, one of the most daring and accomplished of aeroplane pilots. After taking part in the meeting at San Sebastian, during the course of which he made several excellent flights in his accustomed style—though in one, by the way, he came down in the sea, on March 29, owing to the failure of his motor—Le Blon, with his usual courtesy, resolved, on April 2, to give an exhibition flight over the sea in order not to disappoint the crowd of spectators who had assembled in the hope of seeing a flight, albeit the meeting was over. After flying for three minutes over the sea, Le Blon was in the act of turning in order to regain the shore when, at a height of 60 feet, his monoplane suddenly fell prow foremost into the water. When help arrived he was found to have been held down beneath the water by his machine, and to have been drowned. Le Blon was flying a cross-channel *Bleriot* monoplane driven by the same 50-h.p. Gnome engine that drove Delagrange's monoplane on its fatal ascent. First taking up aviation after the Rheims week last September, Le Blon, after a brilliant motor-racing career, had already given proof of his remarkable skill as an aeroplane pilot at Spa and at Doncaster. Shortly before his death he had been engaged as aeronautical engineer by the Humber firm, for whom he constructed the remarkable racing monoplane shown at the Aero Show at Olympia in March. The cause of the accident, though not clear, may be ascribed, without unduly straining probabilities, to the *Bleriot* machine itself. *Verbum sap.* We have to regret the disappearance of one of the most courageous pilots and one of the most courteous of men.

Three Balloon Accidents in Germany

Two fatal balloon accidents occurred in Germany on April 3. Early on that morning the balloon *Pommern*, which had been victorious in the Gordon-Bennett balloon race in America in 1908 and belonged to the Pommersche Verein für Luftschiffahrt, ascended from Stettin, near the Baltic, piloted by Dr. Delbrück (member of the Reichstag), with Herren Heyn, Benduhn and Semmelhack. The weather was very stormy throughout Germany. The balloon in starting during a lull first of all collided with some telegraph wires which it tore apart, and was next thrown against an adjoining brewery. All the occupants hereby were severely injured, while the netting was torn. The balloon rose quickly to 6,000 feet, but owing to the valve-line being



The Disaster to "Zeppelin II."
View of the bows of the dirigible. The rear portion is seen above the crest of the hill.