

flat planes, as far as he was aware, and none of his laboratory work was with curved planes, whereas in the machine shown in the pictures by the lecturer, there curved planes were used of the order of 1 in 12, similar to that which Otto Lilienthal had in Germany. One wondered, therefore, whether there was not a gap in his experimental work which was not recorded.

In the present paper they had a good record of the historic research into what was and what was not done, and, as Colonel Ogilvie had said, the Wright brothers were fortunate in having so able and persistent an investigator as Mr. Griffith Brewer. He had unearthed a good deal of data which would otherwise have been completely lost. It seemed strange to think of procedure similar to what was adopted in America being carried out here. Could one imagine, for instance, two rival patentees discussing the steam engine and going to the South Kensington Museum, or somewhere else, and having out the original Watt engine, or some similar engine, and attempting to run it on the Great Western Railway to prove their point. To his unenlightened and English way of thinking it seemed extraordinary that a historical specimen of a machine should have been taken out from the Museum, where it very properly rested, and used for the purpose of determining whether or not the warping as laid down by the Wright patent was an essential feature or not, for the purpose of vindicating the Wrights' or Curtiss's claim. It seemed an extraordinary procedure to have adopted.

He thought everyone, from a historical point of view, owed a great debt of gratitude to Mr. Brewer for going so patiently and persistently through the details of all the experiments that were made.

The CHAIRMAN said, on behalf of the members, he moved a vote of thanks to the lecturer for his very interesting lecture. He had added to the sum of their aeronautical knowledge, and of human knowledge as well. As he had said, the lecture dealt with facts, and he (the Chairman) had no doubt the facts were as Mr. Brewer said. They would look forward with the greatest interest to any reply that might be received from the other side of the Atlantic.

From Dr. WALCOTT.

Smithsonian Institution,
Washington, U.S.A.,
October 10th, 1921.

Dear Sir,—With reference to the paper of Mr. Griffith Brewer on which you are so obliging as to request my comment, I regret very much that having been inaccessible in the field until October 9th I have not had time to take up the matter as thoroughly as I would have liked to do.

I take pleasure in saying at the start that the Smithsonian Institution fully recognises the well-deserved success of the Wright brothers in being the first to make actual flights in heavier-than-air power propelled machines. This recognition the Institution was among the earliest to make in a formal manner by an award of merit. This being so, I am not prepared to concede that the Wrights were the first to construct such a machine capable of such flight.

Mr. Brewer appears to claim:—

1. The Hammondsport experiments were made for Curtiss' ends at Curtiss' expense.
2. The changes made in guying the wings were indispensable to prevent their collapse.
3. The changes made in the engine were indispensable to make it run.
4. The changes made in the propellers were indispensable to give them sufficient thrust.

5. The changes made in the form of the wings were indispensable to flight.
6. The changes made in the rudder were indispensable to flight.
7. The Hammondsport tests of the original machine did not result in flight but merely in hops.

Mr. Brewer is misinformed in regard to some points, and I think imparts a false impression regarding others. I take up the points in the order just given.

I. I quote from my letter to Mr. Curtiss of March 31st, 1914:—

“In connection with the re-opening and development of work under the Langley Aerodynamical Laboratory, it seems desirable to make a thorough test of the principles involved in the construction of the Langley heavier-than-air man-carrying flying machine, especially the question as to the tandem arrangement of the planes, and general stability, especially longitudinal stability.

“After my recent conference with you, I am writing to say that there will be an allotment of \$2,000 made from the funds of the Langley Aerodynamical Laboratory for the purpose of conclusively testing the Langley machine and the principles involved in its construction. If you care to undertake such experimentation, will you answer at your earliest convenience? All materials connected with the original machine will be placed at your service for the purpose.”

The Smithsonian Institution has still on file Mr. Curtiss' receipt for the payment made him in accordance with the above proposal.

II. Photographs show that in the original tests of the Langley machine, October 7th, 1903, one pair of wings stood up wholly without appearance of collapse after the machine took the air at full speed. Moreover, in the test of October 7th, 1903, the photographs show that the other pair was only slightly changed, and this change occurred for a very well attested reason, quite other than from their lack of efficient guying.

Mr. Brewer, in ignoring what follows on the same page, quite unfairly as it seems to me, quotes Mr. Manly's first statement to the newspaper reporters who beset him even before he was out of the water and who had to be pacified. Mr. Brewer should recognise that the succeeding well-considered statement by Manly and Langley after investigation of the causes of the failure of the test is the only one deserving of any weight:—

“After recovering the machine the foreman of the workmen (Mr. Reed) (who together with Mr. McDonald were the only ones on top of the boat when the launching actually took place), busied himself to discover what had caused the jerk to the machine at the moment it was released, which had been immediately followed by the great depression of the front end. After some little time he discovered that the upright guide at the extreme front of the launching car (which, as heretofore stated, was slotted to receive a metal lug projecting from the end of the guy-post, and thus prevent the front end of the framework from being twisted by a side wind striking the machine while it was still on the launching car) had been distorted, and the metal cap on it being stretched out of shape in a way which indicated that the pin of the front guy-post had hung in the cap, and that the guy-post was not therefore free from this part of the car when the end of the launching track dropped. The shock which the writer felt at the moment of launching and which had also been seen by others to occur was thus conclusively shown to have been due to the falling track, dragging the front end of the machine down with it. As the machine was travelling forward and the car had been almost instantly brought to a standstill by its buffer pistons co-acting with the buffer cylinders at the foot of the track, this front guy-post had been pulled backwards, and thus not only pulled the main guy-wires of the wings backwards and thereby

depressed the front edge of the front wings so that they had no angle of inclination, but had also bent the front end of the metal framework downward—effects which were discovered from the later examinations of the frame and the guy-post itself. From the instantaneous photographs which were obtained, indisputable evidence was obtained that this was what actually occurred. . . .

“ After completing the recovery of the machine and the examination as to the extent of the injuries it had sustained, and finding unquestionable evidence that the accident had been caused by the front guy-post hanging in its guide block on the launching car, the workmen were set to work straightening out and arranging the various parts, fittings and accessories, and cleaning up the engine which fortunately had sustained no injury whatever. After a consultation in Washington with Mr. Langley, who had been unable to be present at the experiment, both concerning what had already occurred and also what should be done regarding the future of the work, and in view of the fact that the statement which the writer had given to the Press representatives, immediately after the accident, had been made before there had been time to make an examination of the machine itself, it was decided that it would be best to give to the Press a short statement to correct the earlier one, and Mr. Langley accordingly made public the following note:—

“ Mr. Langley states that he was not an eye-witness of the experiment at Widewater yesterday, having been detained in Washington by business, but that on the report of Mr. Manly, immediately in charge, he is able to say that the latter's first impression that there had been defective balancing was corrected by a minuter examination, when the clutch, which held the aerodrome on the launching ways and which should have released it at the instant of the fall, was found to be injured.

“ The machinery was working perfectly and giving every reason to anticipate a successful flight, when this accident (due wholly to the launching mechanism) drew the aerodrome abruptly downward at the moment of release and cast it into the water near the houseboat. The statement that the machine failed for lack of power to fly was wholly a mistaken one.”

In view of these facts, I believe the photographic evidence of Langley's own tests of the great machine show that Mr. Brewer is wrong in maintaining that the Langley wings lacked efficient guying to prevent their collapse.

But one who knew Langley's work and the enormous mass of still unpublished tests of the wings, on the whirling table, with sand loading, and otherwise, and the experiments on lift, centre of pressure, and resistance for curved surfaces which occupied Mr. Huffaker many months, knows that Langley and Manly had sound experiments behind all they did and had not neglected to learn either the proper place or the sufficient strength for the guying.

III. The facts about the engine are quite opposite to Mr. Brewer's suggestion. In 1903 the engine ran far better than in 1914. There are many witnesses here who know that it ran with all five cylinders speaking for hours and hours at a time in 1903, delivering the full brake horsepower claimed by Manly. The Curtiss mechanics were unfamiliar with it, Mr. Manly was not present to instruct them until they had materially injured it by their unfamiliarity, and it never worked at Hammondsport anywhere nearly as well as in 1903.

IV. The propellers were cut down at Hammondsport, not because they could not hold up, but because the engine worked so poorly there that it did not run to speed with them. The thrust of the propellers at Hammondsport never exceeded 325 pounds, while in 1903 it was measured over and over again at 450 to 475 pounds with the original propellers.

V. The wings made at Hammondsport were far inferior in workmanship to Mr. Reed's wings. That they lacked the additional supporting surface of the overhang was to save time and expense, not for better design.

VI. Langley's whole energy was thrown to the single aim of making a single flight and a safe landing. He knew well that if this was accomplished the means to continue would be available. Owing to the wreck of October 7th, 1903, his means were exhausted before he got a flight. The rudder used in the 1903 tests was, as previous flights with the quarter size models had shown, adequate to guide a flight even without a pilot.

VII. I was present at Hammondsport on May 31st, 1914, and saw the machine with the original engine* giving only two-thirds the original thrust and with wings approximately of the original design, but far rougher executed, get under way from rest and fly gracefully, carrying, besides a man, over 300 pounds of floats in excess of what the machine was designed to carry. I am still confident that what it did under these relatively adverse circumstances is far inferior to what it was capable of doing in its original condition.

Very truly yours,

CHARLES D. WALCOTT,
Secretary.

Colonel W. Lockwood Marsh,
Secretary, Royal Aeronautical Society,
London, England.

* Mr. Brewer's Figs. 1 and 2 are wholly misleading. The earlier flights of 1914 were made with the original engine, the original propellers, and original cockpit. The later introduction of the Curtiss engine, traction propellers, etc., was for the purpose of gaining the further aerodynamical knowledge mentioned in my above cited letter of March 31, 1914.

From Dr. A. F. ZAHM.

REVIEW OF EXPERIMENTS WITH THE REHABILITATED LANGLEY AEROPLANE IN 1914.

Origin and Motive of the Experiments.

The experiments with the rehabilitated Langley aeroplane, made by Mr. G. H. Curtiss, at Hammondsport, N.Y., in 1914, were initiated by the Smithsonian Institution for purely scientific purposes, were conducted at its expense, and were reported by its delegated observer, the Recorder of the Langley Aeronautical Laboratory, who then had no connection, either actual or prospective, with the Curtiss Aeroplane Co. These tests were no more initiated for the purpose of patent litigation than were Langley's original experiments, though both were later cited for that purpose.

The statement that none of the costs, apart from the carriage of the machine to Hammondsport, have ever been paid by the Smithsonian, is the irresponsible gossip of a partisan who could easily have ascertained the truth. To impugn the motives of the Smithsonian men associated with the work of retesting the Langley aeroplane in 1914 is a discourtesy and injustice that well might be discountenanced by an impartial society. Aside from the innuendoes and direct imputations, is it quite decorous to regale an honourable body of men with isolated bits of testimony selected from a compromised patent unit by a special pleader who paid a friendly visit to the complainant, but failed to interview the accused? Surely the members of a royal society cannot relish the false report of a partisan member upon the financial conduct of an honoured institution of a friendly nation.

Main Objects of the Tests.

As stated in the *Smithsonian Annual Report for 1914*, "The main objects of these renewed trials were, first to show whether the original Langley machine was