

Summary of Results.

(1) The short flights of the overloaded Langley aeroplane in May and June, 1914, proved the propulsive system of the 1903 machine powerful enough for sustained flight.

(2) The half-mile flights made later in 1914, as well as the earlier ones, proved that the control surfaces of the 1903 aeroplane were sufficient to balance and steer the machine in the air.

(3) All the flights indicated that the hollow wing ribs were strong enough for the 1903 machine, but proved nothing for or against the adequacy of the general wing trussing. A special wing test could easily decide this question, if further evidence beyond that supplied by the original Smithsonian records be still required by a representative body of competent engineers.

Laundry List Objections.

It seems superfluous to notice seriatim all the trivial differences a hardware clerk can find between the Langley aeroplane of 1903 and that of 1914. Multitudinous, minute, inconsequential, they read like a laundry list. And if a thousand objections were answered, a thousand more would follow. In the list of changes why should the colour escape attention? A canary yellow wing differs from a faded buff.

Conclusion.

The foregoing paragraphs, written on too short notice, treat some of the major questions at issue. If the summarised inferences cannot be admitted, neither have they been disproved. A strut is not weakened by denying its strength. The objections so far offered have little cogency, for they are mainly declaratory and statistical. They are the objections of a book-keeper rather than of an engineer. But they doubtless have as much potency as the arguments of those mighty mathematicians who used to prove the impossibility of sustained free flight in any possible aeroplane of man's contrivance. And they have the merit of separating Langley's upright life from association with anything which is not strictly true or honourable.

Epilogue.

The foregoing paper, prepared by invitation of the Council of the Royal Aeronautical Society, was forwarded to the Secretary with the following recommendation:—

“Permit me very respectfully to recommend to the Council that it rigorously delete from the papers presented in this case, pro and con, all harsh personalities, all innuendoes and imputations of bad faith, as unbecoming to the dignity of scientific discussion, and distasteful to the urbane and cultured audience of a royal society.”

Cosmos Club, Washington, D.C.,

October 10th, 1921.

From Mr. C. M. MANLY.

I would have been amazed at the contents of this paper by Mr. Griffith Brewer had I not, upon reading it, immediately recalled the great similarity between a large portion of the statements made in it and the very bald statements made in the affidavit of Mr. Orville Wright in the suit of the Wright Company *v.* the Curtiss Aeroplane Company in 1915, in which Mr. Wright claimed for himself

and his brother the credit for everything that had been achieved in aviation from the beginning of time, and had dismissed everything that went before as "mere speculation and theory excepting," as he described it, "the desultory experiments of men like Lilienthal and Chanute with powerless gliding machines, and like Maxim, Ader and Prof. Langley, with miniature models and experimental full-sized machines which never flew, nor were capable of flight." In fact, it appears, upon a review of this affidavit of Mr. Orville Wright, that the present paper by Mr. Brewer is merely a condensed statement of the assertions, arguments, speculations and errors of this affidavit by Mr. Wright.

Before discussing Mr. Brewer's paper in detail, I wish to say that there is no one more willing and anxious to give due credit to Mr. Orville Wright and his late lamented brother, Mr. Wilbur Wright, for the brilliancy of their achievement in building the first machine that actually transported a human being through the air in controlled flight, as well as for being the first to actually fly in such a machine, and also for their great ability and pluck in achieving these results entirely through their own resources. I do deny, however, that the machine which they built and with which they accomplished the first flight was the first full-size machine capable of controlled flight, and I do assert that the Langley machine, which was built prior to theirs, was capable of successfully transporting a human being through the air and being controlled in such flight, and that it was prevented in 1903 from actually demonstrating its ability to achieve this result entirely through unfortunate accidents in connection with the two launchings which were made of it, and not through any inherent weakness, inefficiency, lack of power, or lack of any essential element necessary to the securing of such a result.

It must be remembered, in connection with the large Langley machine, that the general plans for it had been settled by Dr. Langley prior to my becoming associated with him in June, 1898. Also that these plans called for this large machine to be as near a duplicate as possible, on an enlarged scale, of the two steam-driven models which had so successfully flown in 1896 and which were approximately one-quarter the linear dimensions of this large machine. While Dr. Langley had, from the beginning of the undertaking, felt that it was necessary to replace the steam power plants of the successful models of 1896 with an internal combustion engine, and recognised the securing of such a power plant as being probably the most formidable obstacle to be overcome, still he felt that in an art which was so new and in which there was practically no other proved experience than these successful models to guide him, it was fundamentally important to rigidly adhere to the general design of the successful models, and not to change the design in any particular, except where absolutely necessary, at the same time that the scale of the machine was being changed. While, therefore, many proposals were made and discussed with reference to many of the features of the machine, which it was recognised would need to be changed before it would be thoroughly practicable for war or commercial purposes, still he resolutely brushed aside all such proposals that were not felt by him to be necessary in securing the one first object, that of enabling the machine to prove the practicability of transporting a human being through the air in controlled flight, by actually doing so under the guidance of such an operator. It is, therefore, to be understood that the Langley Memoir, to which Mr. Brewer refers, was prepared by me in such time as I could spare from an otherwise busy life, after ceasing my connection with the Smithsonian Institution, and that it was not intended as a treatise on the subject of aviation, but merely as an historical record containing such manuscript as Dr. Langley had himself, prior to his death, prepared, relating to the steam-propelled models, supplemented by a description of what seemed to me to be the more important constructions and tests carried out during the several years that I was associated with him. In fact, the draft of the entire manuscript which I prepared for this publication was completed prior to the fall of 1907, but

owing to the extreme pressure of other matters which could not be postponed, I was unable to complete the review and proof reading and release the manuscript for publication until I again took it up, in 1911, after a lapse of four years. The period of time, therefore, between the actual carrying on of the work and the writing of the manuscript record of it was not so great as would be supposed from the fact that the volume was not published until 1912.

It had been my hope and ambition, on closing up the work of Dr. Langley in 1904, as stated in the Langley Memoir referred to, to personally provide the necessary funds and to personally make a further test of the machine to demonstrate that it would fly successfully, exactly as it was attempted to fly it in 1903, and thus achieve what Mr. Brewer very aptly describes as "one of the most dramatic events in the history of aviation." However, fortune did not smile on me to the extent to enable me to personally meet the expense which such a demonstration would involve, and, consequently, I was very much pleased in 1914 when I learned that Dr. Walcott had arranged for the Langley machine to be tested by Mr. Curtiss at Hammondsport. Unfortunately I was not present at any of the tests of the Langley machine at Hammondsport, and therefore did not have the satisfaction of seeing it fly equipped with its original motor and carrying a load of approximately forty per cent. more than we had burdened it with in 1903. However, I did see the machine itself when I visited Hammondsport on June 30th, 1914, and was most bitterly disappointed to find not only that in rebuilding the wings such portions of the structure as had had to be supplied to supplement the spare parts which we had remaining after the second trial in December, 1903, were so crude and the wings so roughly assembled that it seemed hardly possible that the machine could be expected to fly with such crude and poorly assembled wings, but that the engine and transmission and propeller shafts had been so greatly abused by attempting to run the engine without having it in proper adjustment, that I was more than ever surprised that the machine would fly under such conditions carrying even its original weight, much less its greatly increased weight, as well as greatly increased head resistance, which had been added to it in mounting it on pontoons.

From what I did learn when I was at Hammondsport on June 30th, 1914, from personal interviews with various eye witnesses of the tests which had previously been made of the machine, I was convinced, however, that the machine had been successfully flown for short intervals on several occasions previously, and that this had been accomplished in spite of the fact that at no time had the original Manly motor, with which it was equipped, been operating at anything like the power that it was capable of giving, as shown by the previous tests in 1902 and 1903, this lack in power being due solely to the improper adjustment of the engine and its being equipped with a carburettor which was either not suited to it or not adjusted to give the best performance. When I left Hammondsport on this visit of one day only, I was assured that the defects in the adjustment of the engine would be remedied and the ball bearings on the transmission and propeller shafts, which had been damaged, would be repaired before further tests were made with it. I later understood, however, that the engine was removed and a Curtiss engine substituted in order to avoid both the danger of damage to the original engine and the delays which it was feared would be encountered in keeping the ball bearings of the transmission shafts in proper repair, and that as the original engine had already demonstrated its ability to successfully propel the machine, such further tests as were desired in connection with the study of the machine aerodynamically could be accomplished more expeditiously with an engine driving a single propeller without the use of gearing, etc., required by the original engine and its dual propeller arrangement.

Referring, now, more specifically to the criticisms which Mr. Brewer has made under the heading, "Original Langley Flying Machine, 1903," Mr. Brewer's statement in the third paragraph under this heading that the evidence

which he was presenting "is only a part of the evidence in my possession" is very apt indeed, for it is quite apparent that in attempting to prove his point he has deliberately suppressed part of the evidence which he had in his possession. For example, he has quoted in full from the Langley Memoir the statement which I made to the Associated Press, immediately after my emergence from the water and return to the houseboat, following the launching of the machine on October 7th, 1903, but has not quoted what is stated in the Memoir—that "although his (my) first inclination was not to make any statement (regarding the test) until a complete examination could be made to determine both the cause of the lack of success and also the extent of the damage which had been sustained by the machine, yet owing to the very great pressure brought to bear by the Press representatives, who said that unless some statement was given out they would write their own conclusions as to the cause of the mishap, he (I) finally gave out the following statement," the statement which he quotes. Nor has Mr. Brewer seen fit to quote you the immediately succeeding paragraph which reads as follows:—

"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, 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 examination 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. Referring to the photograph, Plate 95, which was taken by Mr. G. H. Powell, Secretary of the Board of Ordnance and Fortification, and which shows the machine just a few feet in front of the point where it was actually launched, it will at once be seen that the front end of the frame is bent downward and that the front guy-post instead of being parallel with the rear one has been deflected backward at the lower end through an angle of thirty degrees. Referring further to the photograph, Plate 96, which was taken at the same instant as the one just described, it will be seen that even this one, which is a view of the machine as it passed almost directly over Mr. Smillie's head, most clearly shows the extreme extent to which the front wings had been distorted, the rear edges of the wings near the frame having been twisted up until they struck the cross-frame, and the outer ends being free to twist had been forced up very much higher."

Neither does Mr. Brewer, in his immediately preceding quotation from Mr. Reed's affidavit, see fit to give you sufficient from the said affidavit to enable you to see that Mr. Reed's use of the expression, "it appeared at first," refers merely to an instantaneous impression which he immediately corrected by making an

immediate examination of the launching car to determine what had caused the machine to plunge downward at so sharp an angle, and that it was he who immediately discovered that the front guy-post had hung on the launching car, as evidenced by the distortion of the metal fitting of the car on which the guy-post had hung. Furthermore, Mr. Brewer is very careful to call your attention to the distortion of the front wings, as shown in these two photographs and as described in the Memoir in the paragraph which he omitted, but he does not call attention to the fact, which was called attention to in that paragraph, that the photograph taken by Mr. G. H. Powell, Secretary of the Board of Ordnance and Fortification, and which he has reproduced as Plate 3, shows very clearly that the lower guy-post of the front wings has been bent back by its having hung on the launching car, and that through such hanging it has twisted the bowsprit and front nose of the framework around through an angle of twenty degrees or more, thus permitting these front wings to twist as described in the said paragraph. Mr. Brewer's statement that "it also will be observed that the rear wings are likewise distorted, though to a less degree," is a mis-statement of fact, as there is no distortion of these rear wings, either in Plate 3 or in Plate 4. In fact, there is nothing more noticeable on Plate 4 than that the rear wings are not in the least distorted, although they must have been supporting their portion of the weight at the time, while the front wings are distorted, as would naturally be expected with the front guy-post bent backwards to the position shown in Plate 3.

Mr. Brewer's statement is misleading in connection with his reference to the metal cap, which held the front guy-post from rising during the launching, having had to sustain the entire lift of the wings during such launching, since the metal cap would only have to sustain such lifting force of the wings as was in excess of the weight of the machine at that end, and as the launching speed was made to be the same as the soaring speed, there would not be any lifting force of the wings in excess of the weight except such as might come from a wind, and it was not expected that the machine would be launched at first in anything but a generally quiescent atmosphere, and there was no appreciable wind blowing when the machine was launched on October 7th, 1903.

It is also noted that among the other evidence in Mr. Brewer's possession, which he has failed to lay before you, is the statement which Dr. Langley made to the Press on the next day to correct the earlier one which I had given to them before having had an opportunity to examine the machine, or even to think quietly about the matter. Dr. Langley's statement was as follows:—

"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.

"The engine, the frame, and all the more important parts were practically uninjured. The engine is actually in good working order. The damage done was confined to the slighter portions, like the canvas wings and propellers, and these can be readily replaced.

"The belief of those charged with the experiment in the ultimate successful working of the machine is in no way affected by this accident, which is one of the large chapter of accidents that beset the initial stages of experiments so novel as

the present ones. It is chiefly unfortunate in coming at the end of the season when outdoor work of this sort is impossible.

“Whether the experiments will be continued this year or not has not yet been determined.”

Mr. Brewer's supposition that I did not feel, or remember, until some years later, the shock experienced in launching is quite in keeping with the many inaccuracies of statement which he has made in the paper, as could be very easily demonstrated, were it desirable to do so, by the testimony of a number of those who were present at the time and with whom I discussed the matter while we were investigating the cause of the machine having pitched downward at such a sharp angle when it was launched.

Referring to Mr. Brewer's comments on the second launching of the Langley machine on December 8th, 1903. Mr. Brewer has not seen fit to even mention that, although the conditions at the moment of launching had become so bad, due to the fact that the houseboat, which could not be controlled by the tug-boat which was lashed to it, was veering rapidly under the combined influence of a strong tide and a strong counter wind, and that this combined with the lateness of the hour and the river being full of floating ice, made it most hazardous to attempt to launch the machine, still, owing to the depletion of the available funds and the feeling that it was “then or never,” as explained fully in the Langley Memoir, I decided to take a chance. It was, no doubt, a grave error in judgment, but under the stress which existed at the time it was perhaps human, if not pardonable. What really occurred in the second test I have set out to the best of my ability in the Langley Memoir. It may be briefly summarised into the statement that with the boat swinging first in one direction and then in the other, as it was under the counter influence of the wind and tide, the machine was caught, while running down the launching track, by a wind gust striking it from a direction that it was not any more prepared to meet, or designed to meet, than many later and unquestionably successful machines, which have been wrecked by wind gusts on a flying field, and that the large tail rudder at the rear dropped down at its rear end to the track as the machine was running along it, and that this pulled the entire machine down into the launching car at the moment of launching so that both rear wings were destroyed along with the rudder, and the right hand front wing, and probably the front guy-post, were similarly damaged, as the bowsprit is seen very clearly in Plate 5 to be bent down somewhat as it was in Plate 3. What probably gave way first was the upper guy-post of the large rear rudder and tail. This guy-post, which was at the extreme rear of the main frame, does not happen to show in any of Mr. Brewer's plates, but is shown very clearly in Plate 53 of the Langley Memoir. It is there indicated by the numeral 43, being provided at its upper end with a pulley over which ran the upper control wire by which this tail was operated up and down for longitudinal balancing and steering vertically. This guy-post was braced by a horizontal wire 44 running from the top of it to the upper guy-post of the rear wings. As the swivel mounting of this large rudder had been clamped to the short vertical tube at the rear of the machine to prevent the rudder from turning in a horizontal direction during this first test, leaving it free to be operated up and down only, it was expected that the only pull which this rudder would exert on this rudder guy-post would be straight backwards and downwards, and therefore it was not braced except in this direction by this single wire just described. It is quite probable, therefore, that with the houseboat swinging around as it did, the clamping of the swivel fitting of the rudder did not hold it against swinging around under the influence of the wind striking it from the side, and that this caused the rudder to pull on this guy-post in a direction diagonal to this horizontal bracing wire at the top of it, and that the guy-post gave way under this diagonal pull, thus permitting the rear end of the rudder to drop to the track, which would readily account for all of the resultant pulling of the machine down into the

launching car when it was released and the smashing up of the rudder and rear wings and the damaging of the front wings, as seen in the photograph, Plate 5. I do not assert, however, that we know definitely that this is the way in which the accident started. I have stated very frankly in the Memoir that we did not know definitely exactly how the accident started, or the sequence of events, except that the large tail rudder was seen by Mr. Reed to be dragging on the track at least ten feet or more before the machine was launched, and that the smashing up of this tail rudder by this dragging of it caused the whole machine to be pulled down into the launching car when it was released, resulting in the damage already described. Mr. Brewer, who was not present, maintains that he knows all about it, and that it was caused by weakness of the wing structure, but none of the score or more of those who actually witnessed the accident were able to give any description of just what occurred first earlier than their seeing the tail rudder dragging on the track, though all who were able to give any detailed description of what they saw concurred in stating that the tail rudder was dragging on the track before anything else appeared to go wrong.

Regardless, however, of whether or not the accident in launching the machine on December 8th, 1903, was due to a side wind striking it, the main point is that the machine, both in design and construction, was sufficiently strong to safely transport itself and its operator under the quiescent atmospheric conditions for which it was designed and under which the first tests were supposed to be made. The rear wings and the tail rudder with its operating cables and supporting guy-posts had shown no weakness in the first test on the 7th of October when the tail rudder was subjected to even heavier stresses than it would normally be called upon to withstand, due to my having pulled it up to the upper limit of its motion, or negative angle, in trying to right the machine when I found it plunging nose downward in this test.

There is no foundation for Mr. Brewer's statement that "there were several perfectly good reasons why the wings would collapse under any conditions of the launching." He cites as one of these reasons an alleged weakness of the cross ribs of the wings as shown in the sand loading tests, for which the deflection figures are given in the Memoir, and states that "when a sand load of only twenty per cent. above the flying stress was imposed, most of the ribs were bent from twelve to thirteen inches out of shape." He does not in any way distinguish the difference between flexibility and weakness. Dr. Langley's method of development was not to guess at anything, or depend on mathematical computation, where exact data could be procured by tests. It was for this reason that he devised, many years before I was associated with him, the scheme of loading the wings with sand to predetermine not only whether they were strong enough to stand the loads which it was known they would have to meet, but also to determine whether the change in contour was sufficient to interfere seriously with their effectiveness as supporting surfaces. Dr. Langley insisted most strongly at all times on the wing structure being kept as flexible as possible without such flexibility militating too greatly against their effectiveness. His opinion was that if the wings were too rigid in the early experiments, it would result in the machine being much less stable, and his great concern at all times was to minimise, as far as possible, the danger of a fatal accident occurring in the early trials before the operator had become sufficiently experienced to be able to manage the machine effectively. His greatest concern at all times was longitudinal stability, and he felt that this must be automatically maintained, certainly until the operator became quite experienced in controlling the machine. He insisted, therefore, that the wings for the large machine, especially at their trailing edges, should have practically the same degree of flexibility in proportion to their size as the wings of the successful models had. The cross-ribs, while, therefore, flexible to the extent shown by the deflection figures in the sand load tests, were not weak, but even stronger in proportion to the load they had to carry than those of the successful

models which had not only shown no weakness in flight, but had not even been broken in many of their landings on the water. The wings on the quarter-size model, which flew on August 8th, 1903, were proportioned in all their parts with reference to the corresponding parts of the large machine in exact accordance with the relative weights of the two machines, and there was certainly no weakness apparent in this quarter-size machine. A number of very clear photographs of it in flight are given in the Memoir.

In discussing the position of the guy-posts and the centre of pressure on the Langley machine, Mr. Brewer makes the statement, "Mr. Langley had made no measurements to locate the centre of pressure at small angles." He thus asserts as a fact what is untrue. For several years before I became associated with Dr. Langley he had been making a very thorough study, by actual tests, on the whirling table of the position of the centre of pressure of a score or more different shapes of curves supporting surfaces, a large number of these being indicated by the letter "T" in Plate 48 of the Memoir, where they are shown hanging on the wall in the background of the picture. Furthermore, in the Memoir, in describing the supporting surfaces, I have stated that the main rib (or wing beam) was placed at approximately the point where the centre of pressure of the wings was at their flight angle of ten degrees, and that this main rib was located at about forty per cent. from the leading edge of the wing. The reason the guy-posts were located as they were on the large machine was solely and only because they had been located at this point on the small machines, and that shop tests of both machines had shown that the system of guying was thoroughly strong and effective.

Mr. Brewer has made great point of the fact that the camber of the wings of the Langley machine in the Hammondsport test was not the same as that of the wings in the tests of 1903. I have clearly stated in the Memoir that the wings for the large machine were originally made with a one in eighteen camber, and that later when the rib construction was improved and the wings, which were finally used in 1903, were made in 1900, the front extension or leading edge was added, which changed the camber to one in twelve, to make these wings more nearly a duplicate of the wings of the steam-driven model No. 5 which had flown in May, 1896, instead of like those of No. 6 which had flown on November 28th, 1896, as they were originally planned to be. Both of these wing curves had given very good results on the whirling table tests, and Dr. Langley favoured the one in twelve camber and ordered it used in the final wings. It is quite certain that, aside from the greater cost of building the wings with this front extension, there would have been no particular difference in the results of the Hammondsport tests had they been so built. I am quite certain that any unprejudiced observer, who could have seen the Langley machine as it was rigged up at Hammondsport with the heavy floats attached to it and with the heavy, cumbersome system of rough struts and braces used in attaching it to the pontoons, would not have thought that it stood as good a chance of making a successful flight, using the crudely constructed wings with which it was equipped, as it would in exactly the condition obtaining in the tests in 1903, or as it now is in the National Museum in Washington.

Owing to the very brief time available between the receipt of the advance copy of Mr. Brewer's paper and the close of the last mail which will enable these comments on it to reach London by October 20th, it is impossible to attempt to go into Mr. Brewer's paper in more detail. I will only add, as I have always stated and do affirm, that the accident in the test of October 7th, 1903, was due entirely to the hook on the end of the lower front guy-post catching in the launching car, and that but for this accident a successful flight would, no doubt, have been achieved on October 7th, 1903. I have always and do still maintain that the accident to the machine on December 8th, 1903, was due to its being caught in a squally wind, which, under the influence of the counter force of the tide and wind,

caused the houseboat to swing around in such a way that the machine was subjected to stresses which it was not expected that it would be subjected to, nor necessary that it be subjected to, in order to give it a test in free flight, and that the machine, exactly as it existed in December, 1903, was thoroughly capable of making a successful flight and demonstrating that it was the first machine constructed in the history of the world capable of successfully transporting a human being through the air and being properly controlled in such flight. The tests at Hammondsport certainly demonstrated that the machine was capable of doing more than had ever been expected of it in the matter of carrying weight, and that the original engine with which it was equipped not only furnished sufficient power to enable it to carry its original weight, but also a greatly increased weight requiring considerably more than fifty per cent. more power than the original weight required, and that the claim is thoroughly well founded that it was the first machine built capable of safely transporting a human being through the air and being properly controlled in such flight.

From Mr. GLENN H. CURTISS.

Garden City,
New York.

THE TRIALS OF THE LANGLEY FLYING MACHINE AT HAMMONDSPORT, N.Y.

I have read the proof of Mr. Brewer's Paper on the Langley Flying Machine. In the first paragraph Mr. Brewer quotes Lord Northcliffe as saying that "in the United States there have been long and persistent attempts to belittle the work of Orville and Wilbur Wright." I think Lord Northcliffe has been misinformed. Personally, I have always thought the Wrights are entitled to and have received full credit for having invented and built the first airplane to make successful flights. The Langley Flying Machine of 1903 *did* however fly in 1914 at Hammondsport, N.Y., in its original condition with its original motor and propellers with no alterations except the addition of floats and their necessary supports, weighing altogether 350lbs. On several occasions this original machine rose from the water and flew for a short distance, and there is at least one photograph of the machine in the air.

Some time prior to June, 1914, I received a letter from Dr. Walcott, of the Smithsonian Institute, stating that an appropriation had been made by the Trustees of the Institute for the expenses of having the original Langley Flying Machine of 1903, which had been wrecked in launching at that time, given a trial flight and asking if I cared to undertake the commission. As we had every facility for doing the work and were very much interested in learning what the Langley machine would do, I replied that I would accept the commission. The machine was shipped to Hammondsport and assembled for the trial. We fitted three pontoons, two forward and one aft. The two forward pontoons were placed in such a position under the forward wing so that the angle of the guy wires leading from the wings would be unchanged. It was, of course, necessary to remove the long post to which the lift wires had been attached when the machine was launched from the catapult. I personally made the early trials and succeeded several times in rising from the water and flying for a short distance, carrying the additional head resistance and weight of the pontoons and fittings, which latter we found was about 350lbs. The dihedral wings gave excellent stability, and we were so pleased with the fact that we were able to fly the machine with this great amount of extra weight that we asked permission to instal a more powerful motor with a direct drive propeller and make more extended flights. This installation was made and flights were then made with the machine by two of my assistants.