

Nov. 16~ 1

Mr. C. F. Marvin
Washington, D. C.

My dear Mr. Marvin,

I thank you very much for your letter of Oct. 30~ and for your pamphlets on Anemometry, which letter add one more to my collection of co-efficients for computing wind pressure, and conjure fresh doubts as to the accuracy of my Anemometer. This is a "Richards" hand instrument which I have had ten years, and was said to have been very carefully adjusted in Paris. It agreed fairly well with an English instrument (Wood Bro. Liverpool) which I also have, and I was able to compute my glides of 1896 & 1897 without trouble, with the velocities of wind which I measured. When I went down to "Kitty Hawk" N. C. last August, to witness Mr. Wright's gliding experiments, we compared my anemometer with the government [?] instrument here, and found that they agreed. Now, your anemometry gives big corrections on the air, I am afloat as to the anemometrics, the co-efficients of wind pressure and the co-efficients of curved surfaces.

Now, let me tell you of Mr. Wright's experiments. Our glides of 1896 & 1897 having shown that much of the resistance to a soaring machine arose from the frame work and the upright position of the operator, Mr. Wright has been bold enough to place himself in a machine prone (flat on his stomach) and to increase the surfaces to about 300 square feet, or double as much as Lilienthal or Pilcher, or myself, had dared to do. Moreover, he has placed the rudder in front, where he can manipulate it with ease. I send a few photographs. With this apparatus he has made many glides, alighting on soft sand without breakages. The object, as you will infer at once, it to diminish the angle of descent, and eventually to soar like a bird. Thus far he has not glided at flatter angles than myself, and this is the first puzzle. See Herring's computations. Aero Annual 1897, p. 69, 70. The next puzzle is why the angle of descent is not much flatter in a high relative wind than in a moderate relative wind. Please compare glide No. 5 Aug. 8~ P.M. with glide No. 3 Aug 8~ A.M. on the annexed "record" which I have made for you. I hope that it contains all the data required. Still another puzzle is why the empty machine when floated in the wind should show such a low co-efficient of "lift." See "record of tests [...]," more particularly columns 13, 14, 15. You will note that in computing the lift I have discarded the pressures (7,8) corresponding to .005 and to .004, and have used the co-efficients (11) in your Anemometry. I do not think the Duchemin co-eff. can be accepted as correct, as

subsequent experiments with models have shown the curved surfaces to have far greater lifting power than planes. - I may add further, as a possible explanation, that the Wright machine, when floated at rest, was but a short way from the ground, the rear almost touched.

Yours truly

O. Chanute

see P. S.

P.S. I see that I omitted to mention the Köppen kite, the photo of which I return. Some of the surfaces seem to be curved, and this doubtless adds to its efficiency.

I may also mention Berthenson. I have had a number letters from him, recommending a curve, which he calls "Pline's curve" as most efficient and stable. It chanced to be almost exactly that of Mr. Wright's machine. We found it reasonably steady, but less efficient than [?] other curves subsequently tried.

It has occurred to me that if I would send you my two anemometers you might have them tested without much trouble. Should this be the case, you would there by do me a service.

O. C.

I also enclose a translation of Lilienthal's Chapter 8 in Moedebeck's pocket book for Aeronautics which you may not have seen.

O. C.