

criticised by his enemies for things which were deserving of highest praise and especially praised by his friends for things which were unfortunate lapses from scientific accuracy. I should consider it both unwise and unfair to him to specially rest his reputation in aerodynamics upon the so-called Langley Law, or upon the computation which gave rise to it, as they do not seem to represent his best work. The particular computations which led him to enunciate this law are found on pages 63-67, 'Experiments in Aerodynamics.' A careful reading shows that he never actually tried the experiments of which he professed to give the result. . . . It is clear from the Doctor's statement that he never demonstrated by direct experiment that weight could be carried at the rate of 200 pounds per horse-power at 20 meters per second, nor that the power consumed decreased with increase of speed up to some remote limit not attained in experiment. He merely assumed that he could have done it by varying the experiments a trifle and based the so-called Langley Law on this mistaken assumption."

The Regents of the Smithsonian Institution adopted this suggestion and the Langley Law was not inscribed on the tablet.

The article in NATURE of November 3 states that "the Wright Brothers are equally clear in their acknowledgment of Langley's work," and gives a quotation from them to support this idea. This quotation, taken in connection with the suggestion of the writer in NATURE, may have carried to some readers the erroneous impression that the Wright Brothers acknowledged an indebtedness to Langley for his scientific work. This was not the fact. The quotation given makes no reference whatever to Langley's scientific work. It is simply a generous acknowledgment by the Wrights at the time of Langley's death for the inspiration received from his faith in the possibility of human flight, and containing an expression of gratitude for information as to books on the subject of flight other than those they had already read. The Wright Brothers have also acknowledged their indebtedness to Chanute, Mouillard and others, but have always made it clear that their greatest debt was to Lilienthal.

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SOME difficulty is felt in continuing a discussion of the relative merits of the great pioneers in aviation, Prof. Langley and the Wright Brothers, since they are all entitled to our esteem, and comparison seems to be unnecessary. Mr. Griffith Brewer does not appear to dissent from such a general statement, but one suspects that his enthusiastic admiration for the work of the Wright Brothers has led him to make extravagant claims.

It is very surprising to hear that "the Wright Brothers first established a scientific basis for aeroplane design," and that their laboratory measurements "covered a field many times greater than had been covered by the work of all other experimenters together." The only publication cited in support of this contention occurs in two pages of the *Century Magazine* in 1908, and readers of scientific literature in aeronautics will realise that they do not know where to look for data based on the work of the Wright Brothers. Indeed, Mr. Brewer indicates that this must be so when he says, "While in Dayton (in 1914) I was allowed to examine, with the privilege of copying, much of the personal correspondence and diaries, as well as the records of the early purely scientific work of the Wright Brothers"; apparently

the work was not publicly available. Is it then strange that one should look to Langley as the scientific pioneer, since he took the normal steps of a man of science and published complete accounts of his results as he obtained them?

Mr. Brewer refers to the "Langley Law" that the faster an aeroplane be flown the less will be the power required to sustain it. He says: "The fallacy of this law is well known to all aeronautical engineers to-day, but up to 1910 this was generally accepted as Langley's chief contribution to the science of aerodynamics." The inadequacy of the law is evident now, but it is still at least partly true; in the case of the most modern aeroplanes the horse-power for flight decreases as the speed increases from the least at which support can be obtained. The increase of power required to increase the speed of the modern aeroplane above a certain limit is due to the light-weight engine, a factor which did not come into consideration in early practice. The error of unsound extrapolation outside the experience of the day was made, but only superficial observers could regard the enunciation of the law as "Langley's chief contribution" to aeronautical research.

One can only disagree with Mr. Brewer in his review of the situation and regret that this aspect of pioneer work in aviation was introduced in the tone of the paper on "The Langley Machine and the Hammondsport Trials." The point of the paper was not so much missed, as suggested by Mr. Brewer, as countered owing to the fact that the statements therein did not carry conviction. One of the articles in NATURE intimated this in the suggestion that the Royal Aeronautical Society should take up the matter and after full investigation issue an official report. The views on the Langley aeroplane expressed by Mr. Brewer cannot be accepted as final although given in all good faith.

THE WRITER OF THE ARTICLES.

Some Biological Problems.

DR. CUNNINGHAM (NATURE, February 9, p. 173) cannot be more weary of this discussion than I. It is many years since I, becoming doubtful, first tried to discover the precise meaning of certain biological key-words. To this day I have not succeeded. It has been my misfortune to encounter authoritative people who, instead of perceiving that I was genuinely puzzled, thought I might do "much harm by leading many who have no special knowledge of heredity and evolution"—e.g. Professors Goodrich and Bayliss—"to distrust the work of those who are engaged in research on these subjects." May I suggest that in this matter authority and regard for public opinion are out of place. Most biologists profess to know the meanings of their terms; but there is no agreement, and no definitions can be framed which cover the whole of common and accepted usage. A science which lacks a precise and significant means of expression labours under paralysing difficulties.

Dr. Ruggles Gates thinks that a variation is a character. Surely he is mistaken. When one individual varies from another (e.g. child from parent) the difference is revealed in a character. If this new character becomes established in the species, it remains a character; but, even colloquially, it ceases to be a variation. How then can a variation be a character? A variation cannot be thought of without a comparison, explicit or implicit, between two separate individuals; a character can always be thought of without such comparison. Evidently, then, a variation is not a character, but an unlikeness between two individuals which is displayed in a character. When we