# S. P. Langley's Pioneer Work in Aviation.[1]

By Prof. L. BAIRSTOW, C.B.E., F.R.S.

THE work of Prof. S. P. Langley in aviation is such a first-rate example of systematic inquiry—of a type rightly called scientific—that no excuse is needed in again directing attention to it. Progress was made step-by-step in the face of formidable difficulties, and no attempts were made to solve the problems of mechanical flight by bursts of brilliance or invention. The scientific method appears to be most suitable for the great bulk of human endeavour and is required in the interpretation and development of striking innovations.

Langley was a creative investigator and not merely a producer of data. It is probably not wide of the mark to say that his experimental results are now rarely appealed to, yet who can doubt that the whole course of aviation was largely determined by his efforts? Langley's work may be divided into two periods—1887 to 1896, and 1896 to 1903. The end of this period is almost coincident with the earliest successes of the Wright Brothers. The later Hammondsport trials on a modified Langley aeroplane have obscured the real issue, and it is better to leave these out of account as having nothing to do with Langley and his methods.

The story can be readily told in extracts from the originals; in 1901 Langley said:

"And now, it may be asked, what has been done? This has been done: a 'flying machine,' so long a type for ridicule, has really flown; it has demonstrated its practicability in the only satisfactory way—by actually flying—and by doing this again and again under conditions which leave no doubt.

"There is no room here to enter on the consideration of the construction of larger machines, or to offer the reasons for believing that they may be built to remain for days in the air, or to travel at speeds higher than any with which we are familiar. Neither is there room to enter on a consideration of their commercial value, or those applications which will probably first come in the arts of war rather than those of peace; but we may at least see that these may be such as to change the whole conditions of warfare, when each of two opposing hosts will have its every movement known to the other, when no lines of fortification will keep out the foe, and when the difficulties of defending a country against an attacking enemy in the air will be such that we may hope that this will hasten rather than retard the coming of the day when war shall cease."

This note was written before the advent of the man-carrying aeroplane—two years before. Some of the prediction is yet unfulfilled, particularly that as to remaining for days in the air, but it accurately anticipated war uses before civil. In continuing his story Langley shows that he had no commercial interests in his efforts:

"I have thus far had only a purely scientific interest in the results of these labours. Perhaps if it could have been foreseen at the outset how much labor there was to be, how much of life would be given to it and how much care, I might have hesitated to enter upon it at all. And now reward must be looked for, if reward there be, in the knowledge that I have done the best in a difficult task, with results which it may be hoped will be useful to others. I have brought to a close the portion of the work which seemed to be specially mine—the demonstration of the practicability of mechanical flight—and for the next stage, which is the commercial and practical development of the idea, it is probable that the world may look to others. The world, indeed, will be supine if it do not realise that a new possibility has come to it, and that the great universal highway overhead is now soon to be opened."

This passage is of extreme interest; it emphasises the scientific spirit and the relation of science to industry. Monetary reward did not come to Langley, nor did the merits of his work save him from biting criticism in the press on the failure of his man-carrying aeroplane. Time has probably enabled us to take a more detached and fairer view. These early remarks by Langley prepare us for a note by his assistant, Mr. Manly:

"In the spring of 1904 after the repairs to the main frame were well under way, the writer [Mr. Manly] on his own initiative undertook to see what could be done towards securing for Mr. Langley's disposal the small financial assistance necessary to continue the work; but he found that while a number of men of means were willing to assist in the development of the aerodrome [aeroplane] provided arrangements were made for later commercialisation, yet none were ready to render assistance from a desire to assist in the prosecution of scientific work." On the other hand, Langley "had given his time and his best labours to the world without remuneration, and he could not bring himself at his stage of life to consent to capitalise his scientific work."

The problem of financing and directing scientific research is seen here as a striking example of the failure of our systems. The troubles still exist in large measure, and much has yet to be learnt before science and industry combine for efficiency and economy. The relation caused comment by Manly to the effect that:

"Persons who care only for the accomplished fact may be inclined to underrate the interest and value of this record [1911]. But even they may be reminded that but for such patient and unremitting devotion as is here enregistered, the new accomplished fact of mechanical flight would still remain the wild unrealised dream which it was for so many centuries."

Throughout his writings, Langley made a clear distinction between two subjects which he called "aerodynamics" and "aerodromics"—a distinction which still exists but is differently described. His division corresponds very closely with the modern expressions "performance" and "control and stability," both being now regarded as branches of aerodynamics. The scientific advisers of the Air Ministry are more and more turning to the study of "aerodromics," on which progress towards safety in flying is seen largely to depend. Its problems are still very difficult. In concluding this note probably the best summary is Langley's own:

"I am not prepared to say that the relations of power, area, weight, and speed, here experimentally established for planes of small area, will hold for indefinitely large ones; but from all the circumstances

[1] Extracted from an address delivered as chairman of the Royal Aeronautical Society on October 5.

of experiment, I can entertain no doubt that they do so hold far enough to afford assurance that we can transport (with fuel for a considerable journey and at speeds high enough to make us independent of ordinary winds) weights many times greater than that of a man." And

"I desire to add as a final caution, that I have not asserted that planes such as are here employed in experiment, or even that planes of any kind are the best forms to use in mechanical flight, and that I have also not asserted, without qualification, that mechanical flight is practically possible, since this involves questions as to the method of constructing the mechanism, of securing its safe ascent and descent, and also of securing the indispensable condition for the economic use of the power I have shown to be at our disposal—the condition, I mean, of our ability to guide it in the desired horizontal direction during transport,—questions which, in my opinion, are only to be answered by further experiment, and which belong to the inchoate art or science of aerodromics, on which I do not enter."

The problems of Langley are still problems, and we have very much to learn about the control of aeroplanes. An interesting commentary on Langley's work is provided by the fact that on October 19 the world's record for gliding flight was obtained on a replica of the Langley machine and not by a glider following the modern conventional aeroplane. It would be wrong, I think, to argue superiority of type for the successful glider, but it is a not unwelcome reminder of the enormous progress made by a scientific pioneer at a time when science in aviation is at a very low ebb.

## The Early History of the Land Flora.[1]

By Dr. D. H. SCOTT, F.R.S.

II.

WHEN we reach the Upper Devonian flora we find ourselves in the midst of a comparatively familiar vegetation. A few of the early forms may have survived, but the bulk of the plants were highly organised Vascular Cryptogams or Spermophytes. While in the Early Devonian no true Ferns have been found, a branched, naked rachis being the nearest approach to a frond, the later vegetation has been called the Archæopteris flora, after the magnificent ferns or fern-like plants of that genus, of which the famous *A. hibernica* is the type. We do not, however, know for certain whether these fine plants were really Ferns, or fern-like seed-plants. The presence of true Ferns is more surely attested by Dawson's Asteropteris, from the State of New York, which has the structure of a Zygopterid, a group well known from Carboniferous rocks. Lycopods had attained a very high development, as shown especially by the genus Bothrodendron, of which the large heterosporous cones are known.

The now extinct group of the Sphenophyllums, characteristic of Carboniferous times, had also made its appearance in the Upper Devonian flora; the whorled leaves of these early forms were deeply cut, not wedge-shaped as in most of the later representatives. Nathorst's genus Hyenia, which already appears in the Middle Devonian, may probably have been a precursor of the Sphenophylls.

Another family, represented by Pseudobornia, of Nathorst, from Bear Island, is only known from the Upper Devonian. It was a large plant, with whorled leaves, palmately divided, and further cut into narrow segments, while the long cones are believed to have produced spores of two kinds. Pseudobornia is at present quite isolated; its affinities may be either with the Sphenophylls or the Horsetails. Apart from this case, the Equisetales do not appear to be represented among our present Devonian records, for the evidence for the occurrence of Archæocalamites at that period seems to be inadequate. The group, however, was so well developed in Lower Carboniferous times that there can be no doubt it had appeared long before.

[1] Continued from p. 607.

The best proof of the presence of seed-plants in the Upper Devonian is to be found in the occurrence of petrified stems, which, from their organisation, must presumably have belonged to advanced Gymnosperms. The genus Callixylon, apparently allied to the Lower Carboniferous Pitys, has a peculiar and beautiful structure in the secondary wood, the pits being localised in definite groups. The wood appears more highly differentiated than that of most living Conifers.

Thus the main lines of subsequent evolution were already well laid down in Upper Devonian times. We know practically nothing of their origin. Some botanists believe that the higher plants may have had a common source in some group, already vascular, such as the Psilophytales, while others hold that the main phyla have always been distinct, from the Algal stage onwards. The existence of these rival monophyletic and polyphyletic hypotheses, both maintained by able protagonists, shows how little definite knowledge of the evolutionary history we possess.

The Lower Carboniferous flora bears a close general resemblance to the Upper Devonian, but is much better known. The wealth of forms is, indeed, so great, that only the merest outline of the main features can be given here.

The Lycopods were abundantly developed. Many species of Lepidodendron and Lepidophloios are known, not only by external characters, but often by anatomical structure. While the primary ground plan of their anatomy was not unlike that of some of the simpler Lycopods of our own day, most of the old forms developed a considerable zone of secondary wood, and a massive periderm. They were, in fact, adapted to play the part of forest trees. The genus Sigillaria, however, so important in the Upper Carboniferous flora, was still scantily represented.

As regards their fructification, the Lower Carboniferous Lycopods had attained the highest level which the class ever reached. Not only were their cones constantly (so far as observed) heterosporous, with an extreme differentiation of the two kinds of spore, but some of them even developed a kind of seed, a structure quite unknown among Club-mosses of later than Carboniferous age. In the seed-like fructification