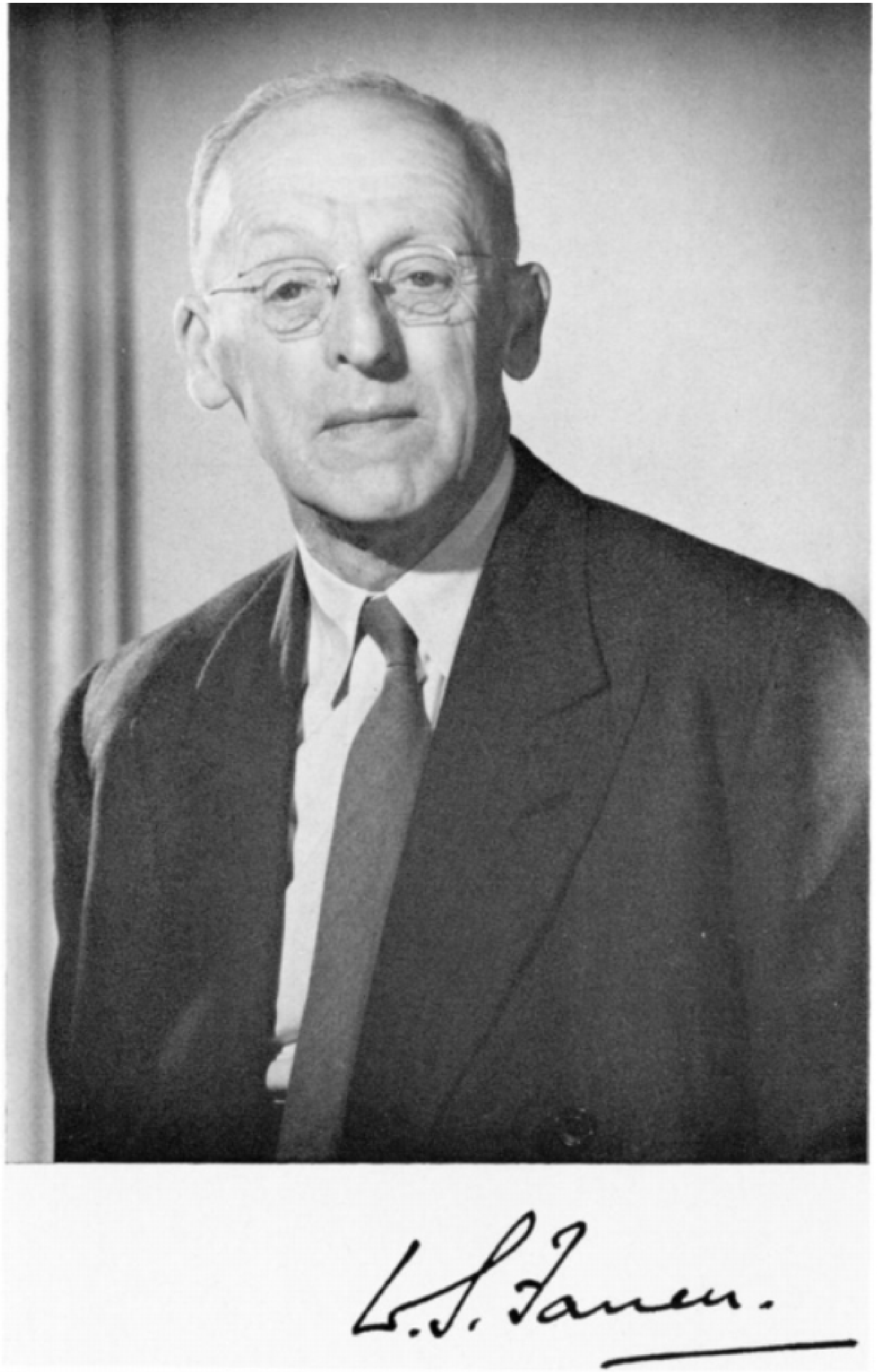
WILLIAM SCOTT FARREN

1892-1970

Elected F.R.S. 1945



WILLIAM SCOTT FARREN was born in Cambridge on 3 April 1892. I feel I cannot do better than quote from his own record sent to the Royal Society on his election.

'On my father's side, the family legend is it came from Ireland, but I know of no records on the point. My great-grandfather was in humble circumstances in Cambridge. My grandfather was, in turn, rose-grower, photographer (the first in Cambridge) and publisher of pictures. He was a great naturalist, particularly Lepidoptera, the friend of most of the chief workers in this field. My father was trained by him, and is (I think) almost equally well-known as lepidopterist and ornithologist. (By profession he was for many years a taxidermist, but has now* a furrier business.) My grandfather's brother was Robert, well-known as a painter and etcher, particularly of Cambridge.

'On my mother's side my grandfather was a chemist. Of his brothers, one was cook at St John's College, one was manager of Foster's Bank, and one was the priest of the Roman Catholic Church, and was (I believe) largely responsible for getting it built.

'My father was relatively unsuccessful in money matters, and we were seldom free from anxiety. But we lived well and found life too interesting to bother much. We had no "holidays" as children do now, but we were left free to roam as far as we liked. My father tried (but without success) to turn me into an ornithologist, etc. (I knew Wicken Fen first when I was three years old.) My mother did everything she could to encourage me to follow my determination to be an engineer, and by the time I was 14 I had a workshop. Two friends† of my father's helped me and taught me how to use tools, and by the time I was 19 I was a fairly competent craftsman.

'After a short time at the Convent School, I went to the Higher Grade School, and from there got a scholarship to the Perse School in 1901. (At this date I was 9, but there was no preparatory school.) The outstanding teachers were Dr W. H. D. Rouse, L. de Glehn, L. Chauville,

* Written in 1945. He died in 1953.
† One was Mr Thornhill of Boxworth (Cambs.).

V. E. Mitchell, A. J. B. Green and V. M. Turnbull. The last was a great teacher of mathematics and he gave me a very sound, and severe, training. But my real interest in my early years was in languages and history, and I was very nearly turned into a classic. I have never regretted this. I lost nothing by it and gained the friendship of Rouse, who taught me many other things besides Greek. Possibly my greatest pleasure was in French and German, which I learned to speak fluently and well, and to read widely in their literatures. This has remained with me and has been of inestimable value in many ways. History fascinated me, and at one time, when circumstances seemed to be driving me into classics, and away from engineering (owing to money difficulties in getting trained) I seriously thought of turning to it. I found mathematics and natural science easy, but did not believe I was good enough to get the essential University scholarship in either. I think I was right. There was, however, a chance in the Trinity group of colleges in the combination of mathematics and physics described as "engineering".'

In fact Farren earned an Entrance Scholarship at Trinity College in Mechanical Sciences in December 1910 and came into residence in October 1911, with rooms in Whewell's Court. As he and I were close friends at the Perse School (then opposite the Roman Catholic Church) from 1905 to 1910 I may be permitted a few words of comment. The Perse School under Dr W. H. D. Rouse was one of the leading schools in the country. It specialized in languages using the 'direct method' even for the classics, but the teaching of mathematics under V. M. Turnbull and J. H. Hersch and of science under J. L. Davies, who succeeded Mitchell, was also very good. The methods used for the sixth form—other than Dr Rouse's classics—were very different from the present. There were only three of us in the mathematical part of the 'Special Sixth', Farren, W. H. Mayo and myself. By a curious coincidence all three of us spent a considerable part of World War I at the Royal Aircraft Factory (now Establishment) at Farnborough, under Mervyn O'Gorman, and were concerned with aerodynamical experiments in flight.

We had little direct teaching, and spent a great deal of time reading and doing problems in the great hall. There was usually either Turnbull or Hersch sitting at the end of the room, probably correcting exercises, but patient when interrupted to explain real difficulties. However, they were not always there and Farren and I had time for uncensored conversation, chiefly on mechanical matters, for I too was interested in making things, mostly models of warships, and had been allowed by my father, J. J. Thomson, some instruction in the workshop of the Cavendish Laboratory during vacations. Farren was the engineer, and I well remember his explaining to me the geometrical relations between the wheels of a four-wheeled vehicle when turning a corner. I had to come to him for help with model engines. Even in those early days he had a feeling for design, an ability to assemble parts with different functions into an organic whole. He looked at a machine as an animal, not as an example of various scientific laws. I think his artistic background and inheritance helped in this, which was a feature of his life and one cause of his success.

His years at Cambridge were successful if not exciting. He obtained a First Class in Part I of the Mathematical Tripos in 1912, became a Senior Scholar of Trinity in 1913, and finished with a First Class in the Mechanical Sciences Tripos in 1914. He then took a post with British Thompson Houston, at Rugby, where he was put to work on the testing of electric motors and stayed till April 1915. He then joined the group O'Gorman was assembling at Farnborough. Research on aeroplanes there had been badly upset by the death in flight in November 1914 of E. T. Busk, though not before the first practical stable aeroplane, the B.E. 2c, had been produced, using the mathematical framework due to G. H. Bryan. O'Gorman had already collected B. M. Jones (afterwards Sir Melvill), G. I. Taylor (afterwards Sir Geoffrey, O.M.), R. McKinnon Wood (Chairman, L.C.C. 1957-1958), F. W. Aston (afterwards Nobel Prize winner), F. A. Lindemann (afterwards Viscount Cherwell), G. T. R. Hill (designer of 'Pterodactyl'), and the present writer. He also relied on long established members of the staff, especially F. M. Green, chief engineer.

A few weeks after he arrived Farren was appointed head of the aerodynamics department. In those days science as applied to aeroplane design was chiefly dependent on model experiments in wind tunnels, the best of which were those carried out at the National Physical Laboratory by the group led by L. Bairstow. The Factory in those days both designed and built aeroplanes and naturally the aerodynamics department among other duties was expected to predict in advance the performance of new or proposed designs. The predictions of the top speed were roughly achieved in practice but those of the rate of climb proved grossly optimistic, so much so that at least one machine from which much had been hoped was never put into full production. Apart from tests of top speed and rate of climb there had been virtually no flying experiments since Busk's death. It was, I think, largely pressure from B. M. Jones which got better experiments started before he left for Orfordness. These took the form of glides, with or without stopping the airscrew, and what were called 'partial climbs', i.e. flights at constant airspeed in which the gain in height was measured but the airspeed was not that which gave the best climb. These gave other points on a curve connecting the drag and lift co-efficient of the wings after correcting for the drag of other parts of the aeroplane. The results differed substantially from those obtained by the N.P.L. from model tests of the wings. This led to a serious controversy. The N.P.L. claimed that their readings were more consistent than those of the experiments in flight and were, therefore, to be preferred. We said that though their results were more consistent the discrepancy was greater than the random errors of the flight tests allowed and that anyhow we were measuring the quantities which were really needed. We attributed the difference to 'scale effect'.* This controversy went on till after the end of the war. As so often it turned out that both were wrong. The scale effect for wings is small in the range of Reynolds number concerned, but the model results were badly in error (which the group at R.A.E. had never seriously suggested), because of inadequate allowance for the influence of the tunnel walls and of turbulence. In most cases wings and body had been tested separately and the assumption that resistances measured apart could simply be added was sometimes badly wrong.

It was not till the summer of 1916 that the arguments of the scientific staff that they should be taught to fly as pilots and not merely take observations, prevailed over the authorities. When Farren, Lindemann and Keith Lucas, a Cambridge physiologist who had done admirable work on compasses, were sent as pupils to the Central Flying School, then at Netheravon and Upavon, Lucas was unfortunately killed in an air collision with another pupil. Farren and Lindemann completed the course and flew frequently at Farnborough. G. I. Taylor had arranged to be taught at Brooklands. On his return he measured the distribution of pressure over a wing in flight. Lindemann concerned himself with observations made during spins which led him and H. Glauert, who by then had joined O'Gorman's team, to a satisfactory theory of this manœuvre, dangerous but useful in war. Farren, besides the work on scale effect and on stability, was concerned with Green in the design of several aeroplanes, including the very successful S.E. 5A, a single seater fighter.

In the course of 1916 O'Gorman resigned as a result of political pressure from those who said that it was unfair for Farnborough as a Government establishment to design, and even build, aeroplanes in competition with industry. He was succeeded by Sir Henry Fowler. In the latter part of 1917 Farren worked, almost single-handed, on the design and construction of a sea-plane called C.E.1, which was launched on the Hamble near Warsash in the presence of Lindemann and H. Grinstead from Farnborough on 5 January 1918. Only one machine was, I believe, made to this design (see p. 223 below).

About the middle of 1918 there was a second upset at Farnborough, as a result of which a number of the senior staff left, including F. M. Green and Farren. Both joined the staff of Armstrong Whitworth Aircraft at Coventry. There Farren took a large share in designing the Siddeley 'Siskin', a single seater fighter which went into production, but after the end of the war.

In August 1917 Farren married Carol Warrington. A daughter, Helen Mary, now Mrs Granfield, was born in April 1919. In 1920 he began to lecture in engineering and aeronautics at Cambridge though he was not appointed University Lecturer until 1926 when new Statutes were introduced. Here he served under B. M. Jones (now Sir Melvill), who had been appointed in the previous year to the Mond Chair of Aeronautical Engineering, as its first holder. In addition to his work in Jones's department he gave lectures on structures. From 1922 to 1931 he also lectured on aircraft structures at the Royal College of Science. From 1933 to 1937 he was a Fellow of Trinity College and supervised Trinity undergraduates.

* Aerodynamical forces at speeds well below that of sound, reckoned per unit area of similar bodies similarly presented, vary roughly as the density of the air times the square of the speed and this rule was used in scaling up model results, but theory showed that it could be modified by an arbitrary function of the 'Reynolds number', a quantity which depends on size, among other things, hence the term 'scale effect'.

Jones was building up a research team of which Farren was an important member. The general line of study at first was that of airflow over wings with special reference to the stall (the behaviour of a wing flying at a large angle of incidence when the normal flow breaks down and much of the lift is lost) and also to the process by which the circulation of air round the wing, which is essential to its lift, is built up when the aeroplane is taking off. Later Jones passed to the study of the 'boundary layer', as part of a general study of ways of reducing the drag of wings. The boundary layer is a thin layer of air next to the plane which does not share the general motion of circulation, with respect to the aeroplane, of the main mass of the nearby air. It was being discovered that there are two possible kinds of boundary layer, called 'turbulent' and 'laminar'. The larger the fraction of the whole that can be persuaded to stay laminar the smaller the resistance of the wing. Jones's team got considerable support, especially on the mathematical side, from G. I. Taylor, now Sir Geoffrey, who, however, did his experimental work at the Cavendish.

Farren's share was predominantly the instruments, though the first paper he published developed a method of applying the principle of least strain energy to redundant structures with special reference to biplane wings. The next two papers were analyses of the effects of changing the proportions and parameters, such as wing shape and wing loading, on the performance of a proposed aeroplane; all were intended primarily for designers. He showed in them a love of graphical methods of expression which was characteristic of his work.

One of Farren's earlier designs at Cambridge was for an apparatus to examine the changes that occur in the pattern of flow as a wing is accelerated from rest. This was done in two dimensions in a tank filled with water in which drops of oil had been suspended. The wing was driven through the water and the moving drops photographed. This method has advantages over a wind tunnel in that the water is initially at rest and free from turbulence and it was also an easier way of getting moderately high Reynolds numbers, thus approaching more nearly to full scale conditions at low cost. This apparatus was somewhat improved and used by P. B. Walker, under Farren's direction, to compare the observed circulation with a calculation using Wagner's development of Prandtl's theory. Good agreement was found for the growth of circulation expressed as a fraction of its final value. Later on the method was used to study the flow round a wing kept at above the stalling angle and accelerated from rest. This showed that in some cases a wing can exert, for a short time, much more than the normal lift, a conclusion later confirmed and studied in more detail in the air tunnel (see p. 220 below).

At an early date Farren designed the first wind tunnel for the department, a modest affair, only 20 inches by 28 inches, for which he also designed a simple and original balance to measure the forces on the model. Later on, in 1935, he invented for it a much more elaborate balance with a period of only $\frac{1}{20}$s capable of investigating the rapid changes of flow that often accompany a stall. This he used himself to follow up more fully the conclusions of the earlier work in the department on the behaviour in stalling of wings with various sections. Wings often show hysteresis near the stall. The forces when the angle is *increased* from below stalling may be quite different at the larger angles from the forces observed at the same large angles when these have been reached from a state of complete stall by *decreasing* the angle. The differences may be as great as 50 per cent of the normal and may occur in times corresponding to 1 to 1.5 s on the full scale. They were thus of considerable practical importance both from the point of view of controlling the machine near the stall and from the large stresses that might be caused.

Farren, with Taylor's help on the theory, designed a second tunnel in which the turbulence was kept very low, a very important matter in the studies on which the department was then engaged of the conditions which govern the division of a boundary layer into laminar and turbulent parts. This was also studied in flight and involved measurement of the drag of a section of the wing by means of what became known as 'The Pitot traverse method'. It also involved the measurement of where the transition point in the boundary layer occurred in flight. The idea that one could determine the drag of a section of a wing by traversing the wake with a Pitot tube had been put forward by Prandtl—but it was Farren who devised the apparatus in a form which could be carried in flight. Melvill Jones and he evolved the technique into an accurate means of conducting the measurement. This flight work was probably the first time this had been done—the technique subsequently was employed at Farnborough and rapidly spread into other laboratories. It shed a good deal of light on drag generally and how it could be divided into its various origins.

Farren also devised the equipment whereby the transition point at the boundary layer was determined in flight. This was done by drawing a very tiny Pitot tube rearwards along the surface of the wing, and observing the pressure developed there. It was possible to find the transition point very accurately not only because of the change of the absolute level of pressure which occurred, but also because the character of the reading, which became oscillatory, gave an excellent visual indication. Again, I (A.A.H.) think this was the first time this sort of thing had been done in flight, and the success of the work rested very much on Farren's contribution as the instrument designer.

I (A.A.H.) took part in these flight experiments, and subsequently adapted the Farren instruments to carry a very fine wire instead of a Pitot tube. The wire, which was about a quarter of a thousandth of an inch in diameter, was heated electrically and formed part of a Wheatstone bridge circuit, from

which I took the voltages and amplified them for cathode ray presentation. With Farren's help, I managed to get this gear into the aeroplane, and we were then able to observe the transition point by the change of frequency of the oscillations transmitted, and also to start investigating the low frequency oscillatory character of the laminar boundary layer.

It is interesting to look back on this, because when the low frequency oscillatory character of the laminar layer had been noted in wind tunnels, it was thought that it was a phenomena reflecting the turbulence in the wind tunnel stream rather than a true characteristic of laminar boundary layers. By getting equipment into the aircraft, and flying high and therefore free from any small eddy turbulence, we found the oscillations still there and so plainly demonstrated them to be characteristic of laminar boundary layers. Subsequently, work of Dryden and his colleagues in America provided an analytic background for the development of these low frequency oscillations.

Even outside his department Farren was most generous and unselfish in designing apparatus to cope with special experimental difficulties in a friend's work. He at least once went so far as to make the device himself. His name twice appears as co-author with G. I. Taylor in the *Proceedings* as the designer of apparatus to measure in one case the distortion of crystals of aluminium under compression* in the other the heat developed during the plastic extension of metals.† The second of these is a wonderful piece of work measuring to 1 per cent a quantity of heat which is the difference of two much larger quantities, namely the heat equivalent of the work done and the heat shown by the rise of temperature produced, which differ by from $4\frac{1}{2}$ to $13\frac{1}{2}$ per cent, depending on the metal. These measurements must be made in a few seconds. A self-recording testing machine was made, the rise of temperature of a thermocouple being recorded photographically on a moving plate.

He also helped Taylor on the detailed design and the testing of the C.Q.R. anchor which Taylor had invented just before World War II and which is widely recognized as the best anchor for yachts and other small vessels.

Teaching

In his lectures to undergraduates, as in the more formal public lectures which he gave later, Farren made much use of illustrations, both diagrammatic and experimental. He used to sketch on the blackboard in isometric projection, and preferred graphical to analytical expression of results. He took much trouble to design and show striking experiments especially to demonstrate the flow of gases and liquids. Especially for the abler students they were a memorable experience.

Consultancy work

Besides his work as a member of Jones's research team, as a lecturer in the Engineering Department, and as a member of the various A.R.C. Committees

* *Proc. R. Soc. Lond.* **111**, 529 (1926).
† *Proc. R. Soc. Lond.* **107**, 422 (1925).

and panels, Farren continued to act as consultant to Armstrong Whitworth Aircraft till he went to the Air Ministry in 1937. His work for them was varied. A leading member of the firm writes:

> 'His advice was sought not only on aircraft design, but also on aero engines and engineering problems in connection with motor cars. He designed a static test rig for testing a complete car, less body, that is engine, gear box and transmission. During this first period he lived with his family in Kenilworth until he left the Company to go to Cambridge in 1919. From then on he was our consultant, principally on aircraft design, not only on aerodynamic problems such as stability, drag and control but structural ones. He developed the strain energy method of calculating the stresses in the Siskin redundant wing structure, advised us to go for monocoque construction and advised on strength calculations.
>
> 'He was a consultant on all Armstrong Siddeley aero engines, right up to the Deerhound, when he left to join the Ministry just before the last war.'

During his years at Cambridge he served on the Aeronautical Research Committee and on a number of its sub-committees, including being Chairman of the Design Panel of the Aerodynamics Sub-Committee for a year or more up to 1924 while it was producing a report on scale effect.

We are much indebted to Dr R. W. Gandy, the present Secretary of the Aeronautical Research Council, for the following list of Farren's membership of the Council and its subsidiary bodies. (*Note:* The Aeronautical Research Committee was renamed the Aeronautical Research Council in April 1945. Some sub-committees continued without break when this change occurred; others were reconstituted with other names, or disappeared because of broad changes in the committee structure.)

Main Committee or Council

Sir William Farren served as an Independent member of the Aeronautical Research Committee for two periods, 1930-1933, 1934-1937; and as an Official member of the Aeronautical Research Council, March–December 1945, at which date he left the R.A.E. to go into industry. He later served as an independent member of the Council for two periods, 1957-1960, 1962-1965.

Aerodynamics Sub-Committee or Committees

Farren was a member of the Aerodynamics Sub-Committee from its inception in December 1971 until it became the Aerodynamics Committee in April 1945; he remained a member of the Committee until he left the R.A.E. at the end of that year. Although he was presumably an official member in the first instance, he served as an Independent member during most of the inter-war period until he joined the Air Ministry in 1937, after which he was an Official member. At a much later date, he served as an S.B.A.C.* member (i.e. a member nominated by the S.B.A.C., under a scheme that has been in operation since the A.R.C. became a Council) for six years, 1950-1956.

Structure Sub-Committee

I (R.W.G.) mention this Sub-Committee next because he was its first Chairman, from 1930 when it was formed until he ceased to be Independent in 1937; he continued to serve as an Official member for a year or so after that.

It appears that the only other A.R.C. bodies of which Farren was Chairman at any time were comparatively short-lived Panels, viz. the Design Panel of which he was Chairman for about two years, 1923-1925, the Range of Aircract Panel (1928) and the Vibration Panel (1935-1936). For his work on other sub-committees see Appendix (p. 240).

Farren as pilot and test-pilot

Farren at school, though keen on cricket and a competent wicket-keeper, was not a specially good games player. Lindemann definitely was, he had played tennis with some success at Wimbledon and was a beautiful skater. They returned from the Central Flying School, having graduated in November 1916, with about 23 hours each total flying—Farren went solo after three hours forty minutes. At first Lindemann was the more striking pilot but after a few months there was nothing in it, though a difference in style remained. After a few practice flights Farren started a series of stability tests. Some of these were 'phugoids', a name due to Lanchester for the damped longitudinal oscillations that a stable aeroplane will perform if disturbed from its trimming speed and then flown hands off. But there were other tests called just 'stability' which were, I (G.P.T.) believe, attempts to measure lateral stability. Then there were tests of balanced elevators and a local flight during which it is recorded that the oleo, part of the landing gear, dropped off. As he was flying the same machine a month later he must have made a good landing. By the end of May he records a flight in a single seater described as S.E.4a (possibly an early S.E.5) which he rolled four times. There were various experiments with elevators of different sizes, rudder forces, speed courses to test airspeed indicators and the like.

A large part of Farren's time in 1917 must have been taken up with the seaplane called C.E.1, mentioned above, for which he was almost entirely responsible. This was at the height of the submarine menace. Negotiations with the Air Board seem to have started early in 1917. Farren has left a note book with a summary of these. The start is a letter to the President of the Air Board asking permission for the Factory to design a seaplane for which a specification was enclosed. There were a number of letters but the project seems to have hung fire till a letter from G.A.S. dated 10 May, saying that the Factory was to go on with the seaplane. There was then an interview

* Society of British Aircraft Constructors.

between the Superintendent (then Sir Henry Fowler) and Sir William Weir, as a result of which Farren was the next day put on the job. His first task was to arrange for permission to learn to fly seaplanes at Calshot, which was granted on 26 May. Whitsun and bad weather intervened but on 3 June he had his first flight in a seaplane and the same day went solo on two types of these. After two more flights he returned to Farnborough and 'worked out a rough scheme' on 5 June. He visited the naval seaplane stations at Isle of Grain and Felixstowe and then spent most of his time at Farnborough.

The arrangements including planning, designing, water testing a model and actually constructing the C.E.1 seaplane at Farnborough, culminated in frantic work in the Southampton area in December 1917 and successful flights with himself as test pilot from 6 January 1918 to 11 March 1918. Of these tests, nearly 7½ hours were on the water and nearly 11 hours in the air.

When Farren went to Armstrong Whitworths soon after the end of the C.E.I episode, he seems to have stopped flying as a pilot, at least there are no records of any such flights. Nor did he start again when he went to Cambridge, though there were aeroplanes attached to Jones's department and a good many airborne experiments. Indeed it was not until towards the end of his time at Cambridge that he even took part in these as observer. When he joined the Air Ministry war was imminent and flying solo might have been frowned on. It it the more remarkable that when at the age of 49 he was appointed Head of Farnborough, by then not a Factory but an Establishment, he immediately took to flying as a pilot again and after some necessary reconditioning flew not only training machines similar to the fighting aeroplanes of the first war with which he had been familiar, but the most up-to-date single seaters. In 1942, his first full year of office, his name appears as a pilot of 91 flights and in 1945 it occurred 95 times, though the record available lacks ten days. A considerable proportion of these flights were of about half an hour before starting work in the morning, many of them on Spitfires. He also flew the last of the piston fighters, Tempests, Typhoons and Thunderbolts, not to mention very many less exciting species both British and foreign, and this at the age of 53. He is thought to have also flown a Meteor, our first jet.

He continued his flying activities after leaving the R.A.E. for Blackburns and commuted regularly from his home in Cambridge to Brough by air. He is particularly remembered for flying a single seat Blackburn Firebrand, a naval strike aircraft, a relatively high performance machine in its day, no small achievement for a man in his mid-fifties. Farren was also instrumental in creating a staff flying club at Brough where selected members of the technical staff could learn to fly for a mere ten shillings an hour, an example of his desire to bridge the gap between scientist and pilot.

Second World War

In 1937 Farren left Cambridge to join the Air Ministry as Deputy to David Pye, then Director of Scientific Research. Many people felt that war

was imminent, rearmament was starting. Air Marshal Roderic Hill (later Sir Roderic) was reorganizing the Directorate of Technical Development and in 1939 Farren became his Deputy Director for Research and Development of Aircraft. After the creation of the Ministry of Aircraft Production he became Director of Technical Development with direct responsibility to the Air Ministry for the development of new types. Soon afterwards (1941) he went to Farnborough as Director of the Royal Aircraft Establishment, the first to carry that title, the old one of Chief Superintendent being abolished.

One of his most striking contributions to Farnborough, however, was the consequence of work before he had official standing there. In the year or so before Farren left Cambridge the Aerodynamics Department at R.A.E. Farnborough had started thinking about the construction of a wind tunnel capable of 600 m.p.h., those most active there being W. G. A. Perring and G. P. Douglas. Farren had been involved while at Cambridge through discussions on Committees of the Aeronautical Research Council.

When he became Deputy Director of Scientific Research he concentrated on the business of launching this project, In the setting of the time, the capital cost estimated at about £¼ million was a very large sum for scientific equipment, particularly in the environment when many writers were setting the maximum speed of flight much below the envisaged speed of the wind tunnel because of the limitations of the piston engine. Farren, however, was aware of the thinking on jet engines which Whittle had introduced—Whittle had come to Cambridge in 1933 and the first jet engine was drawn there in 1934/35. Taking the situation as a whole, Farren was convinced that a wind tunnel with a 600 m.p.h. capability was essential to provide backing for further evolution especially of military fighting aeroplanes, and to exploit the possibilities the jet engine opened up. It was undoubtedly his enthusiasm and drive that brought the authorities to authorize the project, and then Farren turned his attention to setting up the small team under W. G. A. Perring at Farnborough to handle its development and the detailed design.

The wind tunnel was built and came into operation in 1941. By then the jet engine had shown, particularly through the flight work in the Gloster E.28, what it was capable of doing in revolutionizing concepts on aeroplanes of all types, and the wind tunnel proved to be a timely and vital piece of equipment for this evolution.

His work on the tunnel was by no means Farren's only part in the development and exploitation of the jet engine. It fell to him, just before the war, I (A.A.H.) suppose as one of the normal duties of the post he was occupying, to be concerned with getting an aeroplane around the Whittle engine. The Gloster Aircraft Company was chosen to be the constructor and it was Farren who co-ordinated the views of the Royal Air Force, the Air Ministry, Whittle and Gloster Aircraft. I remember this phase well, because having been induced by Farren to go to Farnborough to help with the high speed wind tunnel job, he pulled me in on the work on the first jet aeroplane.

I saw a good deal of it personally, and attended several meetings which he chaired. I well remember being fascinated by the skill he showed in bringing together the various pressures that inevitably surround a situation like this. The Royal Air Force, partly I think for procedural reasons, but also because they had come to recognize that a very important new development was taking place, showed considerable enthusiasm for wanting to see it in a fully-fledged fighting aeroplane as soon as possible—no doubt seeing the shadow of war across their shoulders as well.

Looking back now, I can see that the run of Farren's mind was that it would be wise to get through an experimental phase of flight before getting involved in a full military specification. Whittle, who quite rightly had every confidence in his invention, tended to feel that the sooner it was 'full military' the better. Farren with great skill—and I am sure that in retrospect everyone would agree with great correctness—brought about a situation in which all parties were satisfied by a decision to construct an experimental aeroplane, but simultaneously to initiate studies of a full military specification with the object of following, as rapidly as data were accumulated, with the experimental machine. He brought exactly this situation about, and so it was that the E28/39 experimental aeroplane got moving quickly, and meanwhile the specification of what turned out to be the Meteor started. By this means the data coming from the experimental flying were rapidly transferable to the military aeroplane, and I am sure that this had a good deal to do with the Meteor turning out to be so good a weapon when it was brought into service in 1944.

Going back to the E28/39-Meteor situation I mentioned above, we introduced onto the Meteor a wing of new cross-sectional shape which I was asked to devise by Farren to deal with the, at that time, totally new aerodynamic problems of wings in the 600 m.p.h. area. The Americans had made very valuable mathematical advances in the calculations of the flow pattern around the wings, and Squire and Young working at Farnborough in the same field had also made substantial contributions. This work gave us the means of devising wing profiles likely to be less susceptible to early shock wave making. Farren was very much in touch with the scientific work and encouraged us to apply it in this early jet aircraft area, while making sure that we did not take it to extremes which might have affected the stability of the aircraft sufficiently to confuse the main object of the experiment, which was to establish how one used a jet propulsion engine in an aeroplane.

I think the point of this story is that again it illustrates Farren's particular understanding of how development should be done, and the particular political ability he had to bring factions with different and strongly-held views into agreement on a sensible and well-based programme.

The impression Farren made during this early was period is well described by the following, for which we are indebted to Mr H. E. Rowe, C.B.E., his colleague at the Air Ministry:

'Farren was a splendid colleague for a young man. He encouraged one to use one's initiative to the maximum and to run one's own show; he wanted to be kept informed, but not unreasonably. He was normally gay and optimistic and was a great worker. When talking to one about a situation, problem, development, in which he expected one to take the main, or a significant part of the action, he had a very special technique of communication, presumably developed in his days as a Don at Cambridge. He would talk all round the situation, look at it from all angles, bring out the salient points, the pros and cons, the full nature of the problem as he saw it. Then he'd ask how you would propose to tackle it and as likely as not, on hearing your views, leave you with it. Some, I think, mistook the technique for mere talkativeness, but I'm quite sure it was a conscious method of communication. He was trying to put into one's mind his own clear, overall concept of the matter and its special features and points of difficulty so that all possibilities of misunderstanding were avoided. I'm glad to say we worked well together and I learned a very great deal from him. He never panicked and was always confident that appropriate measures could be taken to deal with any of the problems with which we were confronted —and there were plenty in the feverish days before the war and the early days of the war and the night bombing of this country.

'When we were sent out of London to Harrogate in the early days of the war, I saw a different side of him—the lover of the countryside and of the country walk. He would love to get away in the charming surrounding country for an hour or two on a Sunday when with a map he would guide one unerringly along the footpaths to the places with the most lovely scenery on to some little village where he knew a cottage where tea was served. On one such occasion he was called to the phone in the heart of Yorkshire's countryside; Lord Beaverbrook had tracked us down and we had to return post-haste to deal with some urgent matter. He was a bit put out—he so loved his walk.

'He was good company and those stolen hours were a great solace to him—he was not an "office wallah". He had a very keen sense of humour, never long kept in abeyance, but it was never malicious or cruel.'

We are indebted to Professor A. R. Collar, F.R.S., for the following impression of Farren's work at Farnborough:

'I first got to know Farren in 1936 when I joined the Secretariat of the (then) Aeronautical Research Committee and attended a number of meetings with the Secretary. I had seen Farren previously at meetings which I was attending as a visitor; he was clearly a man of some consequence, to whose opinions the Committee would defer. My first close contact with him was following one of these meetings when we had tea together and, very surprisingly, he unburdened himself to me of some of the perplexity he was feeling about his own position and future. He was, I remember, concerned that his future at Cambridge did not look very bright. In the light of hindsight one realizes that he felt that in a sphere dominated by Geoffrey Taylor and Melvill Jones, his own talents could not be fully deployed and recognized. He was an enthusiastic and dedicated engineer with a strong belief in the practice of engineering, so that the academic world was not offering him full scope or full recognition and he was, therefore, contemplating a move.* I think this unusual and unexpected confidence can only be ascribed to the fact that he was basically a rather shy and reserved man (though he could cover this with an assumption of brusqueness) who had reached a point where he felt he must overflow to someone—even a junior member of an organization such as myself.

'In the light of this it was no surprise to me when I learned that he had resigned his academic post and had gone to the Air Ministry as Deputy Director of Scientific Research under D. R. Pye. In this post he could clearly have a far greater influence on the development of aviation and this at a time when world events were making it plain that aviation would be of the utmost importance in the near future. With the changing Ministries he changed his post more than once, becoming eventually Director of Technical Development in the Ministry of Aircraft Production; in this post he worked closely with Lord Beaverbrook and it was Farren who was the original inspiration of Beaverbrook's famous phrase "The boys in the back room". He had clearly made a great impression on Beaverbrook, though I believe they were sometimes at odds.

'In 1941 he moved to the Royal Aircraft Establishment to become its first Director, Previously, the establishment had been under the direction of a Chief Superintendent and there had been what Farren recognized as an unfortunate dichotomy in the organization at Farnborough. Most of the staff were responsible to the Chief Superintendent, but the large and important Airworthiness Department was responsible directly to the Director of Technical Development. The resultant division of interest and responsibility had led to some friction. Farren therefore asked that the staff should be welded together as a unit under himself as the sole head and with the title of Director, and he appointed new Heads of Departments to help in bringing about a unity which was previously absent. In particular, the Airworthiness Department disappeared and was replaced by the Structural and Mechanical Engineering Department with Dr A. G. Pugsley (later Sir Alfred) as its Head. He also set about recruiting to the R.A.E. (the work of which was expanding enormously with the war effort) anyone who in his view would be able to help the work of the Establishment and assist in crossing the former departmental boundaries. I myself joined R.A.E.

* In a letter to Professor O. A. Saunders (dated Feb. 1961) he says that his reason for the change was that Wilfrid Freeman persuaded him that war was inevitable and that he had better take this opportunity of getting worked into the organization.

in this way; so did Professors G. Temple and W. J. Duncan, among many others.

'My move to the R.A.E. necessitated my resignation from the Secretariat of the A.R.C. and paradoxically as a result I saw less of Farren thereafter than I had done previously, He was, of course, in London a good deal and was a very busy man, but there is no doubt that Farnborough responded remarkably to his leadership, though for most of the staff he was, of necessity, a somewhat remote figure. We knew, of course, and admired his intense practical interest in all the work of the Establishment, and particularly his solo flying of the most advanced aircraft, He was very ably assisted by his two Deputy Directors, W. G. A. Perring and H. L. Stevens, as well as by Heads of Department such as G. P. Douglas, Pugsley and Hayne Constant.

'Inevitably, much of the work of the R.A.E. during the war years was in the nature of trouble-shooting. Aircraft were being produced in large numbers and in developing series, and there was almost always some immediate trouble which required urgent attention and solution. It was, however, interesting to note that these troubles almost invariably stimulated more generalized research and under Farren this was encouraged as far as circumstances would allow. It is, perhaps, unfair to pick out particular items, but there were two that I remember because of a personal connexion. One was the question of spring tab flutter; the spring tab—an aerodynamic servo with an added spring, as a means of lessening the heavy control surface movements that resulted from ever-increasing speeds—showed very great promise for almost universal application, but being an aeroelastic device, it was very prone to flutter and it was urgently necessary to deal with this, not only in application to specific aeroplanes, but also in general terms. The second was a series of accidents to the new Typhoon aircraft, a number of which experienced structural failure in the air. Farren's method of dealing with this was to choose a member of staff, not necessarily very senior, and to give him absolute authority to demand from any Department any assistance he felt was required to solve the problem. This method of delegation of full authority was remarkably successful. In the case of the Typhoon, very extensive investigations of all conceivable causes were necessary, but it was possible in a surprisingly short space of time to implement appropriate modifications, after which the trouble ceased. One further instance may be mentioned: when the viability of the jet engine had been fully established by the flights of the E28/39 and the Meteor, plans were put in hand for the design of an experimental supersonic aeroplane which was to be built by Miles Aircraft. This projected venture into a wholly unknown field meant that a great deal of effort at Farnborough had to be turned to urgent research for design purposes, and criteria for wing section thickness, strength and stiffness were all produced in a very short time. In the event, the project was abandoned because it became clear that there would not be enough thrust; but the research done was of considerable help subsequently.

'There was tremendous pressure to initiate and develop equipment of all kinds to meet operational needs often of a new nature. For example, the developments of day and night photography for reconnaissance were of immense importance and urgency, as also were such things as the invention and development of the gyro gunsight, oxygen systems for high altitude flying, bomb gears, the whole complex of activities connected with operational use of parachuting, involving research and development.'

One of us (A.A.H.) was a junior at the Establishment at the time and feels that when Farren took over, though there were some excellent scientists on the staff, doing good work, the place generally was not in the best of shape to face its wartime tasks. It was, to some extent, wrapped in a bureaucratic ball. It took Farren very little time to unwind the ball, de-centralize authority, show that people were expected to act on the authority given them and take responsibility, and generally liven the place up. In this he was greatly helped by the new Secretary for the Establishment, a Mr Jack Wilson, whom he personally sought out and persuaded to join him. The reason for his choice soon became clear in that Wilson, a genial man, loyal in the extreme to anything he undertook, had the peculiar quality of the best Civil Servants in being able to cut through red tape in no time and to get the system working in the direction of enterprise, rather than in deadening initiative. This was one of the changes so badly needed, and Wilson and Farren together lost no time in producing it.

Development of R.A.E. and Bedford

We are also indebted to A. R. Collar, F.R.S., for the following:

'Farren, before he left R.A.E. created, virtually, a new complex of laboratories at Farnborough, with many new buildings, a great wealth of new equipment, a new airfield with the most modern Air Traffic Control, and great advances in the engine laboratories. In addition, he was the moving spirit behind the Bedford R.A.E. project, flew far and wide to find a suitable place, supervised the drafting of all the plans and obtained acceptance of them including a splendid and most powerful addition to the aerodynamics laboratories and a full-scale testing complex, capable, in addition, of giving adequate runways for the testing of any very advanced aircraft then foreseen—a most memorable and potent achievement.

'During the war years at Farnborough, Farren became something of a legend. He was, without question, universally respected, though he was not universally liked; he affected a brusqueness of manner and brevity of speech, which was no doubt dictated by the fact that he was always very busy, but which was less palatable to some of his staff. He had nevertheless a very real regard for his staff and for the work

that they were doing. He left Farnborough to join Blackburn Aircraft in January 1946 as Technical Director. I myself left Farnborough at the same time, for academic work, and Farren almost at once recruited me as a Consultant to advise him on any matters of a technical nature.

'I got to know him as a man very well during his two years with Blackburns, though I never knew for certain what prompted him to go. I can, however, hazard a guess. Blackburns was one of the pioneer firms in British aviation and Robert Blackburn, the founder, was devoted to aircraft. He said to me on my first visit as a Consultant "This firm is not primarily interested in making money: it is interested in making aircraft. If as a result of this we also make money, all well and good; but it is not our prime concern." I think he may well have said the same thing to Farren and this would unquestionably have found a sympathetic and live response in Farren's mind. Moreover, it was a difficult job offering a powerful challenge. Blackburns had become specialists in Naval aircraft, but had not produced any really successful designs during the war years. The design of Naval aircraft was not easy; dimensions and weights were limited by the necessity for use on aircraft carriers, while the detailed requirements of the Navy in such matters as radomes, black boxes, and so forth were such as to limit the designer very closely. But in addition to these difficulties, the design staff at Blackburns was not then of the first quality. There was clearly an urgent need for a strong technical figure to try to put things right and this was the challenge that Farren accepted.

'During this period, he had moved back to Cambridge and his week was divided between Brough and the London offices of Blackburns, with weekends at home. This meant that he lived a rather lonely life. While at Brough, he lived in a hotel in Beverley and it was his delight in the evenings to drive out over the rolling hills of the East Riding countryside. Frequently, when I was visiting Brough, I accompanied him on these outings and we became very good friends. I think it was then that I realized that he was essentially a shy and reserved man in his personal life, while at the same time on technical and engineering matters he was incisive and forthright to a degree. It was on one of these trips that Farren expressed to me his absolute conviction of the importance of engineering: he said "If the prize is worth achievement, the Engineer will always find the way to attain it". Under his leadership, Blackburns began to advance fairly rapidly on the technical side; up-to-date and able young men were recruited and technical equipment was purchased on a scale beyond anything that seemed to have been contemplated earlier; in this way the design team became far more self-sufficient than it had been in the past, and did not have to rely to anything like the same extent on outside advice and on tests carried out elsewhere.

'Events, however, were too strong even for Farren. Blackburns had for a long time owned premises at Feltham which were tenanted by General Aircraft and moves were afoot to effect a merger, which in the event* came about; the firm became Blackburn & General Aircraft. The proposed new organization implied a new structure for the Board of Directors and I suspect that some Directors, in Farren's view, were over-concerned with finance. Understandably, Farren did not confide in me the detail of the disagreement that developed between him and members of the Board, but the breach became too wide and in December 1947 he resigned from Blackburns and joined Avro, Manchester.

'Looking back on Farren's two years at Blackburns, it is difficult—since two years is a very short time—to pinpoint anything of a major character for which he was responsible. Blackburn's had only one aircraft of any consequence at the time, namely the Firebrand, and while this went through various marks, there were no major modifications. During the War little, apart from the Firebrand, had been done on the design side; the factory had been mainly occupied in building other aircraft for the War effort, including Swordfish and Stirling aircraft. There was, however, one new design put in hand, the Blackburn YA5, an anti-submarine monoplane, which began its life at this time, though the prototype did not fly until after Farren had left Blackburns. However, the firm was struggling to expand and Farren must have been a party to the decisions in July 1946 to set up North Sea Air Transport Ltd at Hanworth Air Park, and to form in 1947 Blackburn (Dunbarton) Ltd as a separate subsidiary. He also took a considerable interest in trying to develop the engine side of Blackburn's work. They had been in the engine field since the early 1930s with the well-known Cirrus light engine; there was a continuing, though small, demand for this engine and Farren foresaw that there might well have to be changes and developments, which indeed did appear subsequently.

'He really gave of his utmost and wore himself out in the service of aeronautics in this country—his pragmatic mind constantly envisaged the essential basic needs for science, technology and industry, in men and major equipment and he acted to create the answers.'

Farren joined A. V. Roe in 1947. Davies was at that time Chief Designer, following the death of Chadwick, who had started the thinking at Avros on the delta wing aircraft which subsequently emerged in the Vulcan. Farren was appointed Technical Director of the Company. The two men, totally different in character, struck up an accord of great value to each—Davies would concentrate on the engineering, design and experimental manufacture. Farren would look over the basic concepts, but in particular would handle the flight test and research departments, and deal with external relations, particularly with the Royal Air Force, and Government Departments involved. The Vulcan was evolved through a series of flying models—the 707s—which established the basic aerodynamics of what was then a totally new configuration. In Davies's view Farren was superbly good at the 'politics', while giving a personal depth of analysis to the flight and wind tunnel results that were crucial to the ultimate success of the project.

* January, 1949.

In the course of the evolution of the Vulcan, a difference of opinion arose between Farren and his colleagues and his friend and successor at the R.A.E., W. G. A. Perring. Perring, backed by the aerodynamic department at R.A.E., concluded that there was a basic aerodynamic fault in the concept of the Vulcan, particularly related to the interference drag between the wings and the body, which was of so radical a nature as to make the project unsound. Farren, with his remarkable flair for amalgamating the theoretical and the practical managed to sustain the project despite this very difficult position, and subsequently events justified the view he took.

Under his leadership, the development programme proceeded with the one-third scale flying model aircraft; the 707 in 1949; the 707B in 1950; and the high speed 707A in 1951.

The success of this development resulted in the Vulcan Weapon System of which the prototype flew in August 1952, and the first production model in early 1955, and into Service in 1956.

Farren was knighted in 1952. While at A. V. Roe he was also directly connected with the Shackleton developments and was partially responsible for the policy which produced the most successful development of Shackleton aircraft through Mk. 1 to Mk. 3.

In retrospect it was clearly a master stroke to invite Farren to head the technical team responsible for the Vulcan project. Before Farren's arrival, A. V. Roe had employed a few highly skilled or experienced scientific engineers, particularly in the aerodynamics field. They had no wind tunnel, no aerodynamics or flight development departments and it was these shortcomings which were soon perceived and remedied by Farren.

The Vulcan went into service, Davies left A. V. Roe and was succeeded by Ewans, who had been the Chief Aerodynamicist under Davies. By then the Company had been given a new project to design and build a supersonic bomber—the project was subsequently cancelled. Farren entered into the thinking on the new project on the same basis as had occurred with the Vulcan, but there is little doubt that the remarkable accord between him and Davies which was essential to the Vulcan situation was not reproduced with Ewans. The men worked closely, but the accord was less than it had been, and with the cancellation of the project Farren turned more of his attention to other matters in the Hawker Siddeley Group—and in particular to the possibility of the Company entering into work on nuclear power—and to external work particularly with the S.B.A.C. and the Royal Aeronautical Society.

In 1954 Farren took responsibility for another new division at Manchester covering the design and development of missiles and systems. This Weapons Research Division ran in parallel with the Aircraft Design Division and was under the day-to-day control of R. H. Francis. Farren was thus directly concerned with the development of the Blue Steel Missile, toegther with the programme of scale models. He was also responsible for the integration of the missile into the V Bomber Force.

In early 1954 catastrophic accidents occurred to two Comet aircraft. The Comet was the first jet-propelled aeroplane to be put on passenger service, flying at speeds and heights substantially above those that previous piston engine types had been able to attain. The accidents led to a very thorough technical and scientific enquiry into the aircraft and its characteristics and to the holding of a Court of Inquiry under Lord Cohen. Sir William Farren was appointed as one of the three Assessors attached to the Court. The Court sat for six weeks of continuous taking of evidence and subsequently produced a report which, together with the reports on the technical and scientific investigations, had very important and considerable influence on the structural design of aircraft. The cause of the accidents was established as a failure of the pressure cabin under fatigue stresses.

In the late 1940s Farren initiated discussions within the aircraft industry about whether the industry should set up a research association with wind tunnel facilities to supplement equipment of that type available in the Government Research Establishments. He was undoubtedly motivated by recognizing that it was to some extent desirable that work directly pointing to the design of specific aircraft should first be done under the auspices of the aircraft industry, while recognizing that equipment as expensive as this could not be laid down at every designing company. The Association was formed in 1952 and Farren became a member of its Council and remained so until he retired in 1965, being Chairman from 1962 to 1965. From 1953 to 1962 he was Chairman of the Technical Committee of the Council which particularly concerned itself with the design of the equipment and subsequently with the research work carried out with it.

His great interest in the training of apprentices was shown in the build up of the A. V. Roe training department. It was partly his work in the training of apprentices which led to his receiving the Honorary Degree of D.Sc. from Manchester University.

Farren continued to have overall responsibility for both aircraft and missile sections at A. V. Roe until August 1961 when he transferred to the headquarters staff of Hawker Siddeley Aviation as a special consultant.

There is no doubt that Sir William Farren was an exceptional leader and a very capable and qualified man. He directed, but did not interfere, and placed full responsibility on the senior members of his team. The appearance he invariably gave of aloofness almost to the point of rudeness belied the hidden human side to his character. The lasting impression is of a man of immense intellect, somewhat ascetic on the surface but with an underlying warmth which occasionally came through.

Public Lectures and Honours

Farren became a Fellow of the Royal Aeronautical Society (about 1930) and President of it in 1953-1954. He was knighted in 1952. Other honours included the M.B.E. in 1918, C.B. 1943, F.R.S. 1945. He was an Hon. D.Sc. of Manchester University and an Hon. Fellow of the Institute of Aeronautical Sciences (U.S.A.) 1953.

It so happened that Farren made important speeches on three occasions connected with the Wright brothers. The first of these was in Washington on 1 December 1943 when he gave the Wright Brothers Lecture to the American Institute of the Aeronautical Sciences, of which he later became an Honorary Fellow. As it was in war-time, security reasons prevented him giving a detailed up-to-the-minute account of some branch of aeronautics as was usual for this lecture. Instead he gave a brilliant exposition of how aeroplanes had improved between the wars and of the ways in which improved knowledge had caused changes in design. He stressed the great importance of basic research, design, construction and flying being done by people who are in close contact. This is a view with which few would disagree, but few have the qualities of brain and character to bring it about. He in fact was one of the very few who had the qualities and experience needed to understand and sympathize with all four groups. As it happened, his year of office in the Royal Aeronautical Society included the 50th anniversary of the Wrights' first flight. In proposing the toast to their memory at a dinner, held in conjunction with the Royal Aero-Club, he described their work as a combination of that of an artist, of an engineer and of a scientist, and started with the artist because that capacity he believed, supplied the force driving them all on.

Two years later, in May 1956, he received the gold medal of the Society and gave the 44th Wilbur Wright memorial lecture on 'The aerodynamic art', being then Technical Director of Avro. The lecture was largely about the boundary layer, laminar and turbulent, and effects on it of speeds near the speed of sound or even beyond it, which were already important and growing still more so. He used no mathematics but many diagrams. He thought visually. The lecture is remarkable for its combination of very difficult science with practical problems. It was applied science at its best. He finished by explaining why it takes about ten years to produce a new aeroplane, four of these are mostly occupied in learning about the problems which the novelty produces.

We are indebted to Dr S. B. Gates, F.R.S., who during a large part of each of the two wars was at Farnborough with Farren, for the following:

> 'When Sir George Thomson reminded me that I worked with William Farren at Farnborough during two critical periods in his career (1916-1920 and 1940-1945) and was good enough to suggest that I might amplify this memoir of a mutual friend by recapturing my impression of him in those years my first reaction was to look sadly at the gaping hole in my near octogenarian memory and say no . . .

'However, after some months of thinking about the Farren I knew and writing nothing I find at least two reasons for saying yes. The first has nothing to do with the alleged frailty of old memories, that clarity is gained as the past recedes. I find much more to write and also to laugh about in the early period just because it has the sharpness and happy point of pioneering: everything was worth doing because very little had been done before. The second reason is a personal and even private one. The Farren I remember was the front room boy almost by definition: dynamic and dominating, aggressive and formidably articulate, able and more than willing to kick open the door and accept every challenge the headlong pace of aviation in our time had to offer him. By contrast I was almost exactly his opposite, static in space, passive, detached and nearly inarticulate in habit and inevitably the back room boy.

'There must have been around 1910 an imaginative civil servant who connected Kitty Hawk with the looming prospect of a European war, who dreamed of air power and said to himself "Is this the beginning of a new science? Is it possible to refine and improve the Wrights' designs until they become new weapons, lethal in their own right? Very well then, the obvious course is to make Farnborough the main growing point of aeroplane design and assemble there men who were already devoted to aeronautics or were distinguished enough in related fields to help a new technology through its infancy".

'This first essay in deploying scientific manpower was enormously developed the second time war came round, when Farren and I met again. The first time round it was an imaginative stroke of organization, which only ran into trouble on the rocks of aircraft design. Its men were collected mainly from Cambridge. The obsolescent balloon factory was reorganized roughly to receive them in a free for all to stretch their talents for productive output. They were housed in a mess at Chudleigh. Perhaps I may recall them at this levelling distance of time by calling them the Chudleigh lot. Divested of the letters they later collected before and after their names they were:

Geoffrey Taylor, Hermann Glauert, George Thomson, Melvill Jones, Frederick Lindemann, William Farren, Ronald McKinnon Wood, David Keigh-Lucas, F. Aston.

'I don't remember how the Royal Aircraft Factory was split up between them. They spent their days in B.A. Dept. The significance of B.A. escapes me now, but as was almost inevitable it was rendered Bloody Aristocrats by non-members. The rest of their time was spent similarly at Chudleigh where they were said to play erudite games like 3-dimensional chess when they were not analysing what the day's inspiration and observations brought them. They were as wildly individualistic as any élite are—and about twice as hardworking, which they had to be if they had any chance of keeping up with their contract.

'I can only imagine how I met Farren, who was then among other things head of Aerodynamics Dept but I have no doubt about the consequences. He must have looked me up and down in his bright brusque way, measuring and probing me. He certainly introduced me to Bryan's skeletal summary of the harsh mathematics of aeroplane motion. Someone had to be launched on that choppy sea of derivatives and left to find his way to the shelter of approximate design rules. Farren breezed in from time to time to see how I was getting on with "those angles", the θ, ϕ, ψ which anyone who has dabbled in stability and control will recognize. I was not getting hold of anything very firmly. I remember asking myself what I was doing among all those odd contraptions of wood, fabric and wire and those who argued so fiercely about them. All the same something was happening to me. The code names of the aircraft of those years—BE2C, FE2B, RE8, SE5—do more than ring a bell, they play a nostalgic tune. In one aspect these children of the Wrights were the saurians and pterodactyls of aeronautical evolution; in another they were the emblems of a new order of adventure, perhaps of chivalry.

'It was through the pilots that I latched on to aeronautics. But I shall not forget how Farren was my linkman as I grew with this strange community. He was always busy in my background, interrupting my meditations among the "derivatives" to haul me into the air (though I was horribly airsick) when some sharp demonstration of the importance of reducing them to order was always to be found. He had made up his mind about me and let me grow into that bracing air. I shall not forget the wisdom of his underlying kindness.

'I remember those years most vividly as the battle of the drag curves, where there was an annoying discrepancy between flight observations at Farnborough and wind tunnel results at N.P.L. There was the daily chore of poring over the forest of points won from the day's flights and wondering how to collapse them to a single curve. This could not be done because most of the parameters to advance the analysis were years away. Reynolds number was on the aerodynamic map and was hopefully labelled scale effect. But they had a great deal of risky fun, particularly those (Farren and Lindemann among them) who climbed into the exposed cockpits of those years to find out what the pilots had to put up with.

'But their fun stopped short at the central insecurity of their arbitrary status as aircraft designers. As the first war in the air elevated its crude concept of air power to the dogfight the notion that a scientific Brains Trust could be relied on to design even the dogfighters grew very thin. Was aircraft design going to be an art or a science, the inspiration of a drawing board or the solution of a system of equations? Put in these too simple terms the choice has never been made, though Farren was often to plunge into the controversy. The facts of life in 1915-1920 leant heavily away from the idea that aircraft design could operate effectively by government agency alone. The B.A.s were not alone in the Chudleigh field as the war ended. Other men in obscure corners up and down Britain, with little money and makeshift equipment were asking whither from Kitty Hawk as they fashioned their own dreams of the next aircraft generation. They were in true succession from the Wrights: many of their names would become famous. Consequently the authorities of those days reversed their first decision towards the creation of hardware and got an aircraft industry firmly on the move.

'It was less than a generation later when Farren and I worked together the second time round. So hot had been the pace of aeronautical progress that those carried along by the immense improvizations had ceased to regard it as a near miracle. It was the year of the Battle of Britain. Churchill had arrived and was dragging Lindemann along with him. Trenchard, secure in his lonely vision of what air power could do had gone into battle for his chosen instrument and won for the R.A.F. out of the remains of the R.F.C. and the R.N.A.S. what was to be an equal place in the trinity of Britain's military power. Melvill Jones had shown how to conjure the streamlined monoplane out of the Xmas tree biplane of the Kitty Hawk. Mitchell had brilliantly taken the hint and created for Fighter Command the generation of Spitfires and George Edwards was soon to do the same for Bomber Command. Watson Watt, worrying at the problem of spotting an attacking aircraft long before the eye could see it had gone into partnership with Tizard to erect the radar defence of Britain. The aircraft industry rejoicing in its luck (if that is the word) of supplying two air wars in less than 50 years had so made haste to quicken the aircraft generation that its hole and corner origin was already like a bad dream of the past. Against the odds, Britain's side of the account looked like adding up to the opening chapter of a success story. But Hitler stood on the other side of the equation, and until he moved to destroy, the tension of our mood could not be more than one of grim resolve.

'The signs of great change were less at Farnborough than at most other centres of this powerful technological advance. The R.A.E. was well settled in its routine as a strictly research establishment. The planners had not been at its buildings and what they housed: they had grown by scruffy addition to the muddle of huge shanties they started with. The only interloper was concrete, replacing the grassy expanse of Laffan's Plain. But when Farren returned as the latest of a quick and comically various succession of R.A.E. Directors he had fulfilled his destiny as a restless climber. He had soon kicked open the back room door to launch himself on a notable piece of mountaineering through the complex layers—scientific, academic, industrial, administrative—of the aircraft establishment.

'He was now one of the small group of top men who literally had another and much more critical war on their hands. This second time round I had hardly moved from where I was the first time: he used me as one of his consultants. Our association was edgy with the pervading tension of the times, it had none of the informal fervour of the B.A. days when anyone could push into the unknown with the support of everyone else. Now I was sometimes invited to listen through a directors' meeting, at which this director did most of the talking. And here I must mention what I made of Farren's talk. He was without exception the most rapidly articulate man I have ever known. You had to be pretty hardy and even-tempered to weather the torrents of Farren's private and public speech without (as I would often say) getting wet. He seems to go on and on not always because he knew where he was going but always because he knew the line he would take and was confident and aggressive enough to push it home regardless. Those with tongues were always trying to get a word in edgeways; my score remained at zero. Beneath his discourse there was, if you listened hard enough, a lurking question. Occasionally he would appeal to me: "If you think this won't work for God's sake say so now", a freedom I seldom used. I would watch these displays of virtuosity, scoring up silently my "yet-but's". But more often than not I found in thinking over his lectures (and they were always that) that most of my "yes, but's" had turned into "yes". In speaking of aircraft design he spoke of an art, however much he often implied to the contrary. I was not alone in feeling fuffled by these displays—nor in admiring the courage of his panache.

'So what do I think about William Farren, having looked back at the morning freshness of his youth and the strenuous activity of his hard pressed middle age? He was by temperament a fount of restless energy, a ubiquity man. He would try everything once, never staying long in one place to serve the headlong pace of the aircraft he loved to develop, never long enough to get stuck as a specialist. There is a certain pattern in the truth about him as stated by one who knew him better than I did.* You would think that a man who spread himself so busily over so large an area of great development as the British aircraft industry would have left his name on some of its pages. But Farren is not written there. This is not to be associated with any shallowness in what he did. Farren did what he did over an immense area because he wanted to be himself. It was in his nature to be always busy, and busy he was until in the end he stopped—astonishingly it seemed—into silence. Some notable men leave no memorial but the rumour of the energy that drove them. William Farren was one of these.'

* Arnold Hall, *J. R. Aero. Soc.* November 1970, p. 898.

Sailing and Sketching

Farren's very real love of nature showed itself during the time between the wars in sailing on the network of artificial and natural waterways which drain the former fens north-east of Cambridge. He acquired a small cabin sailing vessel in which he and his family spent week-ends. After the second war this was sold and he bought a small house for holidays near Burnham Overy where he kept a sailing dinghy. Throughout his life he amused himself by sketching, mostly the scenery of the fens.

As a friend he was most faithful and unselfish. He had wide interests outside engineering and was a good talker, impressive by his judgment.

Lady Farren died on 28 February 1963. She had been in poor health for some years. In October 1963 he married Miss Mildred A. Hooke, formerly Headmistress of Bradford Girls' Grammar School, who survives him. She looked after him devotedly throughout the five years of his final illness. He died on 3 July 1970.

Besides those whose names appear with their contributions, we wish to thank the following who have most kindly helped us with information on various points:

S. D. Davies	J. Lloyd
P. de K. Dykes	Sir Morien Morgan
J. A. G. Haslam, M.C., D.F.C.	N. B. Surrey
Ronald Hills	Sir Geoffrey Taylor, O.M., F.R.S.

Our special thanks are due to Lady Farren for her care in preparing her husband's papers.

The photograph is by Walter Bird.

G. P. Thomson
A. A. Hall

Appendix

Membership of Committees (see p. 223)

Other Sub-Committees and Panels of the Aeronautical Research Committee

Other Sub-Committees and Panels on which he served prior to April 1945 include: Engine Sub-Committee (1929-1945), Stability and Control Sub-Committee (1931-1937, 1939-1945), Accidents Investigation Sub-Committee (1937-1938), Elasticity and Fatigue Sub-Committee (1937-1938), Meteorology Sub-Committee (1935-1938), Oscillation Sub-Committee (1936-1938), Fleet Air Arm Research Committee (1940-1945), Seaplane Sub-Committee (1931-1941), Flutter Sub-Committee (1927-1931), Load Factors Sub-Committee (1929-1931), Compressed Air Tunnel Sub-Committee (1928-1930), Large Wind Tunnel Sub-Committee (1929-1930), Relations with Industry Sub-Committee (1935-1936); Lubrication Panel (1931-1937), Interference Panel (1928-1937), Servo Control Panel (1933-1936), Carburation Panel (1931-1936), Spinning Panel (1931-1936), Atmospheric Turbulence Panel (1931-1935), Air Cooling Panel (1932-1934), H.E.2 Panel (1933-1934), Sleeve Valve Panel (1930), Free Flight Panel (1938-1941), Whirling Arm Panel (1936-1938), Airscrew Panel (1923-1925, 1927-1937).

Committees of the Aeronautical Research Council

Apart from his service on the Council and on the Aerodynamics Committee (see p. 222), Farren served on the following Committees subsequent to April 1945:

Mechanics Committee (Official March-December 1945), Naval Committee

Research Committee (Official March-December 1945), Air Warfare Committee (S.B.A.C. member 1953-56).

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