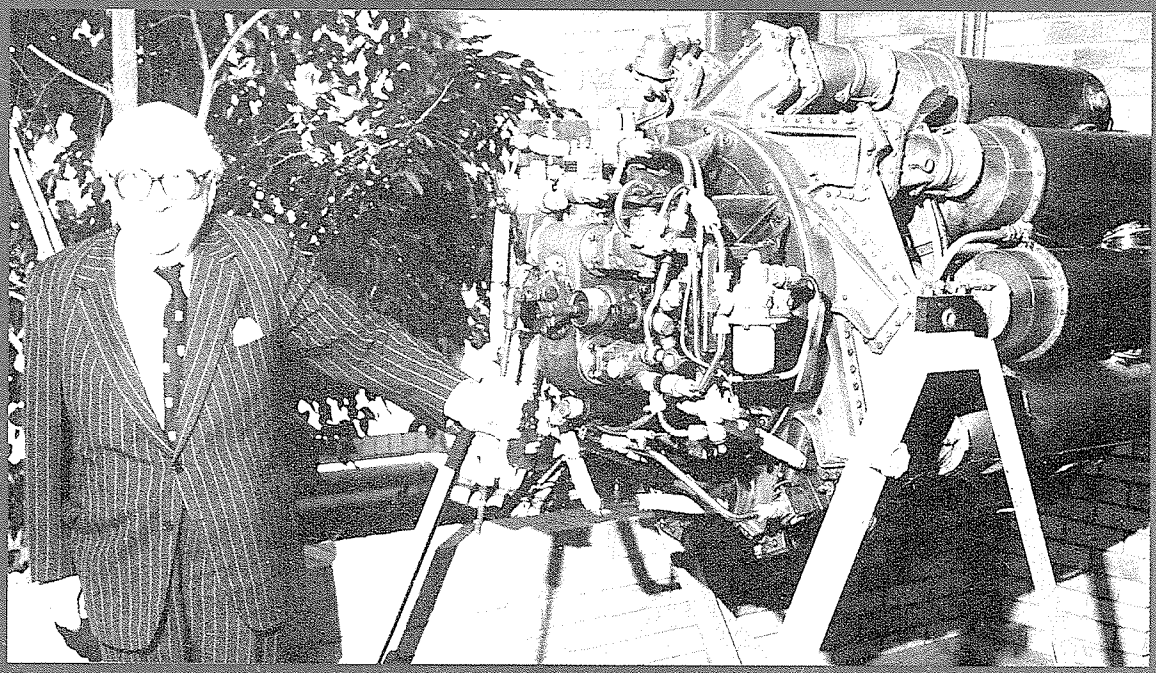
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The AERONAUTICAL JOURNAL



Lord Kings Norton commemorative issue

Volume 103, Number 1022 April 1999

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The aims and scope of *The Aeronautical Journal* are intended to reflect the objectives of the Royal Aeronautical Society as expressed in its Charter of Incorporation. Briefly, these are to encourage and foster the advancement of all aspects of aeronautical and space science. Thus the topics of the *Journal* include most of those covered by the various Sections and Groups of the Society, such as aerodynamics (including fluid mechanics), astronautics, dynamics and control, flight simulation, guided flight, noise and vibration, propulsion, rotorcraft, structures and materials, systems and test procedures. Papers are therefore solicited on all aspects of research, design and development, construction and operation of aircraft and space vehicles. Papers are also welcomed which review, comprehensively, the results of recent research developments in any of the above topics.

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The Royal Aeronautical Society reserves the right to reject a paper which is not submitted in the required manner.



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1.0 PREPARATION OF PAPERS

1.1 General

For a paper to be considered, three clearly typed (double spaced) copies must be sent to the Editor with photocopies of figures (including any photographs) if not included within the printed text. Handwritten manuscripts are not acceptable. The accompanying letter must state that the paper has not been published previously or submitted for publication elsewhere.

The receipt of papers will be acknowledged by return, with a copy of these conditions and a reference number which should be used in all correspondence.

Prior to submission, manuscripts should be read critically by a third party who is familiar with the subject area and has a good grasp of the English language. Authors must also obtain permission where necessary to use any material in a paper which is copyright or the property of any other persons or entity, including their employers. Any fees incurred are the sole responsibility of the authors.

1.2 Figures

All figures must be provided by the authors. Illustrations should be kept to a minimum and should, where appropriate, be produced to the same scale. A list of figures helps in the production of the paper.

1.3 Full paper format

Formal papers should comply with the structural guidelines below and should preferably not exceed 10,000 words. The following is the recommended generic format:

Title: The title should be kept short and concise.

Abstract: A single paragraph abstract of around 150 words which summarises the paper and contains no references.

Nomenclature: A list of all symbols used in the text and figures, whether familiar or not, should be given in alphabetical order, with for example c before C, and all English letters listed before Greek symbols. Subscripts and Superscripts should, where possible, be listed separately. SI units should be used throughout and are thus not required to be shown here.

MAIN TEXT

1. **Introduction:** Discuss the *raison d'etre* of the work, including previous work by others and how the work being presented aims to advance or complement this.
2. **Descriptive section:** This could be either description of apparatus if an experimental paper, or a discussion of the practical applications if a more theoretical paper.
3. **Theoretical section:** Equations should be numbered in the order given and referred to in the text by number as, for example, Equation (19). Complex groupings should not be included in text, but should be numbered as equations.
4. **Procedural section:** Describe the procedure which utilises that described in (2) above.
5. **Presentation and discussion of results:** Tables of results, numbered in order, should be referred to here, and should include only the main results. Errors should be considered an important part of any analysis.
6. **Conclusions:** This section should be very concise, and bullet points are recommended for clarity. The degree to which the aims have been achieved should be clearly portrayed to the reader. Suggestions for future work or work in progress are encouraged.

References: References should be numbered sequentially in the text as they occur. For example, most commonly for papers[1] and reports[2]

1. Miller, P and Wilson, M. Wall jets created by single and twin high pressure jet impingement, Aeronaut J, March 1993, 97, (963), pp 87-100.
2. Green, J.E., Weeks, D.J. and Brooman, J.W.F. Prediction of turbulent boundary layers and wakes in compressible flow, ARC R&M No 3791, 1979.

and for books[3]

3. King-Hele, D. Satellite Orbits in an Atmosphere, Blackie, Glasgow, 1987.

Appendices: If no suitable reference is available appendices may be used to clarify certain points, such as a step in the theoretical analysis.

1.4 Technical Notes

These can be up to 2,000 words in length and have no set form. They can be abstracts, comments upon unpublished papers, notes on interim results or a prompt for further research. They do not have to contain figures or nomenclature and may be in the form of a letter.

1.5 Engineering Notes

These are a maximum of one page and may be used to communicate practical solutions to problems encountered on the shop floor or in the laboratory.

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2.5 Following acceptance

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The Lord Kings Norton of Wotton Underwood

PhD, DIC, FEng, FIMechE, Hon FRAeS

1902-1997

FOREWORD

Lord Kings Norton of Wotton Underwood, formerly Harold Roxbee Cox, was president of the Royal Aeronautical Society during 1947-1949. His contribution to the Society and the world of aeronautics cannot be underestimated. Throughout his life he was witness to, and influential in, a great many advances, much of which are touched upon in this special issue of *The Aeronautical Journal*. From his early work at the Royal Aircraft Establishment, Farnborough, Lord Kings Norton showed understanding beyond his years. Following this, his pivotol role in the development of the gas turbine should not be underestimated. Later, both the civil service and industry was to benefit from his presence. His contribution to the Royal Aeronautical Society mirrored his role in aeronautics, the two often combined — the discussions that led to the establishment of Cranfield are a case in point. Few of the Society's past or future presidents could hope to contribute as much.

Capt W.D. Lowe, FRAeS
RAeS president

Extract from *A Wrack Behind*

Myself, when young

Lord Kings Norton



I was born on 6 June 1902, in Handsworth, a borough in Staffordshire, which in 1911 became part of Greater Birmingham. Handsworth, in those Edwardian days, could be described as a dormitory suburb of Birmingham. Its character is now vastly changed and today it houses large numbers of our immigrant population.

My father, William John Roxbee Cox, is described on his marriage certificate as a master jeweller and indeed was, at the time of his wedding, working with his father, Samuel James Cox, a reasonably prosperous manufacturing jeweller in Birmingham. Samuel had married an American lady, Emily Power Jones, who was, I believe, distantly related to him, and my father was their only child.

My mother was Amelia Stern, the third child of Henry Stern, a Pole who became a naturalised Englishman. My memory of him is vague but pleasant. I think he must have been a nice man. He was an engraver on glass. He died of pneumonia in 1904 at the age of 54. His brother Louis became a Church of England parson, but did not achieve major preferment.

Harry Stern — he seems to have been more often Harry than Henry — married Hannah Stokes, a forceful personality from Bewdley in Worcestershire, and they had issue Louis, Eva, Amelia, Hildred, Celia and Edith. Eva died very young, but all the others lived into old age. Indeed Louis, of whom I was very fond, died at 84 and Hildred at 94. They were all people of character but not philoprogenitive. I was an only child. Louis had another child, a boy who died in infancy, and Celia, who married Allan Murray, had a boy, Duncan and a girl, Mildred, who are my only first cousins. I believe I have several cousins of more remote degree, for my paternal grandfather had seven brothers, but I am aware of only one, David Roxbee Cox (later Sir David Cox), became Professor of Statistics at the Imperial College of Science and Technology and afterwards warden of Nuffield College, Oxford.

In 1904 my father, then aged 26 (he was born in 1877) went to Canada with the idea of expanding the jewellery exports or starting to manufacture there, I am not sure which. His prognosis was presumably favourable as, a few weeks after his departure, my mother and I followed him. We lived in Montreal long enough for my mother to decide I was destined for McGill, but in some ways which I never determined the venture was a failure and we returned to this country to share a home with my paternal grandparents in Kings Norton, a Worcestershire suburb of Birmingham which also, in 1911, was absorbed into Greater Birmingham.

My earliest memories are against the background of this house, its garden and the road on which it stood. I remember getting lost in the garden playing hide and seek with my father, who had disappeared from my two-dimensional world by climbing onto the roof of a garden shed. I remember my grandmother lifting her skirts to put on high lace-up boots — I had not previously been aware that she had legs — before taking me out in the snow in my push-chair. I remember scribbling in imitation of my grandfather's handwriting and of his attempting to lull me to sleep by promenading with me in his arms up and down the dining room, singing a song entitled 'All over the shop'. I cannot recall why he or the rest of the family, who were, I believe, all present, thought this would work. I remember well a circus procession passing the house but somewhere in this distant little world reality must merge into dream because equally vividly I remember floating down the staircase without touching the stairs or banisters.

I think it must have been shortly before my fourth birthday that we moved into another house in Kings Norton, not far from the first. On one side our neighbours were a family called Wasserman — father, mother and daughter. Father Wasserman was a German Jew who had decided to seek his fortune in England and had married an Englishwoman. His wife was called Lily and their daughter, a fortnight younger than me, was called Sidonie. Siegfried Wasserman

became a naturalised British citizen; my grandfather was one of his sponsors in the process. Sidonie became friendly, so friendly that at the age of four we decided to marry. This did not, however, come to pass, though we remained friends to the end of her life in 1983.

It was at about this time I suppose, that I became interested in trains — not particularly in travelling on them, but in watching them. My grandfather, who in retrospect seemed to have plenty of time to spare, used to take me to Kings Norton station on a fine afternoon and we would sit on one of the benches thoughtfully provided by the railway company and watch the trains go by. Expresses, dashing through at high speed, smoke and steam trailing, whistle blowing and strangely changing note as it passed, were of course the favourites. Goods trains — I think we called them 'luggage trains' then — we thought rather boring. I remember it all so clearly: the two foremen in charge of the station, one tall, middle aged, red moustached, the other rather older, shorter, stouter, black moustached, both wearing blue frock coats with trousers and hats (rather like those worn by French generals except in colour) to match.

The engines of the Midland Railway were dark red and always clean. Best of them were the 4-4-0 express engines; next, in my affections, the 0-4-4 tank engines; least the 2-4-0 and 0-6-0 light traffic engines. They all had an attractive smell of hot oil.

At the age of four I first went to school. The school was conducted by Miss Dagnall. I cannot recall any other teachers. There were not many of us and the eldest two, both girls who seemed tremendously grown up, were ten. I quickly learned to read and write and was encouraged to draw and to model in plasticine.

It was during this period that Birmingham was thrilled by the visit of King Edward and Queen Alexandra to open the new University in Edgbaston. My grandfather hired a room on the first floor of a building in New Street down which the procession was to pass after the opening ceremony about three miles away. We had a wonderful view of the stolid King rhythmically raising his silk hat and the Queen waving vivaciously to left and to right. This was in 1907.

How long I remained with Miss Dagnall I do not remember, but I was still very young, probably still only five, when I went to what was then a famous local school called Saxonhurst. This was an establishment of a very different kind. There was a junior school of little boys and girls, a boys' preparatory school, and a boarding school for adolescent girls. It was really a quite remarkable place. It was run by four sisters, the Misses Stevens — Miss Louie, Miss Bessie, Miss Nellie and Miss Evelyn. There were other teachers, but I remember only Miss Barnard from my infant period and later the young French woman who taught us her language while learning ours.

I know that the methods that the Misses Stevens employed were old-fashioned but they were tremendously effective. In the boys' school we did not see much of Miss Louie, who spent most of her time with the big girls but did a stint with us in emergencies. Miss Bessie sometimes taught us Latin, and Miss Nellie drawing, but the bulk of our instruction was from the youngest and toughest of the sisters, Miss Evelyn. She rammed history, geography, arithmetic and Biblical knowledge into us with ruthless energy.

I cannot remember playing any games at Saxonhurst. Our exercise was to drill with a sergeant in a red coated uniform — on the lawn in summer, in the classroom in winter. When not drilling us, the sergeant was one of the local postmen, who delivered letters three times a day, with oil lamps fixed to their belts in the winter to that they could read addresses in the dark.

In those days the tradesmen called for orders and delivered the goods. On Thursday mornings, for instance, Mr Stokes called to consult my mother about the provisions for the forthcoming week. In a rather mournful voice, having heard her ideas, he would recite a list of things she might have overlooked (pepper, salt, mustard, marmalade, tea, eggs, bacon and so on). Of course, sometimes we had to go to the shop he conducted with his partner, Mr Fittall. As you entered, Mr Fittall would be found on the left behind his counter, dispensing teas, coffees, pepper, biscuits and other provisions which seemed in keeping with his spare appearance and dark moustache. On the right, except obviously on Thursday mornings, and I don't know what happened then, Mr Stokes, fair, stout, pale and placid, equally appropriately served the bacon, ham, eggs, butter and cheese.

This shop was in a street called the Cotteridge. Nearly all the shops were there — Holbeach the butcher, Faulkes the baker, Lynam the ironmonger, Lawton the dairyman (who also sold cold ham and *à la mode* beef), Griffiths the fishmonger, and Bloomfield the chemist, whose shop was dominated by a large glass vessel filled with water in which leeches leisurely disported themselves. There were Rhodes at the china shop, the Misses Day who sold cottons and fabrics, Brookes the corn chandler, Stephens at the post office, and the United Counties Bank, destined one day to become part of Barclays. Altogether a very comprehensive shopping street, with at one end the terminus for the trams from Birmingham, rather unstable vehicles which always made me feel ill.

At Saxonhurst our knowledge was tested in a way of which Mr Robert Robinson's Brain of Britain quiz is reminiscent. We stood in a row in an order determined by the last test in the same subject. A question was fired at the boy at the top. If he answered correctly, boy number two was given a question, and so on. Correct answers produced no change, but failure to answer correctly did. If, say, boy number three answered wrongly, number four had a go. If he was right he moved up to number three and number three moved down to number four. It was possible, in a class of eight — and that was approximately the size of a class — for numbers one to seven all to fail and for number eight to go right to the top, but drama of this kind was infrequent.

We all learned the catechism and, learning a verse a day, memorised whole chapters of the Bible. By the age of 10, we were speaking simple French and reading *De Bello Gallico*. We did not learn Greek. Drawing consisted only of copying patterns from sets of printed cards, which is probably why a great deal of my doodling is of an abstract but symmetrical character. History came mainly from a book called *Little Arthur's History of England*. I do not recall the identity of Little Arthur, but I do recall the rather misleading opening words of the book — "You know, my little Arthur, that the country you live in is called England; it is joined to another country called Scotland and the two together are called Great Britain."

Geography was largely a matter of memorising the counties of England, the bays of England, and the capes of England. Scotland we ignored and Wales was merely part of England. Starting on the capes somewhere in Northumberland we would descend in monotonous recitative the East Coast, noting Flamborough Head, Spurn Head, and Orford Ness; sweep round Kent with due tribute to North Foreland, South Foreland and Dungeness, turn West past Beachy Head and Selsey Bill, reaching in an excited crescendo St Catherine's Point and The Ne...ee...ee...dles. We took a deep breath at The Needles and in a calmer tone, about an octave lower, we began again with Portland Bill, Start Point and so on, working our way steadily round Cornwall, round Wales, up the Lancashire coast ultimately arriving breathlessly at St Bee's Head in Cumberland.

The methods of the Misses Stevens appear extraordinary now, but I don't think they were extraordinary then. We learned a tremendous number of facts, which was particularly useful when we sat examinations for entry into the secondary stage of our education.

In those days the leading secondary schools in Birmingham were all part of the King Edward VI Foundation. They probably still are. Chief of them was King Edward's New Street, and there were King Edward's Five Ways, King Edward's Camp Hill and King Edward's Aston. How the choice was made in my case, I am uncertain. Possibly my family were influenced by convenience in travelling for, except for Aston, they were all close to nearby railway stations on the line which ran through Kings Norton. Whatever the reason, I sat the entrance examination for King Edward's Five Ways, the headmaster of which was a reverend gentleman named McCarthy, in 1912. The examination gave me no difficulty at all. I did the Latin paper, completely correctly, in a quarter of an hour. The Misses Stevens clearly knew what they were about. But though formally received

into the school at a ceremony to which my mother accompanied me, I never went there. My parents, perhaps mindful of the disturbing effects tramcars had on me, appeared to have decided that the journey by train from Kings Norton (stopping at Bournville, Selly Oak, Somerset Road and Church Road) to Five Ways would be bad for my health.

In 1911 the Worcestershire County Council started in Kings Norton itself a new secondary school, which ultimately became one of Birmingham's newer grammar schools. The headmaster was Albert James, who was determined to create a new centre of excellence. My Mother went to see him and as a result, after learning of my entry into KE Five Ways and giving me a superficial test in his study, he accepted me and a few days after the start of the term, he put me into Form IIb, the master of which was a young man called Berryman.

As I had to pick up what had happened in those first few days, I was put in charge of a good looking boy of about my own age called Alan Slatter, and I remained in touch with him, at intervals for the rest of his life.

At Kings Norton I was extremely well taught. When I went there, aged 10, I knew nothing of algebra, geometry, or trigonometry and found my new kind of knowledge fascinating. History became more interesting — though we learned little of history other than our own — and English Literature was a joy. I think it was when I had reached Form IV that I enjoyed the cadences of Lycidas and when I had reached Form V that I was fascinated by the *Morte d'Arthur*. About that time too I read P.G. Wodehouse's *Mike*, and today I confess I am attracted much more to Wodehouse than to Milton and Malory. Wodehouse is as great a master as either of them.

And now I am about to boast. I do not think I am a particularly vain man, but I cannot refrain from remarking the unbeatable record I achieved at my school.

We had a highly competitive system which, I expect, would be criticised very adversely today. We were allotted marks for everything and on every Friday of the term the marks we had gained in the week were added and our position in form recorded. I was frequently first and shared the first places with Messrs Davies, Holbeach and Brown. We moved up the school together and no one else was ever in the first four of our form.

At the end of term we had examinations. These, of course, were marked too. Then each boy's term marks were added to his examination marks and the grand totals used to decide the final 'positions' in the form. At the end of every term in which the system operated (I think in my last term the small VI form, in which people were specialising it did not) I was top. That is the end of my boasting. I do not know whether my achievement was ever equalled: it cannot have been beaten.

In only one subject was I never comfortable — chemistry. I did well enough, but I never liked it. In my senior years I was taught it by the headmaster. Whether chemistry is in fact lacking in theory and logic, or whether I was taught in a way which made me think so, I am uncertain. I hesitate to criticise the headmaster. He taught me French excellently. But somehow, in chemistry, I was certainly far less at home than in anything else, and this failing had a decisive effect on the conduct of my career, an effect which might indeed have been disastrous.

I was at that time, 1918, destined for the University of Birmingham. Being excused the matriculation examination of that university on the strength of my performance in the Oxford Senior Local examination, I was expected to enter the second year after what was called the university's Intermediate examination. The headmaster told me that I should be taking chemistry in that examination. This depressed me utterly, and I was determined to escape the test. I persuaded myself that the pursuit of a university degree was an unnecessary delay in developing the career I wanted, and I persuaded my family, who were utterly ignorant of the importance of academic instruction, that I ought immediately to enter the world with which I had for so long been enamoured, the world of aviation. So my father, then working as his war effort at the Austin Motor Company, got in touch with John Kenworthy, who was at the head of the aeroplane design organisation of the company. The result was that at the end of the summer term in 1918 I left school and entered the aeroplane drawing office at Longbridge.

Contributions of Harold Roxbee Cox (Lord Kings Norton) to specialist aspects of aeronautical technology

Dr Geoffrey Pope, FEng, FRAeS

Prior to his departure from the Royal Aircraft Establishment in 1938, Roxbee Cox published an impressive range of specialist papers relevant to the design and analysis of airships and aircraft.

His earliest published paper[1] (Refs are on p 227), on the external forces on an airship structure, was based on his work on the R101 at the Royal Airship Works, Cardington. This comprehensive monograph, which was awarded the R38 Memorial Prize by the Royal Aeronautical Society, addresses, in turn, the range of loading conditions that needs to be considered, safety factors to be used in design, calculation of individual load components, and estimation of stresses within the structure (this consisted of longitudinal and transverse members braced with wires).

Papers produced by Roxbee Cox after his move to the Royal Aircraft Establishment in 1929 relate mainly to aeroelastic effects resulting from the finite stiffness of aircraft wings. Initial publication was generally in the Reports and Memoranda series of the Aeronautical Research Committee; the papers were written, in some instances, jointly with colleagues, and are concerned either with the calculation of wing torsional stiffness (the calculation of bending stiffness was already well understood) or with aeroelastic effects *per se*.

This work was undertaken at a time of rapid change in aircraft structural design due to the advent of stressed-skin construction. The earlier papers on the estimation of torsional stiffness[2, 3] relate to two-spar wings whose stiffness is contributed entirely by the differ-

ential bending of the two spars and the restraint imposed by stiff ribs. A subsequent paper[4] takes account of the torsional stiffness of closed-cell spars and makes some allowance for the contribution of the skins. The implications of the assumptions made in the analysis are stated clearly, with due acknowledgement of the desirability of treating the wing more rigorously as a multi-cell tube (reference is made to the work of Duncan on this aspect). The only other publication by Roxbee Cox on the torsion of thin shells is a brief note on the optimum design of single-cell tubes which includes some useful and now familiar results deduced by his students at Imperial College[5].

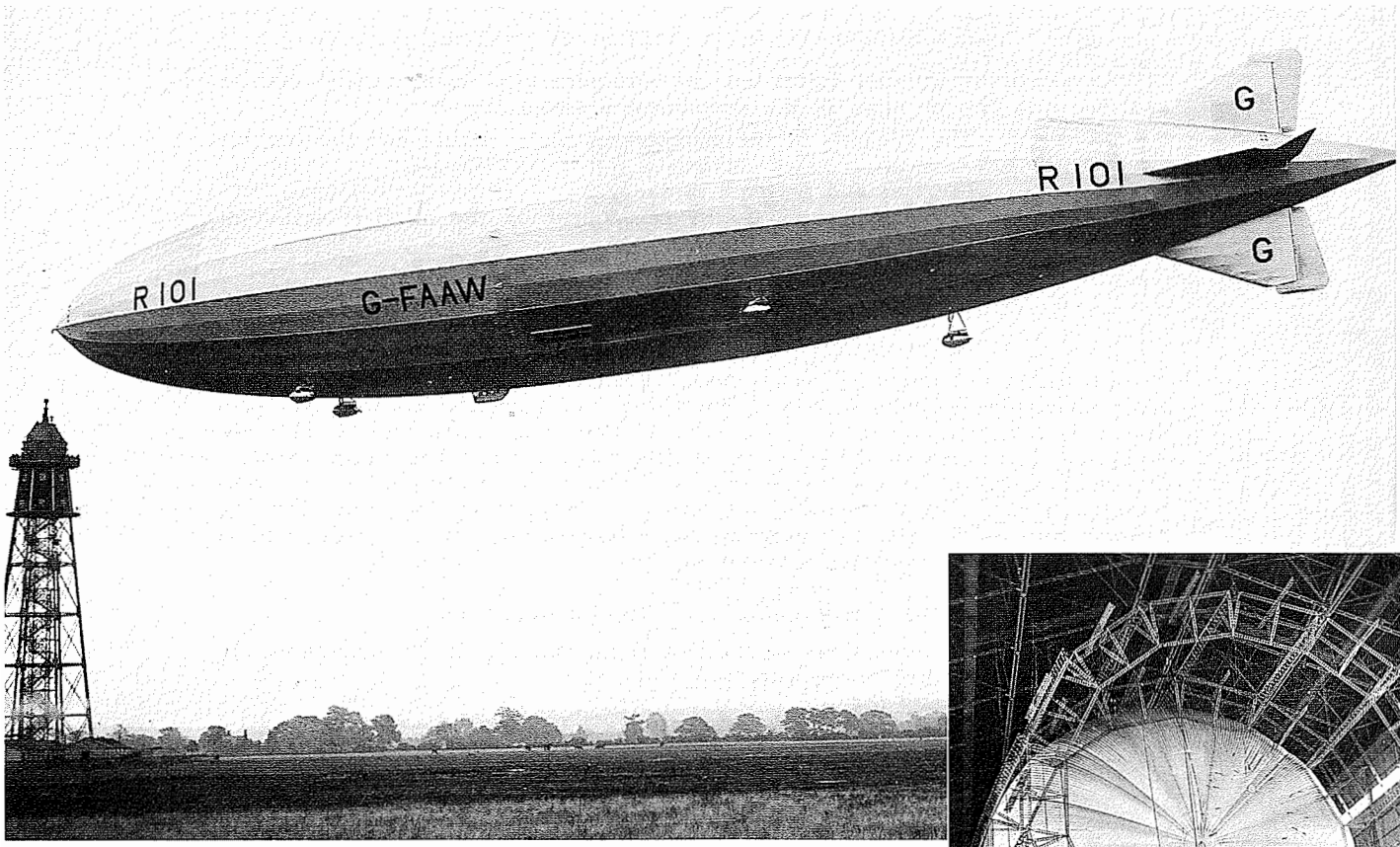
Turning now to static aeroelastic effects resulting from finite wing stiffness, three significant papers[6-8] were produced by Roxbee Cox in collaboration with A.G. (subsequently Sir Alfred) Pugsley. These are concerned both with divergence and with the estimation of aileron power and introduce, for the first time, the concept of the reversal speed at which aileron effectiveness drops to zero. A rapidly convergent iterative method is described for the calculation of this speed and a simpler alternative approach, based on a 'semi-rigid' representation of the wing, is proposed for use in wing design. Some general conclusions are also drawn on the effect of wing taper and aileron span on reversal speed.

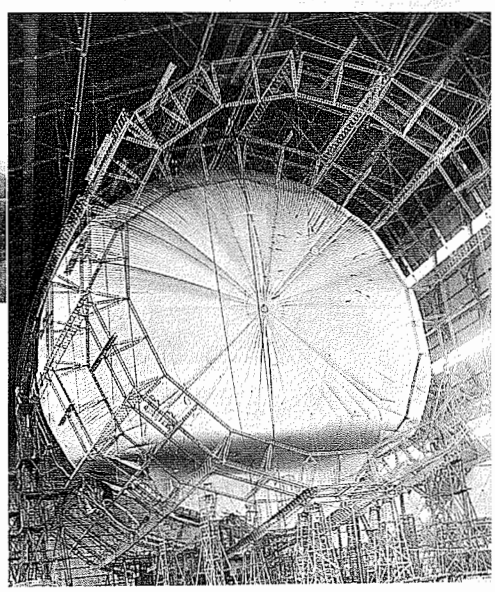
Roxbee Cox's paper, A statistical method of investigating relations between the elastic stiffness of complex wings and wing–aileron flutter[9], is of interest from the historical viewpoint. His basic thesis, at that time, was that it would not be practicable to carry out an elaborate series of tests and calculations, based on the work of Frazer and Duncan, to evaluate the flutter characteristics of every wing design. He advocated therefore an approximate technique whereby the behaviour of a new wing could be deduced from the characteristics of existing wings. While the paper is well argued, its basic rationale is obviously superseded as a consequence of subsequent developments in analogue and, more recently, digital computing capabilities.

Much of the work reported first in the aforementioned Aeronautical Research Committee publications is brought together conveniently in a paper presented by Roxbee Cox to the Royal Aeronautical Society in November 1933 and published subsequently in the Society's journal[10].

In the realm of structural analysis Roxbee Cox also produced a series of fundamental papers[11-13] on the synthesis and analysis of three-dimensional statically-determinate frameworks, two of which were written subsequent to his departure from the Royal Aircraft Establishment. His interest in this subject had presumably been stimulated by his earlier involvement with airship design. Such structures are seldom used today, so the rather elegant theorems that he deduced appear now to be mainly of academic interest.

All the diverse papers referred to above demonstrate clearly Roxbee Cox's ability to synthesize complex structural and aeroelastic problems in physical terms and to produce dependable analytical results in a form comprehensible to an aircraft designer. The two contributions of most enduring historical significance are the paper concerned with external forces on airship structures, and those relating to loss of aileron effectiveness due to wing torsion.





Above: The R101 leaves the mooring mast at Cardington on its final trial flight before leaving for India, 4 October 1930. Right: The R101 structure with a partially-inflated gas bag.

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THE EXTERNAL FORCES ON AN AIRSHIP STRUCTURE* WITH SPECIAL REFERENCE TO THE REQUIREMENTS OF RIGID AIRSHIP DESIGN

BY

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THE R.38 MEMORIAL PRIZE PAPER FOR 1928

§1. THE COMPONENTS OF A SYSTEM OF EXTERNAL FORCES

A system of external forces on the structure of a rigid airship can be analysed into two or more of the component systems due to

1 W. The Weights of the members of the structure and the weights of the masses carried by it;
2. L. The Lift and pressure of the gas;
3. A. The Aerodynamic forces consequent on the relative motion of the airship and the air;
4. T. The Thrusts of the airscrews;
5. R. The Reactions due to either the mooring arrangements, the hauling-in guys, the supports used in the shed, or handling on the ground.

As these component systems, though restricted by their interdependence, are individually variable, a very large number of complete systems is represented by the possible combinations and variations that can occur. It becomes necessary, therefore, for the purposes of design, to attempt to limit the number of combinations to be considered, and to restrict the variations of the components, but in such a way that the structure designed to carry the resulting systems with adequate factors of safety, shall be able to carry any other possible system with an appropriate factor of safety.

§1.1. *The Possible Combinations of the Components*

The possible combinations of the five components with the conditions for the ship to which they correspond, are

(a) Ship in the Shed
I W, R;
II W, L;
III W, L, R;
IV W, L, T, R;

* Submitted March, 1928.

The first few pages of The external forces on an airship structure with special reference to the requirements of rigid airship design[1].

(b) Ship in Free Flight I W, L;
II W, L, T;
III W, L, A, T;
IV W, L, A;

(c) Ship at Moorings I W, L;
II W, L, A, R;
III W, L, T, R;
IV W, L, A, T, R;

(d) Ship being moored I W, L, A, R;
II W, L, A, T, R;

(e) Ship being handled I W, L, R;
on the ground II W, L, A, R;

The number of these combinations cannot be conveniently reduced until the restrictions on the variations of the components have been defined.

§1.2. *The Variations of the Components*

§1.21. *Component W*

The weights supported fall naturally, as has been indicated, into two categories. The first, described as the Fixed Weight, consists of the essential structures and fittings, which cannot in ordinary circumstances be moved, and the second, known as the Disposable Weight, consists of all remaining items, and includes the load which the ship is designed to transport. The Disposable Weight can vary both in amount and distribution, though the portion which may be termed the Transport Load, if it consists of passengers, cargo, or mail, can only vary in distribution. If the Transport Load consists of aeroplanes, however, as it may in future airships, it will be possible for it to vary in amount during a flight, but this possibility will not be considered implicitly in this essay, as the means of taking it into account will be apparent, without additional explanation, from the procedures to be described.

When all the disposable weight which can be carried is in the ship, it is said to be in the Heavy Condition. This may be characterised as the condition of the ship at the beginning of the longest flight it can make when carrying its full Transport Load. The most extreme condition of an opposite character can obtain at the end of the flight, when all the disposable weight other than the transport load and the minimum safe reserve of ballast, has been used up or jettisoned. This condition may be termed the *Light Condition with Transport Load* on board.

The ship will not always be required to fly with the maximum transport load, so that at the opposite end of the scale of heavy conditions to the one described above, is one representing the conditions at the beginning of a flight of the same length with no transport load. This may be termed the *Heavy Condition with no Transport Load* on board. At the end of a flight under these initial conditions, the disposable weight left in the ship will be a minimum, and the ship is described as being in the *Light Condition.*

There doubtless exist, intermediate between these four, weight conditions equally severe in their effects on the ship's structure, but there is generally no reason to suppose that they could produce actions anywhere on it appreciably *more* severe than those occasioned there by at least one of the four clearly defined limiting conditions that have been briefly described. The design is safeguarded, moreover, by adequate factors of safety.

The primary restriction placed on the component system due to weight, therefore, is that its allowable variations shall be four only, defined as

Wa. The Heavy Condition.
Wb. The Heavy Condition without Transport Load.
Wc. The Light Condition with Transport Load.
Wd. The Light Condition.

In the combinations given in §1.1, therefore, W can generally be replaced by Wa, Wb, Wc, Wd, in turn. The weight distributions corresponding to the four conditions should all balance about a point in the ship which, when the ship's axis is horizontal, is vertically below the point marking the centre of lift when the gasbags are full, but modifications to these distributions are permissible in cases where the equilibrium of the complete loading system requires it.

§1.22. *Component L*

In normal circumstances the ideal arrangement is for the lift and weight to have such magnitudes and distributions that they together provide a system in static balance. It is reasonable, therefore, to adopt as the bases of possible variations in component system L, four conditions, which, when taken with Wa, Wb, Wc, and Wd, provide four systems in equilibrium. The primary variants of the lift component system can therefore be

La. All gasbags full, when the lift balances the weight for Wa.
Lb. All gasbags proportionately deflated until the lift balances the weight for Wb.
Lc. All gasbags proportionately deflated until the lift balances the weight for Wc.
Ld. All gasbags proportionately deflated until the lift balances the weight for Wd.

The lift of the gas may become unequal to the weight for various reasons, and its distribution may vary. The consequent possible inequalities and variations in the static force system are accounted for by the consideration of the effects of superheating and of certain conditions of gasbag deflation to be specified. The first of these conditions is that due to the complete deflation of any one gasbag. This produces for the portion of the structure near the deflated gasbag a distribution of static forces more serious than can be derived in any other way, and is, perhaps, more logically regarded as catering with some reserve of strength for the worst static force distributions that are likely to occur, rather than as catering for the remote possibility of accidental deflation.[1] Unless balance is achieved statically, the differences between total weight and static lift arising from the consideration of each gasbag completely deflated in turn for each of the variants La, Lb, Lc, Ld, provide a set which it is unreasonable far to exceed in building up cases for design; and the corresponding set representing the maximum design conditions for excess of lift over weight is equally reasonably provided by the maximum amount of superheating of the gas which can occur.

If the more literal view of complete gasbag deflation is taken, then what may prove for Lb, Lc and Ld a more serious distribution of static forces than that implied above, occurs if the deflation happens simultaneously with superheating, though the excess of weight over lift is less than when the gas and air temperatures are the same. This change in distribution cannot arise in the variant La, however, as the inflated gasbags are already full and gas is released from the automatic valves when expansion occurs, leaving the total lift the same. If, however, when all the gasbags are full, superheating has occurred and the gas has then cooled to the air temperature, the effect is one of the

[1] It has occurred once in the history of British airships.

proportional partial deflation along the ship. This state of affairs is realised to some extent if the ship enters a region of warm air, and, as it is also representative of the general heaviness to which an airship is liable from various causes, the case of proportional partial deflation should be examined for all four variants of the lift component system, though it is unlikely to prove serious. The simultaneous occurrence of gasbag deflation with this uniform deflation produces a greater difference between weight and lift than any other system, and in a list of variations of the L component which contains superheating with deflated gasbags, this variation should be included.

The maximum amount of superheating will occur when the gas volume is originally such that the expanded volume just fills the gasbags. If, before the superheating of the gas, the ship is in trim, a condition which may be considered to represent La with superheating is obtained, and may be considered with gasbags deflated in turn.

It would appear then that the possible inequalities between weight and lift, and the possible distributions of lift, are comprehended in a scheme of component systems L which, in addition to the systems La, Lb, Lc, Ld, includes the variations in Table 1.

TABLE 1.

VARIATIONS IN THE COMPONENT SYSTEMS La, Lb, Lc, Ld, TO BE CONSIDERED IN DESIGN.

1	La	with superheating
2	Lb	
3	Lc	
4	Ld	
5	La	with gasbags proportionately deflated
6	Lb	
7	Lc	
8	Ld	
9	La	with gasbags deflated in turn
10	Lb	
11	Lc	
12	Ld	
13	1	with gasbags deflated in turn
14	2	
15	3	
16	4	
17	5	with gasbags deflated in turn
18	6	
19	7	
20	8	

§1.23. *Component A*

The distributions of aerodynamic forces which it is possible for the ship to experience fall into three classes:—

1. Those due to steady relative motion of the ship and the air;
2. Those due to a relative motion of the ship and the air which changes in a steady manner;
3. Those due to an unsteady motion of the ship and the air.

These forces are conveniently resolved into directions parallel to three mutually perpendicular axes of reference fixed in the ship. These are shown in Fig. 1. Forces parallel to the axis OX are termed *Longitudinal Forces,* those parallel to OY are termed *Lateral Forces,* and those parallel to OZ are termed *Normal Forces.*

When the axis of the ship and the direction of the relative wind are inclined to one another and both lie in the same vertical plane, the angle between them is the *angle of pitch* of the ship. The inclination of the ship to the horizon is measured by the algebraic sum of the angle of pitch and the angle between the horizontal and the direction of the relative wind or flight path of the ship. If the ship's axis is in the horizontal plane and at a point on it the relative wind is inclined to the direction of the axis there, the angle of inclination is the *angle of yaw* at the point in question.

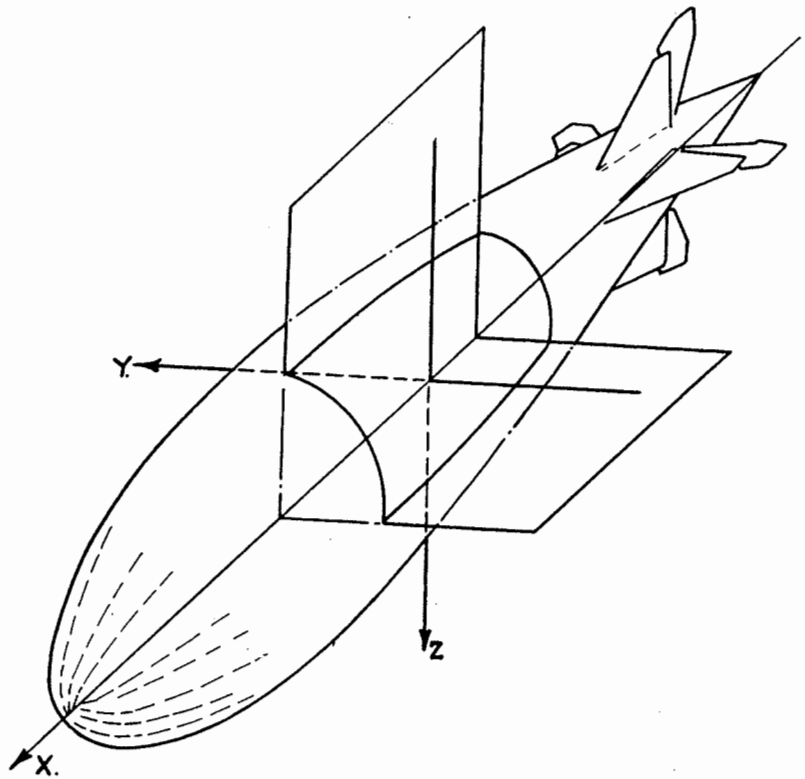


FIG. 1.

§1.231. *In Free Flight*

When the relative motion is steady, the angles of pitch and of inclination to the horizontal are constant, and have each the same value at all points in the ship. The forces produced can be ascertained fairly accurately, and can be resolved into longitudinal and normal components. For the angles of pitch which the ship is likely to have in flight, these components both increase with the pitch angle, so far as the hull is concerned, while the air speed is constant. In actual flight, however, a change of pitch has, among its consequences, changes in thrust and probably elevator angle, as well as a change in air speed, so that it is not possible by inspection of experimental data to select an angle of pitch which is likely to produce the most stringent conditions for pitched flight.

It becomes necessary, therefore, to consider the forces for various degrees of pitch at the maximum possible speeds. The angles suggested are 0°, 5°, 10°, 15°, but numerical work on these may indicate the advisability of considering other angles as well.

When the ship is performing a steady turn its path is circular, while the angle of yaw varies continuously from nose to tail. This case also can be satisfactorily dealt with, and produces forces which resolve naturally into longitudinal and lateral components. As in pitch, it does not follow that the forces are most severe when the yaws are greatest, so that it is advisable to consider circling flight both at maximum rudder angle and at some smaller rudder angle, at the probable maximum speeds in each case.

It is possible for the ship to have simultaneously both angles of pitch and yaw. It is suggested, however, that this case, which is extremely difficult to analyse satisfactorily, is unlikely to produce conditions which are not covered by the worst pitch and circling flight cases, for the speed of the ship, lessened by both pitch and yaw, must necessarily be extremely low. The question is discussed further in Appendix I.

Accelerated motions in free flight can arise in several ways. The most important are :—

1. The acceleration[2] consequent on switching off the engines;
2. The acceleration consequent on starting the engines;
3. The angular and linear accelerations due to attaining or returning from an angle of pitch;
4. The angular and linear accelerations due to going into or getting out of a turn;
5. The angular and linear accelerations consequent on coming into contact with gusts.

In the first two cases it is difficult to imagine lateral or normal forces greater than those arrived at in the steady motion cases. The inertia forces are mainly longitudinal, so these cases are adequately dealt with by considering longitudinal forces for the ship in horizontal motion only.

Cases 3 and 4 can, with certain assumptions, be treated as step-to-step calculations, but are more conveniently dealt with by assuming the elevators and rudders respectively moved rapidly from amidships to the hard-over position. It is concluded in §16 that the conditions, while getting into a steady turn, are intermediate between those for rapid rudder movement and steady turning, and a corresponding conclusion may be reached in the case of pitch. Any attempt to consider the case of rapid movement of the rudders from one hard-over position to the other while in a steady turn will not result in more severe actions. It is a less accurate investigation, and will not be considered, though the treatment would be on the lines of §16.

With regard to case 5 above, the information on gusts is at present meagre, and calculations cannot be made with sufficient accuracy to warrant their application to actual design. Calculations on severe assumptions, some of them along the lines suggested by Mr. Burgess,[3] indicate, however, that gust conditions have a reasonable factor of safety in a design produced without implicit reference to them, and experimental information recently obtained supports this view.

§1.232. *Ship at the Mast*

When the ship is at the mast, and a steady wind is blowing, the worst aerodynamic conditions that can occur depend on the maximum degrees of pitch and yaw which the ship can attain and the maximum wind speed at which it is considered safe to remain moored. The maximum degree of yaw depends on the maximum possible rudder angle. The primary purpose of the rudder is the turning of the ship, and there is, in consequence, no particular reason why full rudder should occur at the mast, and thereby produce a most severe condition of loading. It has been suggested, therefore, that arrangements be made to limit mechanically the possible rudder angle at the mast. There is only one reason, apparently, why the rudders should not be locked amidships, and that is that most ships are not stable at the mast, and need a small rudder angle to prevent hunting. It seems, therefore, that the allowable rudder angle must be either the angle of maximum travel, an angle sufficient for stability, or some arbitrary angle intermediate between these. The choice will affect the factor of safety to be associated with the case.

The maximum degree of pitch depends on the elevator angle, the trim of the ship, and the possible temperature gradients, together with any automatic means which are designed to limit the pitch. A reasonable limiting figure is 5°, as before the ship had attained such an inclination measures would have been taken to arrest it.

The worst aerodynamic condition exists when the ship has at the same time its maximum pitch and maximum yaw at the maximum wind speed decided on. This speed is given as 60 m.p.h. by the Airworthiness of Airships Panel[4], though a designer may have reasons for choosing a higher.

Gusty wind conditions at the mast can cause severe lateral loading on the ship, which is especially important in the region at the nose, and if gusts are treated as rapid, but steadily varying changes in the wind, it is possible to calculate their effects on lateral forces in certain simple cases. The necessary theory for the cases of

1. " A wind of constant linear velocity changing in direction with constant angular velocity."
2. " A wind constant in direction increasing from zero velocity with constant acceleration, some initial angle of yaw being assumed;"

is given[5] in Appendix III. to " A Review of the Present Position with Regard to Airship Research and Experiment," by Lt.-Col. V. C. Richmond.

These conditions represent the effect of gusts which are sufficiently large to operate simultaneously on the whole ship.

§1.233. *Ship being Moored*

With the latest methods for bringing the ship on to the masthead, described by Major Scott in his paper on " The Development of Airship Mooring,"[6] the possible aerodynamic conditions are sufficiently like those when the ship is actually attached to the masthead to need no independent calculations. The direction of the reaction at the nose can be different, however, and the effect of this needs examination.

§1.234. *Ship being Handled on the Ground*

It must be assumed that the ship will *not* be handled on the ground in a wind greater than 20 m.p.h. The effect of this wind blowing broadside on to the ship may be considered.

§1.235. *Variation of Component A*

Summarising, the variations of the component system A to be considered are those given in Table 2.

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[2] Negative.

[3] U.S.A. National Advisory Committee for Aeronautics, Report No. 204.

[4] R. & M. 970, p. 10.

[5] Journal of the Royal Aeronautical Society, Vol. XXX., p. 577.

[6] Journal of the Royal Aeronautical Society, Vol. XXX., p. 465.

TABLE 2.

VARIATIONS IN THE COMPONENT SYSTEM A TO BE CONSIDERED IN DESIGN.

Relative Motion Steady.

1. Ship in free flight, travelling at the maximum speed possible at pitch angles o°. . .15°.
2. Ship in free flight, circling at the maximum speed possible with maximum rudder angle and at some smaller rudder angle.
3. Ship at the mast with maximum pitch and wind speed 60 m.p.h.
4. Ship at mast with maximum yaw and wind speed 60 m.p.h.
5. Ship at mast with maximum pitch and maximum yaw, and windspeed 60 m.p.h.
6. Ship on the ground, with broadside wind of 20 m.p.h.

Relative Motion Changing Steadily.

7. Ship in free flight, decelerating at o° pitch.
8. Ship in free flight, accelerating at o° pitch.
9. Ship in free flight, in act of pitching.
10. Ship in free flight, getting into or out of a turn.
11. (Ship in free flight, meeting gusts).
12. Ship at mast, wind veering.
13. Ship at mast, wind accelerating linearly.

§1.24. *Component T*

The thrust is definitely settled by the aerodynamic requirements. In Table 2 the requirements of maximum speed in the cases 1, 2, 8, 9 and 10 decides the thrust. Case 7 requires the maximum thrust at no speed of advance, and the remaining cases have no thrust component.

§1.25. *Component R*

The nose reaction due to mooring or hauling-in is the most important reaction. It is determined by the other forces operating. The reactions in handling the ship on the ground are not likely to prove important except for local fittings, and the same remark applies to shed reactions.

§2. FACTORS OF SAFETY

Having reviewed the variations of the component systems, and indicated which of them on their own merits require examination, it becomes necessary to expand the combinations given in §1.1. into the cases which should be considered for the purpose of design, and to allot to these cases suitable factors of safety.

If the distributions of loading of the structure considered for design were all equally likely to occur in practice, then the factor of safety in each case would be the same. Actually, however, some distributions are more likely than others, and it is logical to allot to the stressing actions representing a frequent occurrence, a higher factor of safety than that allotted to the actions resulting from a less frequent occurrence. This view is strengthened by the fact that any chosen distribution of forces is no more than an average of distributions actually occurring in the circumstances from which it is derived. That is to say, if in a given airship, a certain gasbag is supposed deflated and then static balance is achieved, while maximum speed is maintained, the distribution of weight, lift, and aerodynamic force may be made precisely the same as the designer has assumed in his calculations for the case; but in practice, due to the movement of passengers and crew, the consumption of fuel, the exigencies of manœuvring, the unsteadiness of the atmospheric conditions and other possibilities, the forces on the structure will vary in distribution, and in consequence, in almost any structural member, the stress will fluctuate about a mean which is probably close to the figure calculated for the case. Although these fluctuations are probably disordered and intermittent, and unlike what is generally meant by an alternating stress, the idea of fatigue is introduced, and the case for allowing " probability " to control the factor of safety is strengthened. Another consideration is that if in any case there is reason to suppose the fluctuations are greater than the average, the factor should be correspondingly raised. This view can be argued in the case of gusts, where the factor should be higher than that arrived at on the grounds of infrequency.

These considerations are qualitative, and no quantitative rule can be suggested for obtaining the factor. Its selection must be to a large extent arbitrary, though guided by the foregoing considerations, by experience, and by the conclusions of the Airworthiness of Airships Panel[7] on the matter.

§3. THE SYSTEMS OF EXTERNAL LOADING TO BE CONSIDERED IN DESIGN

§3.1. *Ship in Shed*

§3.11. W.R. Combination a1 in §1.1, in which the ship is without gas in the shed, is not likely to provide severe conditions of shear, bending, or transverse distribution, as there will be supports arranged at every frame and under important concentrations of mass, such as power cars. The case need not be dealt with therefore unless special supporting arrangements or other particular reasons make it necessary. The method of developing the primary actions for the case in such circumstances will be apparent from the detailed treatment of ensuing cases.

§ 3.12. W.L. During the gassing of the ship, it will continue to be held in such a way that no important primary actions can arise. It is possible, however, for a passenger flight to start from the shed, so the heavy conditions represented by (Wa, La) (Wb, Lb) are possible; similarly, the possibility of flights ending in the shed makes conditions represented by (Wc, Lc.), (Wd, Ld) possible also. Except perhaps in the heavy condition, the gasbag deflation conditions can also be considered to occur, but it will be seen that all the (W, L) shed conditions can be covered by free flight conditions.

§3.13. W.L.R. If the weight is unequal to the lift in the shed, it may be assumed that the local differences at transverse frames will be made up by reactions applied there, so that the shears across bays will remain approximately zero, and the bending moments correspondingly small.

§ 3.14. W.L.R.T. In the event of a test of the power plant in the shed, it may be assumed that the vertical forces are approximately balanced at every frame. Only if longitudinal reactions are not applied *at* the engine bearers can the whole structure remain unstressed, and the manner in which the structure is stressed depends on where the reaction to prevent movement of the ship is applied. This is not a case[8] which affects the whole ship, but it needs examination for the structural members which carry the power plant, with, it is suggested, a minimum factor of safety of 3.

§3.2. *Ship in Free Flight*

§3.21. W.L. The ship can be stationary in the air, and the condition is approximated to when it rises from, or lands slowly to the ground. All the conditions represented by Wa, b, c, d, can occur, and the gasbag deflated conditions, 5-8, 9-12, 17-20 of Table 1 can be supposed. In these latter cases, as there is no forward relative motion between the ship and the air, equilibrium must

[7] R. & M. 970, p. 9.

[8] It is probably, in any case, covered by Cases 89-104. §13.

Extract from *A Wrack Behind*

Cardington

Lord Kings Norton

The performance of German airships during the war, and the double crossing of the Atlantic by the airship R34 in 1919, sufficiently impressed the Air Ministry that it embarked on a British airship programme which produced R38 and R80. The Ministry was undoubtedly deterred by the appalling R38 disaster in 1921 but, presumably as a result of Professor Bairstow's demonstration of the inexcusable inadequacy of the R38 design, they did not abandon their plans. After all R80, to the design of B.N. Wallis, the great engineer who became famous as Sir Barnes Wallis after the 1939-45 war, had been a success.

So in 1924 the Air Ministry decided to order two airships, each of five million cubic feet capacity. One, to be called R100, was ordered from the Airship Guarantee Company, a Vickers subsidiary headed by Sir Denniston Burney. It was to be built at Howden. The other, to be called R101, was to be built by the Air Ministry itself, at the Royal Airship Works, Cardington. At the head of the Burney design team was B.N. Wallis. At the head of the Air Ministry design team was V.C. Richmond.

In July 1924 I reported for duty at Cardington, which is about two miles from Bedford. Richmond told me that I was a temporary civil servant graded as a 'calculator' and put me in charge of T.S.D. Collins, a naval architect who had been concerned with the design of the ill-fated R38. For quite a long time afterwards the calculating department staff was two in number.

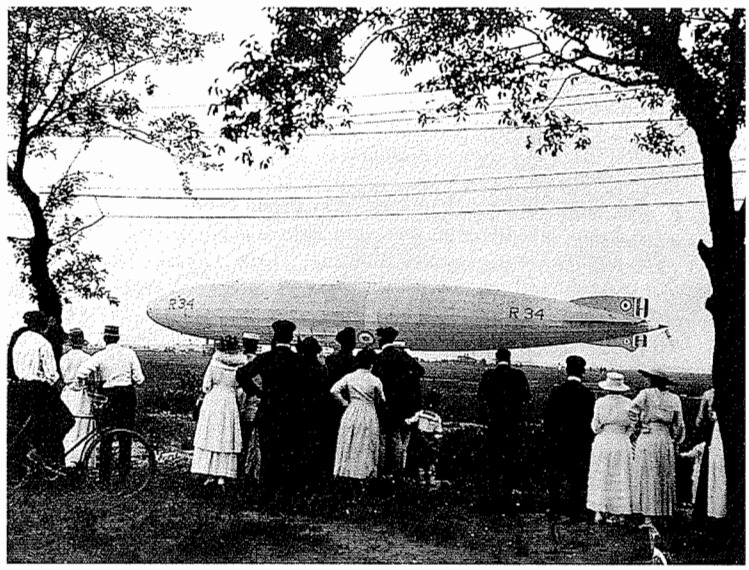
Collins was an extraordinarily kind man and I was technically responsible to him for the whole of my time at the Royal Airship Works, though the organisation was rather loose and Richmond, who had not recruited Collins but had so-to-speak inherited him via the Air Ministry, was apt occasionally to by-pass him.

Collins immediately gave me a conducted tour, via the administrative offices, the drawing office and the hydrogen plant to the two great sheds, one of which housed the unfinished airship R37. He appeared to think I should view this from the narrow walkway which ran from end to end of the shed just under the apex of the roof, which was 180ft high. So up we went.

The first part of the climb was up a series of staircases but the last part included a ladder about 20ft long which ran up nearly vertically with the floor of the shed over 100ft below.

Collins swarmed up this appalling structure. Perhaps naval architects have to climb up ships' masts. At any rate the ladder clearly had no terrors for him. It had for me but there was no alternative. I had to follow, and my satisfaction at safely completing the ascent was utterly negated by the realisation that I had to return. However, I finished the trip unscathed but swore I would never do it again.

Clearly, in those early days, the design of the ship was in a very fluid state, though the basic notion of a structure with heavy transverse frames and relatively light longitudinals was in the air. One day Collins took me into a long room in which were men kneeling on a smooth floor bending long flexible wooden laths into curved shapes. I asked Collins what on Earth they were doing and he said that they were developing the shape of the airship. This I gathered was ship design practice, conducted in what was known as the moulding loft. I remarked that however practical this might be in ship design it was quite unnecessary for the profile of an airship. In its place I proposed that we should adopt as the profile a mathematical curve of streamline form, the ordinates of which could be calculated and used to design the ship's structure.



In 1919 the R34 made a double crossing of the Atlantic.

I provided Richmond with a number of such shapes and we selected one in which the length of the profile was five times the maximum diameter (the fineness ratio). The maximum diameter was at 40% of the length from the nose, and the radius of curvature of the profile increased steadily from nose to tail. This was adopted as the shape of R101, slightly adjusted at the nose and tail to accommodate the mooring tackle and fin structure.

Meanwhile, the staff was being built up. I make no attempt to give the order in which people arrived. A few were before me, more after. But when the team was complete it included some unforgettable characters.

At the top was the Director of Airship Development, Group Captain (later Air Commodore) P.F.M. Fellowes, a natural leader who, years later, commanded a flight-over-Everest expedition. Under him was Wing Commander R.B.B. Colmore, charming and efficient. In charge of flying, current and future, was Major G.H. Scott, who in 1919 had taken the airship R34 across the Atlantic and back. Chosen to command R101 was Flight Lieutenant H.C. Irwin and to command R100, about which I shall have something to say later, was Flight Lieutenant R.S. Booth. In the later stages of the design and construction of the ship these flying people were joined by Flt Lt Atherstone (known as Grabby, he was born Grabowski but had adopted his wife's name), and Flying Officer Steff. The navigating expert was Squadron Leader E.L. Johnson.

In charge of the route planning — arranging for the construction of mooring masts and sheds on the intended all-red‡ route to Australia was Major Sidney Nixon, a man who ran attractively sporty motor cars.

Although I soon became acquainted with all these people, I was of course primarily and professionally associated with the technical staff. Richmond was at the head of it. He was a man I liked very much and I believe he was a good physicist. He was not, however, an engineer. He had been associated with the design of non-rigid airships but had no experience of rigid airship design. Very few people had. In my view, he was a good picker and recruited some splendid people. To balance the gaps in his engineering knowledge and experience he had as his chief assistant a person of great brilliance and extraordinary charm — Flight Lieutenant (afterwards Squadron Leader) F.M. Rope. In the difficult business of determining the aerodynamic and other external loads in the airship structure and calculating the consequent stresses in it, he had Collins, myself and later John Baker (who became Professor of Engineering in Cambridge, Sir John Baker and afterwards Lord Baker) and later still (1926) Alfred Pugsley (who became Sir Alfred Pugsley and professor of civil engineering at Bristol), together with a marvellous young woman, Hilda Lyon, a Cambridge mathematician who unhappily died young.

The drawing office had some very bright people. The most senior was Frank Randle, who was very able and highly regarded. There was Jimmy Uren, able and amusing. There was Bill Hayes, a first class product of naval dockyard training and many other clever and imaginative designers.

The production side of the Cardington team was headed by A.H. Hall. Under him, and directly responsible for the construction in the great shed, was Gerrish. The grand scale on which he worked — on which, indeed, he had to work — was very different from what I had been used to at Longbridge, and impressed me enormously.

While Gerrish was responsible for erecting the gigantic structure, the doped fabric cover was the responsibility of J.W.W. Dyer, a chemist of distinction. The hydrogen for filling the great gasbags which Dyer also made was manufactured on the spot by Woodis Rogers.

While the gasbags, the cover and the hydrogen were all made at Cardington, the aluminium members of the great framework and the engines were not.

The structural members were made by Boulton and Paul of Norwich, a firm of builders and engineers which had constructed aeroplanes in the 1914-18 war. The head of the aircraft side of the company was John Dudley North, an engineer and mathematician of tremendous ability. While Cardington specified the strength required in the longitudinal and transverse members, the design of those girders was the work of North and his staff.

The engines finally decided upon were diesels designed and built by Beardmore of Glasgow. The Cardington officer concerned with the airship's engine problems was Wing Commander T.R. Cave-Brown-Cave. He was a clever and extremely energetic person but his undoubted qualities did not prevent Beardmore, a firm with mainly naval engine experience, producing engines of such great weight that, despite their economical fuel consumption, affected the weight of the airship and handicapped its performance.

The basic structure of all previous rigid airships consisted of a number, 20 or more, of longitudinal members running from nose to tail, supported by transverse frames. The rhomboid panels created by the longitudinals and the frames were cross-braced and the frames were braced in their own planes.

In R101 it was decided at the outset that there should be heavy transverse frames which were, in effect, toruses needing no additional transverse bracing, and relatively light longitudinals. This was a novel departure from current practice. It had the advantage of permitting much more accurate calculation of the loads in the framework than was possible with the 'standard' geometry, and loads such as fuel tanks and water ballast containers could be carried within the torus-like structures.

Another departure from standard practice was the choice of only 15 'sides' to the ship, rather than the 20 or more of earlier airships. The Hindenburg had 36! I pleaded with Richmond to have at least 16 sides (as did R100), otherwise we should have serious difficulty in achieving a symmetrical arrangement of members in the region of the fins at the tail. He foresaw no difficulty but the outcome completely justified my fear: at frame 13 there were 15 longitudinals. At frame 14 there had to be 16.

The translation was accomplished by having at the top of the 13-14 bay a Y shaped member, and for nearly all the other longitudinals to be twisted. To design twisted longitudinals in a tapering bay was an appalling geometrical prospect, but the job was brilliantly done by one of J.D. North's staff called Adkin. He used a method based on the use of direction cosines.

After the establishment of the shape of the ship and the outlines of the basic framework structure the major task was to calculate the external loads on the structure. These arose from its own weight, from the static loads such as fuel, ballast, crew and passengers, from the lifting loads of the gasbags and the propulsive forces of the engines, all of which were readily calculable and the aerodynamic loads which were not. These loads arose from the skin friction and pressure distribution on the hull during flight. The skin friction was not difficult to estimate given the results of simple experiments, but the distribution of pressure over the hull was another matter. We had the results of tests in wind-tunnels to guide us but we could not estimate from these figures the pressures on a full-scale hull. To get the necessary knowledge it was decided to make full-scale experiments with the airship R33.

Numerous pressure points were strategically chosen on the envelope and connected by pipes to manometers within the airship. Pressures were measured at different speeds and in different manoeuvres. The general shape of the pressure distribution corresponded with that given by the wind-tunnel tests but we could not have made quantitative estimates of the loads for design purposes without the full scale tests.

These tests were completed in two night flights. For those concerned the conditions were rather uncomfortable. We were all compelled to wear heavy flying suits smelling strongly of camphor and, on top of them, parachute harnesses. The space available to us was limited. There was a small area about amidships where the pressure information from the manometers was gathered. Otherwise we spent most of the time on the nine inch wide catwalk which ran from nose to tail of the ship. Certainly on each side of the catwalk there were bunks suspended but we had little time for reclining and in any case it was rather difficult to get into them unless acrobatically inclined.

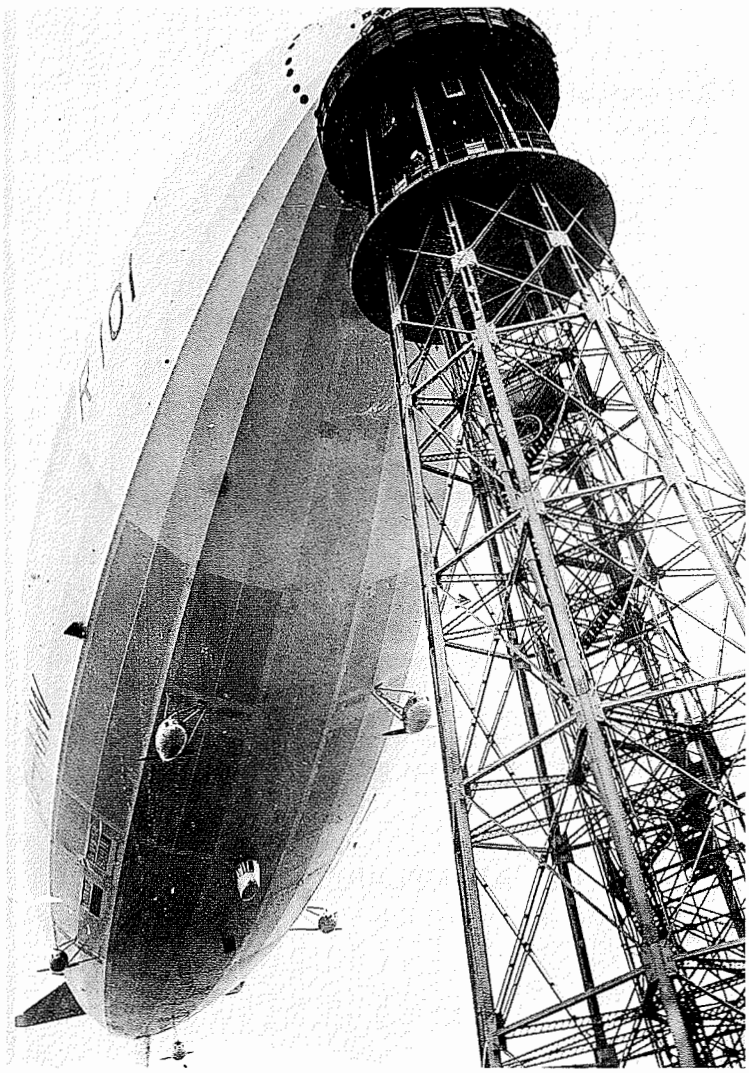
That we did sometimes have time to spare is evident, however, because on one flight I went via the rearmost part of the catwalk and climbed into the tail cockpit, originally designed for a rear gun. Standing there and looking aft, slowly drifting over the English Channel, I felt I was on a little platform in space.

Whether or not it was on the same flight I do not remember, but on one occasion we were over London at about 2am and at about 2,000ft, flying slowly down Piccadilly.

To my very great regret I never flew in R101. In 1929, with the ship nearing completion, A.H. Hall was appointed superintendent of the Royal Aircraft Establishment at Farnborough. He decided that he needed me at RAE and I, by then an established civil servant, was moved to its airworthiness department.

In October 1930 came the awful news of the loss of R101. At first the Government seemed to be set to continue with what was left of the airship programme. Nixon was made superintendent of the Royal Airship Works and I was moved back to become head of the technical staff, with the task of getting R100, which had made a double crossing of the Atlantic and had been moved to Cardington, ready for flight again. But this apparent determination to continue despite

‡These were the days of the British Empire.



R101 on the mooring mast at Cardington shortly before its first test flight in 1929.

disaster ended when, as a result of a governmental economy drive, it was decided to bring the airship programme to an end and, sadly, R100 was broken up and sold for scrap.

Of the two airships, although it had numerous fabric and gasbag troubles, R100 was undoubtedly the more successful. It was designed by a brilliant man who nevertheless stuck pretty closely to established practice, and had the advantage of proven aircraft engines. He made one quite serious error. With only 16 sides the fabric panels formed by the longitudinal and transverse members were shown by flight trials to be inadequately supported. To prevent them flapping in flight a system of wiring attaching them more firmly to the structure was introduced so that the basic panels of the envelope became concave, giving the ship a rather unfortunate appearance and undoubtedly increasing the aerodynamic drag.

Before the ship was finished Burney was disillusioned and Wallis had decided the future was with aeroplanes. He had moved in the Vickers organisation first to Supermarine and then to Vickers at Weybridge, where he found Rex Pierson an agreeable colleague and settled down to design his Wellesley and Wellington aircraft with his geodetic structural system. He never flew in R100.

R101 had the appalling handicap of the Beardmore engines. Theoretically, as they were diesel engines (whereas R100 had petrol engines), it was the safer ship. But it was still not safe enough. The Germans knew that the airship could only be truly safe with helium and, because that was denied them, the Hindenburg caught fire and the great rigid airship era came to an end.

Appendix to Cardington Memoir

When work started on R100 and R101 in 1924, very much in the minds of those concerned in their design was the appalling accident to R38 in 1921.

R38 at the time was the largest airship in the world. It had been designed by naval architects and investigation after its failure indicated that inadequate attention had been paid to the aerodynamic loads in flight. In fact Professor Bairstow, who was at the time undoubtedly one of the world's leading aerodynamicists, calculated that in the high-speed turn during which the structure failed the factor of safety was unity. The ship broke in half and people were killed. When I went to work at the National Physical Laboratory in the long vacation of 1923 I met one of the survivors, Harry Bateman. When the ship broke into two pieces he was in the rear part. He got into the tail cockpit and jumped out with his parachute. Unfortunately, it became hooked in the framework and he found himself suspended below the falling wreck. He saw himself dropping into the Humber and in fact he did, but only up to his waist. He had landed — if that is the right word — on a sandbank!

Another airship accident, in which I was almost involved (and I rather wish I had been) was, I think, in 1926 in the middle of the full scale aerodynamic experiments I referred to in the memoir to which this is appended. The airship R33 was riding at the mooring mast at Pulham in very windy weather. One morning I was scheduled to climb aboard at, I think, 0800 hours. The ship was in charge of a night watch consisting of half the full crew and commanded by Flight Lieutenant R.S. Booth. Unfortunately my car, outside my lodging, failed to start and I took about a quarter of an hour to discover and correct the fault. So I was late in arriving at the foot of the mast, but just in time to see the ship break away from its mooring. The nose structure was damaged and, it transpired, the front gasbag torn and deflated. Booth, who throughout showed admirable coolness and resource, got the engines going and kept the ship under control. To restrict the damage, and while the ship drifted eastward, he used the torn fabric of the cover and the gasbag to make a great mat covering the first transverse frame. In bad weather and this damaged condition the ship flew over the sea and over Holland all day and the following night but Booth got her back and landed her to a ground crew at Cardington.

The 568th Lecture read before the Royal Aeronautical Society since its foundation January 12th, 1866.

PROCEEDINGS

THIRD MEETING, FIRST HALF, 69TH SESSION

The Third Meeting of the first half of the 69th Session of the Royal Aeronautical Society was held in the lecture theatre of the Royal Society of Arts, on Thursday, November 16th, 1933, when a paper on " Problems Involving the Stiffness of Aeroplane Wings " was presented by Dr. H. Roxbee Cox. In the chair, Major J. S. Buchanan, C.B.E., F.R.Ae.S. (Member of Council).

The CHAIRMAN: As long as we in this country were content to build biplanes of moderate speed, the problem of the stiffness of the wings was perhaps relatively unimportant; but having regard to the very greatly increased speeds demanded in modern times, and the more general use of the monoplane, the question of the stiffness of the structure had become one of the important problems which the aeroplane designer had to deal with. Dr. Roxbee Cox was one of those who had been concerned with the problem of the stiffness of wings for some time past.

Dr. Roxbee Cox then presented his paper.

PROBLEMS INVOLVING THE STIFFNESS OF AEROPLANE WINGS

BY

H. ROXBEE COX, PH.D., D.I.C., B.SC., A.F.R.AE.S.

§1. *Introduction*

An aircraft may reasonably be said to be structurally safe if it is known that during the manœuvres which it is likely to perform

(i) The stress in no part of the essential structure approaches the failing stress of the material,

(ii) It does not become unstable or uncontrollable on account of structural distortion.

I intend to discuss in this context the wing structure of the aeroplane.

For many years it was felt both in the design and airworthiness approval of the majority of aeroplane wing structures that the main responsibility for attempting to secure the former desideratum could rest upon the determination of the stresses induced in the wing structural material by load systems appropriate to a rigid aeroplane in certain conditions defined by specified combinations of flight attitude, acceleration, and factor of safety. These combinations—familiarly referred to as the C.P. forward, C.P. back, nose-diving and inverted flight cases—are designed to produce for a particular aeroplane a range of steady load systems which is more severe on the structure than the range of load systems corresponding to the manœuvres contemplated for the aeroplane.

The success of any structure designed to carry varying loads depends upon its qualities of strength and of *stiffness*. The standard loading cases just mentioned merely provide measures of strength under constant loads, and it has

The first few pages of Problems involving the stiffness of aeroplane wings[10].

long been recognised in consequence that, while indispensable, they could not be the only tests of the staunchness of a wing structure. For they take no account of the distortion of the wing structure which must necessarily occur in flight. This distortion, which is dependent upon the magnitude and distribution of stiffness, may be of two kinds. It may be uni-directional and of such magnitude that the conception of the aerodynamic loading as being similar to that for a rigid wing is no longer sufficiently approximate. Or it may be oscillatory, when it is termed " flutter." In connection with the first desideratum, neglect to consider both types of distortion may result in a stress failure. In connection with the second desideratum, the uni-directional distortion may affect stability or lateral control.

It becomes necessary to determine, therefore, what stiffness characteristics a wing structure should have if flutter, undue loss of lateral control, and undue lack of stability are to be avoided and if the assumption of a rigid wing for the purpose of determining aerodynamic load systems is to be sufficiently approximate. Some progress towards this end, which I shall outline in §§2, 3, 4 and 5, has been made. The investigations described indicate the great importance of providing high torsional stiffness in wings, so in §§6 and 7 respectively I shall deal with the torsional stiffness properties of conventional wings and the effect of providing high torsional stiffness on methods of *strength* calculation.

In what follows, bracketed indices refer to papers of which the authors and titles are given in Appendix III.

§2. *Wing Stiffness in the Prevention of Wing-flutter*

The problem of flutter has been discussed in great detail in two monographs[1, 2] by Mr. Frazer and Dr. Duncan, of the National Physical Laboratory, who were able to deduce from their theory certain design recommendations. Of these recommendations, some were in the form of precise statements needing little amplification, but others were necessarily of a qualitative nature. Among the latter was the recommendation " All elastic stiffnesses as large as possible," which, if it is to have practical value, needs quantitative extension.

In the case of a particular aeroplane, it is possible by means of the theory of wing flutter developed by Frazer and Duncan, to determine quantitatively the elastic stiffnesses necessary if flutter is to be avoided, provided that an elaborate series of tests and calculations is made. The procedure is, however, too expensive and laborious to be used either in design or in the formulation of a quantitative rule.

An alternative method which has been attempted in an effort to determine wing stiffness values sufficient to preclude wing-aileron flutter is of a statistical and less approximate nature. The procedure is to correlate the elastic stiffnesses of the wings of existing aeroplanes with their observed behaviour with regard to flutter, noting and allowing for such flutter prevention features of design as may be embodied in them. The adequacy of the wing stiffnesses of a new design can then be judged by comparing them with the information obtained.

§2.1. In developing this procedure, it was necessary first to determine

(i) In what circumstances different aeroplane wings could be considered comparable,

(ii) The criteria by means of which quantitative comparison could be made.

§2.2. It was found possible to formulate a theoretical argument if the wing-aileron systems compared, though of different aspect ratio, had

(i) Corresponding spanwise dimensions in plan in constant ratio,

(ii) Corresponding chordwise dimensions in plan in constant ratio,

(iii) Wing sections similar,

(iv) Distances from leading edge of flexural axes, transverse principal axes of inertia, and axes of independence respectively constant fractions of the corresponding chords,

(v) Similar distributions of mass,

(vi) Similar distributions of elasticity.

§2.3. With these provisos, it was possible to deduce by dimensional theory that

(i) For a motion involving flexure of the wing and flapping of the aileron the oscillatory instability (flexural-aileron flutter) speeds are related for wings for which δ/c is constant by

$$(1/V)\sqrt{(l_\varphi/s^3)} = \text{constant}, \quad . \quad . \quad . \quad . \quad (1)$$

(ii) For a motion involving torsion of the wing and flapping of the aileron, the oscillatory instability (torsional-aileron flutter) speeds are related for wings for which δ/c = constant by

$$(1/V)\sqrt{(m_\theta/sc^2)} = \text{constant}, \quad . \quad . \quad . \quad (2)$$

where s = span of the wing (feet),
c = mean chord of wing (feet),
δ/c = " wing density "
$= \dfrac{\text{wing wt. per unit area}}{\text{mean chord}}$ (lbs./feet^3),
V = airspeed (ft./sec.),
l_φ = typical flexural stiffness (lbs. feet/rad.),
m_θ = typical torsional stiffness (lbs. feet/rad.).

§2.4. Strict recognition of the six provisos above would in practice prohibit any comparison of wings ever being made. Further investigation into the magnitudes of the errors involved in departing from the strict interpretation, however, has indicated that

(i) (i) and (ii) of §2.2 may be taken to mean that monoplanes must be considered separately from biplanes, and that aeroplanes with unusual wing or aileron proportions must be excluded from the comparative data,

(ii) (iii) and (iv) of §2.2 may be ignored,

(iii) Within the classes given in (i) above, (v) of §2.2 is approximately obeyed,

(iv) The variation of distribution in elasticity sometimes must occasion a sub-division of the classes of (i) above. Thus, for example, while many externally braced monoplanes show curves of wing distortion under tip loading similar to the curves for cantilever monoplanes, others differ so considerably as to form a class of their own.

§2.5. As an example of the practical application of the conclusions reached, we may consider the comparison of a number of typical monoplanes. Their several distributions of flexural elasticity may for practical purposes each be represented by the flexural distortion produced by a load placed near the wing tip.[3] Their distributions of torsional elasticity may each be represented by the torsional distortion produced by a torque applied near the wing tip. For twenty-six monoplanes for which the necessary information was readily available the flexural distortion diagrams fell into two groups represented by the envelopes A and B shown in Fig. 1. For these monoplanes, consideration of torsional distortions produces the same classification. It was considered that the monoplanes in each group were sufficiently similar elastically to be compared among themselves.

The next step was to make use of the comparative criteria of §2.3. To obtain these, values for l_φ and m_θ were needed. l_φ was obtained in each case by applying a load W near the wing tip at such a position on the chord that the loaded section deflected without twisting and measuring the deflection Δ of the

loaded section. Then if the loaded section, chosen normally near to the outermost aileron hinge, is distant s from the wing root,

$$l_\varphi = Ws^2/\Delta \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

m_θ was obtained in each case by applying a torque T in the same section near the wing tip and measuring the twist Θ at that section; then

$$m_\theta = T/\Theta \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

In calculating the values of $(1/V)\sqrt{(l_\varphi/s^3)}$ and $(1/V)\sqrt{(m_\theta/sc^2)}$, V was taken as the highest speed at which the aeroplane could fly. In the cases of aeroplanes on which wing flutter was experienced, this was the flutter speed. The values of $(1/V)\sqrt{(l_\varphi/s^3)}$ and $(1/V)\sqrt{(m_\theta/sc^2)}$ were then plotted against wing density, producing Figs. 2 and 3.

In the diagrams, the aeroplanes which have experienced wing flutter are distinguished from those which have not, and those in which the anti-flutter device of mass-balancing is incorporated are distinguished from the rest.

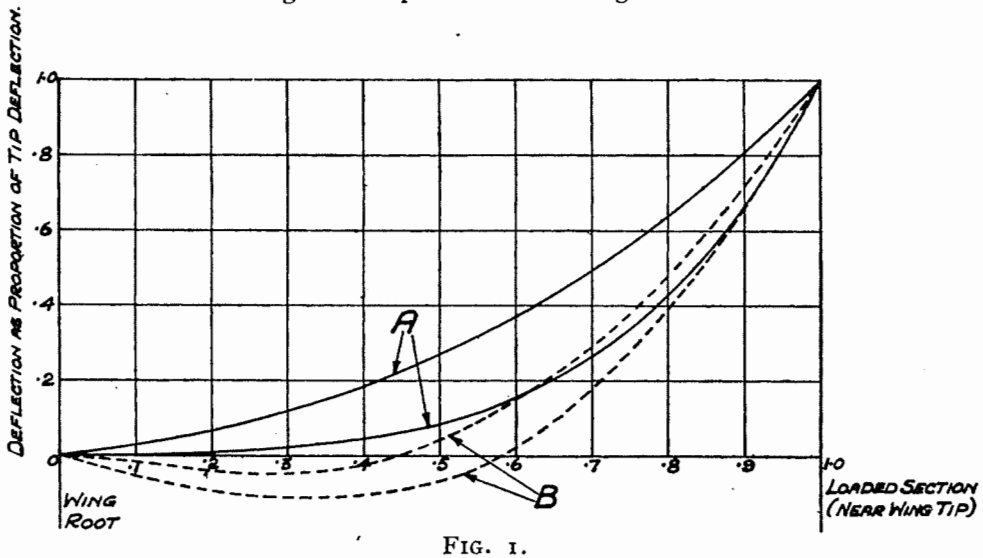


FIG. 1.

Envelopes of diagrams of wing flexural distortion under load applied near wing tip for 26 typical monoplanes.

In both Fig. 2 and Fig. 3, a full line has been drawn so that the flutter cases lie below it. These lines, whose characteristics have received valuable theoretical support,[4] therefore provide quantitative guides to the stiffnesses which should be provided if flexural-aileron and torsional-aileron, and, by inference, flexural-torsional-aileron flutter are to be avoided when no anti-flutter design feature is used. A dotted line has also been drawn in each diagram through the lowest points representing aeroplanes employing mass-balanced ailerons. Monoplanes plotting above this line are consequently probably safe from wing-aileron flutter, but it does not follow that monoplanes plotting below it are unsafe. The dotted lines are therefore merely representative of the low limit of current practice.

The development of diagrams of this kind is clearly of value, and others dealing with biplanes are given in R. & M. 1505.

Such diagrams all indicate that the minimum stiffness criteria values increase with wing density and that the provision of aileron mass-balance, or, in the cases of biplanes, arranging for at least half the aileron span to be inboard of the outer gap struts, permits the use of lower wing stiffnesses than when no anti-flutter

⊙ Indicates 'no flutter experienced'. ⊡ Indicates 'flutter experienced'
† Indicates 'wings with mass-balanced ailerons'.
——— Plottings for wings without mass-balanced ailerons should lie above this line.
– – – – – Plottings for wings with mass-balanced ailerons probably need not lie above this line.
Speed V used in calculating criterion values is highest at which aeroplane can fly. In values represented by ⊡ points, this is flutter speed.

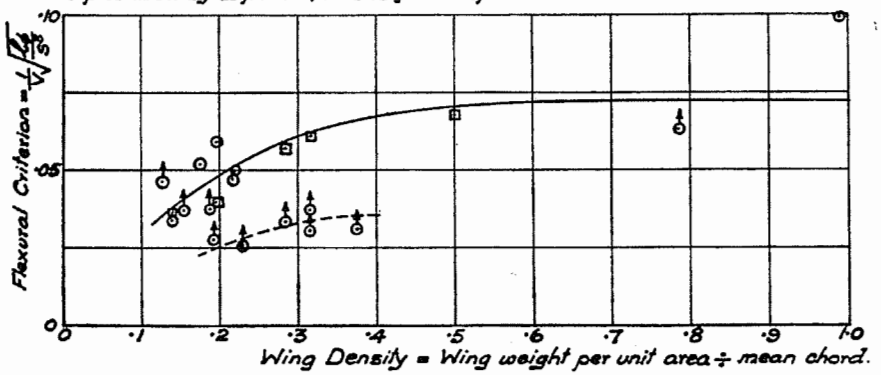


FIG. 2.

Wing flexural stiffness flutter criteria for monoplanes falling into the group defined by envelope A in Fig. 1.

⊙ Indicates 'no flutter experienced' ⊡ Indicates 'flutter experienced'.
† Indicates 'wings with mass-balanced ailerons'.
——— Plottings for wings without mass-balanced ailerons should lie above this line.
– – – – – Plottings for wings with mass-balanced ailerons probably need not lie above this line.
Speed V used in calculating criterion values is highest at which aeroplane can fly. In values represented by ⊡ points, this is flutter speed.

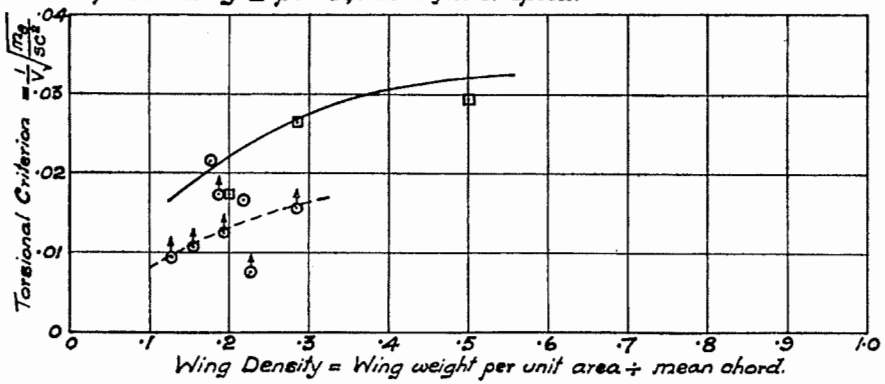


FIG. 3.

Wing torsional stiffness flutter criteria for monoplanes falling into the group defined by envelope A in Fig. 1.

device is employed. It should be emphasised, however, that there are limits to the safe reduction of wing stiffness and that flexural-torsional wing flutter is always a possibility. Aileron anti-flutter devices are potent only in preventing wing-aileron flutter, and have little influence on flutter of the wing only.

§2.6. No authenticated case of pure wing flutter in flight is known to me, but with the building of larger and larger monoplanes and the more and more economical use of material, the difficulties of providing adequate stiffness increase. It can be shown[5] that in legislating against pure wing flutter the ratio of torsional to flexural wing stiffness should be as high as possible, and that the safest way to ensure this is to provide *high torsional stiffness* rather than low flexural stiffness. I cannot, unfortunately, give minimum stiffness figures for prevention of pure wing flutter, but they are clearly lower than those indicated by the dotted lines in Figs. 2 and 3.

§3. *Wing Stiffness in the Prevention of Loss of Lateral Control*

§3.1. When the ailerons of an aeroplane are moved in flight, twisting moments are induced on the wings. These moments normally cause the wings to distort torsionally in such a way as to produce a smaller rolling moment than would have been obtained had the wings been effectively rigid. In some aeroplanes this effect has been so serious that the ailerons were completely ineffective; in others, it has been even worse and reversal of the lateral control has occurred.

§3.2. I propose to consider first how the critical speed at which complete loss of lateral control occurs can be calculated for any wing-aileron system.[6]

First of all some speed V_1 and some small aileron angle ξ are chosen. Next some arbitrary curve of torsional wing distortion is assumed and the aerodynamic loading corresponding to this distortion, V_1 and ξ is determined.

The torsional distortion due to this aerodynamic loading is then calculated, and a second approximation to the aerodynamic loading is obtained from this distortion, V_1 and ξ.

The distortion due to the second approximation to the aerodynamic loading is next found, and the process of successive approximation is continued until the distortion due to the aerodynamic loading is indistinguishable from the distortion producing that aerodynamic loading. Having thus obtained the distortion consequent upon an aileron angle ξ at a speed V_1, the calculation of the rolling moment corresponding follows.

In the same way the rolling moment for the same aileron angle ξ is calculated at speeds V_2, V_3, . . . and a curve of rolling moments against speed is plotted. This curve defines a speed V_r, for which the rolling moment is zero, and this is the critical reversal speed.

In practice it should never be necessary to choose more than three speeds V, and the successive approximation process at each speed does not normally extend beyond the third approximation.

The procedure is very accurate and is applicable to any kind of wing, but it is laborious and requires that the wing structure shall have been designed before it can be applied.

§3.3. For the last two reasons it is clearly desirable that a different method of determining critical reversal speed, or, alternatively, of finding the torsional stiffness which must be provided to avoid loss of lateral control, is required.

Such a method is available.[7] Its theoretical development depends upon a device known as the " semi-rigid wing." This device appears first to have been used by Frazer and Duncan in their flutter work, and has several useful applications.

For the present purpose, a semi-rigid wing is defined as one in which, whatever the magnitude and distribution of the loading,

(i) Purely flexural deflections along the span bear constant ratios to one another,

(ii) Purely torsional deflections along the span bear constant ratios to one another,

(iii) Absolute deflections depend only on the equivalent loading actions at some reference section and the variations of flexural and torsional stiffness measured at that section with flexural and torsional deflections there,

(iv) The aileron displacement relative to the wing at any section is invariant with wing deformation.

Wing distortion shape being, in this type of wing, independent of the distribution of aerodynamic loading, the successive approximation process just described for an elastic wing does not arise, and a direct formula for reversal speed is obtained. This is

$$V_r = K\sqrt{(m_\theta/\rho s c^2)} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where s = distance from wing root to wing tip (feet),
c = mean wing chord (feet),
ρ = air density (slugs/feet^3),
m_θ = torsional stiffness of wing at reference section (lbs. feet/rad.),

and K is a " reversal coefficient," depending upon the wing section, the plan forms of wing and aileron, the position of the reference section, and the distributions of wing twist and aileron angle along the span. Its quantitative definition is rather elaborate and is given in Appendix I. If this formula is to be applied to the calculation of the critical reversal speed for an elastic wing then it is clearly necessary to find rules defining a semi-rigid wing equivalent as regards its loss of lateral control characteristics to the elastic wing.

§3.4. This has been done. At the outset, the assumptions made were that the semi-rigid wing had the same wing and aileron plan form and the same wing section as its elastic pattern. It remained therefore to determine

(*a*) A reference section for both wings at which the semi-rigid wing should be made to have the same torsional stiffness as the elastic wing,

(*b*) The distribution of wing twist which the semi-rigid wing should have,

(*c*) The distribution of aileron angle along the span which the semi-rigid wing should have.

There is no obvious logical connection between the semi-rigid and the elastic wing, so that the connection was not sought theoretically, but empirically. To begin with, three elastic wings of rectangular plan form and with rectangular ailerons were considered.[7] The critical reversal speeds of two of these, of 9ft. span, were determined in the wind tunnel[8] and the critical reversal speed for the third was determined by the successive approximation method already described. The semi-rigid equivalent was in each case supposed to have a linear distribution of wing twist with a constant aileron angle. Reference sections at the wing tip and at mid-aileron were tried and, as seen from Table I, using the former the formula gave in each case a remarkably close approximation to the true critical reversal speed.

TABLE I.

CRITICAL REVERSAL SPEEDS OF WINGS OF RECTANGULAR PLAN FORM.

Wing No.	Theoretical Critical Reversal Speed in f.p.s. Assuming Reference Section at Wing Tip.	Theoretical Critical Reversal Speed in f.p.s. Assuming Reference Section at Mid-Aileron.	Actual Critical Reversal Speed in f.p.s.	Speed in Previous Column Obtained by
1	55·4	53·5	55	Wind tunnel experiment.
2	74·5	87·5	90	Wind tunnel experiment.
3	151·5	182.2	187	Successive approximation calculation.

This encouraging result led to the adoption of a reference section at mid-span of the aileron for investigations into the determination of (*b*) and (*c*).[9]

The variation of the reversal coefficient as defined in Appendix I with the law of twist distribution is shown in Table II for various degrees of wing taper and aileron span. Where y is the distance of a general point from the wing root and $y=ns$ defines the reference section in terms of the span s, the laws of twist distribution considered were of the form

$$\theta \propto (y/ns)^u \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

TABLE II.

VARIATION OF REVERSAL COEFFICIENT WITH LAW OF WING TWIST FOR AN ELLIPTIC WING IN WHICH THE AILERON CHORD IS EVERYWHERE ONE QUARTER OF THE WING CHORD AND THE REFERENCE SECTION IS AT MID-SPAN OF AILERON.

Aileron Span / Wing Semi-Span	Reversal Coefficient when $\theta \propto (y/ns)$	$\theta \propto (y/ns)^2$	$\theta \propto (y/ns)^3$
.6	2.05	2.16	2.14
.5	2.26	2.46	2.54
.4	2.51	2.81	3.00

In so far as the law chosen should represent the shape at reversal of control of an elastic wing, $u=2$ probably gives an approximation applicable to an average wing while $u=3$ gives a far more exaggerated shape which can fairly be considered as a limit (Fig. 4). Even over this wide range K is, from the table, insensitive to the assumed distribution of wing twist. There appears in consequence to be no objection to taking the simplest assumption of $u=1$ for wing twist in calculation, that is, to assuming the semi-rigid wing to distort linearly.

With regard to (*c*), it is easy to prove that the reversal coefficient is independent of the distribution of aileron angle along the span if the wing twist distribution is assumed linear. When the wing twist distribution is assumed cubic (*i.e.*, $u=3$), and the aileron angle is taken to be twice as great at mid-aileron as at the aileron ends, the error involved in assuming the angle constant is only 1 per cent. In calculations, therefore, this assumption is justified.

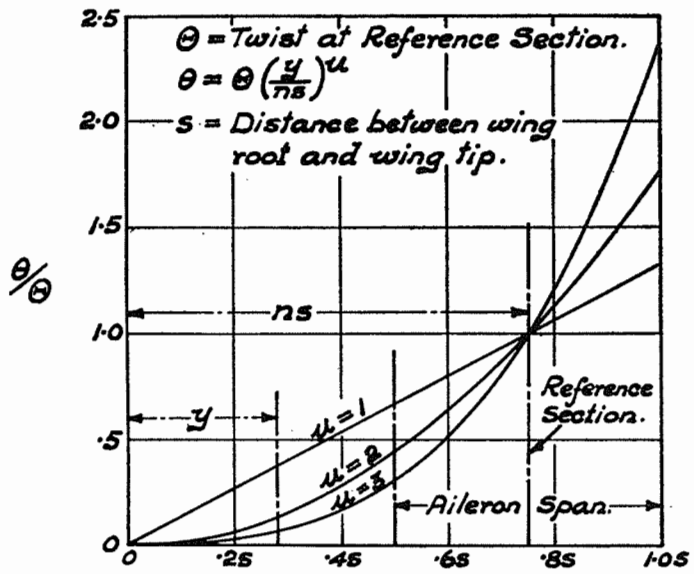


FIG. 4.

Semi-rigid wing torsional distortion shapes selected for investigation in studying estimation of critical reversal speed.

§3.5. We are led to believe, therefore, that the critical reversal speed of an actual wing is given to a good degree of approximation by the critical reversal speed of a semi-rigid wing in which

(i) The aerodynamic characteristics are the same as for the elastic wing,
(ii) The reference section is at the mid-span of the aileron,
(iii) The torsional stiffness measured at the reference section is the same as that of the elastic wing measured at the corresponding section,
(iv) The law of distribution of wing twist is linear,
(v) The aileron angle relative to the wing is constant along the aileron span.

We have already seen that this procedure is successful for rectangular plan form wings with normal elastic characteristics. It has also been applied to wings tapering in plan and with more exaggerated distributions of elasticity. As an example, the critical reversal speed for an elastic wing in which the tip chord was .3 of the root chord and which was unusually flexible in the region of the aileron was calculated by the precise successive approximation process and from the semi-rigid wing analogue; the error involved in using the latter process was shown to be only 2.5 per cent.

§3.6. As the formula (5) in the circumstances defined in §3.3 is applicable to actual wings, it provides a means of finding how different controllable factors influence critical reversal speed. It is immediately clear from the formula that the reversal speed varies directly as the square root of the torsional stiffness of the wing structure, and the advantage of high torsional stiffness is apparent.

Another extremely influential factor, however, is wing taper in plan form. If we imagine a given monoplane wing structure and superimpose upon it wing plan forms of varying degrees of taper but of the same span and area (and hence the same aspect ratio), the reversal speeds of the wings so formed vary with taper as shown in Fig. 5, in which, reading upwards, the curves correspond to

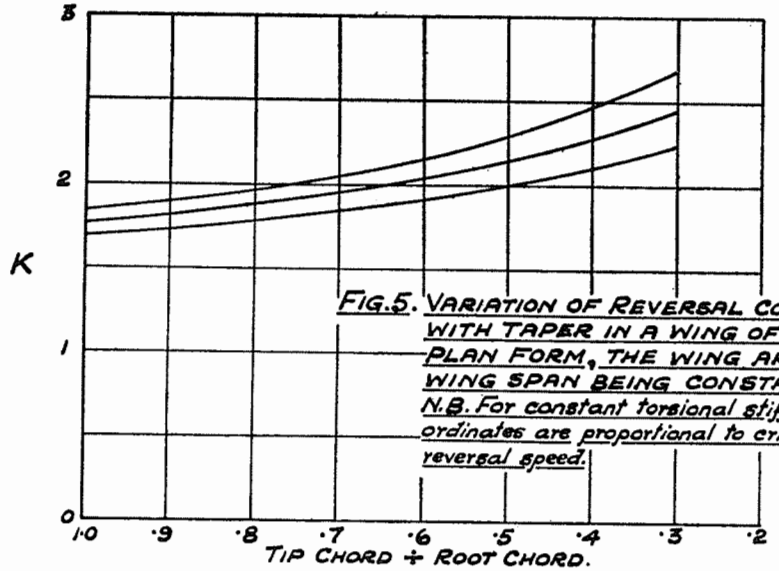


FIG. 5. VARIATION OF REVERSAL COEFFICIENT (K) WITH TAPER IN A WING OF TRAPEZOIDAL PLAN FORM, THE WING AREA AND WING SPAN BEING CONSTANT. (See §3.6)
N.B. For constant torsional stiffness the ordinates are proportional to critical reversal speed.

aileron spans of .6s, .5s and .4s. This indicates the advantage in designing to prevent loss of lateral control of a high degree of taper; this is illustrated differently in Fig. 6 where the torsional stiffnesses of wings of constant span and constant area with the same reversal speed are plotted against taper in three curves which, reading downwards, correspond to aileron spans of .6s, .5s and .4s.

§4. *Wing Stiffness in the Prevention of Wing Distortion in Flight*

An aeroplane wing in flight is distorted flexurally and torsionally by gravity loads, by inertia loads, and by aerodynamic loads consistent with the distortion which they are chiefly responsible for producing.

When the strength of a wing is being checked, the aerodynamic loads used are generally those consistent with the wing being undeformed. Consequently the stresses occurring in the wing of an aeroplane in certain conditions of flight may be considerably different from the stresses calculated for those conditions as, although the total lift on the wings may be correctly estimated, its distribution will not be. The error involved in neglecting the effect of distortion is clearly dependent upon the degree of elasticity of the wing. In certain circumstances it is possible for this error to be " on the right side," that is, for neglect of the effects of the elasticity of the wing to provide a " hidden margin of safety." More frequently, however, the " hidden margin " is negative, a circumstance which may have a bearing on certain accidents for which adequate explanation has not been found.

There are available two methods of calculation which give a good approximation to the stresses in a wing when the effect of its distortion under load is allowed for.

One of these is a successive approximation method similar to the one already described in connection with the calculation of critical reversal speed. Starting with the aerodynamic load distribution corresponding to the undeformed configuration of the wing, the distortion due to it and the gravity and inertial loads operating is calculated. Using the aerodynamic loading distribution corresponding to this distortion, a fresh distortion is calculated. The procedure is repeated until the nth distortion calculated is negligibly different from the $(n-1)$th. Three approximations will generally be found sufficient.

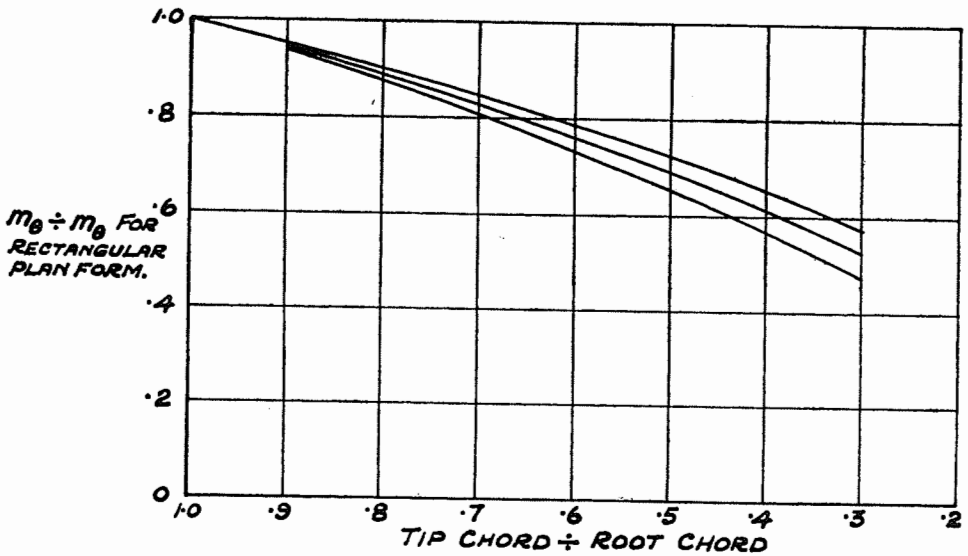


FIG. 6.

Variation with taper of torsional stiffness (m_θ) required to prevent reversal of lateral control for trapezoidal wings of constant area and constant span (see §3.6).

In the second method the aerodynamic load distribution and the wing twist distribution which are consistent are obtained directly by rather involved algebraic methods. The essential procedure is given in R. & M. 1509.[10]

All I have so far implied is that the stresses in a wing in a particular condition of flight may be dangerously greater than the stresses estimated for that condition. That is not all however. On account of its elasticity the structure of the wing may become unstable in circumstances which conventional calculations would indicate to be safe. It is extremely difficult to illustrate this point by reference to actual elastic wings. The point which I wish to demonstrate is, however, conveniently illustrated by making use once more of the semi-rigid wing.

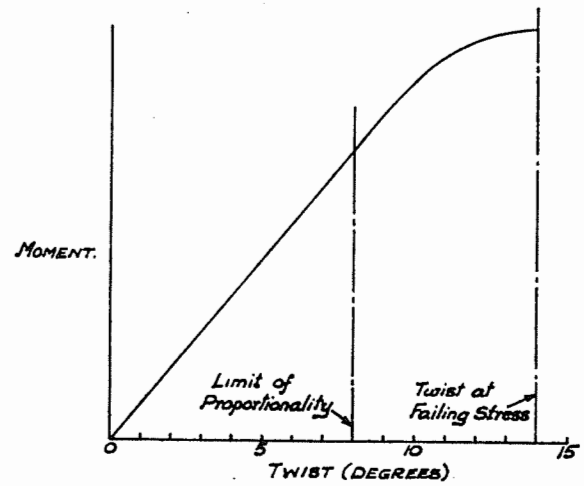


FIG. 7.

Typical relationship between twisting moment and angular distortion measured at a general section of an elastic wing.

Now there is a property of the elastic wing which it is essential to copy in the semi-rigid one in the present instance. If a particular distribution of aerodynamic load is chosen for, say, a mechanical test on a wing, and if the magnitude of this load is increased incrementally, the flexural and torsional deflections at any given section will be proportional to the load so long as all the structural material obeys Hooke's Law and compression members do not bow. As the loading is increased however, sooner or later a stage will be reached at which the material of some unit of the structure passes the elastic limit and enters the plastic state. Thereafter, distortion can no longer be everywhere proportional to loading and, as with further loading more and more of the structural material passes its elastic limit, the rate of growth of distortion with loading increases until at some point the breaking stress is reached.

This characteristic is copied in the semi-rigid wing by hypothesising for it at all sections moment-deflection curves with ordinates proportional to a curve like that of Fig. 7.

Now a semi-rigid wing has the property that forces on it can all be replaced by equivalent forces at a chosen reference section. Its ability to represent

Lord Kings Norton — his contribution to the gas turbine aero engine

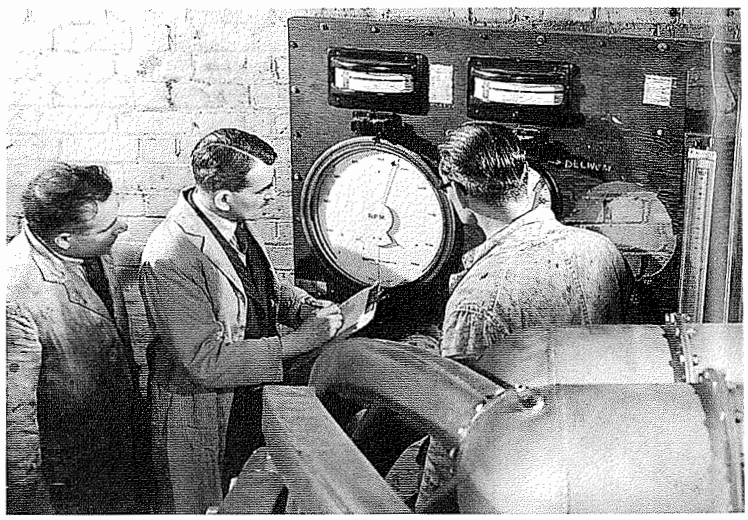
Mike Evans
Chairman, Rolls-Royce Heritage Trust

The twentieth century has seen man's mastery of the air, and fundamental to that achievement has been the provision of power to sustain the process of flight. For the first forty years the piston engine reigned supreme, yet for even longer now the power to fly has been provided by the gas turbine. In the conception and development of this prime mover Britain can justly claim to have led the world.

Two servants of the Crown quite independently had the vision and that amalgam of creative and analytical powers to see what might lie beyond the piston engine — one a scientific civil servant working at the Royal Aircraft Establishment (RAE) and the other, remarkably, a young RAF officer already distinguishing himself as a pilot. But the birth of the jet engine proved to be a painful process. While early aircraft designers had embraced the piston engine, neither Government nor the aircraft or aero engine industry showed interest initially in the gas turbine. The change in attitude began when war approached in 1939 and thereafter accelerated in the effort to move from bench demonstrator status to a full-production engine capable of contributing to the outcome of the war. It was in this difficult and complex process of transition that Dr Harold Roxbee Cox — Lord Kings Norton — was to play such a vital role. To appreciate his achievements it is appropriate to remind ourselves of the progress — or lack of it — on the gas turbine prior to his becoming involved.

The first thesis on the subject was published in 1926 by Dr A.A. Griffith of the RAE engines experimental department. Entitled An aerodynamic theory of turbine design, it proposed the use of a single-shaft turbine engine with multi-stage axial compressor as a means of driving a propeller through a reduction gear. The paper, and supporting test work, were reviewed by the engines sub-committee of the Aeronautical Research Committee in April 1930. It concluded that: "At the present state of knowledge the superiority of the gas turbine over the reciprocating engine cannot be predicted." Britain was in the depths of depression, disarmament was still the political creed and it is not difficult to realise why no more work was funded at RAE until 1936.

Some two years after Griffith wrote his paper, a young Cranwell cadet, Frank Whittle, wrote a paper Future developments in aircraft design. In it he envisaged that very high speeds in aircraft, combined with long range, would demand flying high where the air was rare. In an era when the RAF's fighters were incapable of flying at 200mph, Whittle had thoughts of 500mph in the stratosphere. He was unclear on a means of propulsion, although he did consider rockets and gas turbines driving propellers. It was not until October 1929, however, that it occurred to Whittle that a turbine could be substituted for a piston engine and the jet used to propel the aircraft. The 22-year old Whittle was taken to Griffith, whose work had already been reported on unfavourably by then, and Griffith was not encouraging. History has tended to damn the latter for his views but, in fairness, one should recognise that Griffith was a scientist who sought uncompromisingly scientific fact and optima. Whittle, in comparison, beside his vision, possessed the pragmatism of a true engineer. His proposal, which employed a single centrifugal



compressor of the type already well developed in piston aero engine superchargers, lay within the realms of more immediate and less costly possibility. Nevertheless, he could raise no real interest in the aero engine industry and, as with Griffith, no practical work towards building an engine was undertaken until 1936.

Whittle's struggle and the problems in setting up Power Jets Limited in 1936 need form no part of this narrative, but it is worth noting that the spring of 1937 proved a turning point. In the March, Griffith's co-worker, Hayne Constant, made the case again for developing the gas turbine to the engines sub-committee of ARC. This time, under the chairmanship of Sir Henry Tizard, the committee declared itself in favour of a vigorous policy of gas turbine research, recommending that the Air Ministry should take up the development of the internal combustion turbine (as they were then known) as a matter of urgency and make all possible arrangements for its production at the earliest possible moment. A month later, on 12 April 1937, Frank Whittle made the first run of his jet engine in the BTH factory in Rugby. There was an increasing threat of war. Re-armament was under way and the gas turbine engine was slowly coming to be seen as a potential source of superiority in aerial warfare. The Air Ministry's director of scientific research, Dr Pye — for long a doubter — became a complete convert after observing an engine run at Power Jets, Lutterworth, on 30 June 1939. It was not then known that Hitler's Germany had similar thoughts and that greater priority was being given to the work initiated by Franz von Ohain at Heinkel.

In May 1940, with Britain at war, Churchill appointed the press magnate Lord Beaverbrook to run a newly-conceived Ministry of Aircraft Production (MAP) and it was then that Roxbee Cox began his close association with the gas turbine. His whole working life, of course, had been in aviation, as other tributes in this journal reflect, but 1940 marked his appointment as deputy director of scientific



Three stills from the COI film The Wonder Jet. *Left:* Frank Whittle (centre) and his associates at Lutterworth. *Above:* Frank Whittle (right) and Patrick Johnson, md Power Jets (Research and Development) re-enacting the Cranwell sequence. *Bottom right:* 'The Wonder Jet'.

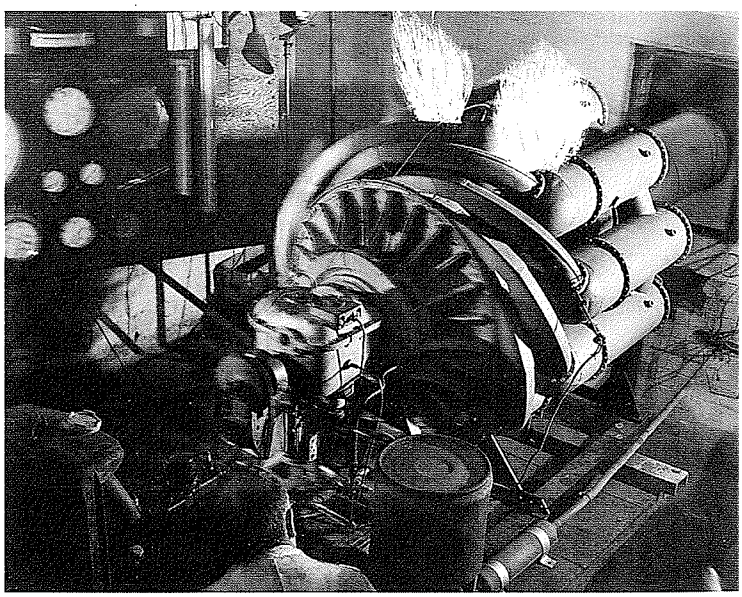
research at MAP. In this role, which he held until 1943, and that of director of special projects at MAP, which he held for a further year, Roxbee Cox became more deeply involved with Whittle and the whole range of 'top secret' gas turbine developments, as well as with many other 'state-of-the-art' technologies such as radar, new armaments and aircraft, and even bouncing bombs. It was an association with gas turbines which was to last until his retirement from the civil service as chief scientist of the Ministry of Fuel & Power in 1954. In that period he was to serve as the first chairman of the Gas Turbine Collaboration Committee from 1941 to 1944 and to return to that role from 1946 to 1948. Additionally, his period at MAP came to an end in 1944 when he was appointed chairman and managing director of Frank Whittle's company when Stafford Cripps nationalised it and restyled it Power Jets (Research and Development) Limited. He then carried through a decision of government in January 1946, namely to create a National Gas Turbine Establishment (NGTE). On 25 May, the Power Jets factory at Whetstone, the Bruntingthorpe airfield, and Pyestock, the jet engine offshoot of RAE, Farnborough, which had been set up in 1941, were combined as the NGTE, with Roxbee Cox as its director. Hayne Constant became one of his deputy directors; Power Jets (R&D) Limited was reduced to a holding company for patents under Johnson as its md. Roxbee Cox remained with NGTE until his move to the Ministry of Fuel & Power in 1948. So these were the positions he held and the dates of his tenure, but what of the nature of the challenges he faced and what made his contribution so unique?

Britain and her allies were at war and Roxbee Cox occupied posts which, by their nature in that circumstance, dealt with conflicts of priority and precedence among some very powerful personalities. For a start, at MAP he served under Beaverbrook, a man of action and singularity of purpose who lacked sensitivity or finesse. He granted a licence, for instance, during the Battle of Britain to the US government to build the Merlin engine. This he did in the name of the British government without making any approach to Rolls-Royce. On a daily basis, Roxbee Cox worked with successive ministers at MAP and the Ministry of Supply, senior officials of the Air Ministry, the Chiefs of Air Staff, a spectrum of government departments and, of course, the aviation industry — and the engine manufacturers in particular. He found himself ever nearer to the centre of the war effort, daily in communication with individuals such as the future Lord Portal, Lord Brabazon, Sir Henry Tizard, Sir William Farren, Air Marshal Sir Francis Linnell and — of course — Flight Lieutenant Frank Whittle. From industry, one has to add to this the captains and engineers of Armstrong Siddeley, Bristol, British Thomson Houston, de Havilland, Joseph Lucas, Metropolitan Vickers, Rolls-Royce and Rover among others. All were as one in being committed to winning the war, but there the similarities began to diverge.

To do justice to Roxbee Cox's work would demand a complete book, but some measure of the success of his facilitation may be gauged by comparing the situation on gas turbines following the outbreak of war with that when he left NGTE for the Ministry of Fuel & Power in 1948.

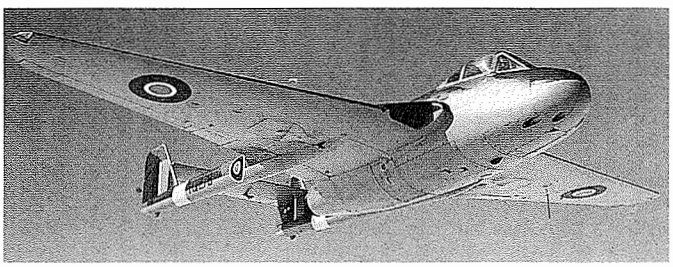
Neither Griffith nor Whittle had any real manufacturing resource at their disposal in the late 1930s and it was the pursuit of this which began to complicate the situation. RAE turned to several firms, including Armstrong Siddeley and Metropolitan Vickers, for help in constructing their axial test units. Then, on 1 June 1939, Rolls-Royce made its first move on gas turbine engines when it recruited Griffith. Constant remained at RAE and sought help from Power Jets in building its first axial turbojet, the F1. Work proceeded until the pressures on Whittle's own W1 flight engine programme forced Power Jets to abandon its part in the F1. Meanwhile Ernest Hives, the general manager of Rolls-Royce, had instructed Griffith to "go on thinking" and Griffith proposed the most advanced of his concepts from RAE — a contra-rotating engine, the CR1, with 14-stage high-pressure system and 6-stage low-pressure ducted fans. He remained negative about the Whittle concept. Constant, however, began to believe it might work but his real commitment was to the axial and he had taken the RAE design for the more advanced F2 to Metropolitan Vickers, where the team under Dr D.M. Smith and K. Baumann were to achieve progress which even now has not been fully recognised. Their first F2 went to test in Manchester in December 1941. Before that had happened, Griffith had advised Rolls-Royce that, in his view, the "orthodox scheme of ICT" namely the simple axial, was not a "sufficiently promising avenue of research" and that it would be wiser for Rolls-Royce to leave Metrovick to show what could be done with it. Rolls-Royce built the high-pressure system of Griffith's CR1 and it went to test on 3 March 1942. However, it would not even sustain itself on its original combustion system and barely did so when Whittle-type combustors were fitted. It was to cause Hives to write, somewhat tongue in cheek, at the end of 1942: "We still think the Rolls-Royce contra-flow turbine is the best in existence, except that we cannot get it to work!" Rolls-Royce shelved the project in 1944, having committed itself to the Whittle approach.

So much for the CR1: what of the F2? It progressed, becoming the Beryl in its F2/4 form, by which time, in two years, Metrovick had increased thrust by 90%. Post-war, in the hands of Armstrong Siddeley, its linear descendant, the F9, became the Sapphire and was giving some 7,000lb by 1948. By then Rolls-Royce had a similar engine



which had been launched in 1945 by Griffith — on Hives' instruction — as the AJ65. It became the Avon.

To write of axials before centrifugals is to jump the gun however. For his WU engine in its various forms — the WIX and the W1 engine which flew in the Gloster E28/39 at RAF Cranwell on 15 May 1941 — Frank Whittle was continually short of money and, therefore, inevitably, of hardware. Initially this had been sourced from BTH, with specialist help on components like combustors from Laidlaw Drew and Shell. To these sources in 1940 was added Rover which, with support from Lucas, was to build complete engines for Power Jets. Additionally, and informally, Rolls-Royce made parts to help Whittle directly and to support Rover on the basis of a sub-contractor. But it got more complicated than that. Whittle's W1 was followed by the more powerful yet more compact W2. When testing showed the need for improvements, Whittle designed the W2B and it was this engine which Rover was given to build. Progress did not stop at Lutterworth, however, the W2B being followed by the W2/500 and ultimately the even more powerful W2/700.



de Havilland Vampire Mk1.

Meanwhile, Major Frank Halford had begun the design of the de Havilland H1, the Goblin, which was to power the Vampire fighter. Based on Whittle ideas, it had a single-sided compressor impeller instead of Whittle's double-sided unit and it had a straight through, rather than reverse flow, combustion system. At Rolls-Royce, Hives decided — with the support of MAP — to build a low-rated Whittle-type engine capable of running for long periods such that something could be learned from it. The WR1 was designed in Derby in collaboration with Power Jets Limited. Work began early in 1942 and the engine was running before the end of the year. But what was happening at Rover? In short, they had begun to 'do their own thing' at Clitheroe and Barnoldswick without consulting Whittle. Capt Spen Wilks of Rover had already proposed a joining of forces with Hives in February 1942, long before relationships with Whittle came to a head. When Rover 'straightened out' the W2B/23 to create the B26 — forerunner of what was later to become the Rolls-Royce RB37 Derwent I — Whittle was furious. Understandably, he saw it as a further diversion and, therefore, delay in getting the jet engine into service against the enemy.

Such was the multiplicity of experimentation when Roxbee Cox took office at MAP in 1940, and it continued to complicate itself in 1941. A first glimmer of a way forward rests in a letter from Roxbee Cox to Hives dated 21 August 1941: "I do not expect that there will be any obstacle to the constitution of a committee for pooling our gas turbine facilities and experience but it will take me a few days to make sure of everyone's co-operation." On 3 October 1941, a letter went out over the signature of Air Marshal Linnell of MAP. It began:

Dear Sirs

As you are aware gas turbine development is now going forward in various organisations and along several different lines, and it is clearly desirable, to ensure economy in our efforts to produce power units of this new kind as quickly and efficiently as possible, that all parties concerned should collaborate.

To encourage and guide collaboration, it has been decided to form a committee under the chairmanship of Dr Roxbee Cox on which all the firms engaged on gas turbine projects will be represented...

Thus was born the Gas Turbine Collaboration Committee (GTCC) and the beginnings of drawing all the efforts to a focus. It was not an easy or a painless process and the losers were the creators of the gas turbine themselves. In the case of RAE that was inevitable. Farnborough had ceased to be the Royal Aircraft Factory in 1918 and its role was recognised as a research and test agency. In the case of Power Jets Limited — and Frank Whittle in particular — the case was very different.

Power Jets had a factory built for them at Whetstone, but the Rover operation remained a significant part of its manufacturing potential. As matters progressed, it became logical in the eyes of MAP that Rolls-Royce should take over at Barnoldswick and Clitheroe. The famous 5/- (25p) meal at the Swan & Royal did take place late in 1942 as Stanley Hooker described, but that was the handshake on what was, in reality, a MAP directive largely inspired, one suspects, by Roxbee Cox. He, in fact, wrote to Hives on 15 December 1942 following a meeting three days earlier at which they, Wing Commander Whittle and A.G. Elliott, Rolls' chief engineer, had been present. Roxbee Cox had undertaken to lay a series of points which had been agreed before his seniors. These included:

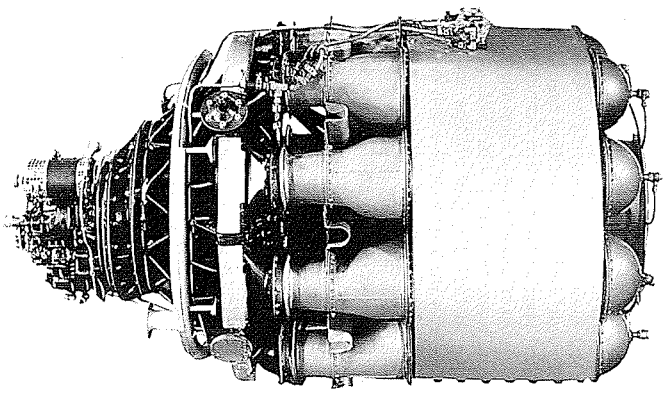
(a) Production of the Power Jets W2/500 engine to be in the hands of Rolls-Royce.
(b) The facilities to be used to be those existing at Barnoldswick.
(c) Research on, and development of, centrifugal type units to be in the hands of Power Jets Limited at Whetstone and Lutterworth.

In the event it did not happen that way. Rolls-Royce did take over from Rover. Hooker moved to Barnoldswick at the very beginning of 1943 and the official handover day was 1 April. Rover, in exchange, took on board the Meteor tank engine over an extended time horizon. But the factory was not cleared of all Rover's work to make way for the W2/500. Rolls-Royce continued with the W2B/23, launching it into service as the Welland in the Gloster Meteor fighter with 616 Squadron in little more than a year. They followed with quantity production of the straight through Derwent I, followed by their own Nene and its scale-down, the Derwent V, which took the world air speed record in 1945 and 1946.

When Hives surveyed the Rover situation at the very end of 1942 he concluded: "We do not blame Rovers for lack of success on the turbine project: we have every sympathy with their difficulties. Broadly speaking, there has been too much slide rule and not enough hardware."

Barnoldswick and Clitheroe, in consequence, became a development centre and not a production unit. In so doing, technical control of the work passed formally to Rolls-Royce and that left Power Jets in a very difficult position. Some would suggest that this was an opportunist move by Hives yet, in late November 1942, he had written, "We are fortunate in as much as we work in the most friendly way with Whittle. We have a great admiration for his ability."

Whatever were the 'ins and outs' of the exceedingly complex situation of Power Jets — and Roxbee Cox had intended that they retain technical control of the W2 — Whittle was understandably deeply concerned. He wrote a letter to the government minister Sir Stafford Cripps, on 29 April 1943, in which he argued that the whole gas turbine aero engine industry should be nationalised including the buying out of the shareholders in Power Jets Limited. It was a letter he was to regret as Cripps turned the proposal round on Whittle. Cripps' initial response was a meeting with Whittle in May at which he expressed a measure of agreement with the proposal but added that he had no mandate to take over companies unless they were operating inefficiently. Whittle was sent off for a three-month break at staff college and saw Cripps again on 25 October 1943. At this second meeting, he told Whittle that he had decided to nationalise Power Jets. It was, he said, essential to have a state-owned gas turbine establishment and it would be wasteful to build a fresh organisation when Power Jets already existed. Whittle was then forced to surrender his assets in Power Jets — valued on the books at £47,000 — and, on 28 March 1944, he had to sign a document confirming this surrender as a gift, although retaining his right to any



General Electric I-A.

ex gratia payment that might be made. A year later he received an interim award of £10,000. In the meantime, Cripps told the other directors of Power Jets that he needed a purchase price for the assets or he would simply take the business over. In this heavy-handed manner, the state acquired Power Jets for the very low figure of £135,000.

The new company, called Power Jets (Research & Development) Limited, was chaired by Roxbee Cox, with Tinling and Williams on the board and Whittle as its chief technical advisor initially; in January 1945 he joined the board. Whittle set out to strengthen the company's position, not just as a designer and developer of engines but in manufacturing methodology too. Plans were tabled for a big increase in manufacturing capacity and, by March 1945, these were on the point of being agreed by MAP. A month later, the industrial members of GTCC objected to Power Jets building even a prototype engine. When it became clear that MAP was not going to authorise the new facilities, Whittle resigned from the GTCC. Subsequently, in a letter to Roxbee Cox dated 22 January 1946, he recounted the whole sorry story and resigned from the board of Power Jets (Research & Development) Limited. Whittle then made a brief lecture tour, to be informed on his return that his post — and those of Williams and Tinling — had been disestablished. After a final attempt in which he proposed to retain his team — soon to name themselves 'The Reactionaries' — by forming a new company had failed, Whittle gave up in March 1946. Before the month was out, sixteen key members of his team had resigned together. Power Jets (R&D) Limited was merged in the following month with Pyestock to form the NGTE, with Roxbee Cox as its director and Constant as one of his deputies.

When the Royal Commission on Awards to Inventors began its work, the Ministry of Supply formally asked the commission to examine the case of Frank Whittle and make a recommendation. Whittle himself had made no claim. The case was prepared by E.L. Pickles and other officials of the Ministry but the most influential contribution was that of Roxbee Cox. In May 1948, the commission duly recommended to the Treasury that Frank Whittle be given an award of £100,000 free of tax (£90,000 net in view of the interim award). In the King's Birthday Honours List, Whittle received a KBE; in the August, he was retired from the RAF on the grounds of ill health with the rank of Air Commodore.

Throughout his period of involvement, Roxbee Cox did all he could to steer the many players in the gas turbine arena towards common goals and he did all that his various offices allowed to protect the interests of Frank Whittle. When he joined the MAP, many of the senior officials did not believe the jet engine would ever work, yet by steering, coordinating, cajoling, supporting and perhaps even mediating, he helped create a very different situation by the time he left NGTE in 1948. The jet engine had entered RAF service in 1944. The Gloster Meteor and de Havilland Vampire, powered by the Rolls-Royce Derwent and de Havilland Goblin respectively, had rendered piston-engined fighters obsolescent overnight and, apart from equipping the RAF, both were exported widely in the post-war era. Manufacturing licences were granted — for Rolls-Royce Derwent, Nene and Tay engines in particular to Australia, Belgium, France and Pratt & Whitney in America. The Soviet Union also built these engines, albeit without any authority. Then came the early Farnborough air shows of 1948 and 1949 and Britain's achievements became clear to all — most participating aircraft were powered by gas turbine engines and these included Meteors with reheat, the naval Wyvern with Python power, the Avon Meteor flying test bed, the Canberra bomber and the world-leading de Havilland Comet and Vickers Viscount airliners. Just a year or two ahead lay the V-bombers, VStol and supersonic flight. Roxbee Cox had helped give all this a sense of direction.

One thing in all this had struck Roxbee Cox forcibly and this was a comparison of British and American commitment. Here there had been doubt and procrastination. When, in contrast, America entered the war, advances with turbine engine technology in Britain and Germany had caused concern, particularly to General 'Hap' Arnold. In February 1941, Arnold asked that a special committee be appointed to coordinate early US acquisition of jet engine capability. It was formed within weeks, encompassing representatives from the Army Air Forces, the National Bureau of Standards, Johns Hopkins University, MIT and three firms with experience of steam and gas turbines — Allis Chalmers, GE and Westinghouse. Under wartime technology interchange agreements, Whittle's WIX engine and a complete set of Power Jets engine drawings were given to the US Government. These items, plus three Power Jets engineers, reached the General Electric turbine plant at Lynn, Massachusetts, on 4 September 1941. Within six months, on 18 April 1942, GE had their IA engine running and on 2 October of the same year the purpose-designed Bell XP-59A Airacomet took to the air for the first time at Muroc Dry Lake.

Roxbee saw this in his later years as a 'betrayal' of Whittle and of Power Jets. In times of peace, it might have been. In times of war — like the Merlin licence — the shared interests of nations took precedence over commercial considerations. Had the interchange agreement remained for military purposes only, as was the wartime intent, then Roxbee's view might have been a little harsh. Later, however, the British government agreed to extend the use of this technology to civil gas turbine engines. Not only has the British aero engine industry had to meet or beat the challenge that has generated, but 'betrayal' might be an appropriate term when the British government received $4,000,000 for the rights on the one hand yet rewarded Frank Whittle with only £100,000 on the other.

Even more of concern to Roxbee was his perception that British governments were singularly ill-equipped to be able to understand — yet alone foster — new technologies. He is on record as stating that, apart from the scientific branch of the civil service, which was "always on tap but never on top, there are very few scientists and no engineers in government. Certainly, our governments never seem to know how to handle new technology and nurture innovation in our most important national industries." Yet Roxbee knew that it was just these technologies — and these industries — which were the lifeblood of national economy. The perception remained with him until the end of his life; he saw no reason to change it.

The foregoing was not the achievement of anyone less than a remarkable man. He had an outstanding grasp of aviation in all its technical aspects, matched by an equal enthusiasm which remained with him to the end. But that would not have been enough. He had warmth and a goodness of humour which touched all who knew him and all he did, and his choice of words brilliantly matched the needs of the circumstance. Asked late in life about the activities of which he felt most proud he replied that he was a reasonably modest man but that nonetheless he would name his work on the R101, despite its failure, his contributions as chancellor of Cranfield and, above all, his association with Sir Frank Whittle and his invention — the jet engine. It was appropriate that, as Lord Kings Norton, he should have paid his final tribute to Sir Frank in the memorial service at Westminster Abbey on 15 November 1996 when he read one of the lessons; its subject was wisdom.

THE BEGINNINGS OF JET PROPULSION

The Trueman Wood Lecture by

THE LORD KINGS NORTON OF WOTTON UNDERWOOD,

PhD, FIC, FIMechE, HonDSc, HonLID, HonDTech, HonFRAeS

delivered to the Society on Wednesday 27th March 1985,

with Sir Peter Masefield, MA, CEng, HonDSc, HonDTech, HonFRAeS, FCIT,

a Vice-President of the Society, in the Chair

THE CHAIRMAN: I know that everyone here shares with me our pleasure in welcoming as our lecturer one of the most distinguished and one of the most erudite and delightful people of all those who over the years have made their mark on British aeronautics. Lord Kings Norton has been Chancellor of Cranfield Institute of Technology since 1969. He began his career in aviation sixty-one years ago at Cardington, as a young engineer working on the design and construction of that much maligned major project, the airship R101, and then subsequently as Dr. Harold Roxbee Cox (as many of us still remember him) he was the head of the Air Defence Department of the Royal Aircraft Establishment at Farnborough. Next he was Chief Technical Officer of the newly formed Air Registration Board before he returned to the RAE in 1939 as Superintendent of Scientific Research.

When the war came Roxbee was appointed Director of Special Projects under Lord Beaverbrook at the Ministry of Aircraft Production. In 1944 he was appointed Chairman and Managing Director of Power Jets R & D Limited to develop Frank Whittle's original jet engines. In this quite remarkable career he went on to become Director of the National Gas Turbine Establishment up to 1948 and then to be Chief Scientist of the Ministry of Fuel and Power.

Tiring of those corridors of power in 1957, he went into industry and became Chairman of the Metal Box Company and was appointed to a seat on the Boards of other major companies and combined this with the chairmanship of the Council for Scientific Research, chairmanship of the Air Registration Board and for good measure presidency of the Royal Aeronautical Society and presidency of the Royal Institution. He found time to be the Wright Brothers Lecturer in the United States and a receiver, of course, of many honours and awards. He was created a life peer twenty years ago in 1965, to sit on the cross-benches so that he could snipe at – or support – both sides equally.

So you see Lord Kings Norton has an immensely distinguished career over a very wide field of science and technology and, I think (so appropriately in the terms of this Society) throughout all the arts, manufactures and commerce. He is at once a leading aerodynamicist, structures engineer, an authority on airworthiness requirements, an authority on that phenomenon which fortunately we have more or less scotched thanks to his efforts, wing flutter, on balloons and airships at the other end of the spectrum and of course on every form of power flight. So much in one cranium! What is more, on the subject of this evening's lecture, he was in at the beginning of the successful application of jet propulsion with both the RAE and Frank Whittle.

How appropriate it is that Lord Kings Norton should be giving the 1985 Trueman Wood Lecture to commemorate Sir Henry Trueman Wood, who was Secretary of this Society from 1879 to 1917, and followed that by being Chairman of the Council for 1919 and 1920. There could be nobody more appropriate to bring together in this Trueman Wood Series an account of the beginnings of jet propulsion.

The following lecture, which was illustrated, was then delivered.

THE LAST time I lectured in this splendid chamber was over half a century ago; to be precise, on 16th November 1933. My subject then was, at the time, some of the latest work in the theory of aircraft structures, with which I was closely concerned. My subject tonight is the early history of the jet propulsion engine, to-day more accurately described as the jet propulsion gas-turbine, in the development of which as an aircraft engine I also had a part to play. So whereas all those years ago I was speaking of what was then advanced technology, tonight I am on the fringes of industrial archaeology. Indeed, as you will see, I am indeed starting on what might properly be described as technological prehistory, because it was in the region of 150 BC that a pneumatic engineer with the somewhat theatrical name of Hero devised his aeolipile (Figure 1). Whilst he is not on record as having formulated it, he clearly was aware of Newton's third law – to every action there is an equal and opposite reaction. In his device, a spherical chamber is mounted to rotate between two supports. One of these is hollow and steam rises from the boiler up the hollow support and through the bearing into the spherical chamber which, driven by jets from the spouts, rotates. Whether or not Hero constructed his machine history does not record. It certainly would have worked, but I think that if he made it, he probably had some trouble with leakage at the bearing. There seems to have been no development of Hero's design, though as long ago as 1629 Giovanni Branca proposed a turbine driven by a steam jet (Figure 2).

The first reference to jet propulsion as a direct means of locomotion of which I am aware is by the Dutchman 's Gravesand, in his book *Mathematical Elements of Natural Philosophy*, subtitled *An Introduction to Sir Isaac Newton's Philosophy*. This was a Latin work, but was translated into English; in the sixth English edition, published in 1747, there is an illustration (Figure 3) of a machine rather less sophisticated than Hero's turbine, but arranged to give translational motion. Water is first heated in a spherical boiler which is then placed on a four-wheeled trolley. A pin is removed from a nozzle on top of the boiler so that a backward steam jet is produced and, by reaction, the trolley moves forward.

The obvious step of carrying the furnace permanently on the vehicle is illustrated in Charles Dollfus's *Histoire de la Locomotion Terrestre* of 1936 and dated 1804. Dollfus also illustrates an earlier device of 1775 in which the steam jet drives a turbine which in turn activates driving wheels through gearing.

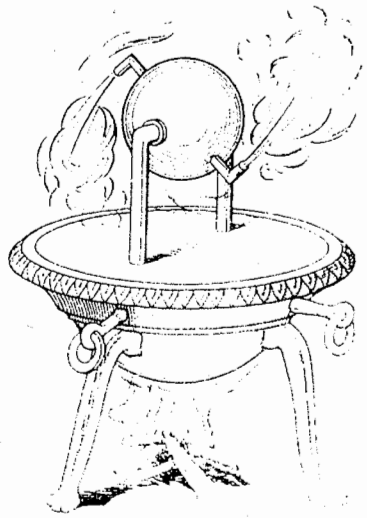


FIGURE 1. *Hero's aeolipile (c. 150 BC)*

So far as I am aware the idea of jet propulsion was first extended to aerial travel sometime in the 1840s (Figure 4). In 1842 W. H. Phillips designed, built and demonstrated on Primrose Hill a model helicopter in which heat from a chemical reaction boiled water, forcing steam from nozzles arranged, much as Hero might have done, to rotate the blades. The glider pioneer Chanute in 1894 described Phillips' model as 'undoubtedly the first machine which has risen into the air by steam power'. I think we can say it was the first to fly by any power and the first, other than a rocket, to fly by jet power, so it is a pity there is no picture of it extant. The Phillips system has, however, been illustrated by Victorian artists.

Other interesting projects have recently been recalled in an admirable paper by Mr. Peter Stokes in the Royal Aeronautical Society's magazine *Aerospace*. He illustrates the de Louvie resonant duct jet of 1865, which seems to anticipate the engine of the German V1 flying bomb of the last war, and the Butler-Edwards proposal of 1867

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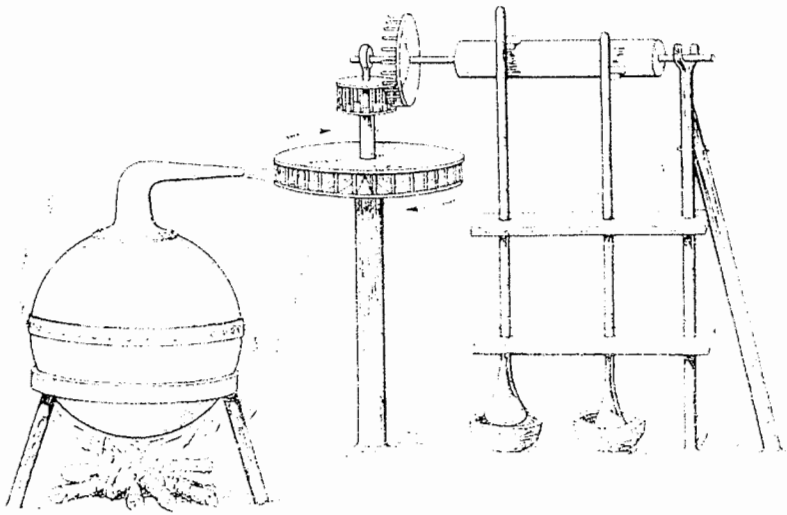


FIGURE 2. *Giovanni Branca's turbine (1629)*

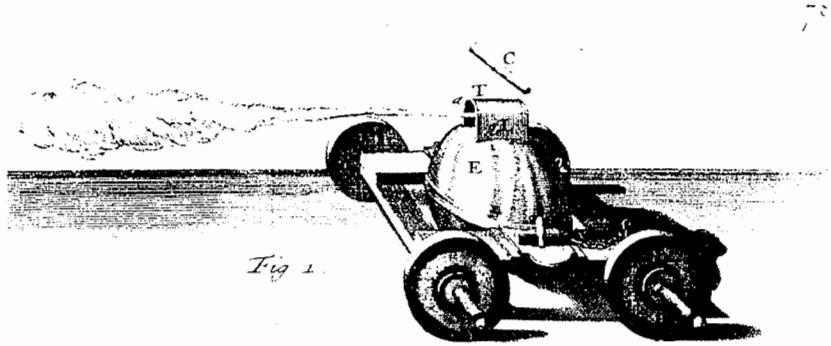


FIGURE 3. *'s Gravesand's jet propelled model vehicle (c. 1740)*



FIGURE 4. *Mr. Golightly on Messrs. Quick and Speed's steam riding rocket (1843)*

for an aeroplane with a small steam generator exhausting rearwards. He also describes the design which Richard Boyman patented in 1866. Boyman proposed a sheet steel airship of 200 ft. diameter, a quarter of a mile long, the whole weighing 600 tons and carrying four boilers, fired from the lifting gas, and with the steam generated inducing, in the manner of a locomotive blast pipe, combustion gas flow through the boilers with subsequent ejection in a jet propulsion system with swivelling nozzles for propulsion and lift adjustment.

As a serious source of power, however, jet propulsion made no progress and wind and water were in effect the only sources of power, in addition to manpower and horsepower, until the eighteenth century when the early steam piston engines, which we particularly associate with Newcomen and Watt, began the industrial revolution. Almost unnoticed however, in 1791, John Barber of Nuneaton was granted a patent for a gas turbine (Figure 5). The gas turbine was destined to be the means by which jet propulsion became an effective motive source, and indeed the modern high speed aircraft power plant should properly be described as the aircraft gas turbine.

At this stage I think I should make it clear how the aircraft gas turbine works. Air which is admitted to the engine is compressed and delivered to chambers in which fuel is introduced and the mixture burnt. From this combustion system the exhaust streams through a turbine which drives the compressor. In the simple jet propulsion engine the gas, having driven the turbine, streams out in the so-called jet pipe, and there is sufficient energy left in the jet to propel the aircraft forward.

Alternatively, with the turbine designed not only to drive the compressor but to provide shaft power as well, an airscrew can be mounted on the turbine shaft to propel the aeroplane in the more conventional way.

It could be argued that all aircraft are jet propelled and always have been. The propeller of all aircraft of the older kind can be thought of as sending backwards a cold large diameter relatively slow jet, whereas the pure jet propulsion engine is sending back a hot relatively small diameter high-speed jet. For a long time the two kinds of power plant appeared to be very different. Indeed, they are, but as the jet propulsion engine has developed, almost every modern engine is of the type known as a ducted fan or turbo fan engine.

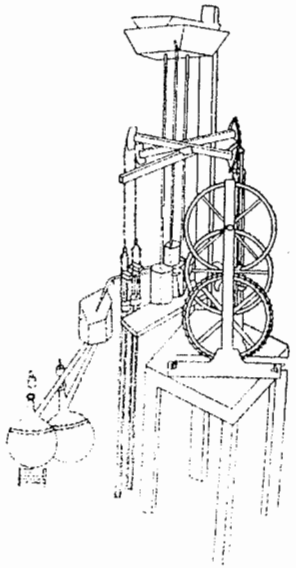


FIGURE 5. *John Barber's gas turbine design (1791)*

This means that in addition to what might be termed the pure jet, there is an outer jet generated by the blades of a fan which is producing its effect in the same way as a propeller. This slightly confused situation was first apparent as long ago as 1935, when the Italians produced the Campini-Caproni aircraft (Figure 6) in which a jet was produced by totally ducted fans, or small diameter airscrews, driven by a conventional piston engine.

In the 1930s in this country there were two distinct schools of thought, corresponding to the two kinds of engine. At the Royal Aircraft Establishment Dr. A. A. Griffith and his colleagues began experiments in 1929 which had as the ultimate objective the creation of a new kind of aircraft engine, a gas turbine designed to drive a propeller. In other words the gas turbine was thought of as an alternative to the aircraft piston engine. The other school of thought derived entirely from the ideas of Frank Whittle, who in 1929 designed an aircraft engine in which the propulsive power derived from a high-speed jet and in which there was no propeller. In those distant days before the Second World War, the two schools of thought were quite separate and the idea of their ultimate coalescence entered no one's head. They represent also two very different engineering philosophies. At the Royal Aircraft Establishment the basic notion was to develop axial compressors of high efficiency for ultimate use in an engine design. Frank Whittle's philosophy was to design an engine and develop it until it was a satisfactory prototype.

The work at the Royal Aircraft Establishment derived from an aerodynamic theory of compressor and turbine design based upon the flow past aerofoils as distinct from the then current steam turbine theory of flow through passages. Griffiths put his proposals before a meeting of representatives of the Air Ministry and the Aeronautical Research Committee, and this meeting expressed itself in favour of preliminary investigations being done. Consequently, in 1927, two sets of experiments were put in hand. One of these investigations, reported on in 1928, was on stationary groups and cascades of aerofoils, representing compressor and turbine blading, in small wind tunnels. This, through the absence of blade motion, was of value only in making first comparisons of the new theory with the older theory. The second set of experiments was done with a special test rig, which consisted essentially of a single-stage turbine and a single-stage axial compressor mounted on the same shaft (Figure 7). This rotor was driven by taking air through the casing by means of an exhauster pump, the air travelling past turbine stators, turbine rotating, compressor rotating, and compressor stator blades in that order. The blading had of course been designed on the basis of Griffith's new theory. The tests in this tiny rig – the internal casing diameter was only 4 in. and the blade height about half an inch – took place in the early part of 1929 and were so encouraging that at the end of that year Dr. Griffiths discussed in an official paper the prospects of an internal combustion gas turbine driving an airscrew as an aircraft power plant. This paper was well received by the Aeronautical Research Committee, and they did indeed recommend that work should continue. However, for reasons which cannot now be discovered, no further work on axial compressors and turbines was done until 1936. In that year the Royal Aircraft Establishment was authorized to build an axial flow compressor which in 1938, after running for thirty seconds, failed. It was reconstructed

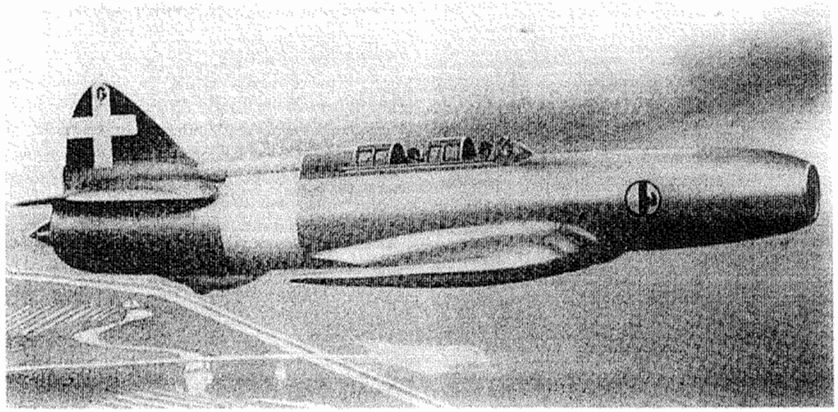


FIGURE 6. *Campini-Caproni propeller-less aeroplane (1935)*

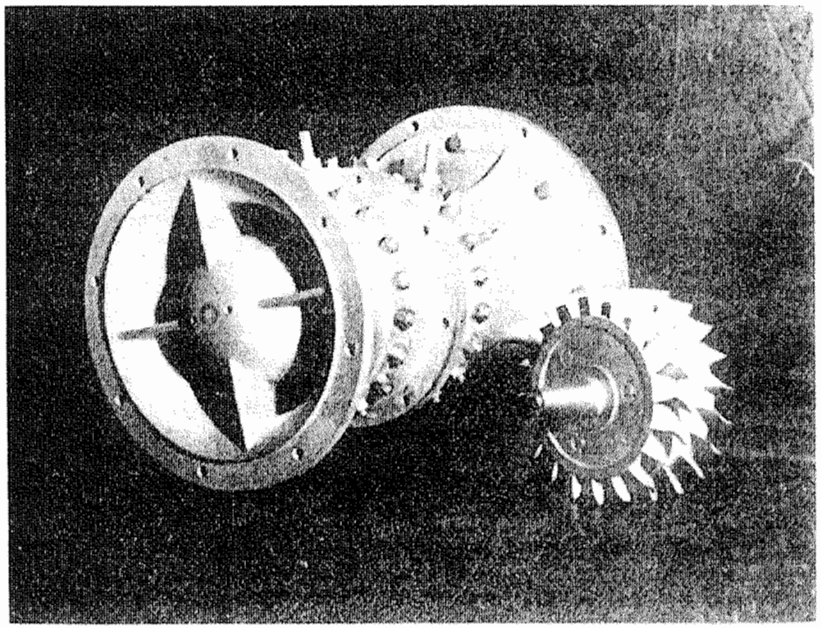


FIGURE 7. *Model turbo-compressor tested at Royal Aircraft Establishment (1929)*

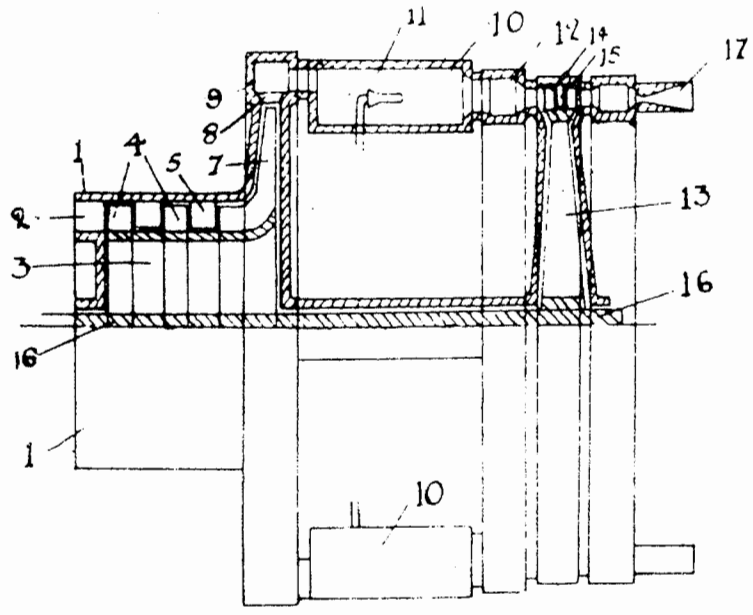


FIGURE 8. *Scheme drawing from Whittle's master patent (Jan. 1930)*

with certain design changes, and the results achieved were such that a more ambitious scheme began to take shape. Hayne Constant of the Royal Aircraft Establishment had put forward in March 1937 another paper on the gas turbine as an aircraft power plant, and once again the Aeronautical Research Committee made recommendations which resulted in work being started by the Metropolitan Vickers Company and the Royal Aircraft Establishment in collaboration on the design of an aircraft power plant, consisting of a low-pressure axial compressor driven by a low-pressure turbine in series with a high-pressure axial compressor driven by a high-pressure turbine, with a separate power turbine driving the airscrew. Although parts of this rather complicated engine were completed and proved satisfactory on test, an improved design took its place. That too was abandoned, but as a result of work on it and other experimental compressors, the Metropolitan Vickers Company began work on a complete jet propulsion engine in July 1940, which went on test in December 1941. This was the first axial compressor jet propulsion gas turbine, and, as I will record later, was successfully flight-tested.

While Griffith and Constant, first his assistant and later his successor as Head of the Royal Aircraft Establishment Engine Department, were advancing slowly with limited resources and limited encouragement towards their successful collaboration with the Metropolitan Vickers Company, the other struggle against orthodoxy was being led by Frank Whittle. Whilst still a cadet at Cranwell Whittle described in a thesis in 1928 the possibilities of the jet propulsion gas turbine engine.

The nature of Whittle's gigantic contribution to the advance of aircraft performance must be clearly understood. Jet propulsion, as we have seen, was known to Hero and Newton and to many others. The gas turbine had been invented by

John Barber and spasmodically developed with minimal success. Whittle believed that the gas turbine was the ideal means of providing a powerful jet and this is what, in his engine, he succeeded in demonstrating. Whittle's first patent was dated January 1930, and the most important drawing from it clearly illustrates his scheme (Figure 8). You will see in this drawing the stages I mentioned earlier – the initial compression, combustion, the combustion gas streaming through a turbine, and its remaining energy providing direct jet reaction. One of the most interesting aspects of this design is Whittle's adoption of an axial compressor for the first stages of the cycle. In all his engines, however, he used a centrifugal compressor. He knew that at Farnborough Griffiths and Constant were having great struggles with axial compressors. He knew that the centrifugal compressor had been successfully used for years in superchargers for piston engines. He argued that the first jet propulsion engines were likelier to succeed if the centrifugal compressor, with its known limitations but with a history of success in other applications, were used alone instead of in combination with the axial compressor of his original scheme. Not only was he undoubtedly correct in this, but he succeeded by applying his genius to it in designing better centrifugal compressors than had ever been produced before.

In the early 1930s Whittle was unsuccessful in getting support either from Government or from Industry, but in 1935 two gentlemen who had been invalided out of the Royal Air Force, R. D. Williams and J. C. B. Tinling, the former of whom had been at Cranwell with Whittle, approached him and between them they succeeded in interesting a firm of investment bankers, O. T. Falk and Partners. The bankers obtained an independent assessment of the Whittle scheme from M. L. Bramson, whose name should never be forgotten, because he concluded that the Whittle jet propulsion gas turbine was viable and accordingly a company called Power Jets Limited was formed in 1936. The outcome was an engine manufactured to Whittle's designs by the British Thomson-Houston Company with certain combustion components and accessories made by Lucas and other contractors. The engine (Figure 9) was a simple jet propulsion gas turbine with a single stage centrifugal compressor, designed to have a compression ratio 4:1 and driven by a single-stage turbine. There was a single combustion chamber between the compressor and the turbine.

This engine deserves its place in history. It was the first British jet propulsion gas turbine and it may have been the first in the world. I say 'may have been' because at this time the Germans were already at work. Unfortunately, when Whittle applied for his first patent, and the more detailed ones that followed it, no one in authority had thought it necessary to keep them secret. They were publicized in engineering magazines and proved to be an inspiration to the Germans.

Most engineers are aware of the extraordinary phenomenon that new ideas occur in several countries almost simultaneously. I could give a number of examples. The only one relevant to my theme this evening, however, is that whilst Whittle was undoubtedly the first in the field, independently in Germany, in Russia and in Hungary work on the gas turbine was proceeding at the same time as the work in this country I have been describing. Jendrassik at the great Ganz Works in Budapest was doing admirable work which inevitably was denied the success it deserved. Lyulka was working at the Kirov works in Leningrad, but the most formidable of Whittle's rivals was von Ohain in Germany. In 1935, apparently at that time uninfluenced by Whittle's published patents, he produced and constructed a gas turbine jet propulsion engine with a two-stage compressor – an axial stage followed by a centrifugal stage – and an inward-flow radial turbine. The first model was built at von Ohain's expense by Max Hahn, but unable, not unnaturally, to finance the development himself, von Ohain, through his Göttingen Professor, met the famous Dr. Heinkel, and Heinkel supported the whole development of the engine in which von Ohain was associated with Max Hahn and Wilhelm Gundermann. At the same time Heinkel was designing an aeroplane to take the engine. As in the case of the Whittle engine, the development of the combustion system took as long as that of the rotating parts, but finally a successful engine was produced and installed in the aeroplane known as the He 178 V_1 (Figure 10). The designer of the airframe, under the leadership of Heinkel's Technical Director, Hertel, was Schwarzler, and taxiing trials of the machine began on 24th August 1939. On 27th August the test pilot Erich Warsitz made the first flight. So although Whittle's main patents were five years ahead of von Ohain's, the enthusiastic

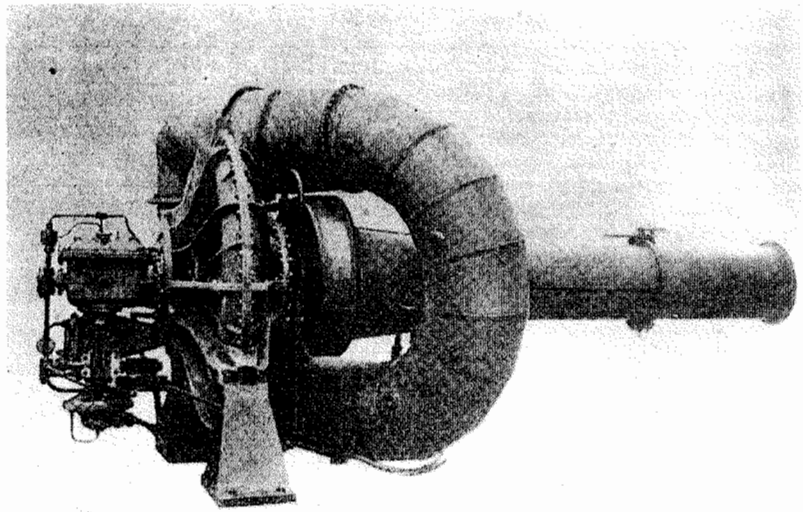


FIGURE 9. *The first Whittle engine (1937)*

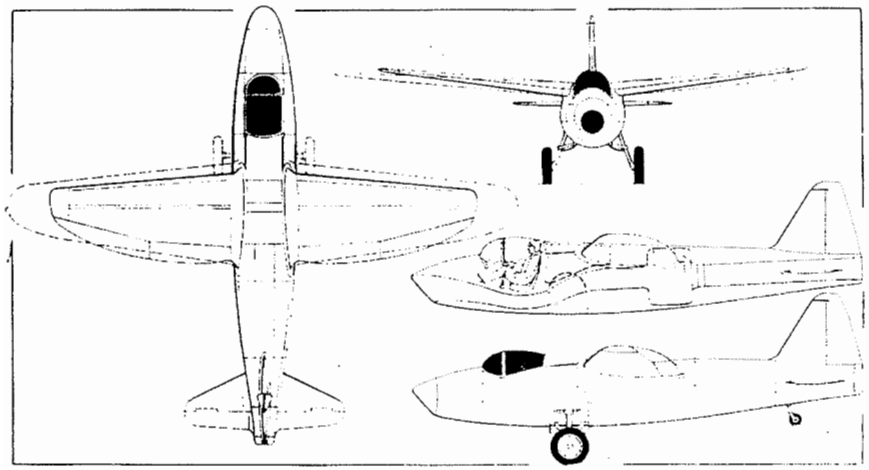


FIGURE 10. *The first German gas turbine aircraft He 178 (1939)*

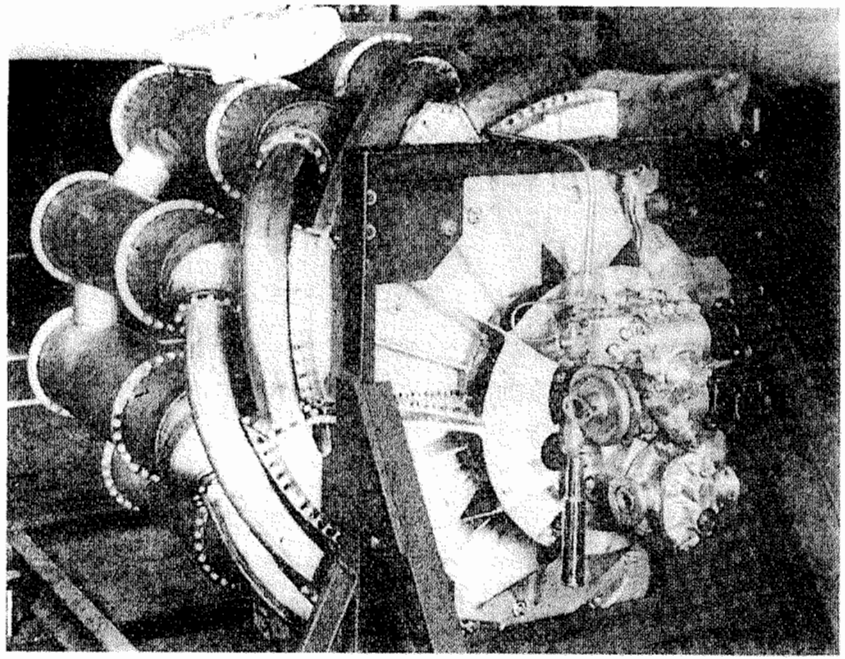


FIGURE 11. *The Whittle engine used in the first flights (1941)*

support of Heinkel led to the flight of the He 178 a year and three quarters before those of the first British jet propulsion aircraft, which until war was imminent certainly did not have the same kind of drive behind it.

Coming back to the first Whittle engine, however, the high compression, the intensity of combustion necessary for success, and the lack of experience in turbine blade design on aerodynamic lines, made it seem almost a miracle that the engine worked. That it did, although imperfectly, is a tribute to the extraordinary abilities of Frank Whittle. To him the most difficult part of the design was the combustion system, and he enlisted the aid of A. B. S. Laidlaw of Laidlaw, Drew & Company. Undoubtedly the association with Mr. Laidlaw was of the greatest value. The tests on the engine began on 12th April 1937 and they continued with intervals until 23rd August. They showed that the compressor performance was below expectations and that a great deal needed to be done to improve combustion. As a result a major reconstruction of the engine was put in hand.

The fresh consideration of gas turbines and jet propulsion by the Aeronautical Research Committee in March 1937 led to the Air Ministry placing contracts with Power Jets Limited for a report on the testing which I have mentioned and for further experiments with the engine.

The reconstructed engine was tested on 16th April 1938 and testing continued intermittently until a turbine blade failure caused considerable damage on 6th May. The combustion was still clearly unsatisfactory, and further combustion testing led to the combustion system in the third version of the engine being radically different. Instead of the single helical combustion chamber, there were nine separate chambers. This engine was on the experimental test bed from the end of October 1938 until February 1941, when a turbine failure brought its life to an end.

Long before this, however, in the summer of 1939, the Air Ministry had decided that on the evidence by then accumulated by Whittle, he had, in essence, a practical aircraft engine. They therefore commissioned Power Jets Limited to

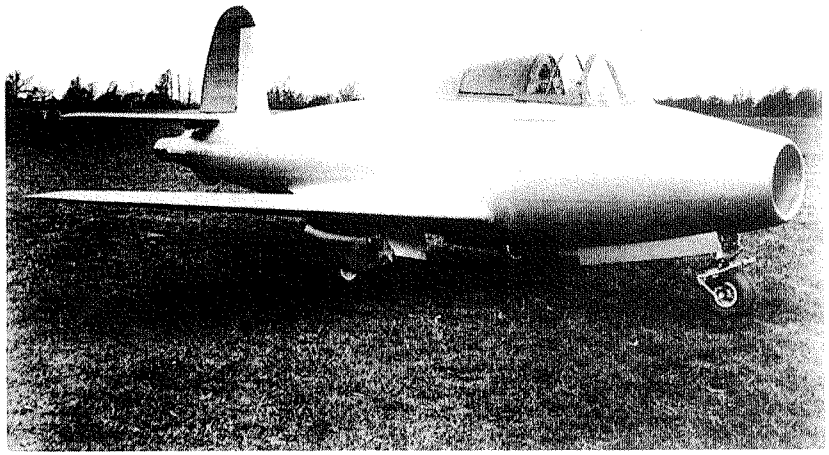


FIGURE 12. *The first British gas turbine aircraft, the Gloster E 28/39, to fly (May 1941)*

supply an engine for flight test. A contract was also given to the Gloster Aircraft Co. Ltd., for the design and construction of an experimental aeroplane. This aircraft, which has claims to renown like those of 'Puffing Billy', 'Rocket', 'Britannia' and 'Turbinia', will go down to posterity by the mere number of its specification – E 28/39.

This was the most critical period in the history of British jet propulsion. The flight engine known as W1 (Figure 11) was designed by Whittle, helped by the engineers of the British Thomson-Houston Company, to whom the drawing and manufacture were sub-contracted by Power Jets Limited. From spares of the W1 engine an engine for test purposes, called W1X, was assembled and run during 1940, and when the W1 engine was delivered by British Thomson-Houston, Power Jets incorporated in it the results of experience gained with W1X. Then, after preliminary bench tests, the W1 engine was put through a 25-hour test to clear it for flight. It was decided that the clearance test permitted about 10 hours flying with the engine.

In the meantime E 28/39 (Figure 12) had been completed and the W1X bench test engine was installed in it. During April 1941 it was taxied and in the course of these taxiing trials at Hucclecote, the Gloster Company's airfield, the aircraft left the ground – no doubt through sheer inadvertence!

W1, the flight engine, having had incorporated in it modifications demanded by the bench tests on W1X, was installed in the aircraft early in May and Flight-Lt. P. E. G. Sayer began the definitive trials on 14th May 1941. In the evening of that day he did taxiing trials and on 15th May he made the first real flight, being air-borne for 17 minutes and doing speed runs at 2,500 and 4,000 ft. By 28th May the flight programme was finished. The total flying time of 10 hours 28 minutes had been completed without incident. The aircraft had made 17 flights and had performed admirably. The engine, with its 25-hour bench test and ground running in the aircraft, had run for 39 hours 57 minutes, and was found on dismantling to be in excellent condition. This historic unit had a thrust of 850 lb. at 16,500 rpm, its maximum speed. Its installed weight was 623 lb.

The almost incredible success of these flight trials naturally produced tremendous optimism

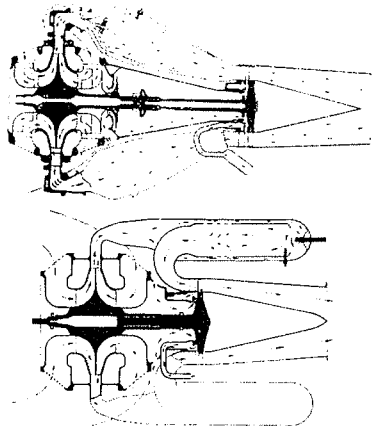


FIGURE 13. *Comparison of 'straight through' and return flow combustion systems*

and enthusiasm. When Mr. Churchill heard of it, he said, not I fear comprehending that E 28/39 was an aircraft designed for research and development, that he wanted 1,000 as soon as possible. But I always remember that Sir John Buchanan, at that time a senior Civil Servant in the Ministry of Aircraft Production and long experienced in aviation, warned us that the results were almost too good to be true, and there was bound to be trouble ahead. And there was.

After the successful trials of E 28/39 interest in jet propulsion aircraft began to spread. Previously, as I have explained, there were broadly speaking two schools of thought, one developing with the axial compressor at the heart of the design, and the other with the centrifugal compressor. In addition to these two types of engine, there were two schools of thought on the subject of combustion. The difference between these two schools is best illustrated by a diagram (Figure 13). Whittle, for a variety of very practical reasons, favoured what was called a return-flow combustion system. Others, notably engineers of the Rover and de Haviland Companies, favoured a straight-through combustion system. I believe that Whittle had very good reasons for choosing the return-flow system for his early engines, but with the development of combustion technology, the straight-through system came to be the more acceptable solution, and to-day there is no vestige of the return-flow system in any design.

The history of jet propulsion engine development in 1941 began to be extremely complicated. The Rover Company had been brought in during the previous year as production manufacturers of engines designed by Power Jets, but the association between the two companies was an unhappy one. Power Jets would have liked to be in charge of production, and Rover were anxious to introduce their own ideas into the design. I do not think that the difficulties of this failure to collaborate have any place in my talk this evening, but the outcome was of major importance. To create a more successful organization for progress, Rovers took over from Rolls-Royce the manufacture of tank engines and Rolls-Royce took over from Rovers the Whittle type of gas turbine engine. Rovers did, however, succeed in producing a development of the Whittle engine with straight-through combustion, and it deserves its place in the history of jet propulsion engine design. This particular engine did not go into service, but it served as a prototype for the Derwent I engine which Rolls-Royce ultimately developed for RAF service.

Whilst the Power Jets – Rover association was in being, Whittle had developed at Power Jets the W2B engine, which must rank among the classics. It was the design from which the twin-engined F9/40 aircraft, its production version the Meteor, and the American Bell P59 derived their power plants.

Engines were installed in the F9/40 aircraft (Figure 14) in the summer of 1942 and taxiing trials began on 10th July of that year. With the runway lengths available, however, the aircraft was not allowed to take off, and enough improvement in engine performance for flight to be allowed was delayed until June 1943. Meanwhile the American Bell XP 59A (Figure 15), with two Whittle type engines designed by the American General Electric Company, began flying on 1st October 1942. This was a tremendous performance; the Bell aeroplane was the second allied jet propulsion aircraft to fly.

It was in January 1941 that the Ministry of Aircraft Production brought the engine side of the de Havilland Company, the head of which was Frank B. Halford, into the jet propulsion field. Halford, with a long experience with centrifugal blowers and with a knowledge of Whittle's progress, decided on a design with a single-sided centrifugal compressor and straight-through combustion (Figure 16). Halford's performance was

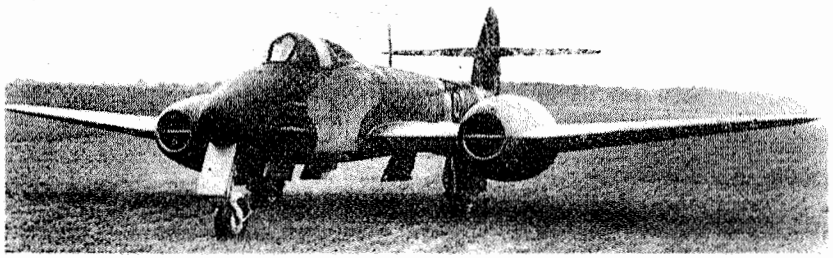


FIGURE 14. *The Gloster F9/40, with Whittle W2B engines (1942)*

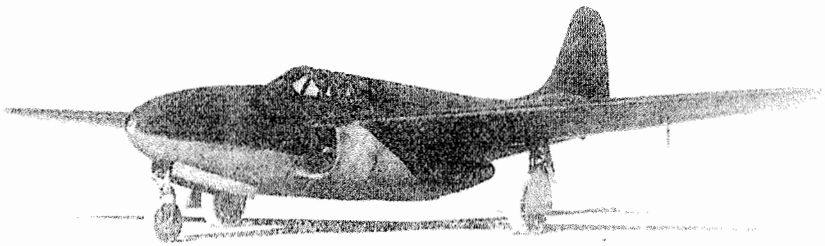


FIGURE 15. *The USA Bell XP 59A with Whittle type engines (1942)*

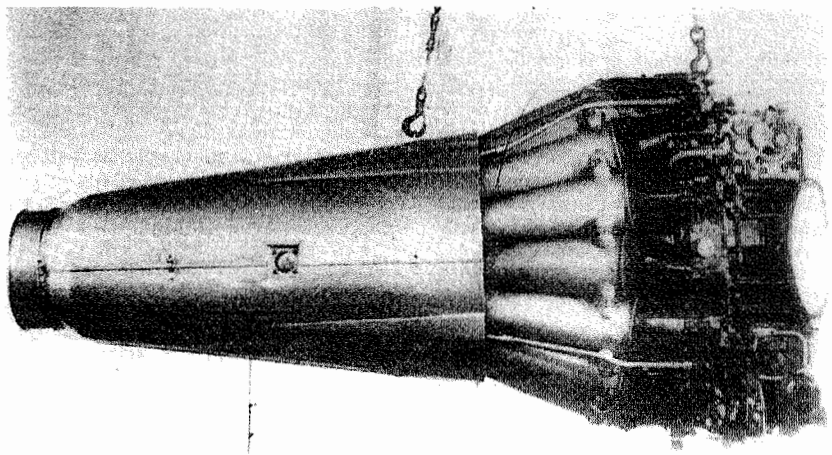


FIGURE 16. *The de Havilland Halford centrifugal compressor H.1. engine (1942)*

brilliant. Design work began in April 1941 and testing of the first Halford engine, called H.1, began in April 1942. A pair of the engines was installed in an F.9/40 aircraft which flew first on 5th March 1943. This was the first F.9/40 aircraft to fly as, although Whittle's W2B engines had been installed in another F.9/40 many months previously, as I have already indicated a series of difficulties prevented it from flying until June 1943. Subsequently, however, this remarkable Halford engine was not associated with the F.9/40 or Meteor aircraft. It found its ideal development in the de Havilland Vampire, whilst the Meteor progressed with the Rolls-Royce development of the Whittle engines.

While the work on the Whittle engines and the Halford H.1 was going on, development of the Metropolitan Vickers F2 engine, to which I referred earlier in this talk, continued. This engine had a nine-stage axial flow compressor, a single annular combustion chamber, and a two-stage turbine, all on the same axis. I have already remarked that this engine began its tests in December 1941, and after modification it passed a 25-hour test in November 1942. A pair of these engines was installed in a specially modified Gloster F.9/40 aircraft which differed from the other F.9/40 aircraft in that the engines were slung under the wings instead of being carried in cells placed symmetrically above and below the wings. It was in this aeroplane that the first British flight of an axial compressor jet propulsion gas turbine took place at Farnborough on 13th November 1943. This engine can fairly be regarded as the progenitor of the long line of axial compressor engines which continue to-day.

I think that it is with the early flights of the Whittle type return flow centrifugal compressor engines, the de Havilland Halford straight through combustion centrifugal type engines and the Metropolitan-Vickers axial compressor straight through combustion engine that the early years of jet propulsion flight in this country can fairly be said to have ended. More and more the development of the jet propulsion gas turbine in this country came into the hands of Rolls-Royce. They developed the centrifugal compressor gas turbine in the Welland and Derwent variants. They produced the Dart propeller gas turbine which had two centrifugal compressors in series and persists to this day, but all new designs at the present time employ axial compressors and there is no doubt that the centrifugal compressor has had its day, except in certain helicopter engines.

As I tried to explain at the outset, the adoption of the centrifugal compressor by Whittle and by Halford did not mean that they preferred it to the axial compressor. Indeed, in Whittle's first patent, as I have pointed out, he proposed an axial compressor and a centrifugal compressor combined. But Whittle and Halford knew that at the time they were designing their early engines, they could design with greater certainty a successful centrifugal compressor than they could a successful axial compressor. As technology has advanced, however, the axial compressor has been developed to give results which the centrifugal compressor could never have achieved, and the future undoubtedly lies with the axial type. Of the present Rolls-Royce range of gas turbine aeroplane engines only the earliest, the Dart, has centrifugal compressors. Moreover, only two are pure jet propulsion engines. The rest, now usually called turbo-fan engines, employ ducted fans producing a flow of cold air around the hot jet propulsion unit. It seems that in the future as well as seeing gas turbines driving old fashioned propellers, as does the Dart, we shall see them driving small diameter, multi-bladed, unducted fans. The simplicity of the early days has, I fear, gone but with the gas turbine playing a variety of rôles, complexity and variation in design is inevitable. The remarkable thing is that with increasing complexity and increasing performance, we have achieved increasing reliability, and the aircraft gas turbine of to-day requires less maintenance and achieves far longer periods between overhauls than its predecessor, the piston engine. It has produced a revolution in aircraft propulsion.

In conclusion I want to record my thanks to Mr. J. A. Bagley of the Science Museum and Mr. A. W. L. Naylor, Librarian of the Royal Aeronautical Society, for their help.

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DISCUSSION

THE CHAIRMAN: We have listened to a fascinating account of one of the greatest technological advances since the industrial revolution. There is no doubt that aviation, both military and civil, would be very different indeed to-day if Whittle and all that has flowed from his work had not come forward. Lord Kings Norton was in the thick of it all the way through and I think he has been very modest about his own contribution. In fact he has hardly mentioned it. When history comes to be written I am sure that will be corrected.

It is somewhat ironic as well as interesting to recall that in 1934, the year before Power Jets was formed to develop Whittle's designs, the then Secretary of State for Air, Sir Philip Sassoon, who was a pretty perspicacious chap in most ways, remarked in the House of Commons that scientific investigation into the possibilities of jet propulsion had given no indication that this method could ever be a serious competitor to the airscrew engine combination. One must not believe everything that is said in the House of Commons. Those were the days when some of the corridors of power were 'a hot bed of cold feet', and Whittle rather warmed up those feet.

I cannot forbear to quote our lecturer in words which I heard him speak in 1947. I took them down at the time to use in evidence against him. 'It is one thing to have an idea; it is another to have the technical and executive ability to give it flesh. It is yet another to have the tenacity of purpose to drive through to success, unshaken in confidence in the face of discouraging opposition. Whittle, whose name in the annals of engineering comes after those of Watt, Stephenson and Parsons, only for reasons of chronology or alphabetical order, had all those things.' That is a very good summing up.

Lord Kings Norton has reminded us this evening that Rovers were brought in early as a rather odd selection. But then they passed over the jet engines to Rolls-Royce and, with them, that great chap, Adrian Lombard, who picked up the ball and became the leading designer of Rolls-Royce's jet engines; sadly to die very young. This lecture is a further important contribution to the history of engineering progress and it comes in a tradition which was launched in this very room in February 1892 when William Robinson lectured on the petrol-burning internal-combustion engine and just hinted at the possibility of jets. In the same tradition was the Trueman Wood Lecture of 1946 by Frank Halford with Frank Whittle in the Chair. None of these lectures, however excellent they were, had the broad background which Lord Kings Norton has brought to his subject this evening.

DR. G. B. R. FEILDEN, CBE, FEng, FRS: We have had a model description of the history of jet propulsion. I cannot add anything significant to the detail except perhaps a personal account of that very first taxiing trial to which Lord Kings Norton referred.

I, along with Mr. Geoffrey Bone, who is sitting next to me, had joined Power Jets in the Summer of 1940 and there was a third engineer, Mr. R. D. van Millingen. We had all been very closely concerned with the work and the question arose which of us should be given the task of installing the engine for the taxiing trials. I remember vividly that as we stood in the corner of the workshop, Whittle produced some straws which he held behind his back and I was lucky enough to pick the one to install the taxiing engine; Geoffrey Bone had the job of installing the flight engine.

We took our engine down from Lutterworth to Gloucester, all very secretly, in the height of the war, and I remember for the first time I had sleepless nights over my work, waking up in a cold sweat. Had we put the split pin in the centre of the turbine shaft? Of course we had, but this indicated the pressure we all felt to do the best possible job.

One evening, fairly late, we had finished everything and the aircraft was taken out on to the grass airfield and the engine was run up. It started very nicely and we got it up to 13,500 rpm, at which we hoped the aircraft would roll quite nicely and we could do a little trundle round the airfield and see that all was well. It barely moved on the water-sodden grass in early April. We went away disconsolate. However, the following day we ran the engine up to a rather higher speed and the aircraft luckily moved. Finally, we got up to 16,500 rpm and, as Lord Kings Norton said, the aircraft did a short hop. For me, that was one of the marvellous moments of my life. The point of this story is that I was standing by the side of the number two pilot, Mr. Michael Daunt. He said, 'Thank God it flew straight.' I said, 'Why ever shouldn't it? Newton's laws and so on.' But he said, 'No, we have all been scared stiff by this jet engine because we have seen compressed air jets in the shop squibbing all over the place and thoroughly unstable. We thought this aircraft was going to do the same.'

The subsequent Cranwell flight trials of the Gloster-Whittle E28/39 aircraft really were most remarkable. As Lord Kings Norton said, in ten hours of flying the engine covers were never once removed, in spite of the fact that we had a totally new system of propulsion, a new aircraft, new everything. That I think must be one of the engineering achievements of all time.

THE CHAIRMAN: Thank you very much. It is fascinating to have first-hand accounts of such moments in history. Fortunately the first E28/39 aeroplane is preserved in the Science Museum. The second one I think was written off.

THE LECTURER: Yes, it crashed, and killed the pilot.

MR. GEOFFREY W. BONE, MA, CEng: May I add to what Bob has said about Lord Kings Norton's magnificent address. Having been interested in jet propulsion since 1940, I have learnt a lot this evening and it is wonderful to have it set out in such an objective way.

Objectivity was not always a feature of those early days. If I may recall for a moment the first few episodes of the first flights at Cranwell, I was put on this because I was in RAF uniform and told to share a room with Flight-Lieutenant Brown of the Fleet Air Arm, who was intensely inquisitive about what was going on. He took pleasure in flying a Grumman Martlet fighter straight at the hanger in which the E28/39 was kept, just to see how close he could get to have a look inside. On the evening of 15th May the airfield was out of bounds to all RAF personnel. There were, however, many hundred airmen behind the hedges trying to see what was happening. The Power Jets team naturally had to keep their mouths shut and the speculations in the mess about this extraordinary machine were quite incredible. The popular bet was that its power plant consisted of a small four-bladed propeller inside the fuselage driven by a specially super-charged Rolls-Royce Merlin piston engine. For security reasons we encouraged that idea assiduously. When the first flight took place somebody, I think it was Flight Lieutenant W. E. P. Johnson, was heard to exclaim, 'My God, Frank, it flies!' At which Frank Whittle turned to him and said, 'Of course it does; it was bloody well meant to.'

DR. AUSTIN WILLIAMS, MBE, JP, BA, PhD: Would Lord Kings Norton give us his thoughts on the future? What after Concorde, etc.?

THE LECTURER: In 1940 I wrote a paper called 'Looking Forward . . . the Future of British Civil Aviation' and I could not have got things more wrong! I prophesied, but things happened ten times as fast and to magnitudes which were greater than I had ever thought possible. One of the most remarkable things about aviation is its unpredictability. In those days things could happen very quickly indeed, because of the impetus which war always gives to research and development. Since then things have become extraordinarily complex and progress seems slower. To-day the argument that is going on at the Council of Ministers is on the design of the European fighter aircraft of the middle 1990s! In other words we do not expect a successor to the Tornado until then. Nevertheless, I think performance will steadily improve for the two obvious reasons – improvement in the performance of the power plant and greater knowledge of the refinements of aerodynamics.

I don't think I am going to be tempted very far in prophecy in civil aviation because there is another factor which has a habit of slowing things up – economics. There could be a successor to the Concorde before the century is out, an aeroplane which will be faster and which will probably be of steel, titanium and carbon fibre rather than the aluminium of the Concorde. When we were thinking about aeroplanes like Concorde in the early 1960s there was a school of thought which would have gone for steel then and produced an aeroplane of greater speed. But Concorde arrived sooner than that one ever could have done. In fact the American aeroplane corresponding to Concorde was dropped because it was looking too far forward. In civil aviation I think the big subsonic aeroplane has many years ahead of it. Broadly speaking, I do not see another revolution in this century.

THE CHAIRMAN: It is no secret that a second generation SST is on the drawing boards. The real problem is how to finance it. The requirements on the drawing boards are three times the size of Concorde because Concorde is too small to be economic. Its seat-mile costs are very much the same as the old DC-6B of years ago which was also about the same size, so that it is really not out of scale against the small size of the aeroplane. But the second generation SST which is being looked at to-day will be three times the size and as Lord Kings Norton has said, rather faster. It will be designed to go non-stop to the West Coast from London, which is one of the requirements, over the Pole.

MR. DAVID MORGAN, TD (Chairman, Transport Trust): At the risk of displaying ignorance of aviation history, I wonder when the Russians got their prototype flying if that is known; if so, was it a separate development, or was it hinged on to developments in Germany and England?

THE LECTURER: The prototype jet propulsion gas turbine? Their development, I think, is derived from information we gave them about 1946.

THE CHAIRMAN: Sir Stafford Cripps in a burst of enthusiasm for Russia handed them over the Rolls-Royce Nene engine in 1946.

THE LECTURER: I should perhaps have made it clearer that when I referred to what Lyulka was doing in the Kirov Works and what Jendrassik was doing at the Ganz Works in Budapest, they were designing gas turbines but not essentially aircraft gas turbines. Theirs were intended for industrial purposes. Jendrassik, who came here, was a brilliant man. I do not know what happened to him in the end.

DR. WALTER BETTERIDGE (formerly of The Mond Nickel Company and its successors): I wonder

if Lord Kings Norton would care to comment on the relevance of material developments to the progress of the engine. I was at Bristol at the time, not involved in turbines until a few years later, but when I joined The Mond Nickel Company about 1947 the alloys which are now conventionally used had already been developed by Dr. L. B. Pfeil and Dr. N. P. Allen and I had the privilege of continuing that development in later years. I have always been interested in knowing how the original interchange of requirements came as between Power Jets and The Mond Nickel Company. I believe that Dr. Hubert Sutton of the Ministry of Aircraft Production, or perhaps the Air Ministry at that time, was largely instrumental in that.

THE LECTURER: Now that you have mentioned that I feel very regretful that I did not somehow introduce an account of the corresponding work on the materials side, because the truth of the matter is that neither the Whittle engine nor any of the early gas turbines could have worked at all without the development of high temperature materials. Among those materials the first was Nimonic 80, a nickel alloy which had been developed in The Mond Nickel Company by Dr. Griffiths, and by Dr. Pfeil. You mentioned Dr. Allen too. There were other people whose names I do not recall at the moment. In Sheffield Dr. Hadfield was struggling hard to compete, using steel, but it was the Nimonic alloys which made the jet propulsion engine a working proposition at that time. Without them, there would have been considerable delay.

MR. THOMAS HERBERT FROST, BScTech, PhD (Department of Mechanical Engineering, Newcastle-on-Tyne University): To what extent was the work done by Dr. Griffith in the late Twenties and Frank Whittle in the early Thirties influenced by the work which Parsons had already done on the aerodynamics of turbines and in fact on multi-stage, axial-flow compressors in the late nineteenth century and early twentieth century?

THE LECTURER: The steam turbines had axial-flow rotors. The steam turbine designers were designing their axial machines on the basis of flow through the passages between the blades, whereas Griffith was the first to propose that you should be considering flow past aerofoils. Frank Whittle was of exactly the same opinion. It was dissatisfaction on the part of Griffith and Whittle with the steam turbine theory which led to the much higher efficiency rotors which were ultimately developed.

PROFESSOR ARTHUR J. ELLISON, DSc(Eng), CEng (Department of Electrical and Electronic Engineering, The City University): May I give another, lighter, reminiscence? I was at RAE from 1943 to 1946 in the electrical engineering department. I shall never forget a lecture by Frank Whittle in about the spring of 1946. What impressed me was the tremendous courage of the mechanical engineers in simply standing around those early engines. Whittle said that when he shut down his engine in the BTH works it tinkled and rattled and he wondered how many blades had actually fallen off. Eventually, he said that the governor failed and the turbine ran away – and so did everybody else!

MR. PETER SPIRO: Allied to the metallurgy is the manufacturing technology of blades. We have had everything there from precision casting to new forms of profile grinding to (in later engines) spark erosion, using electroformed electrodes. Was the blade one of the limiting factors in the early engines or is it that I happen to know more about blades and nothing much about the rest? The tendency seems to be to need higher and higher temperatures and therefore new materials, because ultimately the jet engine must stop throwing so much hot air away, it must digest it.

THE LECTURER: I should like Mr. Bone and Dr. Feilden perhaps to confirm it, but I would say that the blades were for a time a limiting factor, but so was combustion. I confess to being not sufficiently up to date now to know whether there are any limiting factors left. There is continual refinement and I imagine continual improvement in blade materials.

MR. BONE: In that awful and protracted period when the W.2B was unable to pass a qualifying test at enough thrust to be allowed to be flown in the first version of the Meteor, I think that continued blade failure was the factor which caused more sleepless nights than anything else, except perhaps the problem of compressor surging. This was a very intractable problem. Whittle had taken some rather advanced steps in designing a form of compressor diffuser for the W.2B which was not similar to that which had been on the W.1 and this was very sensitive as regards its surge point. Those two things were the main troubles which set back the date on which the W.2B could develop its specified rated thrust.

THE CHAIRMAN: Very rarely, but occasionally, we still do have blade failures. I remember at the end of a runway not all that long ago somebody, looking at a smoking engine, saying 'It looks rather like a horse caught short; everything was coming out at the back end.'

MR. J. A. BAGLEY (Science Museum): I should like to mention the fact that in the Science Museum we not only have the Gloster E28/39; we have the W.1, the first flight engine, the first test engine, and thanks to the National Gas Turbine Establishment we recently acquired all surviving relics of the RE work at RAE Farnborough, including that Griffith model you

showed. It has always struck me as quite remarkable how quickly the gas turbine supplanted the large piston engine. At the end of the war large piston engines were being built by something like fifteen manufacturers. Within ten years none of them was building large piston engines: they were all building gas turbines. Would you like to comment any further on that revolution?

THE LECTURER: I don't think I can comment very intelligently. It is all a matter of performance. I remember doubts being cast on what the jet propulsion engine could do; people saying, 'Look at its fuel consumption, vastly greater than that of the corresponding piston engine.' Then somebody else had the intelligence to say, 'What you should be comparing is not the weight of the fuel necessary for a certain performance, but the weight of the fuel plus the weight of the engine for the given performance.' With the gas turbine being relatively light people began to see that even if it appeared to have a high fuel consumption it had in the end a greater economy. If you attempted to produce to-day a piston engine to give the same performance as a gas turbine, say, for an aeroplane to go at five hundred miles an hour, there just would be no competition. The speed of the changeover was just a realization by designers that in the gas turbine they had a superior prime mover.

THE CHAIRMAN: Piston engines were getting so tremendously complicated. An engine such as the Napier Sabre was so complicated, with umpteen spark plugs and a vast array of valves, that it was a menace to maintain.

THE LECTURER: Even the radial engine had begun to get terribly complex.

PROFESSOR J. L. STOLLERY (College of Aeronautics, Cranfield Institute of Technology): The development of the jet engine has been a great success story, but I wonder whether there are any morals that can be drawn from its development? For example, I am not at all clear how much collaboration there was between RAE and Frank Whittle or how much competition there was. The other thing that intrigues me is that Whittle seems to have been one of these people who, without knowing all the answers, tried something. I wonder whether that is a sensible policy to adopt to-day, or whether one should be more cautious?

THE LECTURER: I think I did make clear that there were the two design philosophies, with the RAE definitely struggling all the time to develop high efficiency axial compressors and Whittle going the whole hog with the complete engine. I always had sympathy with the Whittle approach. He certainly made it succeed. Whether you have to be a genius to make that philosophy succeed, I am not quite sure. He certainly was a genius, one of the great engineers of all time. Perhaps you have to be that to go the whole hog. Parsons had the same sort of philosophy when he astonished everybody in 1897 with the *Turbinia*. Whether to advise Professor Stollery to adopt this philosophy, I am a little uncertain, but the courage which Whittle had is an essential for engineering success.

PROFESSOR STOLLERY: I went to a meeting yesterday and there were two courses of action: to go on studying on paper or really to put some money down. So the same problem does crop up to-day. With the Harrier, there was a tremendous number of problems, but somebody took the decisions to go ahead.

THE LECTURER: I have every sympathy with that philosophy.

A MEMBER OF THE AUDIENCE: Lord Kings Norton, you commented on the development in materials for the turbine blades as being crucial. Was there a parallel development in production of the appropriate mixes of hydrocarbons to fuel it?

THE LECTURER: The fuel was never very special. It was straightforward paraffin.

MR. JOHN GALBRAITH (Economic Study Association): You touched very slightly on the finance of future projects, the development of gas turbine engines for a superior Concorde and the European fighter project. How would you like to see the project financed – by government, or privately, or by some combination of the two?

THE LECTURER: There are some projects simply too expensive for any but the largest organizations to attempt. A new engine for the Concorde type of aircraft, or a new aircraft of the Concorde type, just has to have launching aid from government. I see no other way of doing it. Perhaps if the City were extraordinarily enlightened and ready to wait thirty or forty years for its dividends, it might be persuaded to do it, but I don't think it will ever happen. One only hopes governments have enough imagination and faith to support attractive projects when they come.

MR. JAMES F. BARNES, CB (Deputy Controller, R & D Establishments & Research, Ministry of Defence): I have a comment which relates to the question Professor Stollery asked. Lord Kings Norton very rightly paid tribute to Frank Whittle's courage and drive. I think he also had a considerable amount of foresight. When he was working out his orginal design he had a very clear idea of how it might be applied: a slender aircraft, not necessarily to carry large numbers of passengers, but to carry high value cargoes such as mail.

The other thing I would want to remind you of stems also from Mr. Frost's question. Frank Whittle ran into problems with his turbines. He immediately went back to fundamentals. He examined the steam turbine textbooks and questioned the basis on which the design was made. It was from the questions he asked that the vortex theories for the design of turbines were derived. These theories were then of course great advances and gave greater efficiency. One must pay tribute to the fact that he was a man of great courage and foresight, with the great ability to go back to fundamentals.

THE LECTURER: That is a correct assessment.

THE CHAIRMAN: The sad thing is that Frank Whittle was not a team man when immersed in a large company, and so with all his great qualities he then faded from the scene. He will be remembered primarily for those first ten years.

THE LECTURER: He led his own team with great skill. Everybody was delighted to follow him.

WING-COMMANDER R. W. QUARTERMAINE, MSc, CEng, MRAeS, RAF: I think perhaps Lord Kings Norton has answered my question, namely that Frank Whittle was a genius, but he seemed to get these ideas at a very young age. Is there any lesson we can learn for the training of engineers these days?

THE LECTURER: I don't know. When we think we are spotting genius, we must always nurture it and foster it. His genius was spotted. He was a man who never asked for much for himself and he always felt and still feels that all his achievement is really due to the way he was brought up by the Royal Air Force. He has always said this most clearly – that it was the Royal Air Force spotting his ability when he was a young man which got him into Cranwell and sent him to Peterhouse. Through him, we see the Royal Air Force as a perceptive organization nurturing genius. I hope they are still doing it.

A MEMBER OF THE AUDIENCE: Our lecturer has been unduly modest about the part he played in ensuring that Whittle's genius was brought to full effect. I had the privilege of working briefly for Roxbee, as he then was, at the MAP. During the Rover to Rolls-Royce changeover it was his guidance and his analysis of how difficult it was to rationalize all the people involved which enabled the genius of Frank Whittle to bear fruit, and for Rolls-Royce to come in on the act and carry the flag.

THE CHAIRMAN: That is a good note on which to end this most interesting discussion. I remember that when I was at Cambridge I had a drawing-board alongside Frank Whittle's in the old Scroop House area in the Cambridge Engineering Laboratories. What an enthusiast he was all the way through, and, incidentally, a very good pilot. He performed at the Hendon Air Display in 1929 or 1930 in crazy flying in an Avro 504; which was about as far away as you could get from a jet aeroplane.

I looked up my old records the other day and found that almost exactly forty years ago Roxbee and I went up to Lutterworth to Power Jets (at the time I was Secretary of the War Cabinet Committee on Future Air Transport) and spent the day discussing how this curious device could be applied to the work of the Brabazon Committee, with which I was also involved. We discussed whether three of the Whittle engines could be put on a tailless De Havilland aeroplane for mail flying across the Atlantic. It was thought too dangerous to carry passengers at that time. From that came the D.H. 106, the Comet, with Frank Halford's engines in it.

Five years later, in July 1950, when I was Chief Executive of BEA I had the privilege of inviting Frank Whittle and George Edwards (Roxbee was in America at that time so I could not get hold of him) to come with us on the world's first commercial turbine-powered air service from Northolt to Le Bourget in that original little Viscount prototype twenty-three seater, with four of the very small early Dart engines. Of them, Lord Hives of Rolls-Royce, said, 'Don't you ask us for any more power. They are giving 900 propeller shaft horse power and they will not give one horse power more.' To-day the Dart is still in production, giving about 3,500 horse power. That was the Rolls-Royce conservatism of those days.

At the end of his lecture Lord Kings Norton touched on one of the great other by-products of the jet engine: not only the vast increase in power but also the enormous advance in realiability. We now have jet engines giving more than 50,000 lb thrust as against the 850 lb of the original engines, and they have been on the wing continuously, in service every day, for more than 10,000 flying hours (5 million flying miles) that is two years of flying, without having to be removed for overhaul or maintenance. This would never have happened with piston engines.

I am so glad that the part of Lord Kings Norton has been brought out so well in the discussion, because he was 'conducting the orchestra'. The orchestra may have been playing all sorts of different instruments but he made a good tune out of the whole thing. So, Roxbee, we are grateful to you not only for reminding us of so much that has been achieved in this historic development, but also for evoking many nostalgic memories which we have much enjoyed hearing again. A splendid first-hand account of one of the great pioneering achievements of modern times.

Extract from *A Wrack Behind*

Frank Whittle

Lord Kings Norton

Whittle is one of the great engineers of the century, perhaps the greatest if judged by the scale of the consequences of his invention. Only the discoverer of the properties of the silicon chip could claim comparable importance.

Jet propulsion was first demonstrated by Hero of Alexandria in AD60. The gas turbine was patented by Barber of Nuneaton in 1791. Whittle patented a design for an aircraft engine in 1930 in which propulsion was to be achieved by a jet produced by a gas turbine. By combining the thoughts of two men separated in time by over 1,700 years he made an engine which revolutionised world air transport.

I do not intend to go through the trials, tribulations and opposition which Whittle suffered in the development of his ideas. They have been described by a number of writers, including himself. He had a small and devoted team and I did all I could to help him, first as the director of special projects in the Ministry of Aircraft Production and later as the chairman of Power Jets Research and Development Limited, the company which the MAP under Sir Stafford Cripps formed when it bought Power Jets, the company which Whittle had formed with the help of J.C.B. Tinling, Rolf Williams and Lance White in 1935.

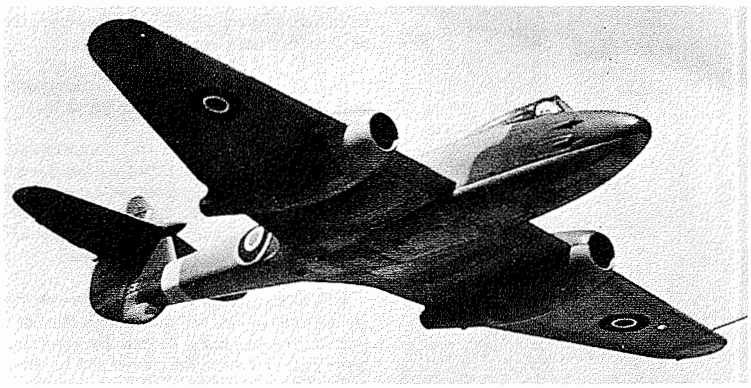
In September 1947 I received a letter from Mr P.H. Goffey, the Patents Branch civil servant working for the Royal Commission on Awards to Inventors, asking me to make an appraisal of Whittle's work for the information of the commission. I regard the letter which I wrote in reply as the most important letter I have ever written and although it has been quoted by Whittle and his biographers, I want to include it here because I am proud of it. I would not wish to alter a word. I knew I was writing about a great inventor, a great innovator, a visionary whose visions I was sure would be realised — a great man. It is a privilege to add — a friend of mine. Here it is:

"You have asked me for an appraisal of the work of Frank Whittle. It seems to me a little strange that in this year of Grace after so many people, including myself, have defined the major contribution to progress which Whittle has made, and after so many learned and professional bodies have given him their major awards, any appraisal is necessary. However, I will do my best to sum up the Whittle achievement once again.

"I think you are aware that jet propulsion is one of the most direct illustrations of a fundamental law of nature — Newton's Third Law — and has been recognised by man since at least 150 BC (I now believe this should be AD 60), when an engineer with the somewhat flamboyant name of Hero made proposals for its employment in an engine.

The gas turbine also is not new. The first design recognisable as a gas turbine was patented by John Barber in 1791. Thereafter there was only very slow progress in gas turbine technology until the present century. It is generally accepted that progress was delayed by the lack of material suitable for continuous operation at high temperatures with appreciable stress.

"Whittle's contribution was the association of jet propulsion and the gas turbine. Before him the gas turbine had been regarded, like other turbines, as a machine for supplying shaft power. Whittle recognised it as the ideal means of providing jet propulsion for aircraft.



Gloster Meteor.

His patent embodying this idea is dated January 1930. His subsequent difficulties in getting his idea taken up and exploited are too well known for recapitulation here. Finally, with the aid of two men whose contribution has been too often overlooked, J.C.B. Tinling and R.D. Williams, a jet propulsion gas turbine engine to Whittle's ideas was designed, constructed and, in 1937, tested.

It is one thing to have an idea. It is another to have the technical and executive ability to give it flesh. It is still another to have the tenacity of purpose to drive through to success unshaken in confidence, in the face of discouraging opposition. Whittle, whose name in the annals of engineering comes after those of Watt, Stephenson and Parsons only for reason of chronology or alphabetical order, had these things.

It is, I think, generally admitted that without Whittle's determination to turn his idea into reality we should not have begun to think about the jet-propelled aeroplane (whatever thoughts we might have slowly developed about the propeller gas turbine aeroplane) until the idea had been forced upon us by the exploits of the enemy with their jet propelled machines. In the closing stages of the war, however, we had in fact developed, on the basis of Whittle's efforts, a new fighting weapon in the form of the Meteor aircraft; now we are well on the way towards a jet-propelled air force, and the gas turbine engined civil air fleet is being developed.

It may be said that without Whittle the jet propulsion engine and the other applications of the gas turbine would have come just the same. They would. But they would have come much later. Whittle's work gave this country a technical lead in aircraft gas turbines of at least two years. Properly exploited and maintained, this lead should mean that this country can sell its aircraft gas turbines abroad for years to come. You know better than I do the extent to which Rolls-Royce and de Havilland's are producing income for the country. The first of these firms is achieving this with engines which are the direct descendants of the Whittle engine. The second is achieving its contribution with an engine which would not have been designed but for

the stimulus and information provided by the early Whittle successes.

The success which Whittle achieved reacted rapidly upon the work of others in the gas turbine field, notably the RAE team working in association with Metropolitan Vickers. They have been concentrating on a propeller engine with an axial compressor, in contrast to Whittle's jet propulsion engine with a centrifugal compressor. Whittle's work inspired them to swing over to axial compressor jet propulsion, and to that extent he is the stepfather, so to speak, of that class of engine, the first example of which in this country was the Metropolitan Vickers F2.

The notable success scored by the British aircraft engine industry in the aircraft gas turbine field, a success due in part directly to Whittle, and as for the rest, directly stimulated by him, naturally attracted attention in other fields. The successful application of the gas turbine in the difficult aeronautical field begat endeavour in the land and sea applications of the gas turbine. The knowledge gained through research and development for the aircraft application was largely applicable in these other directions. Here again, if the chance is grasped, there is a wonderful opportunity to build a great industry and, if our technical lead is properly exploited, one which will provide us with considerable national income. If and when this comes to pass, some of the credit will be Whittle's.

"So far the gas turbine in Great Britain has been generally regarded as a means of propulsion of fighting aircraft. I think posterity, looking at its achievements on land and sea as well as in the air, will see it rather as a great commercial asset — presuming that we today do our duty in exploiting it. They will see too that the initiative in its development came from aeronautical technologists, and at the head of these they will see Whittle."

Lord Kings Norton and the Air Registration Board, Part 1

Graham Cropper, CEng, FRAeS
Formerly head of aircraft projects, CAA

The association of Harold Roxbee Cox with the Air Registration Board began in 1938 (the year following its formation) with his appointment as its first chief technical officer — a post to which he was eminently suited by virtue of his experience and already formidable reputation in a wide range of technical aspects of aviation. Interrupted by the war, his service to the ARB was renewed first as consultant and member of the board, later (as Sir Harold Roxbee Cox) as its vice chairman and finally (as Lord Kings Norton) as its chairman.

In many ways, his special qualities were reflected in those of the organisation. His technical authority, based on an unparalleled range of experience, was respected at the highest levels in industry. This enabled him to lead the council through some of the most profound technical debates, through years of great innovation in civil aviation technology. The consequences of these debates and their conclusions were far-reaching, as the time of independent national safety policies was passing quickly. The principles established in this period had to stand the tests of time and of international scrutiny.

However, technical excellence was only one dimension of the contribution that Lord Kings Norton made in the history of the ARB. His unerring judgement and his persuasive (but always genial) skill in debate enabled him to steer the ARB through its transition from independent body to part of the much greater Civil Aviation Authority. In this process it retained, so far as was possible, the best features of the old organisation. In particular, the role of the old council in providing an effective instrument of self-regulation for the industry was continued in the 'new ARB' (the Airworthiness Requirements Board) which was given an influential role advisory to the CAA.

To those in the ARB who enjoyed the privilege of working with Lord Kings Norton he will be remembered as a colleague who, in spite of his awesome stature, would always receive with respect any genuine contribution to the problem in hand and would respond with kindly and constructive guidance. Such a combination of technical rigour and humanity epitomised the style — 'ethos' was the term he used — to which the ARB aspired and which, under the leadership of Lord Kings Norton and others of like mind, it admirably achieved.

Lord Kings Norton and the Air Registration Board, Part 2

John Chaplin, CBE, FEng, FRAeS
Formerly group director, safety services and board member, CAA

The Air Registration Board came into being as a result of the work of the Gorell Committee. It was incorporated in February 1937 and started work on April 1 of that year. The chairman of the board was Sir Maurice Denny, and there was a very small staff — reflecting the limited duties given to it initially. By early 1938 these had grown somewhat — so, for example, the board was authorised to recommend changes to regulations and requirements and it also started to investigate the design of prototype aircraft (cautiously, the Air Ministry limited it to investigations of aircraft weighing less than 10,000lb.).

In March 1938 Dr Harold Roxbee Cox was appointed to the post of chief technical officer and set about recruiting a team of technical assistants whose tasks included reporting to him on the airworthiness of the aircraft they were investigating so that he could make a recommendation to the Air Ministry that a Certificate of Airworthiness could be issued.

During the ensuing months the relationship between the ARB staff and the aircraft industry was developed, and staff were urged not to consider themselves as policemen. Rather they were there to help industry meet the necessary safety standard. Much of this attitude has persisted over the years, and carried over to the CAA.

War in September 1939 meant a cessation of many activities, and Dr Roxbee Cox went to Farnborough. He managed to find time to deliver a lecture to the Royal Aeronautical Society in 1940 on the future of civil aviation — a remarkable act of confidence at that time.

The ARB staff started to reassemble in 1943 but Dr Roxbee Cox was still engaged on work for the Government and relinquished his post as chief technical officer when peace returned. However, he did not sever his connections with the ARB. Instead, he became chairman of the engine requirements coordinating committee, which prepared the first ever turbine engine requirements which have had a great influence on the world standards for such engines. He was also a member of the Council's design and construction committee, acting as chairman from 1961 to 1965.

In 1955 he was appointed a member of the ARB council and became its chairman in 1966, an appointment which he held until 1972, when the duties of ARB were passed to CAA. On matters relating to airworthiness, however, the new body was charged with seeking the advice of another new body, the ARB! But now it was the Airworthiness Requirements Board, similarly structured to the old council, but with changed responsibilities. The similarity of initials was no coincidence, intended to reflect a continuity of purpose, and owed much to Lord Kings Norton, as he by then was.

During his membership as a board member and later chairman the foundations of modern airworthiness were well and truly laid. The issues of structural fatigue were addressed on the one hand and flying qualities and aircraft performance requirements were further developed. Lord Kings Norton was widely regarded as a brilliant chairman of the board with a decisive approach and positive at coming to a decision.

Extract from *A Wrack Behind*

The Air Registration Board

Lord Kings Norton

In 1936 the Air Registration Board was created by the government and given the task of recommending to the secretary of state for air the granting and renewal of certificates of airworthiness for certain classes of civil aircraft, a duty previously performed by the Air Ministry. It was understood that as time went on and the board gained experience certifying the airworthiness of more and more classes of civil aircraft would become the responsibility of the board.

The composition of the board was very interesting. There were four members representing the constructors of aircraft, four representing the operators of aircraft, four representing the insurers† of aircraft, four independent members representing the general public with another independent as chairman — seventeen in all.

The board began its work with a small staff dealing only with inspection of aircraft for renewal of certificates of airworthiness. In 1937, however, with the idea of qualifying for recommending the granting of certificates, it advertised for a chief technical officer. I applied for this post and, after being interviewed by the chairman and other members of the Board, I got it, and started working for the board in January 1938.

The chairman of the board was Maurice Denny (later Sir Maurice) and the secretary was T.R. Thomas. Both had experience of certifying seaworthiness, and the board had a number of distinguished members. From the constructors was Handley Page (later Sir Frederick); from the operators Nigel Norman (later Sir Nigel); from the insurers A.G. Lamplugh and from the independents J.T.C. Moore-Brabazon (later Lord Brabazon).

Greatly helped by Thomas, I quickly felt at home. At the head of the inspectors who, following marine practice were called surveyors, was Robert Hardingham, a friendly and experienced engineer who was destined, years later, to become a knight and chief officer of the board. I was the head of the technical side and started to recruit staff. The first I selected was a young man I had known at Farnborough, Walter Tye, and I could not have chosen better. He proved to be absolutely first class and ultimately became chief technical officer.

In 1938, with the object of getting to know the airworthiness people and structures experts in the United States, C.J. Stewart, who was superintendent of technical development at Farnborough, Pugsley and I went to America. These were critical days in Europe and my wife and Thomas telephoned me when I was but half way through my programme urging me to cancel the rest of it and come home. I refused, believing that there would be no war immediately — and I was right. While I was still in America, Neville Chamberlain went to Munich and returned believing there would be "peace in our time."

I was less than two years at the ARB. It seems that when I left the civil service to join the board, the Air Ministry and the board agreed that in the event of war I should immediately abandon the board and join the Air Ministry. So, at the beginning of September 1939, I found myself once again at Farnborough, this time as superintendent of scientific research.

†Two from the Tariff companies and two from Lloyds.

Lord Kings Norton, the creator and father of Cranfield University

Frank Hartley, FRAeS
Vice-Chancellor
Cranfield University

Lord Kings Norton's contribution to Cranfield University was immense. He was quite literally the father of the university, having personally conceived the idea of creating what became the university. His involvement began in early 1943 when, as vice president of the Royal Aeronautical Society, he began to consider how best to create an environment in which to educate the practical young men and women who would be needed to take the aeronautical industry forward in the post-war years. Being barely a year old at this time, I personally find it hard to comprehend the degree of optimism reflected in even considering such issues, for although the Battle of Britain had been won, the Battle of the Atlantic was far from won and D-Day was still a year and a half away. But optimism, realistic achievable optimism, was a hallmark of Lord Kings Norton.

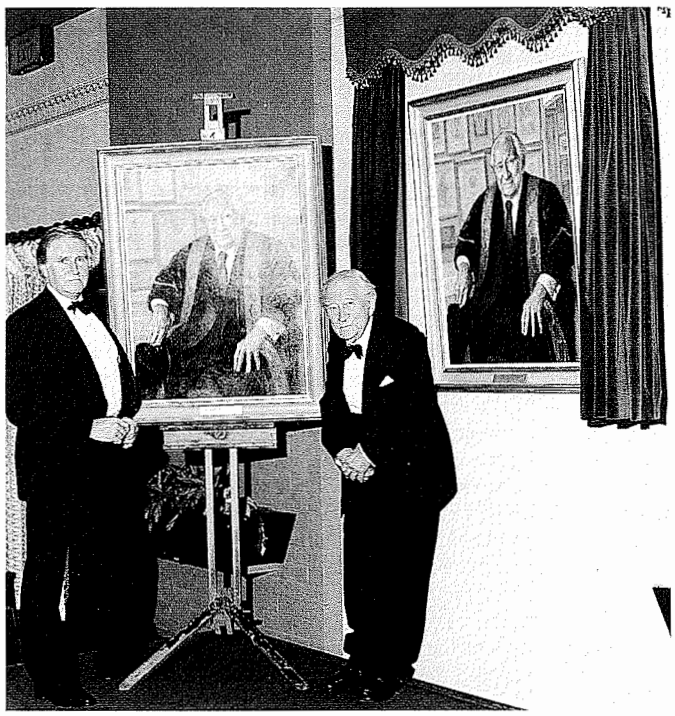
Dr Harold Roxbee Cox chaired two one-day meetings on 25 June and 23 July 1943. Within a month the Aeronautical Research Council chaired by Professor Sir Melville Jones produced a report proposing the setting up of a post-graduate aeronautical school, the College of Aeronautics. Roxbee Cox's influence on that report shines through. It had vision. It was radical, not more of the same but radically different to fit a new set of circumstances: post-graduate and practical. It was produced with a sense of urgency in under a month: no one could ever criticise Roxbee Cox for making haste slowly.

The original report and the succeeding Fedden Report were much criticised by the existing universities and by the aeronautical industry, neither of which saw the need for a college devoted to aeronautical engineering. Nevertheless, Roxbee Cox and his colleagues battled on to bring their vision to fruition, achieving success by getting the determined support of Sir Stafford Cripps, whose portrait appropriately looks benignly down upon me, even as I write.

On Wednesday 28 June 1945, Roxbee and his fellow governors of the College of Aeronautics had their first meeting under the chairmanship of Air Chief Marshal Sir Edgar Ludlow-Hewitt, appointing Professor Ernest Relf, a distinguished aerodynamicist, as the first principal.

The College of Aeronautics grew steadily until 1957 when the most infamous defence white paper was published. It foresaw the air defence of the United Kingdom requiring only unmanned missiles and hence the support defence technology had given to civil aviation in all the leading aerospace countries of the world would cease. Students evaporated as employment prospects plummeted. While the full implications of this disaster were still being digested Sir Frederick Handley Page, then chairman of the Governors, died. In August 1962, Sir Harold Roxbee Cox took over. In 1963 he persuaded Lord Robbins to recommend a degree-granting charter, which was vigorously opposed by the other universities and rejected by the government.

But saying no to what Roxbee passionately believed in has not always proved to be a final decision, and so it proved. In December 1969, under the name Cranfield Institute of Technology, the College finally achieved its Royal degree-granting charter. Roxbee, now Lord Kings Norton, became the university's first chancellor, bringing in Dr Henry Chilver as its second vice-chancellor in January 1970. So commenced a major period of steady expansion, culminating in today's modern university, the present title being graciously bestowed by Her Majesty The Queen on 1 September 1993.



To mark his retirement as chancellor, Cranfield University commissioned a portrait by Michael Noakes (left), a replica of which was presented to Lord Kings Norton.

Throughout its development Roxbee Cox's hand was always close to the tiller. He initiated the concept, was a governor for the first 17 years, chairman of the governors for the next seven and chancellor for a further 28 years. In all those years he missed only one court meeting and presented the degrees at every single graduation ceremony. He chaired his last court meeting on 12 March 1997 and formally said goodbye on 15 October 1997 when he told us he was "planning to keep an eye on us and shortly to look down on us." I am sure he is. I hope he is still as proud of us, as we are of him.

How should the university recall its first chancellor?

I suggest:

"He was a complete gentleman, a noble Lord, a mathematician, an engineer, a man of vision, a clear thinker, a brilliant communicator, a leader. Our privilege was to have known him. My privilege was to have worked with him."

Extract from *A Wrack Behind*

Cranfield

Lord Kings Norton

In 1943, with the world still at war, a great discussion on the future of aeronautical education was held by the Royal Aeronautical Society. Not only would the war years, however many were still to come, demand more well-qualified aeronautical engineers, but the longed for peace years, with engineers turning swords into ploughshares, would want more. The discussion was in two parts. One took place on 25 June and the other on 23 July. Many of the leading figures in British aeronautics took part and in the chair on both occasions was Dr Roxbee Cox, a vice-president of the society. The discussion culminated in a resolution based on a proposal by Marcus Langley. That resolution and the discussion which led to it resulted in the recommendation by the Aeronautical Research Committee that a post-graduate college of aeronautical science should be established. This was followed by governmental action. Sir Stafford Cripps, then the minister responsible for aircraft production, set up a committee presided over by Sir Roy Fedden to make specific proposals, and the committee recommended in its 1944 report that such a college should be a new and independent establishment. In 1945 the government created the College of Aeronautics board of governors under the chairmanship of Air Chief Marshal Sir Edgar Ludlow-Hewitt to bring the college into existence and govern it. The first meeting of this board took place on 28 June 1945 and there were present:
Sir Edgar Ludlow Hewitt, Dr W. Abbot, Mr Hugh Burroughs, Sir Roy Fedden, Mr J. Ferguson, Sir Harold Hartley, Sir William Hildred, Sir Melvill Jones, Dr E.B. Moullin, Mr J.D. North, Sir Frederick Handley Page, Mr E.F. Relf, Dr H. Roxbee Cox, Air Marshal Sir Ralph Sovley, Rear Admiral S.H. Troubridge and Mr W.E.P. Ward. Sir William Stanier, who had been appointed, was not present.

The major tasks of the board were, of course, to find a site for the college and to secure the staff and first students. These tasks they achieved in a remarkably short time. They found Cranfield, an RAF station with its splendid buildings, first on loan and later as the college's permanent home. They collected a splendid staff and the college opened at the beginning of October 1946. The board had chosen one of its own members, Ernest Relf, as principal. He was the senior member of the aerodynamic staff at the National Physical Laboratory and as well as being a distinguished academic was a first class pianist and organist. He retired in August 1951 and was succeeded by Air Marshal Sir Victor Goddard in the following month. At that time Sir Frederick Handley Page was chairman of the governing body, having taken over from Sir Edgar in June 1953. He died in 1962 and I became chairman.

Although the college continued to grow Sir Victor's style of leadership was not appreciated by some members of staff and, unfortunately, he did not enjoy the confidence of the chairman. I found myself more in sympathy with Sir Frederick than with Sir Victor. In the end, Sir Victor retired in July 1954 and in his place we selected, from an excellent short list of candidates, Alfred Murphy, professor of metallurgy at the University of Birmingham. He proved to be a first rate principal, with a gift for gentle leadership.

1957 was a very important year for the College of Aeronautics. The Air Minister, Duncan Sandys, issued a white paper which foresaw a dismal future for British aviation. The role of the RAF was to be seriously diminished with air defence dependent on missiles. This was the signal for diversification of which the college had a little experience with a work study department. The result was adventure into new areas — mechanical engineering, structures, automobile engineering, administration, information technology, biology — and expansion on a very satisfying scale. It rapidly became apparent that the college, initially devoted solely to aeronautics, was assuming the character of a post-graduate university with a standard consistent with the power to award higher degrees. The college appealed to the Queen to make Cranfield a university of this kind and to this she agreed, making the Royal award at the end of 1969. I became the first chancellor and Murphy the first vice chancellor.

Murphy, who had ruled since October 1955, was unhappily due for retirement. Consequently he held the post for only a few weeks. Early in 1970, Henry Chilver assumed the vice-chancellorship and, on the firm basis Murphy had constructed, began a brilliant vice-chancellorship which in a few years gave Cranfield a world reputation with a spectrum of activities ranging from the practical realities of commercial administration at one end to academic technological researches at the other.

Among Henry Chilver's achievements, two stand out. One was the merger with what had begun life as the National College of Agricultural Engineering and is now the large and important Silsoe college of the university, dealing with agricultural and soil survey work here and abroad, and the other was the bringing of the academic side of the Royal Military College of Science at Shrivenham into Cranfield University in August 1984. This was the joint achievement of Chilver (now Lord Chilver) and Major General Sir Richard Vincent (now Field Marshal Lord Vincent). Both these great establishments have flourished as parts of the Cranfield administration and continue to do so under the direction of Professor Frank Hartley, who was chosen to lead Cranfield from a brilliant short list of candidates for the vice chancellorship when Henry Chilver retired in 1989.

There have always been stars in the Cranfield staff firmament. I must be careful about mentioning names in such a distinguished company but I feel I must recall as outstanding in the initial staff of 1945, W.J. Duncan and Robert Lickley (became Sir Robert Lickley). The former I worked with on the wing flutter problem, and the latter, once a student of mine at Imperial College, became one of the country's leading aircraft designers. In the middle period, so to speak, I always think of David Keith-Lucas and John Stollery, both professors in the College of Aeronautics, and today Frank Hartley and his notable colleagues Leo Murray, Ron Fletcher and Peter Hancock.

With all the amazing story of Cranfield with which I have been associated, from the pre-natal discussions of 1943, over which I presided, to the court meeting of 1997, at the end of which I retired from the chancellorship, I have never had any executive responsibility. However, I have presumably have had some relatively benign influence. I say this because there is in Mitchell Hall a room called the Lord Kings Norton room. In the room is a bust of Lord Kings Norton. And as I write these words the great portrait painter Michael Noakes is finishing a portrait of me, in my chancellor's robes, destined for display in the university. There is also a Kings Norton Lecture in the College of Aeronautics and the great new building by Sir Norman Foster is to be called the Kings Norton Library. By these marks of appreciation I feel very very honoured. Cranfield has been a part of my life for over half a century and I shall, I hope and expect, continue to be in touch with it still until I shuffle off this mortal coil.

SEVENTH HANDLEY PAGE MEMORIAL LECTURE

Incorporating

The Second Century Papers: Looking Ahead in Aeronautics—20

LOOKING FORWARD AGAIN

Notes on the Needs, Probabilities and Possibilities in Aeronautics

HAROLD ROXBEE COX, LORD KINGS NORTON, FRAeS

It is, and always will be, an honour to give the Handley Page Memorial Lecture, but for me the occasion has a special pietas because Sir Frederick Handley Page was a friend of mine for many years and I admired his extraordinary intelligence, pertinacity and independence of character. I had the honour to immediately follow him in two offices of high importance—as President of the RAeS and as Chairman of the Governors of this College, and as this is the last major lecture I intend to give, I am particularly happy that it is in his memory, just as the first major lecture I ever gave was in memory of that other great pioneer, Wilbur Wright.

In October of last year I was asked, almost simultaneously, to provide a final and summary paper for the Second Century papers in the *Aeronautical Journal* and to give the 1969 Handley Page Memorial Lecture. I did not feel equal to the two tasks, but after it had been suggested that they should be rolled into one—that the Memorial Lecture should be the last Second Century paper —I finally agreed, on this basis, to write a paper.

In 1940 I had the honour of giving the 28th Wilbur Wright Memorial Lecture[1]. It was called "Looking Forward", and it was about the future of British civil aviation. In his paper introducing the Royal Aeronautical Society's Second Century Papers, Mr. Allen[3] was kind enough to include it among the prophetic writings he examined.

"Looking Forward" was produced under difficult conditions. Had I not been afflicted in the spring of 1940 with German measles, I do not think it could possibly have been written. I could not, moreover, consider in my predictions the influence of the gas turbine, with the development of which I was already closely associated, because the work of Whittle and his friends was necessarily shrouded in secrecy.

I do not mention these facts to excuse the paper, but to explain first how time could be found to create such a mass of verbiage in the midst of war, and second, to explain what might at first seem a curious omission.

I delivered the paper almost exactly 29 years ago, but the occasion is clear in my mind. The Society's President, Mr. Griffith Brewer, was in the United States with his old friend Orville Wright, and in the chair was that wonderful character, Brab, at that time still Lt. Col. J. T. C. Moore-Brabazon. London was under attack, but the lecture was held in the great hall of the Institution of Electrical Engineers. The possibility of a less attractive venue was, however, in the Chairman's mind, for he announced the determination of the Society to continue the Wilbur Wright lectures in the war "even if it became necessary to hold them, like the early Christians, underground in catacombs".[2]

Lecture given at the College of Aeronautics, Cranfield, on 6th June 1969.

But it is not only the abnormal conditions surrounding the composition and delivery of that paper which impel me to refer to it this evening. In addition, it is remarkable for its implied underestimate of subsequent progress and for its wholly misplaced dependence on logical argument.

I was, of course, too cunning to attach a time-scale to my predictions of developments to come. But the projections of aircraft speed and size and the ideas of frequency of transatlantic flight now look extraordinarily jejune. Development has taken place far more rapidly than I, in that paper, appeared to think possible, and I cannot recall that any of the friends who read the paper in draft or those who spoke about it immediately after it was published had any criticism to offer on my ideas for the future.

Indeed, I think one would have had to be clairvoyant to have foreseen the rate at which not only aeronautics, but space travel, nuclear energy and electronics, have been developed since the war. And one matter I want to examine in this paper is the feasibility of the rate of progress we have witnessed continuing.

I have already indicated that my old paper depended too much on logic. The prophet who deduces the future by logical extrapolation from the past and his knowledge of the present is doomed to failure. It is difficult to suggest what other process except clairvoyance is available to him, but he would do better to consult the entrails of the appropriate fowl. I, quite logically, I think, saw a future for the flying-boat. But the rapid development of the large military aircraft, essentially a land-based vehicle, effectively stifled the development of the civil flying-boat: civil air transport was destined to be run by landplanes.

Aeronautical progress since the end of the war has certainly been extraordinarily rapid and we are apt to assume that this extraordinary rate of progress will continue. But we are already conscious of a change in the character of the progress and this change, in fact, may change the rate. If progress is thought to consist of steps upward, then the steps are getting higher and the pause before each is longer. Putting it differently, the quanta are becoming bigger and fewer and the gaps between them greater.

Size is today best measured in prototype capital cost. Quite apart from bigger jobs taking longer, decisions to commission them are taking longer to reach. This is not unreasonable. When a budget can be spread over a variety of projects, successes may cancel out the failures. But when a great proportion of the whole budget is concentrated in one project, it is reasonable to expect a long period of enquiry, planning, design of alternatives, costing and the multitudinous other considerations on which success depends, before deciding to proceed. Twenty years ago it would have seemed incredible that the USA should consider their supersonic transport project for six years and still not reach a decision, but this indeed has happened. And when these great and lonely projects are

organised internationally, when even only two nations collaborate in one of them, delays are naturally longer still.

There is, too, another reason why decision is difficult and often delayed. There was a time when the future could be considered in compartments. There was indeed in the 1920's an admirable series of works among which I recall the titles of two—*Daedalus,* or *the Future of Flying* and *Lars Porsena,* or the *Future of Swearing.* Each area of activity—not, of course, only these two—could be considered in reasonable isolation. But now aeronautics is so intimate a part of the pattern of life, changes in it affect the pattern, and changes elsewhere in the pattern, affect *it.* The decisive improvements in UK railway communications following the electrification developments have reacted on our internal air services. Developments in, and foreseen in, overhead railway systems will influence airport siting[21]. We must indeed think not of air transport but of the whole field of communications, and our aim, sometimes conscious but generally subconscious, must be to make the overall system the best for the majority of people, insofar as politics, international jealousies, wars and rumours of wars will allow.

It is shocking to think how great is the influence on civil aeronautical progress, and indeed all civil progress, of what is euphemistically called "defence". I have already given an example of this influence from the last great war, and it would be childish to believe such influence was at an end. Indeed, governmental financial support today for technological progress in the name of defence is greater than governmental financial support for civil technological development.

Paradoxically, war, despite the appalling waste it means, is the great technological accelerator. Without the great wars of this century, our technological condition would be far less advanced. If we succeed in preventing another great war we shall have succeeded in hindering the rate of technological progress. It may be objected that the races for the moon and the planets, with all the enormous volume of technological advances associated with them, are phenomena of peace. But they are really part of the aftermath of war and continuing enmity.

In this general consideration of the anatomy of progress, we have to remember that lines of development, like seams of coal, can be exhausted. The development of the railway steam engine is a case in point. It reached a stage when not only was technical innnovation less and less frequent in its history, but it became an anachronism. There is perhaps development still to come in railway electrification, but even there one is conscious of approaching a horizontal asymptote, an asymptote which the conventional ocean-going ship has evidently almost reached. In the manufacture of ships there is probably a great deal to be done, but we cannot look for much improvement in the performance of conventional vessels, despite the influences of new propulsive systems based on the gas turbine and nuclear energy. Much of their work has been taken by aircraft, and the future may see some of the rest taken by hovercraft, hydrofoils and submarines.[14] Even the rotorcraft may not, on the time-scale of aeronautics' second century, have much further to go, perhaps not much further than half-way. It could develop as an admirable commuting system: it has great possibilities of improvement in its control and operation: it might grow to accommodate 200 people: it could be—it is—a great weight lifter, a specially mobile crane. But in most visions of the more distant future, it has a relatively humble place[16].

I have already suggested that logical progress in civil transport is unlikely and have mentioned as disturbing influences on it war and preparation for war. If, as we all hope and pray, an era of peace descends upon the world in the Royal Aeronautical Society's Second Century, however, the main influences on technological progress will be deliberate planning, scientific discovery and economics. Scientific discovery is partly chance and partly planning, and I feel that if its influence is wrong in kind or in tempo the fault will be ours, the scientists' and engineers'. But to be sure that economics is operating fairly in its influence on progress is much more difficult. The costing and hence the comparison of the variety of solutions to communications problems is going to be intricate and I believe it is not too soon to begin the deep study of these matters. I will indicate the character of my doubts about our present economic assessments by two examples.

It is sometimes necesssary to compare the costs of aeroplane and helicopter operations. In the former, the cost of providing the runways essential to operation is indeed generally partially reflected in the cost evaluation, but in evaluating the helicopter operation the helicopter is never given credit for the fact that the runway is, so to speak, built into the machine. With the advent of the civil VTO aircraft this matter becomes one of urgency.

My other example relates to the use of foreign equipment in government contracts. Insofar as decisions are made on cost comparison, then, except in times of full employment, when capacity thrown up in one activity theoretically can be absorbed by another, the estimated cost of home produced things should be reduced in the overall cost evaluation by the tax to be paid to the Exchequer by the staff concerned with their manufacture. This has never been done, but clearly it should be.

In both these examples, the error arises because of our habit of compartmenting. Costs in one government department apparently cannot be offset by receipts in another. This is surely absurd. In the future it is essential that our cost evaluation, inside and outside government, becomes more refined, and if we are to evaluate not only aircraft but whole communications sytems accurately, there are complex problems for solution by our accountants and fresh ideas to be developed by our economists, and unless they and we collaborate, serious errors will be made.

There is another aspect of the economics of aircraft production which is much more the concern of the engineers and scientists than the accountants. I refer to the economic use of material. I have long been shocked by the production of high performance structural parts by machining away most of the material supplied. If the material is one with a high scrap value perhaps the situation is not as bad as it looks at first, but the correct evaluation of alternative processes, and the development of potentially economic forming processes, is obviously a matter of great importance for the future[9, 17].

We are in a curious position at present in which, if medical science is left out of account, we could do without the discovery of new facts of science for quite a long time while we were applying to technological advance what we already know and while we are getting our economics straight. Flat statements of this kind always on examination need qualification and, in this case, the filling up of gaps in knowledge as an essential part of the pattern of development, of course, would continue to be necessary. But the assertion is broadly true, and while any attempt to convert it into a policy would be quite disastrous, it does at least suggest that a better distribution of effort might result from more deliberate planning than we now achieve.

Basic scientific discovery cannot be planned. Great and influential discoveries like that of electro-magnetic induc-

tion in the last century and the character of the atomic nucleus in this are always possible and when they occur they make nonsense of extrapolation; but they do not remove the need for planning. If we are to have any control of the technological future we must plan much more effectively than we have in the past.

When I say "we", I mean, for two reasons, we here in the UK. I am not a xenophobe—far from it. But the world is not ready for global planning and even if Europe were ready for European planning, which manifestly it is not, we should still, in my view, need to have a national plan. It can be alleged that we have always planned, and the allegation needs examining. First, there is not much doubt that under the threat of war and in war, we did get down to the job of planning, and, having made plans, executed them with reasonable despatch. Our planning in peace, however, has been lamentably defective and depressingly so in aeronautics. The urgency of war unifies action. Party politics temporarily disappear. And it is politics that is the ruin of plans in peace.

In these remarks I am concerned with the future, but when one considers the string of bad decisions of the past in aeronautics, some of them stemming from the party in power changing, some from sheer lack of understanding by politicians of the issues at stake, we realise the need for a system which, in what should be the uncontroversial field of technological progress, would provide a coherent plan to which we could adhere[23].

Inevitably the national economy will affect plans, but a non-political continuing authority would be more likely to slow up or accelerate a programme than to initiate and cancel in the fantastically uneconomic way we have so often seen in the past, insofar as we have been able to see through our tears.

The planning of progress in communications or, if that seems to be too big a step all at once, of our progress in aeronautics, could in my view be the job of an authority independent of party, which did not *advise* Ministers, but was executive in its development of prototype systems and controlled only by its budget allocation. Because communications, and aeronautics in particular, are inevitably the concern of defence, the defence ministries would need to be associated with this essentially civil authority, if only to ensure no unplanned duplication of research and development effort and no unnecessary duplication of facilities. But the authority would itself be the chosen instrument for prototype communications development.

I put forward this idea of a new authority at a time when the proposal of the Edwards Committee for a Civil Aviation Authority concerned with safety and licencing is still new[24]. I make no comment in this paper on an interesting proposal which at the time of writing is still under examination. And I do not suggest that the new prototype communications planning authority I have adumbrated should be added to it or put alongside it. But I should like the same consideration to be given to its merits and demerits as the Government, the Edwards Committee and the interested parties gave and are giving to the pros and cons of a central safety authority and the adminstrative alternatives to it.

One can scarcely make the point too often that the development of aeronautics in the next century depends on the development of the overall pattern of communications, on the development of other technologies and skills, as do these also depend on the development in aeronautics. This interdependence is basic and emphasises on the one hand the desirability of a single continuing non-political communications authority and, on the other, the overwhelming difficulty of predicting what is going to happen. The development of the video telephone, the possibility of conference between individuals in offices scattered over the world connected by such a device, must clearly make the need to travel less. One can indeed visualise, as E. M. Forster did in a story many years ago[25], a world in which individuals are normally static in cells connected only by super communications, individuals to whom physical travel has once again become an adventure. But while all this makes the unwisdom of prediction manifest, it does not make the discussion of possibilities and probabilities uninteresting or indeed valueless. Indeed, did we think so, the whole series of papers of which this lecture is the last were misconceived.

The possibilities are indeed thrilling. It is difficult to see the limit of what could be done. One chain of possibilities flows from consideration of developments in structures. There is indeed a limit to what can be achieved, defined by our old friend the square-cube law[1,9] and the use of the stoutest materials with the most economical set of physical properties developing their theoretical strengths. Had we stuck to the configuration of the subsonic transport, then with current materials we could confidently predict the economic aeroplane of maximum size. The square cube law is a law of nature and, as the poet[26] said:—

> "However great your strength or stature,
> You can't repeal the laws of nature."

But, just as in the case of the law of income tax, there are ways round. One way, permitted—in fact, dictated—by the aerodynamicists is to change the shape[4]. Aspect ratio does not loom so large as it used to. The other, of course, is to develop materials with greater strengths and stiffnesses and with the optimum combination, from the structural engineers' point of view, of these and other properties. Metals, as at present produced, are so far from their theoretically possible strengths, that alloys with twice the strengths now achieved are possible. Indeed, in the metallic field the alloys of steel and aluminium which have so long been the basis of aircraft construction have still a long course to run and the titanium alloys a still longer one[17].

But just as in the development of the gas turbine whole new fields of performance were opened up by the development of the Nimonic alloys, so do we begin, with the discovery of the carbon filament, to perceive the structural potentials of non-metallic materials. At present carbon is restricted because, to make it usable in anything but tension, it needs to be embedded in a matrix, and its own splendid characteristic of increasing in strength with increasing temperature is nullified by the more normal behaviour of the matrix resin. These difficulties will undoubtedly be overcome, perhaps by the creation of gigantic diamonds, but probably less dramatically.

In fact, it is more likely that ways will be found to exploit the extraordinary properties of the finedrawn wire, the filament and, most refined of these forms, the whisker. These have strengths and elastic moduli significantly greater than the materials they are made of have in their more conventional forms. It is remarkable that some fine whiskers have already been shown to have strengths near to the theoretical strength of the inter atomic bonds, so we are approaching the ultimate possibility in these cases. The potential that such achievements indicate is indeed prodigious[17].

But less dramatic properties than strength are of the greatest importance and we must hope for progress in two directions in particular. One is the improvement of fatigue properties of materials, the other is improvement in forming properties in such ways that the other desirable characteristics are not impaired.

In the case of fatigue failure prevention, the most obvious course is to decrease the ratio of working stress to ultimate stress in a vital area by increasing the latter, and the continual achievement of higher and higher strength materials makes this a hopeful route. Nevertheless, one still hopes for a more fundamental solution of the fatigue problem.

In the case of improved forming properties and hence the avoidance of uneconomic machinery and scrap creation, there is hope in the metallic field in the phenomenon of super plasticity and, in the non-metallic field, in mouldable materials such as glass. Moudability could be influential in the elimination of joints and the avoidance of stress concentration, all of which is of increasing importance as desirable materials become more brittle[9, 17].

Altogether one feels that the aircraft designer has ahead of him an immense choice of exploitable advances in material properties, properties which will enable him to reduce structure weight. How quickly these properties will be available to him, however. is quite another matter, and dependent upon the way government—or an independent authority—allocates its research and development finances and resources.

One way in which a considerable saving in structure weight could be made immediately, is by the elimination of windows, and it can be argued that, as in many large aeroplanes, many of the passengers cannot see out of them, and, further, that those who can are offered a depressing monotony of scene, they would not be missed. Whether the claustrophobes would be consoled by strategically disposed television screens showing what is going on outside is a matter which should be investigated. The other possibility is to develop moulded glass as the structural material for fuselages.

The development of materials and its effect on lightweight structural design gives greater flexibility to the designer in deciding upon aerodynamic configurations. He has, we are told, a whole spectrum of major types of aircraft within which to work[4]. It stretches from the infrared of the classical aircraft to which we have grown accustomed, through the swept-wing aircraft to the slender-wing machine typified by the Concorde, to the hypersonic aircraft in the ultra-violet, the machine which rides on the shock wave it creates. With the development of the VTO slender wing aircraft and the possible advent of the wave-rider, new design problems loom large. One in particular is the choice of fuel for propulsion[11]. Today we use the fuel which gives the greatest calorific value per litre, kerosene. If we feel we need the greatest calorific value per kilogramme, however, we turn to hydrogen. We are then confounded by the fact that its calorific value per litre is only one quarter of that kerosene. Aeroplanes as we know them are embarrassed by bulky fuels. However, the wave-rider may not be. It is a little early to predict, but if it is not, then it may be able to exploit the immense calorific value per kilogramme of hydrogen, nearly three times that of kerosene. If this can be done in connection with some form of boundary-layer propulsion, we shall have a vehicle, the shape of which I cannot at present visualise, capable of reaching the antipodes in a couple of hours.

But the most fantastic of our visualisations of the possible future in air travel pale into the commonplace beside the possibilities in space travel[7]. The materials and some of the fuels which I have just mentioned in their mundane context have their parts to play in the spatial context. But much more dramatic in their gigantic propulsive powers are nuclear devices depending for energy production preferably on nuclear fusion to nuclear fission. The development of earth orbiting satellites as military weapons is a regrettable present and future condition: their advantages for observational purposes, even if not used for offensive ones, is difficult for mankind in the present state of its great knowledge and great unwisdom to resist. But one can in the future see communications satellites developing from vehicles conveying programmes between nations to travelling broadcasting stations with programmes of their own, a possibility for propaganda alarming in its potential. This is only one of the manifold possible developments ahead but it alone is a compelling reason for this country to play a major part in space developments if we are not to be swamped by alien ideologies.

We can readily visualise the communications satellite of today developing in such ways that the need for business travel is reduced, the weather satellite developing from a data gatherer to a vehicle for prediction. Certainly satellites will increasingly be used as aids for navigation and traffic control for ships and aircraft[7].

Whether we need visualise the space vehicle as a means of travel, except for purposes of exploration, in the next 100 years, is doubtful. If, as Mr. Cleaver and Mr. Thwaites think, in AD 2066 there will be people living on the Moon, on Mars, in orbiting space stations and even on Jupiter and Saturn, then a certain amount of physical interplanetary transport will be required[6, 7]. Even then, however, I feel it will be regarded as something of an adventure. It will presumably have meant, however, the achievement of the non-expendable vehicle capable of landing and taking off from both the earth and the other planet, and the extrapolation into space of terrestrial law[20].

It is in this latest and most fascinating and amazing field of human effort that prediction, prophecy, speculation —call it what you will—is most tempting. It is in this field too that the influence of some fundamental scientific discovery still utterly hidden from us, and leading perhaps to a completely unthought of source of energy, is likeliest to be. Conceivably, some understanding of the nature of gravity may develop, possibly from discovery in a scientist's laboratory, possibly from a discovery in the less conventional atmosphere of a séance.

Possibilities for the next 100 years we can discuss, probabilities not. Probabilities for the next 20 years are hard enough to assess, but this year's moon and Venus achievements suggests that we should be cautious indeed as prophets if we did not see a landing on Mars in the 1980's.

However far our thought can reach, however bizarre our conceptions of supersonic aircraft, of wave-riders, of space vehicles, however refined the computers and their programs, we visualise man in control. Man may delegate more and more to machines he has designed as the vehicles of delegation, but, unless he can delegate foresight, prediction, and the handling of the unpredictable, there appears to be a limit to the degree of his delegation. Moreover, the complexity of the machines over which he must still exercise his dominion will increase. and he has the problem of directing them through the functioning of his own physical and neural composition. We usually think of his having available for this control his senses of seeing and hearing, as the direct means of receiving essential information. No-one has so far suggested his senses of taste and smell for this purpose. But there are possibilities in his tactile sense and in the use of his sense of acquiring information about movement and attitude from the balancing faculty in his head[13]. It appears that man is capable of using these senses in more and more refined ways: that he may, for example, learn to use his parafoveal vision, given the right kind of signals for the purpose, in addition to his narrow direct

vision, which requires him to scan. He may be assisted by three-dimensional displays projected in the line of his narrow forward vision. There are, too, the possibility of more precise use of hearing, through technological advance in the two-way reconstruction of original speech, the possibility of other languages, like the language of music, and which convey accurate information. There is the possibility of training and aiding the ears to advance beyond their already extraordinary direction finding abilities[13]. And if the extended abilities of vision and hearing are not enough, there is the possibility of developing for control the tactile sense, which the blind reader of Braille has demonstrated is capable of great refinement in perception.

For the exercise of control, as distinct from the receiving of data, man may possibly develop far beyond the wheel turning, button pressing phase he is now in. He is capable, as the players of musical instruments show, of translating a specialised language by great manual and oral dexterities, and faculties of this kind might be used for control. Man may, at the centre of an elaborate control system, influence action by small movements of his head relative to his body. In fact, for the refined control of elaborate apparatus, there may be almost unimaginable possibilities yet to be discovered in the elaborate neural and muscular complex which each of us is.

We have to remember, however, that this complex mechanism has so far operated in a 1*g* environment. If we are in the next decades to adventure into extra-terrestrial regions, we shall have to adapt our physiology to different gravitational environments, and the man-machine partnership will be varied accordingly[13].

The fantastic speculations possible about man's control of his machines and man's control of man relate, of course, to the whole social scene, of which aeronautics is but a small part. With its immense influence in physical communications, however, it is an important part, and we in aeronautics must realise that we are in the beginnings of a new phase in evolution, a phase in which man himself is beginning to influence significantly his own development[13]. Natural selection is no longer in control. We have a responsibility for the future which is terrifying in its magnitude, and we must pray for a wisdom commensurate with our knowledge and its incredible proliferation.

It is hidden from us still whether in the century ahead we shall still see man sitting in the midst of the vehicle he controls, controlling it by refined apparatus in intimate connection with his own anatomy, or whether we shall see the control exercised from afar by a computer, in communication with the vehicle, controlled by a program man has schemed. The rate at which the computer will develop in its manipulation of our affairs is determinable only by a computer operating to a program of incredible refinement and comprehensibility. But the increasing influence of the computer not only in aeronautics but in our lives is ineluctable. It is probably the most pervasive influence to which man has ever been subjected: it has indeed a potential universality beyond that of fire, of the wheel, of printing[6]. The possibilities indeed are so outrageous that it is a comfort to restrict speculation to aeronautics and think merely of computers controlling aeronautical transport, even though we extend its scope as far as the moon.

The computer will be capable of almost any process which possesses a structure describable by the human intellect[6], and consequently we shall see it, in the coming years, more and more the instrument of aircraft design, more and more the instrument not only of calculation and record but of discovery by the logical integration of its store of knowledge. Design by drawing on paper will be with us for quite a while, I expect, but lines on paper as a process for storing knowledge is already *vieux jeu*. Journalists who for so long have loved to talk of blueprints for the future, unconscious that the blueprint went out of existence before the war, will at last have to talk of tapes for the future. But with the development of advanced systems of visual display, of instructions to the computer by the spoken as well as the typed word, we can visualise the design team of the future as highly trained engineers in intimate dialogue with powerful computers[19], a vision a little disconcerting to those who believe that the great designer is a supreme artist. Will the great designer of the future still be an artist? Will there still be room for gracefulness and imagination to manifest themselves through this incredible machine? We must hope so. But while in the course of considering my material for this paper, I have imagined many things I have never imagined before, I have not succeeded in imagining the characteristics of the Sydney Camm of 2066.

He was, in every sense of the word, an individual, individual in the way he absorbed knowledge, individual in the way he used it. Will education, with the increasing use of televised and other visual programmes[18], computer controlled courses, and computer stores in place of human memory, impose such a pattern as to cause to proliferate the standard man? It is an awful thought. If we are all to be alike, I hope we shall all be tolerant, polite, kind and well spoken. Present trends suggest that this is unlikely and mankind needs to make a conscious effort to arrest his *descensus averno*. This is not the place to discuss social behaviour except in one aspect—language. If we are, as I have said, orally to instruct computers, if we are to have dialogues with computers, it is advisable that both ourselves and the computers use an accurate and compact language. Perhaps it should be Hebrew, which is such a language, but if it is to be English then for goodness sake let it be terse. Let us rid ourselves of the cumbrous Transatlantic neologisms which are invading our language. Drop "assignment", reinstate "job". Cut out "elevator" and "apartment" and restore "lift" and "flat". Let us "start" and not "commence". Let us cross out "currently" wherever we find it—the sense will be unchanged. Believe it or not, I heard an American voice in a space programme say, "Are you in visual contact with the earth?" He meant, presumably, "Can you see the earth". And in an estimable British technical journal I read the other day "A VDR offers aircraft designers the capability of varying (such and such)." Surely "A VDR allows designers to vary (such and such)". I believe we must arrest this tendency to sloppy verbosity and that early in the second aeronautical century we must reinstate English in educational curricula or we are going to have some damned longwinded computers talking to us later on.

I have made the point already that we shall be wise to think of the development of communications rather than of aeronautics by itself, and one of the most remarkable influences in recent years on the communications pattern is the air cushion vehicle, recently by law called the hovercraft[14]. The genus to which such craft belong is the surface effect vehicle—machines which are sustained in travelling across a surface because of their proximity to it. This sustentation could be achieved by means of an air cushion, positive or negative in pressure, through electromagnetic repulsion or just conceivably by other means designed to defeat gravity. So far, the only practical vehicles have been of the air cushion type and, the first great inventive step having been taken (and as recently as a decade ago), there is little difficulty for the most conventional imagination visualising tremendous possibilities, particularly in the fields of tracked and marine vehicles.

The extrapolation of current technology already makes possible marine craft of 750 to 1000 tons with top speeds of, say, 75 kt, conveniently amphibious for docking purposes. Such craft would be propelled by airscrews like the 170 tons SRN4. Beyond 1000 tons, however, this kind of propulsion becomes difficult and the large hovercraft of the future, 10 000 tons in weight, 500 ft long and 150 ft wide, will probably be driven by water jets. These, and the hydrofoil stabilisers which will probably be needed, demand underwater appendages which, from the viewpoint of the purist, seems a pity. Such a step does, however, effect an interesting hybrid craft, bringing together two competing technologies, the air cushion vehicle technology and the hydrofoil ship technology. If this is to happen, the synergy will need to be effected with great skill and recollections of the Great Eastern with its screws, its paddles and its sails. But perhaps it is too soon to assume that the marine hovercraft will not develop systems of stabilisation and propulsion of its own which do not require the help of the turbulent fluid below it.

The hovercraft has been visualised too as a vehicle which might help in the agricultural field, for example, for sowing paddy fields[5]. It is not likely, however, to oust the aeroplane from the position it has so rapidly developed in recent years in less specialised sowing, in spreading fertilisers, in pest control. In the world of agriculture there is not much doubt the aeroplane has an important part to play, particularly in developing farming of the great lands of the agriculturally less developed countries. But exactly what the aeroplane or helicopter or hovercraft does in future depends not only on its own abilities but on the progress in the rich field of agricultural chemicals and the combined operation could well develop wonderfully to the benefit of mankind, bringing prosperity and plenty where there is now poverty and misery[5].

And with the fantastic developments we see as possible do we see ourselves travelling as safely as today? I think we do. Take-off and landing are the most critical phases of air travel, and to achieve them vertically is potentially safer than charging along and approaching miles of concrete runway however refined our control becomes[22]. Furthermore, the development of quality and reliability techniques and the control of quality and reliability exhibited by the achievements of the Americans and Russians in space give considerable confidence in the safety of the high performance travel of the second century.

But as we peer into the mists of the future, first in this direction, then in that, we who are of a certain age see little to console us. The world we have, with its million faults, seems not so bad after all. Should we not be wiser to slow things up a bit? If so, who is to be bold enough to take the first step back. It is as big a responsibility as creating the atomic bomb. But if we never discover another fact, the vast inertia of advancing technology will take us a long way down the roads we have been contemplating to a greater uniformity. Probably, the only thing around us that we shall not succeed in changing is the weather, though we may get better at using it to our advantage and pleasure more efficiently[10], and better at predicting it, avoiding it and taking off, flying and landing through it[8]. Some of us may come to regard it with affection as the only thing we cannot control.

Envoi

Joseph Conrad and Ford Madox Hueffer wrote *Romance*[27]. Somewhere near the beginning occur the words—

> ". . . romance. It lies a little distance before us and a little distance behind . . . One discovers that one has passed through it just as one passed what is today our horizon. One looks back and says 'Why, there it is'. One looks forward and says the same. It lies either in the old days when we used to, or in the new days when we shall . . ."

When I look back on the great things I was privileged to take part in—the building of R101, the creation of the jet engine—the scenes are illuminated with an heroic glow. I have always felt the romance of achievement and I think my contemporaries in aeronautics feel the same. But when I look forward, does the horizon glow with the same glory? I am not sure; the old eyes grow dim. But if it does not, however affluent we become, the future is going to be pretty grim.

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Extract from *A Wrack Behind*

Myself, when old

Lord Kings Norton

I am not quite sure when I became old, but I think I must date it in my seventies when I retired from a variety of activities and settled into a more sedentary style of life. I retired from Dowty Rotol and Bergers (and so Hoechst UK) in 1975, from the Royal Institution in 1976, from Ricardo's and British Printing Corporation in 1977 and sold Withers Estates, very profitably for the Withers family, in 1981.

I was left with the chairmanships of two tiny companies, Landspeed and Cotswold Research. While Landspeed has not served its purpose, Cotswold Research has its place in history. Both companies are concerned with the inventions of Professor Eric Laithwaite, who demonstrated in model and prototype form tracked hovercraft powered by the linear motor. Landspeed failed to develop this, through lack of funds, to a commercially viable transport system, though the Japanese appear to have done so. Cotswold Research, however, made a success of an adaptation of the linear motors to the separation of aluminium from scrap material and for their work, which ended in profitable industrial apparatus and a big saving in manpower, Mr A.P. Bird and I received the Prince of Wales Award in 1986.

Apart from this, however, I seem to have had time for an active social life with many clever and agreeable friends, time to help historians and geographers of the world of aviation and time to develop and enjoy my aeronautical collection, time to look back on a long life and to reminisce about it to my family and intimates. In all this I have had the help of two marvellous women — my late wife, Marjorie Withers, to whom I was happily married for 53 years (she died in 1980) and my present wife Joan Pack who is my unfailing support in my old age. She is a wonderful cook. On my 90th birthday, she organised and cooked for a great party here. We had about 50 guests entertained in a marquee on the lawn. For my 95th birthday she gave me a garden fountain and my two sons were the hosts at a splendid luncheon in the Seymour House Hotel, next door.



Sadly I have given the addresses at the memorial services of old friends, notably J.D. North, Sir Roy Fedden and David Keith-Lucas. And I had the great honour of reading the first lesson at the memorial service in Westminster Abbey for Frank Whittle. I chose the verses from Proverbs 3: 13-20 and 4: 5-7, extolling wisdom and understanding of which I am sure Frank would have approved.

I am, on the whole, satisfied with what I have done as an engineer, applied scientist, and industrialist. But I am delighted with a few trivial achievements outside the areas in which I was professionally qualified. One I have confessed to already in these memoirs, my adventure into pure mathematics marked by my two papers[1] on the theory of frameworks published in the journal of the London Mathematical Society. The other which I cannot resist mentioning is my facility in versification, mostly for the enjoyment of my family, my colleagues and myself.

Extract from *A Wrack Behind*

A resumé

Lord Kings Norton

When I look back, as I occasionally do, on the pattern of my long life, I see it dominated by three great projects — the design and construction of the airship R101, the development of the jet propulsion gas turbine, and Cranfield University. These are, so to speak, the high hills in a plain of diversified scenery.

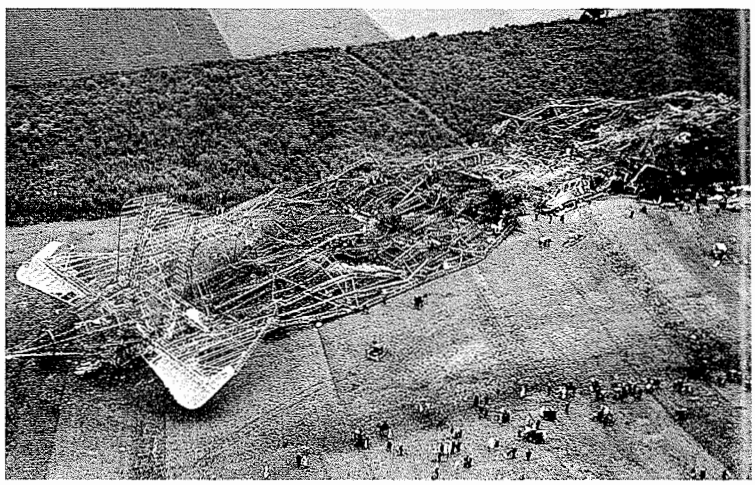
Of these great projects, I have included essays on R101 and on Cranfield, but I have confined my reference to the jet propulsion story to my opinion, long ago first expressed, of a great man, Frank Whittle. I have not expressed myself at any length on the controversies which mar the Whittle story and those who are interested — and who is not? — should read Whittle's own book *Jet*[14] and Mr Folley's biography. Of the two men who I believe had the greatest influence on my career, Sir Henry Tizard and Sir Harry Jephcott, I will have something to say later on.

Of my schooldays I have written a little, whether the masters at Kings Norton School were typical of their time I have no means of knowing, but they were certainly extremely good teachers. Hale introduced me coherently to mathematics, Morris grammatically to French, and Weaver to the joys of English literature. I think I knew something of Shakespeare before I knew Weaver, but he it was who introduced me to the pleasures of Malory and the fine lines of Milton. The headmaster was a chemist, and I have no reason to believe not a good one, but he failed to enthuse me and to present his speciality in a way acceptable to my logical mathematical mind.

In 1918 I joined the Austin Motor Co at Longbridge and I found all the designers and draughtsmen in the aircraft design office helpful. After the war ended, one or two of the original aircraft team who had been on military service returned, and I found myself working side by side, on the designs of the Whippet and the Kestrel, with Joe Smith, who became my friend and who in later years joined the Supermarine team under the great R.J. Mitchell. Smith made a major contribution to the design of the Spitfire and succeeded as leader of the team when Mitchell died.

When the post-war recession in aircraft production of the early twenties developed, Austin abandoned aircraft and I went to work in the shops and in my spare time worked for a London University external degree in engineering. This I got, with first class honours, in 1922, and after two years post-graduate work at Imperial College, of which I have written, I went to the Royal Airship Works, Cardington.

My years at the Royal Airship Works were full of interest and hard work. I feel I contributed a great deal to the mammoth task we who worked there had and I feel I learned a lot too, particularly of the theory of structures which we (Baker, Pugsley, Hilda Lyons and I) expanded in new and valuable ways. Before the great ship flew I was transferred in 1929 to the Royal Aircraft Establishment at Farnborough and appointed to the airworthiness department. I was put to work with a remarkably able scientist, H.G. Howard. In those days, with the biplane type of aeroplane still dominant, the calculation of the stresses in its wing structure was one of the major tasks in its design. This was done by perfectly satisfactory, established methods but Howard devised a most elegant polar diagram method of representing and calculating the shear loads and bending moments which



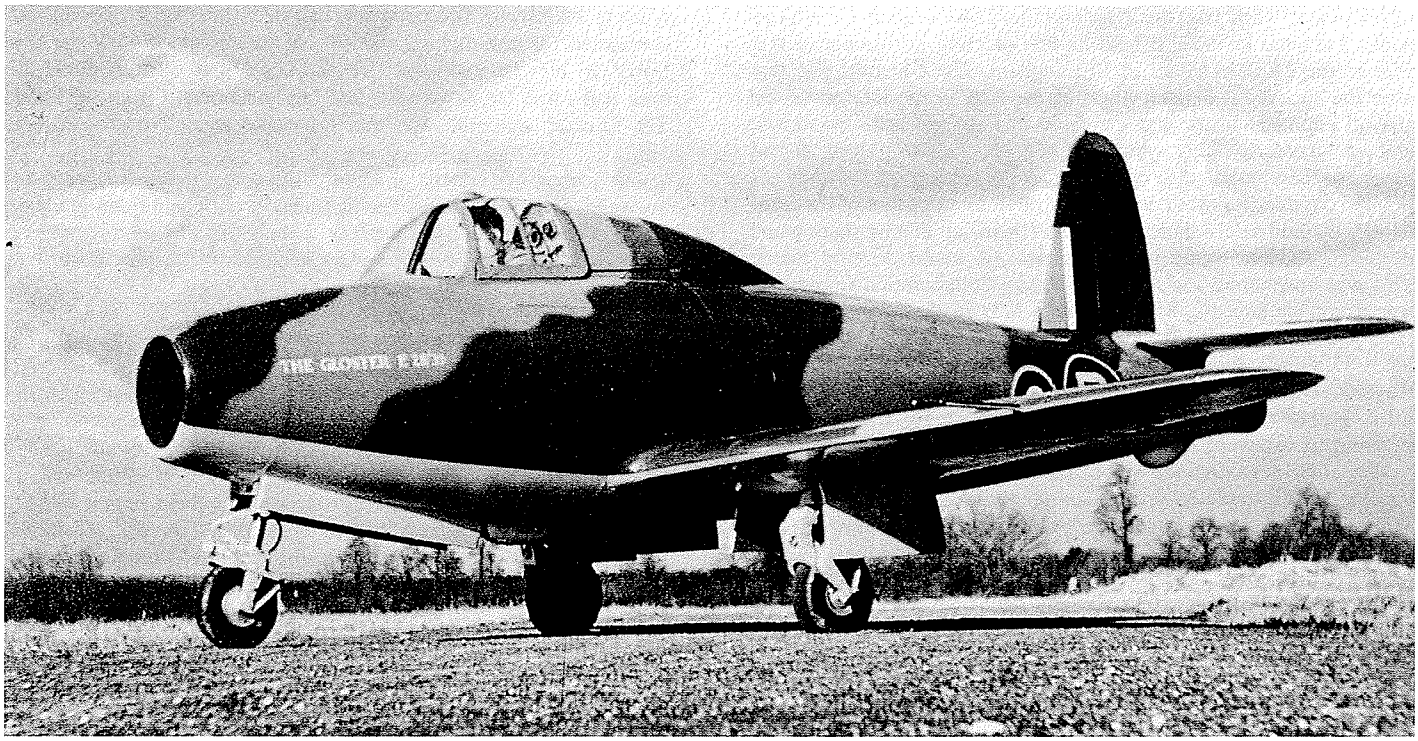
R101 crash scene.

the wing structure had to withstand. He was, after a year or two of our working together, transferred to an administrative post in the Air Ministry headquarters and I lost sight of him. I became acquainted too with another remarkable man, Kelvin Tallent Spencer, and we soon became friends for the rest of his long life.

My wife, our son Christopher and I were living, in between houses, in the Ridgemont Hotel in Farnborough, when in October 1930 came the appalling news of the R101 disaster with the deaths of all but one of the senior members of the Cardington team. Only five people survived. I do not intend to discuss here the possible causes of this awful accident, which has been the subject of not only an official enquiry but of much public and private discussion. No satisfactory explanation has, however, been offered and the best interested parties can do — and there are still many — is to read Sir Peter Masefield's *To Ride the Storm; the Story of the Airship R101*[15]. He marshals the facts and the speculations but he does not provide the solution. For me, the result was to be transferred back to Cardington in charge of the technical staff, with Sidney Nixon, the one senior figure who did not embark on the fatal voyage, as director of the establishment. In one of the sheds we had R100 and at first our task was to prepare it for further trials. But this rather pathetic *interregnum* was cut short by a government economy committee whose recommendation to scrap the whole outfit was in fact carried out. This was the sad end of R100 and of a great adventure.

As for me I was, of course, moved back to the airworthiness department at Farnborough, and I was soon involved in work of intense interest but of less romantic content.

In the 1930s aeroplanes, both biplanes and monoplanes, suffered from structural failures due to what was called wing flutter. The vulnerable craft, as speed increased, developed oscillations of the fixed and movable surfaces which in some cases caused structural failure and crashes. This phenomenon was investigated at the National Physical Laboratory by R.A. Fraser and W.J. Duncan and I extended



Gloster E28/39.

their pioneer work into the design field. The cures for wing failures were the mass balancing of ailerons and the achievement of certain degrees of flexural and torsional stiffness in the wing structure expressed by the so-called stiffness criteria, which I defined algebraically. The application of my recommendations correctly did indeed eliminate this form of failure.

The other notable job I did was to discover and remedy the cause of a series of accidents to the de Havilland Puss Moth. In 14 crashes, 13 were fatal and the pilot in the other had no recollection of what happened. The fault lay in the structural design above the pilot's cockpit and was eliminated.

My concern with the integrity of structures led me into basic investigations into the theory of frameworks and resulted in two essays in what must be described as pure mathematics and which I am proud to record were published in the journal of the London Mathematical Society. My other papers were in applied mathematics directed to the wing flutter problem and the calculation of wing stiffness, several in collaboration with A.G. Pugsley. It was in these areas that my lecture before the Royal Aeronautical Society in 1935 gained the Edward Busk Memorial Prize.

More and more, however, research and development became focused on preparation for war and in 1936. I became the head of a new department at the RAE called the air defence department. I found myself engaged with a first class staff on the design problems of our kite balloon barrage and the penetration of a similar enemy barrage. That is where James Martin's cable cutter played such an important part. I have dealt with his work in this area elsewhere, but I shall never forget the first very tentative full scale experiment in which I flew with Squadron Leader (later the Rev Air Chief Marshal Sir) Peter Frazer in the two-seater all-metal Northrop aircraft which flew into light wires supported by bunches of toy balloons. This could have been developed into an aerial game.

It was while my team was engaged in these quite useful exercises that two vastly more important projects were under way. One was the Whittle jet propulsion gas turbine, struggling against the odds at Lutterworth. The other was the radio detection system, ultimately called radar, the brainchild of Watson Watt being developed at Orfordness.

All these technical advances were under the eye of Henry Tizard. He was undoubtedly deeply concerned with Watson Watt's work, ably assisted by A.P. Rowe at Air Ministry headquarters. However, though he supported my work at Farnborough and Whittle's early struggles, he had very little influence on their progress.

It was at the end of 1937 that I jumped back into the field of aircraft airworthiness when I applied for and got the job of chief technical officer of the newly created Air Registration Board, leaving the civil service but with an agreement between the Air Ministry and the board that if war was declared I was to be immediately available to the government. I had in fact the whole of 1938 and, until war was declared on 3 September 1939, was in the service of the ARB. I was in the USA conferring with the American airworthiness authorities when Neville Chamberlain did his extraordinary deal with Hitler, deferring the opening of hostilities for a year.

The day after war was declared I was back in the service of the government and I spent the war years first at Farnborough, then at Harrogate and afterwards in the Ministry of Aircraft Production, getting more and more involved in the jet propulsion gas turbine saga with my headquarters office in Thames House South. My first minister was, as I have recorded already, Lord Beaverbrook. He became minister of supply and his place was taken by Colonel Moore Brabazon, with whom, through the Royal Aeronautical Society, I was already acquainted. He was a splendid man to work for but his career in the Ministry of Aircraft Production was cut short when he made an indiscreet remark in a post prandial speech at a public function. He was removed from office and given a barony with the splendid title of Lord Brabazon of Tara.

Certainly, my war period was dominated by my work furthering the gas turbine programme. In this I was helped greatly by my deputy, Group Captain Watt, and in other senior posts the test pilot Group Captain Willy Wilson and the scientist O.A. Saunders (later Sir Owen Saunders), destined for higher things in the hierarchy of Imperial College. My personal assistant was Maureen Price, a splendid manager of my official affairs who married Philip Lawrence, a leading member of my staff. Their daughter is my godchild and Maureen is still a dear friend.

I suppose that the most effective instrument in my jet propulsion

development work was the Gas Turbine Collaboration Committee which I created in 1941. It held its first meeting in October of that year in the Midland Hotel in Birmingham. The situation was that after the successful maiden flight of the E28/39 the interest of the aircraft engine industry was increased and several firms wanted to embrace the technology which Whittle had created. They all had ideas but they hesitated to share them with Whittle or each other. I pointed out that if a firm patented any obvious extension of the technology it could safely share it with the others and that by sharing and discussing advances the whole technology would develop faster. This was understood and the Gas Turbine Collaboration Committee was a notable success. Meetings were held at the premises of the collaborators who were Power Jets, Metropolitan Vickers, de Havillands (Halford) Ricardo, Bristol, Armstrong-Siddeley and Rolls-Royce, the last of which took the greatest advantage of the arrangement and in the end benefited most. Surprisingly the committee remained in existence for three years after the end of the war.

The other development which remains foremost in my memory was the liaison in the jet propulsion gas turbine field with the USA. When it was evident that the Americans would ultimately come into the war, and we were already planning aircraft production with them, American personnel were already here, both civilian engineers and military personnel in civilian clothing. My own chief contact was Colonel Lyon, a most agreeable man who became a General but unhappily died soon after. His assistant and successor was Colonel Keson, another man it was a pleasure to deal with and who, incidentally, in the discussion following my lecture in Washington in December 1945, paid a tribute to the Gas Turbine Collaboration Committee and the advantages it offered to progress.

After the war of course there were all sorts of organisational changes. The one that concerned me most was the amalgamation of Power Jets (Research and Development) and the National Gas Turbine Establishment at Pyestock. The result was a powerful R&D establishment, with excellent staff, of which I was the director and Hayne Constant my deputy. A great deal of excellent work was done there and great advances made in the fields of compressor design and combustion.

Then, in 1948, came a big and, at the time, not entirely welcome change in my career. I became, largely I believe because of the influence of Tizard, chief scientist of the Ministry of Fuel and Power. At the time, the Minister was Hugh Gaitskell and the parliamentary secretary was Alf Robbins, two politicians it was a pleasure to work for. The Permanent Secretary was Sir Donald Ferguson and a most influential and unforgettable young man named Leslie Murphy (afterwards Sir Leslie Murphy) was the minister's private secretary. I suppose the basic notion of my appointment was to bring the attitudes and experience which war had developed in the aircraft world into the fuel and power area which, by comparison, had somewhat languished. I believe I succeeded in introducing a new outlook and effort. Perhaps I overdid it, but with the first class team I collected in which the remarkable and imaginative Canadian Campbell Secord was prominent it was difficult, though conceivably premature, to test the feasibility of underground combustion of coal, the burning of peat (in a gas turbine!) and the use of the methane in the ventilation of mines.

Although in the Fuel and Power hierarchy my post was at deputy secretary level, I concluded that being a scientist, even though an applied scientist and not a pure one, I should never achieve a permanent secretary post. I therefore decided to leave the civil service.

I became a director of Wilmot Breedon, suppliers of bumpers and other accessories to the motor industry, consultant to Rolls-Royce and a member of the Council for Scientific and Industrial Research, the body which at that time determined the policy of the department with the corresponding name (DSIR). The chairman of the council at that time was Sir Harry Jephcott, chairman of the great Glaxo company. In 1961 I succeeded him in the chair. Before that in 1957, however, undoubtedly through his influence, I had become a director of The Metal Box Company, chaired by an outstanding and formidable character, Sir Robert Barlow. He had formed the company by amalgamating a number of companies in the can making, packaging and printing industries. The company had been notably successful and dominated the can-making industry of this country. However, Sir Harry (by now created a baronet) and one or two others thought the time had come for new blood and less dictatorship from the chair. The ultimate outcome was that I became executive chairman in 1961.

The managing director of Metal Box at this time and during all my years as chairman was David Ducat, a tall Cambridge graduate who was a first rate administrator. I think he was more respected than loved. He had been a worthy lieutenant to Robert Barlow in building and developing Metal Box.

The large overseas business of the company, with factories in India, Israel, Italy, South Africa, Rhodesia, Pakistan, Bangladesh, the Caribbean, Singapore, Malaya and close relationships with companies in Germany, Holland, France and Scandinavia was under the command of David Brough, a genial and well liked figure who needed little, if any, direction from above. Others high in the administration in the Baker Street headquarters were Alex Page (later chairman), Derek Huffam, the secretary McAllum and the finance experts Sutton and Candson. All these were directors and there were in addition Sir Harry Jephcott and a successful addition I brought on to the board from the world of higher education, Lord Murray of Newhaven.

I greatly enjoyed my years with Metal Box, especially my contacts with overseas organisations on tours abroad. Under the rules we had laid down for directors I retired in 1967 at the age of 65 but was immediately invited by the directors of Berger Jenson and Nicholson (largely I think at the insistence of their bankers, Schroders), to become chairman of the board which was split into two factions and had somehow to be composed again as a harmonious whole. This daunting task I accepted and in the end succeeded, though not without a great deal of unpleasantness, details of which I do not intend to record. I will refer, however, to a great personality with whom I managed to disagree and still remain friends. This was Mrs Vera Hue-Williams.

In 1941, our flat in the Cromwell Road having lost windows in a bombing raid, we decided the time had come to leave London. Incredibly we managed to let our flat and, through social contacts, rented an attractive house in the grounds of a large property belonging to Mr Lilley of the then well-known shoe firm of Lilley & Skinner. We became friendly with Mr Lilley and his charming wife and got to know his son Tom Lilley, who was married to Vera, a lady of means from, I think, Rumania. At the time of my taking on the chairmanship of Berger Jenson & Nicholson, Tom Lilley had died and his widow, thereby even better off, had married Roger Hue-Williams and Mr & Mrs Hue Williams were directors on the troubled Berger board. They were indeed leading members of the two opposing factions. When we once more had a Berger board concerned with policy and management instead of a power struggle the company became desirable in the eyes of other organisations with similar interests. This in the end resulted in Bergers being acquired by the great German chemical company Hoechst — one result was that Vera Hue Williams retired from the board richer than ever — and wholeheartedly devoting to be a distinguished owner of racehorses, and my association with her was restricted to a rare meeting in the Royal enclosure at Ascot.

Another result was that while remaining chairman of Bergers I became a director of Hoechst UK, a subsidiary of the main Hoechst organisation in Frankfort. The chairman of Hoechst UK at that time was Dr Gustav Bunge whom I came to like immensely. It is a long time since we both retired from Hoechst affairs but we still exchange Christmas cards and I like to remember that we worked harmoniously together.

My retirement from the Berger-Hoechst business was part of my general retirement from commercial affairs which I have remarked on elsewhere in these memoirs. I was in my middle seventies and felt that I should enjoy a less strenuous life, and that is what I proceeded to do.

Address at St Margaret's, Westminster, The Lord Kings Norton

Sir Peter Masefield, CEng, Hon FRAeS

We, his family and friends, are gathered here today in the heart of London to remember and to give thanks for the life and work of one of the truly great men of our time — the Lord Kings Norton of Wotton Underwood, 'Roxbee' to the legion of his friends and colleagues.

Through the many years of his active life, there were hardly any avenues of the aeronautical sciences to which he had not made (and enjoyed the making of) inspired contributions.

He was, indeed, "A Happy Warrior".

If the question was to be asked — "What, among all Lord Kings Norton's many achievements, were those most to be acclaimed?", the answer, among such a galaxy, could, perhaps, be four.

All of them will continue, widely, to bestow their benign influences for years to come:

Of those four, the first is his key role in advancing Frank Whittle's inspired concept of jet propulsion, so that it has become "the international reality" it is today in both military aviation and for civil air transport.

Second: his establishment through the Air Registration Board in 1938 of a series of sound, comprehensive and practical airworthiness requirements for all British (and many foreign) civil aircraft. They remain the firm basis of civil aircraft certification and operation to this day.

Third: his detailed precepts for the design and construction of safe and secure modern aircraft structures, free from their earlier (brief but deadly) aeronautical disease of wing–aileron flutter.

Fourth: his pivotal role, and leadership, for 54 years in the creation, at Cranfield, of a world-class university, a centre of excellence and of advanced post-graduate education.

Any of those four would be acclaimed as a crowning achievement for the whole of a man's life work.

For Roxbee, each one of them was indeed, to him, an episode of separate and intensive interest and satisfaction, successively reinforced by other major attainments to which he brought his special qualities of understanding and expertise.

But perhaps most of all Roxbee was revered and acknowledged by all who knew him as "One of the nicest of men one could ever hope to encounter."

He inspired devotion, not only for his intellectual qualities but also for his kindliness, his courtesy and his integrity, all delightfully interlaced with erudition and wit.

All the while he never held back from difficult decisions, nor from his meticulous regard for others as a considerate and warm-hearted human being.

This is not the occasion to innumerate the details of his many attainments throughout the more than 80 years of his active life. They included such valued worthy bodies as the Campden and Chorley Wood Food Research Association, of which Roxbee had been proud to be president since 1961. That, alongside the much larger Metal Box Company and the Bird Group of Companies, which to Roxbee's great pleasure won the Prince of Wales Award for Innovation. They, together with a number more besides, are represented here today, as Roxbee would have valued so much.

The remarkable diversity of his good works extended from his earliest days at Kings Norton Grammar School, to those years in Whitehall when he made such extensive contributions to national welfare. They began with his engineering apprenticeship at the Austin Motor — and Aviation — Company in 1918, and went on through nearly 40 years to the time when he was the chief scientist and 'forward thinker' at one of the chief post-war ministries.

Fortunately, for posterity, there exists today a compendium of some of Roxbee's outstanding creative works (set out modestly in his own clear and trenchant words) presented in the form of a series of five classic lectures.

They were delivered at intervals in the course of 33 years as he progressed from one major involvement to the next, and presented before meetings of the Royal Aeronautical Society, the Royal Institution, the Royal Society of Arts and the American Institute of the Aeronautical Sciences.

Together they constitute a unique record.

Now, the highlights are gathered into one volume, to form a tribute to a lifetime of unparalleled versatility and technical prowess.

All of this came to a fulfilment, to Roxbee's special satisfaction, with the successful creation at Cranfield, from tentative beginnings in 1943, of, first, a post-graduate 'College of Aeronautics'. In due course, it became an Institute of Technology, then a post-graduate university with powers to award higher degrees and finally, and triumphantly (to Roxbee's crowning delight) the establishment, under Her Majesty's Royal Charter, of the broadly-based Cranfield University.

All through it has been served by a brilliant, hand-picked staff, headed from 1969 by Lord Kings Norton, who was (for almost 30 years) its chancellor. In Roxbee's words, Cranfield has gone forward "with many scintilating stars in its firmanent."

Let me end with some words from Roxbee himself, written just a short time ago:

He said:

"I confess in my old age that I am on the whole satisfied with what I have done as an engineer, applied scientist and industrialist. But, in addition, I am delighted with one or two trivial achievements, outside the area in which I was professionally qualified.

"One was my adventures into pure mathematics, marked by my two papers on the 'Theory of Frameworks'. The other, which I cannot resist mentioning, is my slight facility into versification, mostly for the enjoyment of my family, my colleagues and myself.

"In my retirement, my home in Gloucestershire is to me charming. And there I seem to have had time for an active social life with my close and agreeable friends, time to develop and enjoy my aeronautical collection, on television to listen to The Antiques Road Show and to the delightful As Time Goes By, and to follow the adventures of those legendary creations of my friend, P.G. Wodehouse in the (I suppose, equally legendary) *Blandings Castle*, which I sometimes seek in leisurely local explorations, driving my elegant Bristol car.

"In all of this I have had, in my life, the help of two marvellous women, my late wife Marjory Withers, to whom I was happily married for 53 years (she died in 1980), and my dear present wife, Joan

Pack, who is my unfailing support in my old age. She is indeed a perfect companion and hostess and, with all that, a wonderful cook for small parties and large.

"For my 95th birthday she gave me a garden fountain, while my two sons were the kind hosts of a splendid luncheon."

Thus Roxbee.

As his friend and colleague Professor John Stollery has written:

"During his 27 years as Cranfield's revered chancellor, Lord Kings Norton presided over every graduate ceremony. He congratulated every single graduate. To him this was not just an onerous duty, but a pride and a pleasure.

"At the end of every graduate ceremony, the audience was treated to a short speech, seldom more than four minutes long, (which he regarded as appropriate for such occasions); carefully crafted, always optimistic, and drawing upon his wealth of experience and wisdom, to point the way ahead.

"His vision of Cranfield was: 'Excellence plus a mixture of academic rigour and practical application.'

"That vision, plus his qualities of kindness and humour, describe the man."

"We miss him indeed. We shall not see his like again."

And so, let me recall those words of Stephen Spender which seem so appropriate for our dear friend of so many years, Harold Roxbee Cox, Lord Kings Norton:

"See how his name is fated by
The Whispers of wind in the listening sky
The name of one who in his life
Wore in his heart the fire of achievement
Born of the sun he travelled a short way towards the sun
And left the vivid air signed with his honour"

And so for Roxbee, an inspiration to all who knew him, a guiding spirit for generations to come.

Acknowledgements

Professor John Stollery, CBE, FEng, Hon FRAeS
Editor

This special issue commemorates the life of Lord Kings Norton by describing and evaluating some of his most remarkable aeronautical achievements. In assembling the material I have received generous assistance from a large number of people. In particular the authors, Dr Geoffrey Pope, Graham Cropper, John Chaplin, Mike Evans, Professor Frank Hartley and Sir Peter Masefield responded promptly and enthusiastically to my requests. The library (for the majority of the photographs) and publications department of the Royal Aeronautical Society have been ever helpful and both John Harrington (Library) and Bob Fry (photography) at Cranfield have provided very useful information.

Lady Kings Norton has catalogued Lord Kings Norton's papers and has kindly given us permission to use extracts from his unpublished memoirs. Many of the papers are to be archived in the Kings Norton Library at Cranfield University. I am very grateful for her help.

Finally it is a pleasure to acknowledge the work of Dr Eric Rogers, who until recently was chairman of the editorial advisory committee of *The Aeronautical Journal*. His critical comments, always delivered with such good humour, are sadly missed.

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