SIR FRANK WHITTLE
K.B.E., C.B., Air Commodore R.A.F. (ret), M.A., F.R.S., F.Eng.
Hon. F.I. Mech.E., Hon. F.A.I.A.A.

GAS TURBINE AERO-THERMODYNAMICS

WITH SPECIAL REFERENCE TO AIRCRAFT PROPULSION

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PREFACE

In the early 1930s, when I was deeply involved in the design of Britain's first jet engine, I was regarded as a 'crazy optimist' by the many who found it difficult to believe that a young officer of the Royal Air Force could be successful in the field of gas turbines when this field was littered by a history of failure. Yet, in retrospect, I was a pessimist. I certainly did not foresee that the day would come when (on Sept 4th 1976) I would be a passenger in the Concorde and fly across the Atlantic from London (Heathrow) to Washington DC (Dulles) in exactly 3½ hours. Neither did I foresee that, in less than four decades, engines would be in service having about twenty times the power of the W2/700 (2000 - 2500 lb static thrust at sea level)—the last of the series of engines, designed by the team of Power Jets Ltd. led by me, to be built and flown. Nor did I foresee that time between overhauls would increase to thousands of hours as against the 500 hours or so that I predicted. Also, though my 1936 notebooks contain jet engine performance for speeds of the order of 1500 mph and clearly showed that such supersonic speeds were best suited for high efficiency with this type of engine, it did not seem likely to me that the aerodynamics of supersonic aircraft could be improved to the point where long range SSTs and bombers would become possible. In those days the very few—including myself—who refused to accept that the sound barrier could never be broken, (despite ballistic proof to the contrary), were not optimistic about achieving lift/drag ratios much above about 4:1. It seems that I was a pessimist in all respects except one—the time scale for development. In 1945, since we had found that it was possible to design, build, and test a jet engine in 6 months, I was predicting the advent of jet powered civil aircraft within five years and that the turbofan would take over in quite a short time thereafter. In the event I proved to be over-optimistic on both counts. I still believe that these things could have been done had not there still existed the barrier of skepticism which obstructed them.

Perhaps the thing I least expected was that I would ever attempt to write a textbook on aero-thermodynamic theory of gas turbine design. Especially, as a consequence of the formation of the National Gas Turbine Establishment in 1946, my team and I were denied the right to continue to design and build engines. As a result, my interest in the work I had pioneered began to wane and virtually disappeared to the extent that, after 1952, when I resigned my post (honorary) as Adviser to British Overseas Airways, I lost touch with the course of development altogether for many years, my interests having turned to other fields, the chief of which was oil well drilling technology. I did not, however, stop thinking about gas turbines in general in a somewhat superficial way, but remained aloof from becoming involved with them in any practical sense, having ceased to believe that anything I could hope to do could possibly compare with my earlier achievements. I did, however, accept a number of lecture engagements over the years, mainly on the subject of the early history.

My involvement with oil well drilling technology from 1953 onwards (as "Mechanical Engineering Specialist" to Bataafsche Petroleum Maatschappij—the main operating company of the Shell Group) included the design and "paper development" of an oil well drilling motor—the Whittle turbo-drill. It was, however, outside the policy of the Shell Group to enter into the drilling equipment business beyond the point of backing innovation to the stage where it could be handed over to specialist firms. So, under the terms of my contract with Shell, all the turbo drill patents were eventually assigned to me and this gave me the opportunity to seek a sponsor for its practical development. Strangely enough this started a chain of events which led to a revival of my interest in aircraft engines.

This came about as follows: In 1959 I succeeded in arousing the interest of Bristol Siddeley Engines—then one of the two largest British aircraft engine companies—in the turbo drill project, largely by virtue of their confidence in me of Sir Arnold Hall, the Chairman and Managing Director, and Sir Stanley Hooker, the Technical Director (Aero). Both these brilliant men were friends of long standing. Arnold Hall had been a fellow undergraduate at Cambridge University taking the Mechanical Sciences Tripos and had assisted me in the design of the compressor impellor of the first experimental jet engine before we both graduated in 1936. Stanley Hooker and I first met in 1940. He was then with Rolls Royce. He became an immediate convert to the jet engine and was successful in enlisting the interest of his Chief Executive, E. W. Hives (later Lord Hives). Thereafter, Power Jets received much assistance from Rolls Royce and vice versa. This resulted in increasing technical collaboration and a ripening of the friendship between Hooker and myself. Eventually under Hooker's leadership Rolls Royce became responsible for the production of jet engines based on Power Jets designs, and began their own vigorous and highly successful development of increasingly powerful jet engines.

After I ceased to be connected with jet engine design and development in 1946 and Stanley Hooker left Rolls Royce to become Chief Engineer of Bristol Siddeley, we rather lost touch with each other for about 13 years until, in 1959, he invited me to visit Bristol "to see what they were up to." On that interesting occasion I received a warm welcome and succeeded in arousing Hooker's interest in the turbo drill. The outcome was that B.S.E. agreed to back the project and, by 1961, practical development began. Thereafter I made frequent visits to B.S.E.'s Patchway factory near Bristol for several years. Hooker and I saw much of each other and the inevitable and frequent contacts with B.S.E.'s other aero engine designers re-aroused my interest in aeronautical engineering, especially as several ambitious projects were "on the boil," including the Concorde and its Olympus 593 engines, the Harrier "jump jet" and its Pegasus vectored thrust turbo fan engine, etc. I played no official role in these projects. I was a 'spectator from my sidelines,' but was often drawn into discussions of technical problems and flatter myself that I made an occasional useful contribution.

At the same time (1961 onwards) I became involved in the patent infringement action Rateau v. Rolls Royce and accepted an invitation from Rolls Royce to act as expert witness and technical advisor. Little did I realise that this was to absorb an increasing amount of my time until the case was decided in favour of Rolls Royce in January, 1967. It did, however, oblige me to brush up on jet engine theory and this stirred my interest to such an extent that I began to explore the possibilities of improving on the Olympus 593 for a second generation SST and did indeed succeed in convincing myself that very considerable improvements were possible in the form of a low bypass ratio turbo fan. Rolls Royce (now merged with B.S.E.) and U.S. firms were also engaged on project work on similar lines. The turbo drill, however, remained my main activity until it went 'on the shelf' as a consequence of the takeover of B.S.E. by Rolls Royce in 1968. At that time it had reached an advanced stage of development and was in limited production and commercial use. But Rolls Royce were already in the financial difficulties which led to bankruptcy and the nationalisation of the aero engine divisions in 1971. This, plus a general lack of interest of the new management in a field of engineering so remote from their main sphere of activity, resulted in a rapid reduction of support for the turbo drill project until it dried up altogether. This unhappy course of events and the failure to find alternative sponsors caused me to lose interest in the drill and to increase my 'private venture' exploration of SST power plant possibilities. A strong desire to see a second generation SST come into service in my own lifetime became almost an obsession.

It was clear that such a venture would be far beyond the means of any single firm or small group of firms without massive support from government and probably beyond the means of any single government, so, in 1974-75 I took it upon myself to attempt to act as a "catalyst" in the promotion of a major cooperative scheme for the development of a second generation SST involving the U.S. and U.K. governments and, possibly, the French government and the major aircraft and engine firms of these nations. I visualised a jointly owned international company at the centre of the web which would be responsible for the project phase of the operation, for development, and for the assembly phase of production and to which the several aircraft and engine firms would contribute personnel and act as subcontractors. This scheme differed from the Anglo-French Concorde joint venture in that in the case of the latter no single central company was ever formed. There were, of course, coordinating committees.

It should be mentioned that the Boeing 7011 had been smothered to death ostensibly by the environmentalist lobby in the U.S.A. (though one may venture the surmise that the vast cost to the U.S. taxpayer was the real cause).

The opportunity for my attempt was furnished by a few invitations to lecture in the U.S. Before leaving the U.K. I had sounded out the views of Rolls Royce (1971) and obtained an encouraging reaction, but refrained from contacting the U.K. Government so as to be able to claim that I was speaking for no one but myself. In Washington D.C. I succeeded in obtaining the 'unofficial blessing' of H. M. Brittanic Ambassador, Sir Peter Ramsbotham, and had a series of encouraging reactions from officials of the F.A.A. and N.A.S.A. from whom I received much help in arranging my schedule of visits to McDonnell Douglas, Lockheed, Boeing, General Electric and Pratt and Whitney with all of whom I discussed my proposals. One of these companies was distinctly 'lukewarm' to the scheme, but, in general, the results were most encouraging and I reported accordingly on my return to the U.K.

I visited the U.S.A. again from April-June 1976 with the intention of further pursuing the matter, but suffered minor injuries in a fall after arrival at J.F.K. airport and so accomplished little in furthering the scheme which, in the event (so far as I know) came to nothing. This particular visit, however, marked the beginning of the chain of events which led to the writing of this work. In the course of it I received several important invitations, the chief of which, relevant to this book, was one from Professor A. A. Pouring to lecture at the U.S. Naval Academy in October, 1976.

For personal reasons I was then seriously considering the possibility of becoming a resident in the U.S.A., but it was necessary to return to the U.K. to make preparations for this.

I returned to the U.S.A. on September 4, 1976, as a passenger in the Concorde, to keep the engagements I had accepted, to become a U.S. resident, and, hopefully, to marry an ex-U.S. Navy nurse with whom I had been friendly over many years.

During my visit to the Naval Academy, Annapolis (to lecture) in October, 1976, Dr. Andrew A. Pouring, the then Chairman of the Aero Space Department, on hearing that I was contemplating becoming resident in the U.S., asked if I would consider joining the Faculty of the Academy as a Distinguished Visiting Research Professor. The appointment would be for one year. I indicated that the offer was of interest. This led to a number of visits to the Academy and a formal invitation from the Superintendent to become 'Navair Research Professor.' This I accepted and duly became a member of the Faculty on August 1, 1977.

At this point I must go back in time to the early days of my work on jet engine development. During that period I evolved my own special methods of dealing with the aero-thermodynamics of gas turbine design which were fundamentally far simpler than the then accepted ways of treating aero-thermodynamic problems. Stanley Hooker, for one, often remarked that this theoretical innovation was more important than the original concept of the jet engine itself.

During one of my talks with Dr. Pouring at the Naval Academy I explained my methods of dealing with thermal cycles and was extremely surprised to learn that, after a period of some thirty years, these had not found their way into textbooks and so were not available to students though I knew they had become familiar to aero engine designers. Dr. Pouring urged me to write up the subject in a thesis as part of my duties at the Academy. This I agreed to do. That became the starting point of this work.

My original intention was to do no more than explain my treatment of thermal cycles, but as time passed my so-called 'thesis' began to grow and grow. The main reasons for its expansion was the course of lectures I gave on aircraft propulsion to a class of senior midshipmen who had elected to take this subject as one of their majors. Since much of the content of these lectures was not included in available textbooks I decided it would be necessary to write every lecture in advance and distribute copies to my class. It then seemed logical to expand my thesis to include the subject matter of my lectures which covered much more ground than the treatment of thermal cycles. Moreover, as time passed, I found myself extending my methods into realms of theory beyond those with which I was familiar. This intellectual exploration proved very fruitful and I added quite substantially to my knowledge of the aero-thermodynamics of turbo machinery. The boldest of these ventures was a limited excursion into shock wave theory (Section 4). This was a field of aero-thermodynamics with which I was almost entirely unfamiliar and I was curious to find out whether, if at all, my methods could be applied. I was pleasantly surprised to find that they could. I decided that these (to me) novel results must be included in this work. It now became clear that the original thesis was becoming a book.

This piecemeal expansion has had its drawbacks. It has meant much re-writing and re-arrangement of its sections into a more logical sequence as additional material was included. I hope the reader will be tolerant with any remaining 'patchiness' he may find. I, myself, am far from satisfied with my efforts. I fear that some evidence of over-hastiness (e.g., some inconsistencies in notation) will be apparent because I have had to meet a financial deadline imposed by the fact that the funds which have been allotted for its initial publication will be cut off automatically very soon after these words are written. The completion of the work has been considerably delayed by preoccupation with other very interesting but time consuming projects plus the domestic complications arising out of transplanting myself from the U.K. to the U.S.A. and five moves of home in little more than eighteen months.

As the reader will find, the fundamental basis of my methods was to treat air as a perfect gas of constant specific heat and to deal with thermal cycles by using temperatures and temperature ratios. Also to treat velocities (or kinetic energy) as having a temperature equivalent and vice versa. These assumptions are very accurate for the temperature range 180-400°K where specific heat is, in fact, virtually constant, but become increasingly inaccurate with increase of temperature above 400°K owing to the increase of specific heat. Nevertheless, as will be shown, they give good comparative results, and make it possible to deal with even compound thermal cycles in a matter of minutes. When, in jet engine design, greater accuracy was necessary for detail design, I worked in pressure ratios, used $\gamma = 1.4$ for compression and $\gamma = 1.33$ for expansion and assumed specific heats for combustion and expansion corresponding to the temperature range concerned. I also allowed for the increase of mass flow in expansion due to fuel addition (in the range 1½ - 2%). The results, despite the guesswork involved in many of the assumptions, amply justified these methods to the point where I was once rash enough to declare that "jet engine design has become an exact science." (This statement was inspired by the fact that on the first test of the W2/500 engine every experimental point fell almost exactly on the predicted curves of performance.)

Much of Sections 1 and 2 will be familiar to advanced students, but they have been included for the benefit of students comparatively new to the subject and to provide the foundation for later sections.

I have assumed throughout that the reader will have a knowledge of the rudiments of thermodynamics, aerodynamics and calculus.

As the reader will find, I have made frequent use of numerals instead of such constants as $\frac{\gamma}{\gamma-1}$, $\sqrt{2gK_p}$ etc., thus formulae which would otherwise look most complicated are greatly simplified. I make no apology for this. I feel sure that most readers will welcome it. Any purist who would prefer otherwise can always substitute $\frac{\gamma}{\gamma-1}$ wherever he sees the exponent 3.5 and $\frac{1}{\gamma-1}$ wherever 2.5 appears as an exponent. If he wishes to translate

$147.1\sqrt{\theta}$ into $\sqrt{2gK_pT_1\left[1-\left(\frac{P_2}{P_1}\right)^{\frac{\gamma-1}{\gamma}}\right]}$ he should have no trouble in doing so.

A Note on Units. I apologise to those who might have preferred the S.I. system of units. I grew up with the foot-pound-second system (except for chemistry and physics). In my young days the metric system was never used in mechanical engineering in the U.K., and I find it hard to break the habit of a lifetime. Moreover, if I had talked about thrust in kilograms, height in metres, speed in metres/second and so on, it would have been much