damping is $\frac{1}{0\ 127\ \sqrt{1-\zeta^2}}$ giving for $\zeta = 0\ 98$, say, a period of 40 seconds while for $\zeta = 0\ 5$ say, the period becomes 9 seconds Unfortunately it is unlikely that the original assumption of forward speed constant will be valid over times of this magnitude Thus the analysis above is not likely to give exact answers but only a guide to the response behaviour of the helicopter

The use of the full equations of motion for the analysis of the step type of control input is obviously more complicated, but the method of Appendix 2 can be used to give the corresponding result and this in turn, by using the linearity of the equations of motion can be modified to give more accurate results for the conventional " tooth " input considered here, thereby giving more accurate response characteristics and handling data than the simplified equations above

Discussion

Mr F O'Hara (*Royal Aircraft Establishment*) (*Member*), said that they must all have been impressed by Dr ROBERTS' account of the problems which have to be faced by a worker in the aerodynamics office of a helicopter firm As one who had done a certain amount of work on research aerodynamics, in which one dealt with a general theoretical analysis, he sympathised with the need for specific answers to be given and for quantities to be fed into a given design At one stage of Dr Roberts' Paper, however, he had wondered what was the exact function of the aerodynamics office in the process of the design of the helicopter Dr Roberts had commented on a method of ground effect analysis by Dr Cheesman and had said that reasonable estimates of ground effect could be obtained by a theory provided by Dr Cheesman It would appear to him that this was something which an aerodynamicist would elect to have, but Dr Roberts considered that he would prefer to have a fundamentally more satisfactory theory The requisite object, Mr O'HARA considered, was to supply the design aerodynamicist with a theory which gave the right answer He did not think Dr Roberts could ask for more

With regard to the theory of stability, Dr Roberts had given an account of a standard type of classic theory and the more recent developments of that theory in relation to the helicopter, but he had raised questions about the appropriate values of certain concepts in that theory, such as the manoeuvre margin in relation to helicopters Mr O'HARA said he would like rather to learn from Dr Roberts whether the aerodynamics design office found it possible to get reasonably reliable estimates of manoeuvre margin and whether he felt that on the designs with which he had been concerned he could achieve the value he quoted as desirable These were points on which information from design aerodynamicists could be particularly interesting

On the question of evaluating low speed performance, Dr Roberts had referred to Oliver s method by which the ratio of induced power was said to be 1 2 This was not inconsistent with the figure which Dr Roberts had quoted, because in Oliver s analysis separate account was not taken of the fuselage vertical drag This resulted in an apparent reduction of the efficiency of the rotor, which showed up in a larger ratio between the practical and the theoretical values of the induced velocities

Commenting on the stability and the handling qualities of a compound type rotor-wing helicopter in which one had a certain amount of control over the distribution of lift between the rotor and the wing, Mr O'HARA suggested this would lead to something in the nature of an additional major control, and was a point on which one had to be careful It was desirable in flying machines to limit the number of controls Some of the basic handling difficulties in helicopters arose from the necessity for a collective pitch control in addition to the main control column The variation of the distribution of lift between the rotor and the wing had to be considered very carefully He considered that it was probably desirable to work as far as possible with the optimum arrangement for significant flight conditions, removing the factor of lift distribution from the control of the pilot This was particularly important in relation to the technique of blind flying, where flying attitude was an essential indication of the flight state

On the question of control with the rotor offloaded, reference had been made to the longitudinal control, including the use of an elevator The latter gave a certain amount of longitudinal control to replace the loss as the rotor was offloaded of rotor control power Similar consideration had to be given, however, to the need for ailerons to supplement lateral control

On the more theoretical aspects of stability, in Dr Roberts' treatment of the build-up of normal acceleration, Mr O'HARA said there was some tendency to confusion It had not been made clear that in the consideration of the manoeuvre margin application to helicopters, as to fixed wing aircraft, one was concerned with flight at constant speed in which there was a change from a steady flight condition to a condition with normal acceleration at the same speed The time to damp to half amplitude on the other hand was used in connection with assessment of the general dynamic stability motion A point he would like to put up in return to the lecturer's contention that the performance analysis definitions were often not clearly made, was that in considering stability analysis it was equally important to define the axes and the Paper did not make clear what axes were being dealt with

Some general comments had been made on engine failure, but presumably mainly in relation to shaft-driven rotors One was interested in the Lecturer s views on engine failure characteristics with tip jet drive, and in particular on the use of emergency sources of power

The **Author** (in reply), said that although Cheeseman s method was adequate for normal use, he did not find it satisfactory in the sense that it did not lend itself to some of the other problems which we had to face One had to differentiate between a feeling that one was approaching the problem in a correct manner and the adequacy of the method Wind Tunnel constraint was an important extension of ground effect and it was because of this that a possibly more difficult but less semi-empirical would leave a happier feeling

On the question of the amount of manoeuvre margin to which one should design, the Author said that if designers were given a free hand they could design to almost anything, but unfortunately they were never given a free hand All they could do was to point the direction in which they ought to go He felt that they should try to get as much manoeuvre margin as possible The methods of obtaining this were fairly standard, and they had been dealt with in the Paper

Dealing with the distribution of lift between the rotor and the wing, the Author said that in order to fly fast off-loading on to a fixed wing offered advantages which outbalanced the probable disadvantages Control by two possible sources naturally caused complication and difficulty but this has had to be faced , probably what would happen is that we should use rotor controls at low speeds and elevator control at high speeds The Author was not clear about Mr O Hara's question on the response to cyclic pitch It seemed to him that since we are concerned with what happens within a matter of two or three seconds we can only be concerned with the short period motion and not with the phugoid damping

Referring to the subject of engine failure, the Author said there was one advantage with a tip jet drive, namely that the tip blade inertia happened to be on the high side compared with the root drive condition Unless one had control of the orifice areas at the tips, the fact that the power from one engine had been lost might mean that sufficient power could not be supplied to be of any use It did not follow that more than half power, or any power at all, was retrieved This was a case in which one had to start thinking about special devices

The **Chairman** said that it was possible with a tip drive system to arrange, by splitting the air ducting, that if an engine failed in a twin engine machine, the rotor power would be not less than half Although one pair of blades had no power the other pair of blades could still extract full power from the remaining engine and hence half power was still available at the rotor If, on the other hand, the power supply were fed into a common trunk, engine failure could have serious consequences One would then lose much more than half power

Mr R H Whitby (*B E A*) (*Member*), said he felt sure that he would be able to make use of much of the material given in the Paper A few years ago he had had to compare half a dozen different helicopter designs, and, not possessing a large aerodynamics office, had found it necessary to arrive at some fairly simple methods of performance analysis An essential feature of some of the designs was that they had large excrescences under the rotor Obviously, one had to make allowance for this fact, and he would confirm what Dr ROBERTS had said on the subject Dealing with the R 4, Stewart (*Jl Royal Aero Soc*, May, 1948), whose work on the subject was probably still the best, gave a value 1 20 times the ideal induced drag when hovering Taking into account the R 4 drag in the rotor downwash, one arrived at a figure of 1 15 instead of 1 20 One could then apply further corrections, which were obtainable from an American work, for taper and twist (Myers & Gessow NACA Tech Note 1542), to give a datum value for an untwisted, untapered rotor in free air

The Author had mentioned Glauert's hypothesis on induced drag in forward flight , Mr WHITBY said this was the first time that he had heard anyone mention a point which had always struck him as being remarkable a rotor seemed only remotely similar to a horseshoe vortex system He was encouraged to hear Dr Roberts say that the hypothesis also seemed remarkable to him However, he expected that this hypothesis would continue to be used in the interests of simplicity, with arbitrary factors being applied to it

The tandem helicopter had been the subject of closer analysis in some later work by Stepniewski (Paper to West Coast Forum of American Helicopter Society, September, 1955), and he had given the effect of variations of one rotor in relation to the other However, again as a simple assumption the induced drag of a tandem helicopter seemed to be in the neighbourhood of three times the induced drag of one isolated rotor He would be interested whether the firms concerned with tandem rotor designs had any more accurate but yet simple assumptions to suggest He would also like to know whether the refined methods of performance estimation which were, no doubt, necessary but laborious really paid off, and whether anybody had been able with any degree of precision to predict the performance of a helicopter in advance of its flying

The curve with regard to the effects of compressibility around the rotor confirmed some estimates he had made a few years ago, but he would like to know what assumptions were used in calculating the incidence Dr Roberts had criticised the calculations of compressibility effects It had seemed to Mr WHITBY that many assumptions as to flow through the rotor had to be made in order to make such calculations Was not the method of arriving at incidence round the rotor also open to criticism?

With regard to the figures relating to the increase of parasite drag with blade incidence, he presumed that the C_L mentioned by the Author was related to the main blade C_L

On the subject of fuselage drag variation with incidence, the only data that he could recall seeing before was some early work in connection with full-scale tests on the R 4 in an American wind tunnel, in which at all ranges of negative incidence the drag was more or less constant at the value of zero incidence , at positive incidence it increased considerably This American data did not seem to be of any help at all in that context He presumed that the curves shown by the Author were based on tunnel results in which case scale effects might be very significant

In reply, **Dr Roberts** said that variation of incidence over the rotor had been calculated in the conventional way He agreed that it left the whole method open to a certain amount of suspicion, but the answers seemed to agree with flight tests The figure of 0 01 for $\Delta C_d/\Delta C_L^2$ was mainly associated with the t/c ratio, and it did not vary much on that ratio With regard to wind tunnel models, one used threads or transition wires He was not sure about the extent to which the variation of drag with incidence was affected by scale, although the order of drag was well affected

The **Chairman** said that he would like to hear of the impact of the philosophy of Dr ROBERTS as it was felt by those who were concerned with designing tandem helicopters

Mr P R Payne (*Bristol Aero-Engines Ltd*) (*Member*), said he had not been associated with tandem helicopters for some years, and was now concerned with engines, but that he would reply as no one else had volunteered He said that it was difficult to comment in any detail upon a lecture as detailed as the one they had just listened to without the aid of a pre-print, particularly since Dr ROBERTS had eliminated all mathematics from the spoken lecture and many of his arguments

His experience of wind tunnel measurements of drag and pitching moment had indicated that unless the wind tunnel was very large the results were likely to be of little practical value Two helicopters of which he had personal experience had been found to have parasitic drag losses in flight of the order of three times the figures estimated from tunnel tests He thought that the most reliable way of obtaining parasitic drag estimates was to use the well-established methods of direct calculation (with suitable modifications for induced velocities) as described in Hoerner's " Aerodynamic Drag " for example

Wind tunnel tests on rotors were even more difficult to carry out satisfactorily, and he would be interested to hear whether Dr Roberts had any experience of this

On the subject of mean blade drag coefficients he was in complete agreement with Dr Roberts In the past (and in one case quite recently) absurdly low figures had been quoted for δ_0, even as low as 0065 for NACA 0012, whereas the minimum value achieved in practice was of the order of 01 This sort of thing permitted very attractive design studies to be submitted ! There was a great deal of difference between wind tunnel figures measured on a test section in two-dimensional flow, and the value actually achieved on a rotating blade, with the boundary layer subject to C F effects Dr Roberts' method of determining the latter was both interesting and elegant

Referring to vertical drag Mr PAYNE thought that Dr Roberts' estimates were about half what they should be

Mr PAYNE said that whenever helicopter aerodynamicists discussed induced power losses they were sure to argue about the size of the factor by which the ideal actuator disc value should be multiplied Figures as different as 1 2 (Boscombe Down) and 1 1 (some American tests) were suggested, and it seemed to him that this only indicated a lack of appreciation of the fundamental picture on the part of the disputants, since they were all right The " effective actuator disc area " of a rotor was an annulus, the inner and outer radii of which were determined by the blade tip loss and the inboard position of the blade root Moreover the circulation along a practical blade was rarely constant, so that the calculation had to be made for a large number of thin annuli When these two effects were allowed for the momentum theory gave exact agreement with practice in vertical flight, as Brotherhood had shown many years before The " correction factor " for simple actuator theory thus obtained obviously varied from blade to blade

With regard to induced losses in forward flight, he could not understand why Dr Roberts and Mr O'Hara had queried Glauert's hypothesis This was a matter of simple dimensional analysis, and the mean induced velocity must be given by the relationship

$$v_1 = \frac{T}{\rho \pi R \; V^1} \times K$$

Glauert had suggested that $K = \frac{1}{2}$, by analogy with an elliptically loaded wing, and flight tests had indicated that K was roughly 1 08/2 The more refined theories, such as that of Castles and DeLeeuw, gave substantially the same result, except that asymmetry of the wake caused K to vary slightly with μ and λ In the face of modern advances in the theory of induced flow it was surely very rash to suggest that $K - 2/2$ without arguments to justify it

Mr PAYNE said that the calculation of the downwash induced at the rear rotor of a tandem by the front rotor was a very complex problem, although he knew that work on this was proceeding at Bristol Aircraft Ltd , and that they had made very great progress American tunnel tests showed that Stepniewski's approximation (first used at Bristol many years before) was very reasonable, whereas the theory of Castles and DeLeeuw underestimated the interference, and that of Squire and Mangler tended to over-estimate it Thus the induced power loss of a tandem in forward flight was the same as that of an equivalent single rotor helicopter of twice the disc loading, and on the basis of this type of approach Mr O'Hara has suggested at an earlier lecture that the tandem was inferior to the " single " rotor configuration This was only true in the limited sphere of aerodynamics, and even then ignored the considerable superiority of the tandem in vertical flight, and its superior stability and control characteristics When the parasitic weight and drag of the tail-boom and rotor, shafting, etc , was allowed for, together with less obvious factors such as uselage weight, Mr PAYNE thought that the tandem was superior to the Sikorsky configuration for many applications He did not think it was the only attractive configuration by any means, but he understood that the very complete studies made at B A C over the last few years had conclusively demonstrated its superiority for large *helicopters*

On the effect of aerodynamic compressibility he thought that people were worrying unduly about it He thought that no-one had ever measured any appreciable compressibility effects on an efficient rotor blade (there was an American tower test report, but the results were meaningless because of incorrect analysis of the results) and in propeller applications there was no large fall in the factor of merit until supersonic tip speeds were reached, around M_T equal to 1 1 or 1 2 Compressibility was not taken into account in normal calculations because at constant C_L it was found that C_D was independent of Mach number until the drag divergence point was reached He suspected that Dr Roberts' definition of critical Mach number was that at which a local velocity exceeded the speed of sound, and he wished to point out that in practice drag divergence was often delayed until a considerably higher Mach number was reached It should also be remembered that a rotor blade was a " swept wing for most of its rotation, a fact that was often forgotten because of the general use of the rather crude relative velocity assumption $U_T - x V_T + V \sin\psi$ which was greatly in error at azimuth angles of 0 and 180 He did not mean to imply that compressibility did not affect the control angles to trim a high speed rotor , the effect here was quite significant, particularly in the calculation of coning angle, which compressibility can increase by as much as 30%

In reply, the **Author** said that he was never happy about making parasitic drag assessments on fuselages He preferred the wind tunnel test to a completely guessed estimate

His experience on wind tunnel tests on rotor blades showed that there was no apparent difficulty where parts of a full scale blade or a model blade were being used Of course, the size of the wind tunnel was important in relation to interference effects and it was possible that scale effects on small complete rotors were on the doubtful side

Referring to the correction factor on Glauerts' hypothesis, he agreed that the expression was right but, he asked, was the factor of proportionality 1 or more than 1 ? For normal calculations one could use 1 or 1 5, but he was not sure that in this case it was of the order of 1 or of 2

On the subject of tandem helicopters, he felt that Mr Payne's experience was greater than his As far as Mach No losses were concerned, he thought that Mr Payne was really saying what he had already said—namely, that in panic there had been a tendency to over-rate the losses due to compressibility In one case there would be produced by Liptrot's method a result 50% greater than would be produced by the method that he himself would use One tended to over-estimate the Mach No losses With regard to the transient effects of a blade reaching $M = 1$, the fact that it was transient made little difference at all

Mr F E Bartholomew (*Hunting-Percival Aircraft Ltd*), said that the Author had mentioned that the gas-driven helicopter suffered the penalty of the thick blade sections Had he been a thermo-dynamicist instead of an aerodynamicist he would have used the expression " advantage " and not " penalty " Thermo-dynamics and aerodynamics of gas-driven rotor must be considered together, and neglecting this had led to a lot of misconception particularly with regard to the rotor produced by his firm People mentioned high specific fuel consumption which was based on rotor horsepower without considering the thermodynamic efficiency of the system or the aerodynamic efficiency of the system It was possible to give almost any specific fuel consumption based on rotor horsepower by varying the rotor tip speed

In the deceleration tests it was not clear to him how the induced power was eliminated from the result

The Lecturer had not mentioned control forces His firm had been making extensive enquiries for a considerable period on this question, and with a few exceptions, the general policy seemed to be brute force and ignorance They had made a calculation to see what happened as a result of blade distortion, and with a small distortion at the trailing edge amounting to about one-tenth of an inch on a 2 6 ft chord, the control forces amounted to about 2,000 lbs in

Control forces occurred as a result of all sorts of causes, and in most unusual circumstances Blade weight would be reduced and sometimes the control force would increase It was sometimes suggested that the cause was to be found in the bearings, but when the bearing manufacturers were consulted they denied that this was so He suggested that there was plenty of scope for research in this field

Commenting on the effect of induced power on the rotor rig tests, the **Author** stated that ground effect which was present during these tests had a small effect on the curve of power versus blade angle , the thrust at constant blade setting was increased, but the induced velocity was reduced, giving small net effect

Mr J M Harrison (*Westland Aircraft Ltd*), agreed with the Author that more time should be spent in the industry's design offices considering the problems of stability and control It was possible in his firm to allocate a major part of the aerodynamics staff to this subject, since performance estimation to a degree of accuracy required by service and commercial users had been reduced to a routine procedure They had tested a fuselage in the Wind Tunnel, and contrary to what Mr PAYNE had said, they had been able to predict the parasite drag accurately from this basis The main handicap in a Wind Tunnel test was that it was difficult to simulate the rotor hub The whirling of the blade shanks and the other pieces of pitch mechanism had the effect of creating a stagnation region, and one could account for a large percentage of parasite drag by considering the rotor hub as an equivalent flat plate or solid cylinder This device successfully accounted for the extra drag deduced from flight tests

They found there was little variation of drag with incidence over a range of plus or minus 10 degrees, which adequately covered the high speed range He did not consider the fuselage drag to be of much importance at low speeds

They could predict hovering performance accurately using momentum theory, with a tip loss factor of 95 to 97 per cent instead of using empirically corrected curves with a factor of 1 2

He was sure that the fuselage was the most important factor in the longitudinal stability of a helicopter The fuselage pitching moment could be predicted by means of a simple formula in which the moment was divided into two parts That due to potential flow could be estimated using Munk's method Representing the fuselage by a chain of equivalent circular cylinders in transverse flow, the viscous component could be estimated by numerical integration using Wind Tunnel data On this basis they had fitted a curve to the measured Wind Tunnel moment in the form

$$\frac{M_f}{p} = AV_f \sin 2\alpha \text{ and } BS_f l_f \sin \alpha$$

In hovering when the fuselage made no contribution there was a Dutch roll type of instability This curve showed a stabilising contribution in the lower speed range with a transition to a divergence at higher speed It was possible to determine with its aid the tailplane configuration required to give satisfactory handling characteristics

Mr HARRISON concluded by stating that his staff had found it difficult to apply O Hara's methods of performance reduction and described briefly an alternative numerical method

Notation

M_f	Fuselage pitching moment	α	Incidence of fuselage datum
V_f	Fuselage volume	q	Dynamic pressure
S_f	Fuselage wetted area	A, B	Emperical constants
l_f	Fuselage length		

In replying, the **Author** commented on the problem of parasite drag of a hub He was reminded of some tests which were carried out on streamlining the hub of a helicopter The more they tried to refine it, the worse the problem became His conclusion was that it was pointless to try to streamline the hub That fact probably accounted for Mr HARRISON'S comment that a cylinder was good enough for simulating the effects of a rotor hub

With regard to the importance of fuselage drag, he agreed that incidences of 30 degrees would not be achieved in a normal flight but there were obvious regimes in which such angles do arise , for example, at high rates of climb at low speeds, and in auto-rotation with all power losses The resulting conditions in the latter case due to an unexpectedly high rate of descent might be critical

Commenting on Mr Harrison's methods of predicting pitching moments, he expressed the reluctant opinion that the problem was by no means as simple as Mr Harrison had indicated No theoretical method could possibly give the shapes of curves of pitching moment which had been measured on some fuselages

Mr R G Austin (*Auster Aircraft Ltd*) (*Member*), said that much more work had to be done on the stability of the helicopter if it was to be a reliable and satisfactory flying machine If the rotor could be made to be stable in its own right—in other words, to have a forward speed and pitch stability—they would have gone a long way towards getting a useful machine He felt that this was possible Methods had been suggested to enable this to be done, but, as far as he knew, little if any test work had been carried out of a full-scale nature or in a wind tunnel

They were all sadly aware of the small amount of capital that had been put into helicopter research in this country, and that was borne out by the fact that although it was agreed that the gas turbine was the natural power unit for a helicopter, no gas turbine powered helicopter was yet flying in this country, although several were flying in the United States and in France

Dr Roberts had said that the flight envelope was pretty well rectangular Work had been carried out on a Hiller and a R 5, and normal acceleration had been produced by the application of collective pitch and cyclic pitch together or slightly phased The maximum G that could be applied on either of those machines by applying collective pitch and cyclic pitch together was 2½G, and yet by phasing, so that cyclic pitch was first applied and then collective pitch applied shortly afterwards, no more than 2 7G was achieved on either machine Therefore, it appeared that this was not a matter that one should worry about, as Dr Roberts had said

The **Author,** in reply, said that all he needed to add to Mr Austin's remarks was a slight correction The Fairey Ultra-Light Helicopter was, of course, a gas turbine helicopter and was still being flown , probably Mr Austin had meant to say that there was not at present being flown under Government contract a gas turbine helicopter The Rotodyne, although under development under Government contract, was, of course, not yet flying

The **Chairman** invited further speakers to discuss the question of boundary layer control

Mr R J Jupe (*Bristol Aircraft Limited*) (*Member*), said he was gratified to hear Dr Roberts and Mr Whitby express doubts about the application of Glauerts formula relating to down-wash in forward flight He had tried to analyse the problem during the past two years He had plotted the vortex sheet shed by individual blades in the form of lines per inch (like a magnetic field), and then integrating the lines with a mechanical device similar to a planimeter This gave some rather interesting results The peak values of the down-wash shown by this method were of the order of three times the mean in certain cases This type of distribution had recently been confirmed by model tests at the Massachusetts Institute of Technology The reason for these high variations in induced velocity had been discovered There was a big interference effect between blades which caused any disturbance to build up as blade after blade went by, and this produced an amplification of any variations in the down-wash It did not appear to be necessary to have any lift on the rotor for this to occur

He would be interested to ascertain whether it was possible to increase the forward speed of helicopters solely by fitting wings, because it appeared at the moment that a lightly loaded rotor might also suffer severe vibration from the above cause at critical speeds These large variations in the down-wash in high-speed forward flight would also appear to affect such things as performance and the strength load factor on helicopters

Mr C Faulkner (*Saunders-Roe Ltd*) (*Associate Member*), responding to the Chairman's invitation to discuss the subject of boundary layer control as applied to helicopter rotors, said he felt that we in this country had already devoted too much effort to the development of rather questionable refinements similar to this one We should concentrate much more on the art of producing satisfactory conventional helicopters

The trend was inevitably towards higher disc loadings This was based on practical engineering problems, coupled with the need to produce a compact machine This trend was being accelerated by the introduction of the gas turbine, as its low specific weight allowed the designer greater flexibility enabling him to accept the resulting lower aerodynamic efficiencies With moderately high disc loadings (about 6 lb /ft ^{2}), the rotor profile power was perhaps 20 per cent of the total power required Whatever was done in the way of boundary layer control, we would be fortunate if we reduced this by more than say 30 per cent giving a net power saving of the order of 6 per cent If one allowed for the increased complexity and hence weight resulting, the gain in aircraft efficiency was probably negligible

He felt that we should not worry too much about " gadgets," particularly at the expense of losing ground on the development of the basic product He did, however, agree with Dr ROBERTS that we should not exclude necessities, such as the investigation of various stabilising devices

In reply, the **Author** said that he had felt for many years that boundary layer control, certainly in the form in which they knew it, would not work even on a fixed wing, let alone on a rotating wing He was not sure that it was not possible to make a jet flap work in highly specialised helicopters, and one could equally develop some form of boundary layer control for one type, but he thought they had been over-sold on boundary layer control Care should be taken not to confuse lack of belief in a particular scheme with the more general statement that gadgets in general do not work on helicopters Designers had to be discriminating in their work and in the gadgets that they tried to develop, but that work must go on if they were eventually to get where they wanted

The **Chairman** said that the Author had given a highly technical and specialised paper which had succeeded in stimulating interest in an extraordinary number of diverse branches of aerodynamics Many points had been covered, and some could only be touched on He had the impression that it was generally felt that the performance side was reasonably well developed, but a lot had yet to be done in the field of stability and control In particular, as the more advanced forms of multi-rotor helicopters, compound helicopters and so on came into production there would be a demand for higher speeds, especially in the transport field, and this would place even greater emphasis on stability and control There would always be a demand for a machine which could fly and be controlled at high speeds and yet retain good slow speed and hovering qualities How these requirements could best be met was obviously a field where great effort would be required from the aerodynamicist

There was no doubt that their efforts had to be concentrated on developing control and stability He made a plea once again, for more Government support in helicopter research From the military point of view there was a tendency to favour carrying loads below the fuselage One could visualise tanks being slung underneath helicopter fuselages on short journeys of a mile or half a mile, and this would introduce another stability problem

He asked the audience to join with him in thanking Dr Roberts for devoting so much time and effort to a very good Paper and for answering their questions so well

The vote of thanks was carried with acclamation

WRITTEN CONTRIBUTION FROM MR P R PAYNE

The Chairman (very wisely) thanked me before I was half-way through my comments at the lecture, so I should like to take this opportunity of summarising my remaining points As I have still not received a pre-print I must apologise in advance for any mis-representations of Dr Roberts' views

Dr Roberts referred by implication to the " Amer effect " on damping, which was originally pointed out by Miller in 1948, and said that it was a function of C_T This is not strictly true unless the collective pitch angle and the solidity are included in the expression I have found that a more realistic form of presentation which can be proved to hold true for forward flight, as well as the hovering case treated by Miller and Amer, is

$$\frac{da}{da_1} = \frac{db}{db_1} = 1 - \frac{t_2\,\lambda_T}{2\,C_T}$$

where λ_T is the inflow ratio relative to the tip path plane, and the other symbols are as defined in Ref 1 This expression clearly shows the importance of inflow ratio, indicating high damping for the gyroplane configuration, and low, or even negative damping for the compound helicopter at high speed There are also other terms present in the calculation of forward flight stability which Dr Roberts appears to have ignored, notably the change of downwash gradient with forward speed $\frac{\delta(K\lambda_1)}{\delta u}$ and attitude $\frac{\delta(K\lambda_1)}{\delta\theta}$ and the change of thrust vector inclination to the tip path axis with speed and attitude His suggestion that damping is reduced by the use of offset hinges is surely wrong $\frac{\delta a_1}{\delta q}$ and $\frac{\delta a_1}{\delta\mu}$ are diminished, but the *damping increases* with offset up to a critical value (Ref 1)

Dr Roberts presented a plot of fuselage attitude against forward speed and off-load ratio, where the fuselage attitude is shown to shoot off to infinity at one condition, and suggested that this might introduce problems It is easy to show that such a phenomenon cannot in fact occur, and I suspect that an error in the sign of a_{1s} occurs somewhere in the analysis

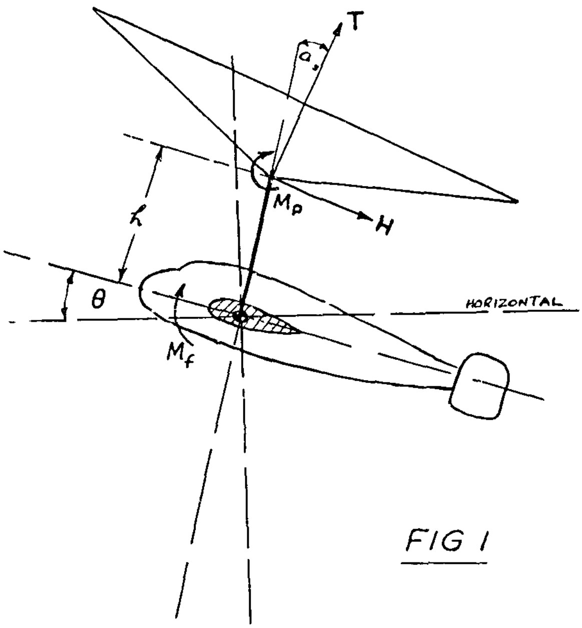

FIG 1

Taking the simplest case (Fig 1) where the C G coincides with the intersection of the wing quarter chord and the shaft axis, the equation for equilibrium of moments about the C G is

$$T\,h\,a_{1s} = M_P + M_F + H\,h \quad 0 \tag{1}$$

The hub pitching moment for offset flapping hinges is

$$M_P = \tfrac{1}{2} be(CF)a_{1s} \tag{2}$$

where (CF) = the centrifugal force in one blade
b = number of blades
e = flapping pin offset
a_{1s} = rotor flap-back angle with respect to the shaft

Equation (1) becomes

$$-a_{1s} = \frac{M_F + H\,h}{[T\,h + \frac{1}{2}\,be(CF)]} \tag{3}$$

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The disc incidence (i) does not vary greatly with the gyroplane configuration in cruising flight, so that it is convenient to write $a_{1s} = -i - \theta$. Substituting in equation (3)

$$\theta = \frac{M_F - i[T h + \frac{1}{2}be(CF)] + H h}{[T h + \frac{1}{2}be(CF)]} \qquad (4)$$

Equation (4) cannot tend to infinity unless $M_f \rightarrow \infty$ or $H \rightarrow \infty$, which is obviously impossible, or Th and $e \rightarrow 0$. In the second case, since $\frac{\delta M_F}{\delta \theta} < 0$ the fuselage will adopt a slightly nose-up attitude so that the pitching moment M_F balances H h. With 100% off-load the gyroplane is (for static considerations) merely an aeroplane with a rather high centre of drag.

I have dealt in Ref. 2 with other subjects mentioned in the lecture, and also vertical drag, induced power losses and tandem interference, so that I need not repeat these points here. Dr. Roberts did allude however, during the course of the lecture to a "controversy" between Mr. Shapiro and myself, in a weekly magazine, on the subject of performance methods. I should like to make it plain that this is very definitely not a controversy in the accepted sense of the word. A part of my second letter to the Editor was published two months after it was written. This did not give chapter and verse for my points because I knew that the reviewer of Mr. Shapiro's book would not need it, but the letter was in fact answered by Mr. Shapiro. I therefore wrote a second letter briefly explaining my points, on January 1st, and since this has not yet been published (April 29th) I assume that Mr. Shapiro now accepts my views. Dr. Roberts has in any case borne out some of my contentions in his lecture, as in the case of practical values for δ_0.

Finally, I should like to take this opportunity of saying how much I enjoyed the lecture. It did not perhaps refute the contentions of the anonymous letter writer whom Dr. Roberts took for his text at the beginning of his lecture (I cannot recall having seen the letter, so that I have to infer the contents) but he has clearly shown how much work remains to be done before helicopter aerodynamicists can approach the exactitude of their subsonic fixed wing counterparts. Is it possible for him to say whether his claims for the predictability of performance and stability in the design stage have been borne out by recent experience?

REF. 1. "The stiff-hinged helicopter rotor." *Aircraft Engineering*, Vol. XXVII, No. 321, November, 1955.

REF. 2. "Induced aerodynamics of helicopters." *Aircraft Engineering*, Vol. XXVIII, Nos. 324–327, February–May, 1956.

DR. ROBERTS' REPLY TO MR. PAYNE'S WRITTEN CONTRIBUTION

With regard to "Amer's Effect" the expression given in the lecture for b_1'/b_1 involving θ and λ is equivalent to Mr. Payne's expression for the particular case considered (hovering, untapered blades). Mr. Payne has, I understand, extended the analysis to forward speed. Since roll damping for conventional helicopters is adequate providing the hovering case is covered, the extension is of value for the case of off-loaded rotors only. I agree Mr. Payne's extension is important in that case and I, among others, will doubtless find it of use.

I am not in agreement with Mr. Payne on the question of the effect of varying the position of the flapping hinge. It is possible that I have considered offsets greater than Mr. Payne's so that having exceeded the value for this critical offset I then get the decrease he himself would obtain, were he to increase the offset in his own calculations.

With regard to Mr. Payne's analysis, devoted to showing that θ does not approach infinity at any speed, I am afraid Mr. Payne has omitted a most important term from the denominator. This should read:—

$$\text{Denominator} = Th + \tfrac{1}{2}\,be\,(C.F.) - l\frac{\delta L}{\delta \theta}$$

where l = tail arm

L = wing lift

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The last term arises from the pitching moment equation in which one substitutes for tail lift as the difference between aerodynamic lift forces (wing and rotor) and the weight   It is this term which produces the odd result since it is of opposite sign to the remainder of the denominator

Mr Payne's closing remark is rather less naive than would appear at first glance We can certainly predict performance providing we have the correct data to start with, correct powers available, accurate drag estimates, etc   The extent to which one gets the correct answer is the extent to which one's basic information is accurate   Given a free hand so that the best available information can be obtained, I am pretty confident that performance is accurately predictable, and indeed have found it so

The same cannot be said of stability analysis since our experience on stability testing is far too limited   We cannot even say at the present in this country, that we know how to test for stability of helicopters   On this basis we certainly are not able to say the agreement between analysis and test is acceptable—other than in a purely limited qualitative sense

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The Eleventh Annual General Meeting of The Helicopter Association of Great Britain

The Eleventh Annual General Meeting of the Helicopter Association was held at the Royal Aeronautical Society, 4 Hamilton Place, London, W 1, on Tuesday, 12th June, 1956, at 5 30 p m   The Chair was taken by Dr G S HISLOP, Chairman of the Executive Council

The routine business of the Meeting was conducted and the result of the ballot for election of members to the Executive Council was announced The constitution of the new Council for the year 1956/57 is as follows

B H Arkell	R Hafner
J A J Bennett	J E Harper
R A C Brie	G S Hislop
A E Bristow	A McClements
J A Cameron	F T Meacock
L G Frise	J W Richardson
W R Gellatly	H Roberts

D L Hollis Williams

Following the Chairman's address, which is fully reported below, and the conclusion of the business of the Meeting, an informal discussion took place on the affairs of the Association

The Chairman's Address

On the occasion of these Annual General Meetings it is customary for the Chairman to give a brief review of the British helicopter world and to take the opportunity of appraising its achievements, its prospects and its general state of health

My own conclusions on this the 11th Annual General Meeting are that the most striking events which have taken place are in the operational and Government policy fields

Taking the operational side first, the Royal Navy and the Royal Air Force have continued their sterling work in dealing with emergencies which arise from time to time, particularly around the coasts of our country   Daring exploits have been performed and the crews of merchant ships have been plucked from the decks of their ships in the most hazardous sea and weather conditions, when the prospect