Discussion

The Chairman said that Mr O'Hara had given a most interesting and stimulating account of some of the research problems of helicopters and some of the work which was going on at Boscombe Down, with which he was directly concerned It had been a typical Section Lecture of the Royal Aeronautical Society, and when someone who had done real work in a particular field talked to those who were similarly engaged it usually resulted in his colleagues peppering him with questions and a most interesting discussion Although the lecture had involved a great deal of mathematics, there were many people in the audience who were well qualified to discuss it

Professor H B Squire, who opened the discussion, said that he proposed to confine his remarks to the question, discussed at some length in the lecture, of dynamic stability With single rotor helicopters, such as the S 51, at the higher speeds there was an oscillatory instability He thought that the explanation of this was on the following lines If one used the nomenclature introduced by Mr O'HARA for the static margin,

$$K_n = -\frac{dC_m(\alpha, V)}{dC_T}$$ which could also be written

$$K_n = -\frac{\partial C_m}{\partial \alpha}\frac{d\alpha}{dC_T} + \frac{V}{2C_T}\frac{\partial C_m}{\partial V}$$

where the condition for trim has been introduced For a single rotor helicopter without a tail $\frac{\partial C_m}{\partial \alpha}$ and $\frac{\partial C_m}{\partial V}$ are both positive

Now since the observed motion is an oscillation the coefficient E in the stability quartic is positive and K_n, which is proportional to E, is also positive The second term in the above expression for K_n is therefore greater in magnitude than the first, that is, the effect of speed changes is more significant than the effect of incidence changes

On the other hand the manoeuvre margin H_m could be written as

$$H_m = \left(\frac{dC_m}{dC_T}\right)_V = -\frac{\partial C_m}{\partial \alpha}\frac{d\alpha}{dC_T} - \frac{\partial C_m}{\partial q}\frac{dq}{dC_T}$$

$$= -\frac{\partial C_m}{\partial \alpha}\frac{d\alpha}{dC_T} - \frac{V}{2\mu_1 R}\frac{\partial C_m}{\partial q}$$

Now for single rotor helicopters without tail at speeds which are fairly high the second term is likely to be smaller than the first so that H_m will be negative since $\frac{\partial C_m}{\partial \alpha}$ is positive The observed oscillatory instability is associated with a negative sign of Routh's discriminant (and also a small or negative value of C) which in turn is probably associated with this negative value of H_m

If the above argument is correct in outline it follows that a negative value of $\frac{\partial C_m}{\partial \alpha}$, that is stability with respect to incidence changes is very desirable Of course there are complicating features but he would like to know if Mr O'HARA was in agreement with the general line of the above approach

For a tandem motor helicopter the second term in the above expression for H_m was much more important and the above argument did not apply

Mr O'Hara (*in reply*) said that he was in general agreement with the approach outlined by Professor SQUIRE but questioned whether it was possible to obtain adequate insight into the general dynamic stability from the manoeuvre margin theory which was developed on the assumption of constant speed He pointed out that the work so far done had had the limited objective only of developing a form of stability theory, similar to that by Gates for fixed wing aircraft, for assessing the handling qualities of helicopters in flight tests He had been concerned in other words with *what* the helicopter did rather than *why* it behaved as it did, and he had not yet fully considered the values of the aerodynamic derivatives involved in the stability quartic coefficients These would, however, have to be determined in developing the method to assist in designing helicopters with selected handling properties, and this he hoped to tackle at a later date

With regard to the possibility of a negative manoeuvre margin, it had been stated that if C was positive one would not expect a pitching divergence This, of course, depended on the quartic coefficient B being positive, but this would normally be the case and the manoeuvre stability at constant speed therefore depended on the sign of C He wondered if Professor SQUIRE was suggesting any criticism of the method of approach

Professor Squire pointed out that he had not suggested that the method was wrong, but had merely asked whether Mr O'HARA could amplify it in relation in particular to the observed oscillatory instabilities of single-rotor helicopters

Mr O'Hara replied that he could not at present do so, because they had not yet considered the limited flight data available from that point of view

Professor Squire asked which was the helicopter with the positive manoeuvre margin

Mr O'Hara said that it was the Bristol 171

Professor Squire Has that an oscillatory instability, or do you not know ?

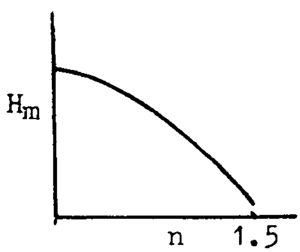
Mr O'Hara I think it is fairly neutral

The Chairman It depends on the speed, does it ?

Mr O'Hara Yes

Professor Squire I was referring specifically to the high-speed end

Mr O'Hara said that they had been working at about the central speed range He thought that the figure would be in the region of 60 to 70 knots, which was in the stable range They obtained rather scattered figures for the variation of stick position with acceleration, and there was a marked fall off in the manoeuvre margin at higher accelerations It was tending to become negative as shown by the curve



He pointed out that the helicopter manoeuvre theory was developed on a slightly more general basis than Gates used, because Gates related the increment in C_m to a finite increment in acceleration and obtained a constant value for H_m, whereas for the helicopter H_m was obtained as the derivative of C_m against acceleration Details of the analysis would be given in a later A & A E E report

Mr R Hafner (*Member—Bristol Aeroplane Co Ltd*) remarked that Mr O'HARA had touched on so many interesting points that it was difficult to know what plums to pick, but there were a few points which perhaps interested him more than others, and the first was the reference to second order blade flapping and vibrations in the blades due to variation of induced flow Mr O'HARA referred to the work of Mangler Mr Hafner sympathised with Mr O'HARA'S view, but felt that perhaps the basic phenomenon was different Mangler had assumed a disc with a certain pressure distribution and calculated from this the induced flow which produced blade flapping very much bigger than that obtained with the simple rotor disc In that way there was at least one explanation of these large blade movements in the higher harmonics, he had the feeling, however, that they had the wrong theory to align their practice with, and he would like to outline the picture as he saw it, which he thought would explain also these higher flapping orders

The point was that they had a limited number of blades, not an infinite number He could not draw what he had in mind, because he would want something like a moving picture to depict it, but a simple way to visualise it was to assume that the blades were travelling across a complex vortex field, which itself was moving down stream This resulted in a lift distribution along the blade rather like that of a busy bridge with a lot of traffic up and down That was the sort of picture which gave high blade strains and high orders of flapping When that pattern was applied to more and more blades one came eventually to Mangler's picture, but there was a significant difference between Mangler's picture with an infinite number of blades and a finite number, because Mangler's induced velocity distribution was only one with space and not time, but with a finite number of blades there was a very important variation with time, and it was the variation with time which seemed to be the most significant When one sat in a helicopter and put one's head to the window, one could feel the pronounced bangs which came to the window due to the fluctuating induced flow He felt, therefore, that the variation with time was far greater than with the variation of space He believed that that was the significant variation and therefore this side of the picture ought to be investigated a little more It would enable them to learn, for instance, the difference between two and three blades, which could not be explained on the basis of Mangler's work It might lead to ways of improving the design of the helicopter and actually reducing vibrations

His second point related to the induced velocity as a function of the axial velocity and of the tangential velocity Mr O'HARA showed in Fig 5 a number of semi-circular curves, and pointed out that there was a difference between the experimental curves and the theoretical circles , the circles related to the momentum theory and the experimental curves to the work done at the Research Establishment Mr Hafner did not think that there was perhaps as much difference as might be thought Mr O'HARA had pointed out that there was a difficulty in marrying the experimental curves in the middle of the picture with the theoretical curves which were at the ends of the picture Mr Hafner felt that the way to overcome that difficulty was the one which he had shown in a paper given to the Anglo-American Conference

He would like Mr O'HARA to comment on that and to say whether it was a reasonable way of looking at the problem

His final point related to the twin-engine as compared with the single-engine performance of these aircraft From the paper, the picture did not look very good, but it might be capable of improvement The proposal had come about chiefly because it was realised that it was very uneconomic to build a twin-engine helicopter with sufficient power to be able to hover on one engine, and they had been forced to look for a better way of safety during flight They had thought in terms of taking off upwards and slightly backwards They were thinking in terms of 60° inclination to the horizontal, which from a performance point of view was vertical flight The idea of going backwards was to ensure that the aerodrome was in front of the pilot all the time

It was a practical approach to the problem, for the following reasons The pilot, during the crucial period, was aware of the danger of engine failure and could concentrate thereon Mr Hafner felt that the assumption of a time loss of 2 seconds was rather large In Chicago a gangster, when trouble arose, could get his gun out in a quarter of a second, and a man who took 2 seconds would be positively unsafe It was to be hoped that there were pilots who were a little more snappy than Mr O'HARA envisaged This could not be seen from the curves, but a great deal depended on the assumptions for delay in corrective action If the delay was short, the picture was not nearly as bad as Mr O HARA made it, while if the delay was great, the picture was a good deal worse

Mr Hafner had made a very much simpler calculation than Mr O'HARA's, who had made a step-by-step calculation of the motion of the aircraft On the basis of a very simple analysis, one obtained the following expression for the lost height H,

$$H = \frac{100}{2g} \left(1 - \frac{F}{A}\right) \left(\frac{A}{W} \frac{g}{100a} + 1\right)$$

where F = power available
A = power required to hover
a = mean horizontal acceleration during the emergency

Mr O HARA's curve dropped very steeply after the acceleration, a, was increased If the pilot was very quick in bringing the second engine up to maximum power after engine failure—presumably the pilot had a warning device, and would be able to bring the second engine up to the maximum power almost immediately—and also if he accelerated forward by applying azimuth control, he would get a rather high mean acceleration Their calculations showed that the height loss for the 173 (not the Mark I but the Mark III, which had a larger engine) was only of the order of 80 ft

Reference has been made, he believed, to a climb of 1 in 2 He felt that the assumption was rather a too severe one

Mr O'Hara (*in reply*) said that in dealing with Mr HAFNER's comments he proposed to start with the final point first He did not think that the performance requirement was for an angle of climb of 1 in 2 This angle arose in the definition of the site, but it only applied up to a height of 150 ft The helicopter took off some distance away from the obstructions in its path and would be able to clear them by climbing away at a shallower angle than 1 in 2 Mr HAFNER's estimate of the height lost in the transition to climb away was 80 ft It would be necessary, however, to add 150 ft to this 80 feet, giving a critical height of 230 ft, which was not very much less than the figure quoted in the paper

He had thought that their analysis of the engine failure performance of the 173 had shown it in a favourable light, but perhaps their hopes were not as great as Mr HAFNER's With regard to the time factor, they had assumed a delay of 1 second before the pilot took corrective action, which was not a long time They had made tests to see how quickly one could react even if one expected an event to happen, and had found that the minimum reaction time was about ½ second, so they did not think that 1 second was an unreasonable time for the pilot to start control action in the event of an engine failure, which was not exactly a regularly expected occurrence It also took some time to carry out the control action, so that the total of 2 seconds to complete control action did not seem too long When pilots had obtained as much practice as the Chicago gangsters it might be possible to reduce that time, but for the present it was necessary to take a realistic figure, and if in fact, they accepted what the medical authorities suggested it would be a time of 3 to 4 seconds

Mr Hafner remarked that if that were the time-lag on the roads, there would be a very high accident rate

Mr O'Hara maintained that it was not possible to reduce the total time delay by much and certainly not to less than 1½ seconds, and since the medical authorities said that it could be considerably more, due weight should be given to their advice, and a longer delay allowed for if possible He agreed that the work involved in their analysis of the engine failure performance was heavy and said they would be interested to have details of Mr HAFNER's simpler approximate method

Going back to Mr HAFNER's second point, Mr O'Hara remarked that he might have said in his paper that their work has been a development of the proposal made by Mr HAFNER in his paper to the Anglo-American Conference An acknowledgement of that had already been made in the original paper by Oliver They had tried Mr HAFNER's method however for low speed performance estimation but it had not been found to give accurate answers

Mr Hafner said that it was right at the two ends, and asked how much it was out elsewhere

https://doi.org/10.1017/S2753447200002092 Published online by Cambridge University Press

Mr O'Hara replied that the results obtained were sufficiently optimistic for it to be concluded that the only thing to do was to determine fully empirical curves and to use these for performance estimation He had not enlarged on the method of fairing the empirical curves into the memontum theory curves but this was in fact a comparatively easy matter to arrange The method of estimation obtained was of comparable accuracy to the f/F curve method for vertical flight, but it applied only to a particular class of helicopter

Mr Hafner said that presumably the curve was obtained on the empirical side by assuming that the profile power was known and that the total power required could be measured, and by subtracting the profile power from the total power one obtained what one wanted In forward flight there was danger in making that assumption, because the profile power in forward flight was variable

Mr O'Hara thought that the errors at low forward speed would be small The vertical flight points were fairly accurately determined and the variation of profile power with speed in the range considered was small , the speed range covered was only up to 30 to 40 ft /sec

On Mr HAFNER's first point, Mr O'Hara agreed that the present theory, based on the replacement of the rotor by a disc, was not fully satisfactory and that the next step was to develop a theory for a finite number of blades

The suggested importance of the variation with time, however, was not entirely clear to him It appeared to be simply a question of interference between the flow patterns of the blades when there was a finite number of them

Mr Hafner said that the Mangler condition or pattern was one which could be established for a given condition and which would not change with time

Mr O'Hara agreed that for a finite number of blades, there was variation as the blades went round Had Mr HAFNER actually developed a theory taking this into account ?

Mr Hafner replied that they had done a few actual calculations, and developed his point by diagrams on the blackboard With a finite number of blades, he said, the induced flow at any one point in the rotor disc varied with time, whereas in the Mangler pattern this was a constant, f His experience was that the variation with time felt much greater than the variation with space

Mr O'Hara remarked that it was certainly an interesting approach to the problem It was suggested in his paper that one of the fields for further work was pressure plotting on blades, which was an experimental way of getting information on the interference between blades

He said he would like to add another point to his reply to Professor Squire's comment on the manoeuvre margin theory This was to stress that the significance of the manoeuvre margin for helicopters had not yet been fully assessed, and it might not prove to be an adequate criterion This would certainly be the case if C were negative, but even for positive C it might still be necessary to go a stage further and consider the build-up of acceleration in a manoeuvre with time, in much the same way as in the N A C A test , a theoretical analysis had been made of the variation of acceleration but numerical estimates were not yet available

Professor Squire suggested that if the manoeuvre margin were found to be negative the design should be changed until it was positive

Mr O'Hara agreed, but added that in the testing field they had to take helicopters as they found them

The Chairman suggested that this was a matter which Professor SQUIRE and Mr O'HARA might discuss between themselves later He added that he did not know how many pilots there were in England who were ex-gangsters

Dr G Hislop (*Member—Fairey Aviation*) said that Mr O'HARA had referred to the growing interest in the use of the combined rotor and elevator control It would be interesting to hear much more about this matter and to know what he thought the main spheres of application were Would it be to improve the stability and or manoeuvrability of an aircraft, and was he thinking in terms of control associated with a fixed or an all-moving tail ? Mr O'HARA had touched on a point which was important for the so called converter plane projects, and if he thought that the use of such a control was a necessity to provide adequate manoeuvring power and stability, particularly at high speeds, it would be of interest to have more details of the main issues involved

Dr HISLOP added that Mr O'HARA's excellent lecture had, somewhat paradoxically, filled him with dismay It had shown the effective work which a small team could do if given the chance, but had also high-lighted the enormous fields which were completely unfilled with knowledge That was particularly true when one thought of the yawning gaps which existed in regard to model work Hardly anything had been done in this country in the way of model tests—and nothing was being done on helicopters That was a very grave omission, and he hoped that those responsible for planning the aeronautical experimental effort in this country would take note of it and repair these vital omissions before it was too late

Mr O'Hara replied that when he said that there was a growing interest in the use of combined control systems he was giving his personal impression from discussion with other people that the use of combined rotor and tail controls had something to commend it, and not only the use of rotor and elevator controls, but perhaps also directional control by means of a rudder The Americans had shown that a tail plane could have beneficial effects on the manoeuvrability and stability characteristics of the helicopter The use of an elevator had not been explored to the same extent, and it was not clear whether the effects would always be satisfactory , it was possible, for example, that the manoeuvre characteristics with an elevator might become dangerous at high speed

Dr Hislop You think that the rotor instability might overwhelm it ?

Mr O'Hara agreed and added that he thought it possible to investigate the effect of adding an elevator by the method of stability analysis which he had outlined earlier He had not yet, however, tackled the problem

Professor A R Collar (*Prof Aero Eng —Bristol University*) said that, since time was short, he would confine himself to one question It related to what Mr O'HARA had said about very low-speed work and flight in difficult conditions He would like to ask Mr O'HARA what importance he attached to the development of some device to measure really low air speeds of helicopters

He put that question because last year, at the University of Bristol, they had started trying to develop a device based on the fairly well known principle that if one put total head tubes near the tips of a rotor, one would get an impact pressure which derived from a combination of the rotational speed and the forward speed, giving a positive increment (relative to the mean) on the advancing blade and a negative increment on the retreating blade The pressure measured at the hub between the two blades would be proportional to the product of the forward speed and the rotational speed Since the rotational speed was fairly high, the pressure difference which could be obtained would be useful even at very low forward speeds, and, in addition, it was linear with forward speed

It had, moreover, one advantage which they had not originally envisaged They had expected, owing to pressure differences, to have very considerable trouble with the rotating seal at the hub , but that had proved not to be too difficult, because although there was a high impact pressure there was also a centrifugal pressure gradient along the blade which theoretically cancelled the impact pressure Thus, if there were an open-ended tube, one end facing forward at the blade tip and the other end at the hub, there would theoretically be no flow through it , and the pressure at the centre was in practice nearly atmospheric

They had been hopeful when they began this work, because the first experiment gave them a beautiful straight line of pressure difference against wind tunnel speed That was for a condition of no rotor disc tilt and no collective pitch That had seemed extremely promising, but as soon as they started applying collective pitch—they had, incidentally, taken the precaution of making the blades flapping—or rotor tilt, the straight line relation developed an unpleasant kink, and with increasing collective pitch this kink steadily moved up the line, so that it was not possible to use the instrument to measure forward speed at all accurately This work had been done by a student, who had since left the university Professor Collar still had the apparatus and the will to do something with it, if Mr O'HARA thought that that particular investigation was worth while

Mr O'Hara replied that there had been for some time a requirement for very low-speed measuring equipment, and they themselves would very much like to have such equipment for testing purposes, because performance measurements had to be made at all speeds The operational necessity for such equipment, however, was less obvious The airspeed was not normally of major importance in low speed landing approaches or when a helicopter was hovering with reference to a spot on the ground He was uncertain whether an instrument reading below 10 knots would be needed for operational use on helicopters There appeared to be more case for a low ground speed measuring device

On the other hand, the Americans were trying to develop low-speed measuring equipment, and they had developed a scheme using strain gauges to measure the deflection of tip plates under air pressure, but the drag power of the plates was understood to be relatively high He did not know whether this system was being developed for operational application , it might only have been for testing purposes B E A had at one time been interested in obtaining an instrument to measure airspeeds down to 5 knots

The Chairman said that they had, in B E A , wanted something for dealing with blind approach and blind conditions generally, because they thought that helicopters must be able to operate under the very worst conditions He still thought that that was correct if the helicopter was going to compete in this country with normal means of transport It must be able to run under the worst conditions of visibility, and if it could not do so it would not be successful Fixed wing aircraft would have to work under very much worse conditions of visibility than at present

Mr J K Zbrozek (*Royal Aircraft Establishment, Farnborough*) The empirical f,F, curve is not a unique curve, but is a function of rotor parameters mainly blade loading distribution (see NACA T N 2474) No doubt in the vortex ring range we have to rely on experimental values, but for hovering and vertical rates of climb there is no " empirical corrections ' to the theoretical curve Using strip theory almost a perfect agreement can be obtained between theory and experiment, as was shown some years ago during downwash measurements under the rotor when hovering

There are two main differences between the stability of fixed wing aircraft and helicopter First, longitudinal and lateral modes of helicopter motion are strongly coupled This difficulty can be alleviated by assuming that the pilot takes corrective action and eliminates, *e g* , lateral modes during longitudinal investigation Second, there is another degree of freedom in helicopter motion, *i e* , the rotor speed In defining helicopter static stability we have to state clearly under what conditions it was obtained The usual stick to trim curve is obtained under constant revs condition, *i e* , the collective pitch varies with forward speed So defined static stability cannot be related directly to the free term in the stability quartic as this is obtained under constant collective pitch conditions It is not very clear to the speaker at the present moment, how the static stability of the helicopter should be defined, but he feels that the trim curves should be given as " partial " trim curves at constant collective pitch, θ plus a trim curve joining appropriate values of θ

It is not very clear if Gates and Lyon s criterion of manoeuvring stability as developed for fixed wing aircraft, has the same significance for helicopters No doubt, in steady turn, at constant speed, the stick displacement is related to parameter C in stability quartic, but to get to this steady condition the helicopter has to go through the transient stage, which is by no means short and negligible, and the pilot is more concerned with the immediate helicopter response to his control movement, and not with ultimate steady state, which could be only obtained by skilful use of controls The American approach, however, unscientific, may be is more along the line of the pilots impressions

The problems of helicopter stability and control are very difficult ones, and the speaker is very glad that Mr O'Hara is brave enough to include these problems in his, already very impressive, research programme

Mr O'Hara, dealing with the question of low speed performance estimation, said that the empirical curves were mean curves for a particular class of helicopter Within their limited range of application, however, they had been found to give reasonable results, and were certainly sufficiently accurate for estimating changes in performance due to small changes in the flight conditions

On the question of trim curves, it was now recognised at A & A E E , and he understood at N A C A , that the correct practice was to obtain partial trim curves at constant collective pitch as suggested by Mr Zbrozek , it was simpler for analytical purposes, however, if the rotor speed was kept constant by use of the throttle It was, however, possible to assess static and manoeuvre margins for variation of both cyclic and collective pitches, the manoeuvre margin in a pull out, for example, was then defined in the form

$$H_m = -\left[\frac{d\,C_m\,(\alpha,q)}{d\,C_T}\right]_{B_1,\,0}$$

$$= \left(\frac{d\,C_m}{d\,B_1}\right)_{C_m\,=\,0}\frac{d\,B_1}{d\,C_T} + \left(\frac{d\,C_m}{d\,0}\right)_{C_m=0}\ \frac{d\,0}{dC_T}$$

There were grounds for thinking that this form was appropriate in considering manoeuvre characteristics, since cyclic and collective controls might both be used, the simpler form of H_m for constant collective pitch appeared the better criterion, however, of the stability in steady flight

He thought Mr ZBROZEK s remarks on the pilots concern with the transition stage in the turn were not (if he understood them correctly), fully relevant, because he was dealing with the longitudinal characteristics and introduced the steady turn state only because it could be easily related to the steady pull-out state He realised, however, as indicated in his reply to Professor SQUIRE, that it may be necessary to consider the transition stage in the pull-out

Mr J Shapiro (*Founder Member—Consultant*) commented on the fact that in listing the various avenues of research to increase speed Mr O'HARA had not mentioned research into blade profiles That, he thought, was one of the most fruitful approaches, and even so simple a modification as the substitution of a non-symmetrical for a symmetrical profile would do as much as the 2nd harmonic control and more, whilst in most respects it was much easier There were many profiles holding some promise and eventually boundary layer control had great possibilities Contrary to the lecturer, he had come to the conclusion that they knew enough about induced flow They must husband their resources, and he did not see any very great prospect of getting anything out of refinements in the theory of induced flow Harmonics may mean no more than the fact that a stop function contains all the harmonics in the world, but that did not mean that they were all important, or that whenever one started speaking one excited every string there was The question was how strong was the excitation and how much damping there was

He thought, therefore, with regard to the effect of induced velocity on blades, that the kind of thing which the Americans were doing in investigating blades of different stiffnesses and different response was a very much more fruitful avenue These matters, which involved very complicated physical problems, should be approached from the practical angle of how to design the blades, and the right approach was to investigate blades of different characteristics

He felt that the question of reserve power, performance with one engine inoperative, and the question of design for fatigue could be much better co-ordinated in future designs Personally, he had come to the conclusion—he did not propose to go into the details—that the correct number of engines was three That was a very broad conclusion from a very large number of factors He believed that if they chose a reasonable compromise between (*a*) performance, with one engine inoperative, (*b*) good maintenance, *i e*, low cruising power related to take-off power, and (*c*) fatigue strength of transmission for continuous running compared with its maximum strength, they would come to a very nicely balanced helicopter with three engines The details have to be explained in hours, but not in minutes

Finally, he would like to say a word on the question of approach and take-off, and its importance He was very glad to find that Mr O'HARA was now examining vertical take-off, because he thought that vertical take-off and approach were the only proper methods for operating into built-up areas They might find, if they examined the matter from the aerodynamic point of view, that there was a slight benefit in an approach different from the vertical, but if they took into account the fact that the approach aids, with a vertical approach, could completely disregard wind direction, and therefore could be very much simpler, cheaper and easier to operate, they would come to the conclusion that the approach in the first 300 ft should be compulsorily vertical They would have to take steps to make it exactly vertical, in order to make all approaches the same and be always ready with their approach aids

Mr O'Hara said that Mr SHAPIRO had mentioned other possibilities for high-speed flight than those he had included There were certainly possibilities in the use of different blade profiles and probably also in the case of boundary layers control, but it was most important for work to be done to assess what the gains from these various schemes would be In the case of 2nd harmonic control, for example, there was still no definite estimate of what gain in speed would be achieved He was in favour of any scheme which had something practical to offer There would probably be general agreement that there was great need of model work to investigate different systems and to compare slight variations of the same system, dealing with families of rotors and blades, not only from the aerodynamic but also from the structural and stiffness points of view

On the question of the number of engines, he did not know what basis Mr SHAPIRO had for his suggestion of three, but he himself thought that three would be inconvenient, and that it would be better to have two groups of two, particularly on a tandem With a tip jet machine there would of course be a multitude of power units and the failure of one of the tip jets would have to be considered

With reference to Mr SHAPIRO's comments on take-off and landing, he felt that the helicopter should be able to take-off and land vertically with safety That had been the object when helicopters were first conceived, and it should still be a main aim

Written contribution to Mr O'Hara's Paper on "Helicopter Research"

By

P R PAYNE (*Member—Auster Aircraft, Ltd*)

Most research workers will agree that the primary aim of research should be to furnish Industry with information which will be of direct value in the production of future designs Because of the disparity between research and design efforts in this country, the main burden of research falls upon the Industry, and the work done is often of limited value because it is carried out under the stress of direct economic necessity Moreover industrial research tends to be retrospective , for example, when a given design is found to have unpleasant characteristics, the problem is not that of establishing the fundamental causes, but merely of eliminating their effects on this design Such research is of little value to the designer "Government" organisations therefore carry a great responsibility for maintaining a wisely apportioned programme of long-term research It would be pleasant to say that such a programme was maintained, but in fact the amount of money allocated and the actual work done is pathetic and lamentable respectively

I should like to make one general and one particular observation in this connection

Mr O HARA has devoted the first half of his paper to items which appear to him to be "major items for research in the general field" With one exception, I should class them as items about which *sufficient is known* for the time being, in the face of more pressing needs upon our research time

An example of a more pressing need is the exception noted , vibrations and fatigue It is a sobering thought that, although rotor vibration limits the speed of many British helicopters, not one investigation into the fundamental causes of this vibration has been published, despite which designs have been produced for which much higher cruising speeds are confidentially prophesied In fact only one quantitive reference to vibration has so far been published in this country, to my knowledge, and this ignores Coriolis forces (which are twice as great as any other) and neglects the effect of drag hinge articulation (which increases the vibrations by between 200% and 300%) Yet once the fundamental causes are established, it is often possible to eliminate a hitherto troublesome phenomenon A simple example of this is provided by the suppression of in-plane vibration of a two-bladed rotor, illustrated in Figs (*a*) and (*b*) The individual force components at the hub are shown in Fig (*a*) Since Coriolis force (due to blade flapping) is a function only of the flapping amplitudes relative to the mechanical axis, it is possible to suitably incline this axis in the design stage so that the Coriolis forces oppose the aerodynamic ones In the example given, the inclination is such that the total force variation due to each blade is as near a true second harmonic as possible Thus, as Fig (*b*) shows, the resultant hub vibration is very small indeed