Discussion

Dr J P Jones (*Southampton University*) said that his remarks would be addressed to Mr Jones because they were more on the dynamics and not on the stability side

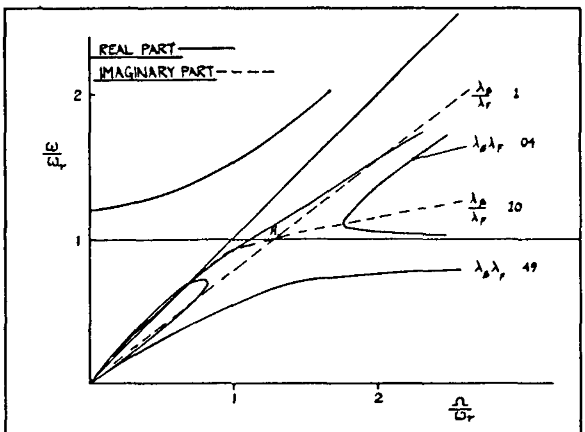
He thought that the explanations about the effects of friction on stabilising motion were very interesting, but what he would like to hear explained was why the increase of damping from 10 per cent critical to 50 per cent critical caused instability to appear What was the cause of the region of violent instability at 50 per cent critical damping ?

His next point was connected with the flutter results which Mr Jones had obtained They disagreed violently with his own He had done some flutter calculations with tapered blades and constant chord blades for the first and second moments of inertia and had found that the tapered blade was distinctly less stable than the constant chord blade

Another point was that he found that the blades which he chose, which were more or less representative, would diverge rather than flutter but he had not realised that blades tended to have the flexural axis so far forward of the quarter chord He had thought it was about 2 per cent

There was something else which they had discovered recently They were doing tests on a blade flutter model, and they found that if the blade bent appreciably—they used one of very low bending stiffness—there was a very sharp drop in the effective torsional damping That gave flutter which was literally in 1° of freedom for a large number of rotor speeds He wondered whether Mr Jones had experienced anything along those lines

Mr Jones (*Bristol Aircraft Ltd*) *Member*, (in reply) said that to illustrate the effect of damping one could look at the Coleman diagram For simplicity, the case of a rotor mounted on an isotropic support could be considered Fig A showed the values of the frequency ratio $\frac{\omega}{\omega_r}$ and the rotor angular velocity for which the real and imaginary parts of the equation of motion were separately equal to zero



PLOT OF REAL IMAGINARY PARTS OF THE EQUATION OF MOTION $\mathcal{A}_1$ 053, $\mathcal{A}_2$ 0, $\mathcal{A}_3$ 058 (COLEMAN'S NOTATION)

Fig A

The boundary between stable and unstable motion was given by intersections of the imaginary equation with the two loops of the real equation

Increase in the non-dimensional damping λ_β λ_F (where λ_β, λ_F are the ratios of the drag hinge and of the fuselage damping to the critical damping in each case) caused the noses of the loops of the real equation to move together, whereas for large values of the ratio λ_β/λ_F, one root of the imaginary equation approached the line $\frac{\omega}{\Omega} = 1$ so that the unstable range tended to extend to infinite Ω Decrease in λ_β/λ_F caused this root of the imaginary equation to intersect the upper loop of the real equation nearer the nose, giving the smallest unstable range at about $\lambda_\beta/\lambda_F = 10$ Further decrease in λ_β/λ_F gave an increase in the upper limit of instability and in the frequency of the limit

The usual difficulty was to obtain sufficient drag hinge damping, so that one tended to have λ_β/λ_F rather small and this gave a shallow intersection between roots of the real and imaginary equations That is, the stability boundary is very sensitive to changes in λ_β This sensitivity was complementary to the nature of the roots of the equation as one passed across the boundary from a region of stable to unstable motion

In the case of very large λ_F and small λ_β, where the product of damping $\lambda_\beta \lambda_F$ exceeds that needed to make the real equation pass through the point A, the " common real point," intersection between the real and imaginary equations occurred to the right of the point A, (if λ_β/λ_F is sufficiently small for an intersection to occur at all) and the instability extends to infinite Ω This was the case where the most violent instabilities are encountered and where the equation was in fact ill-conditioned

The important thing was to maintain an adequate ratio of damping In the present example a value of 10 was most suitable

Most literature which they had seen up to date dwelt on the product, but the ratio of the fuselage to rotor damping was equally important

With regard to flutter, both of their blades, which were actual blades, had a flexural axis in the position shown , it was not possible to move it back He thought that that was probably responsible for the results which they had obtained

Dr Jones asked how large the separation was

Mr Jones said that it was 12% chord for the metal blade Apart from the theoretical work, the practical work which had been done full-scale on actual rotors was difficult to interpret, but they had found that the flutter speed dropped a good deal when they increased the pitch For instance, a rotor having a flutter speed of 30 rads/secs for 8° of pitch, if they increased the pitch to 14°, the flutter speed dropped to 20 rads/sec This may have been aggravated by the onset of periodic stalling caused by the blade motion

Any more information on the effect of blade bending or the flexural axis location on flutter would be much appreciated

Mr V A B Rogers (*Fairey Aviation Limited*) said that his remarks would be addressed mainly to Part II of the Paper

First, he wished to ask Mr Jones for a little more information about the lag plane oscillations which had been encountered on the blade This referred to the transmission part of the Paper where Mr Jones said that the pilot had been able to induce oscillations in the fundamental blade mode He had no disagreements with that statement, but what he would like to know was whether the amplitude of the oscillation had increased with forward speed In other words, for a given input of the cyclic control was it also a function of the associated differential forcing loads Also, was there any indication that there could be lag oscillation for a stick fixed condition (i e the rotor being forced by gusts)?

Turning to ground resonance, he would like Mr Jones to define a little more precisely what he meant by " rigid body modes " He agreed that the values of all the body natural frequencies in the motion must be low Also did that imply that any other body modes which Mr Jones might consider to be unimportant modes were those which gave instabilities above the running r p m ?

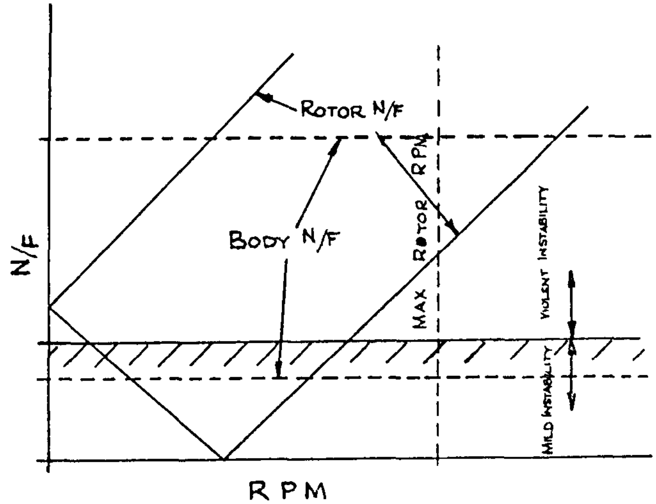
He completely agreed with all that Mr Jones had said with regard to the fact that one had to have low natural frequencies of the body on its undercarriage to avoid instability On the Fairey Rotodyne they had no dampers as such at all, and so he could not get involved in any discussion as to the magnitude of the damping involved to eliminate instability, but he thought that a rather useful rule of thumb had come out of their investigations and he saw that it had also come out of the work that Mr Jones had done Provided that one could keep one's body natural frequencies less than half the value to give instability at maximum rotational speed (body natural frequency approximately at one quarter of maximum rotational speed)—one could, in fact, get theoretically, unstable modes which, as Mr Jones said, produced a very mild instability on the simulator, which it was quite safe for the helicopter to run through Although he had no argument with Mr Jones about the different factors involved in the production of the N/F—rotational speed diagram to which Mr Jones had just referred there was another way of looking at it which might be quite important Taking a virtually undamped state, one found Mr Jones's diagram could be approximated to by the lines drawn in Fig B The horizontal lines being the body N/F's and the sloping lines the resolved components of the Blade N/F

Usually the instability regions occurred in the vicinity of the inter-sections of the body N/F lines with the right-hand sloping rotor N/F line

It was useful to have some rough and ready rules in this very complicated subject since it took hours to plot the true instability diagrams, whereas one could obtain the approximate diagram in a matter of minutes, once the body and blade characteristics were known

On the basis that one had to keep below a boundary as indicated in Fig C and at a value of body N/F previously mentioned, one got the phenomenon which Mr Jones had been discussing, and one found that the type of instability became very critical to magnitude of the body N/F One of the effects of increasing the body damping when near to this critical value was to increase the effective body N/F, and that could put one in real trouble How theoretically correct that was he did not know, but it bore out what Mr Jones said Perhaps it had a more practical slant to it

Fig B



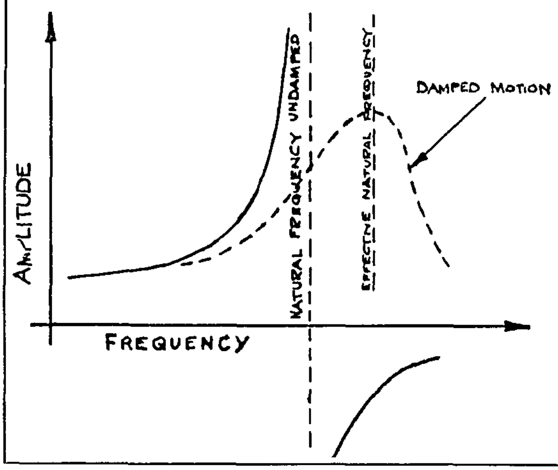


Fig C

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It might also answer the question put by Dr Jones, who asked why, when the damping was increased there was violent instability

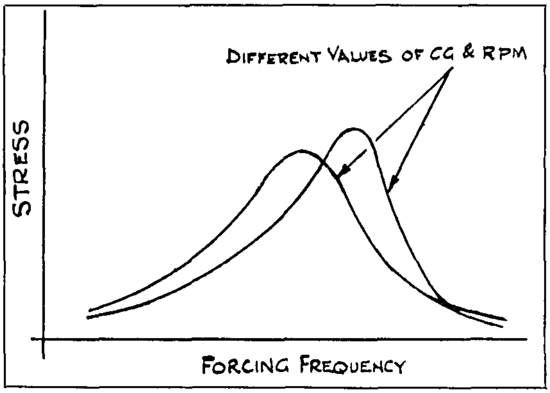
With regard to flutter, at Fairey Aviation Limited they had been carrying out certain experiments by trying to induce flutter on their rig at Boscombe Down The meeting might be interested to know briefly what they had been doing there (although they had not completed their investigations) because he was in a little disagreement with Mr Jones when he said that he got adequate warning of flutter by blade twitching One could get a violent flutter without any indication that it was going to happen when one had definitely set out to produce it Therefore, one should have to be very careful about that statement

On the Rotodyne they also had steel rotor blades The flexural axis was forward of the centre gravity The reason for that was a structural matter If one had a rotor blade with a non load carrying trailing edge the mass that one put in the leading edge to put the centre of gravity in the right place was also part of structure and it was therefore, impossible to get the flexural axis behind the C/G (The condition investigated by Dr Jones)

Turning to the flutter tests, what they had done was arbitrarily to shift the C/G of the rotor aft by putting weights in the back end of the tail cone of one of the blades They had four blades, and they had driven two and left the other two undriven, and that had given them a ready means of putting the C/G aft

They had plotted torsional stress in the blade against the forcing frequency That was by stirring the stick at various frequencies and measuring the response for different magnitudes of aft shift of the C/G They had obtained curves of the type shown in Fig D

Fig D



They had hoped to get an indication from the amount of damping in the modes when they approached a flutter condition The net effect was that they had found almost negligible change in the damping It had been a very insensitive way of trying to indicate the approach of flutter

They had now plotted the peak amplitudes of these curves against rotational speed for different C/G positions, and they had produced curves of the type indicated (Fig E)

All these results were obtained by actually forcing the stick In previous tests when the stick was jerked, they could not obtain any response at all With a " Bonker " force of 300 lb they could not introduce the slightest oscillation whatever in the blade During the stick jerk tests flutter was however accidentally obtained but they were able to get within 5 r p m of the flutter conditions without a murmur from their strain gauges, but by only 5 r p m increase they hit flutter and had to shut down very quickly or they would have had the blades in their laps

They had not yet decided however whether the flutter speed previously obtained was the asymptote to the forced oscillation results

He hoped that what he had said added a little to what Mr Jones had been telling them

His last point was with regard to the first part of the paper, but it was more in the province covered by Mr Jones It was with regard to the introduction of a six-bladed rotor rather than a four-bladed rotor All who were concerned with the matter knew

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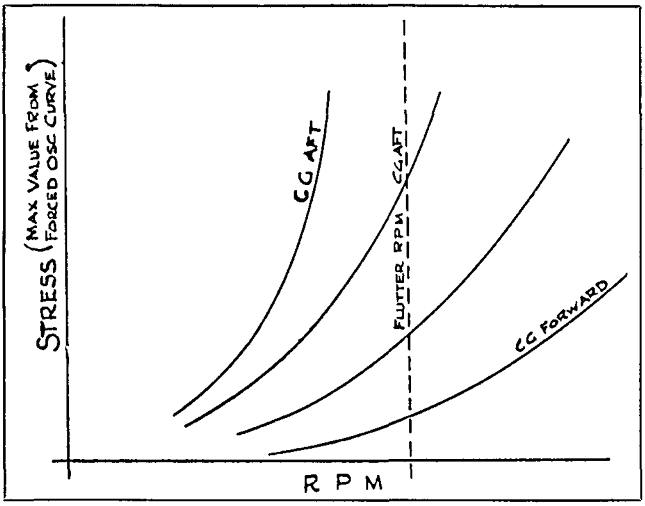


Fig E

that the more blades they put on the less chance they had of any large magnitude of oscillations coming through Theoretically, they would like 100 blades, but since that was practically impossible they had to put up with what they were given He would be interested to know whether the choice of six blades had resulted from the fact that one of the facts of life with helicopters was that their body natural frequencies tended to be in the region of fourth rotor He felt that the idea of going to a six-bladed rotor might have been occasioned by that thought Since he had been well-informed by his firm's experts that going to a six-bladed rotor created the condition which had been mentioned by Mr Sibley earlier, that the lower the blade loading, the greater was the profile drag, and this was not to be encouraged

Mr Jones (in reply) said that, with regard to the point about the transmission modes, the comments that he had made were in reference to the torsional vibration of the transmission system and not the torsion of the blade throughout the pitch axis

Mr Rogers said that he did not mean that He meant that it was the torsional motion of the transmission system with the blade oscillating in its plane Was that mode being induced by the application of cyclic pitch ?

Mr Jones (in reply) said that the point was easily answered, because the worst condition by far was the flare-out, as this was at the time when the pilot had most work to do in stabilising the aircraft In forward flight it was hardly any trouble at all The total force vibration level in the synchronising shaft was about 5%—$\pm 2\frac{1}{2}$%—of the design strength of the shaft

With relation to the experience of stick twitching they had been unable to get flutter of the blade at the hovering condition, either on the helicopter or on the rotor tower They had deliberately made the blade tail heavy and had also gone right up through the pitch range available still without flutter or stalling The rotor speed was also 20% over their flight maximum speed

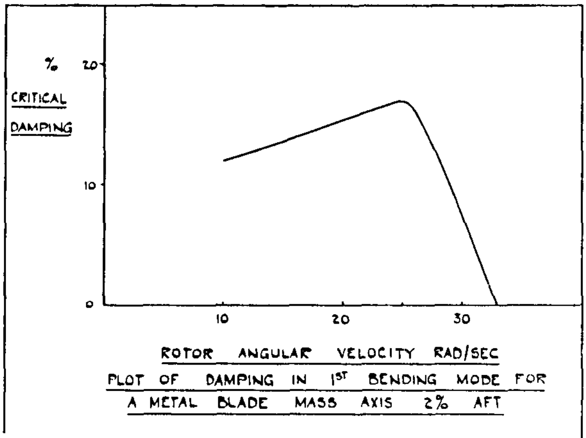
However, their experience in flight was definitely that of a twitch He wondered whether because the aircraft was flying forward the flutter could be a periodic occurrence due to running into a gust and then running out of it, but he just did not know

He was very interested to hear the comments about the Rotodyne in connection with ground resonance He and his colleagues felt that Fairey Aviation Ltd must have gone to some trouble to reduce the damping in the fuselage modes By " rigid body modes " they referred to modes in which for practical purposes the deformation of the aircraft structure could, in fact, be neglected There was no instability on the Type 192 due to modes in which significant bending occurred

The rule of thumb used by Mr Rogers was very useful for a quick assessment It did imply though, a suitable balance between the rotor and fuselage damping properties If this was not so, such a rule would lead one astray

Mr Roger's experiences with flutter were of great interest Perhaps one further

Fig F



point could be added in this connection Plotting the damping in the 1st bending mode against rotor angular velocity, one obtained the curve shown, in which damping increased gradually with rotor speed, reaching a maximum, above which the damping fell off very rapidly to give flutter (Fig F) This was a theoretical prediction and tied up with Mr Roger's evidence One wondered, nevertheless, whether any of the exercises discussed, which all related to particular blades, could be treated as generalisations He rather doubted it

There was one more point about the six-bladed rotor It was recognised that the principal object in going to six blades was to reduce the force vibration in the fuselage It was also their desire from Mr Sibley's point of view—Mr Sibley would enlarge on that point—to go to a higher disc loading The two requirements fitted together

Mr Sibley (*Bristol Aircraft Ltd*) *Member*, (in reply) said that it did not really matter from an aerodynamic point of view whether one had four, five or six blades The profile power is a function of the operating CL of the rotor blades, which is almost independant of the number of blades providing the Blade Reynolds' number is above $3\text{–}4 \times 10^6$

It was purely from the vibration point of view that one should have an even number of blades and as many as possible By using a wing, the rotors can be unloaded in forward speed with a resulting saving in rotor profile power

Mr G F Langdon (*Boscombe Down*) said that he ought perhaps to begin by saying that his views were his own and probably not those of the Ministry of Supply

He asked Mr Sibley to expand a little more on the lateral stability of the tandem At Boscombe Down they had had experience of the 173 from that aspect As Mr Sibley had said, with the final version of the tail plane it ended up stable, but the damping was still about half that of a Sycamore He understood that the 192 would end up slightly unstable, and he wondered whether that was regarded as acceptable He did not know, but he would think that it would add to the work load of the pilot considerably, particularly when flying on instruments

Mr Sibley had confined himself to a discussion of the effects of dihedral and weathercock stability Those were things which were easy to alter by means of fixed surfaces, and one could do it while one was flying fairly fast He asked that Mr Sibley should expand on the possibilities of increasing the roll damping, perhaps by increasing the flapping hinge offset, or of artificially increasing the yaw damping It seemed to him that those possibilities ought to be examined for future tandem helicopters, where playing about with end fins might not give them a sensible answer It might be possible to improve the lateral stability considerably by using simple gyroscopic damping devices without going to the complication of a full autopilot

Mr Sibley (in reply) said that the first point made by Mr Langdon concerned the Sycamore being approximately twice as stable as the 173 in its final form He thought that the fundamental difference between the single type of rotor helicopter and the tandem was rather a difference in ratio of the rolling moment of inertia to the yawing moment of inertia The first thing to be remembered was that the tail rotor was quite a good damper in yaw and probably did not introduce much roll coupling The moment of inertia for yaw and roll might be of the order 1—5 for the single main rotor type compared to 1 in 30 for the tandem , any serious lateral instability tended to go out in the rolling plane rather than the yawing plane

As far as the type 192 was concerned, there was no intention of it being an unstable aircraft He considered the basic requirement for any helicopter is that it shall be inherently stable in cruise, climb and approach without auto-stabilisation aids, particularly if the helicopter is multi-engined with a positive engine out performance By fitting an auto-pilot-cum-autostabiliser the aircraft would have reasonable "all weather" capabilities, at least for making an approach into a small Airfield, although if the requirements were for operation into a small helicopter landing site, it may be necessary to fit a standby auto-stabiliser, dependent on the degree of inherent stability achieved for the approach condition He considered the alternate approach for an inherently unstable helicopter was full triplication of the auto-stabiliser

With regard to Mr Langdon's suggestion about the servo damping devices, they had considered this type of system

No doubt it could be made to work, but it was not the sort of thing he would like to try to do quickly

He had talked to fixed wing designers about their aerodynamic servos and he had discovered that it was sometimes necessary to design and tunnel test such a system a number of times before getting it satisfactory The finesse of balance of such things was rather tricky and so at first sight they decided to leave that for longer term consideration He considered that an aerodynamic servo for yaw was a possibility, but for rolling control, the signal was too small for acceptable limits of roll, unless a large span tail plane was employed

He had seen a paper relating to a simulator study on another tandem helicopter using an aerodynamic servo rudder It appeared to be satisfactory, but looking at photographs of the aircraft, he had not seen any sign of the system being installed

The use of offset flap hinges certainly provided damping in roll, but modifications to an existing design would be costly compared with a fixed aerodynamic surface

Mr D K Rees (*Fairey Aviation Ltd*), commenting on the first part of the lecture, said that Mr Sibley in his introduction stated that comparison between tandem helicopters and other configurations would be avoided That being so, a great deal of the ground for contention had been removed

The first point that he wished to take up was that of the hovering power requirement The tandem helicopter was stated to have a lower hovering power requirement due to its lack of torque compensation That was quite true, but the tandem fuselage extending under the working regions of both rotors must experience a download greater than that experienced by a single rotor helicopter, and that must be reflected adversely in the power requirement

He hardly needed to point out that a tail rotor was not a necessity, although one had to admit that yaw control must be paid for with power losses unless it came as a by-product of the main configuration as in the case of a tandem helicopter

In the pre-print of the lecture it was claimed that a tandem helicopter had an advantage in fuselage parasite drag due to the long narrow fuselage That might not be wholly so, because a single rotor machine, being more compact, might have a smaller surface area weight for weight, and certainly two rotor heads might be worse than one from a drag point of view

On the subject of blade parasite drag, he would agree that the variation with μ took the form $1 + K\mu^2$ with values of K of the order given At higher μs, however, a certain amount of cubic variation with μ should be included, although that might be masked by fuselage parasite drag

With regard to the achieved tip Mach No 0 77, he wished for some information about how that was obtained, whether it was by overspeeding the rotor at moderate forward speeds, at low tip speed ratios, or with normal tip speed at a high forward speed

In the discussion of retreating blade stall and in Fig 10 an aircraft of autogyro configuration, the Rotodyne, had been referred to without sufficient discrimination He wished to point out that the autogyro stall was progressive in character and a much less severe boundary to high speed flight than the helicopter stall, and, indeed, the boundary indicated on the graph was by no means applicable to autogyro flight

The proposed 200k tandem aircraft with a lift-to-weight ratio of 0 45 would require a very large built-in shaft tilt if the necessary forward propulsive force was to be obtained with a moderate flapping angle That would be inconvenient at the low speed end of the flight, and the only alternative would be a nose-down tilted fuselage with the wing set at a high angle to the fuselage, and in that case the wing might be stalled at lower speeds and that would be unacceptable in a tilted aircraft

Mr Sibley (in reply) said that he had allowed for vertical drag of the fuselage in the performance calculations He had assumed that the single rotor helicopter had a smaller C G range than the tandem and hence a wider fuselage for a given cubic capacity The wider portions of the fuselage for the single rotor helicopter would be in the downwash and hence the vertical drags of the two types would be of the same order He agreed that the surface area of the single rotor helicopter might be smaller, but making the same assumption for C G ranges the greater fuselage cross section would give rise to a higher form drag If one could design a single rotor helicopter with the same C G range as the tandem, keeping other drag producing items the same, then Mr Rees' point would be true However, he did not wish to make an issue of the point, and that was why he had quoted the same values for both types of helicopter in the graph

With regard to the blade profile drag, they had made a number of partial climbs, (20 or 30), with the particular helicopter, and had analysed the fuselage drag by a method proposed by Bartholomew This had given them a fairly good mean value for the parasite drag, which together with the assumed induced drag had allowed them to obtain the rotor profile drag for the rotor

The Chairman said that Mr Sibley got a μ to the fourth term when he took into account the retreating part of the blade where the leading edge became the trailing edge

He agreed with Mr Rees and the Chairman that in more rigorous analysis of the blade profile term there were higher values of μ than the square term, but these terms were not included in the preliminary analysis which was intended to give some empirical data for initial project work

Tip Mach No of 0 77 was achieved in forward flight They were flying at 110 knots and 260 r p m

He wished to apologise for any adverse criticism which might be implied from the "guestimated" operating conditions for the Rotodyne in the preprint As he had stated in the lecture, Fairey's had very kindly supplied the information that their aircraft was operating with between 50 and 75% of the total lift carried on the wing Since the boundary in Fig 10 was obviously not too exact at high tip speed ratios, and the band of Wing Lift/Weight given for the Rotodyne was fairly wide, then it was rather difficult to conclude on which side of the boundary the Rotodyne was operating

Mr Rees said that it was indicated as being Mr Sibley's boundary, and contended that the boundary did not exist for Fairey Aviation Ltd

Mr Sibley said that this was presumably on the grounds that autogyro was stalling from the root outwards

Mr Rees said that that was so

Mr Sibley said that he agreed in principle for lower values of μ, but when one got up to high μ's around 0 5 and 0 6 a very small amount of the blade was involved He did not think that there was a great deal of difference in vibration levels from rotors operating at high tip speed ratios whether in the helicopter state or the autogyro state

Both were operating at high C_L's over a very short length of blade The autogyro had a fall-off of C_L towards the tip, which was fairly slow, while the reverse occurred with the helicopter If blade stall at high μ's was producing a critical condition, the autogyro would have a slight advantage due to the rate of change of effect on the blade on approaching the stall would be slower with an autogyro

He advocated working below the stall boundary

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Incidentally, he imagined that the Rotodyne basically operated on an almost constant C_L curve against μ, since it started off with a high r p m and dropped off with speed

He originally thought that the Rotodyne was operating across the stall boundary However, since Mr Rees was operating at the revised level, he was presumably agreeing that basically one wanted to keep below the boundary

Mr R G Austin (*Bristol Aircraft Ltd*) *Member*, said that he wished to enlarge on two points

With regard to the comparison of fuselage drag as between the single rotor machine and the tandem, equal drag figures had, in fact, been taken , but if one looked into the matter more deeply, the tandem rotor fuselage would usually have less drag for a given volume

The single rotor fuselage would have a lower slenderness ratio and therefore greater super-velocities If both fuselages were " clean " then there would be little difference in their drag values , but as in practice such things as hubs, undercarriage struts, door handles, wind-screen mouldings, aerials, vent pipes etc poked into the super-velocity breeze, he believed that the slender fuselage had an advantage Further, with the thickened boundary-layer, pressure recovery behind a fat fuselage would be difficult

There was also the question of the lift distribution on the rotor in the helicopter state compared with the autogyro state at high forward speeds It had been suggested that the autogyro had the advantage here This, however, is usually a matter of designing the right twist into the blade for the inflow condition In practice this is not entirely possible Thus with increasing forward speed, the autogyro tends to reduce the load on the retreating tip and increase that on the advancing blade tip , while the helicopter has the opposite trend The helicopter then is limited more by retreating blade stall and the autogyro more by advancing blade shock stall The resulting maximum forward speed is much the same

The **Chairman** said he wondered whether those present had read Mr P R Payne's new book on " Helicopter Dynamics and Aerodynamics " Mr Payne had discussed the question of the profile power increasing as $1 + K\mu^2$, and suggested in his book that even for $K = 3$ the profile power was over-estimated, in spite of the usual value being 4 65 It was important to note that Mr Sibley's experimental results were in conformity with the classical theory, and not with Mr Payne's suggestion

Mr Payne had asserted that the value 3 was in close agreement with practice, and he had published a curve showing the variation of profile power with μ The experimental values of the required power in level flight, however, were greater than those obtained from the assumed value of K, and the difference was attributed to hub drag If this were true, the tandem helicopter would be at a disadvantage, having two hubs

Mr Payne in his book discussed quite a number of other topics, such as the question of ground resonance, and effects of hinge restraint He wondered whether the author had had any experience of other methods of blade mounting such as had been used experimentally in the Autogyro, especially hinges without any friction dampers The use of a drag hinge with an " alpha two " inclination, giving a flapping component of motion about the hinge had successfully elimated ground resonance A similar result had been achieved using a spring restraint These methods indicated that no drag hinge, was necessary, the natural flexibility of the blade providing displacement with adequate elastic restraint as in the Rotodyne He wondered whether the author had any experience of such alternatives

Mr Sibley (in reply) said that in relation to the question on blade profile power, he was convinced that the total power required in forward flight rose at a greater rate than predicted by classic theory He had analysed the results based on an assumed induced power and a measured mean fuselage drag Obviously this was not exact theory , as one reached higher values of μ the second harmonic flapping caused less lift on the advancing and retreating sectors, and more on the fore and aft sectors, so that the simple lifting line theory assumption that they had an elliptic loading fell down If one considered the simple concept of the mean thrust pattern squashed on to the mean lifting line, the result was no longer elliptic but became flattened out at the end of the mean span, and hence the induced power went up He was initially trying to get some sort of working formula for high speed helicopters, something which gave a reasonable estimate rather than the flights of fancy which resulted from assuming classic theory with $\mu = 3$

With regard to the point about hub drag, the Chairman had said that the tandem with two hubs had a worse hub drag One had to remember that each hub was taking less torque and thrust, and, therefore, the hub in proportion was smaller

The **Chairman** said that the suggestion about the importance of hub drag arose from Mr Payne who in his book accounted for the difference between K = 3 and the practical value of K (viz 4 65) by attributing it to hub drag If that were so, the tandem rotor helicopter would be at a disadvantage However, Mr Payne's statement had been refuted by Mr Sibley's experimental results

Mr Sibley said that they were going to do some tests in the near future to find out the drag of a revolving hub

They had not deliberately used the other hinge co-ordinates, the α_1 and α_2 There were small components present on the hub which came about due to the general geometry, but they were not of primary importance unless one was dealing with something like governor control on the engine, in which case one would have to consider them

The **Chairman** said that α_2 would usually have to be about 50 or 60° to be effective

Mr Sibley said that the values concerned were very small

Mr Jones (in reply) said that with regard to Dr Bennett's experiences of ground resonance with autogyros, he knew of no further work in making use of α_2 hinges He wondered what effect the hinge would have It would appear that due to the phase relation between the motion of individual blades about their lag hinges, one effect would be, to produce a tilt of the thrust sector at the frequency of the fuselage motion The lateral component of this force would be proportional to rotor thrust and would not be available at zero thrust He felt that if Dr Bennett had been able to get out of trouble by this means without knowing why, he must have been very lucky

Mr Rogers said that, since many of them rather disagreed with Mr Payne on his fundamental concepts of classical dynamics, he wondered whether what he had said on ground resonance meant very much

On the motion of the CHAIRMAN, a vote of thanks to the Authors was accorded with acclamation