ψ	=	azimuth angle of blade
ϕ	=	inflow angle
χ	=	first moment of mass number = $Mm/\frac{1}{8}\rho R^3 a$ Co
ω	=	rotor angular velocity
Ω	=	a circular frequency

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Discussion

The **Chairman** referred to the Author's suggestion that some of the vibration could be absorbed by a pendulum He had seen a photograph in a Dutch periodical recently, of a new Italian helicopter which appeared to have pendulous weights attached to the hub This had mystified him at the time, but possibly they were devices of the nature referred to by the Author

The use of metal blades seemed to be regarded as the panacea for all ills, but the Chairman recalled remarks made to him by the engineering officer of a Canadian Helicopter Squadron which he visited last year The Squadron had small tandem machines—Piasecki HUP's—which had been delivered to them with metal blades They had had a great deal of vibration trouble but had after been overjoyed by the performance of a new set of blades which had been produced to overcome those troubles—wooden blades This was contrary to the usual run of experience (and of hope !) , it had surprised the CHAIRMAN, but he had been assured that the aircraft were much smoother with wooden blades

Mr V A B Rogers (*Fairey Aviation Company*), said he thought the Author had put the helicopter vibration problem very clearly by splitting it up under the headings of Fundamental Vibration and Control Vibration, and he wished to comment on the problems associated with fundamental vibration which were discussed in the written Paper

When considering loads from the rotor, he thought that although the aerodynamic forces might be given adequately by rigid rotor theory—although he doubted whether sufficient was known about the effect of the aircraft body on the induced velocity at low aircraft speed for the true oscillatory air loads to be calculated accurately—the whole level of the oscillatory loading could be changed completely, especially in the absence of drag hinges, by the dynamical effects associated with blade flexing which the Author had gone to some lengths to discuss

Further, flight strain gauge results showed that there was still very little agreement between calculated and measured loads on the speed range zero to 70 knots Thus, taking all the above effects into account, there was still a long way to go before they could calculate true vibratory load , and thus he could not see how rigid rotor theory could give—to quote the Paper—" reasonably accurate results "

He agreed that the point about a variable mechanical axis would go a long way to reduce the general level of vibratory forces, since the Coriolis forces could be used to cancel out some of the air forces , but he thought that even with a tip driven rotor there would be a certain amount of mechanical difficulty It would be mechanically difficult, or at least heavy, since all the flight loads were transmitted through some type of gymbal Moreover, they must not forget that unhealthy gyroscopic forces were brought into play when the axis was tilted, and this led to a lot of structural problems

He was pleased that the Author, in dealing with Coriolis force, had now mentioned that no resultant constant force was obtained at the head when the Coriolis force was resolved into fixed directions, since it cancelled out identically with the resolved part of the oscillating radial force associated with the flapping motion Although the Author attributed this information to help received from Mr Brotherhood, in fact Mr ROGERS had pointed it out some time ago in a letter to *Aircraft Engineering*

The **Author,** in reply, said it was true that Mr ROGERS had pointed this fact out In his reply at the time, Mr PAYNE had said he was unhappy about the position but was not convinced by Mr Rogers and thought he was wrong Since then Mr Brotherhood had shown him the light He was sorry that he had not acknowledged Mr Rogers' earlier statement

In connection with Coriolis balancing, the point raised was one which he had forgotten to bring out in the condensed lecture—that aerodynamic forces on the blade were functions of the azimuth angle and the pitch angle which were inevitable with a helicopter and which they could not control But without drag hinges the Coriolis forces were a function only of the blade flapping with respect to the mechanical axis If there were no blade flap with respect to the mechanical axis, there was no Coriolis vibration The shaft position could be altered by moving it about in the fuselage and the Coriolis vibration could be phased in any direction desired

The point which he had forgotten to make, and which Mr Rogers had made, was that certainly on a two-blade rotor they could balance out the second harmonic vibration in theory merely by inclining the shaft at the right angle He did not know whether this had been tried in practice, although he had been advocating it for some time On a two-blade rotor he thought it could be done fairly accurately

Published flight strain gauge and load results for two-blade rotors gave reasonable agreement with calculated figures, even in low velocity regions, provided the correct induced velocity were used If the Glauert constant induced velocity were used, the results were useless, but if the modified Glauert hypothesis, with velocity varying from front to back, were adopted, this was not so Some American work had shown that rotor vibration of a two-bladed helicopter was in very good agreement with the theory

In the general case of three or more blades, where higher harmonics were involved, he agreed that it was very difficult to calculate Balancing by Coriolis in such a case would probably be by moving the shaft axis about in flight, which in practice meant moving the C G , until minimum vibration was achieved That was not practical with a single rotor, unless there were high off-set flapping hinges or elastically stiff flapping hinges, where the C G , once its angle to the shaft had been fixed, was kept in substantially the same position all the time relative to the tip path plane

Mr P Brotherhood (*Royal Aircraft Establishment*), said he was a little worried about the problem of Coriolis acceleration , it seemed to be treated as an independent variable, and he did not see how this could be the case He was not quite clear on this and was merely thinking as he went along, but the blades moved only under the air forces and the air forces were not a function of the flapping angle with respect to the shaft The Coriolis forces were in fact opposed by the air forces He did not see how the Coriolis acceleration could be an independent variable The lift force which caused flapping caused movement on the drag hinge In the case of a rotor with drag hinges, there was an oscillation on the drag hinge due to flapping, but when the associated angular velocities were resolved in the tip path plane the resulting velocity was found to be constant and equal to the angular velocity of the shaft

Mr Brotherhood then demonstrated on the board that the aerodynamic forces were not a function of first harmonic flapping with respect to the shaft He also showed how the lift which caused the blade to flap relative to the shaft had a component along the path of any blade element which caused the blade to move on the drag hinge

The **Author,** in reply, agreed with Mr Brotherhood about a hinged rotor but explained that the point which Mr ROGERS had made, and which had been made in the lecture, was that balancing the Coriolis force against the aerodynamic force could be done only without drag hinges , and in that case the Coriolis vibration was a force, since the blade could not accelerate or decelerate It must be shown as a force in the blade, and that was the resultant force at the hub In the case of the Sycamore, the shaft was inclined forward so that there was no in-plane movement of the blades in cruising flight That was a design calculation some years ago If that aircraft had no drag hinges and if it cruised with the tip path plane normal to the shaft axis, there could be no Coriolis acceleration on the blade, no Coriolis force Did Mr Brotherhood agree ?

Mr Brotherhood said he did not agree He did not think anybody had ever measured an increase in vibration due to tilt of the tip path plane in respect of the shaft In the case of a two-bladed see-saw rotor no drag hinges were needed because the twice per rev drag hinge oscillation is consistent with two blades as a continuous beam

The **Author** asked whether that meant that Mr BROTHERHOOD thought they could not get Coriolis forces—that was, in-plane bending forces—in the root of the blade due to flapping in respect to the shaft

Mr Brotherhood replied that they could not get them due to first harmonic flapping He was assuming small offset hinges In the case of a rotor with dampers about the drag hinges, a restraint had been imposed and the blades could not move , and forces were then introduced at the hub On a two-blade rotor without drag hinges he did not see how there could be the Coriolis force as an independent variable,

The **Author,** in reply, and getting away from the discussion of the physical picture, said that he understood that it was Coriolis bending moments in the blade of autogyros without drag hinges which caused Cierva to introduce drag hinges in the first place Before that was done, and without the tilting hub the high Coriolis bending moments resulted in several accidents due to blades breaking in flight The solution which Cierva had used was that of drag hinges Another solution which he had used at the same time was to tilt the hubs so that the axis of rotation was always normal to the tip path plane

The **Chairman** suggested that this detailed argument on the effect of the Coriolis forces should be continued elsewhere

Mr Shapiro (*Consulting Engineer*) (*Founder Member*), said he would summarise his own experience with helicopter vibration in one over-riding doctrine do not worry about vibrations in the project stage All doctrines about vibrations, except that of the absence of doctrines, had failed miserably He had yet to come across any generalisation on helicopter vibration which had not been disproved in practice Personally, he would quarrel with many statements in the lecture

He wanted, in the simplest possible manner, to correct the impression in the minds of those who did not speak the lecturer's language Vibrations in helicopters were a complex subject which required classification and a broad view This task was necessary and should be done

Mr SHAPIRO said he agreed with some of the Author's statements and thought that to those who spoke his language his lecture would have been extremely stimulating , it would cause them to think again, in some cases to alter their opinions and in other cases to argue with the Author and alter his opinions But he recommended those who did not appreciate the finer points of the argument not to pay too much attention to generalisations about vibration, for which there was no sufficient basis

The **Author,** in reply, said there was a fundamental disagreement between them , he disagreed fundamentally with the statement that they should not worry about vibration in the project stage That was a great mistake and was the attitude which had led to helicopters being as they were at the moment The CHAIRMAN had said that many vibrated badly As he was a technician, the AUTHOR did not often fly in them—and presumably if he were a really good technician he would never fly in them ! But certainly those in which he had flown had vibrated badly, and there was no hope at all of eliminating this fundamental vibration by action on the part of the ground engineer It must be tackled from the basic and first design point of view

Mr SHAPIRO had said that in the past attempts to solve vibration in the design stage had always failed The answer was quite simple , the people who devised the rules and applied them in the past were even more ignorant than the technicians of today ! The AUTHOR said that even today they were all very ignorant on vibration because there was no such thing as a helicopter vibration expert Some helicopter engineers had been told by the boss to think about it for half a day in the week, and as a very recent innovation, one or two firms had specialist departments in vibration, but these covered everything from ground resonance to oscillations of nuts on the top of bolts in the rotor head

This business of designing for zero vibration must be tackled very seriously in the project stage if helicopters were to be a reasonable mode of transport At the moment many of them were quite unacceptable With the Rotodyne a serious attempt had been made to reduce vibration by increasing the number of blades, eliminating drag hinges and using wings The AUTHOR wondered whether the wing technique would be as successful as it was supposed to be Nevertheless, these were steps in the right direction, as was proved by a statement some time ago by a Fairey representative to the effect that the two-bladed Gyrodyne was smooth If that were true, it was a great achievement for a two-bladed rotor to be smooth , and it reflected considerable credit on the design team and on the fact that they had not used drag hinges and had used metal blades

He could not disagree more with Mr Shapiro They must get down to the business of vibration if they were to produce helicopters free from vibration That *must be done in the design stage Some people must be set up somewhere to study* the subject and to tell them what it was all about Their conclusions could be used in the design of helicopters

Mr Shapiro offered one correction He had not said that vibration should be disregarded in the *design* stage but that it should be disregarded in the *project* stage He had said that all generalisations about helicopter vibrations had been disproved, and what he had meant was that a helicopter project should not be chosen with regard to vibrations All the main parameters should be chosen for other considerations and the vibrations should then be removed in the detail design stage

The **Author,** in reply, said that the design stage *was* the project stage, when the project was finalised—if it *was* finalised and was not just a pretty picture to impress a customer If it were designed to be enlarged upon by the design office, then when they had finished the project design they had committed themselves to vibration ,

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and if the wrong decisions had been taken and they had committed themselves to a design which fundamentally would vibrate, they would be in trouble That was the main point where he disagreed with Mr SHAPIRO who was saying, in effect, that there was no method of deciding how to make the project free from vibration

The **Chairman,** who stopped what he said was another argument, observed that in all these things nothing was completely black or completely white Where the problem entered into the design stage was in the question touched on by the lecturer blades should be so designed that the problem of getting them all to a good master standard could be solved before the blades reached the aircraft to which they were to be fitted, so that at least the elementary causes of vibration were eliminated That was a question which could and must be dealt with in the design stage, because it amounted primarily to setting tolerances If it were not done then it would be much more difficult at a later stage

Mr Ciastula (*Saunders-Roe, Ltd*) (*Member*), stated that he would not enter into the previous discussion, but there were some factors like ground resonance which he would not like to leave untouched in the design stage, after the experience they had had There were quite a number of ways in which these things could be tackled

He had a certain difficulty in commenting on the Paper, for the Author had skipped through the written Paper and parts of it had therefore not been heard by the audience Nevertheless, he was compelled to discuss points which had not been mentioned He had a few comments to make on some points which had been raised On drag tracking, he pointed out that Saunders-Roe had done some work in this field It originated a long time ago , Mr SHAPIRO had something to do with it, with Mr NEWPORT After that, the work was taken up by the present Saunders-Roe staff They had run the rotors, using the device which the Author had mentioned, and they had a theory developed for it, but both their theory and the Author's approach had one fundamental snag in using the hand held vibrograph to determine which was the blade causing drag unbalance, they had to remember that the rotor was mounted on the dynamic system, with the undercarriage, in which there was a certain amount of damping and there would be a phase shift The major difficulty was in determining that phase shift What could be done, if one was dealing with existing helicopters, is that the aircraft could be put in the impedance test rig and then the phase angle could be calibrated against rotor R P M Only after that could the vibrograph method be applied

Perhaps another approach would be to do the measurements in flight where the air damping only would be present and the phase angle would be zero or 2π

Turning to control forces and torsion bearings, he said they had done some further work They had found that torsion bearings did not produce a destabilising force in the sense that it was a force which was acting away from the centre of the cyclic stick displacement, but was a force which was perpendicular to the direction of motion This force was theoretically constant for all values of the stick displacement

He said they thought that perhaps in some cases it was this force perpendicular to the direction of stick displacement which had caused quite a lot of stick stirring troubles which they had encountered It was true, of course, that the torque bar solution would alleviate that

One point in the written Paper, which he considered so important that he must raise it, concerned the problem of light alloy and steel blades First, he asked the Author in what possible conditions he expected the fluctuating stresses in the blade to be 30 per cent ultimate The data presented by Williams for light alloy blades could not be applied to helicopter blade design , they applied to the Typhoon tail plane joints, bolts, rivets, holes and so on, the type of design which is obviously out of the question for helicopter metal blades

It was quite true that perhaps a conclusion like 1,000 hours for 2 per cent ultimate stress could be deduced on the basis of such hardly relevant information, but that did not mean that a blade could not be designed in light alloy with only reasonable stress concentrations He personally believed that such a blade could be designed, with a life of anything between 5,000 and 10,000 hours If they took fluctuating stresses at 2 per cent of the ultimate, it was the R A E view that they could then say, even for light alloy, that the blade has for all practical purposes, infinite life If they took 30 per cent fluctuating stresses for a steel blade, he did not see how it could possibly work To begin with, if they took 75 tons steel, the endurance limit for such steel was 57,000 p s i out of an ultimate of 168,000 The endurance limit itself was 34 per cent Before anything could be started that was reduced to 23 per cent of the ultimate by introducing the usual factor 1 5 On the fluctuating stresses of 30 per cent, the steady stresses have to be superimposed, which would lead to fatigue strength factors well below one

What the Author had suggested seemed very nearly impossible and Mr CIASTULA would like to know under what conditions it could be achieved He did not think that fluctuating stresses of that order could occur Even if they could occur, say due to flapping resonance, it would be a fallacy to allow them to exist One would obviously redesign the blade He thought this part of the argument, which had been put strongly in the Paper, should be regarded with suspicion

In dealing with fatigue problems, he thought it useless to try to decide whether to use steel or light alloy on the basis of material considerations alone He did not believe in that Fatigue analysis which was not related to the actual design of a particular project was useless Unless one could clearly see what the stress concentrations were, he did not think such statements of a general nature had any value

The **Author,** in reply, said he had been waiting for Mr CIASTULA to prove that aluminium was better than steel, that seemed the logical extension of his argument ! He had said in his written Paper that no engineer connected with vibration could view with equanimity the use of light alloy for rotor blade construction in view of the high stresses in the blades, a 30 per cent ultimate fluctuating stress was not very high in a blade in resonance Figures of that order—although not 30 per cent — had been measured on rotor blades If the blade had been re-designed so that the overall factor on centrifugal tension were more in line with modern requirements, then the figure would be 30 per cent ultimate This could occur in resonance in the sort of blade fluctuation which Hafner had shown in his lecture some time previously To use aluminium must, to his mind, be extremely bad, for, setting aside all Mr Ciastula's arguments, they knew that steel was better than aluminium or Duralumin

Mr Shapiro asked how that was known

The **Author** replied that it was known from laboratory test specimens He had always been told by the fatigue experts that the sensible thing was to design a blade in steel rather than in aluminium It had been suggested that it was impossible to design a blade in steel, but he had done so and had run such a blade successfully, althought not for a long period It had probably been used for the same sort of period as that of most prototype blades, possible including that of Saunders-Roe to which Mr CIASTULA had referred He did not state that this blade had good fatigue life, for that had not been measured, but he knew that it was possible to make a steel blade and steel blades had, in fact, been used successfully in America for many years

He supposed that steel was better than aluminium by a very large margin, and if that was disputed he must call on a fatigue expert for he was not qualified to answer On that supposition, which seemed to be shared generally amongst technicians, it appeared foolish to use Duralumin

Turning to the question of whirling, he said that the penalty of getting the pre-print of the lecture out early was that the Author then found that he had left a lot out Dealing with the effect of damping, he had included in the written lecture reference to the use of the vibrograph when the damping is large He was glad to hear that Saunders-Roe had used it, he had pressed for that when he was with them It was interesting to learn that it had been used successfully, although apparently not as successfully as the Author felt it could be

Mr R J Jupe (*Bristol Aeroplane Company*) (*Member*), referred to the suggestion that the torque bar was supposed to put the blade in resonance On page 355 of the lecture it was stated that if the stiffness coefficient of the torque bar was equal to ω^2 times the polar moment of inertia of the blade about the pitch change axis, there would then be no resultant torque in the controls

The polar moment of the blade could be split into dragwise and flapwise components of which the former was very much larger than the latter Now the torque due to propeller moment was a stiffness which would put the difference between these two moments of inertia into resonance and reduce the resultant load in the controls All the torque bar had to do in this case was to deal with twice the flapwise moment of inertia and this was very small If the torque bar had to deal with the whole moment of inertia of the blade, then he was afraid that no helicopter would ever fly without torque bars, for the forces would be enormous

The **Author,** in reply, said that if Mr JUPE read on a little further he would find that allowance had been made for this fact What the AUTHOR had said was that the stiffness would be equal to the inertia times the frequency of the rotor squared In both the written and spoken lectures he had then gone on to say that the stiffness of the torque bar for resonance was reduced by the fact that the propellor moment was also in effect a stiffness That was saying the same thing as Mr Jupe He was saying, in effect, that the stiffness was much lower than if a moment of inertia were taken of the blade about the pitch change axis The AUTHOR had said in the lecture that the stiffness was lower because they had to take propellor moment stiffness and subtract it This was clear in the Paper but confusing to speak about He thought it would be found that in the Paper he had done what Mr Jupe suggested

Mr J Zbrozek (*R A E*), who said he was neither a vibration expert nor a helicopter expert, said he had some comments to make on the written lecture Mr PAYNE was a brave man to tackle the subject and they were grateful to him for having drawn attention to all the problems of vibration At the same time, much of it was theory, and if Mr ZBROZEK were asked to provide a written contribution the subject is so immensely wide he would have to write a book about it He had looked at the lecture and he was afraid that he agreed with Mr SHAPIRO , it was not a case of disagreeing with every second sentence, however, but a case of disagreeing with every second equation

The vibration problems of helicopters were so involved that unless one was a good mathematician and physicist combined, one could prove a lot of nonsense He advocated that if the lecture were to be printed, they should avoid all reference to theories and cut down the equations

He would not go into detail about the written Paper, but, *e g* , it seemed to him that much nonsense had been talked in it about propellor moment and simple physical understanding was much obscured With an infinitely thin blade, the propellor moment was identical with the inertia moment and no forces existed anywhere For practical thickness of the blade, the difference was a few per cent

He suspected that there were many loopholes in the mathematics He did not know what the future of the Paper would be but for the sake of the prestige of the Helicopter Assoc of G B he would not advocate putting it into print, at least not in its present form

The **Author,** in reply, said this was the point which Mr JUPE had made He could only repeat that if they took the equation as it stood and did as had been suggested, they would get the same answer—zero elastic stiffness in the torque bar

It was a matter of treatment , Mr ZBROZEK was taking the inertia of the blade on an elastic stiffness and taking the inertia of the blade as a thin plate and the AUTHOR had put the correction in afterwards in the form of the propellor moment, but the answer was the same

Mr Zbrozek said he had also referred to all the subsidiary mathematics It was an extremely difficult problem, and it was easy to criticise Problems had been simplified, but the Author had missed many terms and a lot of wrong conclusions could be drawn as a consequence He suggested that at present the theory should be left alone until they had somebody with a good mathematical background whose full-time job it was to study the matter and who could be supported by experiments Otherwise they might draw a lot of wrong conclusions and find, in designing a helicopter, that they were barking up the wrong tree

The **Author** said he agreed He had said at the beginning of the lecture that helicopter engineers were amateurs working on the problem The trouble was that no one was working on the problem full-time and in nearly every case they had to do their own donkey work If they sat back and waited for somebody to investigate vibration, they would be none the wiser in 10 years' time In such a problem it behoved them to get down to it and try to find some of the answers in the absence of an expert on the subject

He understood that Mr Brotherhood's department had done some work on vibration and, as he had stated, everybody was anxiously awaiting their Paper But it had not yet been published and there was no information on vibration A start had to be made somewhere—for instance, simple examples of first harmonic vertical vibrations It was easy to pull holes in this analysis It was a crude picture, but it gave a picture which worked in the case of vertical vibration and which had not previously been suggested It indicated that the first harmonic vibration due to unbalance would be linear, at forward speed The second harmonic component would be a function of μ^2 Wrongly or rightly, they could find both those characteristics in plots of helicopter vibration against forward speed All he was stating was what the cause of the vibration, when linear, could be assigned to one of two causes, and corrective action could be taken

The AUTHOR said that to argue about vibration in general terms, as Mr Shapiro advocated, was utterly pointless They had been arguing about it in general principles ever since rotary wing aircraft started and it had got them precisely nowhere Helicopters were better than they were—although even that was open to question when one read old reports of the Pitcairn autogyro which said, " This aircraft was very smooth when flying at $\mu = 0\ 7$ " They did not say what " very smooth ' was, but it could not be rougher than some aircraft flying today

They must get down to the problem If Mr Zbrozek and Mr Brotherhood would set up a 10-12 man department and spend some money on the subject, which was what it merited, the AUTHOR would be extremely happy Everybody concerned with the design of helicopters in this country would be most happy if such a department were studying the subject

The **Chairman** said they had had a provocative Paper Quite a number of points remained to be argued but they could best be handled through written contributions The Company with which he was associated would deal with the matter in that way Mr Payne had said that he wished the R A E would set up a 10-12 man department and spend some money on the subject—spend money in a big way If the R A E would do that, whether on vibration or on anything else, he would be delighted ! A small skilled and devoted band of workers had kept the flag flying over the past few years but he did wish the R A E would increase their efforts on rotary wing aircraft

Whether or not they agreed with the Author's point of view, they would agree that he had given them food for thought in presenting the Paper For that he deserved sincere thanks

The vote of thanks to Mr Payne was carried by acclamation

WRITTEN CONTRIBUTIONS

Received from Mr P R Payne I should like to take this opportunity of briefly amplifying two of my replies

When Mr ZBROZEK said that the equations were a " lot of nonsense " I felt unable to reply adequately at the time within the framework of a technical discussion Mr Zbrozek's only example to substantiate his rather unusual statement was the equation for torque bar stiffness Mr JUPE missed the point of this because he had not read the lecture pre-print in detail, and my verbal explanation during the lecture was probably rather confusing Mr Zbrozek read the pre-print, however, and heard both the lecture and answer to Mr Jupe, and yet his criticism was that I had not allowed for the effect of propellor moment , yet I had twice stated, verbally that I had allowed for it, and the correct equation, which is very simple, was in his pre-print copy I still find it rather surprising that a member of the R A E , professedly not a helicopter expert, should make such a sweeping condemnation without a single vestige of evidence to back it up Now that the lecture is printed in its final form, it is to be hoped that Mr Zbrozek will publish any criticism of it that he has, in the pages of the Journal

In reply to Mr CIASTULA, I was unaware that Saunders-Roe had done any work on drag tracking before I joined them, being under the impression that my investigation of the subject was the first With regard to blade fatigue, my contention is as follows, putting it at its very lowest in the hope of reaching agreement Steel has better fatigue characteristics than Duralumin therefore, a steel blade has a longer life than one made from Duralumin, and since the scatter is lower, its fatigue life is also more predictable Alternatively, for the same fatigue life less exacting constructional techniques can be used in the construction of a steel blade If the premise is true, the conclusions are surely logical

Received from D M Davies (*Fairey Aviation Company*) (*Member*) The lifting wing on the Fairey Rotodyne is of course intended to unload the rotor so that higher cruising speeds are possible without stalling of the retreating blade Vibration is thus markedly reduced

Mr Payne suggests that the flow pattern around the wing will produce interference effects which will themselves produce severe vibration The interference will be there but it is most unlikely that the effect will be large enough to give noticeable results Rotors after all must deal with peculiar velocity gradients that they themselves generate, and do so with acceptable results While final confirmation can only come from flight tests no trouble is expected

Received from Mr Brotherhood In a written reply Mr Brotherhood adds My remarks are only valid for the completely articulated rotor With regard to the see-saw rotor I have omitted the once per rev Coriolis acceleration due to coning angle The resultant motion of the blades to alleviate this is inconsistent with the simple see-saw rotor and results in vibration of the hub as Mr Payne states

I should also like to point out an error in the written Paper where it is stated that the harmonically varying forces are multiplied by the factor

$$\left[1 + \frac{(n^2 - 1)}{n^2} \frac{M_{mD}\, r_F}{I_D}\right]$$

I think this should be

$$\left[1 - \frac{(n^2 + 1)}{n^2} \frac{M_{mD}\, r_F}{I_D}\right]$$

Mr Payne also neglects the induced forms in the spanwise direction

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MR PAYNE'S REPLY TO WRITTEN CONTRIBUTIONS

I am glad that Mr BROTHERHOOD now agrees with me about Coriolis vibration, and thank him for correcting my signs in the drag hinge factor, in which he is quite correct

With regard to Mr DAVIES' contribution I would point out that no-one in this country has ever measured any vibration due to retreating blade stall at a normal flight condition, to my knowledge I believe it was at one time thought to cause vibration experienced in the Sycamore at high speed, but blade tufting at the R A E showed that in fact no stalling took place Because blade stalling is a phenomenon which is readily understandable, I suggest that it is often unjustly accused of causing vibration due to other causes In the case of the Rotodyne it is difficult to be precise without knowing the blade tip speed, solidity, twist and section, but if the cruising speed is of the order of 120 knots it is difficult to see how blade stalling could affect it, particularly since both the S 58 and S 59 exceed this speed with blades of(presumably) lower solidity My remarks in the lecture therefore implied the assumption that the wing was used to reduce vibration due to causes other than blade stalling If this is not so it would seem to be superfluous, in the light of the scanty and probably inaccurate information available, even if such developments as offset and stiff-hinged rotors are ignored