The analysis of the higher harmonics of rotor blade flapping has most recently been set down by Stewart[4] Using the same notation, the coefficients of 2nd harmonic flapping are estimated from the following two relationships

$$3a_2 - \frac{B^4}{4}\gamma b_2 = \frac{\gamma}{2}\left\{ -\frac{B^2\mu^2\theta_0}{4} + \frac{B^3\mu B_1}{3} \right\}$$

$$\frac{B^4}{4}\gamma a_2 + 3b_2 = \frac{\gamma}{2}\left\{ -\frac{B^2\mu^2 a_0}{4} - \frac{B^3\mu A_1}{3} \right\}$$

For the Dragonfly at low altitude, the rotor blade Inertia Number is —

$$\gamma = \frac{\rho a c R^4}{I_{flapping}} = 8\ 0 \textit{ approximately}$$

The magnitude of the a_2 coefficient is approximately three times greater than the value of $-b_2$ Consequently, the maximum amplitude of 2nd harmonic flapping is only slightly greater than the value a_2 and is phased about $8\frac{1}{2}°$—$9°$ from the true lateral and fore-and-aft blade positions in azimuth

3 Retreating blade angles

The estimation of retreating blade tip angles was based on simple strip theory and also assumed constant induced velocity over the disc with no corrections for reversed flow or stalled areas Account was taken of the 8° of geometric twist (wash-out towards the blade tip) of the Dragonfly rotor blade, and of the effect on blade pitch of 2nd harmonic blade flapping This latter effect is small and even in the most extreme cases does not amount to more than a 1° increase in blade angle

In the past, comparison of this simple analysis with flight test data at the onset of blade stall vibration has resulted in limiting angles of between $14\frac{1}{2}°$ and $16\frac{1}{2}°$ being used to denote the vibration limiting speed (V_{NE}) of the helicopter

Discussion

The **Chairman** said that the Author had given a most interesting and instructive discourse They would all be grateful to him for the trouble he had taken in preparing his model and the two films he had shown, which helped one to have a physical grasp of the phenomena involved

Dr J A J Bennett (*College of Aeronautics, Cranfield*) (*Founder Member*), said that the vibration survey which the Author had presented was most valuable in helping them to revise their overall perspective of helicopter dynamics They were too apt to view the helicopter as an aero-elastic system with a whole spectrum of natural modes of oscillation on the one hand and another spectrum, of periodic excitation, on the other, and to conclude that it was impossible in practice to avoid having at least one of the exciting frequencies fairly close to the frequency of one of the natural modes, thereby resulting in troublesome vibration of one kind or another Perhaps the frequencies of the natural modes of oscillation of the blades could be included with advantage in Fig 4 Even if resonant vibrations were minimised by appropriate separation of frequencies between the two spectra, there was still a possibility of self-excited vibrations which were non-resonant in character and which had to be kept under control by adequate damping Thus, the Author s expectation of smoothness at high forward speed in the operation of the next generation of helicopters must appear to some people, Dr BENNETT imagined, as rather optimistic

What the Author was saying was that there was no source of excitation which balancing or tuning could not render innocuous for all practical purposes He rightly observed that Cierva, more than 25 years ago, had already designed and flown autogiros with a large part of the retreating blade in a stalled condition The C-19 and C-24 autogiros, for example, operated at a tip-speed ratio of 0 67 at maximum speed , *i e* , there was negative air speed at the retreating blade as far outboard as two-thirds of the radius, and the outer third contributed relatively little to lift Thus the lift of each blade fluctuated substantially from zero in each of the two lateral azimuths to a maximum value in the fore-and-aft azimuths Certainly the fixed wings of the autogiros unloaded the rotor at maximum speed by about 35%, but operation of the blades beyond the periodic stall was accomplished without any discomfort to the pilot or passenger Having flown with Cierva in both these early autogiros, Dr BENNETT could testify that the vibration had been well below the threshold of discomfort

Why, then, had there been subsequent deterioration in rotor vibration, after the fairly low level achieved 25 years ago ? One contributing factor was the reduction in number of blades from four to three, and in some cases to two, mainly to decrease solidity and thereby attain a low disk loading for a high tip speed For a given tip speed, small rotors had a higher rotational speed, and therefore a higher frequency of vibrational excitation, than larger rotors, so that two blades had sufficed for small, low-powered helicopters of a maximum speed below about 90 knots, and three blades for medium-powered helicopters operating up to about 125 knots , but to achieve higher speeds with large rotors of low rotational speed it had long been realised that four or more blades would be desirable

With each blade transmitting to the hub periodic forces of fundamental frequency equal to the rotational speed, the harmonic forces from the several blades balanced each other, except for those of frequency equal to an integral multiple of the number of blades per revolution There was not only the simultaneous upward displacement of the b blades together occurring at b times per revolution—*i e* , periodic coning—but also a bending excitation of the same frequency producing what the Author referred to as a ' wobble ' Vibration from those two sources had been termed by Cierva ' bouncing due to flapping ' and ' bouncing due to bending ' respectively

The blade was subject to two main forces lift and centrifugal force Each of those forces had not in general the same point of application, one being fixed but the other moving along the blade with the fluctuating load distribution To make an analysis, the lift (L) could be represented as having two components The point of application of one component was coincident with that of centrifugal force, and he would call this L_1 The point of application of the other component, L_2, was at the blade root $L_1 - L_2 = L$ It was therefore possible to represent L, which was the one which fluctuated along the blade, by the two components which were respectively at the point of application of centrifugal force and at the root of the blade L_1 combined with centrifugal force could be replaced by tension along the blade The axial component of this tension fluctuated with the flapping angle β, and there remained L_2, which existed only when there was bending of the blade The one produced bouncing due to flapping and the other bouncing due to bending That was brought out in the paper

The bouncing due to flapping, therefore, could be balanced by appropriate harmonic control of blade pitch, as had been attempted by Cierva in his so-called ' autodynamic ' rotor , but the wobbles, being due to periodic span-wise variation in bladeload distribution, might require some form of dynamic vibration absorber Mounting a portion of the blade near the tip on a flexural hinge had been proposed in the U S A to balance so-called bouncing due to bending

It was well known that helicopters were usually rougher in operation at high collective pitch in forward flight than at the same forward speed in autorotative flight One reason for this was that the loading on the blade moved towards the tips with increased inflow downwards through the disc at high pitch, whereas in the autorotative condition of flight the outer portion of the blade transferred its loading inboard due to the negative inflow at low pitch

Perhaps the four-bladed Dragonfly used by the Author would have been appreciably rougher at high speed if sufficient power had been available to maintain level flight With the additional power supplied from the loss of potential energy in a steep dive, the helicopter had been operating in the ' gyrodyne ' condition , *i e* , the rotor had been contributing only partially to propulsion, so that the inflow condition had been tending towards that of autorotation and the Dragonfly in its steep descent had been operating very much like the C-19 and C-24 autogiros at their maximum speed in level flight

There was no doubt, however, that the Dragonfly had been flying under conditions of periodic stall, and the Author was to be congratulated on having been one of the first to explore the vibrational characteristics of a helicopter operating under such conditions It would be interesting to see what happened when similar tests were conducted in level flight The Author was also to be congratulated on the preparation and attractive presentation of a most valuable Paper

Mr O L L Fitzwilliams, in reply, thanked Dr BENNETT for his contribution and expressed particular interest in his experience of flying in the C-19 and C-24, because recently Mr Payne had made some very rude remarks about people who talked about " old cans " without saying whether they were smooth or not

Dr Bennett said he believed that autogiro pilots would confirm his statement

The **Author** did not doubt that there was ample evidence for it, but said it was the first time that he had heard it put on record Dr BENNETT had summarised the argument of the Paper very well , the helicopter was a collection or bundle of vibrations, and it was possible to take each one and deal with it separately In so far as unexpected self-excited vibrations might exist, they were just adding to the bundle and could be dealt with in the same way as the others

What Dr BENNETT had said about the C-19 and C-24 was most interesting The Author had always assumed that they did fly at high tip-speed ratios, but he had never heard anyone say so before All the autogiros of that class had been " much of a muchness " and must have done much the same things as the Pitcairn PCA-2 Dr BENNETT had said that the fixed wings of those autogiros unloaded the rotor, but the Author did not think that that was really true It depended on what was meant by " unloading " If " unloading the rotor " meant reducing the angle of incidence of the blades, it was not true, because the autogiro rotor speeded up when load was put on it and slowed down when load was taken off it, but the angle of incidence remained almost constant, so that by taking the load off the rotor by a fixed wing the rotor was forced to operate at the same angle of incidence but at a much higher tip-speed ratio What had been done in those days with autogiros could be done now The phrase " unloading the rotor " was a " gimmick " which had come from the U S A in the last ten years

Dr BENNETT's discussion of bouncing due to bending and " bouncing due to flapping " was interesting The bouncing due to flapping the Author could understand He did not think that the " bouncing due to bending " was what he had been talking about, but he was conscious of the fact that he did not know anything about vibrations due to blade bending, and he had tried to be discreetly frank about this in the Paper The four-bladed Dragonfly had centralised drag hinges, and that might have covered up the results of blade bending He did not think that it was likely to be very important

In the case of a 3-bladed rotor having vertical excitation from the 3rd harmonic of flapping, the coning angle would be about 6°, and the machine would be subjected to an acceleration of $\pm 1/36$g The vibration caused by the " bouncing due to bending " of which Dr BENNETT spoke would probably be even smaller He had not known of Cierva's work on this point and had been interested to learn of it

Dr Bennett explained that the bouncing due to bending had been decreased by having flexible blades If the blades were stiff, the bouncing due to bending was high

The **Author** said that Dr BENNETT had put a question which was to be expected, concerning the near-autorotative condition of the rotor, in rather an unexpected way It should be noted that the instantaneous centre of rotation is at a distance μR from the centre of the rotor Since this was the instantaneous centre of rotation for horizontal movements, the only air flow must be vertical In an autogiro the angle of incidence at that point would be $+90°$, and in a helicopter it was—$90°$ Starting with, say, 20° at the tip, the angle of incidence over the entire blade with an autogiro would be much more than 20° With a helicopter, it would come down in the way

https://doi.org/10.1017/S2753447200003358 Published online by Cambridge University Press

shown If 12° was the stalling angle, it would be only the tip which stalled This might affect the amount of 2nd harmonic flapping but the four-bladed rotor was not affected by the 2nd harmonic in any case, so that it did not seem that any valid objection could be raised on the ground that the machine was close to autorotation

Mr W Stewart (*R A E, Bedford*) (*Member*), suggested that the Author had somewhat exaggerated the interpretation of his experimental results in helping to prove the general concept of reduced vibration at high tip-speed ratios If reference were made to Figs 9 and 10, it would require a considerable amount of flight testing to establish those curves accurately In particular, it was difficult to justify the separation of the two curves in Fig 9 Although tip-speed ratio would be expected to be one of the main parameters, the difference in r p m was equivalent to only 1% of tip-speed ratio keeping the same forward speed, or equivaent to a change of speed of 1 knot keeping the same tip-speed ratio The separation which the curves showed was considerably greater than this, the inference being that tip-speed ratio was not one of the important parameters The difficulty was then to see which was the important parameter, and perhaps the Author would comment on this

Mr STEWART felt that the real answer lay in the experimental scatter which must pertain in this type of work, and that in fact only one curve was justified in Fig 9 instead of two, and perhaps that curve should not have quite such a sharp peak However, the disagreement on this detailed interpretation of the experimental work did not detract from the general concept of a vibration curve against speed with a hump in it The Author seemed to have demonstrated in general terms that that sort of thing could be expected There were in fact a number of other pointers in that direction

When they considered these very high tip-speed ratios, they automatically cast their minds back to the autogiro, which, both in this country and particularly in America, achieved a tip-speed ratio of about 0 7 There were several quite major differences between the helicopter and those autogiros One of the main points, in addition to the very much lower disc loading and very much lower blade loading of the autogiros, was in terms of Mach number The Author quoted as an example the present-day 550 ft /sec tip-speed and added a 0 7 tip-speed ratio, which meant a 0 85 Mach number on the advancing side Mr STEWART was sure that the autogiros had never been anywhere near this

The Author had shown that there was sufficient promise of flight at these high tip-speed ratios to make it worth while looking into the matter in greater detail, and it was to be hoped that much more work would be done on the subject There was one interesting point concerning the rotating blade of a helicopter as it went through a peculiar condition which in the past had never been taken into account Taking a 0 7 tip-speed ratio, there was, when the blade was in its forward position, an angle of sweep-back on the blade This was of the order of 30° at the tip, increasing to over 60° inboard along the blade When the blade rotated from the forward position into the condition of blade stall it was in fact changing its regime from a very highly swept-back wing to one of reducing sweep-back as it rotated, until when it got into the lateral position it had again straight flow across the blade

One of the things which the Author did not mention, although he was well aware of it, was the anxiety about what happened when the blade did stall It was possible to get quite a kick from the blade if it suddenly lost its lift This effect of sweepback was very much an alleviating factor in this, in that in the straight flow experienced in the really stalled incidence conditions there was still an appreciable lift coefficient, even though one was beyond the C_L max , but one did not in fact go from the top C_L max position with a very sharp drop as the stall occurred, because the blade had been coming through the region of very high sweep-back, where the C_L for a given incidence could be very much reduced That gave a very much smoother transition than would otherwise be obtained in purely rectilinear flow

Mr STEWART had quoted a particular example There were several others, and he hoped that all this would be taken into account and that research work would go ahead to try to prove that it was possible to fly helicopters at as high tip-speed ratios as had been used with autogiros and with the much higher absolute tip speeds which were now possible

The **Author** expressed his appreciation of Mr STEWART's contribution and welcomed the fact that Mr Stewart might be taking up these tests again He accepted entirely the criticism that the evidence offered for some of the conclusions in the paper was very shaky, though he believed that they would be confirmed , he looked to Mr Stewart, who was an extremely ingenious experimenter, to confirm some of the results

He did not think that the differences, which were very interesting, between the four-blade Dragonfly and the older autogiros were important, though admittedly they existed Even though it might be necessary to have slightly thinner blades, he thought that the same performance could be obtained He appreciated very much the reference to the conditions which lay at, say, 45° fore and aft on the retreating side Those were conditions which nobody knew much about, and he hoped that Mr Stewart would find out more about them

Mr R Hafner (*Bristol Aircraft Ltd*) (*Member*), said that the Author had made an excellent examination of a most important subject In dealing with his graph of permissible vibration he had mentioned that there was not much information in the lower frequency range Mr HAFNER suggested that there was one very useful datum, namely, the movement of walking If amplitude and frequency of this movement were put on the graph it would probably be found that Gerstenberger's 0 1g line was a little on the conservative side

The Author was to be congratulated on his " funfair roundabout " model The phenomenon of blade motion, although well established mathematically, is not easily visualised, his model however gave a vivid picture of the flapping of a number of blades in the fundamental and higher harmonic components

There was one point on which Mr HAFNER disagreed with the Author He would say that the adding of stub wings was by no means a ' gimmick ' Whilst fully in agreement with the Author on the improvement which could be obtained by increasing the number of blades in a rotor—in particular, by changing from three to four blades—such an improvement was not sufficient The stub wing offered two fundamental improvements —

1 In forward speed it carried some of the weight of the aircraft, thereby unloading the rotor This meant a decrease in the mean lift coefficient of the rotor blade, which permitted an increase in tip speed ratio and thus an increase in forward speed

2 Unloading of the rotor in forward speed produced a decrease of coning angle An examination of the air flow through a rotor at forward speed would show that the axial speed component (inflow velocity) is a minimum in the forward quadrant of the rotor and a maximum in the aft quadrant The difference between minimum and maximum increases directly with forward speed and coning angle The power to drive a blade is mainly a function of this inflow velocity, so that at high forward speed there is a significant variation of driving power during a revolution, the minimum being forward in the rotor disc and the maximum aft Evidently a decrease of coning angle would decrease such variation of power and thus achieve a smoother running of the rotor

The **Author,** on the question of stub wings, said that at very high tip-speed ratios there would be no lift in the advancing and retreating quadrants, because the rotor must be balanced, so that for the generation of lift there was no complete rotor at all, but simply a couple of segments, as shown That was where the lift was coming from, and the rest was wasted If there was, for really high tip-speed operation, enough blade area to function properly there, there would be enough blade area to postpone the onset of stall sufficiently high up the tip-speed ratio range, so that there would be no serious disturbance in passing through the peak stalling vibration and a stub wing would not be necessary If there was a stub wing, it would be counted on to unload the rotor It was well known that in turbulent air the slope of the lift of that stub wing was much steeper than that of the rotor When a down-draught was encountered, immediately the whole of the lift was thrown on the rotor Mr HAFNER had referred, in earlier discussions with him, to the idiocy of making one piece do two things Personally, he thought that that was good design , what was bad design was to have two pieces to do one thing !

Mr R H Whitby (*British European Airways Corporation*) (*Member*), said he had greatly enjoyed the Paper, which appeared to make the subject very simple Perhaps Mr Hafner had made it seem not quite so simple ! He wished to deal with permissible vibration, and Fig A showed data from various sources The Author had said that there was little information available about what was acceptable, but when he (Mr WHITBY) looked into the subject he found that there was almost too much

Fig A showed Constant's curve, which was based, so far as he could recollect, on a sinusoidal vibration at a single frequency with the subject seated on a parachute pack The AP 970 curve was half the amplitude of the Constant curve He believed that this was an adjustment made for the fact that rarely was there anything but a complex vibration, so that a margin in hand was needed

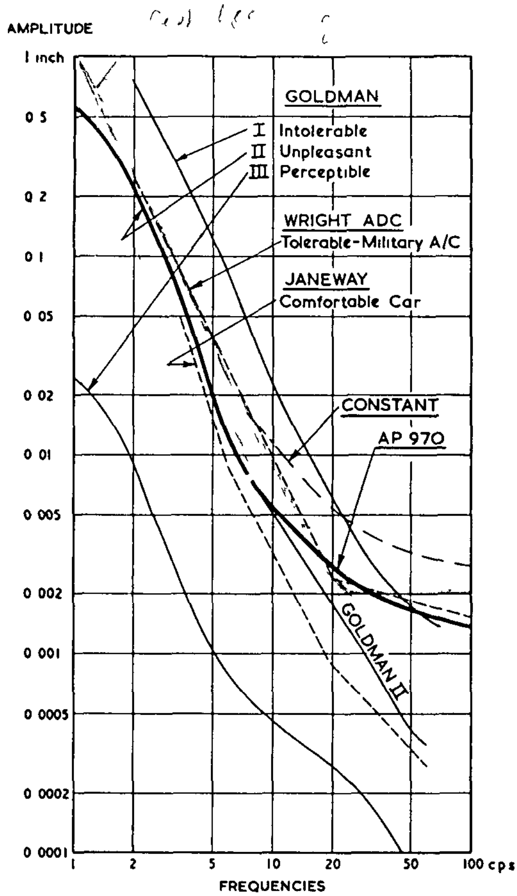

Fig A Physiological Significance of Structural Vibration Levels at Various Frequencies

Some years ago, when looking into this question, he had come across a very valuable report by Goldman, of the U S Navy, which was a model of analysis of results accumulated in laboratories all over the world over a period of more than thirty years On the basis of that analysis, Goldman produced three curves, "intolerable," "unpleasant," and "perceptible" As a matter of interest it might be mentioned that heartbeats were just below the "perceptible" region

About five years ago, when trying to prepare a specification for a large helicopter, he had felt that one could not do better than accept Goldman's digest of many laboratory results, at low frequencies, as a guide to extrapolating the AP 970 curve The discrepancy between the Goldman and AP 970 curves, which increased with frequency, could be explained by the fact that the AP 970 figures, being based on Constant's work corresponded to an acceptable structural level if the subject had something (a parachute pack) between him and the structure, which was not the case in the Goldman results The difference would be much larger at the higher frequencies, while at the lower frequencies the intervening body would not have so much effect

Since 1952 some other standards had appeared One of these was the Wright Air Development Centre standard for military aircraft, which incorporated the assessment by pilots of acceptability of a great many aircraft (including helicopters) There was also the S A E curve for motor cars, which was rather more stringent Comparing the proposed curve of Fig A on the plot of Fig 2 in the present Paper, it followed the AP 970 line and then the Goldman 2 curve swept round and cut the left-hand margin between 0 1 and 0 05g He was not sure where the remaining curve of Fig 2 (Wigdortchik) came from, but he suspected that it was French curve extrapolation on a linear plot of amplitude against frequency

That was all that he wanted to say about acceptable standards of vibration, but he had one or two questions to put to the Author

First of all, he would like to say how delighted he was to find that at last someone had had the energy to get some checks done on Gustafson's results, because, particularly from the civil aircraft point of view where speed was very important, results obtained on an R-4 many years ago, excellent though the flight test work had been, was an inadequate basis for the design of helicopters He was sure that they would all await the results of the fuller exploration of this work with extreme interest In passing through the stalled area, where the vibration was rather high, he felt from the civil point of view that unless the vibration in this region could be kept quite low it was going to be a disturbing feature to passengers There had been a rather alarming comment with respect to the possibility of flutter in this stalled region, following on Professor Collar's analysis If there was a theoretical possibility of something happening, it was not good enough to assume that it would not happen in practice, merely on the strength of a single case in which it did not occur It was better to encounter it in a mild form and discover the practical factors which determined its absence or otherwise It was not so long ago that more than one firm in this country, after employing the standard methods of predicting ground resonance and concluding that the aircraft was designed properly, had experienced it That was the type of situation which might be present with flutter in the stalled region, and, without a full understanding of all the factors involved, it would be a risky procedure to bank on passing through this stalled region without anything unfortunate happening

Speed always cost something It cost something if it was necessary to fit an auxiliary wing to unload the rotor or, in the present case, where one was cutting down the useful area of the rotor There was no indication in the paper of what this was costing in terms of power Allied to this was the fact that the results had been obtained at 107 knots That was no criticism of the work, because that was all that had been possible , but at 150 knots, with the same advance ratio of about 0 45, there would be a great many more compressibility effects on the rotor To minimise these (as the Author had hinted) it would be necessary to reduce considerably the blade loading so as to achieve a suitably low blade C_L

The **Author,** on the question of the limits of vibration, said he felt a little ashamed of himself, since he ought to have known the facts which Mr WHITBY had given, and which he had been most interested to learn Replying to Mr Whitby's questions, he said that one could not expect with a civil air liner to go through a stall stretching over the area of the speed range, but if the stall could be postponed there would be a reduction in the way shown He had tried to emphasise throughout the paper that it was possible to design to get through this, if it was small enough, by a kind of trick, or just go straight through it without too much trouble Apart from that, it was possible to overcome the difficulty by quite a simple system of rotating counterweights That might not be a gentlemanly way of solving the problem, but it was no more ungentlemanly than the method used by the piston engine designers to balance their crankshafts The counterweights were small, light, effective and always in tune He hoped that that method would be used, so that people would see it and get used to the idea

Mr Whitby had asked about power requirements The paper was supposed

https://doi.org/10.1017/S2753447200003358 Published online by Cambridge University Press

to deal with vibration, the Author felt strongly about power requirements, but it would be a mistake to take up more than a minute on the subject So long as the rotor speed remained constant, the profile power was approximately the same for all kinds of rotorcraft If lift was developed over only parts of the rotor, the effective span of the rotor was reduced, but the induced drag of the pure helicopter was in this condition comparable with that of a compound type having a short span wing

Mr A McClements (*Bristol Aircraft Ltd*) (*Founder Member*), said that in any paper by Mr Fitzwilliams they could expect a high degree of originality and a lucid explanation of the subject dealt with, and they had not been disappointed on the present occasion Mr Fitzwilliams had chosen vibration for his subject He was glad of this for a variety of reasons, and he was pleased that the Author set out early in his paper to "debunk" the idea which was still fairly prevalent, that the helicopter gave a rough ride because it had in it inherent vibratory disturbances which could not readily be dealt with This, of course, was not the case, provided the vehicle was operated within its limitations The upper speed limit was, admittedly, too restrictive, and several approaches were being made aimed at pushing this limit higher We had, for example, the compounding of the rotor with a wing and the compounding of the rotor with a wing and propellors, Mr Fitzwilliams had taken quite a different approach

Mr McClements said that he was privileged, some years ago, to know what was going on in Mr Fitzwilliams' mind and he then felt that experimentation was justified to check his reasoning He still took this view and thought that the work which had since been done was justified, although it was a pity that the equipment available to Mr Fitzwilliams was somewhat restrictive He thought further experimental work was justified, aimed at enabling a helicopter to fly at really high tip speeds ratios in straight and level flight, because the results so obtained, if they confirmed the trend shown in Fig 10 of Mr Fitzwilliams' paper, would provide a very convincing answer to the problem

On the question of vibration mountings in helicopters, the author had pointed out that current mountings were singularly ill adapted for the helicopter Mr Shapiro had done some work on the ' Sine ' spring arrangement aimed at overcoming the low frequency vibration isolation problem, and it would be interesting to learn from him how his work was progressing

Mr McClements referred to the higher harmonics of rotor blade flapping which persisted, and which the speaker had skilfully endeavoured to cancel out by suitable rotor blade arrangements, and suggested that they would require to be carefully watched in relation to the blade itself, otherwise the designer could have a serious resonance problem on his hands The problem probably did not arise during the Dragonfly experiments because the metal blades used were relatively stiff The situation might be quite different if the more flexible type of wooden blades employed in certain other helicopters had been used

A final point which Mr McClements mentioned was the need to improve the vibratory levels in helicopters at really low speeds, a condition which the speaker had not considered It might be that some of the improvements which Mr Fitzwilliams advocated, and in particular the avoidance of resonant conditions in the rotor and fuselage structures, would go a long way towards increasing comfort, not only at high speeds but at low speeds as well He invited Mr Fitzwilliams' views on that point

In conclusion, he congratulated the Author on a most interesting and stimulating presentation of his subject

The **Author** thanked Mr McClements for his comments, and added that the question he had raised at the end of his remarks was very complicated indeed As the Author did not know the answer he would not waste time in talking about it, but part of the answer was more care in design and more time to do the design carefully

Mr E T Jones (*President, Royal Aeronautical Society*), expressed some hesitation in speaking after so many helicopter experts, and said he had neither two blades nor five to sell, but there were a few points which had occurred to him and, the meeting being a joint one, it could not be left solely to the helicopter experts

He thought that there was a good deal more in this question of vibration than the Author had described Dr Bennett had referred to the early autogiro of Cierva Mr Jones knew that autogiro and thought that Dr Bennett was right in saying that it did not vibrate much, but it would be wrong to suggest that it did not vibrate at all It had an oscillatory motion very similar to that of helicopters today, but possibly not quite so large

There had been a big difference in the solidity of the blades If he remembered rightly, the chord of the original Cierva autogiro blades was about 2 ft 3 in, and there were four of them, so that there was a very large solidity in the disc Secondly, there was a cable connecting those four blades half-way along the radius, with a 2 lb lead weight in the middle, and that must have had a considerable effect on the vibration characteristics

The off-loading of the rotor by a suitable fixed wing was in fact a little different from that which the Author and Dr Bennett had indicated The original autogiro had no fixed wing, in the generally accepted sense, but it had a shaft extending laterally The first 4 ft approximately, of that circular shaft had no wing surface on it, it was only the last 3 ft which had a wing surface, and that was used as an aileron to give some lateral control during the last stage of the take-off, to stop the machine heeling over In other words it was not a wing contributing much to the lift, and he did not see how at top speed it could give more than 2% of the total lift

Mr T L Ciastula (*Saunders-Roe Ltd*) (*Member*), pointed out that although the paper dealt with vibration problems of the helicopter and their reduction or elimination, it was also necessary to realise how this elimination of vibration affects other aspects of helicopter design He wished to draw attention particularly to what was happening in the control system of the aircraft used in experiments when it was flown at high tip speed ratios He raised this point mainly because, if the experiments were resumed, it would be very important to obtain information on control loads under these high speed conditions The lateral kicks reported by the Author must have been quite violent to overcome the Servo system, what was their frequency, and what was the actual magnitude of control loads ? For instance, if during the flight at high tip speed ratio the control Servo system was disengaged, would control loads be so large that they could lead to a catastrophic situation ?

It would be expected that at higher tip speed ratios there would be a considerable increase in the in-plane vibrations in the rotor, causing a corresponding increase in fluctuating drag bending stresses It would have been extremely interesting if, at the same time during experiments, strain gauging of the blades was carried out This problem had to be dealt with at Saunders-Roe quite recently in connection with fatigue testing of certain components of the Skeeter helicopter The programme was related to a certain pattern of operational flying which was simulated in flight and strain gauging was carried out at the same time This has shown that when the tip speed ratio in flight at full power was increased from 27 to 29, the fluctuating drag bending moments were increased by about 35% This seems to indicate that at these high speed ratios, there might be a considerable increase in the level of fluctuating stresses and he thought that this part of the problem should be carefully investigated For manually operated helicopters, it was also extremely important to know what the control loads would be under these conditions

He also asked the Author what the vibration picture would be if the disc loading was very appreciably increased, which is the tendency nowadays with the introduction of lightweight turbine engines into helicopters Although the discussion was concerned mainly with vibrations, he wanted to draw attention to the fact that it was still essential to know exactly what was happening to the rotor from an aerodynamic point of view which, in fact, can be simply reduced to the exact knowledge on the direction and velocity of the air relative to the blade at every point and in the presence of all other blades One should remember that higher harmonic blade motions are of little significance for rotor control but aerodynamic forces are of utmost importance

He then raised the effect of blade weight and flapping moment of inertia on the higher harmonic vibrations Higher harmonic blade motions are, of course, fundamentally a forced type of vibration and well above the resonance and, therefore, the amplitudes depend almost entirely on the mass and inertia characteristics of the blade which oppose the exciting forces It would be interesting to know what increases in mass and flapping inertias of the blades on a 3-bladed rotor would be required to achieve similar results to those achieved with a 4-bladed rotor

Finally, coming to the Author's pet subject of the giant helicopter with turbo jets mounted at the blade tips, would it be true to say that such a rotor would probably have very little higher harmonic vibration

The **Author** said that Mr CIASTULA had raised some interesting points Dealing with the first, the forces were not so big as might be thought, and there were indications, from a comparison of the S-51 and Widgeon rotor heads, that the increase with speed was not so sharp as might be thought They had had a good deal of experience of switching artificially into manual under very severe conditions, and it was a problem, but experimentally it would not cause any undue risk Their type of machine, if built for speed, would be built for power control 100%, and in fact 200%

He did not know the nature of the forces Lateral kicking occurred at the frequency of the turbulence of the atmosphere, but what the internal nature of the frequencies was they did not know , they had not been able to go on far enough to experience them as something continuous which could be measured They could be measured with the servo still working by strain-gauges on the controls

So far as in-plane vibration was concerned, much depended on what one called a plane He had assumed that the plane did the wobbling If, therefore, one considered that the thrust of the rotor was always normal to that plane, the in-plane forces were only those given at the end of the paper He had not written much about them, because he did not regard them as important

With regard to the drag bending stresses going up 35%, in their blades they were very low in any case, and could go up by more than 100% before becoming a cause of anxiety In the photographs it was not possible to detect any bending in the drag plane He would recommend anyone who was worried to take photographs, because a surprising amount of information could be obtained by looking at them It would be necessary to go up the scale a long way before the situation became serious

Another question concerned vibration with high disc loadings In an autogiro or in the four-bladed Dragonfly the inflow was small If the disc loading was high and the inflow was very high the "hole in the rotor" may become very big The angle of incidence, instead of going up to a reasonable value, remained at large minus values for a considerable distance around the instantaneous centre He did not think that it would matter how high the disc loading went unless it got above, say, 10 lb/ft ^{2} It has already gone up to about 9 lb /ft ^{2} in America and did not seem to give serious trouble

With regard to the last point, Mr PAYNE had said that the type of helicopter with engines on the blades would be from the vibration point of view the worst of all That was not true Broadly speaking, it was not the vertical vibration which mattered but the wobble, the rolling and pitching oscillations That depended on the harmonics of flapping which caused it, and they were proportional to the coning angle , when the coning angle was small, they were small

Mr J W Leach (*A & A E E , Boscombe Down, Helicopter Division*), suggested that the question of vibration of helicopters could be divided into two separate sections the investigation of vibration in relation to the structure of the aircraft, and the investigation of the effect of vibration on the crew and occupants It was the second to which he wished to refer At Boscombe they were in a good position to undertake this work , they had many pilots who could give their views, and they obtained qualitative as well as quantitative results They were able to make a statistical analysis which led to fairly broad conclusions There was a considerable scatter in the results obtained experimentally, and there was also a very big divergence of opinion between individuals about the minimum comfort level The diagram shown by a previous speaker, indicating the various levels which people had decided on, required to be modified in relation to the helicopter, because there was an accumulation of vibrations which differed from those of the usual aircraft , there were gearbox noises and rotor noises, and the configuration was somewhat different, and also the operational use Whereas the overall level of vibration might seem intolerable, it was necessary to bring in the time element, because it might be tolerable for twenty minutes or half an hour, and that must affect the standards adopted

There was one point which he would like to emphasise for the attention of any instrument makers who were present The work of investigation which Westlands had done on the structure involved a great deal of elaborate instrumentation on a particular aircraft

In order to simplify the instrumentation problem hand held vibro-graphs and other types of portable equipment had to be used to measure noise levels and vibration, it being essential that this equipment could be transferred quickly from one aircraft to another The type of equipment available at present was not very satisfactory and much better instrumentation was required, as there was far too wide a scatter in the results which were obtained

In Section I of the Paper the Author dealt with standards of vibration So far as the crew were concerned, it was possible to tolerate bigger vibrations through the stick, which affected the hands and arms and were damped out to a certain extent, than in the rudder pedals or seat In Fig 3 perhaps the Author could give the position where the measurements were taken When talking about measurements and looking at graphs showing levels of vibration it was desirable to know how they had been obtained and where they had been taken In Section 2 2 the Author referred to " twitch " in the Dragonfly That type of vibration might be significant from an operational point of view, because a random variation of about 5° had been obtained in the compass on account of it

In Section 2 5 the Author referred to control vibration Mr LEACH felt that in control systems with servo-operated controls the designers had allowed much bigger tolerances of out-of-balance vibration, because they were largely masked by the servo control system itself, than would have been tolerated on aircraft with manual control only

The **Author** said that control vibration was too complicated to define He agreed with what Mr LEACH had said about instrumentation, but was not an expert on the subject On vibration, Mr WHITBY's contribution showed that more knowledge was available on the subject, and it seemed obvious that they should get together

Squadron-Leader J R Dowling (*Central Flying School*) (*Member*), referring to Dragonfly " twitch," an unnerving experience because of its irregularity, said that the Author suggested that the same source of disturbance might be responsible for some of the yawing motion observed on many helicopters in turbulent air conditions That seemed to be an unnecessarily complicated explanation, because it would seem that the penalty of having a single-rotor shaft-driven helicopter was that in turbulence there was continually varying torque In tropical countries the torque could vary enormously and even be reduced to zero without warning An up-gust could cause the rotor to speed up so sharply as to disengage from the engine, and the engine caught it up on the way back The resulting yaw was very severe With turbulence the twitch which occurred was more likely to be due to this variation in shaft torque, and the other explanation would not be necessary

The **Author** did not think that that was true The records showed clearly, he said, that the twitch had a vibration of much too high a frequency to be concerned with variation in engine torque With regard to the behaviour of the machine in turbulence, however, he was very conscious of the problem and did not mean to suggest that the kind of random excitation which contributed to twitch was responsible in a general sense for the behaviour of the aircraft in turbulence He had merely tried to present a picture of what was going on in the middle of the rotor There was a large volume of highly disturbed flow Nobody knew much about it and some of it might contribute to the twitch and some to the yawing which had been referred to, but he had not meant to suggest that it was entirely responsible If that flow were calmed down there might be some improvement

Mr J Shapiro (*Consulting Engineer*) (*Founder Member*), said that the paper was packed with information, some of it conjectural, and could well give rise to a very long discussion He proposed, however, to confine himself to a few points which could be elucidated at the meeting First, he was not at all sure that the system of using French curves to get information was a bad one It depended on what sort of paper was used, and what curves He would refer again to the comfort curve re-plotted in terms of acceleration against frequency Constant's curve extrapolates to zero acceleration Mr HAFNER had referred to the " walking " experiment, but here was a " sitting " experiment The suggestion was that when one sat one could not comfortably support any acceleration at all, which was nonsense The unknown region was at the low frequency end of the curve If they knew the admissible acceleration at zero frequency it should be easy with the help of a French curve to get the information required, instead of the very extensive experiments which had been reported because someone used in the wrong place a logarithmic scale

His second point was not directly concerned with vibration, but followed from one of the Author's generalisations, and the one which Mr SHAPIRO liked best in the Paper The Author pointed out that the rotor was a gust alleviator They had worked out some years ago the effect of the " hole " in the rotor lift distribution on the maximum lift factor obtainable from a rotor at different speeds It should be made very clear that all the assumptions about the flight envelope of maximum lift factors taken over from fixed wings were completely wrong and in a sense diametrically opposite to what happened with the rotating wing, where the maximum factor decreased with speed

When the Author said that the paper was about vibrations and not about power, that was an attitude which Mr SHAPIRO could not accept The argument seemed to be that it was possible by means of more blades to get higher speeds at the same or lower power The Author did not say it in so many words, but if it did not mean that, it did not mean very much, because there were other ways of getting higher speeds, and the question was which was the cheapest It had been demonstrated very clearly by one of the Bell technicians that even if Gustafson's limitation was accepted it was still possible to get higher speed by using a lot more power

Whenever one strayed outside real knowledge in matters of vibration, especially with the help of simplified models, one was on very dangerous ground Some of the Author's generalisations about the two-bladed rotor were not strictly true Mr SHAPIRO had had occasion to discuss the matter in great detail with Bartram Kelly, who had been in this country last autumn After spending many years advocating the gimbal-mounted rotor, the Bell Company had now found that the gimbal was unnecessary That was an example of the effect of inadequate simplification Mr SHAPIRO'S organisation had worked on two-bladed rotor problems and had found that one point made in the paper, namely that one must in a two-bladed rotor get in-plane vibration, was not really true, and that by underslinging the rotor properly it was possible to get rid of in-plane vibration, except in the sense of torque excitation

The **Author** maintained that the pure helicopter could get high speeds for the same power as, or by less power than, any other means He wanted to make that point clear, because he had no objection to stub wings if they remained stub, but he objected very strongly to seeing the very few people available who had been trained in helicopters going away to make the stub wings into the whole aeroplane

The **Chairman** apologised for the fact that owing to the large numbers wishing to speak and the great number of points which could be debated, he had had to restrict the time of speakers and limit their number He invited written contributions There was little that he could add to the description which Mr MCCLEMENTS had already made of the Paper as instructive, informative, stimulating and provocative, fully in keeping with the one given by the Author some years ago and which was still the subject of discussion The Association and the Society were very grateful to Mr FITZWILLIAMS for his paper

A vote of thanks was carried by acclamation

https://doi.org/10.1017/S2753447200003358 Published online by Cambridge University Press